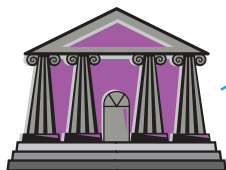


Polycymaking

Insights



from

Behavioral

Economics

Edited by
Christopher L. Foote,
Lorenz Goette,
and Stephan Meier



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**Policymaking Insights from Behavioral
Economics**

Policymaking Insights from Behavioral Economics

edited by

Christopher L. Foote, Lorenz Goette, and Stephan Meier

Federal Reserve Bank of Boston
Boston, Massachusetts

Contents

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1

Introduction

Behavioral Economics: Its Prospects and Promises for Policymakers

Christopher L. Foote, Lorenz Goette, and Stephan Meier

To some degree, all economics is “behavioral economics.” Since Adam Smith, economists have based their models on the optimizing behavior of individuals, so some description of individual behavior is bred deep in the bones of modern economic theory. During the last 15 years, however, the term “behavioral economics” has come to denote a specific branch of economics that draws heavily from psychology and other social sciences. Behavioral economists generally agree with their mainstream colleagues that markets are best understood by building up from the behavior of individual agents.¹ But behavioralists also contend that the model of human behavior found in mainstream models is too simplistic. They argue that human decisionmaking is influenced by complex forces that are familiar to psychologists and other social scientists but are generally ignored by economists. While these forces can make agents appear “irrational,” behavioral economists argue that these influences are also systematic and predictable, so that taking account of them generates better models as well as better policies.

For the past several years, researchers at the Federal Reserve Bank of Boston have wanted to learn more about the implications of behavioral economics for economic policy. In 2003, the Boston Fed invited several behavioral researchers to present papers at its annual Cape Cod research conference. The papers at this conference outlined the broad features of behavioral economics, including the psychological biases and emotions that affect individual decisionmaking, the difficulty that most people have in processing large amounts of economic information, the neurological evidence on how people make decisions, and the empirical research

on real-world policies that encourage good financial decisions through the specification of appropriate default choices. Encouraged by the success of this conference—as well as the success of behavioral economics in the academic marketplace—the Boston Fed began its Research Center on Behavioral Economics and Decisionmaking in 2005. At this Center, two full-time researchers and various visiting scholars have explored a number of policy-relevant issues, including the effects of self-control problems and impatience on savings and the ways that consumers choose among different types of mortgage products.²

In September 2007, the Boston Fed sponsored a second conference on behavioral economics. While the 2003 conference served as introduction to the field, the 2007 conference asked leading behavioral researchers to suggest specific ways in which behavioral economics is relevant for Fed policies, including monetary policy, financial market regulation, and consumer education. The five papers presented at the conference and revised for inclusion in this volume reflect these varied interests. The first paper, by Julio Rotemberg, illustrates how emotions like regret and anger could affect the purchasing decisions of consumers and the pricing policies of firms. A paper by Ernst Fehr, Lorenz Goette, and Christian Zehnder outlines behavioral influences on the labor market, showing how the “fairness preferences” of workers could affect various labor-market outcomes. A paper on financial literacy by Annamaria Lusardi argues that the typical U.S. consumer has great difficulty formulating and executing savings plans, a fact that presents difficult problems for policymakers. Christopher Mayer and Todd Sinai present empirical work asking whether behavioral influences can help explain housing prices. A paper by Rafael Di Tella and Robert MacCulloch discusses how research on the “economics of happiness” could affect the Fed’s views on inflation and unemployment. Each of these five papers is followed by commentary from two expert discussants, who occasionally take issue with the authors’ claims that behavioral insights are needed to understand that particular policy area. Finally, a concluding panel session provides perspective on the conference and suggests some additional avenues for future research.

1. Major Themes of the 2007 Conference

How Consumers and Workers React Emotionally to Changes in Prices and Wages

One of the most important themes cutting across the conference presentations involves the emotional reactions that people have to changes in wages and prices. In the standard economic model, prices and wages affect behavior by altering the feasible choice sets of consumers, workers, and firms. Thus, changes in prices and wages prompt *cognitive* reactions, as economic agents recalculate their optimal plans in light of new information. By contrast, behavioral economists argue that *emotional* reactions to wages and prices are also important. Consumers facing price hikes (especially after natural disasters) are often intensely angry if they think that firms are taking advantage of market conditions to “unfairly” boost their profits at the expense of the public. Similarly, workers facing wage cuts often get angry if they view the employment relationship as one where the parties should treat one another fairly. A firm that cuts wages just because it can (for example, during a recession) is not living up to its side of the fairness bargain. Moreover, behavioral researchers claim that firms try to avoid triggering the negative emotional reactions of workers and customers by keeping wages and prices rigid. In this way, potential emotional reactions underpin the sluggishness of wages and prices that central banks must account for when conducting monetary policy. Emotional reactions may also explain the high degree of public support for laws that economists find economically inefficient, such as those that prevent price gouging or that set minimum wages.

Most conference attendees agreed that emotional reactions to wages and prices are possible, but they differed on the quantitative importance of such reactions. In particular, there was extensive discussion about whether these emotional reactions are as important as the other frictions that are captured by standard models of labor and product markets. For example, the workhorse New Keynesian model from the contemporary macroeconomic literature contends that price rigidity stems from the administrative costs of changing prices. These costs encourage firms to

change prices only at specific times, not continuously. The menu cost model of prices has been subjected to a battery of empirical tests using both aggregate and product-level data—whether these tests indicate a first-order problem that can be addressed by introducing emotional reactions is an open question.

Empirical support for emotional reactions to wages has come mostly from laboratory and field experiments. “Workers” in lab experiments often exert more effort if “firms” pay them high wages, with similar results from the limited number of field experiments that have been conducted. When these experiments are set up as one-shot settings, a good explanation for the effort-wage relationship is that the fairness preferences of workers are giving rise to a “gift exchange condition.” Yet despite being designed as one-time events the experiments may not engender true one-shot behavior by participants—due to the simple fact that one-shot situations are rare in the real world. So even when the experimenters make it clear to participants that a particular game is truly a one-shot scenario, human brains may be hard-wired to react to *all* games as if these represent ongoing interactions. Since strategic motives for an effort-wage tradeoff are not applicable in one-shot games, the issue is of great concern for experiments designed to mimic the labor market, as real-world employment relationships usually last for more than one period. Behavioral economists counter that the repeated nature of the real-world labor market can amplify the effects of fairness preferences on labor-market outcomes. In any case, the idea that emotional reactions embedded in fairness preferences are truly needed to explain how the labor market functions is a fascinating hypothesis that may very well help us understand some fundamental labor-market facts.

Policies to Improve Financial Decisionmaking and Financial Literacy

A second theme explored at the conference involves the difficult policy choices that arise when financial illiteracy or psychological biases lead many individuals to make bad financial decisions. Most attendees agreed that the high level of financial sophistication assumed for people in neo-classical models is rare in the real world. Many persons struggle with comprehending “straightforward” financial concepts, (for example, calculating compound interest) and they often fail to grasp the implications

of basic finance theory (for example, understanding the idea that owning a single stock is riskier than owning a diversified mutual fund). While research indicates that financial literacy is especially low among people with low educational attainment, even graduate business school students often fail to choose the lowest-fee mutual fund when asked to allocate investment dollars among otherwise identical stock index funds. Given the cognitive difficulties that people have when dealing with financial topics, it is not too surprising that people delay making plans that will affect their future financial well-being. Many U.S. workers have not yet formulated solid financial plans for their retirement years, even some who will be leaving the labor force in the next few years.

The consumption-savings decision is not the only area where financial sophistication is rare. The conference took place in late September 2007, about the time that the subprime mortgage market began to unravel. Many commentators have claimed that complex subprime mortgages were marketed to unsophisticated borrowers who did not understand the contracts they were signing. Moreover, new research presented at the conference suggested that less-than-rational consumers may also affect the housing market more generally. The results in Mayer and Sinai’s paper suggest that the lagged five-year growth rate of local house prices affects current prices in a way that is hard to explain with a standard model. If unsophisticated buyers expect that past price trends are always likely to continue, then destabilizing house price bubbles (and busts) are more likely to form, making macroeconomic stabilization more difficult.

How should policymakers respond to the lack of financial sophistication among a broad swath of the American public? Regarding retirement savings, most conference attendees agreed that setting up appropriate default choices for workers is a good idea. The quintessential example of this strategy is the automatic enrollment of new employees in company-sponsored retirement plans (Madrian and Shea 2001). But conference authors and attendees also pointed out that automatic enrollment is only a partial solution to the problem of inadequate savings. Research shows that many people stick with the default allocation even when it is overly conservative (for example, investing 3 percent of an employee’s salary in a money-market mutual fund). Even more important, many financial decisions unrelated to retirement savings cannot be addressed with

appropriate defaults. How could default choices steer someone to save the right amount outside of his retirement account, to pay the right price for a house, or even to use the most appropriate mortgage product? In light of these shortcomings, a potential solution to the financial illiteracy problem is to attack it head on with consumer education. Unfortunately, there is little evidence that costly financial education programs change actual behavior, perhaps because it is difficult to target these programs to the circumstances of individual consumers.

All told, there is little doubt that behavioral economists have contributed greatly to policy analysis by identifying the general lack of financial sophistication in the population at large. They have also illustrated the effects of this problem and suggested a number of effective solutions. But more work can and should be done to give people the tools they need to succeed in the modern financial marketplace.

Behavioral Economics and Economic Policy: Expect the Unexpected

Finally, a third theme to emerge from the conference is that future contributions from behavioral economics to economic policy will probably come in unexpected ways. Panelist Lawrence Summers develops this theme most clearly when he argues that behavioral economists can study questions that are almost impossible to address with traditional tools. Examples include the right way for central banks to convey economic information, the most likely result in models of multiple equilibria, and the types of decisions that are best made by committees rather than single executives. Additionally, as the papers were being discussed, attendees consistently pointed out new angles that could be explored in a behavioral context. A good example is Andrew Caplin's discussion of Mayer and Sinai's housing paper. Caplin agreed that behavioral biases play an important part in the functioning of the housing market. But he added that an important and overlooked housing market bias was that of regulators. Caplin contended that regulators have been remarkably (and puzzlingly) unwilling to permit market mechanisms, such as shared ownership, that could temper destabilizing swings in the housing market. Developing a model of regulator behavior might therefore be very informative to housing-market policymakers. Finally, few economists in the 1970s or 1980s would have predicted that cross-sectional questions on

happiness might influence the behavior of monetary policy. But as illustrated by Di Tella and MacCulloch's paper, research on happiness may someday carry great weight among policymakers as they consider the ultimate effect of inflation and unemployment in people's lives.

Session 1: Behavioral Aspects of Price Setting and Their Policy Implications

Julio Rotemberg argues that the standard model in macroeconomics fails to capture important aspects of consumers' reactions to price changes, and firms' price-setting decisions. Rotemberg starts by documenting several features regarding how individuals process price information that are incompatible with the standard model in economics.

First, consumers seem to be largely unaware of how much goods cost, even if they have just purchased them. The standard economic model assumes that individuals are perfectly informed about prices. Yet a strong tendency for consumers to be hazy about prices may also be responsible for the strong influence that price endings have on purchase decisions. Rotemberg surveys several empirical studies showing that prices ending in the number 9 sell substantially better than prices ending in 6—holding the rest of the price constant. The second feature Rotemberg documents is that many consumers pay too much when confronted with a menu of choices. For example, customers tend to purchase unlimited Internet plans, but a considerable fraction of them would be better off by buying a plan offering more limited service. Individuals also make systematically wrong choices when they have multiple credit cards at hand. A third feature of such studies is that individuals experience strong feelings of regret if a price increased and they did not make a purchase at the old price. Rotemberg argues that this regret is often transformed into anger against the firm. The firm, the argument goes, should have cared about its customers, and therefore refrained from increasing prices. If anger against the firm becomes strong enough, customers may reduce or curtail their purchases in order to punish the firm. The prototypical example is Apple's price reduction on the iPhone, which provoked an angry reaction from previous customers. The anger against Apple was so strong that Apple issued gift certificates in the amount of the price reduc-

tion to all previous customers who purchased the iPhone at the higher price. More systematic evidence for this type of behavior comes from field experiments in which one group was initially sent a “test catalog,” in which, the subjects were told, prices were exceptionally low. They were then sent a second catalog with normal prices. Having received the test catalog with the low prices reduced the purchases from the second catalog.

Rotemberg next discusses several anomalies in price setting that are difficult to reconcile with the standard model in economics. The first anomaly is “all-you-can-eat” pricing strategies. The standard model predicts that the price of any good should always equal at least the marginal cost of that good. However, many contracts exist in which this is not the case. Drinks in an airline’s first-class cabin are free, even though the marginal cost of providing these drinks is not zero. Rotemberg considers many other examples that are violations of this standard principle, such as health club memberships or Club Med-type offers of all-inclusive vacation packages. Rotemberg argues that individuals seem to enjoy not having to maximize through constant decisionmaking, instead just choosing consumption at zero cost. The second anomaly that Rotemberg lists is the choice of end digits in prices mentioned previously. Finally, Rotemberg argues that firms’ price-setting decisions do not respond to the inflation rate as predicted by the menu cost model. In this model, firms face a fixed cost of adjusting prices, such as printing new price labels or printing new menus. Rotemberg argues that for plausible parameter values, the size of price changes should be quite sensitive to the inflation rate. The reason is that the higher the inflation rate, the more often the firm has to incur the adjustment cost for a given price change. Thus, it pays to raise prices by more for two reasons: first, it reduces the frequency with which the adjustment cost has to be paid; second, because inflation is higher, the price distortion weighs less heavily and for a shorter period. Yet detailed studies of individual product prices show that price increases are not very sensitive to inflation, especially when these products are sold to final consumers. Rotemberg shows that his model incorporating consumers’ regret and anger against firms who disregard their feelings produces a pattern of smaller, but more frequent price increases. The reason is that

regret acts like an adjustment cost that scales with the price increase. Thus, it is preferable for firms to have more frequent, but generally small price increases. Rotemberg’s model resolves the puzzle by which price changes do not depend very much on the inflation rate.

Rotemberg goes on to discuss several public policy prescriptions in light of his model. One example is anti-price gouging laws. Many states have put laws in place that make it illegal to raise prices in the face of an emergency, such as a hurricane. Rotemberg argues that it is difficult to see how such legislation could receive so much traction. When, for instance, demand for hotel rooms is high after a hurricane, higher hotel room prices will allow the efficient allocation of hotel rooms to those with the highest willingness to pay. However, it is easy to see how regret and anger against firms can reverse this implication. If price increases as a response to a disaster are considered unfair, this may affect everybody’s welfare, even those individuals not needing a hotel room. Thus, avoiding the indignity of witnessing price gouging (even at a second-hand vantage point) may outweigh the social benefits accruing from a more efficient allocation of hotel rooms, a point that Rotemberg illustrates with a simple model.

In a different realm, Rotemberg argues that his model offers a new straightforward justification for keeping inflation low in general. When inflation is low, regret costs become less relevant. Indeed, empirical evidence suggests that individuals have strong concerns about inflation, even though the standard economic model predicts that their welfare should be nearly unaffected, as inflation acts on all prices, goods, and wages alike.

Jonas Fisher, the first discussant, thinks that Rotemberg’s overall case is weak. He disagrees with much of Rotemberg’s interpretation of the evidence he presents. For example, Fisher argues that ignorance about the prices of many small-ticket items may simply be a rational reaction to the fact that humans cannot remember every detail. He contends that individuals are likely to remember the prices of big-ticket items. Fisher also disagrees with Rotemberg’s strategy of including regret in the analysis, instead arguing that emotions are already contained in the utility function: consumption raises happiness. Thus, negative emotions could

already be contained in the utility function as well. Fisher also points out that Rotemberg's focus is on interactions between consumers and firms. But many transactions take place between firms, and the discussant conjectures that emotions play no role there. Fisher also disagrees with Rotemberg's assertion that the standard model of menu costs does not perform well. Fisher holds that menu costs should be thought of as the costs of changing the pricing strategy, not the mere relabeling of goods. He argues that small price changes may not be so damning for the menu cost model when interpreted this way.

John Leahy offers different interpretations of some of the evidence on price changes. He argues that price changes are surprisingly large, not small, as Rotemberg argues. A newly available data source from the Bureau of Labor Statistics shows evidence that the average price change is around 8 to 13 percent, depending on how sales are treated. Leahy argues that an important force that drives price changes is idiosyncratic shocks to firms' costs that trigger price changes, not firms' adapting prices to the inflation rate. Thus, the primary reason for price changes is adapting to cost shocks, not adapting to inflation; the fact that price changes do not respond much to the rate of inflation, Leahy maintains, is not a puzzle in itself.

Leahy argues that an important aspect of macro models is simplification, and that some aspects of behavior need to be abstracted in such models. Since macro models do a reasonable job of explaining the pattern of price changes, he argues that it is not necessary to incorporate a more complicated model of consumer behavior. However, Leahy argues, Rotemberg's proposed model may have more to say about the welfare implications of business cycles. Current macroeconomic models imply that the costs of business cycles are small, that the costs of inflation are small, and that the costs of policy mistakes are small. All three go strongly against one's intuition, and suggest that the standard model omits something important. Leahy sees a potentially significant role for emotions here, as these are nonrival goods. For example, everyone can fear losing a job even though only very few individuals may actually lose one. Thus, if emotions enter into the utility function of individuals, these reactions are a potentially important source of welfare losses due to business cycles.

Session 2: Household Savings Behavior in the United States: The Role of Literacy, Information, and Financial Education Programs

The second session of the conference dealt with U.S. households' saving behavior and the effect that financial literacy has on this important decisionmaking process. **Annamaria Lusardi** began by noting that financial decisions are becoming increasingly complex. For example, the shift from defined benefit to defined contribution retirement plans leaves more responsibility to individual households. She asks the simple but very relevant question of whether U.S. households are well-prepared to deal with complex financial decisions related to their retirement savings.

Lusardi presents evidence on three crucial issues to answer that question. First, are households planning for retirement? Second, do households understand the basic concepts of financial decisionmaking in order to make reasonable decisions? And third, are they getting advice to help them make those decisions? The evidence Lusardi presents shows that for a majority of Americans the answer to all three questions is "no," and she discusses the implications of this finding.

The theoretical workhorse model for thinking about retirement savings, the permanent income hypothesis, predicts that individuals smooth their consumption over their lifetimes. In actual practice this task is very difficult as it depends on a number of factors, such as predicting future inflation, interest rates, pension plan savings, and so on. Individuals have to spend considerable time figuring out their optimal savings behavior. To test whether individuals indeed look ahead and plan as theoretical models of savings assume, Lusardi investigates actual retirement saving decisions made by U.S. households. Using the Health and Retirement Study she asks individuals close to retirement (those aged 51 years or older) whether they have given any thought to retirement. Surprisingly, the majority has not. This is especially true for individuals who might be most vulnerable, like minorities or the less educated, who often do not engage in basic retirement planning. The follow-up evidence on this finding engenders great concern. Not only have very few individuals made plans to save for retirement, those who have done so do not always follow through with their intentions. Only 18 percent of respondents were able to develop a plan and to stick to it.

To answer the question of whether U.S. households' lack of planning has any systematic consequences on their financial wealth, Lusardi presents evidence that planning is an important determinant of wealth, controlling for many socioeconomic characteristics. With respect to the first question, Lusardi shows that households plan very little for retirement (even those very close to retirement) and that this lack of planning has large consequences on wealth accumulation. But why does planning have such a powerful effect on wealth? Lusardi's answer centers on the possession of basic financial literacy.

The second question Lusardi attempts to answer is whether individuals have the basic financial knowledge to make informed decisions. The evidence presented shows that most individuals lack basic financial literacy and numeracy. For example, only about 56 percent of the individuals surveyed were able to divide \$2 million by 5. Moreover, of the individuals who answered two basic numeracy questions correctly, only 18 percent seem to be able to correctly answer a simple question about compound interest. This result adds to the growing evidence that most Americans lack basic financial knowledge. Lusardi shows that financial awareness is associated with the retirement planning discussed earlier. Individuals who make plans for the future are financially more literate. A number of papers have shown that a lack of financial literacy affects individuals' decisionmaking negatively in terms of their financial well-being.

The final step in answering whether U.S. households are ill-equipped to make necessary saving decisions is to analyze whether they get financial advice. Individuals who lack basic financial literacy could be advised by experts. As a corollary example, very few people know much about medicine. Yet this is not much of a problem, as most individuals get advice from their doctors, who are medical experts. Lusardi presents evidence that very few individuals get advice from experts when it comes to making decisions about their financial well-being, concluding that financial illiteracy is a real problem for retirement savings. This evidence deviates from standard economic theory which proposes that individuals make well-informed decisions to maximize their welfare. The result that individuals use professional financial advice to a limited extent can either be due to the demand for, or the supply of, such advice. Asked about whether they would rely on the professional advice provided by compa-

nies that manage employer-sponsored retirement plans, only half would fully implement such advice. The rest show some reluctance to rely on advice from these types of experts. Similarly, the effectiveness of educational programs is still under debate.

In the second half of her paper, Lusardi discusses initiatives to promote better financial decisionmaking and increase retirement savings. She concentrates on three policies. First, many policymakers promote educational programs to increase individuals' financial literacy. This policy has a natural appeal, and some studies showed that firms who offer retirement savings seminars did indeed have higher contribution rates (Bernheim and Garrett 2003). But Lusardi finds that the "evidence on the effectiveness of these programs is so far very mixed." Most studies on the effect of educational programs suffer from selection biases and find very few effects. Lusardi discusses a number of studies which show that some participants of retirement seminars change their intentions somewhat but do not follow through. Duflo and Saez (2003), who are very careful to eliminate selection issues, found extremely small effects of retirement seminars on savings. Lusardi therefore concludes that more studies have to show that educational programs do have an impact.

The second policy initiative to increase retirement savings are automatic enrollment programs, meaning individuals have to opt out of retirement saving plans instead of opting in. This intervention does not change the options available to individuals, and should have limited impact, according to standard models. However, as Lusardi shows, automatic enrollment in employer-provided retirement savings programs have an unambiguous and large positive effect: individuals do save more. In fact, Lusardi points out that automatic enrollment programs have worked too well, in the sense that individuals not only do not opt out, they also stick with the default contribution rate and asset allocation. As passive default levels and conservative allocations are chosen, a number of individuals might not accumulate the right level of assets or the optimal allocation of these assets. Lusardi explains that defaults work so well because "if individuals are poorly informed about their pension, lack basic literacy, and do not have good sources of financial advice to turn to, defaults are very useful because they tell workers exactly what to do." The trick is to use the default mechanism to better incentivize individual savings behavior,

not to let such automatic enrollment programs become a substitute for proactive decisionmaking.

Third, Lusardi discusses some newer initiatives to increase savings. Most of these plans target the complexity of either the enrollment process or the savings decision. Lusardi argues simplifying those decisions will be particularly powerful in fostering better savings behavior, as many individuals lack financial literacy, the ability to plan ahead, and access to effective expert advice.

Alan Blinder agrees with a great deal of the work Lusardi presents. But he adds some cautious remarks about how to think about the importance of information and planning for human decisionmaking and how to assess different policy implications. Blinder argues that the concept of *homo economicus* is merely an allegory and individuals do not have to be perfectly informed in order to make decent financial decisions. In fact, it is often rational to be uninformed. Even a Princeton economics professor like himself has not made perfectly informed plans for retirement even though he is within the age range of the Health and Retirement Study participants. However, the evidence that individuals' well-being seems to be decreasing given a greater number of choices is somewhat disturbing for economic theory and the general assumptions it makes about the behavior of *homo economicus*. Blinder is convinced by most of Lusardi's evidence that individuals score very poorly on financial literacy questions. He believes, however, that some of the testing criteria are too stern. Knowing the exact month one is eligible for collecting full Social Security benefits is an extremely hard question to answer, and getting it wrong might not matter that much. The question really is whether a particular form of ignorance has any real consequence. Blinder thinks that a lack of financial literacy will have some significant consequences. For example, if less sophisticated individuals are more likely to get complicated mortgage products like adjustable-rate mortgages (ARMs), it might explain some of the current mortgage mess in the United States.

Blinder ends his remarks by discussing the different policy implications of Lusardi's findings. He mainly favors default policies like automatic enrollment; he is not yet convinced that educational efforts work effectively, based on the mixed evidence to date. Blinder then adds two more policy suggestions, the first being commitment devices on the order of

"Christmas Clubs" to deal with individuals' temptation to save too little. The "Save More Tomorrow™" plan designed by Thaler and Benartzi (2004) is a prime example. Second, Blinder argues that the use of simple rules of thumb can improve individuals' decisionmaking capabilities. For example, the rudimentary rule of thumb to hold an equity share equal to 100 minus one's age does not give any specific individual the optimal share of equity to hold in his or her portfolio, but it gets closer to the optimum than what people might obtain without using this guideline.

Blinder concludes by saying that U.S. households probably don't know what they are doing when making financial decisions, and states that he is not optimistic about increasing educational efforts. Promoting default choices, commitment devices, and rules of thumb seem more promising.

David Laibson agrees with Lusardi's claim that financial literacy plays an important part in individuals' financial decisionmaking. In his discussion, Laibson offers a number of extensions relevant to Lusardi's evidence on poor financial decisionmaking and adds his own conclusions on how these findings can inform policymaking choices.

Laibson augments Lusardi's conclusion that a large amount of the U.S. population is financially illiterate by showing evidence of people's poor financial decisions. He and some coauthors have shown that only 50 percent of Americans contribute to a 401(k) plan with an employer match, even though these people do not have withdrawal penalties. That is, more than half of all Americans (those with access to an employer-sponsored retirement plan) are leaving a substantial amount of money on the table by not participating in such plans. In this study, a very targeted "educational program" explained to individuals that they are forgoing what is essentially "free money" and how they should change their behavior in order to get the employers' match. The result was devastating in the sense that no significant behavioral change was detected. Even such an educational program with a clear and simple objective did not induce people to change their behavior. (Needless to say, the long-term consequences of such inaction could very well be substantial in terms of retirement saving shortfalls.)

Therefore Laibson is very skeptical about the effectiveness of educational efforts to promote financial literacy. In that respect he agrees with both Lusardi and Blinder that evidence on the success of educational

efforts is mixed at best. Most evidence is actually discouraging about individuals potentially changing their behavior for the better. Laibson is also skeptical that explicit disclosure of the stakes will do the job. Laibson and coauthors have shown that disclosure policies do not work that well. Specifically telling participants in a study on choices between index funds that the various funds charge different fees did not lead the majority to put all their money into the lowest-cost funds. Even among Wharton MBA students only around 20 percent did so after this “fee disclosure” intervention.

Unlike Blinder, Laibson would nevertheless promote financial literacy. He argues that even though it will not change the aggregate saving rates of U.S. households significantly, it will still improve individuals’ financial decisionmaking in everyday life. Laibson would put more emphasis on personal finance in high school classrooms. And like Lusardi and Blinder, he has great faith in the power of default options; Laibson’s various work with coauthors has supported the theory that defaults are indeed a powerful step to increase savings.

Laibson ends his discussion by making an analogy to the health care sector. Nobody expects people to self-diagnose or medicate themselves, so institutionally we manage health care in different ways. For example, health plan choices are very narrowly defined at most employers. We should help individuals make smart financial decisions by simplifying their choices and giving them more advice and guidance in what to do.

Session 3: The Behavioral Economics of the Labor Market: Central Findings and Their Policy Implications

Labor markets present important puzzles to economic analysis. Why do firms shy away from making nominal wage cuts? Why do some sectors pay systematically more than others, independently of individual occupations? Why do wages fail to fall rapidly in the face of unemployment? The standard model in economics has a hard time explaining any one of these phenomena, and an even harder time coming up with one theory that can explain all of them. However, understanding these phenomena matters for policy. **Ernst Fehr, Lorenz Goette, and Christian Zehnder** propose a model of the labor market that can explain these phenomena,

and discuss its policy implications. There are two central parts to this model: one concerns the assumptions about individuals’ preferences; the other involves the specific properties of the labor contract.

The standard model in economics assumes that all individuals are self-ish and perfectly rational decisionmakers. These assumptions are rarely questioned, even though they matter critically for the prediction of the ensuing models. To see this, consider the following example: the standard model predicts that in the face of rising unemployment, firms will cut wages because workers’ outside options have deteriorated. Employees may be unhappy about this cut, but they are willing to accept it given their diminished alternatives. Furthermore, out of their own self-interest, workers would never expend resources to retaliate against the firm for enacting wage cuts. Thus, in the standard model in economics, there are no forces inhibiting firms from making wage cuts as the business cycle worsens. Such predictions, however, are at odds with evidence from interview studies of personnel managers and compensation officers. They strongly counsel against wage cuts, even in a cyclical downturn when more workers are looking for jobs and labor can be hired more cheaply.

Fehr, Goette, and Zehnder argue that what is wrong with the standard labor-market model is the assumption that individuals are strictly selfish. They argue that once this assumption is relaxed, the properties of the labor-market model generate natural explanations for all of the above phenomena and others. The authors begin by reviewing the evidence that individuals have selfish preferences. Evidence from controlled laboratory experiments strongly rejects the notion that individuals are strictly selfish. The best-known evidence comes from a familiar experiment called the ultimatum game. In this experiment, one subject, called the proposer, receives an amount of money, say, 10 dollars. She can then decide how to divide this amount between herself and a second subject, the responder. The responder sees the offer from the proposer and can then decide whether to accept the offer or to reject it. If he accepts, both parties receive the share of the 10 dollars determined by the proposer. If he rejects the offer, both get nothing. The game is only played once. The selfish model makes a clear prediction in this case: a selfish responder should accept any offer, since the game is over afterwards. Accepting a bad offer has no negative consequences, as there are no future inter-

actions. However, offers of 20 percent or less of what is at stake are almost always turned down, but offers of 40 to 50 percent of the pie are accepted. Thus, if the proposer makes an offer that gives her a much larger share of the pie, many individuals are willing to punish her for it by giving up the potential money they would have received. More fine-tuned experiments further show that when individuals feel treated unfairly, they respond much more strongly than when they feel they are being treated generously. This asymmetry is consistent with what psychologists Daniel Kahneman and Amos Tversky call loss aversion: the idea is that falling short of what one expected to receive is more painful than the pleasure derived from exceeding one's expectations.

These findings can have important implications in the context of labor markets. Employment contracts are typically incomplete: they do not specify every detail of a job assignment, and leave many decisions, such as whether to take an extra short lunch break, at the individual employee's discretion. It is easy to see that if an employee feels treated unfairly, she may no longer be willing to take shorter lunch breaks, or put in extra effort in other domains. Thus, if the employee feels a wage cut is unfair, this implementation may well impose costs to the firm. By contrast, these costs would be absent if all workers behaved selfishly, since a selfish individual would have been unwilling to shorten his lunch break in the first place.

The authors discuss two types of experimental studies that test this kind of market mechanism. One kind tests these mechanisms in rather abstract form in the laboratory, while the other tests the mechanism in field experiments, where often the mechanism is quite literally the one described in the example. Both types of studies provide support for the central prediction that workers care about being treated fairly. The evidence from lab experiments is particularly strong, possibly because the fairness manipulations are strongest in the lab. Evidence from field experiments is less clear. In each study when fairness was increased, worker effort went up, though in some studies not significantly so. When fairness was decreased, effort dropped, and by more in absolute terms than it increased in response to fair treatment. Thus, this mechanism creates an incentive for firms to pay high wages, which could prevent labor markets

from clearing. However, the evidence from both lab and field experiments also indicates that there are a substantial number of strictly selfish individuals that are unmoved by fair treatment from the firm, and put in just as much effort as they otherwise would have. Indeed, at least in the field, the evidence indicates that while the fairness mechanism is clearly in place, it is often not powerful enough to give firms a monetary incentive to pay high wages when the employment relationship lasts for only a limited time.

However, a second important feature of the labor market is that employment relationships are typically repeated and can last many periods. Evidence from lab experiments shows that this creates a powerful multiplier effect. The reason is that now, even strictly selfish individuals have a *strategic* reason to appear like they are fair-minded. By contrast, if everyone was strictly selfish, the fact that the employment relationship was repeated does not change the basic motivation problem: as long as repetition is finite, the authors argue, selfish preferences will make it ineffective. But evidence shows that in experiments in which interactions are repeated, employees reciprocate receiving high wages by exerting high effort. Paying high wages now becomes highly profitable, and gives the firms an incentive to raise wages. Evidence from field studies is more scant, but generally supportive of the same mechanism.

Fehr, Goette, and Zehnder then turn to discussing the policy implications of the mechanisms they have described. An important consequence of the fairness mechanism is that firms may shy away from cutting wages, as this is considered particularly unfair. Indeed, the evidence is very strong that firms only rarely cut wages when inflation is low but often only raise wages by very little when inflation is high. Thus, this mechanism places additional responsibility in the hands of the central banks, as they can influence firms' real wage costs. This conclusion is in sharp contrast to what standard models imply and again shows how crucial assumptions about workers' preferences are to interpreting the predictions from these models. A second consequence is that this mechanism also makes wages less responsive to changes in the economic environment and, thus, marginal costs less volatile over the business cycle. This implication is in line with the empirical evidence. Traditional models in macroeconomics

typically have to resort to implausible assumptions about labor supply to get sufficiently inelastic marginal costs to match the evidence. The fairness model also implies that some policies may not easily be reversed. For example, evidence from lab experiments shows that when a minimum wage is introduced, this creates a reference point for what individuals consider fair. Even when the minimum wage is then removed, workers still feel entitled to receive this wage, and this effect may be strong enough to survive even in competitive environments. Thus, abolishing the minimum wage does not necessarily remove its effects from the economy, a fact that needs to be considered at the time of its implementation.

George Baker, the first discussant, mainly took aim at two parts of the paper by Fehr, Goette, and Zehnder. His first objection concerns the evidence obtained from one-shot experiments, meaning from experiments where the experimenters make sure that, by design, no subject will ever interact with another subject twice and all interactions are strictly anonymous. Baker questions whether the evidence from one-shot experiments really shows individuals' preferences in these situations. Baker argues that it is natural for many individuals not to think about one-shot situations because in real life, almost no situation is truly a one-shot deal. Many norms are shaped by our experiences in repeated interactions from everyday life. Therefore, Baker argues, many individuals may have difficulty adapting to one-shot situations. Thus, selfish individuals may behave unselfishly in one-shot experiments because they apply the reasoning from repeated interactions, unable to tell the difference between a one-shot game and a repeated interaction. If we think about these instances, we do not need a new concept of fairness, but we can instead apply what we know about the theory of repeated games. Baker's second and related point of criticism is that many of the features that Fehr, Goette and Zehnder discuss can be interpreted as equilibria in infinitely repeated games. Baker rejects the criticism that individuals are only finitely lived, and that therefore, any game is finite. He argues that this is not the way individuals think. In this case, he argues, paying high wages and supplying high effort can be optimal, because it is an equilibrium in the repeated game. He also argues that downward wage rigidity can be explained this

way: if a firm cuts the nominal wage, individuals may take this as a clear signal that the firm has decided to renege on the initial agreement and therefore reduce effort.

John List, the second discussant, offered a number of criticisms on Fehr, Goette, and Zehnder's interpretation of the data. There are two main points to his objections. First, based on his own earlier work, List argues that economic preferences measured in the laboratory do not translate into preferences in the field, so caution needs to be applied when findings from labs are projected to settings outside the lab. List says that this caution applies to almost any of the dimensions the authors talked about—the degree to which individuals deviate from perfect selfishness and the degree to which they care distinctly more about avoiding losses than realizing a gain of the same size. List also argues that reputation is tremendously important in the labor market. He cites evidence from his own work which shows that reputational concerns provide an important mechanism to discipline opportunism, and that social preferences play only a minor role in this example.

In the ensuing general discussion, the argument about how to interpret the experimental results and how to extrapolate from experimental results to the field continued. Ernst Fehr strongly disagreed with the comments by the two discussants. He argued that it was wrong to reject a behavioral explanation on the basis that another, perhaps less plausible one that doesn't rely on nonselfish preferences, exists. The question is which explanation is the correct one. Fehr also argued against Baker's hypothesis that individuals mistake one-shot interactions for a repeated game. Evidence from studies using neuroscientific methods, he argues, show, for example, that individuals take pleasure in punishing unfair behavior: their brains show increased activations in the same areas that show activations when they receive other rewards, such as orange juice, money, or cocaine. Fehr also argued that perhaps another reason why reputation is so important is not only because individuals care about the dollar value attached to it, but rather because individuals value having a good reputation intrinsically, that is, as a preference. He argues that none of the evidence so far can distinguish between the two.

Session 4: U.S. House Price Dynamics and Behavioral Economics

The conference's fourth session examined whether behavioral concepts are needed to understand housing prices—a topic of great concern to U.S. policymakers since 2007. **Christopher Mayer and Todd Sinai** begin their paper with a blunt assertion, writing that: “The question of whether psychology matters in the housing market has been settled long ago: the answer is yes.” The main goal of the Mayer and Sinai paper is to explore how psychology matters by adding variables that reflect capital availability and behavioral influences to an otherwise standard model of housing prices.

Mayer and Sinai place this exercise in context with a quick examination of local housing data. Many housing economists are convinced that bubbles can exist because the behavior of some local housing markets is difficult to explain with a fully rational model. The boom-bust cycle in Vancouver, Canada during the early 1980s is an oft-cited example.³ When analyzing U.S. data, Mayer and Sinai group the nation's cities into three separate segments.⁴ At the time of the conference, “Steady Markets” (which include Atlanta, Charlotte, Chicago, and Houston) had seen little variation in real house prices, even during the early 2000s. “Cyclical Markets” (including primarily coastal cities such as Boston, New York, San Diego, and Washington) have enjoyed both higher long-run price increases as well as more pronounced cyclical patterns. Finally, “Recent Boom Markets” (including Miami, Minneapolis, Phoenix, Las Vegas, and Tampa) until the early 2000s had enjoyed fairly smooth price growth, but saw much greater price appreciation afterwards.

This disparate behavior among local housing markets requires a model of prices that allows for local influences. Most of this modeling effort has taken place within the rational paradigm of the standard neoclassical model. Mayer and Sinai survey this literature to show that, unfortunately, housing economists have yet to develop a rational, forward-looking and dynamic model of housing prices that accounts for both local influences and national factors. Glaeser and Gyourko (2007) develop a dynamic model of housing in a spatial equilibrium, so they are able to capture local influences. But they are unable to account for national influences, such as interest rates. Alternatively, Himmelberg et al. use a static user-

cost-based model of the house price-rent ratio to examine price behavior in 46 metropolitan areas. While this model can account for local influences, it is not dynamic, so this model must take future price expectations as given.⁵

Outside of rational models, empirical tests of psychological influences that might account for house price patterns are difficult to devise. A classic paper by Case and Shiller (1989) showed that house price increases were serially correlated, suggesting that these prices were not set in a fully rational way. Yet serial correlation in house prices could also result from serial correlation in rents, and time-series data on comparable rents are hard to obtain. In a series of other papers, Case and Shiller have looked for psychological influences on prices by simply polling homeowners about their price forecasts. These papers have often revealed very optimistic expectations of house price appreciation among owners (at least until 2006, when the last paper in this series was written). Another psychological influence is studied by Brunnermeier and Julliard (2008), who argue that homeowners cannot distinguish between nominal and real changes in interest rates and rents. As evidence, these authors show that inflation is correlated with the residuals of a dynamic rational expectations model of house prices. Finally, support for loss aversion in the setting of house prices is found in the papers by Genosove and Mayer (2001) and Engelhardt (2003). Yet loss aversion would tend to reduce volatility in housing prices, because it causes owners who are facing losses in a down market to keep the asking prices for their homes stubbornly high. As a result, loss aversion is not a promising avenue for explaining the boom-bust pattern in cities like Vancouver and the cyclical American markets.

In their paper, Mayer and Sinai further the study of psychology in housing markets by adding proxies for capital availability and psychological influences to an empirical model of housing prices. The workhorse model on which this exercise is based is that of Himmelberg, Mayer, and Sinai (2005). This model relates the price-rent ratio in a single metropolitan area to the user cost of owning a home, which in turn depends on interest rates, taxes, maintenance expenses, and the expectation of future price appreciation.⁶ In equilibrium, the attractiveness of owning a home (rather than renting) should depend inversely on the user cost; a low user cost will increase the benefits of owning rather than renting and

thereby push up housing prices relative to rents. Mayer and Sinai generate a regression equation that explains the (log) price-rent ratio in terms of the (log inverse) user cost. They then expand this regression to include other variables that proxy for the availability of capital and potential psychological influences on the housing market.⁷

This empirical strategy requires good measures of prices, rents, and the determinants of the user cost. The authors pay particular attention to one crucial component of the user cost: the expected future price appreciation of each locality's house price. These expectations are obviously unobservable. Drawing on their previous work with Himmelberg, the authors proxy for future price expectations with each locality's past price increase, measured from 1950 to 2000. A justification for doing so is that there is substantial serial correlation in long-run house price inflation, with a number of "superstar cities" enjoying high-price growth throughout the second half of the twentieth century (Gyourko, Mayer, and Sinai 2006).

Turning to the results, the first set of regressions indicates that, as expected, the user cost is an important determinant of the price-rent ratio. Estimated from 1984–2006, the user cost coefficient is 0.48 (standard error = 0.03), which is strongly significant but below the theoretical value of 1.00. Splitting the sample has large effects on this coefficient. It falls to 0.12 (.03) when estimated on a sample from 1984–1994 but rises to 1.24 (0.06) on a sample from 1995–2006. These results suggest that the late 1980s boom-bust cycle in many markets may have had little to do with changing user costs, while the early 2000s run-up in prices was more closely linked to fundamental forces.

Mayer and Sinai then augment the model with variables that proxy for the availability of capital. The hypothesis is that more widely available capital will increase the pool of potential homeowners and thereby push up prices. As the authors expect, regressions indicate that increasing the share of mortgages that are ARMs also increases the price-rent ratio. The average level of points and fees also has the expected (negative) effect on prices relative to rents. A surprising result comes when they enter average loan-to-value ratios (LTVs). Higher LTVs, which probably signal looser lending standards, reduce the price-rent ratio, which is the opposite of what the authors expect.

Another proxy for capital availability is the subprime share of the mortgage market. Adding this variable shrinks the sample period to 2000–2005 due to the relatively recent availability of subprime mortgages. The authors find that high subprime shares are correlated with higher price-rent ratios. Moreover, when year dummies are also included, the user cost coefficient rises to 0.90, close to its theoretical value of 1.00, while the puzzlingly negative LTV coefficient becomes insignificant.

The authors then add some behavioral variables. The most important of these is lagged price appreciation. Regressions indicate that the lagged five-year growth rate of prices (that is, the growth rate from year $t-6$ to $t-1$) enters very significantly, regardless of the sample period or list of additional regressors.⁸ This finding is strongly suggestive of behavioral influences. If people believe that future prices will be higher simply because prices have risen in the past, then housing bubbles are possible. Mayer and Sinai point out, however, that people may simply be incorporating past price growth into future expectations optimally, especially if there is serial correlation in demand growth. Moreover, lagged one-year appreciation is not significant. A final behavioral test comes when the authors include inflation in the regression in order to test the inflation-illusion theory of Brunnermeier and Julliard (2008), but find very limited support for it.⁹

Summing up, Mayer and Sinai write that they find a "mixed bag" when evaluating the effect of both fundamental and behavioral forces on house prices. Fundamentals, as measured by the user cost, seem to be important, especially during the recent boom. Coefficients on some of the behavioral variables (inflation and one-year price appreciation) did not enter the regressions as one would expect, though the five-year price-appreciation variable was significant. Overall, they write, "these results suggest that the 1980s house price boom was more of a behavioral bubble than the bubble in the 2000s, where fundamentals dominated in importance but backward-looking expectations continued to play a sizable role."

In his comments on the Mayer and Sinai paper, **Robert Shiller** takes issue with their opening statement that psychological forces are widely accepted to be important in the housing market. While real estate economists may realize that this market is not perfectly efficient, the typi-

cal economist is likely to approach the housing market with insufficient appreciation for behavioral forces. “Psychological factors are still difficult for most economists to incorporate into their thinking,” Shiller writes. “I think that this bias towards describing people as almost perfectly rational has led many people astray in the past, and continues to do so now. For example, the current ‘subprime crisis’ appears to have been a surprise to most people.”

While he views the Mayer and Sinai paper as a useful overview of the evidence for behavioral finance as it relates to housing, Shiller disagrees with the paper’s conclusions. It is true that tests of serial correlation in housing prices are not pure tests of market efficiency because rents may be serially correlated. Yet even though comparable rents for single-family homes are difficult to measure, the wild swings in price-rent ratios observed over time are *prima facie* evidence that prices and rents are not as closely related as traditional theory would imply. Shiller also points out that some economists have studied the price-rent relationship while assuming a high degree of sophistication among market participants. Sinai and Souleles (2003) argue that owning a home allows consumers to hedge against volatility in local rents. To support this view, they find that homeownership rates are higher in cities where rents are more volatile. Shiller writes that this theory assumes that owners are quite rational with respect to hedging rent risk. Yet in buying a home, most homeowners also take on a highly leveraged investment that is undiversified with respect to local-level shocks. It is unlikely that a thorough analysis of the lifetime portfolio allocation problem would find this type of investment optimal.

Shiller then turns to Mayer and Sinai’s empirical results, claiming that they are “interesting, but not decisive evidence about the efficiency of the market.” The sample periods are generally short, with one boom-bust cycle (the late 1980s) and one-half of another (the early 2000s). Moreover, the variables added to the user cost regression do not have clear interpretations. Mayer and Sinai claim that variables such as the prevalence of ARMs, LTV ratios, and subprime availability are proxies for capital availability. But Shiller contends that capital availability is not exogenous with respect to “boom psychology.” The subprime market in the United States grew from practically nothing in 1995 to 20 percent of

all mortgages by 2005. Subprime lenders were able to obtain the capital required to do this in part because of the boom psychology, adding that the failure of rating agencies to predict current problems in the subprime market was also related to their inability to fully appreciate behavioral forces.

“I see nothing in the Mayer and Sinai paper to change my general opinion about the recent housing boom,” Shiller concludes. “The overriding fact about the recent housing situation is that people in general were excessively optimistic about investments in housing, that this optimism was part of a social epidemic or bubble, and that the psychology is rapidly souring at the present time.”

In his comment, **Andrew Caplin** notes that buyers, sellers, and lenders are not the only persons who might be acting “irrationally” in the housing market. The U.S. housing market is subject to a slew of regulations that limit efficiency-enhancing innovations. Thus, a theory of what drives housing regulators may be a worthwhile complement to one that describes the behavior of buyers and sellers. As one example of regulator behavior, Caplin cites the limited number of ways in which distressed mortgage borrowers can be helped. When a debt-financed corporation is at risk of default, there are many ways to deal with the problem. In some cases, it would be economically inefficient to replace the firm’s managers or liquidate the firm. If so, then equity investors may find it worthwhile to buy out the debtors in exchange for a larger ownership stake in the firm. For a distressed homeowner, the corresponding strategy would call for the mortgage lender to take on a shared equity position in the home, to be paid out when the home is eventually sold. Caplin points out, however, that restrictions on what lenders can and cannot do have limited attempts to rationalize workouts in this way.¹⁰

Another way in which regulations have limited housing-market efficiency is through restrictions on the development of house price insurance products. It is possible to construct an insurance contract whose value depends on a local house price index. In this way, homeowners can partially hedge against the risk of local labor market declines. However, recent attempts to develop this type of product in New York ran afoul of state housing regulations. Specifically, these mortgages were interpreted as “price-level adjustment mortgages (PLAMs), which are illegal.¹¹ Regu-

latory hurdles and tax consequences are also problems in the development of reverse mortgages, which can be socially efficient ways of converting home equity into cash. “Taking stock,” Caplin writes, “it is almost as if most major U.S. institutions have been constructed to preserve an archaically structured housing finance market.” Producers and consumers in the housing market are subject to widespread risk as a result. Caplin observes that the regulatory resistance to mortgage-market innovations suggests a research agenda for academic economists: find out what drives regulators. Indeed, the neglect of regulatory behavior represents a poor allocation of academic attention from a social viewpoint.

Caplin adds that a similar issue of academic priorities emerges in the study of housing prices. Models of house price dynamics remain “rudimentary,” he writes. By often relying on unlikely arbitrage conditions, these models assume away the possibility of bubbles and can obscure the *ex ante* predictability of housing prices across localities. Pointing to the recent softening of the housing market, Caplin writes that “the crash is in part a sudden recognition that the return properties of these assets are little understood, even by leading academics.”

Session 5: Happiness, Contentment, and Other Emotions for Central Banks

The last paper presentation was devoted to the question of whether data on subjective well-being can inform policymakers in central banks. Economists are in general skeptical about using self-reported measures to make policy recommendations or welfare calculations. In particular, direct and self-reported measures of utility are rarely used in economics. **Rafael Di Tella and Robert MacCulloch** presented results using self-reported happiness data to shed light on the important tradeoff between unemployment and inflation. While most economists believe that utility can be inferred through actions, Di Tella and MacCulloch argue that such an indirect approach is not always superior to a direct approach in which utility is measured through self-reported measures. In so-called happiness research, such direct approaches are used to capture emotions which are hard to quantify through revealed preference approaches. They argue and show empirically that such approaches can quantify the

social costs of unemployment and inflation and as such, inform central bankers about how to think about any tradeoff between these variables.

Di Tella and MacCulloch use a question on life satisfaction as their proxy for contentment. The question asks participants in large-scale, repeated cross-country surveys: “On the whole, are you satisfied with the life you lead?” They answer on a four-point scale. Happiness researchers like Di Tella and MacCulloch claim that the signal-to-noise ratio is low enough to use these measures as proxies for utility. And indeed, a number of studies show that answers to happiness/life satisfaction questions are correlated with proxies for utility. In their basic regression, Di Tella and MacCulloch use the answer about life satisfaction as the dependent variable with the unemployment and inflation rate as the independent variable. They additionally control for individual characteristics like personal unemployment spells and income, as well as year and country fixed effects. Their results show that both unemployment and inflation rates have substantial negative effects on individuals’ subjective well-being. Quite surprisingly, unemployment matters at least as much as inflation—even though personal unemployment spells are controlled for. This indicates that the social costs of unemployment significantly exceed the private cost of unemployment—much more than is assumed in real business cycle models.

The basic estimations of the effect of unemployment and inflation on life satisfaction were then extended in various ways to paint a clearer picture of how happiness is related to macroeconomic outcomes. In a first extension, Di Tella and MacCulloch asked whether different groups are affected in different ways by unemployment and inflation rates. In particular, they were interested in whether the effects differ by income levels, which might explain differences in views about what constitutes the optimal responses to macroeconomic shocks. Interestingly, inflation seems to exact the biggest costs on people with low incomes, while recessions—periods with high unemployment rates—are particularly costly for older and more educated individuals. Surprisingly, income seems not to affect how much unemployment rates reduce life satisfaction.

To investigate more closely the channels for the effect of unemployment on life satisfaction, Di Tella and MacCulloch discuss the effect of unemployment insurance. Unemployment’s social costs are smaller in

countries with generous unemployment insurance. This result points in the same direction as the study by Luechinger, Stutzer, and Meier (2009). They show that the effect of the unemployment rate only affects the life satisfaction of people working in the private sector and not in the public sector, where job protection is substantially higher.

According to Di Tella and MacCulloch, there is some habituation to unemployment. The long-run effect of unemployment on happiness is only 34 percent of the short-run effect. No such habituation effect can be detected for inflation. In such a regression with lagged unemployment rates and inflation, causality becomes a relevant issue as central banks might react differently to shocks given the potential costs of unemployment and inflation. Di Tella and MacCulloch therefore interpret the results cautiously.

The authors conclude by stressing the important result that both high unemployment and high inflation have substantial negative effects on people's life satisfaction. The measures obtained from happiness functions can be used as weights in a social loss function that can be compared to the costs obtained in more traditional models. But much work remains to be done. In particular, Di Tella and MacCulloch note that it is important to understand the channels for those happiness or contentment effects better, and to investigate further what measure of subjective well-being is best suited as a proxy for utility. They encourage more macroeconomists to work with happiness data to perfect the measures and answer questions about how happiness impacts macroeconomic outcomes.

In his comments, **Greg Mankiw** reported that he is often happier after reading papers on happiness research. He is aware that economists generally are skeptical about relying on self-reported data, but Mankiw is more open about doing so, particularly as there is "diminishing marginal utility from looking at yet another set of regressions on the conventional macroeconomic time series." He believes that happiness research can provide various insights about what influences happiness or life satisfaction.

When it comes to what central banks can learn from regressions of inflation and unemployment on happiness, Mankiw discusses various assumptions and issues, which need further investigation to be fully con-

vincing. The first critical assumption, according to him, is whether happiness is the right objective. He submits that happiness and utility are not necessarily synonymous. Without fully understanding how happiness maps into utility, one has to be careful to treat happiness functions as utility functions. Mankiw then offers a more detailed consideration of how to think about identification issues. The independent variables, unemployment and inflation rates, can be caused by various factors. Mankiw mentions just three sources of variation: labor-market policies, shocks, and the competence of the nation's policymaking institutions. For Di Tella and MacCulloch's regression to make sense, these exogenous disturbances should affect happiness only indirectly though the effect on unemployment and inflation. Mankiw, however, believes that there are good reasons to think that those three factors influencing unemployment and inflation rates might also directly affect happiness. Take labor-market policies, such as minimum-wage laws or generous unemployment insurance. Those policies not only influence unemployment rates but also help create a more egalitarian society which in turn might have beneficial effects on happiness. Similarly, Mankiw mentions potential channels for how economic shocks and policymakers' general competence can influence happiness directly. This creates a classic omitted variables bias in estimating a social welfare function from observed inflation and unemployment rates.

Mankiw recommends looking for plausible instruments, so the causal relationship between unemployment/inflation and happiness can be established. Until such a regression is run, Mankiw sees the empirical results presented in this session as intriguing correlations, but ones that have to be taken with a grain of salt.

Alan Krueger starts by commenting on how satisfied he was with the paper overall, giving it an 8 on an 11-point scale ranging from 0 to 10, where 10 is the highest score. He immediately points out that interpersonal comparisons of such a score, as with a life satisfaction score, are difficult to make, as we do not fully understand how respondents answer such a question. As mentioned by Di Tella and MacCulloch, the question is how much signal is in this obviously noisy data. Krueger agrees with the authors that the answers to the global life satisfaction questions do

contain a signal. But he points out a number of issues that arise with using such a global measure. Krueger also mentions some concerns about the paper's econometric methods and conclusions.

Like many other studies in happiness research, the paper uses a global measure of life satisfaction: "How satisfied are you with your life overall?" Krueger points out that a substantial difference exists between a such a global assessment and the satisfaction domain gauged by a moment-to-moment recall method. People might use some sort of heuristics to answer the global satisfaction question, and this type of shortcut might bias the results. Krueger argues that there might be, for example, a "good economic performance heuristic" in which people in a survey that compares different European countries might answer by thinking "My country is doing pretty well, I should be satisfied." Such a heuristic might bias the results. Krueger does not argue that there is a single best measure of well-being right now, but one should acknowledge that one is dealing with one of many potential measures of subjective well-being. Talking about a proxy for utility raises the bar unnecessarily.

Krueger reports the lowest satisfaction with the paper's econometric approach. He suggests a number of changes, like thinking more about the level of analysis and whether the residuals are serially correlated at the country level.

Related to the empirical approaches, Krueger wonders why various studies find different effects of unemployment and inflation on life satisfaction. While Di Tella and MacCulloch's paper shows that unemployment and inflation have a similarly large effect on life satisfaction, previous studies (some of them by Di Tella, MacCulloch, and various co-authors) show a much stronger effect of unemployment than of inflation. But still, the most astonishing result of the paper, according to Krueger, is that unemployment rates matter much more than assumed by real business cycle models. Why? It would be interesting to try to tease out the reason why this is the case.

What is the relevance for central banks? Krueger points out that for the central bank, the effect of their monetary policy lever, the federal funds rate or the European equivalent, on life satisfaction would be interesting to know—taking endogeneity problems into account. In general, such

research supports the Federal Reserve's dual mandate to stabilize the real economy as well as keep inflation low. While some economic models would stress the low inflation objective, central banks get their mandates not from regressions but from elected governments. As such, the results of happiness research may help us to better understand the political constraints that central banks might face when conducting monetary policy.

Session 6: Behavioral Economics and Economic Policy in the Past and in the Future

The conference concluded with a panel session outlining the past contributions of behavioral economics to economic policy and suggesting some avenues for future work. **James Poterba**, the first panelist, began by noting how behavioral research has already promoted the implementation of one specific policy: the automatic enrollment of new employees in retirement plans. By the 1990s, many private firms were eager to increase enrollment in their pension programs. This eagerness stemmed in part from Internal Revenue Service (IRS) nondiscrimination rules stipulating that enrollment in these programs could not be skewed toward well-paid or senior members of the firm. Firms therefore wanted to increase enrollment among new and lower-paid workers, but doing so was not easy. Most firms subsidized participation with matching contributions or with outright contributions on their workers' behalf. This method often proved to be expensive, yet the lower-cost method of simply educating workers about the importance of saving for retirement was often ineffective. Moreover, firms feared that giving investment advice to workers left them exposed to future lawsuits if the workers' choices fared poorly.

Automatic enrollment emerged as a low-cost solution to this problem. The IRS issued a favorable ruling on automatic enrollment in 1998. Within a few years, a number of firms were experimenting with the policy. In 2001, Brigitte C. Madrian and Dennis F. Shea published a seminal paper in the *Quarterly Journal of Economics* showing that at one of these firms, automatic enrollment raised plan participation by as much as 40 percentage points. Other economists have also explored the behavioral effects of defaults in various saving contexts (Thaler and Benartzi 2004;

Gale, Iwry, and Orszag 2005). A general theme of this literature is that defaults can impact choices that have first-order welfare consequences.¹²

Policymakers have generally embraced behavioral research on saving defaults in their efforts to increase retirement savings. Lessons from this research are now reflected in legislation; in 2006, the Pension Protection Act provided a “safe harbor” for nondiscrimination testing among plans that employ automatic enrollment in various forms. Behavioral themes are also increasingly reflected in financial products offered in the private market. Individuals can now purchase “life-cycle” retirement funds that automatically rebalance across different investments as the investor ages.

Poterba offers three reasons for the large impact of behavioral research on automatic enrollment policies. First, the research was directly related to a problem of immediate concern (that is, increasing plan participation at firms that were concerned about the welfare of their employees as well as the IRS treatment of their plans). Second, the results of the research were easy for nonspecialists to interpret and were highly persuasive. Third, academics could draw on theoretical insights from psychology to explain the findings in ways that were appealing to nonexperts. This gave policymakers additional confidence in automatic enrollment as a worthwhile policy tool.

Poterba then discusses the welfare implications of the overall behavioral agenda. One of the key assumptions of standard neoclassical economics is that the preferences of individuals are stable over time. Behavioral economics, by contrast, stresses that decisions can be influenced by framing effects (for example, whether enrollment in a retirement plan is “opt-in” or “opt-out”), by self-control problems, or by a myriad of other psychological forces. Recognition of these forces may allow economists to predict individual choices more accurately. But these forces imply that knowing what is “good” for a particular consumer is not easy, because the consumer’s own preferences are shifting and malleable.

Another behavioral challenge to welfare economics comes when individuals are altruistic, so that their utility depends on the utility of others. This feature of individual preferences becomes doubly complex if levels of altruism can be influenced by factors like framing effects. Finally, behavioral economics complicates welfare economics by illuminating the psychological biases that should affect the decisions of policymakers and

voters. Some empirical evidence suggests that particular legislative rules (like a supermajority requirement) can have independent effects on outcomes. One can also wonder what a default choice might look like in a political context. Poterba speculated that these questions could form a basis for research on “behavioral political economy” in the years to come.

Poterba concludes his discussion with some words on future directions for behavioral research. He predicts that empirical researchers would continue to find behavioral anomalies that would be difficult or impossible to explain with standard models. In some cases, these anomalies may have minor implications for welfare. But other anomalies could resemble the work on automatic enrollments and have very large implications. The challenge for behavioral economists will be to find simple, general, and tractable models that can be applied to a wide variety of circumstances. Neoclassical economics will undoubtedly be important in this effort—by marking the boundaries of what can and cannot be explained with more traditional approaches.

The main thrust of **Janet Yellen’s** remarks concern the effects of behavioral economics on monetary policy. Many behavioral insights have already been applied to the Phillips curve, the well-known relationship between inflation and unemployment that lies at the heart of most macroeconomic models. By casting new light on the Phillips curve, behavioral research may not only give the Fed a clearer idea of what it *can* do with monetary policy, but also what it *should* do.

The Phillips curve determines what the Fed can do with monetary policy because it lays out the Fed’s menu of choices in both the short and long runs. In the short run, the Phillips curve stipulates that inflation and unemployment are inversely related. Thus, a reduction in interest rates tends to raise inflation and reduce unemployment. In the long run, however, unemployment settles to a natural rate that does not depend on nominal variables such as the money supply or the inflation rate. The Fed can therefore target the long-term inflation rate, but it cannot peg the long-term unemployment rate, which always returns to its natural level.¹³

The ability for monetary policy to affect unemployment in the short run results from frictions in the setting of wages and prices, or from imperfectly maximizing agents. Given these frictions, standard New Keynesian theories of the short-run Phillips curve posit that current

inflation is linked to expected inflation one period ahead and to average marginal costs (which are often proxied for by unemployment).¹⁴ Yellen writes that behavioral research is shedding new light on the Phillips curve by providing additional justifications for rigidity in wages and prices and by incorporating new frictions into the New Keynesian model.

As an example, Yellen writes that the New Keynesian model assumes that firms are highly sophisticated in setting wages and prices, given the relevant frictions they face. In the real world, however, economic agents may follow rules of thumb when setting these nominal values, eschewing the complex mathematical formulae embedded in the standard model. Additionally, agents' price and wage decisions may be influenced by factors that are omitted from the New Keynesian model, such as nominal (rather than price-adjusted) frames of reference, fairness, envy, social status, and social norms. Yellen points out that the Rotemberg paper presented at the conference suggests one way in which these additional factors might explain short-term price stickiness and thereby rationalize the short-run Phillips curve.

Other examples of behavioral research on inflation-unemployment dynamics center on the inflation-expectations term in the Phillips curve. In the standard model, firms change their "sticky" prices intermittently, but they form up-to-date inflation expectations at every moment in time. Mankiw and Reis (2002) reverse this setup by assuming that inflation expectations, not prices, are sticky. They find that this assumption allows the Phillips curve to more accurately reflect inflation dynamics than the traditional model. Ball (2000) assumes that agents set inflation expectations by looking back at a single variable, rather than inferring future inflation levels based on all the data in the economy. In the postwar era, this approach means that expected inflation is close to last period's inflation, but this relationship could change quickly if monetary policy causes inflation to be more volatile.

Phenomena such as downward nominal wage rigidity, money illusion, and fairness have also been shown to affect inflation-unemployment dynamics. In two papers, Akerlof, Dickens, and Perry (1996, 2000) have explored the long-run consequences of money illusion and downward nominal wage rigidity on inflation and unemployment. These authors

find that either of these phenomena could impart a long-run negative correlation between inflation and unemployment at low levels of inflation. Clark, Laxton, and Rose (1996) posit that fairness considerations may prevent firms from imposing low or negative wage changes on workers when unemployment is high. If so, the short-run Phillips curve could "flatten out" at high unemployment rates, making the Phillips curve convex to the origin. Thus, a highly volatile unemployment rate will lead to higher inflation, on average, than would be the case with a linear Phillips curve. Thus, the possibility that the Phillips curve is convex provides additional support for the view that the Fed should keep unemployment close to its natural level, not just keep inflation low. Finally, Ball and Mofitt (2001) have argued that the presence of wage norms can mean that changes in productivity growth will affect the natural rate of unemployment. When productivity growth is high, firms have no problem granting workers their expected pay increases. But when productivity growth lags, as it did from the mid-1970s to the mid-1990s, then wage norms cannot be met, and workers are laid off. The natural rate of unemployment rises until wage norms are adjusted downwards, or until productivity growth improves.

Yellen contends that insights such as these have already affected the Fed's interpretation of recent macroeconomic events. First, some Fed policymakers have attributed the good macroeconomic performance of the mid-1990s to the increase in productivity growth and its effect on medium-run unemployment. Second, model simulations conducted by Fed economists often assume that agents set expectations in a less than fully rational way. Finally, recent discussions concerning the Fed's communications strategy and the public's understanding of Fed policy often include behavioral concepts.

Moving beyond what monetary policy can do, Yellen then takes up the impact of behavioral work on what monetary policy should do, beginning with inflation. Standard neoclassical models often struggle to generate significant social costs of moderate inflation rates. Inflation encourages people to economize on their money balances so as to reduce the loss of purchasing power that comes from holding money. These "shoe leather" costs of inflation, however, are generally small—though

Yellen points out that the real costs of inflation's interaction with the tax code can be larger. The costs of inflation have also been examined by behavioral economists, who generally find that people dislike inflation much more intensely than would be predicted by the standard model. One reason for this discrepancy may be the difficulty that agents face in separating real from nominal quantities; when inflation is high, thinking in inflation-adjusted terms is that much harder. Yellen writes that a widespread disdain for inflation would seem to suggest that the Fed should aim for zero inflation. However, behavioral arguments may also support a low-but-positive inflation rate if workers tend to ignore inflation at low levels or if firms find it difficult to impose nominal wage cuts.

Yellen then discusses how behavioral concepts affect the Fed's mandate to stabilize the real economy. Work by Lucas (1987, 2003) suggests the benefits of reducing aggregate volatility in consumption to zero are surprisingly small for the average American, worth about \$16 per year according to the calibration of Reis (2007). Some economists working in the New Keynesian tradition have suggested that these costs could be higher, primarily because imperfect competition in product markets means that output is too low on average. If so, then further declines in output are more costly than increases are beneficial. Yellen said that behavioral economics might go further than this by suggesting that stabilization policy could "fill in the gaps" in a time-series plot of output by connecting the cyclical peaks in output. In doing so, stabilization would raise the *average level* of output, rather than simply reduce the *volatility* of output. One particular way in which stabilization policy might do this is if the short-run Phillips curve is convex because of wage rigidity or money illusion.

For policymakers, the bottom line of this research is that the Fed should try to stabilize output, which, along with low inflation, is one of the Fed's congressionally mandated goals. Indeed, work presented at the conference by Di Tella and McCulloch show that surveys on self-reported happiness of individuals shed light on how people might value a more stable economy. While more work must still be done in this area, correlating happiness with macroeconomic outcomes could someday provide policymakers with guidance on how to value inflation and unemployment in their loss functions.

Yellen concludes by noting that behavioral research is as exciting for policymakers as it is for academics. "It helps policymakers understand what they should care about and improves the quality of our economic models," she said. "The work at this conference highlights some of the progress that has been made, but also suggests that the marginal product of further research in behavioral economics is still likely to be high."

Lawrence Summers began his remarks by following up on a topic broached by James Poterba, who claimed that neoclassical theory may be able to explain why so many people made the default choice in the automatic enrollment studies. Poterba writes that the neoclassical explanation for so-called default bias is that prospective 401(k) participants recognize their lack of financial sophistication. When offered a default, people deduced that someone who has more knowledge than they do has pre-selected the option most appropriate for them, so they choose this default. The behavioral explanation, by contrast, claims that decisionmaking costs lead to inertia. Workers find it too onerous to invest in determining whether savings in a 401(k) plan makes sense for them, so they simply take the default when it is offered.

Summers writes that while the neoclassical explanation might make sense with retirement savings, the presence of default bias across a wide range of contexts strongly supports the behavioral view. "People don't 'opt' because they find it costly to 'opt,'" Summers writes. "That is the case in a vast range of settings, and was actually quite well-known before this research." As an example, Summers said that for 75 years, book-of-the-month clubs have been making money exploiting human tendencies to accept the default (in this case, the tendency to purchase books by not opting out).

In his main remarks, Summers offers five potential research questions that might be profitably explored with behavioral tools—questions that are difficult if not impossible to address within a neoclassical paradigm. The first concerns the communication strategy of central banks. Summers writes that there is near-universal support among economists and policymakers for central bank transparency. Yet there is also widespread reluctance to assign specific numerical probabilities to potential economic outcomes (for example, an announcement that there is a one-third to one-half chance that a recession will occur in the next 12 months). Also,

most central bank governors speak formally in public settings about the current economic situation, but they also communicate through less formal channels (for example, in deep background briefings to particular reporters). Because these disparate methods of communicating are so widespread, Summers writes that they probably reflect optimal behavior in some sense. Behavioral economists might be able to explain why.

The second research agenda Summers proposes is an explanation of the power of “cheap talk.” Consider the following example: macroeconomic textbooks point out that when domestic demand in a country declines, the negative effect on GDP in that country is usually offset to some degree by a decline in the value of the country’s currency and a subsequent increase in the country’s net exports. While this chain of economic reasoning is well-known among market participants, a U.S. Treasury secretary who repeats it in offhand remarks is likely to spark a severe decline in the dollar. Something similar occurred in 1996 after Alan Greenspan made his now-famous remark about “irrational exuberance” in the stock market. Though there was no indication that the Fed would—or even could—try to reduce equity values, stock markets still fell sharply thereafter. “It is clear that exhortation and commentary are thought to be an important part of the arsenal of financial policymakers,” Summers writes. “What is that all about? Behavioral economics should have something systematic to say on the question.”

The third question Summers offers is why some decisions are made by committees while others are made by one executive policymaker. Americans allow a single president to have sole control of the armed forces. Yet monetary policy decisions are made by a committee composed of many people. Moreover, this committee is led by a chairman with significant *de facto* power but little statutory authority. Summers writes that there may be good reasons for why decisionmaking authority has evolved differently in various contexts. An approach that explores the “nonpurely neoclassical aspects of human behavior” could shed light on why this is so.

Summers then offers a question related to the choice among multiple equilibria. A thorny issue in standard economics is understanding why bank runs occur. Policymakers believe that runs are less likely if a bank has adequate reserves, but the precise amount of reserves needed to forestall bank runs is not well-known. Summers illustrates this concept by

describing a game that could be played by a group of people. Each person is asked to play one of two strategies:

- **Strategy A:** Pay nothing and receive nothing.
- **Strategy B:** Receive \$500 if everyone in the room plays Strategy B. However, if someone plays A, then players choosing Strategy B must *pay* \$500.

In this game, most people are likely to play Strategy A, for fear that someone in the room will also play this strategy and force all of the B players to pay \$500. Then Summers altered the game, so that Strategy B has to pay \$500 only if more than 15 people in the room play Strategy A. In this case, virtually everyone is likely to choose B, since it is now a “safer” bet than it was in the original formulation of the game. “But what is special about 15?” Summers asks. “How does it depend on the full context of factors?”

Summers’ fifth and final research question concerns principal-agent problems. He notes that Max Weber believed that the emergence of the professions may have helped solve particular principal-agent problems among educated and powerful persons. In the medical field, Summers writes that professional norms and ethics meant that “doctors would not be looked at with respect by other doctors if they performed more operations on their patients in order to make more money.” He claims that the standard neoclassical framework has little to say on how norms could be established to limit unscrupulous behavior, but that behavioral approaches might be informative.

Summers concludes by placing the conference in context: “It’s probably the case that if the Federal Reserve Act were being legislated today, there would not have been a decision made to have 12 regional Federal Reserve Banks,” he writes. “But one of the virtues of having 12 regional Federal Reserve Banks has been that over time, it has been possible for some of the Banks to develop distinctive perspectives in their research, and to become centers of thought of a particular kind.” After providing some examples (monetarism at the St. Louis Fed, and rational expectations at the Minneapolis Fed), Summers concludes by suggesting that the Boston Fed should consider adopting a “behavioral thrust” in its own research output.

Notes

1. The alternative to a theory based on individual behavior would be one in which social norms affect individuals from the “top down.” For an argument that social norms do have a place in economic theory, see Akerlof (2007).
2. A full description of the Center’s activities and research output can be found on its website: <http://www.bos.frb.org/economic/bedm/index.htm>.
3. In the 18 months between January 1980 and July 1981, real house prices in Vancouver grew 87 percent. But then prices began to fall so quickly that their eventual resting place was only 6 percent above the price level before the boom began. Mayer and Sinai write that news about Great Britain’s eventual return of Hong Kong to China may have swayed the Vancouver market because many Hong Kong residents own second homes in Vancouver. Even so, they concede that “fundamental factors would have great difficulty explaining the sudden boom-bust pattern.”
4. The price analysis draws on data through early 2007, which was the most recent vintage available when the paper was written.
5. The house price expectations for each city are extrapolated from price increases from 1950 to 2000. The Himmelberg et al. model forms the basis of the model that Mayer and Sinai will estimate and will be described more fully below.
6. Future price expectations enter negatively in the user cost formulation. If the house is expected to rise in price during the next time period, this increase will benefit the owner, who will be able to sell the house at a higher price in the next period. Hence, high price expectations reduce the user cost of owning a home.
7. Local-level dummy variables can also be added. Yearly dummies are generally not added so that variables with only national variation can be included. Note that in estimating a model of the price-rent ratio, rather than prices alone, Mayer and Sinai focus their attention on *asset market* influences on housing prices. General changes in the supply and demand for housing that are unrelated to asset markets will affect both price and rents. These influences will therefore fall out of a model based on the price-rent ratio.
8. The results suggest that a one standard deviation increase in five-year price appreciation (about 3 percentage points) is associated with more than a 6 percent increase in the price-rent ratio. The coefficient on the lagged one-year growth rate, however, is small and unstable.
9. Inflation sometimes enters regressions with the opposite sign to what the inflation illusion theory would predict, while other times it is insignificant. Mayer and Sinai concede, however, that these results may stem from difficulties in measuring the user cost variables, some of which are influenced by changes in the price level.
10. Such restrictions have already hindered attempts to originate shared equity mortgages (SAMs), in which the lender takes an equity stake as soon as the house is purchased. Caplin writes that SAMs were originally proposed in the 1970s as a

way of addressing affordability problems caused by high interest rates. However, IRS rulings have reduced the attractiveness of SAMs by limiting the cases in which the lender is not interpreted as a part-owner of the house for tax purposes. Because lenders are understandably wary about complicating their tax returns with shared ownership of individual properties, SAMs have never caught on.

11. Caplin writes that PLAMs were initially proposed by Franco Modigliani in the 1970s, and that the lack of receptivity to this idea is his first-known important example of regulatory resistance to mortgage market innovations. Interestingly, Caplin points out that the PLAM is the precursor to the SAM, “which was initially designed precisely to overcome regulatory resistance to the PLAM!”

12. It is perhaps not surprising that the “opt-out” provisions of book-of-the-month clubs could induce people to purchase books or DVDs that club members may not have specifically chosen to buy. But the new research on savings defaults showed that opt-out provisions could also affect more significant life outcomes, such as how well one would live in retirement.

13. Thus, when the Phillips curve is pictured in a graph that has inflation on the vertical axis and unemployment on the horizontal axis, the short-run Phillips curve slopes downward and the long-run Phillips Curve is vertical, intersecting the horizontal axis at the natural rate of unemployment.

14. Marginal costs can also be proxied for by the output gap, defined as the actual level of output produced by the economy minus the level of potential output, or the amount of output that is produced when all factors of production are operating at their long-run levels. New Keynesian models posit that when unemployment falls (or, equivalently, when the output gap becomes negative), marginal costs tend to rise, pushing up inflation. In this way, the models explain the negative short-run relationship between inflation and unemployment.

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Behavioral Aspects of Price Setting and Their Policy Implications

Julio J. Rotemberg

In the textbook conception of economics, consumers use prices to determine the bundle of purchases that maximize their utility subject to the constraint that the total value of these purchases cannot exceed their income. In this paper, I consider the implications of letting consumers have somewhat different reactions to prices. First, I allow consumers to be unsophisticated when they use price information to plan their expenditures. This is a departure from the cognitive assumptions used in standard economic analysis. Second, I let consumers have emotional reactions to prices, including reactions that are motivated by concern for the welfare of others (as opposed to being driven by pure self-interest). The paper also discusses how these consumer reactions affect how firms set prices, as well as their implications for government intervention in markets.

Before turning to the psychological facets involved in understanding, setting, and regulating prices, it is worth recalling that the standard view that consumers regard prices only as incentives to guide their purchases has very little evidence on its side. Consistent with this theory, consumers prefer low prices to high prices—meaning that people do have a preference for being able to make more purchases. But this preference demonstrates only that *one* of people's desires is being able to acquire goods and says little about whether they do this well or whether they also have other objectives that guide their choices.

The additional conditions that rational utility maximization imposes on understanding consumer behavior are difficult to test, in part because consumers do not spend all their income at once. A vast empirical literature has thus devoted itself to analyzing whether people respond to incentives by entering less frequently into transactions whose terms are

more unfavorable. Unfortunately this “law of demand” is a very weak test of rational utility maximization, not only because consumer rationality does not strictly imply such a law, but also because fairly irrational consumers could still satisfy it.

There is, on the other hand, a great deal of laboratory evidence suggesting that people are not fully rational. However, no consensus exists among economists about the relevance of these lab-based observations for describing what takes place in actual market settings. Economists commonly react to evidence that consumers are sometimes not fully “rational”—particularly when this observation occurs in the laboratory—by suggesting that nonrational consumer behavior may not matter very much when describing the “real world.” I am thus particularly keen on studying aspects of consumer behavior that seem to matter for the prices that firms charge or should charge. This concern leads me to focus on several aspects of price setting that do not seem easy to rationalize in the standard utility-maximizing setting. In the process, I try to link these pricing patterns to psychological studies of consumers. It is important to stress that I do not think we already have proof that nonrational behavior causes the unusual pricing patterns I discuss. Systematic thinking about the connection between consumer nonrationality and firm pricing decisions is still at a fairly early stage. Nonetheless, the two behaviors do seem to be intimately related.

The paper also spends time discussing the policy implications of the consumer nonrationalities that are suggested by the behavior of consumers and firms. This topic is somewhat perilous to approach because we lack a rigorous way of discussing social welfare considerations in the presence of the consumer nonrationalities I emphasize here. A reason to analyze policy implications in spite of this impediment is that one of the ways in which consumers react to prices is by mobilizing politically and demanding changes in legislation. These political reactions seem to be part and parcel of how consumers behave with respect to prices. One important benefit of bringing realistic psychological considerations to bear on resource allocation issues is that these considerations may explain people’s behavior in the political realm as well as in the market arena. It is thus worth asking how the legislative initiatives we observe fit with the psychological reactions of consumers that I emphasize.

The paper is organized as follows. In the first section, I provide evidence garnered exclusively from consumers about how they react to prices. This evidence suggests two things that counter the standard textbook assumptions about rational utility-maximizing consumers. First, many people seem to find price information difficult to process. Second, people’s emotions and moral judgments inform their responses to the prices that they observe. In this paper I emphasize that consumers feel regret when they conclude that they made mistakes in their past purchase decisions, and that they experience anger when confronted with prices they regard as unfair.

The second section focuses on three particular aspects of firms’ pricing decisions. The first is the tendency of many firms to charge prices with a lump sum component and a “per unit” component well below the marginal cost of providing an additional unit. In the example of DellaVigna and Malmendier (2006), the most popular health club plans involve paying a monthly fee which allows buyers an unlimited number of visits so that the “per visit” fee is zero. What is puzzling about this practice is that health clubs’ marginal cost per visit is not zero; more frequent visits do raise costs (at least for towels).

This section’s second focus is that prices often end in “attractive” numbers, of which the most popular by far is the number 9. The third is the fact that prices for finished goods do not change as often as do commodity prices in futures markets. Indeed, price changes of finished goods do not just depart from the canonical model where every change in marginal cost leads to a change in price, but also depart from the predictions of models where there is an administrative cost associated with changing prices. While the modeling of this issue is still in its infancy, some of the qualitative features of price changes appear consistent with the idea that firms are setting prices to deal with nonstandard aspects of consumer behavior. Moreover, this approach has the advantage of being consistent with the fact that firms routinely cite their desire to please customers as their main reason for keeping prices relatively rigid.

In the third section, I turn my attention to policy and discuss two government policies that interfere with the freedom to set contractual terms. The first involves legislation to limit “price gouging,” while the second concerns legislation to regulate mortgages for low-income people. In both

these cases, standard economic arguments would seem to point towards allowing people to write contracts as they see fit. It is thus worth understanding why people seem to wish to limit the ability to freely contract in these market settings. One reason that fits with the earlier analysis is that people are angered by the terms generated by the free operation of the price system. I then argue that, if such feelings about prices are recognized as a legitimate source of utility, laws that interfere with the freedom to set prices can result in Pareto optimal allocations.

I close the paper by showing that the feelings about prices explored in this paper provide a rationale for keeping steady-state inflation low. While more conventional analyses also reach this conclusion, I argue that the extent to which even moderate inflation is unpopular suggests that the conventional analysis of this issue is incomplete.

1. Consumer Processing of Price Information

One important question regarding consumers is whether they maximize their own utility given the many opportunities that they face. A large strand of literature in economics has focused on decisionmaking by consumers who are imperfectly informed about the alternatives that are potentially available to them. This lack of information leads to outcomes that resemble in certain ways the outcomes obtained when consumers lack the ability to fully exploit their opportunities. In both cases, an all-knowing advisor could help people reach decisions yielding consequences that they would prefer.

There are, however, two important differences between the consequences of imperfect information and the cost of those imperfect maximizations. The first is that outside observers with only moderate knowledge can tell whether an individual used her information well, and may feel differently about mistakes made due to insufficient information than mistakes attributable to imperfect maximization. The second is that after making a decision, the decisionmaker herself may learn whether she ignored some of the information she had at the time. A human activity that has received a great deal of attention from psychologists is “counterfactual thinking,” where people revisit actions they have taken in the

past and experience regret when they feel that they should have pursued alternate courses of action. This regret would naturally be accentuated if people determined that their past acts were not justified given the information that was then available to them.

The second important question regarding prices is whether people only have a “cognitive” reaction to them (meaning that they use the information in prices to determine their best course of action) or whether they also have an “emotional” reaction to prices. The connection between cognition and emotion (or thinking and feeling) is a complicated one but there is a great deal of evidence that the two processes are somewhat independent (see Zajonc 1984). Many researchers view emotions as discrete reactions (anger, happiness, fear, sadness, and so on) that are common across cultures and detectable in facial expressions (see Ekman 1993).

An emotion that has attracted considerable attention from economists is happiness, which some view as akin to utility.¹ Unlike happiness, which is a “positive” emotion, regret is a “negative” one. What makes regret particularly important for economics is that, as discussed below, there is substantial evidence that people engage in actions whose purpose is to reduce regret. It follows that, if utility functions are to be derived from the preferences that guide people’s conscious choices, people’s dislike of experiencing regret should be incorporated into these functions.

I also consider the effect of prices on engendering feelings of anger. This is a negative emotion as well, but it is less clear that people engage in purposeful action to avoid anger. Nonetheless, avoiding anger seems useful for social welfare not only because it avoids the negative emotions associated with being angry, but also because anger seems to cause other harmful externalities. It is well-established that angry people often have an impulse to hurt those with whom they are angry. It is thus common for angry people to demand policies that punish those who have angered them. Any pain inflicted by this punishment may well increase the utility of those who are angry. These punitive impulses may also serve two broader social policy goals. First, they provide incentives to reduce the incidence of anger-causing actions and thereby reduce anger. Second, they may tame the reactions of those who become angry by establishing a formal mechanism that punishes those who cause this anger.

Price Knowledge and Awareness

The first question to ask about prices is whether people know how much they are paying for things. The numerous demonstrations of the “law of demand,” where total purchases for particular goods are lower when their price is higher, suggests that at least some people do respond to price incentives. But the validity of this law is consistent with the existence of large subsets of the population who are only dimly aware of the prices they pay. One vehicle for learning the extent to which people possess price information is to ask them about the prices of items with which they are supposedly familiar.

In Dickson and Sawyer (1990), interviewers were deployed inside stores so that they could approach shoppers immediately after they had selected a particular item by putting it in their shopping cart. Shoppers were then asked to recall the price of the item they had just bought. Even though no more than 30 seconds had elapsed between the time of buying the item and the time of answering this question, less than half of these subjects could recall the price perfectly. About a quarter of the respondents claimed not to know this price at all, while the rest gave estimates that differed from the true price by an average of 15 percent.

The Dickson and Sawyer (1990) analysis leaves open the possibility that people store price information in a part of memory that, while useful for decisionmaking, is not available for immediate recall. To test this result, Vanhuele and Drèze (2002) thus approached people before they entered a French hypermarket. Subjects were asked about the prices of goods whose pictures they recognized as depicting an item that they bought regularly. The fraction who could recall the price of these items accurately was significantly smaller than in the Dickson and Sawyer (1990) study. Vanhuele and Drèze (2002) also gave their respondents a series of possible prices (in random sequence) and asked them to say whether they saw these as good, bad, or normal deals. Using these responses, Vanhuele and Drèze (2002) deem about a third of their respondents to be “fairly knowledgeable” about prices. Still, about 14 percent of their respondents were so uninformed that they viewed prices 20 percent above the regular price as good deals (or prices 20 percent below the regular price as bad deals).

This hazy awareness of prices may also explain why some studies show that price endings have a surprising influence on people’s purchase decisions. The most extreme example of this is reported in Schindler and Kibarian (1996) who, with the cooperation of a seller, sent mail-order catalogs with different price endings for certain items to randomly selected customers. They found that items with prices ending in 99 outsold those with a lower price ending in 88. Similarly Kalyanam and Shively (1998) show that Chiffon margarine sold more when it was priced at 59 cents than when it was priced at 53 cents. It is important to stress, though, that other studies (and other commodities within the Kalyanam and Shively 1998 study), do not show such strong benefits of ending a price with the number 9.

Consumer inattention to prices is also consistent with the evidence reported in Rotemberg (2005) that increases in the regular price of Nabisco’s saltine crackers led to negligible reductions in the sales of that brand’s crackers, even when competing brands had not raised their own regular prices. Such inattention is not inconsistent with the large effect of temporary special prices reported, for example, by Hendel and Nevo (2006). Special prices are heavily advertised and signposted, so consumers who generally do not pay much attention to prices may nonetheless increase their purchases considerably when they see a special. Such inattention can also be consistent with the nontrivial long-run elasticities of demand reported by Hendel and Nevo (2006), since this long-run response may involve a gradual absorption of price information by consumers.

Paying Too Much When Confronted by a Menu of Price Choices

Many services are sold in packages that differ in their profile of required payments. Examples include credit cards, cellular phone plans, service plans for appliances, vacation packages, and health club fees. Because it is possible to compute how much consumers would have paid for the services they consumed if they had picked a different package than they actually chose, it is possible to learn whether they typically choose packages that minimize their out-of-pocket costs. This is, in a way, a very weak test of rationality because different packages also provide different incentives and consumers who respond to the incentives provided by the

package that they buy ought to have a consumption pattern that would be more expensive under alternative packages. This makes the finding of Della Vigna and Malmendier (2006) that people who buy monthly passes to a health club pay significantly more per visit than they would have if they had opted to pay “per visit” all the more remarkable.

Along the same lines, Lambrecht and Skiera (2006) show a similar pattern for a sample of purchasers of Internet access. In this sample, 25 percent of the people who pay the highest fixed fee for unlimited Internet access would have paid less if they had chosen a “three-part tariff” with a lower fixed fee, a maximum amount of free usage, and a marginal per-use fee for usage exceeding this maximum free limit. Also using a sample of actual customer records, Agarwal et al. (2007) report evidence of mistakes people make in their financial transactions. One particularly dramatic instance they document involves people’s usage of credit cards with low “teaser” rates on balance transfers. A catch with many of these cards is that the interest rate on new purchases is relatively high and that interest charges on purchases accrue to anyone who keeps a balance on the card. Given the availability of multiple cards, it is therefore optimal not to use these teaser rate cards for making purchases. While many people either use this optimal strategy from the beginning or learn it rapidly, others do not. Interestingly, these mistakes are more likely to occur among younger and older customers, whereas middle-aged ones are more sophisticated. Agarwal et al. (2007) consider several other instances (such as the payment of late fees on credit cards) where people pay more for financial services than is possible if using an optimal strategy and discover a similar age-related pattern of naïve and sophisticated behavior.

While the unsophisticated use of credit cards with teaser rates suggests that many consumers process price information poorly, the health club and late fee data suggest that some consumers may also suffer from overconfidence. These individuals may believe that they will attend frequently when facing low marginal prices or that they will be disciplined and pay their bills on time. In at least some of these examples (certainly in the case of late fees), consumers eventually learn when they make mistakes. At that point, consumers probably experience regret for not having made better decisions. Indeed, according to Zeelenberg and

Pieters (2007): “Regret can stem from decisions to act and from decisions not to act: the more justifiable the decision, the less regret.”

Regret

People have no difficulty recalling decisions that they regret. In the domain of purchases, Patrick, Lancellotti, and De Mello (2003) asked people to remember either purchases they regretted or instances where they regretted not having made a purchase. While the intensity of the purchase regrets exceeded that associated with the nonpurchase regrets, both were substantial. In both instances, respondents particularly recalled having taken actions to cope with their regret.

For nonpurchase decisions, the source of regret is often that consumers passed up a good deal. Indeed, in predicting their future regret, the subjects in the Simonson (1992) study said that they expected to feel a lot of regret if they postponed purchasing a wedding present until August and ended up paying more than they would have paid in July. This effect is so strong that overall purchase satisfaction often depends on whether consumers paid more than they could have paid if they had made their purchase at a different time. Cooke, Meyvis, and Schwartz (2001) asked subjects to gauge their purchase satisfaction in situations where they are sometimes forced to buy because the experimenter tells them that they have “run out” of the product. Not surprisingly, purchase satisfaction depends (negatively) on the price paid. In addition, this satisfaction depends positively on the prices that the individual observed before making the purchase. Consumer satisfaction also declines if the individual is told that he could have paid a lower price if he had delayed his purchase. These survey responses suggest that individuals compare the outcome they actually obtained to outcomes they could have obtained. When they could have obtained better outcomes, they blame themselves and suffer a loss in utility.

While psychologists find self-reported measures of satisfaction (and regret) as indicative of people’s well-being, economists may be more skeptical of the relevance of these self-reports. However, regret also matters for decisionmaking. People’s desire to avoid blaming themselves for bad outcomes leads them to modify their choices. The most compelling evidence for this comes from an experimental comparison of two treatments. In one treatment, individuals do not learn what would have

happened under an alternate course of action while in the other they do. Notably in Cooke, Meyvis, and Schwartz (2001), subjects faced a sequence of offers and had to make a purchase. In one treatment, they saw no offers after they made a purchase while in the other situation they did see the offers they would have received if they had not made the purchase. In seeking to avoid regret at paying “too much,” individuals are less prone to make a purchase in the treatment where they will continue to see offers after making a choice.

Regret looms large as a potential problem in situations where the price in question is an interest rate and the service people have acquired is the use of someone else’s money. When the time comes to repay the loan, borrowers having repayment difficulties will typically regret having borrowed funds in the first place. This regret is likely to be particularly severe in cases where people have to give up ownership of their house. Most people are extremely attached to their home and view its loss as a major catastrophe. This should imply that people who anticipate that taking out a mortgage will lead to regret if their financial condition deteriorates should avoid borrowing against their house to finance current purchases.

One problem, though, is that all people may not be equally adept at anticipating that certain contracts have a high potential for inducing regret at a later point in time. People who are overconfident, in particular, may well enter into contracts that put their homeownership in jeopardy and eventually end up feeling a great deal of regret. At the same time, people who are capable of rationally anticipating their own regret should also be able to anticipate the regret that is likely to be felt by people who act in an overconfident manner. Insofar as people who anticipate regret feel empathy for people who do not, the regret-inducing acts of the overconfident cause utility losses to the more prudent. An indirect piece of evidence for this empathy is that people sometimes appear to be upset when they learn that other individuals have engaged in transactions that they regret. I show an example of this in the following section.

Anger and the Fairness of Prices

Regret and anger are both triggered when people learn that they are worse off than they could have been.² One difference between these emotions is that anger is often directed at someone else who is blamed for this

misfortune. A related difference is that, as Berkowitz and Harmon-Jones (2004) put it, anger is “linked associatively with an urge to injure some target.” One way of thinking about this emotion in traditional utility terms is to see angry people as individuals whose utility increases when the target of their anger is harmed.³

While anger may not be activated in experimental settings where, as in Cooke, Meyris, and Schwartz (2001), offers are generated by a machine, in real-world purchase settings individual sellers bear some responsibility for what happens. People can thus blame sellers as well as themselves when they are unhappy with their purchases. Yi and Baumgartner (2004) provide some evidence for this co-mingling of emotions. Their study consists of an attempt to distinguish among the various emotions triggered by making purchases. Nonetheless, they report that “a prestudy indicated that when respondents were simply asked to recall a situation in which they experienced, say, disappointment, they tended to report emotional episodes in which they felt not only disappointment but also other negative emotions, such as anger and regret, with equally high intensity.” Similarly, when Patrick, Lancellotti, and De Mello (2003) asked people how they had coped with the purchase (or nonpurchase) decisions that they regretted, several of their respondents said that they expressed anger to someone about the problem.

There appears to be an association between feelings of anger and feelings that outcomes are unfair. Mikula, Scherer, and Athenstadt (1998) asked a large sample of respondents to recall recent situations where they had experienced one or more of these seven emotions: anger, disgust, fear, guilt, joy, sadness, and shame. They also asked their respondents whether the event that had triggered this emotion had been unfair. Unfairness was more strongly associated with anger than with any of the other emotions.

In the case of pricing, evidence of consumer anger tends to be anecdotal. In a recent dramatic episode, the September 5, 2007 reduction in the price of the Apple iPhone by \$200 led to the Internet posting of many angry messages by people who had bought the phones before the price cut. Such anger at price declines after people have made a purchase (which leads people to regret their purchase) is matched by anger at price increases in cases where people did not purchase at the earlier lower price. Rotemberg (2004), in particular, reports several newspaper articles

where people became angry (and somewhat violent) in response to price increases that took place after storms or after a terrorist attack. Moreover, it is well-established that such price increases are deemed unfair by many people. Kahneman, Knetch, and Thaler (1986) asked their respondents whether after a snow storm it was fair for a hardware store to raise the price of its snow shovels from \$15 to \$20. Of their respondents, 82 percent viewed this price increase as either “unfair” or “very unfair” and only 18 percent saw this change as either “fair” or “acceptable.” A large subsequent literature has verified this basic finding.⁴

One question that remains unsettled is why such price changes are seen as unfair. Kahneman, Knetsch, and Thaler’s (1986) theory is that consumers feel that they are entitled to their “reference transaction,” while firms are entitled to their “reference” level of profits. In their formulation, these reference transactions and profit levels refer to past offers made by the firm and to past profits that the firm earned. Thus, after a blizzard, the consumer is entitled to the same price charged before the storm because nothing has reduced the firm’s profits at this price. By contrast, price increases that are triggered by cost increases are fair because, even though consumers lose access to their reference transactions, firms come closer to protecting their reference level of profits.

Rotemberg (2004) discusses some limitations of Kahneman, Knetsch, and Thaler’s theory and provides a related and complementary theory that seeks to derive more directly the anger that consumers experience. The basic idea of Rotemberg (2004) is that consumers become angry at firms that accentuate their feelings of regret because firms that are even minimally altruistic would refrain from doing so. A minimally altruistic firm can be expected to feel a consumer’s regret vicariously and would thus suffer a loss whenever it contributed to this emotion. Firms that raise their prices in circumstances where this increase heightens consumer regret considerably thus demonstrate their selfishness. The model of Rotemberg (2004) is based on the idea that consumers maintain their forbearance if they cannot reject the hypothesis that the firm is minimally altruistic. If they can reject this hypothesis, however, they become angry and seek to hurt the firm.⁵

A field experiment whose results are consistent with these basic ideas is presented in Anderson and Simester (forthcoming). They compared the

purchases from a mail-order catalog sent to people who received a “test” catalog with prices that were considerably lower than earlier prices for the purchases made by individuals who received a “regular” catalog without such discounts. The post-mailing purchases of the people who received the test catalog were lower, consistent with the idea that they turned their regret at having paid “too much” into anger against the firm.⁶

Regret-based anger may also explain why firms that increase prices in response to natural disasters are particularly hated by consumers. First consider the simple example of buying a snow shovel around the time of a blizzard. An individual doing so regrets not having bought this shovel earlier and this regret is obviously accentuated if he learns that the price has been increased in response to the storm. Now consider a hurricane victim. People who are adversely affected by hurricanes inevitably feel regret at a variety of different past actions, since negative outcomes trigger counterfactual thinking and self-blame. When people in this situation encounter increased prices for hurricane-related needs, this regret is presumably accentuated since this information makes past decisions appear worse relative to past alternatives. A somewhat altruistic seller would thus abstain from accentuating such regret in this manner, and might lower his price in such circumstances (rather than merely keeping it constant).

In practice, price-setters do not all respond in the same way to natural disasters. As I discuss further below, some firms raise their prices to such an extent that they then become accused of violating legislation that forbids “price gouging.” Others, by contrast, improve the terms that they offer purchasers. After Hurricane Charley struck Florida in 2004, some hotels lowered their rates, allowed pets to stay in rooms in which they were usually not permitted, and gave free food to hungry guests.⁷ This diversity of reactions suggests that suppliers vary in their altruism. In normal times, this variety may be hidden because relatively selfish suppliers gain little by charging more than their more altruistic brethren. After a natural disaster, however, the benefits of charging a profit-maximizing price may be quite substantial. Thus, the extent to which firms are genuinely altruistic stands revealed. As suggested by the title of a story that ran in September 2004 in the *Deseret Morning News*, “Disasters reveal the stuff we’re truly made of.”⁸

Consistent with Rotemberg's (2004) model, the set of people who become angry at firms who raise prices is not limited to those individuals that actually end up paying more. Indeed, the loaded expression "price gouging," which is widely used in this context, suggests that many people view these price increases as an affront to decency. In a *Miami Herald* editorial published shortly after a hurricane, Associate Editor Martha Musgrove gives further expression to her anger and says "I'd like to punch out those price-gouging creeps."⁹

2. Price-Setting Anomalies

All You Can Eat

It is fairly common for firms to offer price schedules where customers pay a fixed fee that does not depend on their level of consumption and, in exchange, face a zero per unit cost. What makes this pattern of prices surprising is that it occurs in settings where, as in the health club example of DellaVigna and Malmendier (2004, 2006), marginal cost is strictly positive. This outcome seems problematic in that it seems to provide inefficient incentives to consume more than what is socially optimal.¹⁰ It also implies that people who plan to consume relatively little are subsidizing those who plan to consume a great deal. A firm's embrace of this adverse selection is peculiar because one would expect those who plan to consume a great deal to have a larger willingness to pay.

In discussing the pricing of health club memberships, DellaVigna and Malmendier (2004) suggest a sophisticated efficiency rationale for this pattern of prices. They suppose that health club visits are "investment goods" that reduce utility on the day that they take place and increase utility only in the future. They further suppose that individuals discount the future hyperbolically. This means that, looking just three periods ahead for simplicity, individuals at t care about $u_t + \beta\delta u_{t+1} + \beta\delta^2 u_{t+2}$ where u_t is the level of utility at t while β and δ are parameters that lie strictly between 0 and 1. When these individuals stand at u_t , an increase by one unit of utility at $t + 2$ is worth a sacrifice of δ units of utility at $t + 1$. In the health club example, DellaVigna and Malmendier (2004) suppose that the benefits at $t + 2$ of a visit are larger than the sum of the disutility of the visit at $t + 1$ and the social marginal cost of the visit at $t + 1$.

Because this individual has time-inconsistent (and "present-biased") preferences, he maximizes $u_{t+1} + \beta\delta u_{t+2}$ at $t + 1$. A unit of extra utility at $t + 2$ is now worth only the sacrifice of $\beta\delta$ units of utility at $t + 1$. As a result, the individual may no longer be willing to incur the personal disutility of a visit to the health club plus its social marginal cost even if he sees the same increase in u_{t+2} from this visit. From the perspective of the period t "self," it makes sense to trick the period $t + 1$ self into going to the health club by artificially facing the period $t + 1$ self with a low price for the visit. The contract with a zero price thus acts as a commitment device that leads people to do things that they would wish to do at t but are unwilling to do at $t + 1$.

In the health club case, the assumptions of DellaVigna and Malmendier (2004) are reasonable, though many health club users seem to enjoy their visits rather than regard them as a burden. There are, however, other examples of firms charging a zero marginal price for costly services where these assumptions seem less natural. Club Med, for example, also charges a fixed fee for a period of time and charges nothing for many activities, meals, and drinks. If people had the "present-biased" preferences discussed above, they would overconsume food and drink at $t + 1$ (when they are on vacation) relative to their desires at t (when they are booking the vacation). With these preferences, the period t self would like to impose artificially high prices for these activities at period $t + 1$. Nonetheless, just as in the health club case, people seem to like the "all you can eat" aspect of Club Med pricing. This "all-inclusive" preference is also prominent in car rental contracts—whereas marginal (mileage) charges used to be common, their relevance has waned over time.

The ubiquitous practice of charging zero for additional units of consumption suggests the desirability of a more general explanation than the one provided above. Two explanations readily suggest themselves. The first, which is mentioned by DellaVigna and Malmendier (2006) as well, is that people are overconfident about their tendency to use particular services. Instances where services are priced at zero marginal price lead lots of consumers to feel that they will benefit disproportionately from buying the service, even if they know that the average consumer does not really benefit from this type of pricing scheme.

The second explanation is that people dislike facing tradeoffs between paying a price and consuming; rather, they prefer to avoid having to make recurring “purchasing decisions” by making one decision at the outset. Consistent with this observation, Prelec and Loewenstein (1998) show survey evidence that, for a variety of goods including health clubs and meals during cruises, more people prefer to pay such a fixed fee than a “per-use” fee even if the total cost and usage is the same. This explanation still leaves open the question of why metering is so distasteful. Prelec and Loewenstein (1998) provide an explanation based on mental accounts. Another possibility is that charging a single fixed fee reduces people’s cognitive load by cutting down on both the need to carry out calculations regarding whether an additional purchase is worthwhile and regarding whether a particular purchase (or mile driven, in the case of a rental car) will lead to future regret for having paid too much. In the health club example, a customer might worry that she will face a regret-prone decision on those occasions where she only has limited time available for a visit to the health club. In the example of vacation packages, a customer might worry that staying at a hotel where the price of the room does not include meals will lead to regret about the chosen hotel when a meal purchased there proves to be expensive. This concern might be particularly acute if the hotel is in a remote location, which is common for Club Med properties.

It might be thought that a consumer who pays a fixed fee may be subject to some kind of regret if he ends up using the service relatively little. One advantage of the fixed fee, however, is that the consumer is unlikely to know how much his actual pattern of visits would have cost under a per-use payment scheme (because he is unlikely to recall either the amount he has used the service or the per-use charge under alternative contracts). By contrast, a customer using a per-use contract runs the risk of regretting his marginal transaction and is much more likely to be aware of its price.¹¹

While there is still no consensus on what determines whether a price is fair, a zero marginal price presumably also lowers the computational burden needed to decide whether a price is fair or not. There may thus be a connection between people’s desire for fair prices (and their extreme displeasure at being confronted with unfair ones) and their desire to enter into agreements that cut marginal prices to zero.

Price Endings

Consistent with consumers’ preferences for purchasing goods whose price ends with a 9, firms use this price ending extensively. Twedt (1965) and Levy et al. (2007) use quite different samples and both studies find that over half the prices they observe end in the digit 9. One explanation for this behavior is that consumers absorb price information from left to right and recall only the first few significant digits. If this were true, one might expect consumers to be more confused when a price ends with several 9s, so that prices ending with several 9s would be particularly common.

Interestingly, Schindler and Kirby (1997) show that firms are less likely to charge a price ending in a zero rather than a slightly lower price ending in 9 if the latter leads the price to end in several nines. In other words, prices ending in zero where reductions by one unit would lead a digit that is three positions to the left of the last digit to fall (as in the case of 2000) are particularly rare relative to prices ending in 9. This suggests that firms find it particularly difficult to resist lowering a price by one unit when this affects a relatively important leftmost digit. This strategy seems particularly well designed to take advantage of consumers that only react to the first few digits.

Levy et al. (2007) connect the behavior of price endings with the behavior of price changes. They show that prices ending in 9 are less likely to be changed than prices ending in other digits while, at the same time, the typical size of price changes is larger for prices ending in 9. It thus follows that firms are less attached to 9 endings so that 9 endings are “more sticky.” Still, and perhaps surprisingly, the distribution of price endings has not converged to a degenerate distribution, as other numerical price endings continue to be used for many products. Since not all price changes are multiples of 10, this means that some products go from having a price ending in 9 to a price ending with another digit. The conditions under which this occurs are deserving of further study. I now discuss price changes more generally.

The Amplitude and Timing of Price Changes

Commodities that trade in open exchanges have prices that vary frequently, often from transaction to transaction. Since essentially every

industrial good contains some commodities that are traded on these exchanges, the marginal cost of producing these goods varies as well. Nonetheless, final goods prices are rather rigid relative to the prices of raw commodities. The standard reason given for this rigidity is that there are administrative costs associated with publicizing new prices and with modifying the equipment that ensures that consumers pay a different amount for the units that they buy.¹² In this subsection, I first discuss a variety of empirical regularities that cast doubt on the idea that, by themselves, administrative costs of this type can explain the price rigidity we observe. I then turn to a more tentative treatment of why the consumer nonrationalities discussed above may help explain the pattern of price rigidity that we observe.

When the administrative costs of changing prices are independent of the size of price changes, Sheshinski and Weiss (1977) as well as Golosov and Lucas (2007) show that there is a “band of inaction,” meaning a range in which firms will not change prices. In other words, firms will keep their price constant if it falls between an upper and a lower threshold price. In the case covered by Sheshinski and Weiss (1977), there is constant inflation, and the two thresholds s and S are fixed. When inflation erodes the firm’s real price to the point that it equals s , the firm raises its real price to S —only to see the real price being eroded again. Golosov and Lucas (2007) consider a more complicated setting where firms are also subject to idiosyncratic shocks. Nonetheless, the basic logic of the Sheshinski and Weiss (1977) analysis carries through, with the firm raising its price by a discrete amount whenever history has left the firm with a price that is too low.

If the firm is setting its price optimally, two things must be true about this band of inaction. The first is that, during the period in which the firm expects its price to be constant, the expected discounted value of the change in profits from raising the price slightly must be zero. The second is that profits after the adjustment must exceed profits before the adjustment by the adjustment cost’s time value of money. The reason is that the firm can always delay adjustment for a short while and thereby save the time value of money on its adjustment cost, and must thus be compensated for this by an increase in profits when it does eventually adjust its price.

As shown in Sheshinski and Weiss (1977), this finding implies that an increase in inflation must necessarily lead to an increase in the size of price increases $S - s$. To see this, consider a firm that keeps its band of inaction constant after inflation rises. An increase in inflation then implies that the firm reaches prices near the lower bound s more quickly than before. Since profits increase with prices when the price is relatively low, this means that the present discounted value of the benefits of raising the price become positive when inflation rises. This tends to push up S , the price after adjustment. Since S is always larger than the profit-maximizing price, profits at SR fall when S is increased. Finally, since the level of profits before adjustment needs to stay in the same relation to the profits after adjustment, the price before adjustment must decline. So S rises and s falls, and $S - s$ unambiguously rises.

Rotemberg (2004) demonstrates that, for plausible parameter values, inflation’s effect on the size of price increases is quite substantial. In particular, it is much larger than the actual increase in the size of price increases one observes when comparing low to high inflation periods. One of the most striking and robust facts reported by researchers who have studied price adjustment in both low and high inflation periods is that the size of price increases barely rises even if inflation rises substantially. This finding is present in Cecchetti (1986), in Lach and Tsiddon (1992), in Goette, Minsch, and Tyran (2005), in Gagnon (2007), and in Wulfsberg (2009). The Gagnon (2007) study of Mexican data and the Wulfsberg (2009) study of Norwegian data are particularly notable because each one shows that the typical size of price increases actually rose (instead of falling) after inflation dropped in the 1990s and the 1980s, respectively. This inability of a model with administrative costs associated with changing prices to account for changes in the size of price increases seems like a substantial drawback.

An equally serious drawback was pointed out by Carlton (1986) and Kashyap (1995). They both showed that the minimum size of price increases for the goods that they studied was extremely small. This minimum increase is extremely important in models with administrative costs because it must equal $S - s$ and is small only if administrative costs are unimportant. Thus, a finding of small price increases suggests that the costs of increasing prices must be trivial, at least for some goods.

I have talked so far about models that include the administrative costs of changing prices because these models have the proper “micro-foundations” in that they derive price rigidity from an appealing and simple underlying friction. In applied macroeconomics, it is actually more common to simply assume that each firm has a constant probability of changing its price in each time period.¹³ This assumption is due to Calvo (1983), and leads the aggregate price level to behave as if firms faced costs to change prices that are quadratic the size of the price change, as in Rotemberg (1982). Taken literally, the Rotemberg (1982) model implies that each firm changes its prices by a small amount each period, which is counter to the evidence. Unfortunately, when taken literally, the Calvo (1983) model is also inconsistent with firm-level evidence.¹⁴ As shown by Gagnon (2007), Nakamura and Steinsson (2008), and Wulfsberg (2009), the fraction of firms changing their prices is not at all constant. Rather, the fraction of firms raising prices increases with inflation while the fraction of firms reducing prices is not closely related to inflation—so that the overall fraction of firms changing prices is procyclical.

If the administrative costs of changing prices were the main impediment of price flexibility, firms would presumably give this as their reason for keeping prices constant. This issue can be checked by interviewing firm managers who set prices, and several studies including Blinder et al. (1988) and Fabiani et al. (2005) have done so. In these studies, managers do not seem to put much weight on administrative costs when asked to explain why they keep their prices constant for extended periods of time. What managers cite as the main reason for price rigidity, instead, is that not changing prices avoids antagonizing their customers.

One issue that remains unsettled is whether a model where price rigidity is due to concerns about inciting negative customer reactions can account for these two features of price changes discussed earlier. One interesting model of this sort is provided by Heidhues and Köszegi (2008). They focus on consumers who become unwilling to buy a good if the price exceeds the price that they expected to prevail. Consumers react in this manner because they are averse to the loss associated with paying too much. The result is that firms face a very elastic demand for their product at the price that consumers expect to pay. This model has several attractive features, including that it represents a relatively small depar-

ture from standard economic models. Another benefit is that, while firms are not reacting directly to the anger that consumers feel, the model is quite consistent with consumers being very upset when they encounter a price that does not match their expectations—since such a price increase leads them to lose something relative to their expectations. What is less clear is whether this model is consistent with the fact that many regular price changes seem to be associated with insignificant changes in purchases or whether it can explain the patterns of price changes discussed above.

It is also not clear whether this pattern can be explained with models where consumers get upset if the firm acts selfishly, as in Rotemberg (2004, 2005). Rotemberg (2004) shows that one can at least explain the weakness of the relationship between the size of price increases and inflation under reasonable assumptions about consumer regret. A consumer facing a price that was recently increased regrets not having bought the good before its price was raised. It is therefore plausible to suppose that these “regret costs” are larger when price increases are larger. Firms that want to appear altruistic should then avoid large price increases because these induce a great deal of regret on the part of consumers. More importantly, such firms should not substantially raise the size of their price increases when inflation is higher. The reason is, in part, that a higher rate of inflation implies that regret rises by more when a firm postpones its price increase by one unit of time (since the resulting price increase will have to be larger). Postponing price increases thus becomes less attractive to a firm that wishes to be seen as acting altruistically. Since this effect is larger when inflation is larger, it has a larger dampening effect on the size of price increases when inflation is higher.

This still leaves the question of whether a model of this type can explain the fact that so many price increases are small. One possibility, suggested by Rotemberg (2005) is that there are occasions in which firms become aware that small price increases would be particularly acceptable to customers. Given the simultaneous objectives of raising prices and preventing customer anger, firms may raise their prices by a small amount on these occasions. Whether this mechanism can explain the frequency of small price increases deserves continuing theoretical and empirical research.

3. High-Low Pricing

An obvious question raised by the reluctance of firms to change their regular prices is why so many retailers adopt a “high-low” strategy where goods are routinely put on special sale below their “regular” price level, rather than adopting an everyday low pricing (EDLP) strategy. EDLP economizes on transaction (and menu) costs and some stores, Walmart in particular, are supposedly successful with EDLP.¹⁵

One factor that may contribute to the profitability of the high-low strategy is that people derive a great deal of personal satisfaction from purchasing what they consider to be bargains (see Darke and Dahl 2003 for evidence on this). Still, according to Hoch, Drèze, and Purk (1994), only about a quarter of the revenue generated by stores using a high-low pricing strategy consists of items that are being promoted. To study the costs and benefits of the high-low strategy, Hoch, Drèze, and Purk (1994) ran an extensive experiment using different stores in the Dominick’s supermarket chain. Some of these stores increased their regular prices to pursue a high-low strategy while others lowered their prices to pursue an aggressive EDLP strategy. The latter strategy was less profitable in the Hoch, Drèze, and Purk (1994) data because the reduction in prices had only a modest effect on demand. The 10 percent reduction in EDLP prices relative to those of the control stores only raised unit volume (in the category in which prices were reduced) by 3 percent.

Perhaps the most important overall conclusion of this study is that high-low stores manage to sell a considerable volume of goods at non-promotional prices so that EDLP is quite costly. This raises the obvious question of why customers do not regard the existence of high “regular” prices as unfair. While this question remains unsettled, two observations are in order. The first is that, as argued by Rotemberg (2004), regret may be kept relatively low by price specials whose duration is short and spelled out in advance. The reason is that because these specials are temporary, people who become aware of the special take advantage of it. By the same token, people who do not become aware of the special see only a relatively stable “regular” price and therefore they do not know that there is a specific opportunity that they failed to take advantage of.

A second aspect of special prices is that the people who disproportionately take advantage of them are “price sensitive” shoppers. Insofar as people who pay higher prices perceive price sensitive shoppers as valuing money (or income) more highly, they may feel that an altruistic firm would indeed wish to offer such individuals a better deal. Thus, specials, at least in the form that they take in modern supermarkets, may be seen as less unfair than other forms of unstable prices. As an illustration of these differences, Haws and Bearden (2006) report that fairness perceptions depend on the amount of time that elapses between the purchases of consumers who pay different prices. People regard it as particularly unfair if another consumer obtains a lower price within an hour of their own purchase, while price differences separated by a month are less likely to be seen as unfair. At the very least, this fact shows that firms with rigid prices are less likely to upset their customers by behaving in ways that they regard as unfair.

4. Government Price Policies

The previous analysis suggests three behavioral elements of prices that are relevant for public policy. First, people appear to be confused by certain aspects of pricing, so they may well make mistakes in their choices. Second, they see certain pricing practices as unfair and they react to these with anger. Some firms act so as to avert this anger but others do not, so this consumer anger is observed. Lastly, people who are not directly affected by a particular price do sometimes share the anger of those who are, presumably because they empathize with their sense of being treated unfairly. Unfortunately, these considerations mean that policy analysis is more difficult than in the usual case where people are rational decision-makers who care only about their own bundles of consumption. Indeed, relatively little is known in general about how policy should be conducted if people make mistakes, experience regret both directly and vicariously, or get upset at people whose behavior exacerbates regret.

I illustrate the complexities of the resulting welfare analysis by considering two policies that are currently under discussion in the United States. Both involve interference with the right to set prices freely, both already

have the force of law behind them, and there appears to be widespread support for expanding the scope of these laws. The first of these policies forbids firms from raising prices in emergencies while the second limits the contractual terms that can be offered when people take out mortgages to purchase their primary residences.

In standard economic models, these interventions lead to Pareto sub-optimal allocations so everyone's welfare can be increased by freeing prices and making lump sum transfers. As I discuss below, the presence of regret, anger, and empathy make it harder to increase everyone's welfare in this manner. The analysis also reveals who gains and who loses from these policies and thus makes clear why it is possible for these policies to be supported by a majority of the population. By doing so, the analysis may also shed light on the elements of these policies that people see as particularly desirable, and this might help improve their design.

This section ends by discussing monetary policy and inflation. Because the analysis of inflation when people have the concerns that are explored in this paper is still in its infancy, this portion is mostly conjectural. Still, the psychological issues stressed in this paper may help explain why inflation is so widely disliked.

Anti-Price Gouging Legislation

As of September 2005, 28 U.S. states had laws against "price gouging." These statutes outlawed certain price increases during periods in which government authorities declared a state of emergency or during periods of "market disruption." The details of these laws differed, with some states treating offenses as criminal violations subject to jail while others treated them as civil offenses subject only to fines. The existing laws often exempted price increases based on cost and outlawed only "excessive" or "unconscionable" price increases. Connecticut, Oklahoma, and West Virginia each forbade price increases in excess of 10 percent of the price in the pre-emergency period, though they differed in the range of products that were covered by this requirement. In 2006 and 2007 there was also an effort to impose federal anti-gouging legislation specifically targeted at oil products.

States with anti-gouging legislation tended to make it easy for consumers to lodge complaints. During the hurricane emergencies of 2004–2006,

the Florida Attorney General dealt with about 13,000 such complaints. Many of these were resolved quickly and there were only 81 formal investigations, which resulted in 17 lawsuits. Several of these lawsuits resulted in businesses paying restitution and fines.¹⁶ For example, the West Palm Days Inn, which charged guests up to \$144 in spite of having a nearby billboard advertising rooms for \$49.99, agreed to pay \$70,000. This was supposed to pay for the investigation, with \$10,000 set aside to compensate hotel guests, and the rest being directed to the Florida Hurricane Relief Fund.¹⁷ Similarly, a Honda dealer that sold electric generators in Long Island for 67 percent above the normal price after a 1985 hurricane was ordered to give refunds to its customers and was fined \$5,000.¹⁸

Anti-price gouging laws were billed by their supporters as protecting consumers. In introducing federal anti-gouging legislation, Senator Joseph Lieberman (Democrat, Connecticut) said: "This law is necessary because there is really nothing available to protect consumers and businesses from being gouged."¹⁹ The idea that price controls "protect" consumers seems incompatible with standard economic models. In a competitive market, prices below the market-clearing level lead to an inefficient allocation of scarce goods among consumers who value them differently and yield an insufficient incentive to bring more goods to the market. So consumers as a whole are clearly hurt. It should be noted, however, that these deleterious effects may be relatively modest if prices are temporarily held near their pre-emergency level for a short time. The reason is that the people buying critical goods during emergencies may all need them a great deal so the problem of inefficient allocation across consumers may be small. Similarly, the pre-emergency price may still maintain a reasonable incentive to bring goods to the affected area.

Nonetheless, there is little doubt that some inefficiency arises during the period where price increases are capped, raising the political economy question of why such caps have political support. This is an important question because it casts doubt on the idea that people's full reaction to prices is captured by the standard economic model in which selfish consumers react rationally to prices as signals of scarcity. If people were purely selfish, this political mobilization should be championed by its direct beneficiaries. But who are the beneficiaries here? The affected firms lose money so they should organize against these laws and, according

to the view that firms find it easier to organize than consumers, they should win and keep such legislation at bay. Some consumers do benefit by paying lower prices, but others lose by being rationed. Thus, if the traditional model of consumer preferences is valid, it is not entirely clear whether consumers as a whole could expect to gain from this legislation. Even assuming that consumers come out ahead, it would seem that the traditional model has no explanation for why consumers organize to keep prices low in this particular case, rather than organizing to lower prices in more normal times.²⁰

One possibility is that policymakers and the public at large are confused about how markets operate. This interpretation is unappealing because economists have written a large number of popular media accounts on the topic, begging the question of why the standard economic arguments have been so unpersuasive in the past. Since these arguments do not seem overly complicated, an inability to comprehend them would seem to bode poorly for people's capacity to make rational decisions.

A rather different possibility is that people understand the economic arguments full well but that they do not find them convincing. Some evidence for this can be found in the *Miami Herald* of September 1, 1992 where Martin Hoffman gave the standard economic arguments against anti-price gouging legislation and Associate Editor Martha Musgrove forcefully rejected them. From this and the earlier discussion of consumer reactions to price increases, we can conclude two things. First, people who faced price increases during emergencies were upset, with their utility loss exceeding the financial burden of having to pay a higher price. Second, some people who were not directly affected by the price increase were also furious at the gougers who raised their prices during the emergency.

These two factors reduce the social benefits from letting prices rise after an emergency. Indeed, the existence of regret and anger make it difficult to achieve Pareto improvements from the outcome with anti-gouging legislation even if transfers are allowed. Without transfers, individual losses from the abandonment of anti-gouging legislation are larger still.

To see this, consider a setting where we would normally expect such legislation to be Pareto suboptimal. Suppose that a law of this type forces a firm to charge a price p for a hotel room that ends up being occupied

by a person to whom it is worth $x > p$. Moreover, suppose that there are two additional people to whom the room is worth $y > x$ and that they are both rationed. Suppose, further that an additional room could have been obtained at a cost y so that a price of y would have led both the people who value the room more highly to have obtained lodging.

In the conventional analysis, we reach a Pareto improvement by charging y for the two rooms and giving the person to whom it is worth x a transfer slightly larger than $x - p$. To find the people willing to pay y , this improvement requires that the price y be charged for the rooms. But, as soon as y is charged, everyone who sees the higher price (all three potentially customers) suffer the nonpecuniary losses that are triggered by the difference between y and p . Let these losses equal ℓ . This can be thought of as the costs of regretting not having bought the good earlier at p . Alternatively, one can imagine that the reference price p is particularly salient in a natural disaster with people feeling relatively acute pain when they spend more for a hotel room than they would have in normal times.

Those observing the situation, meanwhile, are upset if the hotel owner is receiving y rather than p . We could relieve the observers of some of their anger by charging y but giving the hotel owner only p and using the remaining proceeds for charity. This fits with Campbell's (1999) demonstration that the auctioning of a desirable Barbie doll during the Christmas shopping season is more acceptable if the proceeds go to charity. But this remedy would not be sufficient to induce the hotel owner to bring the second room to the market. For that, we would have to pay her y , at least for the second room.

Leaving aside the problem of anger at the hotel owner, we can only make all customers as well off as they were with the anti-gouging law if we give all three of them ℓ . Once we do that, the money left over after the two room occupants pay y may not be sufficient to compensate for the cost of the extra room y , plus the price the hotel room initially commanded, p , plus the gain to the initial room occupant $x - p$. In other words, $y - 3\ell$ may be less than x . The impediment to reaching a Pareto improvement (even in the presence of transfers) is that the process of identifying the person who is willing to pay the most imposes direct costs to other consumers. Without transfers, of course, simply raising the price

is not a Pareto improvement, since there is at least one customer who is worse off if $x > p$.

In this example, one could argue that the two rationed customers benefit from freeing prices and that, since the hotel owner also gains, a majority of the agents is better off. Even if people who value the good at y are better off because the regret costs ℓ are relatively low, a majority could still favor anti-gouging legislation. This would occur if there existed a large number of people who purchased the room at both the old and the new price since each of these would lose ℓ in addition to the price difference. It would also occur if the anger induced by the hotel owner that raised prices is counted sufficiently in social welfare.

“Suitability” Criteria for Mortgages

According to Persky (2007), the idea that emergencies should lead lenders with other-regarding preferences to make loans at zero interest was central to the medieval prohibition against usury. Persky (2007) quotes a 1572 text by Thomas Wilson saying: “lend to your poore neighbors in time of their great need” and “[lending] shoulde be ...free, simple, and for charities sake ...without anye thinge at all more than the principall.” Persky (2007) further suggests that charging positive interest only became socially acceptable as firms gained productive opportunities that made it easy for them to repay such loans. The debate over limits on interest rates continues to this day. My focus here, however, is on a very specific set of regulations concerning loans, namely limitations on contracts that allow people to borrow using their principal residence as collateral.

In the United States, the bulk of the federal regulations concerning extension of credit to consumers involves the disclosure requirements imposed by the 1968 Truth in Lending Act and its subsequent revisions. An important and interesting exception to this emphasis on information is provided by the 1994 Home Ownership and Equity Protection Act (HOEPA), which sets limits on the contractual terms of “high cost” mortgages. Mortgages that are classified as high cost either on the basis of high interest rates or high up-front fees are not allowed to contain penalties if the borrower pays down the principal before it is due nor are they allowed to have the principal grow over time (meaning have negative amortization).²¹ In addition, lenders who offer such loans are

not allowed to engage in a “pattern or practice of extending credit . . . to a consumer based on the consumer’s collateral without regard to the consumer’s repayment ability.”²² In this section, I analyze whether the psychological considerations stressed in this paper rationalize restrictions of this type.

The “endowment effect” of Kahneman, Knetsch, and Thaler (1990) leads people to demand a higher price to part with an object they already have than they are willing to pay to acquire the same object. This attachment to objects that one owns suggests that people who lose their house through foreclosure suffer enormous pain. Moreover, the desire to help people in these circumstances suggests that this pain elicits empathy from others.

The recent escalation of subprime mortgage defaults in the United States begs the question of how so many individuals reached the point where they stood such a high probability of losing something that is so valuable to them. One obvious possibility that is consistent with the cognitive difficulties discussed earlier is that overconfident individuals may have been particularly prone to enter into such contracts. A related possibility is that this population of borrowers proved easy to manipulate by brokers who posed as their friends. For purposes of discussion, consider a very stark case where, for one reason or another, some people make such mistakes.

Let there be only two periods (labeled 0 and 1) and a person I will call A who believes that he derives total expected utility $\bar{u}_0 + \beta\bar{u}_1$ if he does not borrow. Thus, \bar{u}_0 and \bar{u}_1 denote his baseline levels of expected utility and β denotes his discount rate. Let us suppose that A is offered a loan backed by his principal residence and that this individual believes that accepting this loan will lead to levels of expected utility \hat{u}_0 and \hat{u}_1 in the two periods. This person thus accepts the loan if he believes that $(\hat{u}_0 + \beta\hat{u}_1)$ exceeds $(\bar{u}_0 + \beta\bar{u}_1)$. Indeed, if one took a revealed preference viewpoint, one would conclude that $(\hat{u}_0 + \beta\hat{u}_1) \geq (\bar{u}_0 + \beta\bar{u}_1)$ from the observation that A took the loan.

Now consider an observer (possibly an econometrician armed with data and a model, possibly a friend) who agrees with the assessments \bar{u}_0 , \bar{u}_1 , and \hat{u}_0 so that she has no quarrel with the baseline levels of utility or the extra time zero utility from consuming the proceeds from the

loan. The observer believes, however, that expected utility at time 1 with the loan equals $\tilde{u}_1 < \hat{u}_1$. In other words, she believes that there is a good probability that the individual will lose his house, experience regret and be extremely unhappy. If $(\bar{u}_0 + \beta\bar{u}_1) < (\hat{u}_1 + \beta\tilde{u}_1)$, this observer believes that A is better off not borrowing.

Now consider an ideal mortgage limitation that prevents A , and only A , from taking on this loan. The conventional analysis gives credence to the utility function that is consistent with A 's actions, and thus sees this prohibition as inefficient because it makes both A and the lender worse off. One might, instead, use either the utility function of the observer or, equivalently, a social planner's expectation of the true long-run utility of A .²³ This point of view is somewhat problematic because there is at least one moment in time where A believes that this prohibition makes him worse off, though there may well be other times (particularly in period 1) where A is in fact better off.²⁴

Even if one is willing to evaluate A 's welfare using A 's assessment of utility, the existence of empathy can still make the outcome with the loan prohibition Pareto optimal. Suppose, in particular, that observers have a utility function that puts a weight of λ on *their* perception of the utility of potential borrowers. In other words, observers "put themselves in A 's shoes" but use their own assessments of utility when they do so. Then each loan received by people with the characteristics of A is costly to these outside observers if $(\bar{u}_0 + \beta\bar{u}_1) > (\hat{u}_0 + \beta\tilde{u}_1)$. These costs are experienced mostly in period 1 but observers already anticipate them as of period 0. To obtain a Pareto improvement from the outcome reached with the prohibition, it is thus necessary to compensate observers at some point for these losses. Even if A and the lender feel that they are better off when the loan is allowed, their subjective gains may not be sufficient to compensate these observers for their vicarious losses.

There also may exist outside observers that are upset by the behavior of the lenders that loan to A . The widespread use of the pejorative term "predatory lending,"²⁵ already suggests that many people regard certain lending practices as morally reprehensible. The activist organization ACORN (the Association of Community Organization for Reform Now) has gone further and adopted the chant "predatory lenders, criminal offenders."²⁶

While the evidence that people dislike "predatory lending" seems strong, different people (not all of whom may be equally opposed to the same lending practices) use the term quite differently. In 2000, the Department of Housing and Urban Development and the U.S. Treasury Department published a report called "Curbing Predatory Home Mortgage Lending," where this concept was defined in terms of specific practices. These included the use of high fees of which borrowers were unaware, frequent refinancing so as to collect fees repeatedly, as well as other forms of fraud. The report also objected to loans that were made without attention to the borrower's ability to repay, where such loans could only be profitable if the home was eventually foreclosed.²⁷ While agreeing that predatory lending is based on fraud, the California Association of Mortgage Brokers defines predatory lending as "intentionally placing consumers in loan products with significantly worse terms and/or higher costs than loans offered to similarly qualified consumers in the region."²⁸ Lastly, and most closely related to the discussion above, mortgagenewsdaily.com defines loans as predatory if they do not benefit the borrower.²⁹

This last definition has the advantage of fitting with the idea that customers demand a minimal level of altruism from firms. A lender that benefits marginally from providing a loan whose borrower can be expected to lose a great deal of utility might well be seen as not having this minimal required altruism. Because transactions in mortgage lending markets are not repeated very frequently, the anger of past customers is not very effective at keeping lenders in check (particularly in comparison with the effect of potential anger on suppliers of food items that are purchased regularly). It is thus not surprising that anger at lenders spills over into the policy arena.

Whether this interpretation of the source of consumer anger is warranted or not, it is hard to dismiss the importance this anger has for policy analysis. Angry individuals get utility from the punishment inflicted on those that make them angry. So, one could argue that a law that criminalizes behavior that induces anger has a direct positive effect on the ex post utility of angry individuals. More importantly, the elimination of anger-inducing behavior seems useful for social cohesion. In the absence of well-defined social welfare functions that incorporate this concern,

one might wish to treat anger as a loss that can be triggered by contracts among third parties, and thus as a kind of externality.

Even if one accepts both that overt proofs of limited altruism cause anger, as in Rotemberg (2007), and that lending terms that are seen by outside observers as harming borrowers are viewed as signs of insufficient altruism,³⁰ there is still the question of which practices should be forbidden. One advantage of limiting prepayment penalties and negative amortization is that these features of mortgages may be ones that unsophisticated borrowers do not understand at the time they sign these contracts. Particularly for mortgages with “teaser rates,” borrowers may not realize that the existence of prepayment penalties will prevent them from refinancing cheaply once the period of low rates expires. Similarly, borrowers of negative amortization mortgages may be lulled into complacency by the affordability of monthly payments without noticing that their main payments lie in the future. Thus, the elimination of these practices may prevent borrowers from signing contracts that ultimately cost them their house.

On the other hand, these limitations also make borrowing more difficult for some individuals whose risk of default is low. Prepayment penalties, for example, should reduce interest rates and make mortgages more affordable at first, with this benefit to the borrower being offset by a reduction in the likelihood of refinancing when interest rates drop. Negative amortization mortgages, meanwhile, may well be very useful for borrowers that expect their income to rise over time. Rather than forbidding practices that might be advantageous to borrowers, it would seem more desirable to target only those loans that are likely to end in tears of regret and anger. In the case of housing loans, the pain is likely to be particularly acute for those whose loan ends in foreclosure.

One policy that therefore appears to be somewhat desirable is to require lenders to compute the probability that a loan will end in foreclosure, with penalties attached when this computation is not credible. Since regulators and credit agencies also care about these probabilities (albeit for different reasons), widely acceptable models for computing this risk should become available. These models would obviously integrate features of mortgages such as prepayment penalties and negative amortization, both of which could raise the probability of default.

Once mortgage originators are forced to compute these probabilities, there are two different regulatory regimes that can be envisioned. In the first, lenders would be required to disclose the results of this computation to borrowers. In the second, borrowers would simply not be allowed to sign loans whose probability of ending in foreclosure exceeds some critical number. The former solution would seem preferable except for the fact that naïve borrowers might not take the warning implicit in these calculations seriously.

The Rationale for Low Inflation

Anti-price gouging legislation and limitations on mortgages are controversial policies. By contrast, there is substantially more agreement that inflation should typically be low. The question I tackle here is why this consensus is so strong. One reason that flows directly from the earlier analysis is that inflation increases consumer regret. The reason it does so is that inflation increases the frequency with which prices rise, and each price increase has the potential to lead consumers to wish they had bought the good earlier. Thus, a policy of low inflation lowers regret, and thereby increases well-being.

Di Tella, MacCulloch, and Oswald (2001) demonstrate that, indeed, inflation reduces reported “life satisfaction.” Equally remarkable is the fact that in opinion polls inflation has historically often been seen as the most serious problem faced by the United States. Fischer and Huizinga (1982) display Gallup Poll data showing that there were more people seeing inflation as a more serious problem than unemployment in 1951, when inflation was about 6 percent and unemployment about 3.3 percent. While the rank of the two problems reversed in the late 1950s and early 1960s, inflation became more important once again starting in the mid-1960s, when it was equal to about 3.5 percent. Hibbs (1979) computes the determinants of the relative importance of these two issues. His conclusion is that, when the unemployment rate is unchanging, more than 50 percent of respondents see inflation as a more serious problem than unemployment as long as inflation exceeds 6 percent.

The question is whether the depth of people’s concern for inflation would make sense if people cared about prices only in the manner that is

standard in economic models. In other words, could consumers who see prices exclusively as indicators of what they can afford be as perturbed by inflation? Inflation is known to have two consequences. The first is that it leads people to economize on money balances. However, because total expenditures on money balances are modest, this effect should be modest as well.

The second is that inflation increases the volatility of relative prices because different firms do not adjust their prices at the same time. From the point of view of conventional welfare measurement, Rotemberg and Woodford (1997) show that this is an important reason to keep inflation low. What is less clear is that this explains why typical consumers want inflation to be low.

To see this, it is worth recalling that for fixed real income in terms of a particular good, price volatility is actually good for consumers. Even with dispersed relative prices, consumers can afford the bundle they would buy if all prices were set at their mean levels. Consumers can do even better, though, by tailoring purchases to the particular pattern of relative prices that they face. The volatility and dispersion of relative prices induced by inflation can therefore only hurt consumers if it reduces their mean real income.

As it happens, inflation does reduce real income for a given level of employment. The reason is that those firms that charge a low price sell more since firm output is determined by demand. This reduces the average income of firms, and indirectly that of workers. Moreover, the law of diminishing returns implies that the firms that sell more have lower labor productivity, so that price dispersion across firms implies that a disproportionate fraction of goods is produced by firms whose productivity is relatively low. These effects reduce real income for a given level of employment and thereby also imply that inflation raises the level of employment that is needed to produce a given level of real income. This required increase in work effort (and reduction of leisure) is the reason why Rotemberg and Woodford (1997) find that inflation reduces welfare even for a given level of GDP. What is not implied by this analysis is that people will be aware that inflation is reducing GDP for a given level of employment, and much less that this is the reason they dislike inflation.

Indeed, the opinion polls analyzed by Fischer and Huizinga (1982) suggest that consumers do not regard inflation as having had a major effect on their real income. Rather, what bothers them about inflation seems to be something else.

5. Conclusions

This paper has considered three psychological reactions to prices. The first is that consumers are unmindful of them. The second is that consumers experience regret upon discovering that they paid more than they could have if they had acted differently in the past. The third is that people become upset when they see prices they deem unfair. I have tried to connect these reactions by noting that regret can be enhanced if consumers do not pay close attention to prices, and that feelings of regret can cause anger if consumers conclude that the price-setting firms were not sufficiently empathetic towards their regret.

These reactions complicate the price-setting problem of firms. On the one hand, consumer naïveté opens up many opportunities for exploiting consumers. On the other hand, consumers can become angry when they see firms that seem uncaring in their willingness to cause regret. In some cases, this potential for anger is sufficient to discipline firms. The result is that certain pricing patterns can be explained as attempts to avoid arousing such anger.

At the same time, however, it is clear that some firms are willing to anger their customers, particularly in the event of pricing decisions made following natural disasters. Similarly, consumers are upset when lenders contribute to the loss of other people's homes. This paper suggests that these reactions can explain why consumers seek legislation that limits the freedom to set prices in credit markets and in markets where emergencies suddenly raise consumer demand for certain goods and services. The paper also shows that such public policies can be Pareto optimal in the presence of these reactions. Lastly, I have suggested that consumer regret at not having purchased goods right before a price increase can be reduced by curtailing inflation and that this is a reason for central banks to pursue price stability.

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Notes

1. Di Tella and McCulloch (in this volume) show that self-reported “life satisfaction” is more correlated than self-reported happiness with macroeconomic variables. Interestingly, life satisfaction does not appear to have the same emotional intensity as happiness.
2. For a discussion of the determinants of anger, see Berkowitz and Harmon-Jones (2004) and the symposium that follows.
3. For a model of this change in preferences, see Rotemberg (2007). For neurological evidence that some pleasure centers of the brain light up when people harm those that have behaved selfishly in an economic exchange, see De Quervain et al. (2004).
4. For a paper that discusses this literature, see Xia, Monroe, and Cox (2004).
5. As discussed in Rotemberg (2007), the idea that people react with anger when rejecting the hypothesis that others are minimally altruistic can also explain other field and experimental findings.
6. For a survey that obtains somewhat similar results, see Feinberg, Krishna, and Zhang (2002). They show that subjects that have purchased a brand’s product in the past (in the sense of allocating “points” to it), reduce these purchases if this brand offers a discount to new customers. They also show that subjects that used to purchase brand *b* reduce their purchases of this brand if brand *a* offers a discount to its past customers. They see this reaction as being driven by “jealousy,” though this effect might also be due to an assessment that brand *b* is not sufficiently altruistic in its promotional strategy.
7. See Scott Barancik, “Hotels treated guests like family,” *St. Petersburg Times*, August 18, 2004. Available at <http://pqasb.pqarchiver.com/sptimes/access/680124751.html?dids=680124751:680124751&FMT=FT&FMTS=ABS:FT&date=Aug+18%2C+2004&author=SCOTT+BARANCIK&pub=St.+Petersburg+Times&edition=&startpage=1.D&desc=Hotels+treated+guests+like+family+Series%3A+HURRICANE+AFTERMATH> (accessed August 18, 2009).
8. Lois M. Collins, “Disasters reveal the stuff we’re truly made of,” *Deseret Morning News*, September 16, 2004, A19 Opinion. Available at http://www.deseretnews.com/cgi-bin/cqcggi_plus/@plus.env?CQ_SESSION_KEY=QSOBFGWCTAXK&CQ_CUR_DOCUMENT=1&CQ_TEXT_MAIN=YES (accessed August 28, 2009).
9. Martha Musgrove, “Free market? It’s price-gouging—and it stinks!,” *Miami Herald*, September 1, 1992, 39A Editorial.
10. These incentives are absent in the Barro and Romer (1987) analysis of ski-lift pricing because they assume that firms operate at a capacity constraint (though their analysis would of course be equally valid when ski areas operate at less than full capacity as long as marginal cost were zero) so that consumers are unable to increase total output.
11. This regret-based explanation is not so much an alternative to the “mental accounts” hypothesis of Prelec and Loewenstein (1998) as a potential reason why people create mental accounts in the first place. One advantage of carrying mental accounts for different categories of consumption is that if one does so successfully, they may eliminate regret from marginal purchases in a category as long as total purchases within the category are within the amount budgeted in the mental account.
12. See Sheshinski and Weiss (1977) for an early formalization of this idea and Golosov and Lucas (2007) for a recent one.
13. As shown by Gertler and Leahy (2006), if idiosyncratic shocks are sufficiently large and recurrent, firms with administrative costs of changing prices will mostly change their prices in response to such idiosyncratic shocks. Thus, the probability of a price change will be essentially constant if the distribution of idiosyncratic shocks is constant.
14. While these models do not describe the literal behavior of individual firms, they are both tractable and capture two essential features of sticky prices. The first is that this stickiness increases the correlation of current and past prices. The second is that if firms are forward-looking, the rigidity of prices leads current prices to be more correlated with the future determinants of prices. In spite of their imperfections at describing micro phenomena, these models may thus remain useful as vehicles for organizing macroeconomic data.
15. In fact, Hoch, Drèze, and Purk (1994) show that stores that are known for EDLP also sell a high fraction of their goods in special promotions, though these discounts tend to be less deep than those at high-low stores.
16. See Steve Bousquet, “Few suits in cases of price gouging,” *St. Petersburg Times*, August 16, 2006. Available at <http://pqasb.pqarchiver.com/sptimes/access/1095765241.html?dids=1095765241:1095765241&FMT=FT&FMTS=ABS:FT&type=current&date=Aug+16%2C+2006&author=STEVE+BOUSQUET&pub=St.+Petersburg+Times&edition=&startpage=1.B&desc=Few+suits+in+cases+of+price+gouging> (accessed August 18, 2009).
17. See Allison North Jones, “West Palm Days Inn settles storm gouging suit,” *Tampa Tribune*, October 4, 2004, 3 Metro.
18. Barry Meier, “Do higher prices for gasoline mean drivers were gouged?,” *New York Times*, October 6, 1990, Section 1, p. 29. Available at <http://www.nytimes.com/1990/10/06/style/consumer-s-world-do-higher-prices-for-gasoline-mean-drivers-were-gouged.html?scp=9&sq=electric%20generators%20%245,000&st=cse>.

19. Ibid.
20. The implausibility that in this setting selfish considerations lead to political mobilization may also cast some doubt of its importance in other settings where observers have been quick to presume that self-interest is responsible for government-induced inefficiency. See Rotemberg (2003) for a discussion of these issues in the context of tariffs.
21. See Ho and Pennington-Cross (2007) for a description of the numerous state laws that strengthen HOEPA either by changing the high cost trigger or by curtailment specific practices including the use of “balloon” payments when the mortgage comes to an end.
22. U.S. Code 15, 1639(h).
23. This is similar to the perspective of Gruber and Köszegi (2001) who compute social welfare by assigning their “long-run preferences” to time-inconsistent smokers.
24. A libertarian might further claim that A himself is made worse off by the simple act of restricting his choice.
25. There were over one million entries for this term on Google as of July 2007.
26. “Protesters at KC Fed,” *Kansas City Star*, June, 7, 2007, C3 Business.
27. This report is available at <http://www.huduser.org/publications/pdf/treasrpt.pdf>.
28. See <http://www.cambweb.org>.
29. http://www.mortgagenewsdaily.com/Mortgage_Fraud/Predatory_Lending.asp (accessed August 19, 2009). These definitions are related in that, for example, fraudulent loans do not benefit borrowers. These definitions are not identical, however, in that a borrower might well not benefit from a loan even if its interest rate is properly “risk-based.” What matters, of course, is not how different people define the concept but which aspects of lending induce the most revulsion. Empirical research on what upsets people about different loans is urgently needed.
30. Competition among lenders, so that their margins are low, implies that their altruism must be particularly low if they are willing to impose large costs on borrowers. This need not imply that monopoly lenders will be seen as more altruistic if they extend such loans because their monopoly status should lead them to value the marginal utility of poor borrowers highly relative to their own.

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Comments on "Behavioral Aspects of Price Setting and Their Policy Implications" by Julio J. Rotemberg

Jonas D. M. Fisher

Rotemberg's paper fits into a theme found in his other work in macroeconomics, which is to explore ways in which the price mechanism may break down. His examples and analysis show how various aspects of human behavior might lead to the breakdown of the traditionally accepted price mechanism. Rotemberg distills from the behavioral literature a short list of phenomena which he thinks are important for understanding prices. He then describes several aspects of consumer behavior which he thinks are hard to understand from the perspective of traditional economic thinking and argues that such behavior can be explained by these behavioral phenomena.

In my view the overall case Rotemberg makes for resorting to behavioral thinking is weak. I make my case in two ways. First, I describe simple neoclassical alternatives to his behavioral interpretations of the evidence. Second, in the case of sticky prices, I argue that the empirical evidence is not as damning to conventional theories of sticky prices as Rotemberg would like us to believe, and that the behavioral theory he describes is actually inconsistent with the available empirical evidence.

Rotemberg motivates the importance of the behavioral concepts he describes using the straw man device, his being *homo economicus*, the rational, clear-thinking automaton who populates traditional economic models. This straw man is torn down in several steps. First, Rotemberg describes empirical evidence that consumers do not remember the prices they paid for recently purchased goods. This evidence is interpreted to mean that certain economic outcomes are not driven by the actions of *homo economicus*. I am skeptical of this interpretation. Certainly it is the

case that there are some prices consumers do remember. People remember the price they paid for their house, their car, and other big-ticket items. So in some situations consumers remember the prices they paid, and in other less important situations they do not. This is not particularly damning for neoclassical economics. It merely suggests that information is costly to process.

Accepting that there are limitations on how we process information does not mean that economists need to abandon their neoclassical principles. One way of interpreting the evidence is that individuals weigh the costs and benefits of performing a detailed analysis of the prices they face, and sometimes choose to avoid paying the costs of processing the price information. Such a theory would suggest paying more attention when the price of the good is likely to be very high. Costly information acquisition can and probably should be incorporated into our models. Observing that human beings do not process information perfectly is not in and of itself a compelling reason to abandon neoclassical principles. However, the notion that consumers face difficulties processing information is important for Rotemberg's analysis because he needs people to make mistakes so that they can later regret these imperfect decisions.

The next behavioral concept Rotemberg introduces is that economic agents often react emotionally to the prices they face. For example, consumers sometimes express anger when prices are perceived as being "unfair." Human beings are bundles of emotions. We respond emotionally to everything around us. So it should not be surprising that in the realm of economic activity emotional behavior is observed. Happiness is an emotion, and this is conventionally thought of as being well-captured by the utility function formulation. It is not clear why anger, a form of displeasure, is not consistent with the utility function formulation. So the presence of anger on the part of economic agents is not obviously damning for conventional modes of economic thinking.

What is crucial for Rotemberg's analysis is that there is feedback from emotional behavior to observed patterns of pricing by firms. In Rotemberg's view of the evidence, consumers get angry at firms that make them regret a purchasing decision, they act in such a way as to avoid making a purchase that they may later come to regret, and firms set prices in such a way as to avoid angering their customers. An alternative view is that

this observed anger is merely a reflection of some displeasure being experienced along with the bundle of goods being consumed. That firms seek to supply goods that yield pleasurable outcomes for consumers is a basic premise of neoclassical economics. The buying experience is part of the bundle of goods that is consumed when making a purchase. That firms act to make this buying experience pleasurable, for example by ensuring that the experience does not generate "anger" among consumers, seems entirely within the realm of standard economic analysis. It does not require a special explanation of firm behavior.

Another point worth making here concerns Rotemberg's focus on consumer-firm interactions. Indeed many of the examples he describes are based on evidence collected from grocery stores. It seems important not to place too much emphasis on such evidence—there is a huge amount of economic activity that takes place outside of grocery stores! For example, a huge fraction of economic transactions occur on a business-to-business basis. I am not convinced that emotions like regret and anger are very important in these situations. Business-to-business transactions are inherently cutthroat. Of course, relationships are formed by agents of firms, but ultimately business is about making a profit. Anger and regret may be experienced as a by-product of this process, but these emotions are not inconsistent with the pursuit of profit. Again, humans essentially are bundles of emotions, and it would be surprising if we did not observe these feelings arising in the course of engaging in economic activity.

The final key behavioral idea described in the paper is that people expect a minimal level of altruism from those with whom they conduct transactions. According to Rotemberg, firms chose to display a certain level of altruism because this is what consumers expect of them. He brings up the recent example of the iPhone in which there was consumer outrage at Apple dropping the price soon after the phone was put on the market. I actually think this example has a simple explanation in terms of implicit contract theory. It is natural to express anger when one party to a contract has reneged on the terms of the contract. It is well-known that the prices of new consumer durable goods are initially high and then fall over time. All consumers purchasing the iPhone should have been aware that the price would eventually fall. The implicit deal consumers who purchased the iPhone at the initial price had with Apple was that

they would have exclusive use of the iPhone for a certain period of time because they paid a high premium price. The “mistake” Apple made was in dropping the price by \$200 only two months after the iPhone first went on sale. Apple’s response to the ensuing anger, which was to provide a \$100 coupon for future Apple purchases, was its way of admitting it had violated the implicit contract. By making the admission, it was hoping to retain its reputation as an honest broker in the marketplace, thus protecting future sales. There is nothing about this situation which requires one to resort to behavioral concepts to explain the outcome. Apple’s response certainly had nothing to do with altruism.

Yet I do not want to suggest that firms never display altruism. An example not raised in Rotemberg’s paper is that firms advertise their charitable giving. Such behavior presumably is intended to convey a favorable impression of the firm. Such positive impressions are part and parcel of the buying experience and easily fit into a standard utilitarian analysis.

Of course, in many commercial transactions consumers do not expect any altruism whatsoever on the part of firms. We are all familiar with the Latin phrase, “caveat emptor,” which in plain English means “let the buyer beware.” For example, no one expects a used-car dealer to behave altruistically toward its customers. And with firm-to-firm transactions, it seems unlikely that there is an expectation of altruism among either party. Without incorporating a universal expectation of altruism, it is hard to see how one can build a theory of economic behavior with wide-ranging applications. It also seems unwise to build a separate theory for every case.

The interactions between firms and workers may be an important exception in which altruism does play a role. Rotemberg (2008) has used the idea that firms are expected to deliver a minimum level of altruism to describe firm-worker interactions. In his model Rotemberg is able to generate a weak response of wages to productivity shocks, an empirically appealing result. The difficulty I have with such a model is that firms behaving in a way that appears altruistic may just reflect a particular remuneration strategy. It is not necessary to assume that firms are expected to behave altruistically to account for outcomes that appear altruistic. Indeed implicit contract theory yields outcomes that appear

altruistic but are nothing of the sort. Again, Rotemberg is describing phenomena which he suggests require a behavioral explanation, but actually have a straightforward interpretation in terms of conventional economic thinking.

The final issue I will discuss concerns sticky prices. In this context the straw man is “menu costs.” Rotemberg argues that the recent explosion of microeconomic evidence on sticky prices is inconsistent with a menu cost interpretation. Yet menu costs are probably the most well-developed micro-founded model of sticky prices. So if the evidence does not look good for menu costs then, according to Rotemberg, this leaves an opening for a behavioral explanation. Rotemberg views the evidence as damning for the menu cost view because it reveals that price changes are frequently very small. This is hard to square with Rotemberg’s interpretation of menu costs as reflecting the purely administrative costs of changing every single price. I think the current prevailing view of menu costs is that firms follow pricing strategies, and that menu costs reflect the costs of changing these strategies. Formulating a price strategy involves using high-wage talent. This high-wage talent has better alternative uses of its time. Consequently, firms infrequently adjust their pricing strategies. Any given pricing strategy could involve small price changes over time, so evidence that there are many small price changes is not damning for the modern menu cost view.

Rotemberg describes a behavioral theory which generates acyclical price increases and procyclicality in the fraction of goods with changing prices. The size of price increases is acyclical because firms refrain from increasing the size of their price increases during periods of high inflation to avoid generating consumer regret at having not bought these goods and services at a lower price. To get the prices to rise as much as firms would like during periods of high inflation, firms must change their prices more frequently at such times. Since inflation is procyclical, Rotemberg’s model appears to fit the evidence.

However, the implications of Rotemberg’s behavioral model are inconsistent with other evidence which Rotemberg does not discuss. Specifically, Klenow and Kryvtsov (2008) decompose U.S. inflation into the part due to the fraction of goods whose prices change at a given time, and the

average amount by which such prices change. They find that roughly 90 percent of the variance of inflation is due to variations in the average size of price changes. That is, the size of price changes is actually procyclical, not acyclical as suggested by Rotemberg, and variations in the number of firms changing prices is not important to price dynamics at all. These findings contradict Rotemberg's behavioral theory of inflation.

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Comments on "Behavioral Aspects of Price Setting and Their Policy Implications" by Julio J. Rotemberg

John Leahy

Julio Rotemberg has written a very interesting and provocative paper. I find it hard to disagree with his premise that people respond emotionally to prices. I sympathize with the editor of the *Miami Herald* whom, according to Rotemberg, wanted to "punch out those price-gouging creeps" who raised prices after a hurricane. Raising prices may help to efficiently allocate resources, but there is also something unsavory about making a profit off of the misfortunes of others. Given that consumers react emotionally to prices, it makes sense that firms would respond to these emotions. The fallout from Apple's decision to cut the price of the iPhone by 40 percent just a few months after its introduction illustrates many of the themes in Rotemberg's paper. There was an emotional response among those who had bought the iPhone at the original price. Internet message boards were full of angry customers, some of whom felt betrayed by Apple. These consumers felt that Apple had let down the early adopters who had supported the company. In response, Apple felt obliged to issue rebates to customers who had paid the higher price.

I am going to focus my comments on the macroeconomics of pricing. In my mind the issue is not whether people respond emotionally to prices, but whether we need to include these responses in our macroeconomic models of pricing. Macroeconomics is all about simplification. The world is quite complex, and there are a lot of very real phenomenon that we could potentially include in our models. This makes the criterion for inclusion quite strict. A theory must help explain some first-order fact regarding the macroeconomic time series. I am not yet sure that the emotional responses Rotemberg is pushing meet this test at this time.

Behavioral theories have a long history in the macroeconomics literature. In some sense macroeconomics owes its existence to behavioral theories of pricing. Keynes's major amendment to the classical model was the consideration of money illusion and downward price rigidity.

Modern macroeconomics, however, has attempted to move beyond simply assuming that prices are sticky. Instead modern macroeconomics tries to derive rigid prices as the outcome of an explicit model of price determination. Some models motivate price inertia with adjustment costs, some with decision costs, and others with imperfect information. Rotemberg has been a major contributor to this research program.

In this paper, Rotemberg argues that there are major gaps in this research program. He mainly attacks theories in which there is a fixed cost of changing prices, theories commonly referred to as "menu cost pricing models." My guess is that Rotemberg questions these theories because they are the most advanced. These are the theories for which we have the most sophisticated models. These are the theories for which we have made the most progress in matching theory to data. Daniel Levy and his co-authors (1997) have gone out and tried to measure the cost of changing prices. Other people have calibrated models to fit the data on the size and frequency of price adjustment. You can take these theories and plug them into a general equilibrium macroeconomic model such as those of Christiano, Eichenbaum, and Evans (2005) or Smets and Wouters (2003)—models that do a good job or replicating macroeconomic time series—and then you can analyze counterfactuals and do policy experiments.

Rotemberg criticizes menu cost models on three levels. First, menu cost models are unable to explain small price changes. Second, menu cost models counterfactually imply that the size of price changes should increase with the rate of inflation. Third, survey respondents tend to prefer other explanations of price inertia. Personally, I am not overly troubled by these failures. The pricing literature has dealt with each of these issues. Let me address them in turn.

Let's begin with the issue of small price changes. The first thing to keep in mind is that the average price change in the U.S. economy is surprisingly large. We have been placing price stickiness at the center of our macroeconomic models since Keynes, and it is only very recently that we have had any access to data on a broad cross-section of prices. Now

several researchers, most notably Klenow and Kryvtsov (2008), have obtained access to Bureau of Labor Statistics pricing data. This is a sample of the prices that go into the Consumer Price Index. The data begin around 1988 and represent the economy as a whole. The average price change in this data set is between 8 to 13 percent, depending on whether or not you include sales and product substitutions. Those are big price changes, and these price changes are also relatively frequent. Firms tend to adjust prices every four to seven months (again the difference depends on how one treats sales). Given that the annual inflation rate has been around 2 to 3 percent since 1988, these facts imply vast heterogeneity in price movements. Individual prices are moving all over the place. There are big jumps up and big jumps down. But explaining small price changes is not our biggest worry.

In an effort to explain large price changes, menu cost models typically rule out small price changes. This practice is troubling, but the literature has attempted to fill this gap in several ways. Some models incorporate time-varying costs of price adjustment. In other models, small and large price changes coexist because firms sell multiple goods and the costs of changing prices may be spread across goods. The price of one good may change a lot, while the price of the other one changes only a little. Decision costs and imperfect information can also give rise to small price changes. In these models some of the costs of price adjustment are born before the firm learns the true state of the world, then these costs become sunk costs when the price adjustment takes place. Before learning the true state, the firm expects to change its price by a large amount. In some cases, however, it turns out that only minor adjustments are necessary.

The second criticism is that in the data the size of changes does not respond to inflation. Let's think again about the data. There is massive heterogeneity in price adjustments. Most price changes do not take place in response to inflation. Most price changes have to do with responses to the idiosyncratic situation of the firm. It would not be surprising if the idiosyncratic situation of the firm were uncorrelated with inflation. To observe a correlation between the size of price adjustments and inflation would therefore require a large change in the inflation rate. In such cases, there are lots of other changes going on in the economy.

Third, menu costs don't come up big in surveys. In my mind, it is not obvious that they should. The Bank of England survey (Hall, Walsh, and Yates 2000) asks how important a theory is for price adjustment. Blinder's (1991) survey asks how important a theory is for the speed of price adjustment. If I am thinking about what's important for pricing, then costs, demand, and competitors' behavior are the first three things I'm going to come up with. Menu costs are tiny. Menu costs are supposed to be tiny. The entire point of the papers by Mankiw (1985) and Akerlof and Yellen (1985) was that second-order costs of adjustment have first-order effects. It is therefore not surprising that menu costs are not among the primary determinants of prices.

In my view, the survey evidence is actually favorable to menu cost theories. In the Blinder survey 70 percent of firms report that they face price adjustment costs. Moreover, survey after survey comes to the conclusion that price reviews happen more frequently than price changes. It would be quite surprising if these reviews yielded no information. The only explanation is that firms encounter some cost to changing prices in response to this information.

Now let's return to Rotemberg's theories. As I said above, one of the strong points of the menu cost theory is that you have an explicit model. You can write that model down, parameterize it, and take it to the data. I personally find Rotemberg's models and stories fascinating. I believe pretty much every one of them, but his behavioral theories of pricing have not been developed to the same level as the menu cost model.

What do these behavioral theories of pricing need to get up to this level? First of all, we need some canonical forms. We need to figure out which behavioral theories are important and develop general formulations of these theories that are applicable to lots of different situations. We need parameters that we can think about and measure. For example, Rotemberg began by arguing that people don't pay attention to prices and that they have no idea what they pay. Later, however, when discussing regret, Rotemberg argued that even small changes in prices might anger consumers. Both effects are true, but both probably do not simultaneously coexist in the same situation. We need to know when people do not pay attention to prices, and when people pay a lot of attention to prices. We need to know when people regret some action or inaction, and when people let things be. We then need to figure out which is the theory we really want

to take seriously. Macroeconomics has developed by looking for general explanations that work most of the time. It avoids using this story for one situation and that story for another situation. We need canonical theoretical representations that fit a variety of different examples.

Once we get a canonical formulation we can think about measurement and calibration. For example, with hyperbolic discounting, we know how to write down the problem. We know how to solve it. We can think about measuring the parameters of the model, or we can estimate these parameters by fitting the model to data. Most of the theories that Rotemberg has surveyed are not quite there yet. We do not have the decision problem written down. We don't know what parameters are important. We do not know how to go out and measure them. Right now, these are simply interesting stories. There is a lot of work that needs to be done before we can plug these theories into the pricing equation of a dynamic general equilibrium model.

Rotemberg's favorite theory, which I find attractive, is based upon a combination of regret, altruism, and anger. The idea is that when prices change, consumers regret either that they are paying too much today or that they paid too much yesterday. Altruistic firms, because they care about their consumers, take this regret into consideration when setting their prices. Consumers, expecting firms to be altruistic, react with anger when firms fail to behave altruistically. This expectation forces firms that are less altruistic to mimic the altruistic firms.

Among all of the theories discussed in the paper, this is the one that is the most fully formulated. Rotemberg has fleshed out the model elsewhere (Rotemberg 2008). The formulation, however, looks a lot like the menu cost model. I think that this is intentional. Rotemberg wants to keep what is good about menu cost pricing, while extending the theory in a direction that he sees as important. The main difference between Rotemberg's formulation and the menu cost model is that in the standard menu cost model the cost of changing prices is independent of the size of the price adjustment, whereas in Rotemberg's formulation the effective cost of the price adjustment increases as the size of the price adjustment increases. This extra degree of freedom is what allows Rotemberg to match certain facts.

I am skeptical, however, that this extra degree of freedom will be of great importance in macroeconomic models. There are many nonlineari-

ties in the menu cost model. None of these have been shown to matter in the aggregate data. What is important about the menu cost model is that it generates inertia that is first-order in magnitude. It is what allows these theories to explain the real effects of money on output and to fit the impulse responses of macroeconomic time series. Rotemberg adds another nonlinearity to the model. My guess is that the practical implications of Rotemberg's model will be very similar to the practical implications of the menu cost model. There may be differences, but these will be second-order.

Where I see the potential gains from Rotemberg's line of research is not in building better positive models of the economy, but in interpreting the welfare implications of the models that we already have. The models we have do a reasonable job of fitting the data, but the welfare implications are a bit embarrassing. Our models imply that business cycles are not very costly, that inflation is not very costly, and that policy mistakes are not very costly. If you believe the normative implications of these models, you should not be thinking about business cycles at all.

We can't escape the feeling, however, that these things are important. The amount of attention that the public pays to business cycles and inflation is just too great. It seems that these models are missing something big. Maybe what they omit is the emotional reaction to business cycles. Maybe business cycles lead to fear and regret, as in Rotemberg's models. Emotional reactions are nonrival goods. Everyone can fear losing a job without actually losing a job. Everyone can regret not buying at a low price, without anyone buying at a low price. Emotions therefore may magnify the welfare gains and losses of business cycles, and thereby bring the welfare implications of our models more closely in line with common perceptions.

Rotemberg has outlined an ambitious research agenda. Progress has been made, but there is much work to be done. I look forward to seeing where it goes.

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Household Savings Behavior in the United States: The Role of Literacy, Information, and Financial Education Programs

Annamaria Lusardi

Americans are increasingly in charge of their own financial security after retirement. With the shift from defined benefit (DB) to defined contribution (DC) pension plans, workers have to decide not only how much to save for retirement but also how to allocate their pension wealth. Moreover, in recent decades the complexity of financial instruments has increased and individuals have to deal with new and more sophisticated financial products. How well-equipped are Americans to make their own saving decisions? Do they possess adequate financial literacy? Are they informed about the most important components of saving plans? Do they even plan for retirement?

This paper shows that a large percentage of U.S. workers has not thought about saving for their retirement—even when this retirement is only five to ten years away. Consistent with the evidence on a lack of planning, half of older workers know little about their pension plans and the rules governing Social Security benefits. Moreover, most individuals lack an adequate knowledge of basic financial concepts, such as how interest compounding works, the difference between nominal and real values, and the basics of risk diversification. Financial illiteracy is widespread among the general population, and is particularly acute among specific demographic groups such as women, African-Americans, Hispanics, and those with low levels of education. Notwithstanding the low levels of financial literacy that many individuals display, very few rely on the help of experts or financial advisers to make saving and investment decisions.

Low financial literacy and a lack of information affect one's ability to save and to secure a comfortable retirement; ignorance about basic

financial concepts can be linked to a lack of retirement planning and a lack of wealth accumulation. In the United States several initiatives have been undertaken to foster saving and financial security, such as educating workers in order to improve their financial literacy and knowledge about pensions, automatically enrolling workers in pension plans, and simplifying their pension enrollment decisions. While these programs had some impact on savings behavior among U.S. households, much more can be done to improve their effectiveness.

This paper focuses on how much individual Americans plan for retirement, what they know about the variables that should enter into a savings plan, and the level of financial knowledge and numeracy that they possess. While many of these characteristics have been overlooked in previous studies on saving, they are important predictors of household savings behavior. The discussion is organized as follows. Section 2 provides an overview of the difficulties inherent in making saving decisions. Section 3 examines the evidence on retirement planning, U.S. workers' knowledge of pension and Social Security plans, financial literacy, and reliance on the advice of experts to make their saving decisions. Section 4 reviews the current initiatives to encourage saving and improve financial security through financial education programs and automatic enrollment of workers in pension plans and other programs. Section 5 discusses the major findings and offers suggestions for improving U.S. public policy designed to foster financial literacy and financial security, especially in retirement.

1. Theoretical Framework

The theoretical framework used to model household consumption and saving decisions posits that rational and foresighted consumers derive utility from smoothing consumption over their lifetimes. In the simplest format, the consumer maximizes a lifetime expected utility subject to an intertemporal budget constraint. According to this model, lifetime resources, the distribution of these resources, and the individual's age play a critical role in his or her saving decisions. Thus, those people facing an upward sloping age-income profile will borrow when they are

young in order to smooth their consumption over the life cycle. Similarly, those who have generous pensions may not need to accumulate a lot of extra private savings to provide for the years when they stop working. Individual choices, such as time preferences, also play an important role in decisionmaking. Those persons who place high value on the present will save less and consume more today than individuals who discount the future less heavily.

However, even in this most basic formulation of the household savings decision, the requirements for making these decisions are demanding. Individuals have to collect and make forecasts about many variables in the future, ranging from what income they can expect to receive from Social Security and other pension plans to interest rates and expected inflation, just to name a few. Moreover, they have to perform calculations that require, at the minimum, an understanding of compound interest and the time value of money.

While the majority of previous studies on saving decisions have focused on modeling lifetime resources and preferences in the way that best captures the characteristics of the individuals and the economic environment, including the fact that predictions about the future are inherently uncertain,¹ few studies have recognized that for most people making saving decisions is a very difficult task. Individuals may have to spend considerable amounts of time and effort searching for all the information required to make good saving decisions. Moreover, individuals may not possess the skills and ability to perform the calculations inherent in devising a savings plan.

2. Planning, Information, Financial Literacy, and Financial Advice

Do U.S. Households Plan for Retirement?

One simple and direct way to examine whether, consistent with the predictions made by theoretical models of savings behavior, individuals do look ahead and make plans for the future is to study the extent of retirement planning that actually takes place among U.S. households. Lusardi (1999) examined this evidence using data from the 1992 Health and Retirement Study (HRS), which surveys U.S. respondents aged 51 years

or older. She finds that as many as one-third of the respondents have not thought about retirement planning at all. While some of this behavior may be perfectly rational,² it is nevertheless surprising that the majority of older respondents have not given any thought to saving for retirement even when they are only five to ten years away from leaving the labor force. This lack of planning is concentrated among specific subgroups of the U.S. population, such as those with low education levels, African-Americans, Hispanics, and women. As a whole, these potentially vulnerable groups are not only less likely to save for retirement, but often do not have a minimum level of precautionary savings to buffer themselves against sudden adverse shocks, such as job loss and out-of-pocket medical expenses (Hubbard, Skinner, and Zeldes 2005).

These findings are not specific to a particular time period. Notwithstanding the many changes in the economic environment, including the increased supply of financial products aimed to facilitate retirement planning, a lack of planning is still prominent among the current population of older respondents. Using data from the 2004 HRS and concentrating on respondents who are 51 to 56 years old, Lusardi and Mitchell (2007a) find that close to 30 percent of respondents also have not given any thought to financing their retirement.

To make a tighter connection with the theoretical framework for saving described earlier, Lusardi and Mitchell (2006) devised a special module on retirement planning that was added to the 2004 HRS. In that module, they specifically asked respondents whether:

- “*they have ever tried to figure out how much their household would need to save for retirement?*”

To those who answer affirmatively to this question, they further asked whether:

- “*they were able to develop a plan?*”

and to those who did so, they asked whether:

- “*they were able to stick to this plan?*”

This module has the advantage of measuring different types of planners, from those who merely tried to calculate their saving needs (simple planners) to those who were able to develop and carry through with their

plans (committed planners). The findings are not much different when using this alternative (and perhaps more appropriate) measure of planning: as many as 31 percent of older respondents in the HRS module do not plan for retirement. However, the percentage of planners decreases significantly when moving from simple to committed planners: only 18 percent of respondents were able to develop a savings plan and stick to that plan. This finding underscores the fact that not only have many U.S. families never attempted to devise a savings plan, but even among those who do plan, not everybody was able to follow through with their plan. In other words, few people make saving calculations and saving plans for retirement, and even fewer succeed in implementing those plans.

These findings regarding a lack of financial planning for retirement have been confirmed in other surveys. For example, using data from a representative sample of U.S. workers from the Retirement Confidence Survey (RCS) in 1997, Yakoboski and Dickemper (1997) report that only 36 percent of workers have tried to determine how much they need to save to fund a comfortable retirement. However, many of the workers who have done the calculation could not give a figure when asked. Thus, according to this survey, as many as 75 percent of workers have little idea regarding how much money they need to accumulate for retirement. Moreover, consistent with the finding of Lusardi and Mitchell (2007a), the data from the RCS also show that the fraction of nonplanners has not changed much over time (Salisbury, Turyn, and Helman 2001). While planning is strongly correlated with educational attainment, a sizable fraction of nonplanners is present even among respondents with high educational attainment (Ameriks, Caplin, and Leahy 2003).

Financial planning is an important determinant of household wealth. Table 1 reports the distribution of household wealth holdings across different degrees of planning for two household groups of the same age but from different time periods: the early baby boomers (those aged 51 to 56 years in 2004) and the older cohort (aged 51 to 56 years in 1992).³ Planners have substantially more wealth than nonplanners: looking at the median levels of household net worth, planners accumulate more than double the amount of wealth achieved by nonplanners. The differences are even larger in the first quartile of the wealth distribution. For many households, a lack of financial planning is tantamount to a lack of

Table 1
Financial Planning and the Distribution of U.S. Household Net Worth
(in 2004 U.S. dollars)

A. Early Baby Boomers: Aged 51–56 Years in 2004					
Group	% of Sample	25th Percentile	Median	Mean	75th Percentile
Planning					
Hardly at All	27.9	9,000	79,000	315,579	271,000
A Little	17.0	62,800	173,400	356,552	390,500
Some	27.7	51,000	189,000	365,354	447,200
A Lot	27.4	54,000	199,000	517,252	470,000
B. Older Cohort: Aged 51–56 Years in 1992					
Planning					
Hardly at All	32.0	10,100	76,910	224,3110	200,610
A Little	14.3	37,700	126,560	343,110	292,170
Some	24.8	71,360	172,340	340,340	367,300
A Lot	28.9	71,390	173,690	353,520	356,800

Source: Adapted from Lusardi and Mitchell (2007a).

Note: All data weighted using HRS household weights. Total net worth is defined as the sum of checking and savings accounts, certificates of deposit and Treasury bills, bonds, stocks, IRAs and Keoghs, home equity, second homes and other real estate, business equity, vehicles and other assets minus all debt.

savings. However, there is not much difference in the means. This is due to the fact that in the sample there are several wealthy households who have not given any thought to retirement planning. Note that even a small amount of planning goes a long way toward accumulating high wealth holdings; those households who have thought “a little” about retirement hold substantially more wealth than those who have thought “hardly at all” about retirement.

These findings hold true not only for the older cohort in 1992, but also for the early baby boomers in 2004. Thus, the relationship between retirement planning and household wealth accumulation did not seem to be

influenced by changes in financial markets (including the bust in the U.S. housing market in 1991, the boom in the U.S. housing market before 2004, and the late 1980s boom and bust in stock prices) and changes in the supply of products to foster financial planning, including the many financial education programs undertaken by U.S. employers throughout the 1990s.

Yet these statistics do not demonstrate that financial planning leads to higher household wealth, particularly in retirement. Because a lack of planning is disproportionately concentrated among specific demographic groups, it may simply be a proxy for low educational attainment and low income. Moreover, those who have high levels of wealth may also have an incentive to spend time and effort in planning, since they may benefit more from financial planning than households with little or no wealth. On the other hand, wealthy households may not need to give much thought to saving for retirement.

Lusardi (1999) accounts for many determinants of retirement wealth using a long set of demographic characteristics including educational attainment, gender, race, marital status, and also a host of variables that proxy for individual preferences (risk aversion and time preferences), subjective expectations about the future, past negative and positive shocks to wealth and other motives for low wealth holdings (for instance, a weak precautionary and bequest motive). She finds that financial planning continues to be a determinant of the household wealth accumulated close to retirement even after accounting for many other reasons why individual levels of wealth may be low. According to her estimates, at the mean, those who do not plan for retirement hold from 10 to 15 percent less wealth than those who do plan for this event.

However, as mentioned previously, differences are particularly large in the first and second quartile of the wealth distribution rather than at the means. Table 2 reports quantile regressions of the effect a lack of financial planning has on the wealth holdings of the older cohort and the early baby boomers. Figure 1 illustrates how a lack of planning varies across the wealth distribution. Lack of planning is a dummy variable equal to 1 for those individuals who have not thought at all about retirement. For simplicity, the regressions only include the most important demographic characteristics—age, marital status, education, race and ethnicity, gender,

Table 2
Quantile Regressions of the Effect a Lack of Financial Planning has on Net Worth for Older Cohort (Older) and Early Baby Boomer (EBB) Respondents

	25th % Older	25th %EBB	Median Older	Median EBB	75th % Older	75th % EBB
No Planning	-12.495 (3.563)***	-14.390 (4.022)***	-17.233 (4.391)***	-20.025 (8.818)**	-42.059 (7.450)***	-47.362 (21.751)**
High School Graduate	13.241 (4.297)**	-5.132 (6.220)	21.493 (5.151)**	2.733 (13.753)	31.133 (8.563)**	9.228 (31.611)
Some College	19.963 (5.101)**	-4.127 (6.403)	38.655 (6.150)**	20.278 (14.134)	73.552 (10.406)**	44.360 (32.831)
College Graduate	46.990 (6.344)**	51.527 (7.382)**	83.054 (7.691)**	113.995 (16.195)**	188.936 (13.229)**	237.035 (38.294)**
More than College	70.954 (6.847)**	62.327 (7.966)**	121.807 (8.318)**	169.988 (17.136)**	252.906 (14.153)**	441.711 (40.818)**
Hispanic	-10.389 (5.125)**	-13.237 (6.040)**	-13.289 (6.290)**	-18.879 (13.226)	-25.028 (10.651)**	-45.239 (30.783)
Black	-23.053 (4.058)**	-22.463 (4.656)**	-33.550 (4.875)**	-33.360 (10.032)**	-74.087 (8.062)**	-71.828 (24.231)**
Divorced	-31.876 (4.821)**	-28.229 (4.727)**	-41.669 (5.820)**	-53.389 (10.372)**	-47.224 (9.912)**	-91.769 (25.910)**
Separated	-19.096 (8.528)**	-28.862 (9.091)**	-31.846 (9.942)**	-43.898 (18.951)**	-7.757 (16.231)	-80.357 (44.329)*
Widowed	-13.250 (6.799)*	-18.524 (8.414)**	-25.976 (8.313)**	-21.952 (18.043)	10.445 (14.764)	57.775 (48.528)
Never Married	-33.322 (8.055)**	-26.127 (7.075)**	-44.268 (9.714)**	-52.984 (15.418)**	-41.714 (16.204)**	-105.520 (39.251)**
Female	1.985 (3.384)	-9.671 (3.748)**	12.805 (4.171)**	-10.073 (8.174)	23.687 (7.184)**	-13.595 (19.895)
Log of Income	31.160 (1.891)**	30.540 (1.449)**	45.063 (2.577)**	46.719 (3.854)**	61.048 (5.283)**	61.415 (13.278)**
Adjusted R-Squared	0.12	0.11	0.15	0.15	0.17	0.17

Source: Adapted from Lusardi and Beeler (2007).

Note: Even though not reported, these regressions include controls for age, number of children and retirement status. See table 1 for the definition of total net worth. * significant at 5-percent level; ** significant at 10-percent level; *** significant at 1-percent level.

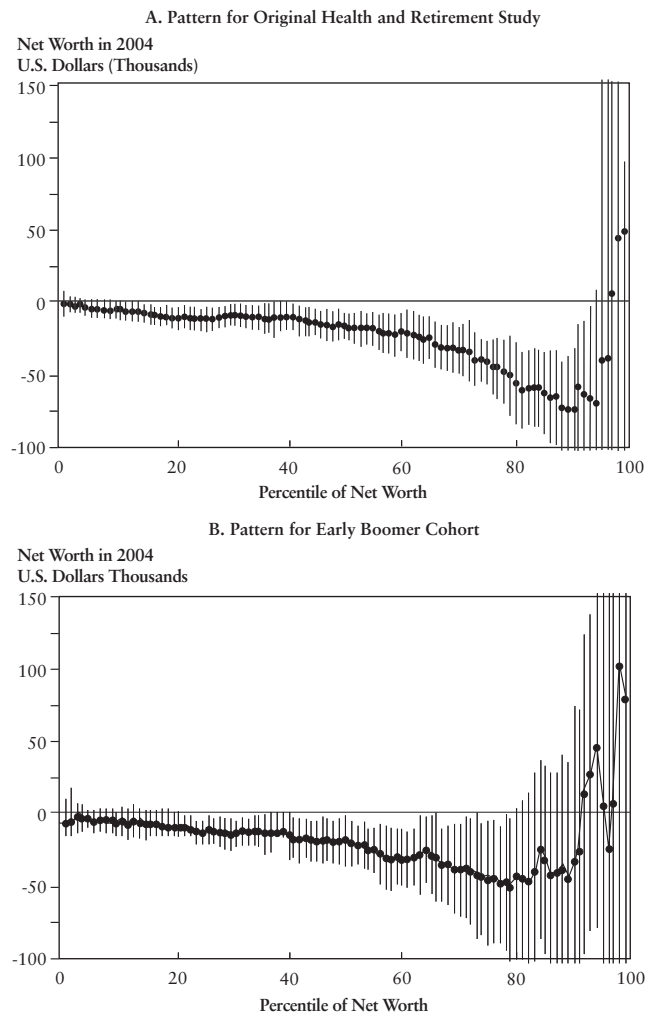


Figure 1

Estimates of the Effect of “Not Planning” on Net Worth by Percentile of the Wealth Distribution

Source: Adapted from Lusardi and Beeler (2007)

Note: The dark line in each panel represents the effect of “not planning” and these estimates are reported at 95-percent confidence intervals.

number of children, retirement status, and income. The coefficient on lack of planning is always negative and statistically significant for each of the three wealth quartiles, indicating that those who do not plan for retirement hold lower amounts of wealth. These estimates are not only sizable but also very similar between cohorts. Looking at the medians, nonplanners accumulate from \$17,000 to \$20,000 less wealth than those who do some planning (whether a little or a lot), which corresponds to holding about 20 percent less wealth close to retirement.

Figure 1 shows the negative effect that a lack of financial planning has throughout most of the wealth distribution. However, the estimates’ sign reverses as we move close to the top of the wealth distribution. Among early baby boomers, as we move past the third quartile of wealth, the effect a lack of planning has first becomes insignificant and then becomes positive rather than negative. Thus, many wealthy households do not plan for retirement. The same is true for the older cohort, even though the sign reversal happens at higher percentiles of the wealth distribution. In other words, there is a lot of heterogeneity in the effect of planning on wealth accumulation, and mean estimates may severely underestimate the effect a lack of planning has across all groups.⁴

The important question, however, is whether there is a causal relationship between financial planning and wealth accumulation. In other words, if someone were to begin planning tomorrow, would he or she end up with a larger amount of wealth because of it? Lusardi (2003) performs a regression similar to the one reported in table 2, but by instrumenting a lack of financial planning with variables measuring planning costs. Specifically, she uses the age difference between the respondent and his or her older siblings as an instrument for planning. Those individuals who have older siblings face lower search and information costs because they can simply learn by watching the behavior of others. Do those who face lower planning costs, and therefore can plan more, accumulate higher amounts of wealth? The answer is affirmative; not only is the effect of planning confirmed, but financial planning becomes an even stronger determinant of wealth.⁵

Lusardi and Mitchell (2007a) use an alternative strategy to pin down the direction of causality between financial planning and amassing wealth. They look at changes in wealth outside of households’ control

and examine whether these changes influence the extent of retirement planning: if households were to become richer, would they plan more as a result of their greater wealth? Specifically, they exploited the increase in wealth generated by the appreciation in housing equity during 2002 and 2003 and examined whether that increase in wealth led early baby boomers to change their retirement planning behavior. Similarly, they examine whether the housing bust before 1992 and the resulting decrease in wealth that the older cohort experienced at the beginning of the 1990s changed this group's planning behavior.⁶ In both cases, Lusardi and Mitchell (2007a) do not find any evidence that this change in wealth influenced planning, a result confirming that the direction of causality goes from financial planning to wealth accumulation rather than from amassing wealth to financial planning.

Why does planning have such a powerful effect on amassing wealth? Anticipating an argument that will be detailed later, nonplanners lack basic financial literacy. The financially illiterate are less likely to invest in high-return assets, such as stocks (Van Rooij, Lusardi, and Alessie 2007). This would lead to low savings accumulation when combined with an intertemporal substitution elasticity that is less than one. Other researchers, such as Hurst (2006), argue that those individuals who are planners are less likely to behave like "hand-to-mouth" consumers, who simply set current consumption equal to their current income. A different explanation about how financial planning might affect wealth accumulation is provided by the psychological literature. Gollwitzer (1996, 1999) shows that people are more likely to translate their intentions into actions when they develop concrete plans to achieve their goals. His research shows that a simple planning activity, such as getting people to write down the specific steps they will take to implement a task, can greatly increase successful follow-through. These findings may help explain why merely thinking about retirement beforehand can produce wide differences in retirement wealth. Moreover, it may explain the bimodal distribution of wealth observed in table 1, and why even a little amount of planning generates large wealth differences, as compared to those individuals who do not think about the financial aspects of retirement at all. If this is the case, helping individuals plan for retirement or providing some planning aid may help foster their wealth accumulation.

Lack of Information

Another way to examine whether and how much individuals prepare for retirement and make plans for the future is to look at how much they know about the crucial components of a savings plan. For example, in the United States two very important parts of total household wealth holdings are pension and Social Security wealth. For households around the median of the wealth distribution, those two components account for about half of total wealth, and even for households at the top of the wealth distribution, the percentage of wealth accounted for by Social Security and pensions is sizable (Gustman and Steinmeier 1999).

Earlier studies indicated that workers were woefully uninformed about their pensions and the characteristics of their pension plans (Mitchell 1988 and Gustman and Steinmeier 1989). Given that in the past most pensions were DB pensions and workers had to make few or no decisions about their pension contributions, this lack of knowledge is perhaps not surprising. However, recent data from the HRS show that American workers continue to be uninformed about the rules and the benefits associated with their pensions, despite the large shift in the last two decades from DB to DC pension plans, which has resulted in giving workers more responsibility for saving for retirement (Gustman and Steinmeier 2004). The calculations underlying pension plans and Social Security wealth are certainly very complex and, as for private savings, individuals do not seem to engage in these calculations. However, Gustman and Steinmeier (2004) simply compare the type of pensions that workers report they have (whether DB, DC, or a combination of both) with the reports from employers. The results are striking: only half of older workers are able to correctly identify the workplace plan they have. Because errors can abound not only from workers' self-reporting but also from the reports of firms, Gustman, Steinmeier, and Tabatabai (2008) use different sources of data, including data from Watson Wyatt, where it is possible to correctly identify the pension type from the firms' data. They also study different time periods, ranging from the 1980s (when DB plans were prevalent) to the more recent period (when DC plans gained popularity). They show that it is workers who are most often confused or wrong about the type of pensions they have.

For many in the United States, information about Social Security is also scant. Only 43 percent of respondents in the sample of older work-

ers used by Gustman and Steinmeier (2004) even ventured a guess about their expected Social Security benefits, and many respondents knew little about the rules governing Social Security. As noted in the Employee Benefit Research Institute report made after conducting the 2007 RCS, even though it has been 24 years since legislation was passed that incrementally increased the normal retirement age for Social Security, and despite eight years of the Social Security Administration mailing out annual benefit statements to individuals, only 18 percent of American workers knew the correct age at which they will be entitled to collect full Social Security benefits (see Helman, VanDerhei, and Copeland 2007).

A lack of information about Social Security benefits and pension plans is concentrated among low-income U.S. households, African-Americans and Hispanics, women, and those with low educational attainment (Gustman and Steinmeier 2005). As mentioned before, these groups of people are also those less likely to engage in financial planning. Most importantly, Gustman and Steinmeier (2004) document that those who do not know their type of pension plan have very low wealth holdings relative to their lifetime earnings. This lack of knowledge may explain why households who have pensions do not have less private savings than households without pensions; Gustman and Steinmeier (1999) found that pension wealth does not crowd out private savings.

A lack of knowledge and confusion are also found to affect other equally important financial decisions. Bucks and Pence (2008) document that households with adjustable-rate mortgages, which are potentially more complex contracts to understand than fixed-rate mortgages, are either incorrect in their understanding of the terms or simply do not know about the terms of their contract. These are disconcerting results, since mortgages are important and often onerous contracts. Again, those individuals displaying low knowledge about mortgages are disproportionately those with low education levels, low incomes, and minorities—groups who may benefit the most by knowing the terms of their contract. These findings are also consistent with the evidence on “mistakes” provided by Campbell (2006), who shows that many households failed to refinance their mortgages during a period of declining interest rates. A lack of financial knowledge may have contributed to that behavior since the absence of refinancing was particularly pronounced among those

with low levels of education and income. Moore (2003) also documents that households that have onerous mortgage contracts are less likely to be financially knowledgeable and skilled.

Lack of Financial Literacy

One reason why individuals do not engage in financial planning or are not knowledgeable about retirement pensions or the terms of their financial contracts is because they lack financial literacy. Bernheim (1995, 1998) was one of the first researchers to emphasize that most individuals lack basic financial knowledge and numeracy. Several surveys covering the U.S. population or specific subgroups have consistently documented very low levels of economic and financial literacy. The Council for Economic Education periodically surveys high school students and working-age adults to measure their financial and economic knowledge. The survey consists of a 24-item questionnaire on topics including “Economics and the Consumer,” “Money, Interest Rates, and Inflation,” and “Personal Finance.” When the results were tallied using a standard grading criterion in 2005, adults had an average score of C, while the high school population fared even worse, with most earning an F. These findings are confirmed by the Jump\$tart Coalition for Personal Financial Literacy survey, which also documents very low levels of basic financial knowledge among U.S. high school students (Mandell 2004). Hilgert, Hogarth, and Beverly (2003) examine data from the 2001 Survey of Consumers, where some 1,000 respondents (aged 18–98 years) were given a 28-question true/false financial literacy quiz, covering knowledge about credit, saving patterns, mortgages, and general financial management. Again, most respondents earned a failing score on these questions, documenting wide illiteracy among the entire U.S. population. Similar findings are reported in smaller samples or specific groups of the population (Agnew and Szykman 2005; Moore 2003).

Lusardi and Mitchell (2006) devised a special module on financial literacy for the 2004 HRS.⁷ Adding these types of questions to a large U.S. survey is important not only because it allows researchers to evaluate levels of financial knowledge but also and, most importantly, because it makes it possible to link financial literacy to a very rich set of information about household savings behavior. The module measures basic financial

knowledge related to how interest rates work, the effects of inflation, and the concept of risk diversification.⁸ The findings from this module reveal an alarmingly low level of financial literacy among older individuals in the United States (aged 50 years and older). Only 50 percent of respondents in the sample were able to correctly answer two simple questions about interest rates and inflation, and only one-third of the respondents were able to correctly answer these two questions and a question about risk diversification. Financial illiteracy is particularly acute among the elderly, African-Americans, Hispanics, women, and those with low education levels (a common finding in the surveys of financial literacy).⁹

Lusardi and Mitchell (2007a) have also examined numeracy and financial literacy among the early baby boomers, who should be close to attaining the peak of their wealth accumulation and should have dealt with making many financial decisions already (mortgages, car loans, credit cards, pension contributions, and so on). The following questions were posed to these respondents:

- 1) *“If the chance of getting a disease is 10 percent, how many people out of 1,000 would be expected to get the disease?”*
- 2) *“If 5 people all have the winning number in the lottery and the prize is 2 million dollars, how much will each of them get?”*

For respondents who answered either the first or the second question correctly, the following question was asked:

- 3) *“Let’s say you have 200 dollars in a savings account. The account earns 10 percent interest per year. How much would you have in the account at the end of two years?”*

Respondents were also asked to name the president and the vice president of the United States.

Table 3 summarizes how the early boomers answered these questions. While more than 80 percent of respondents were able to do a simple percentage calculation, only about half could divide \$2 million by 5. Moreover, only 18 percent correctly computed the compound interest question. Of those who got the interest question wrong, 43 percent undertook a simple interest calculation, thereby ignoring the interest accruing on both principal and interest. These are uncomfortable findings, espe-

Table 3
Financial Literacy Among Early Baby Boomers

Question Type	Correct (%)	Incorrect (%)	Do Not Know (%)
Percentage Calculation	83.5	13.2	2.8
Lottery Division	55.9	34.4	8.7
Compound Interest*	17.8	78.5	3.2
Political Literacy	81.1	11.0	7.7

Source: Adapted from Lusardi and Mitchell (2007a).

Note: *Conditional on being asked the question. Percentages may not sum to 100 due to a few respondents who refused to answer the questions. Observations weighted using HRS household weights. The total number of observations is 1,984.

cially considering that these respondents had already made many financial decisions during their lifetimes. Consistent with the general lack of financial information documented earlier in the paper, a sizable fraction of respondents do not know who is the president or the vice president of the United States, indicating they do not pay attention to the news or read newspapers.

Does financial literacy really matter? Table 4 explores the link between financial literacy and planning. Two sets of dummy variables are defined to characterize those who correctly answered the literacy questions and those who did not know the answers to these questions. The table shows that those who are more financially knowledgeable are also much more likely to have thought about retirement planning. In terms of economic importance, both a knowledge of compound interest and the ability to perform simple mathematical calculations (such as a lottery division) matter the most for planning. This result is expected, given that any savings plan requires some numeracy, the ability to calculate present values, and an understanding of the advantages of starting to save early in one’s working life. Financial literacy is not simply a proxy for low education, race, or gender—as previously noted these groups are disproportionately less likely to be financially literate. Even after accounting for many demographic characteristics—including education, marital status, number

Table 4
Empirical Effects of Financial Literacy on Retirement Planning

	Probability of Being a Retirement Planner		
	I	II	III
Correct Percentage Calculation	-.016 (.061)	-.012 (.062)	-.034 (.060)
Correct Lottery Division	.059** (.030)	.034 (.031)	.001 (.032)
Correct Compound Interest	.153*** (.035)	.149*** (.035)	.114*** (.039)
Correct Political Literacy	.104*** (.032)	.084* (.040)	.016 (.042)
DK Percentage Calculation		.021 (.068)	.054 (.067)
DK Lottery Division		-.154*** (.050)	-.141*** (.051)
DK Compound Interest		-.114 (.080)	-.073 (.081)
DK Political Literacy		-.019 (.053)	-.016 (.054)
Demographic controls	No	No	Yes
Pseudo R ²	.031	.038	.074

Source: Adapted from Lusardi and Mitchell (2007a).

Note: This table reports probit estimates of the effects of literacy on planning; marginal effects reported. Analysis sample consists of HRS Early Baby Boomers who responded to financial literacy questions. Being a planner is defined as having thought a little, some, or a lot about retirement. Demographic controls include age, education, race, sex, marital status, retirement status, number of children, and a dummy variable for those not asked the question about interest compounding. DK indicates respondent who did not know the answer. Observations weighted using HRS household weights. The total number of observations is 1,716. * significant at 5-percent level; ** significant at 10-percent level; *** significant at 1-percent level.

of children, retirement status, race, and sex—table 4 (column III) shows that financial literacy continues to be an important determinant of retirement planning.

One may argue that financial literacy and retirement planning are both decision variables and that planning may also affect financial knowledge. For example, those who want to plan for retirement may invest in acquiring financial knowledge. Lusardi and Mitchell (2007b) address this question using the module on financial literacy and planning they have designed for the Rand American Life Panel, which contains a more extensive dataset on financial literacy than the HRS. Specifically, they use information on a person's past financial literacy—before an individual entered the job market—and show that those who were financially literate when they were young are more likely to plan for retirement later in life.

Other studies have confirmed the positive association between financial knowledge and household financial decisionmaking. Hilgert, Hogarth, and Beverly (2003) document a positive link between financial knowledge and financial behavior. Stango and Zinman (2007) show that those who are not able to correctly calculate interest rates out of a stream of payments end up borrowing more and accumulating lower amounts of wealth. Van Rooij, Lusardi, and Alessie (2007) and Kimball and Shumway (2007) find that financially sophisticated households are more likely to participate in the stock market. Agarwal, Driscoll, Gabaix, and Laibson (2007) show that financial mistakes are most prevalent among the young and elderly, those groups also displaying the lowest amounts of financial knowledge and cognitive ability.

Lack of Financial Advice

The findings that some individuals are uninformed about the most important components of their total retirement savings plan and lack basic financial knowledge would not be so troubling if these individuals relied on professional advice and financial experts to make their saving decisions. In fact, only a small fraction of households consult financial advisers, bankers, certified public accountants and other professionals; the majority of U.S. households rely on informal sources of advice. According to the Survey of Consumer Finances, most individuals rely

on the help of family and friends for making their financial decisions, and this is particularly true for those with low educational attainment (Lusardi 2003). Insofar as there is a positive correlation between the educational level of individuals and the educational level of their family or peers, low-educated individuals may simply rely on crude sources of advice. For example, given the rapid changes in financial markets and the pension landscape in recent years, it may be difficult to benefit from the advice or experience of their parents. Similarly, those with low financial literacy may be particularly disadvantaged in overcoming their lack of knowledge. Van Rooij, Lusardi, and Alessie (2007) show that individuals with low levels of financial literacy are disproportionately more likely to rely on family and friends for financial advice, while more financially sophisticated individuals are more likely to rely on newspapers, books, and the Internet as their sources for financial information.

When asked about the tools individuals use to calculate how much their household would need to save for retirement, few planners have indicated they use worksheets or retirement calculators, while the majority of planners indicate that they talk to family and friends. Many seem to use no tools at all! This may explain why many people are unable either to develop a savings plan or carry such a plan through. Decisions about pension contributions also seem to be influenced by interactions with colleagues (Duflo and Saez 2004; Madrian and Shea 2001). Investments in complex assets, such as stocks, are also found to be affected by word of mouth, such as the advice of neighbors and even fellow churchgoers (Hong, Kubik, and Stein, 2004; Brown, Ivković, Smith, and Weisbenner 2008).

It is hard to know whether U.S. households' use of professional financial advice is limited because of the many problems affecting the functioning of this market, including demand versus supply, but findings from the 2007 RCS suggest some reluctance to rely on financial experts (see Helman, VanDerhei, and Copeland 2007). For example, when asked whether respondents would take advantage of professional investment advice offered by companies that manage employer-sponsored retirement plans, about half of respondents reported they would do so. However, two-thirds of those respondents who were willing to take advantage

of professional investment advice also state they would probably only implement those recommendations that were in line with their own ideas, and one in ten respondents think they would implement none of the recommendations. Thus, the effect of firms offering professional financial advice to their employees may be elusive, as workers may not act upon the recommendations of these advisers.

We still know little about the effects of receiving financial advice and whether it can improve a household's financial decisionmaking, but there is some evidence that financial counseling can be effective in reducing debt levels and delinquency rates (Hirad and Zorn 2001; Elliehausen, Lundquist, and Staten 2007). Mottola and Utkus (2008) also provide evidence in favor of relying on professionals to manage financial investments. They compare the portfolio performances of individuals before and after shifting to a professionally managed account. Those who shifted their investments are not a randomly chosen group of the population but, nevertheless, the effects are remarkable. Those who shifted to professionally managed accounts changed their asset allocation dramatically. Most importantly, their new portfolios did not suffer from several of the "mistakes" identified in the finance literature, such as investing too little or too much in the stock market and not holding well-diversified portfolios (Campbell 2006).

A similar analysis performed earlier by Warshawsky and Ameriks (2000) focused on evaluating household wealth. They input the wealth holdings of a representative sample of U.S. households, as reported in the Survey of Consumer Finances, into one of the most popular financial planning software programs, Quicken Financial Planner. According to this program's predictions, about half of middle-class American households will not have a fully funded retirement. Some will actually run out of financial resources very shortly after retirement. One of the features of U.S. household wealth holdings highlighted by this exercise is that many households, particularly those with low education, accumulate little wealth until late in their life cycle or start saving very late, at a point where it is not possible to achieve much wealth accumulation. Clearly, the predictions of financial planners are based on a very specific set of assumptions, which tend to vary across planners. But the main message

remains: without engaging in any financial planning and periodic evaluations, U.S. household savings and portfolio choice behavior may stray away from what is optimal.

3. Three Different Approaches to Promote Saving and Financial Security

The evidence reported thus far points to the existence of several obstacles to achieving adequate household savings in the United States. Many initiatives have been undertaken to promote financial decisionmaking and retirement security. Three major initiatives are discussed below: financial education, automatic enrollment, and new ways to get people to save.

Financial Education

As evidence mounts that financial illiteracy is a severe impediment to household saving rates, both the U.S. government and U.S. employers have promoted financial education programs. Most large firms, particularly those with DC pensions, offer some type of education program (Bernheim and Garrett 2003). So far the evidence on the effectiveness of these programs is very mixed.¹⁰ Only a few studies find that those individuals who attend a retirement seminar are much more likely to save more and contribute to pension plans (Bernheim and Garrett 2003; Lusardi 2002, 2004). Clearly, those who attend such seminars are not necessarily a randomly selected group of U.S. workers. Because attendance is voluntary, it is likely that those who attend already have a proclivity to save and it is hard to disentangle whether it is the seminars per se or simply the characteristics of seminar attendees that explain the attendees' higher saving rates shown in the empirical estimates. However, Bernheim and Garrett (2003) argue that these seminars are often remedial, meaning these are offered at firms where workers do little or no saving. Thus, the effect of these employer-sponsored educational seminars on fostering better savings behavior may be underestimated.

Lusardi (2004) uses data from the HRS and confirms the findings of Bernheim and Garrett (2003). Consistent with the hypothesis that employer-sponsored seminars are remedial, she finds that the effect of these seminars is particularly strong for those workers at the bottom of

the wealth distribution and those with low levels of education. As shown in table 5, retirement seminars are found to have a positive effect mainly in the lower half of the wealth distribution and particularly for those with low education. The estimated effects are sizable, particularly for the least wealthy, for whom attending employer-sponsored seminars appears to increase financial wealth (a measure of retirement savings that excludes housing and business equity) by approximately 18 percent.¹¹ Note also that such seminars affect not only private wealth but also measures of wealth that include pensions and Social Security wealth, perhaps because these seminars provide information about pension plans and encourage

Table 5
The Effect of Retirement Seminars on U.S. Household Retirement Accumulation

	Total Sample	First Quartile	Median	Third Quartile
a. Financial Net Worth				
Total Sample	17.6*	78.7*	32.8*	10.0
Low Education	19.5	95.2*	30.0*	8.8
High Education	13.1	70.0*	19.4*	10.2
b. Total Net Worth				
Total Sample	5.7	29.2*	8.7	0.5
Low Education	3.4	27.0*	7.1	4.0
High Education	7.3	26.5*	6.5	3.6
c. Total Net Worth plus Pensions and Social Security				
Total Sample	16.0*	18.6*	20.4*	17.2*
Low Education	12.7*	14.7*	12.7*	9.5*
High Education	17.7*	25.4*	25.8*	17.0*

Source: Adapted from Lusardi (2004).

Note: This table reports the percentage changes in different measures of retirement accumulation resulting from attending retirement seminars. Financial net worth is defined as the sum of checking and savings accounts, certificates of deposit and Treasury bills, bonds, stocks, IRAs and Keoghs and other financial assets minus short-term debt. See table 1 for the definition of total net worth. * significant at the 5-percent level.

workers to participate and contribute. This can be important because, as mentioned before, workers are often uninformed about their pension options.

In a series of papers, Clark and D'Ambrosio (2008) have examined the effects of seminars offered by TIAA-CREF (Teachers Insurance and Annuity Association-College Retirement Equities Fund) to a variety of institutions. The objective of the seminars is to provide financial information to assist individuals in the retirement planning process. Their empirical analysis is based on information obtained in three surveys: participants completed the first survey prior to attending a seminar, the second survey was completed at the end of the seminar, and the third survey was sent to participants several months later. Respondents were asked whether they had changed their retirement age goals or revised their desired level of retirement income after the seminar.

After attending the seminar, several participants stated they intended to change their retirement goals, and many revised their expected level of retirement income. Thus, the information provided in the seminars does have some effect on behavior. However, it was only a minority of participants who were affected by the seminars. Just 12 percent of seminar attendees reported changes in retirement-age goals and close to 30 percent reported changes in retirement-income goals. Moreover, their intentions did not always translate into actions. When interviewed several months later, many of those who had intended to make changes had not implemented them yet. Other authors, including Choi et al. (2004), also argue that seminar participants who say they will start contributing to pension plans or boost their contributions often fail to follow through.

It is not surprising that one retirement seminar does little to change behavior. Few surveys provide information on the number of seminars that were offered or that the participants attended, but it seems that participants often attend only once or a handful of times (Clark and D'Ambrosio 2008). Evidence from the financial education sessions offered in programs aimed to promote individual development accounts, which are subsidized savings accounts targeted at the poor, show that a set of consecutive education sessions is effective in stimulating saving (Schreiner, Clancy, and Sherraden 2002).

Other researchers find that education programs have more modest effects. Duflo and Saez (2003) investigate the effects of having employees of a large nonprofit institution attend a benefit fair. This study is notable for its rigorous methodology: a group of randomly chosen participants was incentivized to participate in a benefit fair and its subsequent behavior was compared with that of a similar group which was not offered any incentives to attend the benefit fair. This methodology overcomes the aforementioned problem that those who attend employer-sponsored financial education programs may already be inclined to save. This refinement is clearly important, and the findings from this study show that the benefit fair induced participants to increase their participation in pension plans, but the effect on increasing savings was almost negligible. Perhaps this study's most notable result is how pervasive peer effects are—not only the participants themselves but also their colleagues who did not attend the benefit fair were affected by it, providing further evidence that individuals rely on the behavior of others around them to help make their own financial decisions (Duflo and Saez 2004).

Automatic Enrollment

One way to stimulate worker participation in and contributions to employer-sponsored pension plans is to automatically enroll workers into these pension plans. Thus, rather than letting workers choose whether or not to *opt in*, employers could enroll workers and let them choose whether or not to *opt out* of contributing to a pension plan. This simple but ingenious method has proven to be very effective in increasing pension plan participation. For example, according to Madrian and Shea (2001), after a company implemented a change in its 401(k) pension plan and automatically enrolled its new hires in the 401(k) plan, pension participation went from 37 percent to 86 percent. Sharp increases in employee participation with the implementation of an automatic enrollment policy have been documented in several other papers (Choi et al. 2004, 2006; Thaler and Benartzi 2004). Not only has the increase been very large but these participation rates have remained high for several years (Choi et al. 2004, 2006). Even the U.S. Congress took notice of this remarkable success and the 2006 Pension Protection Act made it much

easier for firms to automatically enroll their workers into employer-sponsored pension plans.

In principle, employers could automatically enroll workers in a pension plan but ask workers to go to the company's human resources office and choose the contribution rate and the allocation of pension assets. In fact, automatic enrollment programs also specify the default rate at which workers are enrolled and how the pension assets are allocated. Choosing a contribution rate and the asset allocation are very difficult decisions for individuals to make. According to most theoretical models of saving, the optimal savings rate depends on a long list of variables, including individual preferences and expectations about the future, which are unknown to the employer. In reality, automatic enrollment contribution rates and allocations are rarely individual-specific. For example, in the firm analyzed by Madrian and Shea (2001), the automatic enrollment rate was set at 3 percent for every worker. This choice has drawbacks since, in that particular firm, the first 6 percent of a worker's contribution received a 50 percent employer match. Thus, a 3 percent contribution rate fails to take advantage of part of the employer match.¹² Irrespective of this problem, not only did new hires stay at the 3 percent contribution rate, but other workers as well changed their contribution rates to 3 percent. Moreover, the default pension contributions were invested in money market mutual funds. This is another problem since this conservative default asset allocation prevents workers from earning higher returns in the bond or stock market. Nevertheless, most workers did not opt out of the allocation in money market mutual funds (Madrian and Shea 2001).¹³

The design of automatic enrollment programs is very important.¹⁴ If an employer's objective is to promote its workers' financial security during retirement, contribution rates and asset allocations have to be chosen very carefully because workers tend to stay with what is chosen as the default. This tendency includes not participating in pension plans if the default choice is to not automatically enroll workers.

Several papers have recognized that default contribution rates that are too low may prevent workers from accumulating enough retirement wealth, taking advantage of employer-matching contributions, and exploiting the tax advantages of investing in pension assets. Thaler and Benartzi (2004)

have devised a program—Save More Tomorrow™ (SMarT)—that incorporates not only automatic enrollment but also increases in the default rate as a worker's income increases. The success of this program is remarkable. Workers enrolled in the SMarT program have achieved saving rates of more than 13 percent versus an average of 5–6 percent for workers who did not enroll.

Similarly, VanDerhei (2007) shows that low contribution rates and investments in conservative assets result in very low median income replacement rates during retirement. For example, an automatic enrollment program with a 3 percent contribution rate and pension assets invested in money market mutual funds results in a median income replacement rate of only 37 percent for the lowest income quartile of workers. However, the replacement rate for this income group increases to 52 percent when the contribution rate is increased to 6 percent and the default investment is changed to a life-cycle fund. Moreover and most importantly, workers seem to favor higher default rates than 3 percent—as many as 44 percent of the respondents in the 2007 RCS stated they would continue to contribute to pensions up to a rate of between 6 to 10 percent of their income, and 27 percent of respondents were willing to go for even higher contribution rates (see Helman, VanDerhei, and Copeland 2007). While these are self-reported figures, they suggest that increases in default contribution rates are possible. Moreover, the 2006 Pension Protection Act has taken away some of the fiduciary problems that were limiting employers from using riskier investment assets than money market mutual funds as default options or from offering advice on how to invest pension assets.

What explains the success of defaults such as automatic pension enrollments and asset allocations? If individuals are poorly informed about their pension plans, lack basic literacy, and do not have good sources of financial advice to turn to, default options are very useful because they tell workers exactly what to do. In fact, defaults do even more; they not only provide potent advice but also overcome the problem that workers may fall prey to inertia and simply not follow through on their intentions. Moreover, if there is any learning in savings behavior, another advantage of defaults is they may make workers appreciate the value and perhaps ease of saving for retirement.

However, there are potential problems with default options that need to be addressed. First, the success of defaults should not be measured according to the participation rate in employer-sponsored pension plans, but according to their ability to improve household financial security. Because adhering to default saving choices means an active decision has not been made and individuals did not have to calculate how much they need to save, eventually these rates and asset allocations may not provide adequately for their retirement income. In fact, workers may not learn much or develop financial savvy. This is a problem because there are no default enrollments (yet) in mortgage loans, credit cards, or children's education funds. Second, in addition to saving for retirement, individuals have other motives for saving (or not saving). We do not know yet how these other motives interact with default choices. For example, individuals may be carrying credit card debt or high-interest mortgages while enrolled in pension plans, and the need to service these loans may detract from their ability to increase their retirement savings. Finally, about half of private-sector workers in the United States have jobs that do not offer any employer-sponsored pension plan. Thus, automatic enrollment is currently leaving out a substantial fraction of workers—about 75 million people—who could also benefit from such a program. The next section investigates other methods to make people save that adopt some of the ideas implicit in defaults but overcome some of their limitations.

New Ways to Make People Save

If saving decisions are very complex, one way to help people save is to find ways to simplify those decisions. A drawback to providing financial education, as discussed above, is that it does not necessarily translate into permanent behavioral changes. Thus, what may be important and perhaps more effective is to find ways to make people ease into taking action. This is the strategy analyzed by Choi, Laibson, and Madrian (2006). They study the effect of Quick Enrollment, a program that gives workers the option of enrolling in the employer-provided savings plan by opting into a preset default contribution rate and asset allocation. Contrary to other default plans, workers have the choice to enroll or not, but their decision is simplified as they do not have to decide at which rate to

contribute and how to allocate their assets. In other words, it is possible to exploit the power of suggestion implicit in defaults to induce workers to enroll in pension plans.

When new hires were exposed to the Quick Enrollment program, participation rates in 401(k) plans tripled, going from 5 percent to 19 percent in the first month of enrollment. When the program was offered to previously hired nonparticipants, participation increased by 10 to 20 percentage points. These are large increases, particularly if one considers that the default rate is not particularly advantageous; the contribution rate in the most successful program is set at only 2 percent and 50 percent of assets are allocated in money market mutual funds while the other 50 percent is allocated in a balanced fund. Moreover, Quick Enrollment is particularly popular among African-Americans and lower-income workers (those earning less than \$25,000) who, as the research shows, are less likely to be financially literate. Thus, changes in the design of a pension plan can have a large impact on participation. Most importantly, Quick Enrollment is a low-cost program.

Another approach that is based on simplifying the decision to save and, in addition, motivating employees to make an active choice in enrollment and asset allocation decisions is the one proposed by Lusardi, Keller, and Keller (2008). They devised a planning aid to be distributed to new hires during employee orientation that has several critical features. First, the planning guide breaks down the process of enrolling in supplementary pension plans into several small steps, describing to participants what they need to do to be able to enroll online. Moreover, it provides several pieces of information to help overcome the barriers to saving, such as describing the minimum amount of income employees can contribute (in addition to the maximum) and indicating the default fund that the employer has chosen for them (a life-cycle fund). Finally, the planning aid also contains pictures and messages designed to motivate participants to save.

The planning guide was designed after a thorough data collection. For example, the researchers devised a survey asking the respondents explicitly about their barriers to saving, sources of financial advice, level of financial knowledge, and what they considered to be the attractive features of a pension plan. Moreover, Lusardi, Keller, and Keller (2008) con-

ducted focus groups and in-depth interviews (with both employees and human resources administrators) to shed more light on the impediments to saving. These data collection methods, which are common in the field of marketing, are well-suited to capturing the wide heterogeneity that characterizes decisionmaking about saving. Even though the sample is small and hardly representative of the entire U.S. population, it displays findings that are consistent with the broader evidence described earlier. For example, many employees state that they consult only family and friends for making saving decisions. Moreover, close to 40 percent state that they do not have enough knowledge about finance and investing, and close to 20 percent state that they do not know where to start. Given this evidence, it is not surprising that the program using the planning aid was so successful; contribution rates to supplementary pension plans doubled after the aid was introduced.

This program shares several common features with respect to other programs designed to enhance workers' participation in employer-sponsored pension plans. First, while economic incentives, such as employers' matching contributions or tax advantages may be useful inducements, they do not exhaust the list of options that can be used to make people save for retirement. In fact, given the massive lack of information and lack of financial knowledge, other more cost-effective programs may exist that can induce people to save. Second, employees are more prone to decisionmaking at specific times. For example, the start of a new job makes people think about saving (often because they have to make decisions about their pension plan participation). As discussed above, many people do not think about saving for retirement even at an advanced age in their working lives, and it may be very important to exploit these "teachable moments." The papers by Choi, Laibson, and Madrian (2006) and Lusardi, Keller, and Keller (2008) both find that newly hired workers are particularly malleable to making changes in their savings behavior. Third, to be effective, employer-sponsored pension programs have to recognize the many differences that exist among individuals, not only in terms of preferences and economic circumstances, but also in the level of information possessed, financial sophistication, and the ability to carry through plans. In other words, relying on "one-size-fits-all" principles can lead to rather ineffective programs.

4. Implications

An individual's saving decisions are derived from maximizing utility not only under a lifetime budget constraint but also under the limitations imposed by low financial literacy, a lack of information, and following crude sources of financial advice. Thus policies that aim to stimulate savings and financial security after retirement should consider a variety of incentives, including how to decrease informational barriers and simplify decisionmaking. Other fields have already recognized the difficulties that individuals face in collecting information and making decisions. For example, hospitals have set up "centers for shared decisionmaking" to help patients make informed choices about medical treatments.¹⁵

The choices confronting U.S. policymakers are not easy. Financial literacy cannot be taken for granted among the general population, and particularly among specific groups (including those with low educational attainment, women, and minorities). This challenge raises concerns about how to communicate information about financial decisionmaking effectively, particularly to those who need it most. Given low numeracy and low literacy, it may be useful to resort to more effective ways of communication (Lusardi 2008). In the health literature for example, there is an increased reliance on testimonials and stories rather than on figures and hard data.¹⁶

Given the increased complexity in financial instruments, the evidence of illiteracy raises the question of whether U.S. consumers will appreciate and take advantage of the opportunities offered by financial markets or will more easily fall prey to scams or unscrupulous brokers. The effectiveness of financial education programs has been measured with respect to specific outcomes, such as increased saving or participation rates in pension plans, but there are other potential—though less easy to measure—outcomes, such as avoiding being taken advantage of and having confidence in making financial decisions.¹⁷ Almost no study provides an evaluation of the costs of financial education programs, and without that information it is not possible to establish a return on investing in these education programs. Moreover, as shown by the studies discussed earlier, few employees ever attend education programs and many of those who do attend do not modify their behavior, at least in the short

run. While these are drawbacks, financial education programs cannot be dismissed. The benefits of receiving information and gaining knowledge can affect many household financial decisions, not simply saving for retirement. Moreover, the gains to such knowledge may take effect over a long period of time and should be evaluated in the long run rather than over a few months or years after a program is offered. For example, according to Bernheim, Garrett, and Maki (2001) persons who were exposed to financial education programs while in high school were more likely to save later in life. Finally, given the extent of financial illiteracy in the United States, it is not surprising that individuals who attend a benefits fair or workers who are offered one hour of financial education show little improvement in their savings behavior. To be effective, financial literacy programs have to be tailored to the size of the problem they are trying to solve. And while it is not possible to transform low literacy individuals into financial wizards, it is feasible to emphasize simple rules of thumb and good financial behavior, such as diversify your assets, exploit the power of interest compounding, and take advantage of tax incentives and employers' pension matches.

Another potential role of financial education is to help individuals assess their ability to make saving and investment decisions. Perhaps a related goal is to make them appreciate the value of obtaining professional financial advice and/or equipping them with tools to deal effectively with advisers and financial intermediaries.

If a lack of financial literacy, a lack of information, an inability to plan ahead for the future, and/or procrastination prevent people from contributing to pensions, default options are clearly an effective remedy. Defaults are the most powerful and innovative programs in the field of savings and pensions and should be exploited. However, the design of default options is crucial; if these are geared towards low contributions rates and investments in conservative assets, such suboptimal defaults may eventually offset the benefits of enrolling workers into saving programs. Moreover, since close to half of private-sector workers in the United States do not have an employer-sponsored pension plan, it is important to expand automatic enrollment to other saving instruments that these workers may invest in.

Contrary to what the previous literature seems to imply, default options and financial education programs are not necessarily substitutes. In fact,

they can complement each other well.¹⁸ Combining default options with financial education programs or financial advice may prevent workers from saving at suboptimal rates. Moreover, these programs may help workers evaluate their total savings, not only for retirement but also private savings; this may help them save for their children's education, to build a buffer to insure against financial and income shocks, or for other reasons. Several big firms, such as IBM, have adopted such initiatives and in the future it will be possible to evaluate the outcome of these combined programs.

Similarly, it is possible to exploit some of the features of automatic enrollment to make current saving programs more effective. If there is significant power in suggesting how much someone should save and where to invest pension assets, why not provide such information to workers when they start a new job or when they have to renew their benefit selection every year? Such "suggestions" can be made more individual-specific, and tailored according to age, number of children, and earnings. Similarly, if such information is scarce but, at the same time, so vital, there may be more cost-effective ways to provide it. For example, information and education campaigns can be conducted at the national level to reach the wider U.S. population, including those individuals who are unlikely to be offered education programs in the workplace.

Another finding that emerges from both the literature on savings behavior and on financial literacy is that there are specific segments of the U.S. population—those with low educational attainment and low income—that save in very different ways than more educated and affluent households. It may be important to target these groups and devise programs that are better tailored to their needs and barriers to saving. There is some evidence that existing targeted programs have had some success in increasing saving among the poor (Schreiner and Sherraden 2007).

Recognizing that many individuals possess limited financial literacy and do not always plan for retirement brings us inevitably to the issue of mistakes. Some of the referenced papers document that mistakes are not rare: when left to their own responsibility, individuals may not save enough for retirement, may invest in assets that are either too risky or too conservative, and may not exploit employer matches or tax advantages available to them. Who ultimately will pay for these mistakes—the individual immediately affected or society at large? If taxpayers will

be asked to support those who have made mistakes, there is a role for regulation and for implementing mandatory saving programs. One such program could be to require that people acquire some basic financial knowledge (Alesina and Lusardi 2006). In the same way that people are required to have a driver's license before they venture onto the road, a "financial license" could be required before individuals contribute to their pensions, invest their pension assets, or borrow to buy a house. In this way, individuals may learn about some basic financial concepts and may reduce their reliance on random advice and tips from those around them.

It is also important to recognize that while the private sector spends millions of advertising dollars every year to convince consumers to spend more, relatively little is spent to encourage people to save and provide for their future. However, if consumption is excessive and saving is too scarce, taxpayers may be asked to support those who have not provided enough for their retirement. Thus, the government may have to think of ways to engage in marketing campaigns designed to promote saving. Such messages would be up against tough competition: one recent commercial from American Express, advertising cash-back rebates to card holders on the amount spent using their credit card, argues that by spending more, people . . . save!

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Notes

1. See Scholz, Seshadri and Khitatrakun (2006) and the references therein.
2. For example, some individuals may not benefit from planning because they face a very uncertain income or have been hit by many shocks. Others may desire to never stop working. This is particularly the case for the self-employed and business owners (Lusardi 2003).
3. Household wealth is the sum of checking and savings account balances, certificates of deposit and T-bills, bonds, stocks, IRAs and Keoghs, home equity, second homes and other real estate, business equity, vehicles, and other assets, minus all debt. All values are expressed in 2004 dollars. For more detail, see Lusardi and Mitchell (2007a).
4. For a discussion of these estimates, see Lusardi and Beeler (2007).
5. For alternative instrumental variables estimates, which provide very similar results, see Ameriks, Caplin, and Leahy (2003).
6. They exploit regional variation in home prices in their estimates. There is wide variation in home prices across regions in the United States. For example, while the Pacific region experienced an increase of 10.3 percent in 2003, the southeast region experienced an increase of 3.6 percent. The older cohort had the opposite experience; during 1990 and 1991 the housing market experienced a bust that was particularly pronounced in the eastern regions. See Lusardi and Mitchell (2007a) and Lusardi and Beeler (2007) for detail.
7. For a detailed discussion of the importance of financial literacy, see Lusardi (2008).
8. For a discussion of the measurement of financial literacy and the extent of measurement error in financial literacy data, see Van Rooij, Lusardi, and Alessie (2007).
9. See Lusardi and Mitchell (2007c) for a review.
10. See Lusardi (2004) and Lusardi and Mitchell (2007a) for a review of the effectiveness of financial education programs, and Hogarth (2006) for a description of many education programs currently offered in the United States.
11. Moreover, Lusardi (2005) uses the supply of retirement seminars to pin down the direction of causality between seminars and savings. Specifically, she uses the proportion of large firms across states as an instrument for retirement seminars. She finds that those who are more likely to be exposed to retirement seminars because they live in states with a high proportion of big firms accumulate more wealth.
12. Note, however, that when left to their own choice, many employees simply do not enroll in pensions, so they do not exploit the employer match *at all*, if it is available.
13. As noted by Choi et al. (2004), many companies have chosen low contribution rates and conservative asset allocations. For example, a survey by the Profit Sharing/401(k) Council of America in 2001 reports that 76 percent of automatic enrollment companies have either a 2 percent or 3 percent default contribution rate and 66 percent of automatic enrollment companies have a stable value or money market default fund. See Choi et al. (2004) for a discussion of these findings.
14. Note that there are several limitations imposed by the law. For example, because of fiduciary issues, many employers were reluctant to enroll and invest workers' assets in the stock market for fear of being sued if the markets experience a downturn. The Pension Protection Act takes away some of the existing limitations.
15. Dartmouth Hitchcock Medical Center is one example of a hospital with such a center.

16. See Volk (2007).
17. See also Hogarth (2006).
18. For a discussion, see Lusardi (2007).

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Comments on “Household Savings Behavior in the United States: The Role of Literacy, Information, and Financial Education Programs” by Annamaria Lusardi

Alan S. Blinder

Annamaria Lusardi’s paper, which summarizes a fascinating and important body of work by her and her co-authors (among others), follows a four-step argument. First, *homo sapiens* are not *homo economicus*. Second, and more specifically, *homo sapiens* are not financially literate. Third, because of this deficiency, they make a number of foolish decisions regarding their personal finances, which are not trivial and which *are* contrary to their own best interests. Fourth, there are a number of things we can do to help them make better decisions. I basically agree with all four of these propositions, and I heartily applaud and admire the research that is summarized here. Still, a discussant’s role is to probe for weak points and to provide some alternative thoughts, which is what I will do. But none of this should obscure my main reaction, which is loud applause for the work.

I will take up the four steps in her argument in turn.

1. The Elusive Homo Economicus

First, as Bill Nordaus once put it:

Somewhere, someone probably believes [in] continuous-time maximization of a consistent preference function maximizing the present value of the utility of consumption using Bayesian updating in light of the constant inflow of data from the Internet, several televisions tuned to the financial channels, and a live feed from the Brookings panel. (2004, p. 388)

Let me state clearly and unequivocally that the “someone” is not me. We must always remember that *homo economicus* is an allegory, a deliberate

exaggeration that enables economic theory to get somewhere. If kept in its proper place, the allegory is tremendously useful. But it can be dangerously misleading if allowed to run amok.

In particular, you don't have to be as smart and attentive as Nordhaus's homo economicus to make pretty decent—which is not to say *optimal*—personal financial decisions. We all remember that even highly skilled billiards players may not understand the laws of physics very well, if at all, much less be able to replicate the equations. In addition, rational inattention may be quite rational, as Mankiw and Reis (2006) have emphasized—especially if the decision involves something far in the future (like a retirement plan) and leaves lots of time to correct any mistakes—if you have limited mental bandwidth to process information and more pressing decisions to make at the moment.

On this point, let me use my own (meager) retirement planning as an example of why some of Lusardi's observations are less than entirely persuasive. Early in the paper she observes that “a large percentage of workers have not thought about retirement.” Well, let me confess that I'm part of that large percentage. I am quite confident that I have provided well for my retirement—but any time the thought of retirement crosses my mind, I banish it.

The fascinating Lusardi-Mitchell (2006) Health and Retirement Study module in 2004 asked people aged 51–56 years (and thus younger than I) whether:

- “They have tried to figure out how much they need to save for retirement?”
- “They were able to develop a plan?”
- “They were able to stick to that plan?”

Well, I have never tried to figure out how much money I'll need; I have never tried to develop a plan; and I certainly haven't attempted to stick to one. Lusardi goes on to report, disapprovingly, that “only 18 percent of workers knew the correct age at which they would be entitled to full Social Security benefits.” Again, I find myself in the 82 percent. Remember that, nowadays, the normal retirement age is a moving target. I know that the correct answer is above 65 and below 67 and, somehow, the exact month doesn't seem very important.

One last point about rationality needs to be made—indeed, emphasized. Optimal retirement planning, or optimal financial decisionmaking in general, requires sorting through hundreds if not thousands of options. One of the main fictions we teach our students is that well-being is non-decreasing in the number of options—after all, you can always discard inferior options costlessly. Well, in a world in which mental bandwidth and time are both scarce resources, you can't consider options costlessly—it takes both time and effort to explore each one.

In our personal, as opposed to our professional, lives we all know that having more options can make us *worse* off. Again, here's a personal example. When I need a new computer—a decision which I defer as long as possible—I am forced to enter a market in which the variety of choices is bewildering. All that choice reduces my utility. So I have adopted a simple decisionmaking algorithm that probably leads me to a very good decision while economizing greatly on my time: I walk across the hall and ask my colleague Chris Sims. Then I buy what he recommends.

Returning to financial decisions, there is a fascinating (if disconcerting) study by Sethi-Iyengar, Huberman, and Jiang (2004), which found that each increase of 10 mutual funds in a pension plan's menu of choices actually *reduced* plan participation by 1.5–2.0 percent. This finding is consistent with lots of other evidence; for example, the famous super-market experiments in which greater variety discourages customers from sampling the cheeses. Bewilderment leads to withdrawal.

While all of us probably experience diminished utility from too many choices now and then, few of us pause to think how destructive of standard neoclassical thought this experience is. If utility may actually be decreasing as the number of available options increases, one of the main foundations of the theory of choice evaporates, and with it most of welfare economics. A goodly portion of the argument for free markets also crumbles. So I think we'd better not follow that train of thought too long or too far. And I won't.

2. The Financial Literacy of Homo Sapiens

I couldn't agree with Lusardi more on this point: we humans are not very good at financial decisionmaking. Some of the evidence she offers is very

convincing. For example, I was charmed by the survey finding that only about 50 percent of respondents could divide \$2 million by five! How about 20? Lusardi also repeats the discouraging findings from Lusardi and Mitchell (2006) that many people do not understand that:

- 2 percent interest compounded for five years exceeds 2 percent.
- Purchasing power decreases if you invest your money at 1 percent when the inflation rate is 2 percent.
- Buying a mutual fund is safer than buying a single stock.

Darwinian financial markets are apt to deal harshly with people who don't understand such basic points. [And, according to Lusardi and Mitchell (2006), only 34 percent of respondents know all three!] Such people need, quite literally, to be protected from themselves.

That said, some of Lusardi's "tests" for financial literacy seem too stern. I have already mentioned that knowing the exact month one is eligible to collect full Social Security benefits is a hard question to answer correctly. And if the adjustments are actuarially fair, getting it wrong doesn't even matter. So why bother to know? Another example is the inability to compound interest properly. I'd be inclined to pass, not flunk, survey respondents who say that money invested at 5 percent per year for five years will grow by 25 percent. (But, in line with a previous question, I'd be inclined to *flunk* those who don't know that it will grow by more than 5 percent!) This conference has mostly macroeconomists as participants. Who among us has not told students that $i = r + \pi$, where i is the nominal interest rate, r is the real interest rate, and π is the rate of inflation? (I did it myself just yesterday.) That well-known "equality" omits the compounding, of course. Yet we let it go.

3. Is the Ignorance Consequential?

The next step in the argument is important. Some ignorance is inconsequential. For example, former Vice President Dan Quayle famously could not spell "potatoes." But he's done okay in life. Similarly, surveys tell us that most American college students cannot locate Mexico on a map. But they nonetheless find their way to Acapulco for spring break.

In the financial-planning context, however, Lusardi reports a variety of evidence that ignorance *is* consequential. For example, table 1 in her paper shows that "planners accumulate more than double the wealth of nonplanners" (if you use medians instead of means). She cites the finding from Bucks and Spence (2008) that "households with ARMs [adjustable-rate mortgages] . . . do not know the terms of their contract," a gap in knowledge that has certainly proven to be quite consequential in the recent mortgage debacle. She also cites Stango and Zimmerman's (2007) finding that "those who are not able to correctly calculate interest rates . . . end up borrowing more and accumulating less wealth."

Financial illiteracy is also of obvious importance in a number of significant policy issues. Let me mention just a few. The country is now migrating from a system in which private-sector pensions were mostly defined benefit (DB) to one in which they are mainly defined contribution (DC). Sensible use of DC plans obviously depends on a degree of financial literacy that is unnecessary for DB plans. In a related vein, there are those who think we should transform our nation's big public DB pension plan, Social Security, into a DC plan by privatizing it, either partially or totally. Opponents of privatization have used the lack of minimal financial literacy as one of their arguments. Third, as suggested in the previous paragraph, the complexity of ARMs and other novel mortgage instruments probably left many mortgagees signing contracts that they did not understand. And many of those contracts are now heading toward default. Finally, and related, the intellectual underpinnings of consumer protection laws like the Truth in Lending Act and the Truth in Saving Act rest on the assumption that borrowers and depositors need protection because they are not quite up to the standards of homo economicus.

4. How Can Policy Help?

If we accept the fact that financial literacy is sorely lacking, and that this lack is consequential, what sorts of public policy interventions, if any, might improve things? Lusardi suggests these solutions, but I'm a big fan of only one.

1. *Training in financial literacy.* I would certainly favor this idea, if I thought it would be cost-effective. But both my priors and the evidence to date leave me a bit dubious. Lusardi herself seems at least somewhat ambivalent on this point: “the evidence on the effectiveness of these programs is so far very mixed.” Mixed at best.

2. *Rely more on financial advisers.* Here I am even more skeptical. It’s just not that easy for the ordinary Joe and Jane to find a paid financial adviser who is both honest and competent—or even disinterested, for that matter. Conflicted financial advice may be less than valueless.

3. *Provide people with more information.* I’m doubtful again. The problem is not that information is scarce. In fact, it’s abundant. The problem is that many people do not understand how to process the available information and to use it to their advantage.

4. *Rely more on good default options, such as automatic enrollment.* This is my favorite remedy, by far. One of the most outstanding and important findings in behavioral economics—indeed, I’d say in all of empirical economics—in the last decade or so has been the overwhelming importance of default options. People tend to stick with the default, no matter what it is, perhaps because they are inertial, (rationally or irrationally) inattentive, lazy, or simply too confused to act. Whatever the reason, it is critically important that they be offered sensible—which may not mean *optimal*—default options. And if accomplishing that goal requires public policy intervention, so be it. In a wide variety of issues, certainly including the ones on which Lusardi focuses, providing a good default option is probably the most cost-effective and least distortionary policy intervention we can think of.

I’d like to close with two other policy approaches that she either ignores or denigrates. The first is:

5. *Using commitment devices to overcome “temptation.”* This is another one of those problems that homo economicus doesn’t have. But homo sapiens are frail; they succumb to temptation even when they “know better.” In the saving context, the commitment problem typically means that people spend too much today and regret it later. For example, Lusardi notes that Choi et al. (2004) found that “[retirement planning] seminar participants who say they will start contributing to pensions or boost their contributions often fail to follow through.” This is a matter of willpower, not financial literacy. If employers, governments, and others can

provide *voluntary* commitment devices that help individuals make (and stick with) the sorts of decisions they would really like to make, then welfare can be improved—and, almost certainly, savings can be increased. I am thinking, of course, of devices like Christmas clubs, making “opt in” the default option on 401k plans, and Thaler and Benartzi’s (2004) SMarT™ plan. More good ideas like these are welcome.

Last but not least, I would like to defend—maybe even extol—something that Lusardi does not appear to favor:

6. *Use of simple rules of thumb.* Rules of thumb, which by necessity ignore most or all of the details of any particular household’s idiosyncratic circumstances, will not drive people toward *optimal* decisions. But they may at least drive them toward *moderately sensible* decisions. And that, in my view, would be a big step forward. Here are two examples.

- An old piece of financial “wisdom” holds that the equity share of your portfolio (in percent) should be 100 minus your age. (Confession: I hold more.) Now I’d hate to have to derive that rule as the solution to a life-cycle portfolio optimization problem. But it is probably a reasonable benchmark for many people, especially those who are not financially literate.
- When I was young, a simple rule of thumb held that your house should not cost more than three times your annual income. Now any quasi-optimal rule for housing expenditure would, of course, have to depend on (at minimum) interest rates, property tax rates, the marginal income tax rate, the typical growth rate of income, the relative price of housing, and its expected appreciation. The simple “three times” rule takes none of those factors into account. Yet if your implicit rent is 25 percent of your income, and your house is worth 12 times this implicit rent (two reasonable multiples) then your house will be worth three times your income. I can’t help thinking that the current housing crisis would have been far less severe if more people had followed this simple rule.

So, in summary, my answer to the question whether “U.S. households know what they are doing” is, in many cases, no. This answer agrees entirely with Lusardi’s analysis, and I have mostly praise for her paper. Where we differ is that I am less optimistic about the efficacy of financial education and advice, and I am more enamored of commitment devices and inculcating simple rules of thumb. I join Lusardi, and many other

participants in this conference, in my unbridled enthusiasm for the use of sensible default options.

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Comments on "Household Savings Behavior in the United States: The Role of Literacy, Information, and Financial Education Programs" by Annamaria Lusardi

David I. Laibson

Annamaria Lusardi's paper is a wonderful summary of what is known about financial literacy and financial decisionmaking. I strongly recommend that anyone who is thinking about household savings behavior or savings policy read her paper. It emphasizes the recent findings that Lusardi and her coauthors have generated: financial illiteracy is an important contributor to suboptimal investment choices.

My comments cover four topics. First, I discuss the classical economic argument that economic choices might be sophisticated even if an economic agent lacks formal knowledge. I acknowledge the general plausibility of this argument, but argue that costly mistakes are nevertheless common in the financial domain.

Second, I argue that we should use field experiments to measure the net benefits of educational interventions. I emphasize the important role of cost-benefit analysis.

Third, I discuss some evidence that educational interventions are likely to have only a modest effect on savings and investment behavior in the United States. I show that many educational interventions have relatively poor effectiveness.

Fourth, I show that there are other kinds of inexpensive interventions that generate large increases in savings. I emphasize the role of defaults, active decisions, and simplified savings mechanisms. Finally, I conclude by emphasizing the parallels between physicians and financial advisers.

1. What about Financial Choices?

Economists often use Milton Friedman's billiards example to explain why untrained economic agents might still make optimal choices. In Friedman's example, expert billiards players, who have no formal physics training, nevertheless play pool as if they had a perfect understanding of kinetics.

Likewise, some economists argue that investors who have no formal knowledge of finance (or dynamic optimization theory) might use an intuitive understanding of their self-interest to make sophisticated saving and investment choices. An economist could therefore argue that Lusardi is wrong to worry about financial knowledge, claiming that "what really matters is behavior and investors will somehow get *that* right."

Lusardi is not wrong. Friedman's expert billiards players are the exception and not the rule. Most of us play pool poorly. Even if Friedman is right about the population of *professional* billiards players, his observation has little relevance for the rest of us.

The same issues arise in the domain of investing. There are some highly experienced (and highly selected) traders who make great investment choices. Many of them have no formal training in finance. The existence of these savants proves that formal education is not necessary for good investment choices. But just because one *can* make good financial choices without formal financial knowledge doesn't mean that most of us do. Indeed, economists frequently find that many if not most investors make large mistakes.

My own work has studied such financial choices. In essence, my collaborators and I have been studying how nonprofessional billiards players perform in high stakes settings where they have strong incentives to make the shot. With collaborators James Choi, Brigitte Madrian, and Andrew Metrick we have found that optimization theory is not a good "as if" model. Investors do not behave as if they optimize. Instead, they accept the defaults that their employers set, even when it is trivial to opt out of the default (Madrian and Shea 2001a; Choi et al. 2002, 2004, 2006; and Beshears et al. 2008). Other violations of "as if" rationality abound. Employer stock dominates retirement portfolios, even when diversification is allowed (Choi, Laibson, and Madrian 2005; Choi,

Laibson, and Madrian forthcoming). Employer-matching payments go unclaimed, even when there is a pure arbitrage opportunity for workers (Choi, Laibson, and Madrian 2008a).

To expand this last example, U.S. workers older than 59-and-a-half-years are allowed to withdraw balances from their 401(k) plan without a tax penalty. Moreover, they do not need to demonstrate financial hardship. Nevertheless, about half of the 401(k)-eligible workforce aged over 59-and-a-half-years does not contribute up to their employer's match threshold. On average they lose 1.6 percent of their pay because they do not make a 10-minute enrollment phone call to take advantage of a (liquid) savings account with a matching employer contribution.

2. Educational Interventions?

Lusardi's research has convinced me that financial illiteracy plays an important role in facilitating these bad financial choices. Public policy should try to redress this problem by raising financial literacy. I think that a key place that we are failing is in U.S. high schools. When I was a high school student, I read dozens of nineteenth-century English novels but nobody mentioned the concept of compound interest.

We should read lots of literature in high school. And we should also spend at least some time learning economics. Our high schools currently have the balance wrong. Indeed, we should reevaluate the high school curriculum. Applied mathematics should partially replace pure mathematics. Likewise, statistics, economics, and speech all deserve some time.

We should also think about creative opportunities for adult education. Wherever we intervene educationally we should be careful to measure the results. As Lusardi emphasizes, for an educational intervention to be desirable it has to change behavior at a reasonable social cost. As I'll argue below, many of the (inexpensive) interventions that have been tried to date have flopped. To find the educational interventions that work, we'll need lots of controlled experiments, executed on a small scale and evaluated with cost-benefit measures. Many experiments will spawn a few successes, and those cost-effective successes should then be adopted as policy. Until these cost-effective interventions are identified in the field, we are not yet ready to make policy.

3. Observations about the Design of Effective Education Interventions

There are five factors to take into account when designing educational policy interventions. Some of these factors are conceptual. First, the investment problem is highly complex. For example, we have a blizzard of savings vehicles: defined benefit, cash balance, money purchase, annuity, variable annuity, 529, UGMA accounts, 401(a), 401(k), 403(b), 457, Keogh, Individual Retirement Accounts (IRAs), Simplified Employee Pension-IRAs, Roth IRAs, Employee Stock Ownership Plans, and so on. To make optimal retirement choices, one needs to understand the ins and outs of the U.S. tax code, as well as basic principles of finance and dynamic optimization. We don't expect people to repair their cars or prescribe antibiotics for themselves. We don't worry about their lack of education in these areas. It is likely that financial decisionmaking should also be delegated to third parties. (If this is right, we should be teaching households how to monitor these third parties, not how to make these decisions themselves.)

Second, even if we did give people a perfect training in personal finance, we would need to continuously update their knowledge and skills, since the institutional environment is always changing. When I started in high school in 1984, most people saved through defined benefit pension plans. By the 1990s, defined benefit plans were on their way out and the 401(k) was the new kid on the block. Even if I had gone to high school in 1994, I could not have learned about saving institutions that are *now* commonplace. For instance, automatic enrollment, 529 plans, exchange-traded funds, exchange-traded notes, target-date funds, automatic escalators, 401(k) loans, hedge funds, mortgage-backed securities, and infrastructure funds were basically unheard of 15 years ago.

Third, "just in time" training has had disappointing effects. I have been repeatedly surprised at how little effect targeted information campaigns have. In one study, employees with low saving rates were randomly assigned to an intervention in which they were paid \$50 to read a short document about how their 401(k) plan works, including an individualized calculation of how much money they were losing by not taking full advantage of the match. This intervention had no effect on the employees' average 401(k) saving rates (Choi, Laibson, and Madrian 2008a). The

Enron debacle had no effect on the willingness of newly hired workers at other firms to choose to invest their 401(k) contributions in employer stock (Choi, Laibson, and Madrian 2005). Employer-sponsored financial education seminars have remarkably little effect on 401(k) enrollment (Madrian and Shea 2001b). A new easy-to-read prospectus proposed by the Securities and Exchange Commission—the "summary prospectus"—has no effect on investor choices (Beshears et al. 2009). Finally, making fees overwhelmingly salient does not lead investors to minimize them, even when investors are allocating real money among index funds. In one study, subjects are asked to allocate \$10,000 among four Standard & Poor's 500 index funds. To assist their decisionmaking, the subjects are told what an index fund is, given a one-page summary sheet that compares the fees of the four index funds, and given the four prospectuses. Only 10 percent of the subjects put all of their money in the low-cost index fund (Choi, Laibson, and Madrian 2008b).

Fourth, I worry that the life-cycle nature of investing is inherently biased against success. Our formative learning years occur when we have no investable assets, a situation which saps our motivation and diminishes our ability to learn by doing. Moreover, when we have the most assets we are entering a period of diminished cognitive function. For example, the median 25-year-old is around the 75th percentile in adult cognitive analytic function. By contrast, the median 75-year-old is below the 25th percentile in adult cognitive analytic function (Salthouse 2005). Most of this cross-sectional variation is due to age effects and not cohort effects (Salthouse, Schoeder, and Ferrer 2004). Dementia and pre-clinical dementia account for some of this decline, while "normal" aging processes account for most of the rest. Some research has begun to study the market consequences of these changes, arguing that older adults make worse financial choices than middle-aged adults (Agarwal et al. 2007). These life-cycle effects may blunt the efficiency of financial education. Early life education comes at the "wrong" time. Late life education targets a population with declining cognitive function.

Fifth, one of the potential payoffs of financial education might be to teach people that they need to save for retirement. However, this lesson seems to already have been learned. About two-thirds of U.S. households already self-report that they should be saving more for retirement (Choi

et al. 2002). Indeed, the problem with undersaving is not a lack of public awareness. Instead, the problem is a lack of action. Financial education might help on this front, but it might also have little effect to the extent that the principal problem is motivational (for instance, procrastination). More work needs to be done to unravel the multiple forces that contribute to low savings rates in households that self-report that they are not saving enough.

4. Cost-Effective Interventions that Improve Retirement Preparation

There are many kinds of inexpensive interventions that generate large increases in savings. I will discuss the role of automaticity, active decisions, and simplified savings mechanisms. These interventions are scalable, highly effective, and nearly cost-free.

The most effective savings interventions all incorporate some element of automaticity. When savings and diversification is automatic (and not compulsory), households have to go out of their way to undersave and underdiversify. Automatic features come in many forms: automatic enrollment, automatic savings rate escalation, automatic diversification, automatic rebalancing, automatic life cycle reallocation, and automatic annuitization. All of these features are now available in some 401(k) plans. The most successful 401(k) plans make good outcomes easy (meaning automatic) and bad outcomes hard (meaning that these plans require some effort on behalf of the plan participant). For example, automatic enrollment raises participation rates (at three months of tenure) from around 40 percent to around 90 percent (Madrian and Shea 2001a; Choi et al. 2002, 2004, 2006; Beshears et al. 2008). Automatic escalators have also been highly effective in raising the retirement savings rate (Thaler and Benartzi 2004).

Active decision mechanisms also increase the likelihood of good outcomes. Active decisions are generated by a deadline. Newly hired employees are required to indicate their preference regarding enrollment (for instance, within 30 days of their hire date). In an active decision regime, passivity is not an option (just like the choice of the employer-subsidized health plan). Requiring plan participants to actively decide whether they

should be saving or not raises participation rates (at one year of tenure) from around 40 percent to around 70 percent (Carroll et al. 2009).

Simplified enrollment has also been shown to dramatically raise enrollment rates. Reducing the transaction costs of enrollment (so that enrollment takes one minute instead of 15 minutes), raises participation by approximately 20 percentage points (Beshears et al. 2006; Choi, Laibson, and Madrian 2009).

Conclusion: Financial Physicians

I conclude by identifying parallels between the investment environment and the health care system. Employers offer a small set of carefully vetted health plans to their employees. Employees are required to make an active choice from this set (or opt out of employer-provided health care). Once an employee is in a health plan, physicians make many of their day-to-day health care decisions—for example, which tests should be ordered, what procedures should be done, and which medications should be prescribed. The employee can opt out of the prescribed therapy or get a second opinion. The most significant decisions—for instance, opting for surgery—are made by the patient with the advice and guidance of her physician. Health plans and physicians are regulated and licensed.

This health care system assigns most due diligence and monitoring roles to employers, health plans, and regulators. Day-to-day decision-making is delegated to physicians. We could organize the financial system in a similar way, with social institutions vetting and monitoring financial advisers, who in turn would play a role comparable to physicians. Annual financial check-ups would be routine. Portable databases would record each person's financial history and these histories could be shared with advisers at these check-ups.

Large employers and/or asset management firms would select and monitor groups of financial advisers. The integrity and rigor of the selection/monitoring process would be legally enforced. Safe harbor rules would reduce the cost of this oversight role. Small employers could choose advisers and asset managers approved by regulators (to take advantage of scale economies in selection and monitoring). Financial advisers who

work outside the boundaries of defined contribution plans would be registered, licensed fiduciaries who have a high level of training and no conflicts of interest (for example, commission-based compensation would be disallowed).

In such an environment, an investor would only need to know how to work with their financial physician. Investors would not prescribe their own financial medicine. In other words, people with low levels of financial literacy would be okay.

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The Behavioral Economics of the Labor Market: Central Findings and Their Policy Implications

Ernst Fehr, Lorenz Goette, and Christian Zehnder

Managers claim that workers have so many opportunities to take advantage of employers that it is not wise to depend on coercion and financial incentives alone as motivators. . . . Employers [believe] that other motivators are necessary, which are best thought of as having to do with generosity (Bewley 1995, 252).

Such employment policies are difficult to explain with an economic model in which all agents have strictly selfish preferences. In this paper we argue that these phenomena can be better understood if one acknowledges that a significant share of individuals has a preference for reciprocal fairness that leads them to work harder when they are treated fairly by their employers. We review the evidence on two major psychological forces that possibly drive such fair-minded behavior. First, research suggests that some individuals are willing to sacrifice considerable resources to prevent unfair outcomes: they may be willing to put in extra effort if they feel treated fairly, but they may also withhold effort if they feel treated unfairly (Fehr and Gächter 2000). Second, in making judgments about fairness, individuals compare what they (and others) get to what they think they (and others) are entitled to receive (Kahneman, Knetsch, and Thaler 1986). If any party receives less than this entitlement, fair-minded individuals will try to reduce this gap. A strong feature of these comparisons is loss aversion—that is, the tendency for losses to loom larger than gains.

The presence of these two forces makes specific predictions for the labor-market setting. In particular, high wages should motivate fair-minded workers to exert more effort to reciprocate such fair treatment. A recent literature has produced evidence from laboratory experiments capturing the stylized features of labor markets. The results show that,

indeed, average employee effort is higher when wages are high. Because all the interactions between workers and firms are strictly one-shot, this impulse can only be attributed to a preference for fairness. However, the results also show strong heterogeneity in how individuals respond to receiving high wages. While some employees are fair-minded and reciprocate with high effort, others are unfazed and choose to exert minimal effort, as predicted by selfish preferences.

More recently, field experiments have also been conducted to test this mechanism outside the laboratory. The results are similar to the laboratory experiments: high wages elicit somewhat higher effort, but the effects are generally small. Just like in the lab experiments, it seems that a considerable fraction of individuals is strictly selfish and take the higher wage without reciprocating by expending higher effort.

The behavior of these selfish individuals towards their employers may change, however, in repeated interactions, which more accurately represent most real-life employment relationships. In this more realistic setting, even a strictly selfish individual has an incentive to mimic a fair-minded person and exert high effort when offered a rent because this behavior guarantees high wages in the future. Shirking, in contrast, reveals that the individual is egoistic. Firms will be unwilling to pay wage rents to selfish workers once they have proven that they don't exert much effort. Thus, finite repetition can greatly magnify the effect of fairness on labor market outcomes. This mechanism can be tested explicitly in experiments and proves to be very powerful. We also discuss evidence from several field studies in which employers violated fairness norms in ongoing relationships with their employees. The results in these studies show that employees respond strongly to how they are treated.

What predictions does a fairness model make for wage dynamics? Given the evidence on how fairness is judged, the behavioral model of wage dynamics predicts that entry-level wages will be cyclical, while the wages of incumbent workers should be largely unresponsive to changes in labor-market conditions. Moreover, loss aversion implies that wage cuts should be particularly rare. We examine the experimental and field evidence on these issues and find the model's predictions largely confirmed in the data. The wages of newly hired workers are highly cyclical, while the wages of incumbent workers hardly respond to the business cycle. Further, there is

strong evidence that wages are downwardly rigid. In many cases, firms prefer to freeze workers' wages rather than cut their wages.

We then discuss several policy implications that emerge from the model, as well as the support for these prescriptions in the data. We consider two important aspects of economic policy—namely, monetary policy and minimum wages. In both cases, the psychological forces underlying the behavioral model of the labor market lead to important new conclusions. They also highlight the importance of forming a detailed understanding of the underlying psychological motivations of any outcome, as the policy implications may differ strongly depending on the model that generates the outcomes. Perhaps our strongest conclusion from the research that we survey here is to caution against “rationalizing”; that is, tweaking the economic model along “standard” dimensions to make it fit the data. We argue that such an approach can be highly misleading and lead to bad policies. Rather, we suggest that microfounding a model and testing each ingredient is a far more fruitful approach for the purpose of informing policy.

The rest of this study is structured as follows: section 1 summarizes the psychological forces central to understanding several labor market outcomes. Section 2 explains how these forces change the tradeoffs that firms face and summarizes the evidence on these tradeoffs. Section 3 illustrates how these findings can be relevant for policy. Section 4 concludes by summarizing how insights from behavioral economics can foster a better understanding of how the labor market functions, and how policies can be designed to build on these insights.

1. Psychological Forces

Standard economic analysis assumes that individuals' preferences are defined over their own consumption only, and that consumption enters the utility function in levels. However, evidence from two decades of research in experimental economics has shown this assumption to be wrong. In this section we discuss two of the most intensively discussed topics of this literature. The first concerns the assumption of selfishness, meaning that just one's own consumption enters the utility function. The evidence overwhelmingly shows that individuals also care about other

people's consumption. The second concerns the way in which consumption enters the utility function. While the standard economic model assumes that only the level of consumption matters, the evidence shows that the level of consumption is valued relative to a reference level. As we will discuss in section 2, incorporating these two features of preferences leads to a new understanding of important labor market phenomena.

Testing Self-Interest: Ultimatum Games

Striking evidence against the self-interest hypothesis comes from a large number of experiments which study the ultimatum game. In this game, introduced by Güth, Schmittberger, and Schwarze (1982), two players have to agree on how to divide a fixed sum of money. The game is structured as follows: At the beginning of the game, one player (the proposer) gets a money endowment. The proposer suggests how the endowment should be divided between him and the second player, the responder. To this end, the proposer makes a take-it-or-leave-it offer to the responder. Then the responder may only decide whether to accept or reject the proposer's offer. If the proposed split is accepted, the money is divided according to the proposer's offer. If the offer is rejected, both players receive nothing.

The self-interested model in standard economics makes a distinct prediction for the ultimatum game's outcome: the proposer offers the smallest positive amount of money, say \$1, and the responder accepts this offer. However, in strong contrast to this self-interest prediction, many experimental studies show that subjects are willing to reject unfair offers, even if this rejection is associated with substantial costs. A robust result across a large number of studies is that offers of less than 20 percent of the available surplus are rejected with a probability of 0.4 to 0.6. The probability of rejection decreases the larger the size of the offer. Once offers reach 40 to 50 percent of the available surplus, they are only very rarely rejected (see, for example, Camerer and Thaler 1995; Fehr and Schmidt 2003; Güth, Schmittberger, and Schwarze 1982; and Roth 1995). Rejections of low offers in the ultimatum games show that many subjects do not care solely about their own payoff. A plausible interpretation for the rejection of low offers is that subjects perceive them as unfair. Receiving the offer of only a small share when the proposer could have chosen a

more equitable split seemingly lowers the responder's utility. Yet if the utility-reducing impact of unfairness is strong enough, the responder may prefer to forgo his share in order to avoid accepting the unfair outcome.

Despite the large number of replications, the experimental findings in ultimatum games have often been contested. A very frequent objection is that the usual monetary amounts used in experimental economics are so small that people do not really care about the decisions they make in the experiments. However, several studies suggest that even very large increases in stakes (up to several months' wages) have surprisingly modest effects on behavior in ultimatum games (see, for example, Cameron 1999; List and Cherry 2000; and Slonim and Roth 1998). The data indicate that when the stakes are high, responders are slightly more reluctant to reject what they deem as unfair offers. But the fear of costly rejection motivates many proposers to make offers close to 50 percent anyway. Hence, increasing the monetary stakes does not destroy the strong impact of fairness considerations on bargaining outcomes.

The results of ultimatum games are also remarkably robust across different cultures. Studies that compare ultimatum behavior across industrialized countries find only relatively small differences in average offers and rejection rates. Although there seems to be some cross-country heterogeneity in the perception of what constitutes a reasonable offer in such an experiment, there is not a single industrialized country where the outcomes are even close to the self-interest prediction (see, for example, Buchan, Croson, and Johnson 1998; Roth, Prasnikar, Okuno-Fujiwara, and Zamir 1991). However, the impact of culture is much more pronounced when nonindustrialized societies also are investigated. A group of anthropologists and economists have studied ultimatum game behavior in a number of small-scale societies in Africa, Asia, and South America (Henrich et al. 2001, 2004). While many of these cultures exhibit behavior similar to the Western students who are the main subjects of these laboratory experiments, some societies follow a completely different behavioral pattern. For example, the Machiguenga in Peru make much lower offers and have much lower rejection rates than are usually observed in ultimatum games. This behavior is probably due to the fact that the Machiguenga mainly interact within their own families,

while transactions with others (even within a village) are very rare. In general, the comparison of the small-scale societies reveals that the stronger the cooperative activity (for example, collective hunting) and the higher the market integration (common language, trade, and developed labor markets), the more likely it is that a society's sharing norms will be close to equal splits. Hence, in contrast to the standard economic model, in which self-interest and functioning markets go nicely together, these studies reveal that people in societies in which markets play a bigger role behave less rather than more self-interestedly (see Camerer 2003 for a more detailed discussion).

While there is no longer much disagreement regarding the fact that people's behavior in the ultimatum game systematically deviates from the prediction of the rational self-interest model, researchers still somewhat disagree on how this evidence should be interpreted. It is probably fair to say that the majority of experimental and behavioral economists is convinced that the existence of fairness preferences is the most plausible explanation for the ultimatum game evidence.

However, some researchers prefer to interpret the rejections of low offers as a consequence of bounded rationality. Binmore, Gale, and Samuelson (1995) and Roth and Erev (1995) suggest a learning explanation grounded in self-interest. In both arguments the basic idea is that the speed of learning depends on the cost of the error. Since rejecting low offers is not very costly, responders only learn slowly that this choice is not optimal. In contrast, because rejections are very costly to the proposers, they quickly realize that low offers are not profitable. Therefore, this asymmetry in learning makes convergence to the equilibrium very slow. Indeed, simulations in these papers show that thousands of iterations may be necessary to bring behavior close to the standard prediction. While we certainly agree that learning models are useful tools to understand behavioral changes in complicated dynamic setups, we doubt that these models are adequate to describe the behavior in simple environments like the ultimatum game. The responders' decision is so straightforward that it seems hard to believe that they systematically fail to make the optimal choice dictated by their preferences.

Other economists put forward the idea that the behavior in ultimatum games may be a consequence of the erroneous application of repeated

game behavior to one-shot laboratory environments (for instance, see Binmore 1998 and Baker in this volume). The argument is that in everyday life the vast majority of decisions are made in the context of repeated interactions. It is well-known that the rejection of low offers can be sustained in equilibrium in an infinitely repeated game of agents with purely selfish preferences. Since people are used to repeated interactions, the appropriate behavior therefore may evolve into a social norm, which people then also apply in the somewhat unnatural one-shot situations in the laboratory. One problem with this explanation is that the folk theorem tells us that, in general, repeated games are characterized by a plethora of equilibria (for details, see Rubinstein 1979). In some cases, the different equilibria can be Pareto-ranked such that efficiency arguments yield equilibria that may be considered focal. In other games, however, such refinements do not work: in the ultimatum game, for example, every outcome in which the responder accepts the proposer's offer can be sustained as an efficient equilibrium of an infinitely repeated game. Thus, while an equal split is a possible outcome of the repeated game, so is any other division of the pie. Accordingly, this alternative approach is not able to explain why people tend to share the pie equally in the ultimatum game.

On the other hand, we agree that everyday experience affects behavior in the laboratory. This is evident from the results of the cross-cultural experiments in small-scale societies: groups who strongly depend on cooperative behavior are more likely to share the pie equally in experiments. However, the fact that external social norms affect people's behavior does not imply that they are unable to distinguish one-shot games from repeated setups. In contrast, the available evidence shows that the possibility of acquiring a reputation or repeatedly meeting the same partner strongly increases people's willingness to cooperate and to punish uncooperative behavior. For example, Fehr and Fischbacher (2003) show that responders in an ultimatum game have a significantly higher acceptance threshold if future proposers are informed about their current rejection behavior than if future proposers do not get information about the responders' past behavior (for more laboratory evidence on the impact of reputation formation on behavior, see Andreoni and Miller 1993; Brown, Falk, and Fehr 2004; Brown and Zehnder 2007; Engelmann and Fischbacher 2009; Fehr and Zehnder 2009; Gächter and Falk

2002; and Seinen and Schram 2006). In our view, the fact that changes in strategic incentives strongly affect people's behavior makes it unlikely that the deviations from selfish behavior in one-shot games are merely the consequence of mistakenly applied rules of thumb. It seems much more plausible that the prevailing social norms in a society directly affect their preferences.

Furthermore, recent evidence from neuroeconomics provides additional support for the assumption that people have a preference for fairness. For instance, the human brain's reward center is activated if people can punish others that defected against them in a game (De Quervain et al. 2004). Singer et al. (2006) conduct a study in which a confederate cooperates or defects against the subjects in a first experiment. In the second experiment, the confederate and the subjects receive moderate electric shocks. If the confederate defected in the first experiment, the reward center of the subject activates when the confederate receives an electric shock. These results show that there is a direct link from experiencing unfair behavior against oneself to gaining utility by retaliating.

The combination of these arguments makes us think that the assumption that a considerable share of people prefer fair outcomes is the most plausible explanation for the observed behavior in ultimatum games and in related experiments.

The Reference Frame for Judging Fairness

In laboratory experiments like the ultimatum game, the equal split of a surplus seems to serve as a natural reference transaction by which the fairness of an outcome is evaluated. However, it is rather unrealistic to assume that this finding can be extended to other settings. Thus, in general, the determination of the fairness standard or the reference transaction is likely to be dependent on the specific environment and context. Kahneman, Knetsch, and Thaler (1986) use questionnaires to investigate how standards of fairness are determined. Their study presented participants with a number of different business scenarios. In each scenario, a firm either lowered wages or increased prices in response to an external shock. For each scenario, the participants had to indicate whether they perceived the wage (price) change to be acceptable or unfair. The responses of participants confirm the hypothesis that the perceived fairness of an action is also dependent on the specific context. In one question,

for example, they asked the participants whether they found it acceptable for a firm to lower a current employee's wage after an increase in unemployment had enabled other firms to hire similar workers at lower wages. A large majority of people perceived this action as unfair. Here, it seems that the fairness standard is determined by the past interactions between the worker and the firm. If in the same situation, however, the current worker left and the firm hired a new worker at a lower wage, the majority of people found this acceptable. In this case, the new worker and the firm do not have a common history, and, accordingly, the interactions of other workers with other firms serve as the reference point for evaluating fairness. This example shows that past interactions provide a context that has a strong impact on the reference transaction used to judge the fairness of a particular action: identical outcomes may trigger very different fairness judgments if they take place in different contexts.

Falk, Fehr, and Fischbacher (2003) provide experimental evidence on how the economic environment can affect fairness judgments by conducting a series of ultimatum games in which they restrict the proposer's strategy set. In one treatment, the proposer has an endowment of ten and can offer either two or five to the responder. In the second treatment, the proposer can offer either zero or two from the endowment of ten. Thus, in both treatments, there is an offer that gives the responder two, while the proposer gets eight. Their results show that the offer of two is more frequently rejected when the proposer could have offered five than when the proposer could have offered zero.

This finding suggests that the set of available actions determines the fairness standard. When the alternative is an offer that would have given five to both subjects, the offer of two is often rejected because it is perceived as unfairly low. However, when the only alternative is the offer of zero, then the offer of two is considered kind, and, accordingly, the rejection probability is low. The behavioral evidence provided in this study confirms that not only the chosen action and its consequences matter—because in different situations the same action may reveal completely different underlying intentions. Thus, the fairness of a particular action is often not only determined by the resulting payoffs but also by the set of available, yet unchosen, alternatives.

In the context of labor markets, an enduring and important question is whether individuals only take into account the real buying power of

their wages, or whether they also care about nominal wages. Dating back at least to Keynes (1936), some economists have assumed that individuals are not fully aware of changes in the price level, and thus also care about the nominal wage. In the context of fairness judgments, Shafir, Diamond, and Tversky (1997) have demonstrated that individuals understand how inflation changes their purchasing power, and correctly take this factor into account when judging economic welfare in hypothetical scenarios. However, Shafir, Diamond, and Tversky (1997) consistently show that individuals also believe that higher *nominal* wage growth leads to higher happiness for other individuals, suggesting a strong belief that other individuals are unable to back out the inflation rate correctly. Consistent with the first set of results in this study, other studies of consumers' inflation expectations have also shown these to be surprisingly accurate predictions (see Mankiw, Reis, and Wolfers 2004). This evidence seems to suggest that individuals may well be able to anticipate price changes, but that they often believe that others cannot.¹ Goette and Huffman (2007b) show that for fairness judgments, individuals only care about real wage changes as long as the nominal wage is increasing. However, if a nominal wage cut occurs, this is viewed as distinctly more unfair. Goette and Huffman argue that the salience of a nominal wage cut triggers strong negative emotions that sway the fairness judgment: because a wage cut is perceived as unambiguously bad, this event leads to a much stronger reaction than when the nominal wage is rising but more slowly than the price level. Therefore, this evidence suggests that for fairness judgments, individuals do take into account the price level and effectively care about real wage changes. However, if the nominal wage were to be cut, this leads to a stronger reaction.

Wage cuts, in general, seem to be perceived as much more unfair than the same reduction in pay if these cuts are perceived as eliminating a gain. For example, Kahneman, Knetsch, and Thaler (1986) gave respondents a scenario where a firm used to pay a 10 percent bonus every year, but then abolished this practice. The vast majority of respondents considered this action to be fair, even though it effectively cut the workers' incomes by 10 percent. Other respondents were given a scenario where the workers base wage was cut by 10 percent. In this case, the majority of the respondents stated that the action was unfair.

Reference-Dependent Utility

The previous examples showed that in an economic environment fairness judgments depend upon reference transactions. This property is not limited to the realm of fairness judgments. In a seminal paper, Kahneman and Tversky (1979) argue that utility depends not only on the level of consumption, but on reference points to which consumption is compared. Kahneman and Tversky propose two central features of how individuals value outcomes relative to reference points: (i) *loss aversion*: falling short of the reference point by one unit hurts more than exceeding it by one unit is pleasurable; (ii) *diminishing sensitivity*: Kahneman and Tversky argue that the marginal benefit decreases with the distance from the reference point.

Loss aversion provides a rationale for why individuals exhibit risk aversion over small stakes and buy, for instance, home telephone-wire insurance or extended warranties. As pointed out in Rabin (2000) and Rabin and Thaler (2001), expected utility theory cannot accommodate risk aversion over such small stakes without making predictions that seem outright crazy. For example, most individuals would reject a coin flip in which they could win \$110 or lose \$100.² If an expected-utility maximizer rejects this coin flip for any wealth level between her current status quo and, say, \$100,000 more, then she would be unwilling to accept a 50:50 gamble in which she can win \$100,000 or lose \$220, irrespective of the specific shape of the individual's utility function.³ Such behavior, which is implied if expected utility is to account for the rejection of small-stake gambles, strikes us as implausible, and makes it clear that something else is needed to explain why individuals reject such gambles. If individuals compare outcomes relative to a reference point such as the status quo, it is easy to see how loss aversion can lead an individual to reject the small-stakes gamble while avoiding the implausible implications for larger stakes.

Loss aversion can also help explain a behavioral pattern in a different area of research. In a classic study by Kahneman, Knetsch, and Thaler (1990), individuals value a good more highly when they have to give it up than when they can acquire it. This is consistent with the interpretation that individuals perceive having to give up an object they expected

to keep as a loss, while they perceive acquiring the same object as a gain, and spending the money to obtain it as a loss. Therefore, loss aversion predicts that selling reservation prices should be higher than buying reservation prices. A recent study by Gächter, Johnson, and Herrmann (2007) measures subjects' loss aversion over risky gambles and the buying and selling prices for model cars. Indeed, they find that more risk-averse individuals tend to display a disparity between the prices at which they are willing to buy and to sell.

The evidence with respect to diminishing sensitivity is more mixed. Diminishing sensitivity predicts increasing marginal utility towards the reference point; that is, it predicts a concave valuation function over gains, but a convex valuation over losses. This implies that individuals should be more willing to gamble when deciding between a sure loss or an unfair lottery offering the chance of not incurring any loss. Kahneman and Tversky (1979) found strong evidence of this pattern, but many of the scenarios were hypothetical. Several studies have now shown that the incidence of risk-seeking over losses is smaller when the decisions involve real payoffs (Holt and Laury 2002).

Recent evidence has renewed researchers' interest in diminishing sensitivity. Post et al. (2008) examined contestants' behavior on the game show *Deal or No Deal*. Some lucky contestants are virtually certain to win large amounts of money, while it quickly becomes clear to others that it is very unlikely they will win much. There is a strong shift in the risk preferences of the contestants who have bad luck: their strategies become risk-seeking, while the strategies of the lucky contestants become highly risk-averse. These preferences are consistent with the interpretation that unlucky contestants face losses relative to what they could have expected from the game, and their diminishing sensitivity to these likely potential losses makes them risk-seeking in an effort to reverse their luck for the better.

The principles of valuation laid out in prospect theory seem to apply to a wide range of phenomena. For example, Heath, Larrick, and Wu (1999) report that (arbitrarily set) goals (for example, "do 50 push-ups") assume the properties of a reference point. Falling short of a goal by one unit hurts more than the pleasure of exceeding it by one unit. An exten-

sive literature also documents the goal-gradient effect. The goal-gradient effect means that an individual will exert more effort to reach a goal the closer he is to the goal. The effect can easily be explained by diminishing sensitivity and loss aversion: if goals inherit the properties of reference points, then the closer an individual is to the goal the steeper the value function, and hence the higher the marginal utility gained from making progress towards the goal. Once the goal is achieved, making additional progress only feels like a small gain and is consequently worth less.

Reference-dependent preferences can have important implications in labor markets, which we detail in the next section. They also have important implications in other areas, as recent research has shown. Odean (1999) shows that investors are much more likely to hold on to stocks with paper losses in their portfolio, despite the strong tax incentives to sell them. Similarly, Genosove and Mayer (2001) find that a homeowner facing a loss (relative to the home's purchase price) asks a higher selling price, holding all other characteristics of the house constant. They also find evidence of diminishing sensitivity: at the margin, a small loss increases the asking price proportionally more than a large loss. Gächter, Johnson, and Herrmann (2007) demonstrate the effect of reference-dependent preferences when evaluating a product's attributes: prospective car buyers require a much larger reduction in a car's purchase price if a feature is removed than they are willing to pay to have the feature added. This disparity again correlates with behavior in lotteries with small-stake risks, lending further credence to the interpretation that loss aversion is driving these disparities.⁴

2. A Behavioral View of the Labor Market

Firms invest significant resources to treat their employees well (see, for example, Bewley 1999): they pay high wages to elicit high effort, refrain from cutting wages because they fear the negative consequences for employee motivation, and provide various other benefits in the hopes of keeping their employees satisfied. However, in the standard model in economics, firms need not care about fairness. In fact, as we argue below, even in the context of repeated interactions, treating employees well

makes no business sense. The reason is that, ultimately, any interaction an employee has with a firm is finite. Paired with *strictly* selfish preferences, this makes repeated-game incentives typically ineffective.

However, the psychological forces we outlined in section 1 can alter the calculus of firms dramatically. Even in one-shot interactions, fair treatment by firms may motivate workers to exert more effort. Further, as we explain, these effects are amplified when combined with *finite* repetition, as it is plausible in many circumstances.

In this section, we outline the predictions of an economic model of the labor market that incorporates the psychological forces reviewed in section 1. We argue that key aspects of labor markets can be better understood if one takes these forces into account. For example, phenomena such as the very rare occurrence of nominal wage cuts, or the observation that employees do not respond to incentives as predicted by the standard theory, can be explained if concerns for fairness and loss aversion are taken into account.

Characteristics of Employment Relationships

A central observation is that the typical employment relationship is characterized by incomplete contractual agreements and repeated interactions. Contractual incompleteness means that the legal agreement between the firm and its workers does not determine many relevant details of the workers' jobs. The reason for the incompleteness of employment agreements is that most occupations consist of multidimensional and complex tasks that can neither be completely foreseen nor perfectly described. Incomplete contracts imply that outsiders can hardly determine whether the trading parties have met their obligations or not. As a consequence, important aspects of the collaboration of firms and workers cannot be enforced by third parties. This lack of enforceability obviously creates a fundamental problem for the firm: if its employees' duties and obligations are only vaguely specified, how can a firm motivate its workers to provide more than minimal effort?

Sometimes the answer to this problem is straightforward. In simple jobs, it may well be that a worker's output is relatively easy to measure. In these cases the firm can use explicit incentive contracts to motivate its workers. If a worker's earnings depend on his output, it may be in his

interest to exert nonminimal effort, even though nobody can force him to do so.⁵ In many jobs, however, output is complex and while some dimensions are objectively measurable, others are not. In all these cases, the provision of explicit incentives may lead to distorted outcomes because workers will allocate all of their effort toward those activities that are rewarded by the firm's incentive scheme. Thus, if workers are expected to devote time and effort to activities which contribute to the nonmeasurable dimensions of output, incentive pay cannot be effectively used. In the literature, this so-called "multi-tasking problem" has been emphasized as one of the main reasons why employment contracts often stipulate a fixed wage payment (see, for example, Baker 1992; Holmstrom and Milgrom 1991).

However, several researchers have argued that even if firms are constrained to paying fixed wages, they can make use of the long-term nature of employment relationships to force high levels of effort from workers. If firms and workers have the possibility of interacting repeatedly, firms can condition the future terms of their employment contracts on workers' current performance. This incentive can motivate workers to provide high effort because the firm will only continue to pay a high wage in the future if they exert the desired effort today (see Bull 1987; Hart and Holmstrom 1987; MacLeod and Malcomson 1989, 1993, 1998; Shapiro and Stiglitz 1984). While intuitively appealing, this argument has a serious flaw: as long as all market participants have completely selfish preferences, it requires infinite repetition. If either firms or workers have a finite time horizon, backward induction implies that performance in employment relationships collapses to the minimal required effort. For firms, the assumption of an infinite time horizon can be justified. Even though firm owners have finite durations, they may be interested in maximizing their firm's long-term value because this value determines the revenue they will receive when they sell their assets to a successor after retirement. The assumption of infinitely lived workers, in contrast, is problematic. Employees retire from the work force with certainty. Thus, from the perspective of workers, the relevant duration of an employment relationship may be long, but always finite. But this sets in motion a process of unraveling: as soon as a worker is close to retirement, the firm can no longer threaten to lower wages in the future (because there is no future). As a

consequence, there is no way for the firm to keep a selfish worker from shirking shortly before retirement. However, since the firm anticipates the worker's behavior, the firm has no incentive to pay a high wage in the period before retirement. This, in turn, destroys the employee's incentives to work in the second-to-last period. This argument can be iterated back to the beginning of the employment relationship. Consequently, finite repetition with selfish workers cannot motivate above-minimum effort.

The prediction that repeated interactions do not change labor market outcomes seems counterintuitive in light of everyday experience. Indeed, we argue that repeated-game incentives in the labor market are effective *because* some individuals care about fairness. In this case, paying a high wage can elicit high effort even in a one-shot interaction. It is the preference for fairness that leads a fair-minded worker to choose a higher effort to raise the firm's payoff because the firm just raised his payoff (Akerlof 1982; Akerlof and Yellen 1988, 1990; see Benjamin 2006 for a formal model). Yet while many individuals may have such strong preferences for fairness, the evidence reviewed in section 1 also suggests that a considerable fraction of individuals is more or less selfish. As a consequence, the one-shot effects of fairness are not always strong enough to make it profitable for firms to pay noncompetitive wage premia.

However, only few people with fairness concerns are needed in order for repeated interaction to have a powerful impact. The results of the seminal paper by Kreps et al. (1982) imply that the presence of a small fraction of nonselfish workers can give perfectly selfish workers an incentive to work hard, even though the worker and firm interact only a finite number of times (see Brown, Falk, and Fehr 2004 for an example). The role of repetition in this case, though, is different from the one in games with an infinite horizon. The intuition is that selfish agents now have an incentive to exert effort in order to maintain a reputation and make the firm believe that they are (at least potentially) fair-minded. Such a reputation is valuable for selfish workers because finite repetition implies that the firm only pays noncompetitive rents to workers who have not yet been identified as selfish. The firm anticipates that a selfish worker will always shirk in the final period of the interaction, which unravels all incentives to pay a rent in any period to a worker known to be selfish. Fair-minded workers, in contrast, exert effort whenever they are paid a fair wage

involving a rent. Thus, if the belief about a worker's fair-mindedness is high enough, the firm will be willing to pay a high wage even in the last period: even though the worker may turn out to be selfish sometimes, *ex ante* it may still be profitable to pay a rent in the last period because such a wage will still cause the worker to exert effort if he is fair-minded. Note that it is the payment of a rent that disciplines the selfish worker, because if the worker shirks he will be identified as a selfish type which, in turn, implies that he will not be paid a rent in the future.⁶

Gift Exchange in One-Shot Interactions

For the above argument of long-run reputation building to work, a necessary condition is that there is some gift exchange in one-shot relationships; that is, there are at least some individuals who respond to higher wages—or better treatment more generally—by exerting more effort. Therefore we examine the evidence from one-shot employment relationships in this subsection.

The cleanest test of gift exchange comes from laboratory experiments that contain essential strategic features of an employment relationship. As we will show in the first part of this section, the laboratory studies provide clear evidence that a nonnegligible number of individuals are fair-minded and respond to receiving higher wages by exerting more effort. However, the evidence also shows that fairness concerns alone may not be sufficient to make it profitable to pay noncompetitive wage rents. In fact, if fairness concerns are the only force that drives worker effort above nonminimal levels, there are (in general) large unexploited efficiency gains.

In interpreting the lab studies, there is a natural concern that the effects of gift exchange found in the lab may not carry over to the real world (Levitt and List 2007). In particular, because the environment in the lab experiments is intentionally stylized, it is not obvious whether the effects of fairness may also be measurable in labor markets outside of the laboratory. In the second part of this section we discuss a number of field experiments which explicitly manipulate the wages paid to workers in real-life work environments. In general, these studies suggest that the effects of wage changes on workers' productivity may also be present in the real world, even in setups characterized as one-shot interactions.

Evidence from the Lab

Laboratory evidence for the positive impact of wage rents on the amount of effort a worker exerts come from studies of the so-called gift exchange game, which has been introduced by Fehr, Kirchsteiger, and Riedl (1993). The gift exchange game is a two-player game that captures the basic features of a principal-agent relationship with highly incomplete contracts. The structure of the game is as follows: First, the employer offers a wage which the worker can accept or reject. In the case of acceptance, the worker must choose an effort level which cannot be enforced by the employer. If the worker rejects the offer, he receives an unemployment benefit. The firm's profit is equal to the returns generated by the worker's effort minus the cost of this effort. The worker's payoff, in turn, is calculated as the wage minus the effort's cost. The parameters are chosen in such a way that the efficient outcome is achieved when the worker chooses the maximal effort level, but because the provision of higher effort is associated with additional costs, the self-interest model predicts that the worker always provides the smallest possible effort.

However, if fairness considerations matter and workers are willing to reciprocate generous wage offers by providing higher effort levels, it may be profitable for employers to induce a gift exchange by offering wages that exceed the worker's outside option. It is interesting to consider the extreme case of workers with a very strong preference for fairness.⁷ In the model used in Benjamin (2005), workers try to equalize the surpluses (the material payoffs minus the reference payoffs they feel are appropriate) they and the firm receive. Thus, if the firm increases their wage by \$1, the workers need to close a \$2 gap in surplus. It is interesting to ask what wage a profit-maximizing firm will set in this context. It turns out that the firm will set a wage that induces the workers to choose the efficient effort level. The key idea behind this insight is understanding how the workers close the \$2 surplus gap after a \$1 increase in their wage. As long as the marginal cost of their effort is below the marginal product of effort supplied to the firm, the firm's revenues will rise by more than \$1, while the workers' cost will rise by less than \$1 in closing the gap. Hence, a \$1 wage increase raises profits. This increase is profitable for the firm up to the point where the marginal cost of effort equals the marginal revenue product of effort, which coincides with efficiency

(see Benjamin 2006 for an extensive discussion and a formal proof). In the case of the gift exchange game discussed here, the prediction is that firms set their wage offer such that it induces the maximal effort level.

A typical example for a one-shot gift exchange experiment is the baseline treatment in Brown, Falk, and Fehr (2004). They implement a simple labor market that has an excess supply of workers. In every period each employer can at most hire one worker and each worker can at most have one job. Although the market runs for several periods, reputation formation cannot play a role because employers cannot distinguish workers from each other when they make wage offers.

The main results of this treatment can be summarized as follows: in line with the wage-effort hypothesis, a worker's effort indeed depends on the firm's wage offer. On average, if firms offer higher wages, workers will provide more effort. As a consequence wages and efforts are higher than predicted by the self-interest model. On average, efforts settle at a level of about 3 (on a scale of 1 to 10). However, while the effort level is significantly higher than the minimal effort of 1 (which the self-interest hypothesis would predict), it is also far from the efficient effort level of 10 (which the complete fairness model would predict). The reason why gift exchange does not have a more strongly positive effect on market performance is due to the huge inter-individual differences across subjects. While there is a considerable fraction of workers whose effort provision exhibits a strongly reciprocal pattern, there is also a substantial fraction of workers who often make purely selfish choices. The relationship between wages and effort is steep enough to render nonminimal wage offers profitable, but the presence of selfish agents restrains many principals from making wage offers which would be high enough to induce efficient effort levels from fair-minded workers. Thus, on the one hand, the evidence in this study confirms the empirical relevance of the gift-exchange hypothesis because, on average, worker effort depends positively on wages. On the other hand, the study also reveals that the impact of one-shot gift exchange interactions on aggregate market efficiency may be small relative to the first-best solution.

The finding that gift exchange has a positive but small impact on market performance is very robust and has also been found in a number of

Table 1
Gift Exchange in One-Shot Interactions: The Laboratory Evidence

Study	Average Effort	Effort Range	Number of Periods	Students as Subjects	Excess Supply of Workers
Fehr, K, and R (1993)	0.40	0.1 -1.0	12	Yes	Yes
Fehr and Gächter (1998)	0.44	0.1 -1.0	12	Yes	Yes
Fehr et al. (1998)	0.37	0.1 -1.0	10	No	No
	0.40	0.1 -1.0	10	No	Yes
Gächter and Falk (2002)	0.41	0.1 -1.0	10	Yes	No
Hannan et al. (2002)	??	0.1 -1.0	12	Yes	Yes
Charness (2004)	0.31	0.1 -1.0	10	Yes	No
Charness, F, and K (2004)	0.32	0.1 -1.0	10	Yes	No
	0.23	0.1 -1.0	10	Yes	No
Brown, F, and F (2004)	3.30	1 -10	15	Yes	Yes
List (2006)	3.50	1 -10	5	No	No
	2.30	1 -5	5	No	No
	2.50	1 -5	1	No	No

Source: Authors' calculations.

other laboratory studies using students as participants (see table 1 for an overview). Charness (2004); Charness, Frechette, and Kagel (2004); Gächter and Falk (2002); Fehr and Gächter (1998); Fehr, Kirchsteiger, and Riedl (1993); and Hannan, Kagel, and Moser (2002) all report evidence from one-shot gift exchange experiments and find results which are very similar to the ones reported above: wages and effort levels are always positively correlated, but the realized effort level is far from efficient. Fehr et al. (1998) and List (2006) replicate the gift exchange findings in laboratory experiments with nonstudent subject pools (soldiers and sports card enthusiasts, respectively) and find that the realized effort levels are in line with the evidence presented in the previous literature.

Despite the fact that fairness concerns alone leave a large part of the available welfare gains unexploited, it should be emphasized that gift exchange is a remarkably robust finding even in competitive environments. In many of the studies listed above there is an excess supply of workers, such that workers compete for jobs (see table 1). Nevertheless,

firms are willing to pay noncompetitive rents to workers in order to elicit nonminimal effort levels. Fehr and Falk (1999) test the robustness of this result in yet another way. They induce a labor market with excess labor supply in which both firms and workers can make wage proposals in a double auction. In many cases, this institution is known to quickly converge to the competitive equilibrium predicted by the standard model. They find that firms do not hire workers who try to underbid the going wage because they fear that they will attract shirkers. The unemployed workers try very hard to gain employment by offering to work for very low wages, but the firms prefer to pay higher wages, as, on average, this guarantees them higher effort. Thus, gift exchange prevails even in highly competitive environments.

Evidence from the Field

Many years ago social psychologists were testing whether generating feelings of being overpaid or underpaid affected work effort (for a review, see Goodman and Friedman 1971). For example, Pritchard, Dunnette, and Gorgenson (1972) attempt to manipulate these perceptions directly by giving (mostly deceptive) information about whether the wage they were paying was considered high or low. The results are generally supportive of the view that if individuals feel treated generously (meaning if they feel overpaid), they exert more effort. However, few economists would accept this evidence, as it relies heavily on deceiving the subjects (see Roth 1995 for a discussion of the use of deception in experiments). Several recent studies explicitly manipulate the wages paid to real-life workers in order to assess whether a higher wage translates into higher effort, much like it does in the experiments. The idea is to test the proposition that higher wages are perceived as more fair and, consequently elicit higher effort. In a pioneering study, Gneezy and List (2006) hire workers to enter books into a library information system. The workers are made aware that this is a one-time employment situation (as once the books are entered, there is no further work for them). The workers are paid hourly, either \$12 (in the baseline condition) or \$20 (in the gift exchange condition), with no particular reason given for the pay rate. Based on the theory reviewed above and the lab experiments, the prediction is that effort will increase when the subjects are paid \$20. Overall, output is approximately 10 percent higher, but this difference is not significant. Thus, while the point

estimate is sizable, it is not significant because of the variation in individual output levels and because of the small sample sizes used in this study. More alarmingly, the authors find a decline in the treatment effect after only a very few hours. Their interpretation is that gift exchange in real life erodes after a very brief period, and effort falls back to its original level. However, it is difficult to back up this interpretation with strong evidence because their sample size is so small.

In a rich study, Al-Ubaydli et al. (2008) correct this problem: their samples are larger (30 subjects per condition, compared to approximately 10 in Gneezy and List 2006). The cleanest comparison in the study is the comparison of the following two conditions. All workers were recruited through a temporary employment agency and given only a vague hourly wage band (\$8 to \$16) that they would be paid for the work that they did stuffing envelopes.⁸ Subsequently, some workers were then paid \$8, while others were paid \$16. Again, nothing was said about why they were paid this wage rate, but presumably the individuals who were paid \$8 per hour felt treated less fairly than individuals paid \$16 per hour. There is a large and highly significant output difference between the two treatments ($p < 0.01$): when paid \$8 per hour instead of \$16 per hour, the number of finished envelopes decreased by about 22 percent. Further, this difference showed no sign of declining—if anything, it increased with time. Clearly, the fixed wage rate affected performance in this treatment. However, it is unclear whether the effect was mainly caused by workers feeling that they were treated unfairly after their wage band was announced and they received \$8 an hour instead of \$16, or whether the highly paid workers put in extra effort.

Thus, while it is quite clear that the fixed wage rate affects worker effort, the main treatment in Al-Ubaydli et al. (2008) makes it difficult to disentangle how much of the effect was caused because the workers in the \$8 treatment felt they got less than they were entitled to receive, or because the workers in the \$16 treatment felt they received more compensation. Another study by Kube, Maréchal, and Puppe (2008) provides a very clear manipulation of fairness and unfairness relative to a reference level. When all workers were hired, it was announced that they would be paid 15 euros per hour. There are three treatments. In the fair treatment, the workers are familiarized with their task (entering data into

a library information system), and the subjects were then told that they would be paid 20 euros, not 15, without any explanation for the difference. In the baseline treatment, the subjects were paid 15 euros. In the unfair treatment, the subjects were paid 10 euros, again without an explanation why. Subjects in the fair condition worked about 10 percent more than did subjects in the baseline condition, though the difference is only significant at $p = 0.165$. Again, very large individual differences in the baseline output and the small sample size (10 subjects per condition) do not allow for interpreting clear-cut results in the fair treatment. However, in the unfair treatment the reduction in effort relative to the baseline condition is so large (27 percent) that it is significant despite the small sample. These results are in line with evidence from lab studies that find fair treatment has small effects on behavior, but that unfair treatment has large effects on behavior (see, for example, Offerman 2002).

A potential problem in Gneezy and List (2006) and Kube, Maréchal, and Puppe (2008) is that the only manipulation done is by offering higher pay without providing any explanation for the difference. Yet firms go out of their way to stress how well they pay their employees to underscore a salient contrast: comparing the treatment they afford to their employees to what workers would get elsewhere.

Only few studies offer a more specific manipulation of fairness perceptions. Cohn, Goette, and Fehr (2007) implemented a wage increase during a newspaper promotion. The newly launched newspaper hired workers from a marketing agency to distribute their newspaper. In their treatment, the workers were given a CHF 5 increase over their regular pay of CHF 22 and asked to approach the passersby as actively as possible *in return for the higher pay*. In a control treatment, the workers were simply asked to approach the passersby as actively as possible. The promotion was limited in time—in fact, each employee only worked a few days for the newspaper. Thus, the interactions between the workers and the newspaper which implemented the extra pay can essentially be considered a one-shot situation. In an anonymous survey conducted by the marketing agency, the workers clearly stated that they perceived the wage increase as generous, showing that manipulating the fairness perception was effective. The increase in productivity was moderate but statistically significant: depending on the specification,

productivity was around 4 to 5 percent. In a follow-up survey, the authors also asked the workers whether they perceived that the base pay for this job was adequate. Interestingly, the subjects answering that the base pay was inadequate responded significantly more strongly to the intervention. This result is consistent with the prediction from fairness models: the perceptions of the workers who felt that they were treated unfairly were impacted the most by the CHF 5 wage increase. Consequently, they raised their effort more.

A different method of attempting to make the fairness manipulations stronger is reported in Kube, Maréchal, and Puppe (2006). This study varied the form of the gift made to the subjects. The authors hired students to enter data and announced wages would be 36 euros for the three-hour work period. There were three treatments. In the baseline treatment, the students were paid the 36 euros, as announced. In the fair treatment, the subjects were told that they would be paid 43 euros, not 36. In the gift treatment, the subjects were given a Nalgene bottle worth 7 euros at the beginning of the work episode. The idea behind the gift treatment was to manipulate the subjects' perception of kindness: in the case of the Nalgene bottle, it was clear that the experimenter went out of his way to be nice to the subjects by giving them a gift. Thus, if fair and kind treatment increases work effort, this gift treatment should work better than simply paying the subjects more. The results show the usual small effect of a monetary gift: when paid an extra 7 euros, subjects entered approximately 6 percent more data than in the baseline treatment. As usual, the effect is not large enough to be significant. However, in the gift condition, the subjects entered 30 percent more data ($p < 0.01$).⁹ A plausible interpretation is that gift exchange in this example is facilitated because it strengthened the signal that the employer cares about the worker, and thus made the difference to the reference transaction more salient. The evidence presented in Kube, Maréchal, and Puppe (2008) suggests that such subtleties are potentially important.

Table 2 summarizes the evidence from field experiments on gift exchange and for each study provides the elasticity of output with respect to the wage. Overall, the field studies on gift exchange show that the hourly wage paid to workers affects their productivity even in short-term jobs that lack the prospect of repeated exchanges—thus indicating that fairness concerns do affect productivity. Moreover, the effect of wage

Table 2
Gift Exchange in One-Shot Interactions: The Field Evidence

Study	Treatment	Elasticity	Type of Task
Kube, M, and P (2008)	Wage Increase	0.30	library task
	Wage Cut	0.82***	library task
Kube, M, and P (2006)	Wage Increase	0.31	library task
	Gift of same value	1.54***	library task
	Gift, value indicated	1.36***	library task
Cohn, G, and F (2007)	Wage Increase	0.16**	newspaper promotion
Gneezy and List (2006)	Wage Increase	0.15	library task
	Wage Increase	0.38	fund-raising task
Bellemare and Shearer (forthcoming)	Wage Increase	0.25***	planting trees
	\$8 to \$16 announced	0.44***	stuffing envelopes
Al-Ubaydli et al. (2008)	Paid \$8 or \$16		

Source: Authors' calculations.

Note: ** and *** indicate significance at the 5-percent and the 1-percent level, respectively. Elasticities are evaluated as percentage changes relative to the baseline condition.

variations is always in the predicted direction—wage cuts or wage levels that are likely to be interpreted as a violation of a fairness norm cause output reductions (Al-Ubaydli et al. 2008; Kube, Maréchal, and Puppe 2008) while wage variations that may be interpreted as an increase in fairness tend to increase output (Gneezy and List 2006; Kube, Maréchal, and Puppe 2006, 2008). In addition, the empirical pattern supports the view that losses loom larger than do same-sized gains because wage cuts that violate fairness norms trigger stronger output reductions than same-sized wage increases. In Kube, Maréchal, and Puppe (2008), for example, cutting the hourly wage by 5 euros relative to the baseline causes a large output reduction (with an implied elasticity of output with respect to wage of 0.82) while a 5 euro wage increase in the hourly wage leads to much smaller output gains (with an implied elasticity of 0.30). Field experiments that test the effect of wage increases sometimes find positive but insignificant effects (Gneezy and List 2006; Kube, Maréchal, and Puppe 2006, 2008), yet this may also be due to the small number of observations and the large inter-individual performance differences

that are typically found in these studies. However, studies with a larger number of observations (Cohn, Fehr, and Goette 2007) find significant effects. Furthermore, studies that implement gift exchange by providing direct gifts to the workers show surprisingly large positive effects with implied wage elasticities of 1.3 to 1.5 percent.¹⁰ This large effect contrasts sharply with the much lower wage elasticity associated with “simple” wage increases, which are typically between 0.2 and 0.4 percent. A plausible interpretation of these differences is that merely increasing the wage does not automatically trigger an increase in fairness perceptions, while a specific gift is unambiguously associated with the perception of fair treatment. If this interpretation is true it may be possible to magnify the effect of simple wage increases by embedding them in the right context; that is, by making the fairness increase associated with the wage increase more salient to the workers. The relatively high wage elasticity associated with fairness violations suggests that, even in short-term jobs, wage cuts that violate fairness norms may not be profitable. Likewise, the high wage elasticity of direct gift giving suggests that it may be profitable for employers to stimulate the motivation of workers with such gifts, even in one-shot interactions. However, if the low elasticity of simple wage increases should turn out to be the rule rather than the exception, and even if such wage increases will turn out to be generally unprofitable in one-shot interactions, workers’ fairness concerns may nevertheless exert a powerful impact on effort provision and wage setting.¹¹ The reason is that repeated interactions are a potentially powerful multiplier of the effect of fairness concerns. Whether this conjecture has empirical substance is the topic of the next section.

Gift Exchange in Repeated Interactions

In the discussion of gift exchange in one-shot interactions in the labor market between firms and workers, we have shown that higher wages have a significantly positive effect on workers’ effort, but we have also emphasized that the positive impact of gift exchange on aggregated market performance is rather limited. However, all the results described so far have abstracted from an important aspect of the labor market: employment relationships are hardly ever spot-market transactions where anonymous trading partners interact only once. Rather, employers and workers have

the option to interact repeatedly with each other. In what follows, we summarize studies which include the long-term nature of relations in labor markets and investigate the interaction of reciprocity and repeated game effects.

Evidence from the Lab

It can be shown that the presence of a fraction of workers inclined towards fairness allows for the existence of a reputational equilibrium in which not only the fair-minded workers but also the selfish ones are motivated to provide nonminimal effort in many periods of the experimental game. The formal argument for why such an equilibrium can be sustained in a finitely repeated game is related to the result of Kreps et al. (1982). The presence of a fraction of fair-minded workers implies that the firm is willing to pay a worker wages above the reservation level, even in the last period of the interaction—provided there is a sufficiently high belief that the worker is fair-minded. Even though it is certain that all of the selfish types will shirk in the final period, the fair-minded workers will still exert high effort. Therefore, if the probability that the worker is fair-minded is sufficiently high, it pays for the firm to offer high wages to all workers, even in the last period. The prospect of receiving future rents gives selfish workers an incentive to hide their true type from the firm, instead behaving like a fair-minded worker and exerting more effort when offered a high wage. As long as the firm does not detect that a worker is selfish, it will offer him a high wage during every period of the employment relationship, including the last one. By contrast, once a worker reveals that he is selfish, the firm will no longer be willing to pay him more than the reservation wage for exerting minimal effort. This possibility provides selfish workers with a strong incentive to establish the same record, or reputation, as a fair-minded worker.

The first paper that investigates the effect of repeated interactions in a gift exchange setup is Gächter and Falk (2002). They set up a laboratory experiment with two treatments. The baseline treatment involved a sequence of 10 one-shot interactions with a matching scheme that ensured that a particular pair of subjects interacted only once. In the main treatment, each pair of subjects was informed that they would play a 10-times repeated version of this gift exchange game. Thus, in this sec-

ond treatment, each pair of subjects has a common history, and both participants can always condition their actions on their past experience with their partner. If, for example, employers only offer attractive contracts to workers who have always provided high effort in response to high wage offers in the past, then selfish workers have a strong incentive to hide their type and imitate the behavior of fair-minded workers. By providing high effort in response to high wage offers, selfish workers can build up a reputation as fair-minded types. Due to the conditional offering strategy of employers, such a reputation can be of value, as it gives the workers access to profitable future offers from which they would be excluded if their true type were revealed.

The data from this study reveal that repeated game effects are important. In the treatment with repeated interactions the wage-effort relationship is steeper than in the one-shot treatment. As a consequence, average effort levels and market efficiency are significantly higher in the repeated game. Effort levels, which can be chosen between 0.1 and 1, stabilize at about 0.55 and remain there until period nine. In the tenth and final period the effort level drops to approximately the average effort level in the one-shot treatment (0.41). A detailed analysis of individual behavior confirms that this development over time is roughly in line with the reputational explanation put forward above. In both treatments there is a fraction of subjects who are genuinely motivated by fairness concerns. Hence, the repeated game incentives leave intact the subjects' fairness motivation. However, in the repeated game treatment there are also selfish subjects who imitate fair behavior. Thus, the repeated game nature of the treatment disciplines many selfish individuals who would—in the absence of repeated interactions—play uncooperatively. These findings illustrate a fundamentally important point. Although gift exchange alone has only a limited impact on market efficiency, these effects may become larger once firms and workers interact repeatedly with each other.¹²

Brown, Falk, and Fehr (2004) allow long-term employment relationships to arise endogenously in a competitive market environment. In this experiment employers have the opportunity to direct their wage offers to specific workers. They can therefore build up a long-term relationship with a worker by renewing offers to the same worker in consecutive periods. Comparing this treatment to one in which conscious repeated inter-

actions are excluded measures whether the market's participants succeed in endogenously establishing long-term relationships, which serve as an effective effort-enforcement device.

The results of this paper show the importance of endogenous reputation formation in labor markets. Figure 1 displays the distribution of effort choices and average effort over time for both the one-shot condition (OS) and the reputation condition (REP). In the one-shot condition where reputation formation is not possible, the modal effort choice is the minimum level. In contrast, when reputation formation is possible the maximum effort level is most often chosen. Overall, average effort increases from 3.3 in the treatment with one-shot interactions to 6.9 in the treatment with endogenous formation of relationships. The reason for this difference is that in the treatment with fixed identities, many employers succeed in establishing efficient long-term relationships with workers. Employers are mainly interested in interacting with fair-minded workers because these workers are willing to reciprocate high wage offers by providing high labor efforts. Accordingly, most employers are only willing to renew their contract with a worker as long as there is no indication that the worker is selfish. This implies that employers strictly condition the continuation of an attractive position for a worker based on his current effort choice. Since receiving high wage offers generates rents for selfish workers, they are motivated to hide their true type and imitate the fair-minded workers' behavior. In contrast to the situation in one-shot interactions, high wage offers in the relationship condition not only motivate fair-minded workers to provide high effort but also motivate the selfish ones who imitate them. At the end of the experiment, however, the reputation for being fair-minded is no longer valuable for selfish workers, and therefore they no longer hide their type. This leads to a significant drop in performance in the last period.¹³

This study reinforces and extends the findings of Gächter and Falk (2002). In finitely repeated relationships, the presence of a fraction of fair-minded agents who only have a limited impact on performance in one-shot interactions is enough to trigger a strong increase in market performance. The reason is that reputational incentives can motivate selfish agents to imitate the behavior of fair-minded workers. Reputational effects are considerably stronger in the second study, most likely because

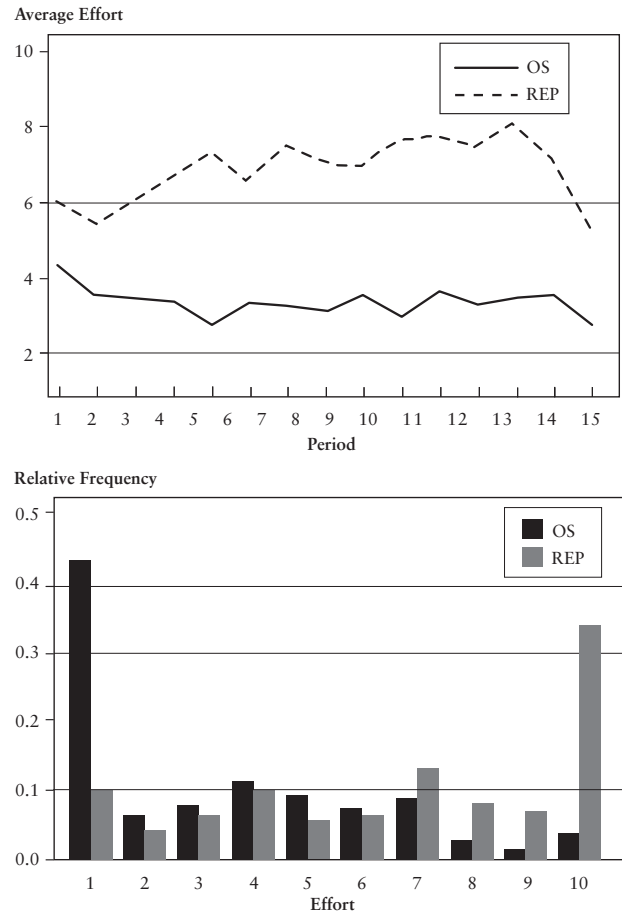


Figure 1
Average Effort and Distribution With and Without Reputation Formation

Source: Authors' calculations based on Brown, Falk, and Fehr (2004).

Notes: The figure displays data from a gift exchange experiment by Brown, Falk, and Fehr (2004). In the one-shot condition (OS) employers cannot distinguish between selfish and fair-minded workers and therefore repeated game effects cannot play a role. In the reputation condition (REP) employers can identify workers such that the endogenous formation of long-term relationships is possible. The top panel of the figure depicts the development of the average effort level over time. The figure's bottom panel shows the distribution of the workers' effort choices over all periods.

of the endogeneity of the long-term relationships. In Gächter and Falk (2002) the participants are forced to interact with the same partner over 10 periods. Thus, while employers can make their offers less attractive within the relationship, they do not have the possibility of terminating the relationship. In contrast, relationships are voluntarily formed in the market of Brown, Falk, and Fehr (2004). Given that there is an excess supply of labor, terminating the employment relationship is a credible threat.

Evidence from the Field

Field evidence on the role of gift exchange in repeated interactions is more indirect and circumstantial; this is because of the absence of controlled long-term experiments explicitly manipulating wages in a way that creates repeated game incentives.¹⁴ Needless to say, conducting such an experiment would be extremely costly. Perhaps closest to this aim, Bellemare and Shearer (2009) conducted an experiment at a landscaping firm. They implemented a one-time wage gift and examined the workers' response: effort increased ($p < 0.01$). Yet while this experiment taps into a repeated interaction, the treatment is not explicitly geared towards testing how the repeated aspect of the employment relationship affects workers' behavior. The setup is also not ideal, as the landscapers are paid a piece rate; thus increasing their effort also increases the workers' pay.¹⁵

In the following section we survey evidence from instances in which firms changed the conditions in an ongoing employment relationship. In each case, it is clear that the change negatively affected the workers' fairness perceptions. It is interesting to study these episodes because they can be interpreted as a permanent change in the firm's policy towards its workers and hence the workers' response to this policy change.

There are several striking examples illustrating the potential costs of treating workers in a way that they view as unfair. Krueger and Mas (2004) examine the quality of Bridgestone/Firestone tires manufactured in different plants and years. The particular plant of interest is the one in Decatur, Illinois, which experienced serious labor strife over an extended period of time. The conflict started when the company announced that at all its plants, new hires would be paid less than incumbent employees and that the shift rotations would be altered to a schedule that the work-

ers generally opposed. This announcement triggered a conflict between management and workers at all Bridgeston/Firestone plants. At the Decatur plant, management was particularly aggressive and threatened to hire, and later did hire, replacement workers. This move was seen by the workers as particularly unfair, as it breached the common long-term understanding between management and the workers. Tire manufacturing is still very labor-intensive and depends on high work effort, as the product quality is of utmost importance for the longevity and safety of a tire. The results show clearly that tires manufactured during the Decatur labor strife were of significantly lower quality compared to the same type of tires manufactured at different plants in the same period.¹⁶ A more detailed analysis reveals that an important quality differential was generated immediately after the announcement, even before any of the new policies were put into place. This pattern is particularly supportive of a behavioral model in which workers care about being treated fairly, as the mere intention to act in a way that workers consider unfair triggered the negative response. The data also show that the quality of the tires produced was lowest when many of the union workers had to interact with the nonunionized replacement hires. Thus, it appears that the union workers were the least motivated when they were working side-by-side with employees who accepted the new working conditions. Again, this evidence is supportive of the view that fairness considerations played a key role in understanding the precipitous drop in tire quality at the Decatur plant.

In a similar vein, there is evidence showing that a labor dispute at Caterpillar, a large manufacturer producing construction equipment, tractors, and other vehicles, had a similarly negative impact on production quality (Mas 2007). Negotiations between the union and management broke down after Caterpillar refused to accept a contract that the same union had closed with John Deere, a firm similar to Caterpillar. This move by management was viewed as an attempt to strong-arm the workers into a worse contract and take away rents to which the workers felt entitled. Much as with tire production, a significant share of work on construction equipment is manual, and requires care and effort to produce a high quality product. Mas (2007) shows that, relative to comparable Caterpillar equipment produced outside the United States, the equipment produced in

the United States during the labor strife shows a lower resale value. Mas argues that work effort is an important determinant of quality, and his interpretation is that work effort was lower during the labor strife. Here, again, a conflict erupted between the firm and its workers, and negative consequences followed after Caterpillar announced it would not agree to the new contract. Like the earlier example, this case is consistent with a model in which employees work less hard if they feel treated unfairly.

No studies exist examining how the repeated nature of an employment relationship affects the effectiveness of gift exchange in labor markets. However, in a related field, Maréchal and Thöni (2007) conducted an experiment that allowed them to tap into a similar business context. They conducted an experiment in which sales representatives visited stores to sell pharmaceutical products. The treatment in the experiment consisted of a gift—six product samples—that the sales representative gave to the store manager at the start of the visit. Giving the gift strongly increased sales during the representative's visit, and the impact on sales is quantitatively quite large. Average sales per visit are approximately CHF 60 in the baseline condition, while sales in the gift condition are CHF 270. Given that the pharmaceutical firm was willing to visit the stores to realize a sale of CHF 60, giving a gift in this context is highly profitable. Interestingly, the effect is only present if the sales representative had visited the store before. Gifts on initial visits led to no change in sales. This suggests that the gift is tapping into an ongoing relationship between the two agents. As predicted by the theory and by the evidence from lab experiments, this repeated interaction is where the effects of fairness should be largest.

There is also evidence that actions by employers that are considered unfair trigger stronger responses than actions that are considered fair. Mas (2006) examined the outcomes of final-offer arbitration cases that involved a New Jersey police department, in which the union of police officers and the city were unable to negotiate a new contract.¹⁷ In this case, it is rather clear which of the outcomes the workers found most fair as they would not end up in final-offer arbitration if they did not disagree with the offer the employer made. Mas (2006) documented a large and significant decline in many indicators of police performance subsequent to a loss experienced by the police department: the number of crimes cleared decreased significantly, as well as the probability of incarceration

and the sentence length of crimes prosecuted. This is suggestive of an overall decline in police effort in many domains. Mas (2006) calculated the size of a gain or loss relative to the expected outcome of arbitration and plotted the change in effort against this variable. The results are displayed in figure 2. As can be seen, there is a marked drop in clearances if the police lost in final-offer arbitration. It is telling that there is a discrete drop in effort if the police lost, no matter how small the loss is. Further, the figure shows that the decline in effort is highly sensitive to the size of the loss, but not nearly as sensitive to the size of the gain that the police get if the arbitrator rules in their favor. These results are consistent with reference-dependent preferences as discussed earlier.

There are two particular caveats regarding this study's results. The first is the potential in this particular case to underestimate the effect of a gain on effort. It is attractive to study the outcome of arbitration cases because this creates credibly random variation in the terms of the police department's contract. However, most police departments are able to settle on an agreement with their city, and do not end up in final-offer arbitration. It is therefore possible that the only cases that ended up in the sample were those cases in which the police felt strongly entitled to their demands. It is not surprising that in these cases there is only a small positive effect on effort, since the police felt that they simply got what they were entitled to and did not experience a gain from this outcome. The second potential issue is that what gets measured is the consequences of implementing the new contract, not the announcement's immediate effects. In the studies we discussed earlier, many of the negative effects had already materialized when the announcement was made. Such effects are present in the group that ultimately lost and the group that ultimately won the arbitration process and thus are differenced out. Since these effects can be large, the study potentially underestimates the effort reductions caused by treating workers unfairly.

To summarize, this evidence shows that if firms treat workers in ways that are perceived as unfair, this treatment may entail very high costs to the firm. Less is known, however, about the impact of treating workers in a way that is clearly perceived to be fair. While the results in Maréchal and Thöni (2007) are suggestive of positive effects accruing from fair treatment, this remains to be documented in a labor-market setting. In

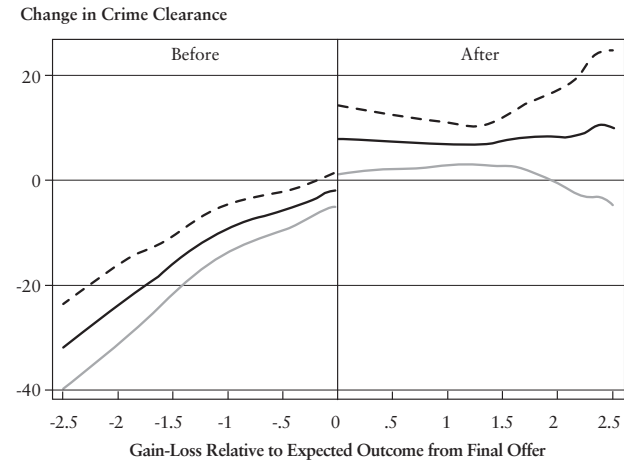


Figure 2
The Relationship Between Gains and Losses in Wage Bargaining and Effort

Source: Mas (2006).

Note: The horizontal axis measures the gain-loss relative to the expected outcome from final-offer arbitration. The expected outcome is constructed using the predicted probabilities of winning and the two offers from the police union and the city, respectively. The vertical axis shows the change in the number of crime clearances before and after the announcement of the arbitration outcome. The relationship is estimated using local-linear regressions.

particular, it is not clear what role the form of the gift exchange plays: the evidence from one-shot experiments suggests that nonpecuniary gifts work better than simply paying a higher wage. If it is also true that gift exchange in repeated interactions works better when a nonpecuniary gift is used, this may provide a potential explanation for why firms invest so heavily in offering nonpecuniary job benefits. However, more research is needed on this issue. In particular, long-term studies with explicit randomization or credibly exogenous changes in compensation policies, such as adopting a set of policies when a firm is bought by another firm, are needed.

Internal Labor Markets

In a pioneering book, Doeringer and Piore (1971) assert that there is a sharp distinction between internal and external labor market arrangements. In particular, workers seem insulated from outside labor-market conditions once they are employed in firms. They argue that these arrangements are difficult to explain from the viewpoint of a neoclassical model: “[W]e doubt that any of the major strands of conventional research will prove capable of assimilating the internal labor market into conventional theory in a useful and meaningful way” (xx). As we argue below, fairness preferences have interesting new implications for how firms set wages over time, giving rise to two of the most important features of internal labor markets.

The evidence on fairness perceptions suggests a shift in what workers feel entitled to as they enter an employment relationship with a firm (Kahneman, Knetsch, and Thaler 1986). While workers who join a firm compared the offer they received from the firm to what they could have gotten otherwise in the labor market when forming their fairness judgments, the evidence strongly suggests that incumbent workers compare any proposed change in the employment relationship to the status quo in order to assess the offer’s fairness.¹⁸ A second important regularity is that there appears to be a strong effect of loss aversion on fairness judgments. For example, a small decrease in the wage does much more damage to fairness judgments than a small increase in the wage does to boost fairness perceptions (Kahneman, Knetsch, and Thaler 1986). It is not clear, a priori, whether loss aversion in fairness judgments applies to the nominal or to the real wage. The survey scenarios in Kahneman, Knetsch, and Thaler (1986) hold the real wage cut constant, showing that over and above the loss in the real wage, individuals consider nominal wage cuts particularly unfair. Shafir, Diamond, and Tversky (1997) also show that nominal wage cuts are perceived as genuinely more unfair. Goette and Huffman (2007b) present evidence that it is the salience of a nominal wage cut—which triggers a strong affective reaction—that informs the fairness judgment. They show that holding the real wage change constant, it is just the wage cuts, not the size of a nominal wage change per se, that influences the affective reaction, a result which is consistent with this interpretation regarding loss aversion.

These features give rise to three specific predictions in the theoretical framework we discussed earlier.¹⁹ The first prediction the model makes is that entry-level wages and the wages of incumbent workers respond differently to changes in labor-market conditions. Entry-level wages should strongly depend on labor-market conditions. If the labor market is tight, workers can find alternative employment at relatively high wages. Thus, a high wage is needed to elicit high effort. When unemployment is high, workers’ outside offers will be worse, and they will be willing to exert high effort for a lower wage. As a consequence, the firm’s optimal entry-level wage is lower when there is slack in the labor market. Conversely, for incumbent workers the reference outcome is the contract that was in place the last period, not the workers’ outside options. This in itself makes the wages of incumbent workers independent of labor-market conditions. The model also predicts cohort effects in wages: because last year’s contract becomes the reference outcome for this year, keeping the same contract is viewed as fair. Thus, if a worker started out with a high entry-level wage, this wage will become the reference wage for the next period, influencing future wage outcomes. The third prediction is related to loss aversion: if workers’ fairness judgments are more strongly affected when they are made worse off, then firms should be reluctant to cut wages. The fairness model is silent as to whether real or nominal wages are the relevant measuring stick for fairness judgments. However the evidence in Kahneman, Knetsch, and Thaler (1986) suggests that it is nominal wage cuts which are considered particularly unfair.

The evidence is generally supportive of the model’s predictions. Several recent studies document that job changers’ wages are more cyclical than job stayers’ wages, including Devereaux (2001); Devereux and Hart (2006); Haefke, Sonntag, and van Rens (2006); and Solon, Barsky, and Parker (1994). In all studies, the wages of individuals entering firms are far more sensitive to business cycle variations. It should be noted that the fairness model does not predict that the incumbents’ wages will never change. In particular, if the firm’s profits rise, so should the incumbents’ wages. Since the studies do not attempt to disentangle shocks that affect the profits of firms (for example, productivity shocks) from other shocks (shocks that only change labor supply), there is no detailed test of this prediction. The study closest to testing this prediction is Beaudry and

DiNardo (1991), who find that current labor-market conditions have almost no effect on current wages, but initial labor-market conditions are a significant determinant of wages. Support also comes from several case studies of firms' personnel files (Baker, Gibbs, and Holmstrom 1994; Eberth, 2003; Treble et al. 2001). Such studies, while less representative, show a much clearer picture of how wages change over the course of a career in ways that are difficult to assess using data from labor force surveys. The evidence of cohort effects is also cleanest in these studies: the picture that emerges is that entry-level wages vary widely between years. Each cohort then gradually increases from the entry-level wage, thus preserving the initial differences in wages.

There is also strong evidence that employers shy away from enacting wage cuts and freeze their employees' wages instead of implementing small wage cuts. Figure 3, using data from Fehr and Goette (2005), shows the distribution of nominal wage changes from two large companies in Switzerland. There are two noteworthy features in the distribution. First, there is a clear drop in the density just around zero. A large fraction of individuals receive a nominal wage change of zero, but almost nobody receives wage cuts. Second, small wage increases are frequent. Hence, there is a clear asymmetry in the distribution of wage changes: wage cuts occur less often than expected, as predicted by the model. The distributions shown here are representative of wage change distributions obtained from personnel files (for example, Altonji and Devereux 2000; Wilson 1999). There are significant measurement problems when moving to more conventional datasets like the Panel Study of Income Dynamics or other labor market surveys. The problem is that wages are typically reported with error (Bound et al. 1994). This problem is accentuated when looking at wage changes and may wrongly lead researchers to conclude that there is a substantial amount of wage flexibility. Indeed, studies that do not control for measurement error find a significant number of wage cuts, though these studies still find a strong asymmetry in the distribution of wage changes (Card and Hyslop 1996; Kahn 1997; McLaughlin 1994). Several methods have been proposed to correct for this problem: some rely on parametric modeling of measurement error (Altonji and Devereux 2000; Fehr and Goette 2005), while others are entirely non-

parametric (Gottschalk 2005). It turns out that the specific form of the correction has very little impact. All studies find, however, that correcting for measurement error is important: once these estimators are applied, the evidence one obtains from the labor force surveys essentially looks like the evidence from personnel files: there are only very few wage cuts.

Alternative models of nominal wage rigidity have been proposed. Malcomson (1997) surveys models of wage bargaining that imply that wages are constant as long as both the firm's and the worker's participation constraint is satisfied. Wages are adjusted only when one of the conditions becomes binding. With positive inflation, this automatically implies that wage cuts will tend to be rare. However, the model also makes the prediction that during deflation, wage cuts would be frequent, and raises rare, with the asymmetry going the other way. However, there is no evidence that wage cuts were frequent in the United States during the Great Depression (Akerlof, Dickens, and Perry 1996).²⁰

At the more aggregate level, our framework may also help explain some of the business cycle facts for which the standard model has difficulty accounting. First, the model offers a new source of wage stickiness. For example, the model readily makes the prediction that employment should be more volatile than wages. The reason is that in the face of a positive demand shock, rising employment lowers a firm's average profit (because of diminishing returns to effort). This leads the employees to work harder for a given wage, because their wages are now higher relative to the average profit the firm makes per worker. This increases the workers' effort, but does not require that the firm pay a much higher wage. Therefore, most of the firm's adjustment will come through changes in employment, making the wage relatively unresponsive to changes in demand on the product market (see Danthine and Kurmann 2004). On the other hand, the model also predicts a difference between demand shocks and productivity shocks for wage and employment reactions. In contrast to the demand shock discussed above, a positive productivity shock increases the firm's profit directly. Thus, the workers will lower their effort for a given wage. However, because the workers' effort now becomes more valuable to firms, this reinforces the firm's incentives to raise wages (Benjamin, 2005; Danthine and Kurmann 2004).

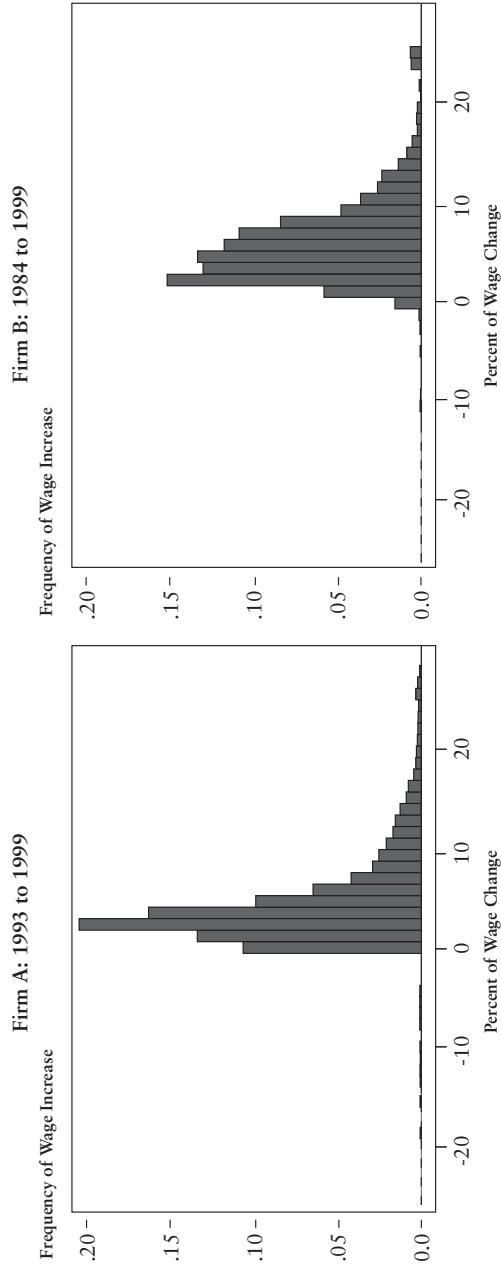


Figure 3

The Frequency of Wage Changes for Two Firms

Source: Febr and Goette (2005).

Note: The horizontal axis shows the wage change category in percent. The vertical axis measures the frequency of a particular wage increase.

Income Targets and Loss Aversion

The previous subsection discussed the implications of social preferences and loss aversion in employment relationships characterized by largely noncontractible effort. This forces firms to pay high wages in order to elicit above-minimum effort from workers through the fairness mechanism described earlier. But loss aversion may have implications for the form of optimal contracts even when effort is contractible. The evidence we reviewed on reference-dependent preferences suggests an additional channel that can affect labor supply. Suppose individuals have an income target in mind, and that, as is suggested by the evidence in Heath, Larrick, and Wu (1999), this target inherits all the properties of a reference point. If a worker is paid on a piece rate and works hard enough to surpass the income target, this causes her marginal utility of income to drop discretely because the money now feels like a gain relative to the income target. Now suppose that the piece rate is raised. This makes it easier for the worker to surpass her income target. Hence, the marginal utility of income will, on average, be lower over the day. If this drop in the marginal utility is strong enough, even a purely temporary change in the piece rate can lead to lower effort. This prediction is in stark contrast to the predictions of the standard model in economics. While it is possible that permanent wage increases do not lead to more labor supply because of an income effect (that is, diminishing marginal utility of consumption), this is impossible if the wage increase is temporary, as it only has a negligible income effect in this case.

Armed with these two predictions, Camerer et al. (1997) examined the labor supply of New York City cab drivers as a function of the daily wage. Indeed, they found a very strong negative correlation between implicit hourly wages and hours worked: on “good days,” cab drivers work fewer hours, in line with the prediction from the income-targeting model. Statistically, the effect is highly significant and has been replicated using different samples (Farber 2005) and cab drivers in other countries (for example, Singapore, as studied in Chou 2002). Several possible problems have been raised with this finding. While some have been dealt with adequately (for example, on measurement error, see Camerer et al. 1997; Chou 2002), other problems have remained. One of the trickiest problems that the studies face is that there is no convincing instrument for wages, which has led critics to speculate that supply-side shocks

may drive the variation in wages (Farber 2005). In this case, it would not be surprising to see that cab drivers work shorter hours when wages are high, as it is precisely their desire to work short hours that may have caused the wages to be high.

Other studies have shown that the participation margin of labor supply, meaning the propensity to work at all on a given day, is higher when wages are high (Oettinger 1999). This alone, however, does not invalidate the income-targeting model. The income-targeting model also predicts higher participation when wages are high. The reason for this is straightforward. While the higher wage makes exerting effort on the shift less attractive because the worker finds it easier to surpass her income target, working an extra day has, overall, clearly become more attractive. Therefore, the participation margin of labor supply should increase when wages are high (see Köszegi and Rabin 2006 for a formal treatment of this problem).

Fehr and Goette (2007) conducted a field experiment with bicycle messengers. The messengers were paid a piece rate, and the experiment increased that piece rate by 25 percent during a four-week period. The data from the bike messenger firms allowed Fehr and Goette to examine the overall impact on labor supply, as well as on the participation margin of labor and effort per shift separately. Their results reconcile the earlier findings and provide support for the income-targeting model. They find that the bicycle messengers worked significantly more shifts while they were paid a higher wage. However, they also work less hard while on the shift. In further support of income targeting, Fehr and Goette find that only messengers showing evidence of loss aversion in a separate, unrelated choice experiment reduce effort while receiving the higher wage.

While the previous studies considered changes in the wage or piece rate, Fehr, Goette, and Lienhard (2007) go a different route: they hire temporary workers to enter data into an information system. They manipulate the workers' productivity by slowing down the functioning of the computer interface the workers are using. This causes workers to earn less money than if the slowdown had not occurred. The income-targeting model predicts in this case that employees will work harder because they are farther behind their income target. Indeed, the workers'

effort increases as a function of how long they were delayed. The increase in effort only occurs in the treatment where workers are paid according to the quantity of data entered, not when they are paid fixed wages. This rules out that the delay may have changed the marginal cost of effort. Again, Fehr, Goette, and Lienhard (2007) measure loss aversion in the workers using a simple risky-choice experiment. They find that only the loss-averse workers respond to the slowdown by increasing their effort. For workers behaving in a less loss-averse fashion in the risk experiment, there is no evidence that they work harder subsequently. This evidence is difficult to explain with the neoclassical model, but follows immediately from the income-targeting model.

The income-targeting model also makes the prediction that a windfall gain should change the incentives to work. If by luck a worker earns more than expected, this moves her closer to her target income. Initially, effort should increase, as the marginal utility of income is increasing when below the income target because of diminishing sensitivity. However, having surpassed the target, effort should decrease, as the marginal utility now drops discretely.²¹ The standard model, on the other hand, predicts no change in motivation after a windfall gain. Testing the two competing models is difficult because it requires data on effort choices over time. Goette and Huffman (2007a) use data from two bicycle messenger firms in San Francisco that allow them to measure effort over the workday. They use random variation in morning earnings to test whether this affects effort in the afternoon. Goette and Huffman find that a windfall in the morning significantly affects effort in the afternoon. Higher morning earnings lead bike messengers to work harder early in the afternoon, but to work less hard subsequently.

One of the important unanswered questions is why the workers may have a daily, as opposed to a weekly or monthly, income target. Theories are silent on the issue of the choice of the reference frame. A plausible interpretation in the case of the bike messengers and cab drivers is that the income target serves as a rough proxy for the amount of money they need to make per day in order to finance their consumption. In these two applications, the amount made per day is particularly salient to the workers. For example, in the case of the bicycle messengers, they are reminded of how much they have made so far every time they drop off a package

and the customer signs the receipt. This may make daily earnings salient and hence lead to a daily, as opposed to weekly, income target.

In different applications, different reference frames have been proposed. For example, Rizzo and Zeckhauser (2003) examine the labor supply choices of young self-employed doctors. They have data on their income and on what the doctors think is an adequate yearly income, which Rizzo and Zeckhauser argue is the doctors' reference income. They find that if a doctor is below his reference income, he will work more the following year in order, they argue, to close the gap to his reference income. In support of this view, they find that if a doctor is above his reference income, there is no significant change in his work effort over the next year. A plausible interpretation of this result is that the doctors' reference income is derived from their consumption level: they may have set their mind on a certain consumption plan and are willing to exert more effort in order to generate enough income to cling to their reference level of consumption.

This model makes a number of interesting new predictions that can be examined. For example, such a model may make individuals more responsive when piece rates are introduced in an environment in which they cannot perfectly control output. In such cases, large gains in productivity are typically observed (for example, Lazear 2000). Fehr and Lienhard (2007) report evidence from a quasi-experimental change in the compensation scheme involving the removal of a daily guaranteed minimum for one group of workers. The change induced the employees to work much harder, in particular the ones who stood to lose most from the change. The group of workers who initially responded the most readily then showed a gradual decline in effort, while the other group responded less initially and then showed an increase in effort. These results are consistent with a gradual change in reference consumption: the group most affected by the change initially increased labor supply to try to contain the reduction in consumption. This led to a gradual decline in consumption and in the reference point (see Bowman, Minehart, and Rabin 1999 for a fully fledged model). On the other hand, the group least affected gradually increased its effort because the new system generated higher income, thus ratcheting up their reference consumption, which in turn made it optimal for them to increase effort somewhat more.

3. Policy Implications

In this section, we discuss several policy implications that are influenced by the behavioral forces that we introduced and for which we examined the evidence in sections 1 and 2. These forces change the way the labor market responds to policy intervention, and we highlight two important areas for policy consideration.

The Importance of Wage Dynamics

As we argued, the evidence strongly suggests a specific pattern of how workers make fairness judgments in employment relationships. The predictions from the behavioral model for the firms' wage policies are largely confirmed by the data. We discuss two issues that are of clear importance to monetary policy. The first concerns the specific nature of downward nominal wage rigidity and its implications for short-term and long-term tradeoffs between inflation and unemployment. The second highlights a feature of the model that has only recently been studied: there is a new source of persistence that propagates macroeconomic shocks in the economy through the mechanism of internal labor markets.

Downward Wage Rigidity

The evidence we reviewed when discussing internal labor markets suggests that wages are downwardly rigid. As briefly mentioned above, it is not clear from the evidence on fairness perceptions whether the downward rigidity is in real or nominal wages.

This distinction is very important from a policy perspective, as few studies have assessed the extent of nominal and real wage rigidities. Two related studies, Dickens et al. (2006, 2007) develop a unified model to assess the extent of downward nominal and real wage rigidities (see also Goette, Sunde, and Bauer 2007, which offers a similar approach for Germany, Italy, and the United States). The basic idea behind this approach is to use the features of the wage-change distribution depicted in figure 3; that is, they try to use the drop in the density of wage changes just below nominal zero to assess the extent of downward nominal wage rigidities. Similarly, one can develop an estimator for real wage rigidities. In many countries, such as Britain in the 1980s, there is just as pronounced an

asymmetry around zero *real* wage changes. Such discontinuities near the expected inflation rate are used to estimate the extent of real wage rigidities. The advantage of the work by Dickens et al. (2006, 2007) is that it combines data sources from 13 different countries and uses the same method to estimate downward wage rigidity on all datasets. While there are several studies from different countries, it has been difficult to compare their results, as each study used a different method.

The results show strong evidence of real and nominal wage rigidities in virtually all countries. There are few correlations with institutional variables that predict the type of rigidity. The largest and most robust correlation is with union density: the higher the union density, the more real wage rigidity in an economy, and the less nominal wage rigidity. The United States has much stronger nominal wage rigidity than other countries, particularly those in the euro area.

Wage rigidities are often dismissed as irrelevant given that employment relationships are long run in nature. Therefore, the argument goes, for a given present value of the surplus from an employment relationship, firms can set many different wage paths, including some that have rigid wages, to accommodate the workers' fairness concerns. For example, firms may refrain from enacting a wage cut in one year, but instead not give the worker a wage increase in the next year (see Elsby 2009 for a formal model along these lines). In the context of the model that we discussed, this argument is clearly wrong. The reason is that all the evidence indicates that effort depends on the division of surplus in every period. In the example above, not giving the worker a wage raise in the future would lead the worker to exert less effort. Consequently, not cutting the wage in the current period *does* raise the costs to the firm because offsetting the higher wage in the next period entails costs in the form of lower effort.

Consequently, wage rigidities can have a strong impact on firms' costs and may therefore be effective on the real side of the economy. These rigidities are important for policy for at least two reasons. First, rigid real wages add persistence to monetary shocks in the workhorse model of modern macroeconomics, the New Keynesian framework. In the prototypical model, firms have sticky nominal prices, for example because

of the costs of adjusting prices (for a review, see Goodfriend and King 1997). When setting prices, they take into account the future development of wages. Because these wages determine the firms' marginal costs of production, and because the firms are stuck with the price they set now, the price they set takes into account the expected development of future wages (see Dotsey, King, and Wolman 1999). The evidence on wage rigidities discussed above implies that the real wage is not going to be very responsive to shocks. As a consequence, firms are going to change their price less in response to shocks. But the less they change prices upon the impact of a shock, the stronger and longer are its effects on the real side of the economy. Therefore, downward wage rigidities can contribute to making monetary shocks more persistent. Jeanne (1998) shows that the interaction between price setting and wage rigidities is actually more subtle than the argument given above, and that it takes only a little wage rigidity to make monetary shocks quite persistent, assuming standard degrees of price stickiness. Therefore, using the standard macroeconomic model as the relevant model for policy can be potentially costly, because monetary policy can have effects on the real side of the economy that may be much more persistent than the standard model would predict.²²

The above argument applies to both real and nominal wage rigidities because the channel through which these affect the real side of the economy results from making marginal costs less responsive in general. However, there is a second, and perhaps more important, channel through which downward *nominal* wage rigidity can affect the real economy. The reason why this may be more important is because downward nominal wage rigidity may affect the long-run unemployment rate, not just the response to shocks. This is because higher wages lead to higher prices charged by firms, depressing aggregate demand and, hence, in equilibrium, employment (Akerlof and Dickens 2007). Akerlof, Dickens, and Perry (1996) build a formal model incorporating such an effect. The empirical estimates of the extent of wage rigidities allow Dickens et al. (2006) to calculate by how much wages have been increased due to wage rigidities. They then estimate a cross-country Phillips curve implied by the model in Akerlof, Dickens, and Perry (1996) that incorporates the

effects of downward nominal wage rigidity. The basic estimate is shown in figure 4. Their results imply that downward wage rigidity substantially increases the long-run unemployment rate. This result is robust to including country-specific intercepts.²³ Further, their results show that nominal and real wage rigidities act in just the same way to increase unemployment. Monetary policy can thus potentially affect long-run output and employment through its impact on how strongly the constraint of downward nominal wage rigidity binds. In particular, tight monetary policy when real wage growth is low could lead to persistent increases in unemployment (also see Fehr and Goette 2005, who find a robust correlation between the impact of wage rigidity on wages and unemployment).

Therefore, the evidence indicates that there is a potential additional constraint on monetary policy: if the labor market is characterized by

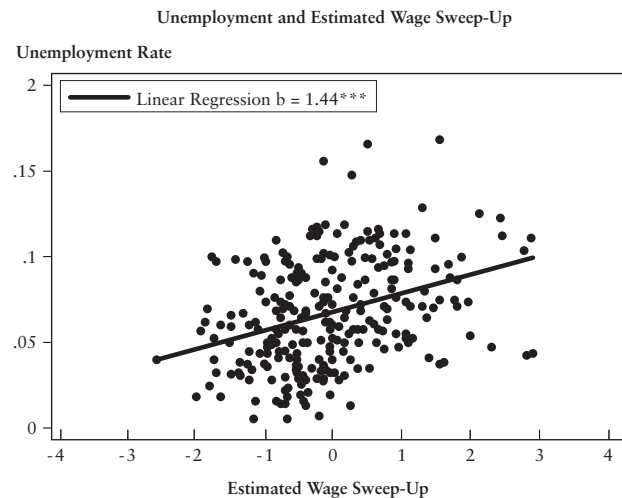


Figure 4: The Relation between Unemployment and Wage Rigidity
Source: Dickens et al. (2007).

Notes: Data is from 13 countries in the International Wage Flexibility Project. The figure shows the relationship between the wage sweep-up, meaning, the extent to which wage rigidities increased the level of wages in a particular year, and unemployment in the same year. Each point is a country-year observation.

strong downward nominal wage rigidity, then keeping inflation low when productivity growth is low may entail a significant employment cost. The evidence also suggests the impact that monetary policy can potentially have on unemployment depends on the structure of the particular labor market: in many countries, particularly in the euro area, there is less evidence of downward nominal wage rigidity than, for example, in the United States or in Switzerland. Rather, real wage rigidities seem to be important. While real wage rigidities still have an adverse effect on unemployment, their effect does not depend on the inflation rate.

Less is known about the shape of wage rigidities when inflation rates are virtually zero over a long period of time. In this case, nominal wage rigidities have been shown to be persistent (Fehr and Goette 2005). There is also some evidence from surveys of inflation expectations that individuals tend to ignore inflation once it becomes low enough (Akerlof, Dickens, and Perry 2000). Indeed, there is also evidence from wage rigidity studies that real wage rigidities tend to become weaker as inflation becomes very low (Bauer et al. 2007). This, in turn, may open the door for yet another channel through which monetary policy may affect the labor market: very low inflation rates may cause individuals to ignore inflation when setting wages, thus giving rise to downward nominal wage rigidity. However, more research is needed to understand how inflation expectations affect wages in a behavioral model of the business cycle.

The Consequences of Business Cycles

The evidence we reviewed in section 2 shows significant and long-lasting effects of labor-market conditions on individuals' wages. This raises the possibility that even short-run business cycle fluctuations have long-run consequences for the workers in labor market transitions.

This effect may be particularly pronounced for graduating students. Oyer (2008) examines the career choices of Stanford MBA graduates as a function of the stock market, which was highly volatile over the sample period he considered. He finds that MBAs are much more likely to choose employment at an investment bank if the Standard & Poor's 500 index is high than when it is relatively low. Plausibly, investment banks offer more lucrative jobs when business is strong, leading many graduates to take jobs at these firms. The evidence on internal labor

markets then predicts that these employees are able to keep the contract they negotiated—because the firm fears retribution in the form of lower effort if it reneges on the initial contract. Therefore, MBAs entering investment banks in a good year should be more likely to stay in investment banking. Oyer finds that the Standard & Poor’s 500 index has significant effects on job choices in the long run. The level of the Standard & Poor’s 500 in the year of graduation is a highly significant predictor for remaining an investment banker for at least five years.

As we discussed in the section on internal labor markets, entry-level wages are highly volatile over the business cycle. Therefore, business cycles could have long-lasting effects on individuals’ earnings and careers more generally. Oreopoulos, von Wachter, and Heisz (2006) use data on Canadian college graduates to examine the long-run effects of graduating during a recession. They find that the labor-market conditions upon graduation have very strong and long-lasting effects on economic outcomes later. If one graduates in a boom year (with an unemployment rate that is 5 percentage points or lower), initial earnings are about 9 percent higher. Figure 5 displays their results and shows that the effects of unemployment on earnings are long-lasting. After five years, long after the economy has slowed down again, earnings are still 4 percentage points higher and the effect only fades after ten years.²⁴ The reason why these effects are so long lasting is that initial business cycle conditions change the job-mobility pattern permanently, as one would expect when the firm’s compensation policy is permanently set by initial business cycle conditions.

In summary, the implication of the model that we set forth is that monetary policy may have more persistent effects on the real side of the economy than the standard model would predict. We have offered three channels through which the model outlined above can become relevant in policy considerations: it makes demand shocks more persistent because the behavioral forces discussed above make real wages unresponsive to current economic conditions. This raises the potential of a permanent tradeoff between inflation and employment because the model predicts significant costs to firms from cutting wages. Finally, the model also highlights a new channel through which business cycle fluctuations can be propagated and generate costs for workers over many years.

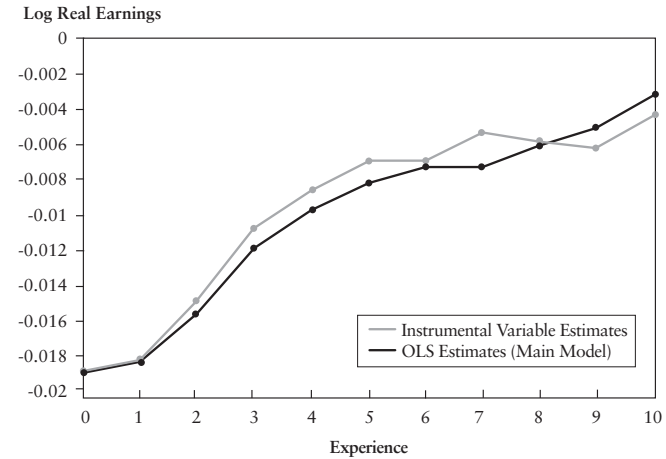


Figure 5: The Impact of Unemployment at Graduation on Log Real Earnings

Source: Oreopoulos, von Wachter, and Heisz (2006).

Notes: The horizontal axis indicates years since graduation. The vertical axis indicates the impact of a 1-percentage point increase in unemployment at graduation in log real earnings. The line labeled “IV estimator” uses freshman year + 4 as the instrument for the graduation year, instead of the actual graduation date in order to avoid endogeneity bias.

Fairness and the Economic Effects of Minimum Wage Laws

In this subsection we discuss another reason why the psychological forces described in section 2 may be crucial for a better understanding of labor market policy. If people have reference-dependent fairness preferences, policy measures may not only operate by affecting outcomes but also by shifting the relevant reference points. We use the case of minimum wage legislation to illustrate the empirical relevance of this possibility. The minimum wage example is an especially important one because minimum wages are one of the most often-used instruments in labor market policy; for instance, see OECD (1998) for evidence that most labor markets in the developed world are affected by minimum wage laws in one way or another.

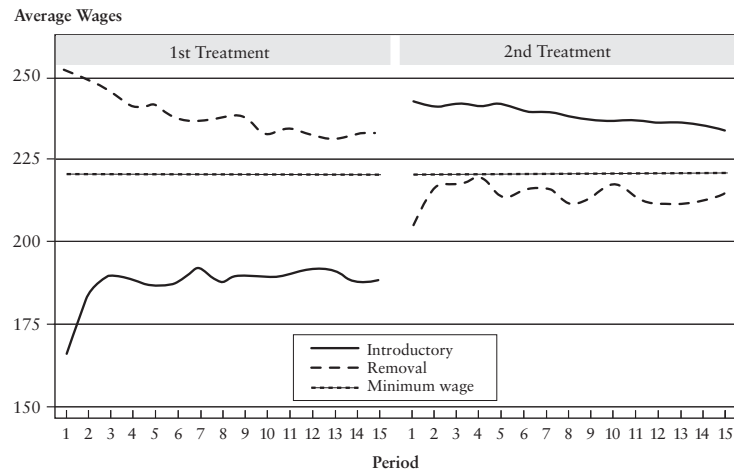


Figure 6
The Effect on Wages of Introducing and Removing a Minimum Wage Law
Source: Authors' calculations based on data from Falk, Fehr, and Zehnder (2006).

Notes: The figure shows the development of wages over time. Two treatment sequences are displayed. In the first treatment, the introductory sequence (depicted by the solid line) has the participants first experience a situation without a minimum wage, and then the second treatment introduces a minimum wage. In the removal sequence (depicted by the dashed line), the order of the treatments is reversed; the participants first interact in a situation with a minimum wage in the first treatment, and then the minimum wage is removed in the second treatment.

Since minimum wages are so widespread, it is not surprising that for decades economists have been interested in the economic and social consequences of minimum wages. However, despite the remarkable attention the topic has received, at least three frequently reported empirical findings remain puzzling in light of the standard approach in labor economics. First, a number of papers show that minimum wages have so-called spillover effects, meaning that many firms increase wages by an amount exceeding that necessary to comply with the higher minimum wage (see, for example, Card and Krueger 1995; Dolado, Felgueroso, and Jimeno 1997; Katz and Krueger 1992; Teulings 2003; Teulings, von Dieten, and

Vogels 1998). Second, several studies report anomalously low utilization of subminimum wages in situations where firms could actually pay workers less than the minimum wage (see, for example, Freeman, Gray, and Ichniowski 1981; Katz and Krueger 1991, 1992; Manning and Dickens 2002). For example, Katz and Krueger (1991) find that introducing the opportunity to pay subminimum wages to youth has not caused a significant decline in teenage workers' wages. Third, there are several cases in which an increase in minimum wages led to zero or even positive employment effects (see, for example, Card 1992; Card and Krueger 1994; Katz and Krueger 1992; Machin and Manning 1994; OECD 1998; Padilla, Bentolila, and Dolado 1996). This result is surprising because the conventional competitive theory predicts that increases in minimum wages should always reduce employment.

All these effects concern the two most important variables in the minimum wage discussion: wage payments to workers and aggregate employment. Thus, from a policy perspective, a deeper understanding of these puzzling effects of minimum wages would be very desirable.

A recent study by Falk, Fehr, and Zehnder (2006) suggests that the economic consequences of minimum wages can be better understood if the labor market is viewed from the same behavioral perspective that we describe in this paper. However, not only do Falk, Fehr, and Goette (2006) take into account that many workers have reference-dependent fairness preferences, but they also emphasize that labor markets are, in general, not perfectly competitive. This view is based on a recent line of research in labor economics stipulating that imperfect competition is probably the rule rather than the exception in labor markets (see, for instance, Boal and Ransom 1997; Manning 2003). The rationale behind this argument is that labor markets are typically characterized by important frictions (like moving costs, heterogeneous job preferences, or social ties) which prevent the elasticity of an individual firm's labor supply from being close to infinity. Therefore it seems reasonable to assume that firms have at least a certain degree of wage-setting power.

Falk, Fehr, and Zehnder (2006) implement a simple laboratory labor market in which workers' mobility restrictions in combination with heterogeneous fairness preferences give rise to upwardly sloping labor supply schedules at the firm level. They observe that the minimum wage strongly

affects reservation wages, suggesting that it influences what is perceived as a fair wage. After the introduction of the minimum wage there is a strong increase in the reservation wages of subjects playing the role of workers. While almost all reservation wages were clearly below the level of the minimum wage before its introduction, a substantial share of reservation wages are above that level after its introduction. The impact on reservation wages of introducing the minimum wage is in line with the evidence presented in section 2. The mini-ultimatum games of Falk, Fehr, and Fischbacher (2003) revealed that changes in the set of available but unchosen alternatives may have important consequences for the perceived fairness of a specific action. The introduction of a minimum wage takes a whole range of previously possible wage payments out of the strategy set of firms. As a consequence, many subjects seem to perceive a wage payment at the level of the minimum wage—which would have been considered as fair and quite generous before its introduction—as unfairly low after introducing the minimum wage.

The impact of the minimum wage on reservation wages has important implications for the wage-setting strategy of profit-maximizing firms: they are forced to pay wages above the minimum. Thus, the strong impact of the minimum wage on workers' reservation wages provides a possible explanation for the spillover effect empirically observed in field studies. Furthermore, the pattern of reservation wages also shapes the employment effects of the minimum wage. Since firms face upwardly sloping labor supply schedules, they can increase employment if they pay higher wages. However, since the minimum wage not only increases wages but also reservation wages, there is no guarantee that workers will be willing to accept these higher wages. Accordingly, the minimum wage can increase or reduce employment, depending on the relative size of the two counteracting effects. Under the parameters chosen in Falk, Fehr, and Zehnder (2006) the minimum wage has a positive net effect on employment. However, the effect is much smaller than it would have been had workers' reservation wages remained stable.

In contrast to the experimental settings discussed in the previous section, Falk, Fehr, and Zehnder (2006) implemented a labor market with complete employment contracts. However, gift exchange experiments by Brandts and Charness (2004) and Owens and Kagel (2009) show that the

impact of minimum wages on labor supply also prevails if the labor market suffers from contractual incompleteness. Both papers show that the introduction of a minimum wage has two effects. On the one hand, the minimum wage increases average wages, which motivates fair-minded workers to exert more effort. On the other hand, however, the minimum wage also changes the fair-minded workers' willingness to provide effort at a given wage level. It seems that with a law in place that forces employers to pay at least a certain minimum, the same wage is perceived as less fair by the workers than before. As a consequence, the net effect of the minimum wage on effort is ambiguous and depends on the relative size of the two counteracting effects.²⁵

In addition, Falk, Fehr, and Zehnder (2006) find that the economic consequences of removing the minimum wage are very asymmetric relative to the effects of its introduction. While workers' reservation wages decrease somewhat after the removal of the minimum wage, they still substantially exceed those before its introduction. It seems that the minimum wage leads to a kind of ratchet effect in workers' perceptions of what constitutes a fair wage. Workers who are used to receiving high wages seem to feel morally entitled to receive them even after minimum wage legislation is abolished. Therefore, the payment of substantially higher wages after the removal of the minimum wage than before its introduction is a profit-maximizing strategy. The asymmetric effect of the minimum wage on reservation wages may explain why firms may find it unprofitable to utilize subminimum wage opportunities—because these opportunities have typically been introduced after a previous increase in the minimum wage.²⁶

Of course, laboratory experiments alone will never provide conclusive evidence. However, as the literature on the gift exchange effect shows, effects that have been found in the laboratory may well generalize to field settings outside the laboratory. Thus, if the asymmetric impact of minimum wage laws on reservation wages turns out to be a robust finding, it will have profound consequences. First, it calls into question the basic assumption that labor supply is not affected by the minimum wage. Second, the upward shift in the labor supply curve that is generated by increases in the minimum wage introduces a further potentially employment-limiting aspect of minimum wage increases. Third, the asymmet-

ric impact on reservation wages calls into question the symmetry of the comparative static effects of policy changes. If economic policies generate entitlement effects that respond asymmetrically to the introduction and the removal of the policy, much of what is taught in economic textbooks needs to be rewritten because the introduction of a policy may have effects that prevail even after it is abolished. In the labor market context this means that reductions in the minimum wage are likely to cause much smaller employment effects than one would expect from standard competitive or monopsonistic models.

4. Concluding Remarks

In this study we provide a behavioral view of the labor market. Contrary to standard economic models, our approach accounts for the fact that many employment contracts are incomplete and relational in nature. In addition, we also consider that a substantial fraction of people exhibits reference-dependent fairness preferences shaped by nominal loss aversion. We argue that combining these elements helps us to provide a better understanding of several empirical phenomena which are otherwise considered as puzzles. For example, our approach offers straightforward explanations for well-documented regularities such as downward nominal wage rigidity, the unresponsiveness of incumbents' wages to labor-market conditions, cohort effects, or noncompetitive wage premia. All these phenomena are hard to reconcile with the standard economic model but their explanations arise naturally in our framework.

We also argue that insights from the behavioral economics of the labor market have important policy implications. In many cases, our analysis implies that more responsibility, and power, lies in the hands of policymakers than the standard economic model suggests.

For instance, the behavioral approach to the labor market suggests that downward nominal wage rigidity arises because firms shy away from imposing small nominal wage cuts in response to negative productivity shocks when inflation is low. The reason is that many employees seem to perceive reductions in their nominal pay as very unfair and respond with behavior that is detrimental to the firm. This result implies that monetary

policy can have a permanent effect on the real side of the economy, a finding that contrasts strongly with what a macro model with strictly selfish individuals would predict. Furthermore, downwardly rigid nominal wages also make marginal costs less cyclical and help propagate monetary policy shocks across periods. Thus, the behavioral model provides a simple justification for why marginal costs are not very cyclical, while the standard economic model with selfish preferences must resort to unrealistic assumptions about the elasticity of labor supply or risk-sharing contracts. In addition, the mechanisms of internal labor markets can also propagate shocks across many periods, as has been demonstrated empirically. This channel, too, adds persistence to monetary policy shocks.

In certain cases the behavioral view of the labor market also suggests that policy changes can be hard to reverse. For example, an increase in the minimum wage cannot simply be revoked by subsequently lowering the minimum wage. The reason is that the higher minimum wage leads to a change in what people perceive of as a fair wage. As a consequence, many workers would feel morally entitled to receive a higher wage even if the policy were reversed and therefore would no longer be willing to work for the same wage as before the minimum wage increase. Thus, policymakers need to take into account that today's policies may have important spillover effects on future policy measures.

Overall, we believe that the behavioral approach to the labor market has generated useful insights, and some of these findings have already produced specific policy recommendations—in particular for the cases in which the behavioral model makes qualitatively different predictions than the standard model. It is difficult to quantify many other effects that are important for policy; for example, how internal labor markets affect the persistence of monetary policy. However, as with other recent advances in economics that have policy implications, it takes time to develop models that are specific enough to allow estimation and calibration, but the first steps have already been taken. For instance, Akerlof, Dickens, and Perry (1996) or Danthine and Kurman (2004) offer specific calibrations for policy, and others will follow.

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Notes

1. See Fehr and Tyran (2001); Fehr and Tyran (2008); and Tyran (2007).
2. Most individuals behave in a risk-averse fashion for risks even smaller than in this example. See Holt and Laury (2002) for a recent study.
3. See Rabin (2000) or Fehr and Goette (2007) for details.
4. See Kőszegi and Heidhues (2005) for a formal model of how loss aversion in consumers impacts pricing decisions by firms. See also Rotemberg (this volume) on the implications for price stickiness at the macro level.
5. This does not imply that, in these settings, individuals always behave as the standard model predicts. We return to an important departure from the predictions of the standard model in subsection 3, “Income Targets and Loss Aversion.”
6. Notice that in order for a reputational equilibrium to be sustainable, it is essential that there are fair-minded individuals in the population. So far, we have implicitly assumed that the population fraction of fair-minded individuals is large enough to render a certain degree of gift exchange profitable in one-shot interactions. However, for a reputational equilibrium to be sustainable this assumption is not required. If the fraction of fair-minded individuals is so small that firms would optimally refrain from gift exchange in the one-shot condition, there are reputational equilibria in which some selfish workers start to shirk in the game’s later periods. Since shirking reveals those workers to be the selfish type, they no longer receive wage rents. As a consequence of some selfish workers dropping out, the fraction of fair-minded workers within the group of workers exerting effort increases such that offering wage rents to these workers remains profitable even in the late periods of the game [for details on such equilibria in a related framework (trust game) see, for instance, Camerer and Weigelt (1988) or Brown and Zehnder (2007)].
7. See Benjamin (2005) for a formal model. The following results apply for what he calls perfectly fair workers.
8. Al-Ubaydli et al. (2008) argue that their results should be considered as evidence of gift exchange in the context of repeated interactions because their script told the subjects that it was possible that they may be rehired. However, the fraction of no-shows in the entire study was very high (about 20–30 percent), showing that the workers at the temporary employment agency did not seem to care much about their reputation. This leads us to conclude that incentives arising from repeated interactions are probably not a strong force in this study.
9. One could argue the gift of the bottle causes a positive mood among workers, and that it is because of this mood effect that individuals work harder. However, research shows that positive affective states are not associated with higher productivity (see, for example, Wright and Staw 1999).
10. The elasticity is defined as the percent change in effort in response to a 1-percent wage increase.
11. In order to be profitable in a one-shot setting, the elasticity of output with respect to wages has to be at least 1.0. A complicating feature in all the studies

reported here is that there is no comparable rule by which the wage differences between the treatments are set up. In some studies, researchers may err on the side of caution and implement large wage differences to be sure to find a difference between treatments, whereas the same behavioral response may be obtained with smaller wage increases. Thus, it is difficult to interpret the differences in elasticities between studies. It would be useful to examine the effect of different-sized wage increases within the same framework to get a better sense of what is the profit-maximizing wage.

12. There are other experiments that confirm the role of reputation as an enforcement device. Camerer and Weigelt (1988) study reputation formation in a lending game and Jung, Kagel, and Levin (1994) examine predatory pricing in an experiment where a monopolist faces a series of potential entrants. Both papers find strong evidence for reputation formation in setups with a finite time horizon.

13. The disciplining effect of endogenously formed long-term relationships has also been experimentally investigated in the context of moral hazard in credit markets. See Fehr and Zehnder (2009) and Brown and Zehnder (2007).

14. A large literature, using evidence from case studies and surveys, exists in the organizational behavior literature. Without going into detail, we—again—take issue with many of them for lack of incentives or randomization of the treatments. See Rotemberg (2006) for an excellent review geared towards economists and many references to these studies.

15. As we discuss in subsection 3, “Income Targets and Loss Aversion,” changing the wage on a particular day may have reduced the motivation to exert effort for a different reason.

16. This lower quality translated into many additional tread separations, leading to a large number of deaths and injuries. See Krueger and Mas (2004) for details.

17. In final-offer arbitration, the employer and the employees have to submit a final bid to a third party. This arbitrator then has to pick which of the two bids to implement.

18. A similar effect can be observed in the fairness judgments of price changes. For example, Bolton, Warlop, and Alba (2003) find that, in repeated transactions, the price that a firm charged last was the relevant reference price, much more so than the price the competitors were offering.

19. This section draws heavily on Benjamin (2005), in which proofs of all the statements can be found.

20. Similarly, during a 20 percent deflation between 1879 and 1890 in Switzerland, Imfeld (1991), using evidence from personnel files from five large Swiss firms, reports virtually no wage cuts.

21. This is also known as the goal-gradient effect, as discussed earlier (see Heath, Larrick, and Wu 1999).

22. There is an inherent problem in this class of macroeconomic models that is not solved by simply making the real wage less responsive. The problem is that the data suggest a fair amount of inflation persistence, which is a correlation between current and past inflation, controlling for the driving process of infla-

tion. Fuhrer (2006) makes this argument in detail. See Fuhrer and Moore (1995) for a model giving rise to “true” inherited inflation persistence.

23. The results are remarkably similar to the estimation results in Akerlof, Dickens, and Perry (1996), using a structural model to implicitly estimate the extent of downward wage rigidity from the inflation-unemployment dynamics.

24. One might argue that the timing of graduation is endogenous to the business cycle. However, the results are robust to using the unemployment rate four years after enrollment as an instrument.

25. Owens and Kagel (2009) show that the relative importance of the negative and positive effects of minimum wages on effort strongly depends on the specific experimental setup. They find that the negative effect is more pronounced if the treatments with and without a minimum wage are compared across subjects than when they are compared within subjects.

26. Owens and Kagel (2009) also report findings from sessions where they eliminate a previously introduced minimum wage in their gift exchange setup. However, since the net effect of the introduction of the minimum wage on effort is positive and leads to a Pareto-superior outcome, it is not very surprising that the elimination does not affect outcomes.

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Comments on "The Behavioral Economics of the Labor Market" by Ernst Fehr, Lorenz Goette, and Christian Zehnder

George P. Baker

This paper uses experimental evidence to argue that "fairness preferences" should be built into models of human behavior, and uses this concept to understand several phenomena occurring in labor markets. Fehr, Goette, and Zehnder argue that diverse phenomena such as the form of the wage contract, patterns of wage dynamics, and income targeting can all be understood as manifestations of employees' fairness preferences.

I begin my discussion of this paper by heartily agreeing with the authors' goal of bringing more "behavioral" insights into labor economics. It has long seemed to me that labor economists miss an essential feature of the employment relationship, one that has been known to sociologists and social psychologists for years: people view paid employment as much more than simply an economic transaction. People derive a sense of belonging, identity, and important social benefits from their work and work life. These are notions that remain outside of traditional economic analysis, yet are important to the functioning of organizations and labor markets.

So bringing a more behavioral approach to the functioning of organizations and labor markets will yield, I believe, large benefits. But is adding a preference for fairness the best way to enrich our models? I don't think so. My concern about this approach stems from several sources. First, I think that evidence for a fairness preference rests on shaky foundations. Second, in the context of labor markets and the employment relationship, a repeated game approach to employee and firm behavior seems like a much more sensible way to proceed.

Virtually all of the evidence cited for fairness preferences comes from behavior in one-shot games, especially ultimatum games and gift exchange games. The authors argue that these experiments provide "striking evi-

dence against the self-interest hypothesis” since people in these situations seem to exhibit behavior that helps others, sometimes at their own expense. But is this evidence of a preference for fairness? This implication rests squarely on the assumption that people know, understand, and *feel* that they are truly playing a one-shot game. It is obvious that if an ultimatum game was to be repeated (with the same partner) even one or two times, then the optimal strategy would be for the responder to reject low offers. If your reputation matters at all, then it is not a good idea to be a patsy. This means that rejection of low offers implies fairness preferences (or any other nonself-interested preference) only if the responder has fully internalized the idea of one-shot interactions. The same is clearly true in gift exchange games.

I would suggest that people have a very hard time internalizing the idea of one-shot interactions. People worry about their reputations almost instinctively, and telling them that “there are no long-term consequences” of their play in this game does not make it much easier for them to feel good about playing a strategy (such as being a patsy in the ultimatum game or not reciprocating in a gift exchange) that would be optimal in a one-shot situation. Because of a long evolutionary history of living in small social groups, we have come to care deeply about our reputations and the long-term consequences of our actions: playing a one-shot game is, in a very real sense, an unnatural act. Expecting that laboratory subjects, who are given only a few minutes to understand and think about these games and to fully internalize this unnatural setting, is a tall order. To draw conclusions about their true preferences from their behavior in this unnatural setting is a mistake.

To use evidence from one-shot games to draw implications for human behavior in the employment relationship is especially problematic. If there is any situation in modern life that looks like a repeated game interaction it is employment. People typically interact with their employers over a long period of time, and generally think about the multi-period consequences of their actions at work. The central puzzle of the employment relationship “explained” by Fehr, Goette, and Zehnder—fixed-wage contracts—is more naturally, and parsimoniously, explained by using the tools of repeated game equilibrium.¹

Fairness preferences are also not needed to explain the well-known patterns of wage dynamics cited by Fehr, Goette, and Zehnder. As discussed in the literature since Doeringer and Piore (1971), a model that includes some firm- and task-specific human capital, on-the-job learning, and worker risk aversion can explain most of the patterns described by Fehr, Goette, and Zehnder (see Harris and Holmstrom 1982; Farber and Gibbons 1996; and Gibbons and Waldman 1999, 2006).

One phenomenon that has been regularly documented in the literature—and not very well explained by standard models—is nominal wage rigidity. It would seem that some behavioral model is needed to explain the sort of money illusion that apparently plagues the employment relationship. Fehr, Goette, and Zehnder argue, disappointingly, that their model is silent on whether nominal or real wages should be downwardly rigid. But I think that this is not so. When using their model to explain business cycle effects, Fehr, Goette, and Zehnder argue that a positive demand shock will not lead to much higher wages: since the firm’s profit per worker does not increase, workers do not feel that working harder for the same wage is unfair. In contrast, when there is a positive productivity shock, the firm’s higher profit level leads fairness-preferring workers to demand higher wages. These conclusions spring from the fact that workers’ fairness preferences are determined by how the *surplus is split* between the firm and the worker.

So the fairness-preference model assumes that workers are sensitive to how a surplus is split. But this assumption implies that workers should be very attuned to inflation: rising prices unaccompanied by rising wages shift the surplus to firms and away from workers. Therefore workers should view inflation unaccompanied by wage increases as unfair and demand higher (nominal) wages. Similarly, in noninflationary times, nominal wage cuts which maintain the same surplus split between firms and workers should not trigger fairness concerns. Thus the fairness-preference model fails to deliver money illusion: on the contrary, it suggests that workers are very attuned to price-level changes. This interpretation is contrary to what we see in the data.

To conclude, I am very sympathetic to the authors’ goal of bringing a more behavioral approach to the study of the employment relationship

and labor markets. But I am not convinced that a model that relies on fairness preferences, derived from observing behavior in one-shot laboratory games, is the right first step.

Note

1. The argument that the employment relationship is finite, and therefore any repeated game equilibrium unravels back from retirement, is highly suspect. While the theory of a repeated game equilibrium unraveling is elegant, there is no experimental or field evidence (of which I am aware) of this actually occurring (except perhaps in games played by game theorists!). Consider the implications of this theory in the real world: a 25-year-old employee fails to exert effort at the beginning of his career because he foresees that at as he approaches retirement (decades in the future) there will be an incentive for the firm (whichever firm he works for) to renege on any deal that is made? People do not think this way (or act this way) in the far less socialized environment of the laboratory: the notion that they would do so in the workplace is ridiculous. Furthermore, firms, recognizing the dangers of renegeing on employees as they near retirement, work hard to develop reputations not to renege in this way. Because other employees can observe this renegeing by the firm, this reputational equilibrium is self-enforcing.

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Comments on "The Behavioral Economics of the Labor Market" by Ernst Fehr, Lorenz Goette, and Christian Zehnder

John A. List

As I sat through the first morning session of the conference, "Implications of Behavioral Economics for Economic Policy," I was struck by the interesting presentations as well as the insightful audience remarks. One particularly astute gentlemen sitting directly at my side made several penetrating comments. One remark related to how one should use experimental methods in economics. He noted that there are distinct advantages in conducting experiments with humans rather than fish, for example, since we can ask humans how they came to make their choice and probe their interpretations of the situation. Of course, this is a valid point. The ichthyologist has no idea what it feels like to be a goldfish when the water temperature suddenly changes from 74 degrees Fahrenheit to 80 degrees Fahrenheit, and the fish might find it difficult to relay that information. Likewise, the chemist has little idea what it is like to transform from uranium-239 into neptunium. Experimental economists are potentially in a more fortunate situation. We are able to study the behavior of people in the laboratory, where we can experience the situations ourselves and ask our experimental subjects about their own experiences.

At the heart of the burgeoning literature that studies fairness in labor markets is experimental evidence of just such a kind. The literature has produced an impressive array of experimental treatments that provide data that have been interpreted as providing strong evidence that many agents behave in a reciprocal manner even when the behavior is costly and yields neither present nor future material rewards (for example, see Fehr, Kirchsteiger, and Riedl 1993; Berg, Dickhaut, and McCabe 1995; Fehr, Gächter, and Kirchsteiger 1997; Fehr and Falk 1999; Fehr and

Gächter 2000; Gächter and Falk 2002; Charness 2004; Brown, Falk, and Fehr 2004; Fehr and List 2004). Such findings have been used to argue, as Fehr and Gächter (2000) put it, that:

Reciprocity has powerful implications for many important economic domains [T]here are important conditions in which the self-interest theory is unambiguously refuted. For example, in competitive markets with incomplete contracts, the reciprocal types dominate the aggregate results (160).

Using experimental evidence as the cornerstone, the Fehr, Goette, and Zehnder study that I have been asked to comment on prescribes how labor market models should be changed to reflect the manner in which fairness perceptions might affect the labor market. This is an important step in the discovery process, and I laud the authors for their progress. They have made important strides in this study, and in the broader literature with this research agenda. Yet my assigned duty is not to heap praise on these scholars, but to discuss some issues at the heart of this most recent commotion.¹ In this regard, my comment will take a step back and consider more carefully the empirical evidence—both from the lab and field—that has caused this ruckus in the economics community. I should stress at the beginning that I find this research agenda fascinating and that I firmly believe that certain agents have social preferences (see my own field work—for example, List and Lucking-Reiley 2002; List 2004; Landry et al. 2006; Karlan and List 2007).

Nevertheless, the main message of my comment is that the evidence on social preferences from gift exchange games is more mixed than the authors conclude in this study. The gift exchange field studies Fehr, Goette, and Zehnder discuss are confounded in their interpretation—both reputational and social preferences are likely at work. And in those cases when in fact the field data are able to provide a clean measure of social preferences, the effect is found to be small. Lab experiments potentially avoid some of the confounding issues in field studies, but slight perturbations of experimental conditions can dramatically alter behavior, and the important properties of the lab situation are not conducive to fluid generalizations extended to the world beyond the laboratory. In this way, estimating deep preference parameters in the lab and generalizing them

to the field should be done with care, especially in light of the fact that we have no theory to generalize such parameters.

In the end, I view the economics laboratory setting as having a comparative advantage at providing unique qualitative insights. In addition, the lab is able to shed light on what can happen rather than pinpoint what will happen in a certain field situation. For the social preference literature, the lab evidence has certainly highlighted an interesting phenomenon.

1. A Framework for Laboratory Experiments in Economics

The basic strategy underlying laboratory experiments in the physical sciences and economics is similar. Yet the fact that humans are the subjects studied in the latter discipline raises fundamental questions about the ability to extrapolate experimental findings beyond the economics lab that do not arise in the physical sciences. Recently with Levitt (2007), I have argued that human decisions are influenced not just by monetary calculations, but also by at least five other factors: 1) the presence of moral and ethical considerations; 2) the nature *and* extent to which one's actions are scrutinized by others; 3) the particular context in which the decision is embedded; 4) the self-selection of the individuals making the decisions; and 5) the stakes of the game.

To make my basic point, I briefly recap the framework we introduced. A utility-maximizing individual i is faced with a choice regarding a single action $a \in (0,1)$. The choice of action affects the agent's utility through two channels. The first effect is on the individual's wealth (denoted W_i). The higher the stakes or monetary value of the game, denoted v , the greater the decision's impact on W_i . The second effect is the nonpecuniary moral cost or benefit associated with action i , denoted as M_i . If, for instance, an individual has strong social preferences, he will derive utility from making charitable contributions.

In practice, many factors influence the moral costs associated with an action, but for modeling purposes we focused on just three aspects of the moral determinant: 1) the greater the negative impact of an action has on others, the more negative the moral payoff M_i ; 2) the strength of the

social norms or legal rules (n) that govern behavior in a particular society and influence behavior; and 3) moral concerns depend on the nature and extent of how an individual's actions are scrutinized. Scrutiny is inherently a multi-dimensional concept, but for simplicity's sake assume that it encompasses only the nature and extent of what is being examined. The nature of scrutiny is exemplified by the presence of an experimenter, who potentially alters the subject's perception of the situation. More broadly, the experimental environment itself might draw upon a different set of expectations than markets. The extent of scrutiny relates to the anonymity of the subject's decision. In the model below, I denote the effect of scrutiny as s , with higher levels of s associated with greater moral costs.

Focusing on the case in which utility is additively separable in the moral and wealth arguments, I make the utility function when an individual i takes action a as

$$(1) \quad U_i(a, v, n, s) = M_i(a, v, n, s) + W_i(a, v).$$

Solving this simple decision problem yields several predictions, as discussed in Levitt and List (2007). For example, the greater the social norm is against the wealth-maximizing choice or the degree of scrutiny, the larger the deviation from that choice. Furthermore, as the stakes of the game rise, wealth concerns will increase in importance relative to fairness concerns; that is, $|\partial M/\partial v| < |\partial W/\partial v|$. Such a framework makes it clear that the greater the extent to which the lab environment mirrors the naturally occurring setting that it is modeling, the more confident one can be that the lab results will be generalizable. If the lab setting diverges from the real-world environment of interest, the model provides a framework for predicting in what direction behavior demonstrated in the the lab will deviate from behavior displayed outside the lab.

2. Empirical Evidence

The model can speak to a wide range of experimental results, but its bite is likely to be greatest for those games in which there is the potential for a strong moral component to behavior. Research on social preferences, the

topic of this conference paper, fits the bill. Table 1 highlights a handful of popular empirical approaches—ranging from methods that generate data to techniques used to model data—that have been used to explore preferences. In the leftmost portion of table 1 is laboratory experiments in economics, which are used to generate data; by construction the *ideal* experimental laboratory environment represents the “cleanest test tubes” case. Some might view sterility as a necessary detraction, but sterility serves an important purpose: in an ideal laboratory experiment this very sterility allows an uncompromised glimpse at the effects of exogenous treatments on behavior that takes place in the lab. Of course, making generalizations outside of this domain might prove difficult in some cases, but to obtain the effect of treatment in this particular domain the only assumption necessary is appropriate randomization.

The rightmost part of the empirical spectrum in table 1 includes several examples of empirical models that make necessary identifying assumptions to pinpoint treatment effects from naturally occurring data. These are well-known and need not be further discussed here. Between laboratory experiments and models estimated using naturally occurring data are the various types of economic field experiments that have been introduced recently.² As discussed more fully in List (2006a), field experiments represent a useful bridge between the laboratory setting and naturally

Table 1
A Field Experiment Bridge

	Controlled Data		Naturally Occurring Data
	Lab	AFE	FFE
			NFE, NE, PSM, IV, STR
■ Lab:	Lab experiment		
■ AFE:	Artefactual field experiment		
■ FFE:	Framed field experiment		
■ NFE:	Natural field experiment		
■ NE:	Natural experiment		
■ PSM:	Propensity score estimation		
■ IV:	Instrumental variables estimation		
■ STR:	Structural modelling		

Source: Author's calculations.

occurring data. Below, we will find that they are crucial when considering generalizability of results.

A useful starting point to summarize the literature is to consider the findings using naturally occurring data, or the rightmost approaches in table 1. An early excellent example is the striking evidence consistent with negative reciprocity on the part of disgruntled Bridgestone/Firestone employees documented by Krueger and Mas (2004). Making use of the variation in product quality induced by the contentious strike and subsequent hiring of replacement workers at Bridgestone/Firestone's Decatur, Illinois plant in the mid-1990s, the authors report that labor strife at the Decatur plant closely coincided with lower product quality. Similarly, Mas (2006) documents persistent adverse effects on police performance following arbitration decisions in favor of the municipality.

The evidence using naturally occurring data is not uniform, however. Chen (2005), who uses a large data set drawn from the Australian Workplace Industrial Relations Survey to explore reciprocity in the workplace, finds little evidence consistent with positive or negative reciprocity. In addition, the empirical results in Lee and Rupp (2006) show that the decreased effort on the part of U.S. commercial airline pilots following pay cuts is a very short-lived behavioral response: even though in the first week after a pay cut frequent and longer flight delays are observed, after the first week there is no difference in airline flight performance. In this manner, these data share important similarities to the short-run effects of shocks observed in the natural field experiment by Gneezy and List (2006).

Moving from the evidence drawn from naturally occurring data to the results from field experiments that explore behavior in repeated play settings, my interpretation of the most recent evidence is that the relationships are *consistent* with models of positive and negative reciprocity (see, for example, Bandiera, Rasul, and Barankay 2005; List 2006b; Cohn, Fehr, and Goette 2007; Marechal and Thöni 2007; Al-Ubaydli et al. 2008; and Bellemare and Shearer 2009). For instance, in List (2006b) I had buying confederates approach dealers on the floor of a sports card show, instructing them to offer different prices in return for sports cards of varying quality. When there was likely to be future interaction and the

consumers could easily certify sports card quality, I found a strong statistical relationship between price and quality provided.

An important consideration, however, is that the theoretical framework proposed above highlights that such data correlations observed in these natural field experiments can operate through social preferences and/or strategic reciprocity. Even if we conclude that the mixed evidence from the naturally occurring data combined with evidence from the natural field experiments painted a picture of an important statistical relationship, by not shutting down the operation of one channel (social preferences or strategic reciprocity), we are confounded in the data interpretation.

Much like we would not ascribe the trendy décor and hip music in Starbucks as evidence of social preferences, the data from the natural field experiments can of course be driven by reputational concerns. Indeed, buttressing this argument is the fact that in my natural field experiment (List 2006b) I also approached the same population of sellers to buy goods that could not be graded. In addition, I approached sellers with whom little future interaction was expected. In both cases a scant statistical relationship between price and quality emerged.

Controlled field experiments as well as laboratory experiments are powerful in the sense that they can *potentially* preclude that reciprocal responses will lead to future material rewards, effectively isolating social preferences. The most common lab game in this spirit is the vast literature on gift exchange, as cited above and reported in Fehr, Gächter, and Kirchsteiger (1997). The experiment is a sequential prisoner dilemma game that has buyers deciding how much money to send to a seller in stage one. In stage two, the seller views this offer (sometimes the offer is multiplied by a factor greater than one) and decides whether to accept it and, if so, what quality to return. The labor-market setting naturally follows if the wage, employer, employee, and work effort are inserted in the relevant portions of the statements. The key behind this approach is that the analyst creates a one-shot environment.

In a natural field experiment testing the gift exchange hypothesis in two actual one-shot labor markets (classifying books in a library and door-to-door soliciting), Gneezy and List (2006) find that worker effort

in the first few hours on the job is considerably higher in a “gift” treatment than in a “nongift” treatment.³ This result is consonant with the bulk of laboratory evidence on gift exchange cited above. As previously mentioned, however, the result in Gneezy and List (2006) wanes over time and in the long run the gift has a small and statistically insignificant effect. Such insights are in line with results from the psychology literature in that there are important behavioral differences between short-run (*hot*) and long-run (*cold*) decisionmaking (see Loewenstein and Schkade 1999). The notion that positive wage shocks do not invoke long-run effects in effort levels is also consistent with Hennig-Schmidt, Rockenbach, and Sadrieh’s (2006) field experiment (and the lab treatments when employees did not know the surplus division) and Kube, Maréchal, and Puppe (2006). However, it is important to note that the latter do find evidence that negative gifts have short- and long-run effects.

As the preceding discussion suggests, the evidence is generally mixed or negative on the gift exchange relationship in the long run when the repeated game incentives are suppressed. Thus, a useful take-away point is that in one-shot interactions the impact of gift exchange on aggregate market efficiency is small. Equally as important, engaging in gift exchange is unprofitable for principals in these settings. Again, Fehr, Goette, and Zehnder seem to agree, which is an important departure from the earlier literature that argued ferociously about the empirical importance of such preferences in one-shot settings.

3. Discussion

Fehr, Goette, and Zehnder stress that the empirical literature shows that “repeated interactions are a potentially powerful multiplier of the effect of fairness concerns (196).” While this claim sounds plausible, I know of no empirical evidence that unambiguously shows this result. More specifically, I am unaware of data that suggests reputational concerns by themselves do not yield the data relationships that are consistent with gift exchange—that is, reputational concerns are able to explain the results without appealing to fairness concerns. Moreover, as alluded to in the empirical data summary, the literature shows that in one-shot environ-

ments the effect of social preferences on aggregate market efficiency is small and trusting actions are unprofitable for the principals.

Yet I am sure that a vector of laboratory design parameters exists that yield a measurable effect of social preferences on market efficiency that is also profitable for the principals.⁴ When this set of laboratory results is released, I will interpret the data cautiously because the model in section 1 and the accompanying empirical evidence suggest that a wide range of factors affect the degree to which an agent’s actions will exhibit prosocial tendencies (including the nature and extent of scrutiny), small changes in the way a decision is framed, the stakes involved, self-selection of participants, and artificial restrictions on the action space or duration of play.

List (2006b) presents evidence in favor of some of these conjectures. As briefly mentioned earlier, that study carries out gift exchange experiments in the lab and field that have buyers make price offers to sellers, and in return the sellers select the quality level of the good provided to the buyer. Higher quality goods are costlier for sellers to produce than lower quality goods, but are more highly valued by buyers. In the lab, the results mirrored the typical findings with other subject pools: strong evidence for social preferences was observed.

I then carried out a second lab experiment that maintained the central elements of the gift exchange game, but in a form that was more closely aligned to the context in which sports card trading takes place. The goods exchanged in this lab treatment were actual baseball cards whose market values are heavily influenced by minor differences in condition that are difficult for untrained consumers to detect. If social preferences are present on the part of card sellers, then buyers who offer more money should be rewarded with higher quality cards. When card sellers were brought into the lab to sell their cards, which were subsequently professionally graded, the results paralleled those obtained in the standard gift exchange game with student subjects. However, as noted above, when these same sellers were not aware that their behavior was being scrutinized, the social preferences so routinely observed in the lab were significantly attenuated in the field. The properties of the situation changed in an important manner and this caused sellers to change their behavior in a predictable way.

Other field-generated data yield similar conclusions. For example, making use of personnel data from a leading commercial orchard based in the United Kingdom, Bandiera, Rasul, and Barankay (2005) find that behavior is consistent with a model of social preferences when workers can be monitored, but when workers cannot be monitored, prosocial behaviors disappear. Being monitored proves to be the critical factor influencing behavior in this study. Further, Benz and Meier (2008) combine insights gained from a controlled laboratory experiment and naturally occurring data to compare how individuals behave in donation laboratory experiments and how the same individuals behave in the field. Consistent with the theory in Fehr, Goette, and Zehnder's section 1, they find some evidence of correlation across situations, but find that subjects who in the past have never contributed to charities gave 75 percent of their endowment to the charity in the lab experiment. Similarly, those who never gave to charities subsequent to the lab experiment gave more than 50 percent of their experimental endowment to the charities in the lab experiment.

Gneezy, Haruvy, and Yafe (2004) find that while behavior in a social dilemma game in the laboratory exhibits a considerable level of cooperative behavior, in a framed field experiment that closely resembles the laboratory game they find no evidence of cooperative play—even though both experimental samples are drawn from the same student population. They speculate that unfamiliarity with the task and confusion are two reasons why negative externalities are influential in the lab but not in the field. Such results are consistent with our simple model.

Overall, these results are consistent with the wealth of psychological literature that suggests there is only weak evidence of cross-situational consistency of behavior (see, for example, Mischel 1968; Ross and Nisbett 1991). For instance, Hartshorne and May (1928) discovered that people who cheat in one situation are not the people who cheat in another. If this result spills over to a measurement of prosocial preferences, it means either that (a) there is not a general cross-situational trait called "other regarding," and/or (b) the subjects view one situation as relevant to social preferences and the other as irrelevant. In either case, such insights are consonant with the model, which predicts that factors generating perceptible differences between environments can lead to important behavioral deviations.

4. Concluding Thoughts

Akin to natural scientists, economists have employed experimental methods to lend insights into important phenomena. Recently, the use of laboratory experiments to measure deep preference parameters has grown in popularity, particularly in relation to measuring social preferences. Perhaps the most fundamental question concerning this line of research is whether findings from the lab are likely to provide reliable inferences outside of the laboratory.

In this spirit, the advantage of experimenting with humans becomes a *potentially* serious liability. The choices that individuals make depend not just on financial implications, but also on the nature and degree of others' scrutiny, the particular context in which a decision is embedded, and the manner in which participants are selected to participate. Because the lab systematically differs from most naturally occurring environments on these dimensions, experiments may not always yield results that are readily generalizable.⁵

As I sat down after delivering this message at the conference I was met with a nod, the astute gentleman to my side kindly noted that experimenting with fish does indeed have some advantages after all. "Thank you, Professor Solow" was the only response I could muster.

Notes

1. The interested reader should see the older literature as well. Kaufman (1988) provides an edited volume that reviews and assesses the work of four institutional labor economists (John Dunlap, Clark Kerr, Richard Lester, and Lloyd Reynolds). The volume also includes discussions from each of the scholars pertaining to strengths and weaknesses of the literature and the current state of the art. The remarks by Clark Kerr are of particular relevance here. These four economists also wrote a paper titled "Does the New Generation of Labor Economists Know More than the Old Generation?" for Richard Freeman in 1987. Thanks to Alan Krueger for pointing me in this direction and providing the citations. One speculation for why this research agenda is now picking up steam is that what separates the current interest in this topic from the older literature is the recent experimental evidence brought forth.

2. Harrison and List (2004) propose six factors that can be used to determine the field context of an experiment. In doing so, they adopted the term "artefactual" field experiment to denote laboratory experiments with nonstandard sub-

ject pools. Moving closer to naturally occurring data, Harrison and List (2004) denote a “framed field experiment” as the same as an artefactual field experiment but with field context in the commodity, task, stakes, or information set of the subjects. Finally, a “natural field experiment” is the same as a framed field experiment but where the environment is one where the subjects naturally undertake these tasks and where the subjects do not know that they are participants in an experiment. Such an exercise is important in that it represents an approach that combines the most attractive elements of the laboratory setting and naturally-occurring data: randomization and realism.

3. This result is qualitatively similar to other one-shot experiments, but these other studies do not find statistically significant results (see, for example, the field and lab treatments in Kube, Maréchal, and Puppe (2006), and Hennig-Schmidt, Rockebach, and Sadrieh (2006) when employees did not know the surplus division).

4. My intuition is that if one simply increases the multiplier, that will do the trick.

5. This point, of course, applies with equal force to data generated from naturally occurring environments.

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5

Behavioral Economics and the Housing Market

U.S. House Price Dynamics and Behavioral Finance

Christopher J. Mayer and Todd Sinai

There has been considerable debate in recent years regarding the role of behavioral factors in determining housing prices. The question of whether psychology matters in the housing market has been settled long ago: the answer is yes. Rather, economists are now debating in what ways psychology impacts market behavior and how large an effect this impact has on housing prices.

One oft-cited example of a clear behavioral bubble in housing is the sharp boom-bust in the Vancouver housing market during the early 1980s (see figure 1). In the 18 months between January 1980 and July 1981, real house prices grew 87 percent. In the subsequent 18 months, real prices fell by nearly 44 percent, plateauing at a level only 6 percent above where prices were three years earlier before the boom began. While news and rumors about Britain's returning Hong Kong to China may have swayed sentiment in the Vancouver market, where many wealthy Hong Kong residents own second homes, it is very difficult to use fundamental factors in explaining the sudden boom-bust pattern witnessed in the early 1980s.

In this paper we examine the relative roles played by economic fundamentals and market psychology in explaining U.S. house price dynamics using two different boom periods, one in the 1980s and the other one in the early-to-mid-2000s. We begin by considering what proportion of the variation in the house price-rent ratio within metropolitan areas can be explained by fundamentals using a single-period version of the user cost model with static expectations of price growth, as in Himmelberg, Mayer, and Sinai (2005). We then consider how much additional variation can be explained by a handful of behavioral finance theories

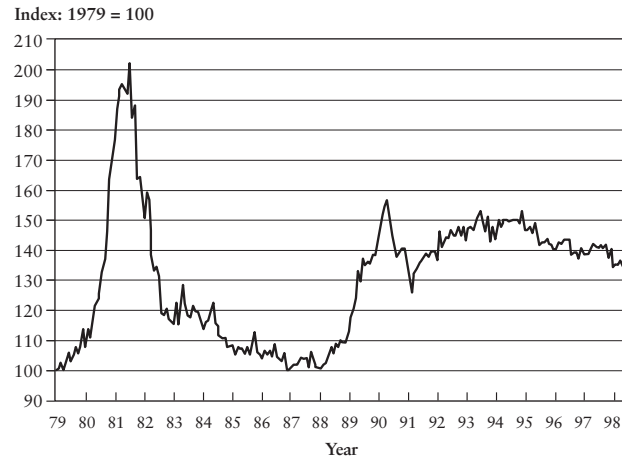


Figure 1
Vancouver Condominium Monthly Real Price Index
Source: Bulan, Mayer, and Somerville (2006).

and conjectures, such as backward-looking expectations of house price growth and inflation illusion. By examining the house price booms of the 1980s and 2000s separately, we can see if the relative weights given to fundamental and to behavioral explanations vary over time.

Our results suggest that both rational and seemingly behavioral factors in the 1980s and in the 2000s explain movements in the price-rent ratio across U.S. metropolitan housing areas over the last 25 years. We find that user cost of capital, which reflects rational asset pricing fundamentals, is one of the most important factors, especially during the 1995–2006 boom. Lending market efficiency also appears to be capitalized into house prices, with higher prices associated with lower origination costs and a greater use of subprime mortgages.

The other important determinant of price-rent ratios is the lagged five-year house price appreciation rate. This result suggests that backward-looking expectations likely play a behavioral role in explaining house price booms, although it is difficult to disentangle backward-looking expectations when using a “rational” model in which households update their beliefs about future house price growth with more recent data. In addi-

tion, the results show little evidence in favor of behavioral explanations based on the one-year lagged house price growth rate or the inflation rate.

We begin with a review of the literature on equilibrium models of house price determination and then examine how behavioral economics and inefficiencies in the lending market may also play a role. Section 2 lays out our simple reduced-form empirical framework. We then describe the data in section 3, which is followed by a description of our empirical findings in section 4. We conclude with a brief discussion of the factors that might influence the direction of future house prices and suggest avenues for future research.

1. Background and Related Literature

There is great dispersion in house price appreciation rates and volatility across different U.S. housing markets over the last three decades. Some southern and midwestern markets like Atlanta, Charlotte, Cleveland, and Houston have shown little long-term appreciation and relatively low volatility in prices (see figure 2a). By contrast, many primarily coastal markets like Boston, Los Angeles, and San Francisco have shown higher long-term rates of house price appreciation and also greater peak-to-trough volatility (see figure 2b). Finally, some markets like Las Vegas, Miami, and Phoenix exhibited recent price spikes despite having experienced little real growth in house prices over previous decades (see figure 2c).

One difficulty in decomposing this wide variation in local house price movements across metropolitan areas into so-called fundamental and behavioral factors is the lack of a widely accepted rational dynamic model of house prices that combines local fundamentals—such as changes in economic conditions, risk, and supply constraints—and aggregate fundamentals such as time-series variation in interest rates and inflation. Without such a model as a baseline, it is hard to determine the relative contributions of fundamentals and psychology in generating movements in U.S. house prices.

Recent papers have made some progress in relating fundamentals to house price dynamics. Brunnermeier and Julliard (2008) develop a dynamic rational expectations model of house prices but do not incorporate local factors. Glaeser and Gyourko (2007) calibrate a dynamic

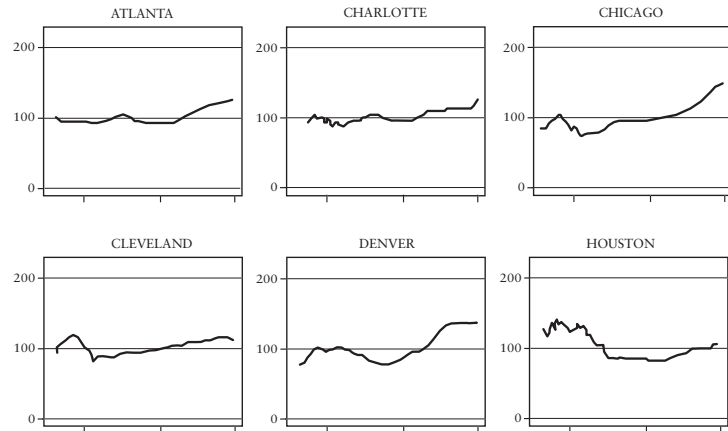


Figure 2a: Steady Markets
 Current as of 2007: Q1
 Source: Mayer (2007) using OFHEO and BLS Real Home Price Index
 Index: 100 = sample average

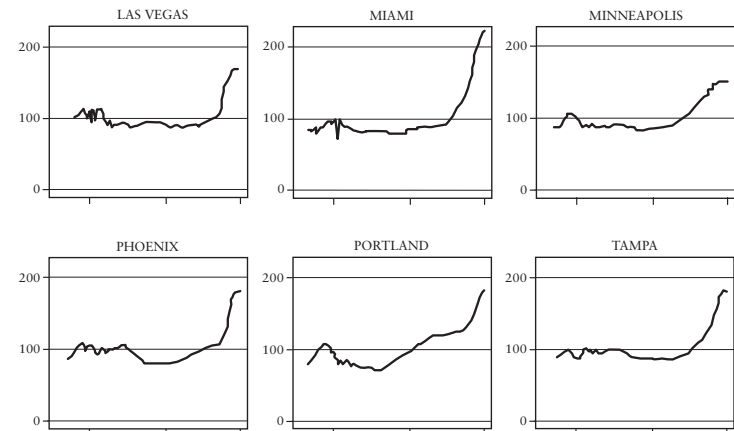


Figure 2c: Recent Boom Markets
 Current as of 2007: Q1
 Source: Mayer (2007) using OFHEO and BLS Real Home Price Index
 Index: 100 = sample average

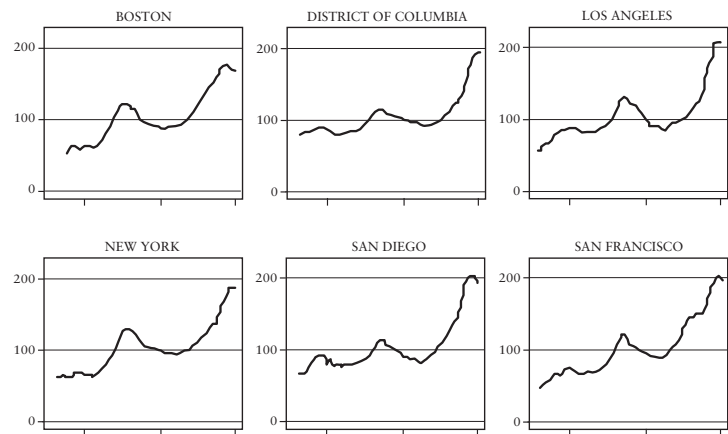


Figure 2b: Cyclical Markets
 Current as of 2007: Q1
 Source: Mayer (2007) using OFHEO and BLS Real Home Price Index
 Index: 100 = sample average

model of housing in a spatial equilibrium which does a very good job explaining the impact of local shocks on house prices. However, Glaeser and Gyourko are not able to incorporate shocks due to interest rates (or incomes), factors which the authors concede may explain some of the serial correlation in their data.

Himmelberg, Mayer, and Sinai (2005) use the standard user cost model (Hendershott and Slemrod 1983; Poterba 1984) to examine whether U.S. house prices relative to rents in 46 metropolitan areas were high in 2004. The authors constructed the user cost using long-term mortgage interest rates and static long-run real appreciation rates, arguing that most households view the purchase of a house based not on a one-year comparison of buying versus renting, but based on a longer-run holding period. Despite its ability to combine local and aggregate factors, the user cost model contains some simplifying assumptions that abstract from important real-world issues. In particular, the standard user cost model does not characterize how households form their expectations of future price or rent appreciation.

Theoretical papers have argued that liquidity constraints might also explain the seemingly excessive sensitivity of house prices to income shocks (Stein 1995; Ortalo-Magné and Rady 1999, 2006). Lamont and Stein (1999), Engelhardt (1994, 1996), and Genesove and Mayer (1997) present empirical evidence in favor of the liquidity constraints hypothesis. Yet liquidity constraints are unlikely to explain why volatility differs across U.S. housing markets.

Some authors have argued that psychological factors rather than fundamental issues play the key role in determining house price dynamics. The earliest academic papers on the role of psychology on real estate prices focused on unexplained serial correlation in real estate prices (see Case and Shiller 1989). Of course, serial correlation itself is not necessarily evidence of irrational markets if the underlying growth in rental prices is also serially correlated. Yet data on rents are very hard to obtain, confounding tests of market efficiency. Meese and Wallace (1994) obtained detailed rental data from advertisements and estimated an asset pricing model for houses in the San Francisco area. The authors concluded that the price run-up in the late 1980s was not fully justified by fundamentals. Both papers concluded that pricing inefficiencies are due to high transaction costs that limit arbitrage opportunities for rational investors.¹

Psychology, too, may affect how households set their expectations of future price appreciation. Case and Shiller (1988) surveyed recent home buyers in four American cities about their expectations of future house price growth. Recent buyers in Los Angeles, a market with strong house price appreciation in the 1980s, reported that they expected much higher long-term house price appreciation than households in a control market, Milwaukee, where house prices were flat in the 1980s. In a subsequent survey (Case and Shiller 2003), recent buyers in Milwaukee raised their reported expected appreciation in line with the national housing boom. By 2006, recent home buyers in both Milwaukee and Los Angeles had lowered their reported expected price appreciation for the next year, although they did not make many downward adjustments in their 10-year expected appreciation rate (Shiller 2007). Shiller cites the survey evidence and other case studies to support his contention that the boom cannot be explained in terms of fundamentals such as rents or construction costs, and he concludes that: “The psychological expectations coordi-

nation problem appears to be a major factor in explaining the extreme momentum of home price increases. Investors who think that home prices will continue to go up because they perceive prices as going up generally around the world may not change this expectation easily since they will have trouble coordinating on a time to make the change” (118).

A second psychological theory proposed by Brunnermeier and Julliard (2008) argues that households cannot fully disentangle real and nominal changes in interest rates and rents. As a result, when expected inflation falls, homeowners take into account low nominal interest rates when making housing purchase decisions without recognizing that future appreciation rates of prices and rents will fall commensurately. They argue that falling inflation leads to otherwise unjustified price spikes and speculative booms, and can help explain the run-up in U.S. and global housing prices in the 2000s. As evidence, Brunnermeier and Julliard show that expected inflation is correlated with the residuals of a dynamic rational expectations model of house prices.

Probably the most direct evidence on the importance of psychology in real estate markets focuses specifically on loss aversion in housing downturns (Genesove and Mayer 2001; Engelhardt 2003). Yet explaining the current housing boom or even excess volatility in downturns through loss aversion may be difficult. Since loss-averse sellers set higher asking prices when house prices are falling, this particular psychological factor actually leads to lower volatility over the cycle, making the puzzle of possibly excess cyclical volatility an even more difficult problem to explain.

Finally, another set of papers focuses on rational dispersion in long-run price appreciation rather than on short-run dynamics. Van Nieuwerburg and Weil (2007) calibrate a model that uses productivity differences to explain long-run price dispersion across cities. More relevant for our exercise, Gyourko, Mayer, and Sinai (2006) present evidence suggesting that increasing numbers of households and income growth in the right tail of the income distribution, combined with supply constraints in some highly desirable cities, has led to a 50-year trend of faster house price growth in certain “superstar cities.” According to this paper’s model, households might rationally expect future prices to rise faster in superstar cities like Boston, New York, and San Francisco than in other cities in the United States.

2. Empirical Model

Our empirical analysis examines which factors, fundamental and behavioral, are correlated with house price dynamics within U.S. metropolitan areas. As a baseline, we begin with a rational model of asset price equilibrium and see how much of the empirical volatility in the price-rent ratio such a model can explain. To that baseline, we add proxies for other rational and behavioral factors to see which are correlated with the unexplained residual.

To form the rational market baseline, we assume that housing markets are perfectly competitive and that in equilibrium, risk-adjusted returns for homeowners and landlords should be equated across investments. This yields the usual user cost formula (such as Hendershott and Slemrod 1983; Poterba 1984) where spot rents in a housing market are set such that:

$$(1) \quad R_{it} = P_{it} \left((1 - \tau_{it})r_t + m - E[\% \Delta P]_{it} \right).$$

R_{it} is the rent for one unit of housing services for one year in city i at time t , P_{it} is the corresponding price for prepurchasing the entire future flow of R_{it} , $(1 - \tau_{it})r_t$ is the after-tax, equivalent-risk opportunity cost of capital, m is a measure of carrying costs (such as maintenance) per dollar of house, and $E[\% \Delta P]_{it}$ is the expectation of future house price appreciation in city i at time t .

To match our empirical work, below we rearrange equation (1) to obtain the price-rent ratio, P/R. Labeling the terms in large parentheses as describing user cost, UC, P/R is:

$$(2) \quad \frac{P_{it}}{R_{it}} = \frac{1}{UC(\tau_{it}, r_t, m, E[\% \Delta P]_{it})}.$$

Examining the price-rent ratio provides a better measure of asset market conditions than does price alone. House prices are determined both by supply and demand for housing services as well as the overall asset market, making it difficult to empirically identify changes in prices due to the asset changes alone.² By conditioning on-the-spot rent for housing, the price-rent ratio leaves only asset market factors to explain how cur-

rent and expected future rental values are capitalized into current prices. In the user cost framework in equation (2), home buyers pay a higher price multiple compared to rents when the after-tax opportunity cost of capital is lower. So, for example, when interest rates fall, purchasers of housing assets will pay a higher price for a given dividend flow (either rental income or the imputed rent from living in the house). Of course, the price-rent ratio also expands when expected future price growth is higher (for example, when more of the return comes in the form of a capital gain).

Re-characterizing the user cost model in the price-rent framework also highlights the highly nonlinear relationship between changes in the price-rent ratio and user costs. Himmelberg, Mayer, and Sinai (2005) and Campbell et al. (2007) point out that when user costs are low, convexity implies that relatively small absolute changes in user costs (caused by shocks to long-term interest rates, for example) can cause very large percentage changes in the price-rent ratio.

The user cost model described in equation (2) provides some empirical guidance, but it is incomplete. For example, the user cost framework does not address how expectations of capital gains are formed. In Poterba's original framework, home buyers are assumed to have perfect foresight. However, Case and Shiller (1988, 2003) provide survey evidence that homeowners have price growth expectations that are inconsistent with perfect foresight. Himmelberg, Mayer, and Sinai (2005) assume that homeowners have static expectations that house prices will grow at their long-run average rate. But another possibility is that home buyers form their expectations based on recent history.

In addition, the measure of the opportunity cost of capital, r_t , does not fall out of the user cost model. Himmelberg, Mayer, and Sinai (2005) uses risk-free interest rates plus a time-invariant risk premium. However, the risk premium required by lenders or equity investors may vary over time, leading them to accept more risk at a given yield. For example, Allen and Gale (1999) and Pavlov and Wachter (2006) discuss conditions in which competition may lead lenders to misprice risk. In the data, lenders allowed homeowners to take on more debt as a percentage of the house value (for instance, allowing a higher loan-to-value ratio) and made loans

to much riskier borrowers (such as borrowers who have lower FICO credit scores or who cannot document their current income). In addition, during the 1980s, low capital reserves maintained by government-insured savings and loan institutions also led lenders to accept more nonpriced risk. In these cases, the decline in the true risk-adjusted cost of capital would be greater than what would be reflected in the Himmelberg, Mayer, and Sinai measure.³ Along another dimension, Brunnermeier and Julliard (2008) hypothesize that households consider nominal interest rates rather than real interest rates when making borrowing decisions.

One way to test for the relevance of these various factors would be to incorporate them into the user cost framework and see which measure(s) of user cost best fit the data. However, a variety of theoretical and practical considerations preclude pursuing this approach. For one, the user cost model presumes a rational asset market equilibrium. Embedding parameters in a framework that potentially derives from an underlying model where expected returns do not equate across investments would be inconsistent and difficult to interpret. In addition, if the expected capital gain is high enough, the user cost can be negative, implying that expected price appreciation outstrips the cost of capital. If that were the case, the return on home buying would be infinite and the user cost would be undefined.

Our empirical approach is to regress the log of the price-rent ratio on the log of the inverse user cost, as defined in Himmelberg, Mayer, and Sinai (2005), and include proxies for low risk premia in the capital markets, inflation illusion, and backward-looking expectations of price growth:⁴

$$(3) \quad \ln\left(\frac{P_{it}}{R_{it}}\right) = \alpha + \beta \ln\left(\frac{1}{UC(\tau_{it}, r_{it}, m, E[\% \Delta P]_{it})}\right) + \delta C_{it} + \gamma B_{it} + \phi \Pi_t + \kappa_i + \varepsilon_{it}.$$

C_{it} is a vector of proxies for the easy availability of capital, including the average loan-to-value ratio, the fraction of mortgage originations that have adjustable interest rates, average points and fees, and the fraction of mortgage originations that are subprime. B_{it} is a vector of backward-looking measures of house price appreciation: the average house price growth in a metropolitan statistical area (MSA) i over the prior year and

over the previous five years. To test for the presence of inflation illusion, Π_t is a measure of inflation. A set of MSA indicator variables, κ_i , is also included.

It bears mentioning that in most specifications we choose not to include year dummies, instead using the variation over time in UC , C , B , and Π to help identify their effects on the price-rent ratio. This specification allows us to incorporate two factors inherent in behavioral finance theories. First, inflation illusion can only be considered without national time dummies. Second, Shiller (2007) argues that part of the social epidemics that give rise to U.S. housing cycles are due to national and even international influences that are commonly felt across regions. However, we include the year effects in a small number of specifications where the sample period is short enough that we believe within-MSA variation is more crucial for empirical identification, and where it would be difficult to separately identify national macroeconomic factors.

If the user cost model holds and is correctly specified when we use a real opportunity cost of capital and static long-run expectations of house price growth, we would expect $\hat{\beta}$ to equal 1. If, in addition, this user cost model were the primary determinant of asset pricing in the housing market, we would expect it to have a high R-squared. To the degree that easy credit, inflation illusion, or backward-looking price expectations affect asset pricing in the housing market above and beyond what is already incorporated into this implementation of the user cost model, the estimates of δ , γ , and ϕ should be statistically significantly different from zero, and including C_{it} , B_{it} , and Π_t should increase the explanatory power of the regression.

While the specification in equation (3) is in a reduced form, we believe it will provide additional evidence on which factors are correlated with the price-rent ratio in the housing market and the relative importance of rational (fundamental) and behavioral components. However, we caution that without a structural dynamic model, our results may be sensitive to misspecification of the functional form, especially if some of the included behavioral factors are correlated with measurement error. Alternatively, a lack of statistical significance might not be taken as evidence that a behavioral factor is unimportant, as it may be due to a misspecified model. However, given the absence of models that combine backward-looking

expectations, inflation illusion, and fundamentals such as taxes and forward-looking expectations, our approach should provide a starting point to explore how fundamental and psychological factors influence changes in the price-rent ratio across U.S. metropolitan areas.

3. Data

The most important variable in our paper is the price index for single family homes.⁵ We use the Office of Federal Housing Enterprise Oversight (OFHEO) repeat sales index in all regressions, as opposed to the two other widely cited alternatives, the median sale price of existing homes from the National Association of Realtors and the Standard & Poor's/Case-Shiller repeat sales price index. The biggest advantage of the OFHEO index is that it is reliable for 287 MSAs and divisions, with most of the MSAs covered since 1975–1979. Yet the index also has two major limitations. First, it includes not only sales transactions, but also appraisals from mortgage refinancings that may be less reliable, especially when prices begin to fall. Second, the sample includes only transactions with mortgages sold to Fannie Mae or Freddie Mac, which have an upper limit of \$417,000 in 2007 and lower loan limits in previous years (so-called conforming loans). However, other house price indexes also have flaws. The median price index is less useful for our analysis, both because it is available for a shorter time period and, more importantly, because it is quite sensitive to the mix of houses that sell over the real estate cycle. The Standard & Poor's/Case-Shiller index is arguably more reliable for the MSAs and time periods that it covers because it is based on the universe of all transactions (but not appraisals) and is not subject to a cap on the maximum mortgage amount. Unfortunately, the Standard & Poor's/Case-Shiller index does not have enough history over time to include the 1980s and parts of the 1990s in many MSAs and has a much more limited coverage of MSAs. When possible, we have compared the results of our analysis using the OFHEO data with those using the Standard & Poor's/Case-Shiller data, and found no substantive differences.

Reliable data on rental prices are more limited. We are unable to obtain rental costs for single-family homes, so we instead use rents on compa-

table quality apartments from Reis, Inc. The Reis data are available from 1980 to present in 43 metropolitan areas in the United States. Reis surveys owners for asking rents on rental units with common characteristics. These are the most comprehensive and reliable U.S. rental data available on a historical basis.

An important complication from using the house price indexes from OFHEO and rents based on apartments instead of single-family homes is that we are unable to compute a price-rent ratio that is comparable across markets. The price index is normalized so that one cannot make cross-metropolitan area comparisons, plus we do not know how the quality of the average rental unit compares to average house quality for different metropolitan areas. We address this problem in several steps. First, we compute a rent index for each MSA by dividing the actual rent in each year by the rent in a base year for that MSA. Next, we divide the price index for each MSA by the rent index for each MSA, and finally we set that ratio equal to 1 in a base year/quarter (1998:Q1). This allows us to compute the relative price-rent ratio across years within an MSA, but does not allow us to compare the price-rent ratio level across MSAs. These price-rent ratios are comparable subject to a multiplicative scaling factor for each MSA because we only observe the estimated price-rent ratio.⁶

Our other major challenge is measuring households' expected growth rate of housing prices. For our base measure of static long-term expected future growth rates, we use the average real growth rate of house prices for 1950–2000 computed by Gyourko, Mayer, and Sinai (2006) from the U.S. Census. All other calculations based on historical appreciation rates come from lagged appreciation of the OFHEO price indexes.

Other variables come from standard sources. We calculate long-term expected inflation by splicing two series together. From 1998 to present, we compute long-term expected inflation as the difference between the yield on the 30-year U.S. Treasury Inflation-Protected Security (TIPS) and the yield on a 30-year U.S. Treasury security. Prior to the beginning of the TIPS market in 1998, we use the 10-year expected inflation rate from the Livingston Survey of economic forecasters as published by the Federal Reserve Bank of Philadelphia. Interest rates are obtained from constant

maturity one-year and 10-year U.S. Treasury securities and mortgage rates from the Federal Reserve Board for 30-year fixed rate mortgages. Per-capita income and inflation (based on the Consumer Price Index less shelter) are obtained from the Bureau of Labor Statistics.

Computing the tax subsidy to owner-occupied housing is a bit more complicated and described in more detail in Himmelberg, Mayer, and Sinai (2005). We use average property tax rates from Emrath (2002) and income tax rates which we collect from the TAXSIM model of the National Bureau of Economic Research. However, data from the Internal Revenue Service show that 65 percent of tax-filing households do not itemize their tax deductions and, if they are homeowners, do not benefit from the tax deductibility of mortgage interest and property taxes. To account at least roughly for the higher cost of owning for the nonitemizers, we reduce the tax subsidy in our calculations by 50 percent.

We also assume constant depreciation rates (2 percent) and risk premia (2 percent) for all MSAs in our sample and for all years. These assumptions, while simplistic, could bias our calculated user costs in either direction. We might overestimate the spread in user costs between high-priced and low-priced MSAs by ignoring the fact that the value of structures is generally smaller-than-average relative to the land value in the highest land-cost markets such as New York and San Francisco. Thus depreciation might be less important than we assume when we calculate the user cost in low user cost/high appreciation rate cities (Davis and Palumbo 2007). At the same time, the effect of lower-than-average depreciation rates in creating an upward bias in our calculated user cost for the highest priced cities like San Francisco might be offset by the possibility that the house price risk is also above average in these high-priced cities, creating a bias in the other direction. Some research has argued that housing in high-priced cities is riskier because the standard deviation of house prices is much higher (Case and Shiller 2003; Hwang and Quigley 2006), while other research argues that homeowners can partially hedge this rent and price risk (Sinai and Souleles 2005). Without further guidance from the literature on this issue, our calculations do not allow for variation in risk across markets.

Finally, we obtain lending covariates from two principal sources. Yearly data on the use of adjustable-rate mortgages (ARMs), the loan-to-

value (LTV) ratio, and average fees/points paid on mortgages comes from the Federal Housing Finance Board and is based on the Monthly Interest Rate Survey (MIRS) of rates and terms from conventional mortgages for 32 metropolitan areas and all 50 states. While the MIRS sample has unique data at the metropolitan area level, it is based on a less than fully comprehensive sample of conventional mortgages that does not include Alt-A and subprime mortgages. In addition, the LTV data are only for primary mortgages and do not include piggyback loans. Thus the MIRS data almost surely understate the usage of ARMs and effective LTV ratios, both of which are more prevalent among subprime loans than the conventional mortgage population. The MIRS data run from 1978 to 2005 for MSAs and through 2006 for states. We use the MSA data from 1984–2005 and substitute state values for MSA values for 2006. However, the MIRS cities do not completely overlap with the Reis markets. We report regression results alternatively using two data samples, listed in appendix table 1. The complete sample includes all 43 metropolitan areas with rent data from Reis. When we include the lending covariates, we restrict the sample to 26 cities that are in the Reis and the MIRS data. Results are generally similar across the two sample groups when we include the same covariates.

Our data on subprime mortgages are reported at the state level and are based on lender-reported mortgage data based on requirements from the Home Mortgage Discrimination Act (HMDA). While these data are commonly used and reported in research reports and in the press, they have a significant flaw. The definition of “subprime loans” is based on a primary categorization of the lender. So-called subprime lenders sometimes originate conventional or high-quality (“prime”) mortgages and some conventional lenders issue appreciable numbers of subprime mortgages. It is impossible to know the overall direction of this bias. We use subprime data from the Mortgage Bankers Association for 2002 to 2005 and from *Inside Mortgage Finance* for 2000 to 2001. These data are not available prior to 2000, when subprime mortgages were much less widely available.

Summary statistics are reported in table 1 for all variables used in our analysis. We begin our analysis in 1984 to allow the inclusion of the lagged five-year appreciation rate as an independent variable. We report both the aggregate standard deviation as well as the average within-MSA

Table 1
Summary Statistics of Variables

	# Obs	Years	Mean	Median	Std. Dev.	Avg. w/in MSA Std. Dev.	Min	Max
Mortgage Interest Rate Survey Subsample								
Price/rent index (P/R)	520	1984–2006	1.06	1.02	0.17	0.18	0.68	2.04
User cost (UC)	520	1984–2006	0.06	0.06	0.01	0.01	0.02	0.08
Ln(P/R)	520	1984–2006	0.05	0.02	0.15	0.15	-0.38	0.72
Ln(1/UC)	520	1984–2006	2.91	2.87	0.20	0.15	2.54	3.69
Lagged five-year growth rate	520	1984–2006	0.05	0.04	0.04	0.03	-0.05	0.20
Lagged one-year growth rate	520	1984–2006	0.06	0.05	0.05	0.05	-0.08	0.27
Inflation rate	520	1984–2006	0.03	0.03	0.01	0.02	0.00	0.06
Loan-to-value ratio (LTV)	520	1984–2006	0.77	0.77	0.04	0.03	0.61	0.86
% Adjustable-rate mortgages (%ARMs)	520	1984–2006	0.27	0.24	0.16	0.12	0.04	0.77
Points and fees (% of mortgage amount)	520	1984–2006	1.15	0.95	0.73	0.81	0.07	3.44
% of loans that are subprime	130	2000–2005	0.11	0.10	0.05	0.04	0.03	0.29
Full Sample								
Price/rent index (P/R)	989	1984–2006	1.08	1.03	0.20	0.18	0.68	2.28
User cost (UC)	989	1984–2006	0.06	0.06	0.01	0.01	0.02	0.10
Ln(P/R)	989	1984–2006	0.06	0.03	0.17	0.15	-0.38	0.82
Ln(1/UC)	989	1984–2006	2.92	2.89	0.24	0.15	2.34	3.86
Lagged five-year growth rate	989	1984–2006	0.05	0.04	0.04	0.03	-0.05	0.20
Lagged one-year growth rate	989	1984–2006	0.06	0.05	0.06	0.05	-0.10	0.33
Inflation rate	989	1984–2006	0.03	0.03	0.02	0.02	0.00	0.06
Loan-to-value ratio (LTV)	520	1984–2006	0.77	0.77	0.04	0.03	0.61	0.86
% Adjustable-rate mortgages (%ARMs)	520	1984–2006	0.27	0.24	0.16	0.12	0.04	0.77
Points and fees (% of mortgage amount)	520	1984–2006	1.29	1.07	0.83	0.81	0.07	3.88
% of loans that are subprime (% Subprime)	258	2000–2005	0.12	0.11	0.05	0.04	0.03	0.29

Source: Authors' calculations.

standard deviation, as the latter better reflects our empirical identification. We should also note that the mean values of the price-rent ratio and $\ln(P/R)$ are not meaningful since both are measured as indexes.

There are several instructive facts in the data. While many commentators have reported the seemingly large variation in the $\ln(P/R)$ ratio, $\ln(1/\text{user cost})$ exhibits the same within-MSA standard deviation. Thus the MSA price-rent ratio is not a priori more volatile than might be expected from a simple user cost model. Second, the lagged five-year nominal growth rate exhibits quite substantial variation, rising as much as 20 percent in the highest-appreciation rate MSA and falling as much as 5 percent, with a within-MSA standard deviation of 3 percent.

4. Empirical Results

We start by establishing a baseline for how much of the variation in the price-rent ratio can be explained by the user cost model with real interest rates and static expectations of capital gains based on long-run real house price growth. The first column of table 2 reports the results from estimating equation (3) over the 1984–2006 period with only $\ln(1/\text{user cost})$ on the right-side. The estimated coefficient on user cost is 0.48 (with a standard error of 0.03), well below (and statistically different from) the value of 1.0 that would be expected if the standard user cost model held. Given this estimate, a 10 percent decline in user cost from

the sample average would lead to a 5.3 percent increase in house prices, holding rent constant.⁷ The R-squared is 0.28, so just over one-quarter of the variation in the price-rent ratio is explained by user cost and a set of MSA fixed effects.

Next we split the sample into two periods, 1984–1994 and 1995–2006. We do so to follow-up on the observation in Himmelberg, Mayer, and Sinai (2005) that the user cost model fit particularly poorly in the 1980s. The sample split shows that the user cost model performs badly in the earlier time period (with a coefficient of 0.12 on user cost), but there is excess sensitivity in the later period (with a coefficient of 1.27). Thus between 1984 and 1994, changes in user cost had little effect on the price-rent ratio, while the effect was 10 times stronger in the late 1990s and early 2000s. This result is consistent with the view that the run-up in U.S. house prices in the 1980s was *not* supported by fundamentals, while the price growth in the 2000s was better supported. Indeed, it is apparent a priori that this should be the case: user costs were high in the 1980s since real interest rates were high, yet house prices experienced rampant growth. By contrast, in the 2000s movements in the price-rent ratio trended with a strong decline in real interest rates. In both periods the R-squared is just over 0.55, suggesting that considerable variation in the price-rent ratio remains to be explained.

Capital Availability as an Explanation for Housing Booms

In table 3, we add proxies (C_{it}) for changes in loan terms or mortgage market efficiency over time—including the fraction of loans that are adjustable-rate mortgages, average points and fees (a proxy for the improved efficiency of the lending market), and the average LTV ratio in an MSA for a given year—because lenders take on more risk when they underwrite with more leverage. Since we do not have these variables for all the cities with price data, we estimate the model on the subset for which we have complete data, which we label as the “MIRS subsample.” The first column of table 3 replicates the regression from the first column of table 2 using the MIRS subsample and finds almost identical results, albeit with larger standard errors due to the smaller number of observations.

Adding the fraction of ARMs or average points and fees has the expected effect. In column (2) the ARM share is positively correlated

Table 2
Variation in the Price-Rent Ratio, 1984–2006

	Whole Sample		
	1984–2006	1984–1994	1995–2006
$\ln(1/\text{user cost})$	0.48 (0.03)	0.12 (0.03)	1.26 (0.06)
R ²	0.28	0.55	0.57
# Obs	989	473	516
MSA fixed effects	YES	YES	YES

Source: Authors' calculations.

Table 3
Changes in Loan Terms and Mortgage Market Efficiency Over Time

	Mortgage Interest Rate Survey Subsample					
	1984-2006	1984-2006	1984-2006	1984-2006	1984-2006	1984-2006
Ln(1/user cost)	0.50 (0.06)	0.56 (0.06)	0.30 (0.06)	0.51 (0.06)	0.37 (0.06)	-0.13 (0.05)
%ARMs		0.13 (0.05)			0.19 (0.05)	-0.04 (0.04)
Points & fees			-0.06 (0.01)		-0.07 (0.01)	-0.07 (0.01)
Loan-to-value ratio				-0.98 (0.20)	-1.11 (0.18)	-0.99 (0.25)
R ²	0.20	0.21	0.28	0.24	0.35	0.72
# Obs	520	520	520	520	520	234
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
						0.88 (0.07)
						0.23 (0.06)
						-0.21 (0.03)
						-0.56 (0.25)
						0.66
						286

Source: Authors' calculations.

with the price-rent multiple, suggesting that when ARMs are more prevalent, the price-rent ratio is higher. Similarly, when average points and fees are lower, the price-rent ratio is higher, reflecting the fact that the effective cost of capital is lower when points and fees are reduced. Adding these two variables changes the estimated coefficient on user cost, indicating that in part these are picking up some measurement error in the proxy for the cost of capital used in the user cost formula. In column (4) the estimated coefficient on the average LTV ratio is negative, the opposite sign to what would be predicted if relaxing liquidity constraints leads to a higher price-rent ratio. However, the LTV ratio as measured by the Federal Home Loan Banks falls in house price booms, so its sign is not surprising. Also, the variable may be measured incorrectly due to missing second mortgages and the lack of high LTV subprime mortgages. The inclusion of these lending variables generally lowers the coefficient on user cost, suggesting that mismeasurement of the true cost of lending in the user cost model might bias our estimation.

When we divide the sample period between the boom-bust in the 1980s and the boom in the 2000s, again there are significant differences in the relationship between the capital markets and the price-rent ratio. The estimated coefficient on user cost over the 1984-2006 period when all three credit market variables are included is 0.37 (with a standard error of 0.06). But that masks a coefficient of -0.13 during 1984-1994 and 0.88 for 1995-2006. Some of the credit market variables also have different estimated coefficients during the two periods, with the coefficient on the percent ARM variable approximately zero and thus insignificant during the early period, but positive and significant in the 2000s, while the coefficient on the points and fees variable triples in magnitude during the latter period. Indeed, with the exception of the LTV ratio, credit market conditions seem to have a magnified effect on the price-rent ratio in the 1995-2006 boom and provide little help explaining the 1980s boom-bust in U.S. housing prices.

Next we attempt to examine the impact of the growth in subprime lending. In table 4, we examine the extent to which subprime lending is correlated with excess growth in the price-rent ratio. Since data on subprime shares are available only for the 2000-2005 period, we restrict our attention to those six years. The first column of table 4 shows that

Table 4
The Growth in Subprime Mortgages versus the Price-Rent Ratio, 2000–2005

	Subprime Subsample				
	2000–2005	2000–2005	2000–2005	2000–2005	2000–2005
Ln(1/user cost)	0.96 (0.04)	0.82 (0.06)	0.81 (0.08)	0.65 (0.08)	0.90 (0.22)
%Subprime Mortgages		0.43 (0.12)		0.63 (0.15)	1.54 (0.22)
%Adjustable-rate mortgages			0.12 (0.05)	−0.02 (0.06)	0.06 (0.07)
Points & fees			0.01 (0.05)	−0.02 (0.04)	0.01 (0.04)
Loan-to-value ratio			−0.81 (0.23)	−0.75 (0.21)	−0.23 (0.21)
R ²	0.89	0.90	0.90	0.92	0.94
# Obs	130	130	130	130	130
Year fixed effects	No	No	No	No	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes

Source: Authors' calculations.

the user cost model plus MSA dummies fit quite well during that period, with an estimated coefficient on user cost of 0.95 and an R-squared of 0.89. In column 2, we add the share of mortgages originated that were subprime loans. We find that greater fractions of subprime mortgages are correlated with higher price-rent ratios, but that the magnitude of the effect is fairly moderate. The estimated coefficient of 0.42 implies that a one standard deviation increase in the subprime mortgage share (5 percentage points compared to a mean of 11 percent) yields just over a 2 percent increase in house prices, holding rents constant. As column 4 shows, this result is robust to including the other measures of the cost of credit, increasing in magnitude by half when these costs are added. However, when we include the subprime share, the other lending variables appear to matter much less in explaining the price-rent ratio, as can be seen by

comparing columns 3 and 4. Since subprime mortgages often involve adjustable-rate features and high LTV ratios, it is not surprising that the inclusion of a control for subprime lending reduces the magnitude of the coefficients on these other lending variables.

One might be somewhat skeptical of using changes in the subprime share of mortgages over time to help identify the relationship between the subprime share of mortgages and the price-rent ratio. Since both the price-rent ratio and subprime share were trending upwards between 2000 and 2005, one cannot be sure if the price-rent ratio rose because of lenders taking on more risk through the extension of subprime loans or if the correlation is spurious. In the last column of table 4, we add year fixed effects to address this issue. The year effects control for any national trends in the price-rent ratio and subprime share of mortgages. Thus the estimated coefficient on the subprime portion is identified by whether a given MSA's price-rent ratio grows faster than the national average when the share of subprime mortgages in that MSA grows faster than the national average. Similarly, the user cost coefficient is identified by whether MSAs with user costs that decline more than the national average in a given year have price-rent ratios that increase more than the average for that year.

In this specification, percent changes in user cost, with an estimated coefficient of 0.90 (standard error of 0.22), have an almost one-for-one effect on the price-rent ratio. The increase in the size of the user cost coefficient in this specification relative to that in the previous column suggests that aggregate time-series factors may actually obscure the relationship between user cost and the price-rent ratio during this period, possibly due to omitted time-varying risk effects or other macroeconomic time-series variables. The estimated effect of the subprime share actually rises by a fourfold increase when we restrict our focus to variation within a given MSA over time. The resulting coefficient of 1.54 (standard error of 0.22) implies that a 5 percentage point increase in the subprime share is correlated with a 10 percent excess increase in the price-rent ratio. The other credit market variables are no longer statistically significant. These results suggest that subprime lending is related to excess growth in price-rent ratios in recent years and are similar in spirit to the findings in prior research (see Pavlov and Wachter forthcoming).

Behavioral Explanations for Housing Booms: Backward-Looking Expectations

Collectively, the user cost of capital and credit market variables explain a great deal of the within-MSA variation in the price-rent ratio from 2000 to 2005: 92 percent without including year dummies and 94 percent with this inclusion. In addition, the estimated coefficient on user cost is very close to one, suggesting that the housing market was priced rationally given the state of the capital markets. But this result leads one to ask: was the capital that flowed to the housing market motivated by some behavioral response, as suggested by Shiller (2007), even if purchasers priced the housing asset correctly?

Discussing the behavioral motivations for excessive lending or insufficient risk aversion on the part of lenders is beyond the scope of this paper, but at least we can examine whether an increase in subprime mortgage lending followed growth in housing prices. In table 5, we regress the subprime share on recent house price growth rates: the average house price appreciation rate between six years and one year prior to the current year and the house price growth rate between two and one years prior to the

current year.⁸ Since the regressions contain MSA fixed effects, the identification comes from within-MSA changes in subprime lending relative to the MSA sample period average. The first three columns of table 5 show that higher past five-year lagged appreciation rates are associated with a much higher share of subprime loans. The coefficient on the lagged five-year growth rate in column 3 shows that a 1 percentage point increase in house prices leads to a 1.29 percentage point greater subprime share of mortgages. However, the most recent year's appreciation rate in house prices has little predictive power for the growth of subprime loans; if anything, conditional on the five-year lagged growth rate in house prices, subprime lending is slightly lower in markets that experienced high housing price growth over the prior year.

When we include year dummies in the last three columns of table 5, we see that increases in lagged five-year house price growth are still associated with bigger-than-average increases in the subprime share of mortgages. However, the magnitude of the effect is about 60 percent as big as without the year fixed effects, with an estimated coefficient on the five-year average prior house price growth ranging from 0.67 to 0.71 with very low standard errors. These results suggest that lenders may have lent more aggressively in markets with high rates of medium-term (five-year) house price growth. The fact that the last year's price growth in the housing market is unrelated to the share of subprime mortgages is evidence against the view that increases in house prices spur rapid expansions of subprime lending, thus causing house prices to quickly spike.

Another way in which behavioral factors can affect the housing market is through the formation of expectations about house price growth by home buyers and sellers, as suggested by Case and Shiller (1988, 1989, 2003), Shiller (2007), and others. We consider two simple backward-looking rules for forming expectations: future house price growth is expected to be the average of the last five years' appreciation in housing prices and future house price growth is expected to be the same as last year's increase. While these are particularly naïve rules of thumb, we have no theory that would give more precise guidance.⁹

As reported in the first column of table 6 and predicted by the behavioral conjectures, the lagged five-year average of house price growth is positively associated with increases in the price-rent ratio. The individual coefficient on the lagged five-year growth rate is highly statistically signif-

Table 5
The Growth in Subprime Mortgages versus the Growth in Housing Prices, 2000–2005

	Subprime Subsample					
	2000– 2005	2000– 2005	2000– 2005	2000– 2005	2000– 2005	2000– 2005
Lagged five-year growth rate from years -6 to -1	1.24 (0.11)		1.29 (0.11)	0.67 (0.06)		0.71 (0.06)
Lagged one-year growth rate from years -2 to -1		0.16 (0.09)	-0.10 (0.07)		0.04 (0.04)	-0.07 (0.03)
R ²	0.50	0.21	0.50	0.92	0.87	0.92
# Obs	258	258	258	258	258	258
Year fixed effects	No	No	No	Yes	Yes	Yes
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Source: Authors' calculations

Table 6
Inflation Expectations and Expected House Price Appreciation, 1984–2006

	Mortgage Interest Rate Survey Subsample						Subprime Subsample								
	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006	1984– 2006
Ln(1/user cost)	0.33 (0.05)	0.38 (0.06)	0.33 (0.05)	0.43 (0.05)	-0.00 (0.05)	0.76 (0.06)	0.63 (0.06)	0.57 (0.06)	0.97 (0.17)						
Lagged five-year growth rate	2.17 (0.13)		2.20 (0.14)	1.99 (0.13)	1.57 (0.12)	2.08 (0.19)	2.35 (0.23)	2.16 (0.24)	1.80 (0.28)						
Lagged one-year growth rate		0.36 (0.12)	-0.07 (0.10)	-0.01 (0.10)	-0.14 (0.07)	-0.03 (0.13)	0.21 (0.10)	0.15 (0.10)	0.19 (0.09)						
Inflation rate				2.12 (0.31)	0.57 (0.33)	2.49 (0.31)	-0.19 (0.20)	-0.34 (0.21)							
%ARMs	0.09 (0.04)	0.17 (0.05)	0.10 (0.04)	0.08 (0.04)	-0.03 (0.03)	0.02 (0.05)	0.11 (0.04)	0.05 (0.04)	0.07 (0.05)						
Points and fees	-0.05 (0.01)	-0.06 (0.01)	-0.05 (0.01)	-0.05 (0.01)	-0.02 (0.01)	-0.08 (0.02)	0.01 (0.03)	-0.01 (0.03)	-0.01 (0.03)						
%Subprime															
R ²	0.55	0.31	0.55	0.59	0.85	0.82	0.95	0.96	0.64 (0.22)						
# Obs	520	520	520	520	234	286	130	130	130						
Year fixed effects	No	No	No	No	No	No	No	No	Yes						
MSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes						

Source: Authors' calculations

icant and increases the explanatory power of the regression appreciably.¹⁰ When the lagged five-year average house price growth rate is above the MSA average, the price-rent ratio for that MSA is also above its average. In particular, the estimated coefficient in column 1 suggests that a one standard deviation change in the lagged growth rate of 3 percentage points is associated with more than a 6 percent increase in the price-rent ratio.

By contrast, the prior year's house price growth rate has little effect on the price-rent ratio (column 2) and what effect it does have is subsumed by the five-year average lagged growth rate (column 3). Neither lagged growth rate affects the estimated coefficient on the user cost of capital, which remains between 0.33 and 0.38, very close to the estimate in the fifth column of table 3. This result is inconsistent with the most behaviorally influenced conjecture, which holds that households set expected house growth rates based on very recent changes in house prices.¹¹

Of course, backward-looking expectations are not necessarily based on behavioral factors: instead, households might rationally incorporate lagged five-year price growth when predicting future house price growth, especially if there is serial correlation in underlying demand growth.¹² Indeed, all one can say with certainty is that house price growth expectations appear to be dynamic since, to the degree that households across different MSAs hold different static expectations about future price growth, these varying price growth expectations are absorbed by the MSA fixed effect. Thus the large and statistically significant coefficient on past house price growth indicates that changes in expected capital gains are correlated with the price-rent ratio.¹³ Even so, the effect of recent house price growth on current price-rent ratios is certainly suggestive of a behavioral component. More work needs to be done so we can better understand how households set their expectations of future price growth and how those expectations are capitalized into prices.

Inflation Illusion

Finally, we examine the evidence on whether households are subject to inflation illusion, meaning that they confuse nominal interest rates with real ones, as has been suggested by Brunnermeier and Juillard (2008). To see if inflation illusion has an effect on expected future house prices,

we add a measure of inflation to the regression. The results showing that higher inflation is correlated with a higher price-rent ratio are reported in the fourth column of table 6. The estimated coefficient of 2.13 (with a standard error of 0.31) suggests that a 1 percentage point higher inflation rate (the mean is 0.03) is correlated with a 2 percent higher price-rent ratio. This is actually the opposite result that one would expect given the results in Brunnermeier and Julliard (2008). Those authors argue that when actual inflation falls, households think that the cost of capital (the mortgage interest rate) is lower even as expected house price appreciation has not changed. If lower inflation made housing appear relatively inexpensive in recent years, the price-rent ratio should have increased, not fallen.¹⁴

Note that the user cost model predicts that higher *expected* inflation should raise house prices as increases in *expected* inflation raise the value of the nominal mortgage interest deduction. However, with the expected inflation rate already incorporated in the user cost of capital and the relationship between actual and expected inflation unclear, it is quite possible that the positive and significant coefficient on inflation may be due to measurement error in the user cost or in expected inflation. In addition, as discussed above, it is difficult to accurately compute the value of the tax deduction for nominal interest payments since many households do not itemize deductions when filing their taxes.

In table 6, the fifth and sixth columns return to the notion that the 1980s boom in house prices was perhaps more behaviorally driven than the housing boom in the 2000s. Between 1984 and 1994 the user cost of capital had no effect—and credit market conditions had almost no effect—on the price-rent ratio once one controls for lagged house price growth and inflation, and even those variables had a relatively small impact on the price-rent ratio during that period. But in the 1995 through 2006 period, the user cost coefficient increased to 0.76, which is much closer to its theoretical value of 1.00. Lagged house price growth also had a larger effect, with an estimated coefficient of 2.08. To give a sense of magnitudes in column six, a within-MSA one standard deviation decrease in $\ln(1/\text{user cost})$ of about 15 percent would lead to an 11.4 percent increase in $\ln(P/R)$. By contrast, a within-MSA one standard deviation increase in lagged house price growth (3 percentage points) would lead to a 6 percent increase in the price-rent ratio. So a one standard

deviation change in the user cost has about twice as large an effect on $\ln(P/R)$ as a one standard deviation change in lagged five-year house price appreciation.

We finish by revisiting the recent boom years of 2000–2005 and the impact subprime mortgages may have had on this run-up in house prices. The seventh column shows that the coefficients estimated over the 2000–2005 period look very similar to those estimated during 1995–2006, except that the coefficient on the inflation rate switches signs and is no longer statistically significant from zero. In the eighth column we add the subprime share and see, once again, that the subprime share is strongly correlated with higher price-rent multiples. With a coefficient of 0.32, a one standard deviation increase in the within-MSA subprime share (4 percentage points) is associated with a 1.3 percent increase in the price-rent ratio. The last column of table 6 incorporates year dummies using just the variation within a given MSA over time to identify the coefficients. The estimated coefficient on user cost, 0.97, is quite close to unity. The coefficient on the five-year lagged appreciation rate is little changed. This specification suggests that in the latest time period, a one standard deviation change in the user cost of capital has almost three times the impact on $\ln(P/R)$ as a one standard deviation change in lagged five-year house price appreciation, and almost six times as much explanatory power as is accounted for by a one standard deviation change in the percent of subprime mortgages.

5. Conclusions

Our results suggest that both fundamental (meaning rational) and seemingly behavioral factors play an important role in explaining changes in the price-rent ratio across U.S. metropolitan areas since 1984. We began by estimating a standard user cost model with long-term interest rates and expected house price appreciation equal to its postwar average. We then included other independent variables to control for measurement error and omissions in the standard user cost model. Finally, we added proxies for behavioral explanations of house price growth, including backward-looking expectations and inflation illusion.

The standard model matched changes across MSAs in house price appreciation after 1994 almost one-for-one, but did a poor job describ-

ing the period between 1984 and 1994. Backward-looking expectations, in the form of five-year lagged appreciation rates, were the only factor to have any sizable correlation with movements in the price-rent ratio between 1984 and 1994, but changes in the user cost of capital appeared to have a larger effect on the price-rent ratio in the 1995–2006 period than did the lagged five-year appreciation rate. Mortgage market factors, especially the growing use of subprime mortgages and the decline in lending costs, also help explain an additional portion of the variation in price-rent ratios in the latter part of the 1995–2006 period.

The results present a mixed bag when interpreting the magnitude of rational and behavioral effects in explaining house price movements. Fundamentals seem to be important—but only in the 1995–2006 boom. Coefficients on the two most striking behavioral variables, the inflation rate (inflation illusion) and one-year backward-looking expectations, were the wrong sign in nearly all specifications and these variables displayed little explanatory power. However, medium-term, backward-looking expectations (five-year lagged appreciation rate) are quite important in explaining within-MSA variations in price-rent ratios and are also correlated with the increased use of subprime mortgages. Overall, these results suggest that the house price boom in the 1980s was more of a behavioral bubble than the boom in the 2000s, where fundamentals dominated in importance but backward-looking expectations continued to play a sizable role in influencing market behavior. Still, there is appreciable scope for additional work exploring how households set their expectations and how lenders determine their lending standards. Without a formal model of expectation-setting for households and lenders, it is nearly impossible to determine the extent to which households and lenders are rationally updating their beliefs about future house price appreciation or are getting caught up in a “zeitgeist” that “is at least in part the result of a social epidemic of optimism for real estate” (Shiller 2007, 96–97).

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Notes

1. Smith and Smith (2006) analyzed a sample of single-family rental units, so that prices and rents were closely matched. However, their estimation procedure did not incorporate differential house price appreciation rates across metropolitan areas and their limited sample appears not to be fully representative of the market. The paper concluded that, based on fundamentals, house prices in some California cities were quite low in 2005.
2. See Gallin (2006) who examined the relationship between prices and rents.
3. Also, an increase in mortgage market efficiency that allows mortgages to be more cheaply originated might be capitalized in higher house prices.
4. We include log P/R and log user cost in equation (3) to address an additional problem that is described further in the data section. Our measure of P/R is not comparable across cities and requires that we factor out a multiplicative error term.
5. More detail on the data used in this paper, as well as updated web links to our sources, can be obtained from the website, <http://www0.gsb.columbia.edu/realestate/research/housingcost> or in most cases from Himmelberg, Mayer, and Sinai (2005).
6. We remove the multiplicative error by taking logs of both sides, regressing $\ln(P/R)$ on $\ln(1/\text{user cost})$ and MSA fixed effects (to pick up the multiplicative scaling factor), plus other covariates. Thus, we can use only within-MSA variation to identify the various parameters of interest.
7. The average user cost over this sample period is 0.06, from table 1. A 10 percent decline would yield a user cost of 0.054. In that case, $1/UC$ would rise from 16.6667 to 18.5185, an 11 percent increase. Multiplying that 11 percent by 0.48 gives a 5.33 percent rise in house prices.
8. We measure the growth rate up through the start of the prior year rather than the current year to avoid a contemporaneous measurement of subprime market share and house price growth. We obtain similar results if we measure price appreciation through the current year.
9. In addition, the five-year-average fits the data better than other approaches, such as overweighting more recent years or estimating an autoregressive price growth process, as in Campbell et al. (2007). Ideally, we would have some measure of peoples' actual house price expectations but we are not aware of any source that collects such data for a wide variety of cities.
10. We exclude the LTV ratio since it appears not to reflect the true degree of leverage. Our conclusions are unchanged even if we include it.
11. This result is not surprising. If very short-run price increases had large impacts on expectations, we would see more bubbles of the form seen in Vancouver in the early 1980s, which was characterized by a quick spike and decline in house prices.

12. For example, Gyourko, Mayer, and Sinai (2006) show that the price-rent ratio falls as long-run price growth increases, at least using decadal data.

13. This result is consistent with the last table from Sinai and Souleles (2005) which shows that that markets with higher historical house price growth have higher price-rent ratios and those price-rent ratios expand when past house price growth rises, holding the metropolitan area constant.

14. One potential reason for the differences between our findings and theirs is that we use a panel with variation in prices and rents across metropolitan areas, while Brunnermeier and Julliard (2008) estimate their model using only national aggregate data. On the other hand, Brunnermeier and Julliard have a more complete dynamic model of price determination, albeit one that abstracts from features like tax advantages accruing to owner-occupied housing.

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Appendix Table 1

REIS/MIRS MSAs	REIS ONLY MSAs
Austin	Atlanta
Charlotte	Baltimore
Cincinnati	Boston
District of Columbia	Chicago
Fort Lauderdale	Cleveland
Fort Worth	Columbus
Jacksonville	Dallas
Memphis	Denver
Nashville	Detroit
Oakland	Houston
Orange County	Indianapolis
Orlando	Kansas City
Richmond	Los Angeles
Sacramento	Miami
San Antonio	Milwaukee
San Bernadino-Riverside	Minneapolis
San Jose	New York
	Philadelphia
	Phoenix
	Pittsburgh
	Portland
	San Diego
	San Francisco
	Seattle
	St. Louis
	Tampa

Comments¹ on “U.S. House Price Dynamics and Behavioral Finance” by Christopher J. Mayer and Todd Sinai

Andrew Caplin

There are many who tie problems in the subprime mortgage market to topics typically covered under the rubric of behavioral economics. It is widely asserted that naïve borrowers did not understand loan terms, and that regulators should intervene to protect those whose bounded ability is so clearly revealed by the episode. In these comments I note that the inadequacy of consumer understanding is as nothing next to that of the regulators and academic economists. I outline how the behavioral limitations of these two groups have contributed to the subprime crisis, and offer suggestions on how future crises of this kind might be avoided.

Consider an obvious asymmetry between options open to households as opposed to corporations when each entity encounters repayment difficulties. When a debt-financed corporation is at risk of default, all forms of deal are open for cases in which replacing managers and/or scrapping the enterprise would be inefficient. For example, an equity investor may find it worthwhile to “take out” the debtors in exchange for some portion of the continuing value of the operation. A similar renegotiation involving equity may make sense for many homeowners, who can be seen as proprietors of small businesses. There are many cases in which the efficient option would be to leave the current owner in place to avoid a fire sale. After all, where is one to find a better-off pool of replacement managers for properties in areas with significant short-term economic stress? This suggests that opening up equity options in mortgage negotiations would prevent many defaults, without choking off long-term capital to less well-off lenders. Arguments on the social value of markets in housing equity are of longstanding (Caplin, Chan, Freeman, and Tracy

1997; Caplin, Carr, Pollock, and Tong 2007). Personal experience suggests that arguments on social value are insufficient to convince regulators to change their ways.

The first equity sharing mortgage to be considered, the shared appreciation mortgage (SAM), was initially proposed in the 1970s to reduce the very high interest payments caused by the inflationary interest rates. At that time, the Internal Revenue Service (IRS) was called in to rule on whether a specific SAM created joint ownership. In response to a request for a ruling as to the “federal income tax consequences to a mortgagor under a shared appreciation mortgage loan used to finance the purchase of a personal residence,” the IRS issued Revenue Ruling 83-51. While it was ruled that regular interest payments during the life of the loan and final payments of contingent interest could be deducted for tax purposes, the ruling was limited to “the fact situations set forth above,” which included a detailed description of the mortgage in question. At one and the same time, the IRS moved SAMs onto the “No Advance Rulings” list. This effectively chilled the market, as noted by the California Housing Finance Agency (2002):

One of the primary attributes of ownership under the federal tax rules is the right to benefit from appreciation in the value of the property. Where that right is shared by the title owner with another party, the Internal Revenue Service has only grudgingly (and in relatively few instances) concluded that the title owner is the owner for federal tax purposes. For example, Revenue Ruling 83-51 concludes that under very restricted circumstances, a shared-appreciation mortgage loan used to finance the purchase of a personal residence results in a debtor-creditor relationship (rather than a joint venture or other joint ownership arrangement). Since the publication of that ruling, the Internal Revenue Service has announced that the shared-appreciation area is one in which they will not issue rulings or determination letters. See, e.g. Revenue Procedures 88-3 (4).

The reason that this policy was of interest to the California Housing Finance Agency is that it indirectly blocked efforts to help with housing affordability on the lower end of the market; in particular, a scheme developed by National Ecumenical Homebuilders (NEH). In ultimately developing an affordability scheme that was acceptable to the IRS, the NEH ran into a second set of obstacles to market development, relating to the securities laws. Evidently efforts to allow sharing of equity for such

socially-minded purposes as increasing the rate of homeownership have, to date, fallen on deaf ears.

Another example of the regulatory morass confronting those seeking to rationally amend mortgage design strikes even closer to home. I participated in a project to offer some form of index-based house price insurance in Syracuse, New York (Caplin et al. 2003). Along the way, we found out that insurance regulations would preclude such an offering. So we proposed developing a mortgage in which the balance due fell with the value of local housing. The existence of such a mortgage would clearly have been very beneficial in terms of the current market problems. Unfortunately we were informed that such a mortgage would be judged in New York State to be a “price-level-adjusted mortgage” (PLAM). These had been banned in New York State some 20 years earlier, a decision that is as hard to change as it is justify. To complete the circle, such mortgages were initially proposed by Franco Modigliani in the 1970s, and the lack of receptivity to this idea is, to my knowledge, the first known important example of the tangle that is the U.S. regulatory system. The PLAM is also the precursor to the SAM, which was initially designed precisely to overcome regulatory resistance to the PLAM!

In the United States, promoting the private sector’s interest in these innovative mortgages may be necessary to overcome such regulatory hurdles, a component which was missing in the above cases. Yet such interest has had little impact on the regulatory framework, as evidenced in 2000 by a failed effort to reintroduce SAMs in the United States with Bear Stearns involved as the securitizer. Given the IRS rulings, the supporting consumer brochure stated that: “The application of the federal income tax rules to a SAM is both uncertain and complicated, and the rules will affect each borrower differently. Accordingly, you must talk to your tax advisor about the federal income tax consequences to you of borrowing under a SAM” (National Commerce Bank Services 2000). Not surprisingly, there were few takers, and those who had pioneered the market development of SAMs were soon working elsewhere. Guess where the creative energies of those in the business of securitizing mortgages went next? To those who are currently looking to justify additional regulations with the refrain “look where all this novelty got us,” the appropriate

response is that the creative effort was misdirected precisely because of poor regulations.

The case of equity sharing is not an isolated example of regulatory and institutional inertia, as revealed by the case of reverse mortgages (Caplin 2002). In 1978 Ken Scholen founded the National Center for Home Equity Conversion in an effort to stimulate development of these markets. The private sector caught on slowly to this idea, and in 1992, armed with qualified applicants and properties, Providential raised \$65 million for reverse mortgage finance in an oversubscribed public offering. A short while later the Securities and Exchange Commission announced an investigation into the company's accounting practices, and then ruled that Providential should not assume any future changes in property value when projecting cash flows. You might ask why, but that appears to be beside the point. Realizing that the legal and regulatory challenges were overwhelming, private capital fled the market.

In an effort to move market development forward, Congress authorized the Home Equity Conversion Mortgage (HECM) as a pilot program in 1989 for the Department of Housing and Urban Development (HUD). Yet in its initial report to Congress, HUD itself was very concerned with legal issues at the state level. In its follow-up report, it noted that some progress has been made, but that there was considerable uncertainty concerning enforcement of the HECM as a first mortgage:

The laws in some states are not clear regarding the lien priority to be granted to loan advances made over an extended number of years under a mortgage that was recorded as a first mortgage. HUD has attempted to ensure that all HECM loan advances will be regarded under state law as mandatory or obligatory advances that, under the laws prevailing in most states, would also have a first lien priority, but there remains some legal risk in some states (HUD 1995, 5–13).

These state-by-state discrepancies are far from the end of the regulatory problem. The Federal Reserve Board considers a reverse mortgage to be an “open-end consumer credit plan under which extensions of credit are secured by a consumer's principal dwelling.” In a Kafkaesque twist, the Truth in Lending Act requires the lender to lie by stating that “loss of dwelling may occur in the event of default.” In truth, the household only stands to lose the property if it fails to pay taxes, fails to keep the prop-

erty in good repair, or otherwise endangers the lender's security interest in the property.

What about the tax treatment of reverse mortgages? Borrowers must sign a certificate disclosing that a HECM may have tax consequences, affect their eligibility for assistance under federal and state programs, and impact on the estate and heirs of the borrower. One open question in this regard concerns the potential taxability of the proceeds of the reverse mortgage. A second concern is the possibility of a phantom gain that may occur when an elderly household sells the home for a handsome capital gain, but at a time when the loan has grown to be even larger than the property's sale amount. It has even been conjectured that the IRS will ultimately rule that reverse mortgages are really sales rather than loans, which would have a disastrous impact on the financial positions of the supposed owners. The situation with respect to benefits is almost equally unclear. In the federal Supplemental Security Income (SSI) program, a loan advance cannot affect your SSI benefits if you spend the loan advance in the calendar month in which you receive it. But if your total liquid assets at the end of any month are above very low limits, eligibility is lost. In addition, the money you get from an annuity can reduce your SSI benefits dollar-for-dollar or make you ineligible for Medicaid.

Taking stock, it is almost as if most of the major U.S. institutions have been constructed to preserve an archaically structured housing finance market. The fiscal, legal, and regulatory systems are incomplete, leaving participants uncertain on how to design new instruments. Rather than being prepared for all future contingencies, regulators have implicitly blocked the development of new products by leaving important questions unanswered. The incompleteness of these systems exposes innovative producers and consumers to widespread risk. The Internet would be but a dream had computer technology been subject to such reactionary oversight.

Given how blatantly disconnected it has become from economic rationality, an intriguing question is why the regulatory system (broadly construed) has not been subjected to a “behavioral” analysis. I believe this to be because we have no first-order theory of what motivates regulators. Neglecting regulatory behavior because we have no idea what drives it,

when viewed from the social standpoint, represents a very poor allocation of academic attention.

A similar issue of academic priorities shows up in the area of house prices. Academics have managed to predict not only six of the last three recessions, but ten of the last two housing bubbles, which gives a true reflection of the state of our knowledge regarding house prices. There have been few strong findings, and the housing indexes that are relied on are poorly measured and contain errors that may contribute systematically to the pattern of returns that are found in the data (Meese and Wallace 1991). Economic models of house price dynamics remain rudimentary, making it hard even to understand the extent to which observed differences in housing returns across locations were ex ante predictable.

Ironically, I believe that our ignorance concerning house price dynamics has played an integral part in the subprime crisis. Implicitly, those who lent with subprime mortgages were issuing equity in down markets. The crash is in part a sudden recognition that the return properties of these assets are little understood, even by leading academics.

More broadly, I believe that academics understate their importance in contributing to market innovation and to policy. If we could take a big lead in developing the relevant knowledge base, then we could actually play a role in promoting and developing socially beneficial financial instruments. We are currently part of the problem, and it is past time for us to become part of the solution. A significant change in academic priorities is second only to regulatory revamping in terms of the potential to improve real-world outcomes. We are in the best position to help overcome the many chicken and egg problems that underlie the failure of potentially beneficial markets to develop. Our research priorities end up impacting the world, and changes in our behavior have the potential to feed through in a beneficial loop to promoting better real-world outcomes.

Note

1. These comments represent my earliest efforts to suggest appreciation sharing as a necessary part of any solution to the subprime tragedy. Since delivering these comments in September 2007, my co-authors and I have presented the ideas in increasingly refined form that has appeared in various outlets (for example, Cap-

lin, Cunningham, and Engler 2008; Caplin, Cunningham, Engler, and Pollock 2008; Caplin, Cooley, Cunningham, and Engler 2008). More than one year after this initial statement, the good news is that this proposal is being taken increasingly seriously by policymakers. In particular, William Hombrecht has proposed a similar plan that is getting attention (Nocera 2008). The bad news is that the two theses advanced in this comment concerning the behavioral limits of regulators and of academic economists have been confirmed. In the face of rising foreclosures and loan losses, regulators continue to search for short-run fixes rather than building policies that will promote the nation's long-run economic welfare. Academic economists, meanwhile, have added few original notes to the policy debate. Moreover, contra Zingales (2008), local (zip code) house price indexes remain highly unreliable. It is alarming, and in some ways tragic, that real estate and real estate finance remain such understudied areas of economics when the consequences of such relative inattention prove so severe.

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Comments on "U.S. House Price Dynamics and Behavioral Finance" by Christopher J. Mayer and Todd Sinai

Robert J. Shiller

Mayer and Sinai begin their paper with the disarming assertion: "The question of whether psychology matters in the housing market has been settled long ago: the answer is yes." I suppose we could get agreement on this claim among real estate economists, but this is a very weak statement of how most economists view the housing market. While efficient markets theorists have always acknowledged that markets are not *perfectly* efficient, I think that there is still a strong tendency in the profession for many economists to describe the housing market entirely in rational terms. Psychological factors are still difficult for most economists to incorporate into their thinking. I think that this bias towards describing people as almost perfectly rational has led many analysts astray in the past, and continues to do so now. For example, the current "subprime crisis," which has now developed into a global financial crisis of magnitude unseen since the Great Depression, appears to have been a surprise to most people, both within and without the economics profession.

I think that proper recognition of the role psychology plays in markets should have sounded a loud warning about the subprime crisis, and other such crises, in advance. In the second edition of my book *Irrational Exuberance*, published in early 2005, I sounded such a warning. Employing an analysis of the stock and housing markets based on behavioral finance I wrote:

Significant further rises in these markets could lead, eventually, to even more significant declines. The bad outcome could be that eventual declines would result in a substantial increase in the rate of personal bankruptcies, which could lead to a secondary string of bankruptcies of financial institutions as well. Another long-run consequence could be a decline in consumer and business confidence, and another, possibly worldwide, recession (xiii–xiv).

Very few people seemed to be warning of this possibility in 2004 and 2005, and I think that perhaps the reason is that they were not appreciative enough of the psychological underpinnings of the bubble in the housing market.

That is all the more reason why the Mayer and Sinai paper, which is a very useful overview of evidence about behavioral finance as it relates to housing, is really very important. Yet I find myself differing from their conclusions.

The paper offers some substantial and careful scholarship in analyzing the literature. One point that they make forcefully is that tests for serial correlation in prices are not necessarily tests of market efficiency. The authors assert that “Of course, serial correlation [of price] is not necessarily evidence of irrational markets if underlying rent growth is serially correlated.” This is absolutely right. But it would be an error to think that the high level of upward momentum that we have seen in the U.S. housing market from 2000 to 2006, with prices going up in double digits year after year in many cities, could be explained by the serial correlation of rents.

Attributing the serial correlation of house prices to the serial correlation of rental prices is attributing the bubble to something unmeasurable. Rents for single-family homes are indeed inherently hard to measure, since there is no regular rental market for conventional single-family homes. Indeed, the largest company in the United States that is in the business of renting out detached single-family homes, Redbrick Partners, has an inventory of only a couple thousand homes. Since they have avoided managing properties that are widely dispersed geographically, no aggregate measure approximating a national average for renting a single-family can be estimated from their data.

Rental properties are different from single-family homes and offer consumers different psychic benefits. Since there is no substantial rental market that captures all the varieties of single-family homes that are available, there is no arbitrage that would produce a market valuation on the fair market rental price of these homes. Each individual assigns a different psychic rental value for a given house.

If we were to explain the recent serial correlation of home prices by the serial correlation of rents, we would have to confront drastic differences through time in the price-rental ratio. The U.S. 10-City Composite Standard & Poor’s/Case-Shiller Home Price Index rose over 10 percent

a year from 1999 to 2006, and now is falling at 4.5 percent a year. For that rise and fall in housing prices to be justified by perfect knowledge of future changes in rents, there would have to be huge forecastable changes in rents. The price-rental ratio would have to be exceptionally low now to offset falling home prices on returns. But price-rental ratios are still at exceptionally high levels.

Todd Sinai and Nicholas Souleles wrote an important 2005 paper entitled “Owner-Occupied Housing as a Hedge against Rent Risk.” In it, they found that homeownership rates are higher in places where the rent risk is higher. Sinai and Souleles argue that people have a hedging interest in buying a home: a way of hedging home price rental risk is to purchase a house. The authors invite the reader to conclude that perhaps homeowners are eminently rational in their decisionmaking: buying a house locks in their housing services for their lifetime. But I am not convinced that homeowners are so rational in their behavior. Many homeowners approach retirement with little more assets in their name than their house, and that house saddled with debt. Hence many Americans only own a leveraged undiversified investment that is exposed to the economic risk of their city, and of their own job and employment prospects. Expenditures for housing (mainly owner-occupied and tenant-occupied nonfarm dwellings—space rent) amounted to only 15 percent of total consumption expenditures in 2007:Q3, according to the National Income and Product Accounts. Rent does not appear to be a highly important component of consumption. I believe that if we did a thorough analysis of the lifetime portfolio allocation problem that individuals face, taking into account all of their uncertainties, including human capital uncertainty, and taking into account all of the aspects of their consumption price risks, we would not find it optimal for people to hold these highly leveraged housing investments.

The regression results that Mayer and Sinai show are interesting, but do not provide decisive evidence about the efficiency of the housing market. They have a short sample period, 1984–2006, which has only one complete housing cycle (up from 1984 to a peak around 1990 to another bottom in the mid-1990s) and then half a cycle (from the bottom in the mid-1990s to the peak in 2006). Their R-squared on user cost alone is only 0.28. Adding in other variables, such as the percent of adjustable-rate mortgages (ARMs), the loan-to-value ratio, and the past growth

rates of housing prices, raises R-squared, but these variables sound like proxies for the boom.

Mayer and Sinai make some interesting arguments that recent increases in U.S. home prices have been driven by capital availability. But capital availability has been largely driven by the boom and is part and parcel of what constitutes boom psychology. Capital availability is not an exogenous factor unrelated to the boom mentality. The subprime mortgage market grew from practically nothing in 1995 to financing 20 percent of all U.S. mortgages issued in 2005. The ability of subprime mortgage lenders to obtain the capital to do this had something very much to do with the boom psychology. These lenders produced a new standard for subprime mortgages, a 2/28 ARM. But such a new standard would likely not have been so popular if home buyers didn't believe that the boom would continue for so many years that they would be able to make a nice profit on their investment, and that they could easily refinance after two years into a lower-interest-rate mortgage after their prepayment penalty period expired. The failure of the rating agencies to accurately foresee the problems of subprime mortgage securities was also related to their failure to fully understand the boom's fragile psychology.

I find a lot that is interesting in the Mayer and Sinai paper but nothing to change my general opinion about the causes underpinning the recent housing boom. The overriding fact about the recent housing situation is that people—financial professionals and the general public alike—were excessively optimistic about housing investments; this optimism was part of a social epidemic or bubble, and the psychology is rapidly souring at the present time. The idea that the housing market has not been deeply irrational is to a large extent what prevented us from taking actions that would have prevented the enormous financial crisis that began in late 2007 and continues today.

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6

Should Central Banks Maximize Happiness?

Happiness, Contentment, and Other Emotions for Central Banks

Rafael Di Tella and Robert MacCulloch

Doctors sometimes ask their patients questions such as, “does it hurt?” Upon hearing these words, reasonable patients do not throw a fit, accuse the doctor of unscientific reliance on interpersonal comparisons of pain, and leave the hospital in disappointment. Presumably, they think these questions help doctors do their jobs. In contrast, economists are suspicious of such questions. Welfare also occupies a central role in their profession, with most papers in economics making some reference to individual utility. However, in their applied work, measures of utility (or of the emotions that are related to utility) are not common. One reason is that economists think that utility can be inferred through actions. For example, if the patient buys a banana rather than an apple, when both are available at similar prices and conditions, we make the inference that the patient likes bananas more than apples. Economists say that preferences have been “revealed” to them. In contrast to standard economics, happiness research takes the position that such an indirect approach to measuring utility is not necessarily always superior to an approach based on direct measures of utility or, more precisely, direct measures of the emotions that are related to utility.

Several direct measures of these emotions can be constructed. One that appears promising and which has received some attention from economists is well-being data (sometimes loosely called “happiness data”). Examples include data on happiness (current mood), often captured by the answers to a simple survey question such as “Are you happy?” and data on contentment (a global judgment on how close we are to achieving “the good life”), often captured by the answers to a survey question such as “Overall, are you satisfied with your life?” Large datasets with well-

being measures, covering many countries and years, are widely available. Of course there are limitations to such data, so the question of how fruitful the approach is will typically depend on the context. In this paper we discuss some uses of well-being data for central banks.

Before continuing it is worth pointing out that there are (at least) two different broad interpretations of well-being data. To economists trained to focus on utility, the natural interpretation is that well-being data are a proxy for utility. Indeed, this is the interpretation we follow in this paper. On the other hand, to a psychologist who is trained to focus on a multiplicity of emotions, the data are likely to refer to specific positive emotions that are relevant to particular aspects of human existence, with no particular connection to an overall assessment of welfare such as utility.¹ Note that economists have suggested an approach which allows individuals to experience many different mental states (regret, anxiety, excitement, and so on) and relate them to a person's summary measure of utility (for discussions, see Elster and Loewenstein 1992; Caplin and Leahy 2004). In this study we focus on proxies for contentment, but note that these measures are just one possible instrument for central banks to use if interested in evaluating policy alternatives without the restrictions arising when welfare can only be evaluated through revealed preference.

The main objective of this study is to illustrate how direct data on emotions—in particular, data on contentment—can be used by central banks. The basic exercise involves the inflation-unemployment tradeoff, a ratio that is important in several macroeconomic models. Of course, a reasonable position is also to question several of the assumptions made in these models, so that a second focus of the paper is to use contentment data to explore the validity of these assumptions. For example, one could question the assumption that people care exclusively about money (and leisure).² Beyond its lack of plausibility, such an assumption forces economists to translate complex effects of changes in prices and business fluctuations into a monetary value. Or one could also question the standard assumption in macroeconomic models that consider the existence of only one type of (representative) agent. A third and final application where contentment data might be helpful is to verify some broad channels through which inflation is assumed to affect welfare.

In section 1 we introduce the larger issues by briefly describing the literature on the costs of macroeconomic fluctuations and the literature suggesting that well-being data can be interpreted as capturing (at least some component of) utility.

In section 2 we present the main exercise, which estimates the correlation between contentment and two basic macroeconomic variables, inflation and unemployment. In particular, we focus on data pertaining to overall satisfaction with life as our measure of positive emotions. Under some assumptions, the coefficients can be used to get one estimate of the welfare costs of inflation relative to those of unemployment. This simple exercise yields a different set of estimates than those typically used by economists analyzing the conduct of monetary policy (for example, see the numerical analysis in Woodford 2001, which draws on Rotemberg and Woodford 1997). This section discusses some possible interpretations of the basic results, both in terms of a narrow reading of the previous literature and the role played by behavioral channels. Section 2 also discusses some limitations that arise because we are unsure about the intertemporal nature of contentment data. Finally, it includes a discussion of the appropriate interpretation of our results when contentment is viewed as just one of the emotions that make up utility.

In section 3, we discuss some ways contentment data may be used to construct tests useful to those interested in understanding the channels through which macroeconomic fluctuations matter, including the available evidence on nonlinearities and adaptation. Section 4 explores the question of which emotion a central bank should target. Section 5 concludes.

1. Some Theory and Well-Being Data

Theoretical Costs of Macroeconomic Fluctuations

Economists have emphasized two important costs of inflation. First, inflation induces people to spend time and mental energy to save on holding money rather than on more productive uses. Second, when price adjustments are staggered, inflation induces spurious volatility in the prices some firms charge relative to others, reducing the price system's

ability to allocate resources efficiently.³ The first problem is typically seen as small (see, for example, Bailey 1956, Friedman 1969, and Lucas 2000) so this channel is unlikely to justify the observed preoccupation with keeping inflation low. The efforts to derive high costs of inflation are more successful in the approach followed by Bénabou and Gertner (1993) and Rotemberg and Woodford (1997), who focus on the second channel.

A similarly mixed picture emerges with respect to the costs of unemployment. Indeed, in spite of a long tradition studying aggregate economic fluctuations, there is disagreement among economists about the seriousness of their effects. In neoclassical economics, the welfare costs of recessions arise from the lost output that occurs when actual output falls below potential output. The welfare cost can be approximated by the area of a Harberger triangle, which is proportional to the square of the size of the gap. This approach is sometimes adopted by real business cycle theorists, who assume that individuals are optimizing and that recessions are desirable adjustments to productivity shocks. This means that the costs of business cycles are small—perhaps only 0.1 percent of total consumption in the United States.⁴ Even when market imperfections are introduced, the costs rise only by a factor of five, and these are significantly lower if borrowing is allowed. As downturns typically follow booms, business cycles do not affect the average level of economic activity. Consequently, these economists have turned their attention to economic growth and away from fluctuations (see Lucas 2003 for a discussion).

Given that one common approach to cooling down an overheated economy is to raise interest rates, which might increase the unemployment rate, there has been particular interest in deriving the welfare losses that arise from changes in the unemployment rate and the inflation rate in the same model so as to be able to compare these losses.⁵ This difficult task was undertaken by Rotemberg and Woodford (1997), who develop a model where structural relations are grounded in optimizing individual behavior and where firms must occasionally keep their prices fixed, resulting in substantial relative price distortions when inflation increases (more on this below).⁶ As discussed in Woodford (2001), their estimates for the United States imply a value for the costs of inflation relative to the output gap of the order of 20 times, when the gap is measured in

percentage points and inflation is measured at an annualized percentage rate. That is, in terms of social welfare the weight on inflation is 20 times the weight on the output gap.⁷ This is a much higher emphasis on inflation than in the literature on evaluating monetary policy, which often gives equal weight to inflation and output as stabilization objectives (for examples of such discussions, see Rudebusch and Svensson 1999; and Williams 2003). As Rotemberg and Woodford (1997) explain, one advantage of their approach is that:

Demanding that one's structural relations be derived from individual optimization also has the advantage that evidence from other sources about the nature of the problems that individuals face can be used to corroborate the quantitative specifications that are used to explain the relations among aggregate time series. Ultimately, this is the only way in which the "observational equivalence" of a multitude of alternative possible structural interpretations of the co-movements of aggregate series can be resolved (298).

Answers to direct questions about why inflation matters are one natural source to draw upon when studying the nature of the problems that individuals face.⁸ Interestingly, such answers point toward a completely different source of difficulties when inflation rises from those typically assumed in the economists' models. The survey evidence presented in Shiller (1997), for example, shows that when asked directly about inflation, individuals report a number of unconventional costs like exploitation, national prestige, or loss of morale. It is likely that the confusion with prices when inflation picks up makes the status quo in the income distribution harder to justify. For example, if relative price oscillations make speculation more profitable, then people will find it hard to claim that effort pays. This change in beliefs will particularly affect right-wingers (left-wingers already believe that luck, rather than effort, determines income).

Rotemberg (in this volume) discusses a range of evidence supporting the idea that there are behavioral costs of inflation related to factors like an individual's price knowledge and awareness, paying too much attention when facing a menu of price choices, and regret and anger about price changes. However, we have only a few models to interpret these empirical findings, with the exception of Rotemberg (2005) and, perhaps, adaptation of work in labor economics on the fair wage hypothesis

(see Akerlof and Yellen 1990). Research on these issues seems to be in its infancy, despite the enormous interest in behavioral economics and the central role of prices in the economics profession.

Similarly, there seems to be enormous potential for behavioral economics to improve our understanding of the potential costs of recessions. Substantial work in psychology and sociology indicate that there are emotional costs exacted upon those who lose their jobs that far exceed the monetary costs (see, for example, Clark and Oswald 1994; Winkelmann and Winkelmann 1998; Helliwell 2003; Blanchflower and Oswald 2004). This large loss is broadly comparable across many countries (see Di Tella, MacCulloch, and Oswald 2003). And there may be emotional costs from knowing that fellow humans are experiencing low utility, perhaps amplified by beliefs concerning the source of unemployment (for example, those who believe that unemployment follows from a lack of effort versus those who believe it follows from bad luck).⁹ Given that such beliefs differ across countries (for example, Alesina, Glaeser, and Sacerdote 2001 report that 60 percent of Americans—yet only 26 percent of Europeans—believe the poor are lazy as opposed to unlucky) the “costs” of unemployment will also differ. These differences will have consequences both for the “correct” response to inflation shocks (see the discussion in section 4) and to unemployment shocks in terms of the optimal amount of unemployment insurance (see, for example, Di Tella and MacCulloch 2006a).

Importantly, it seems that we are still quite far away from having estimates of the costs of inflation that are potentially useful in formulating monetary policy, both because there is little behavior-based research and because there is no easy way of deciding which of the many psychological costs that are theoretically plausible exist in practice, or what weight to give each one of them when an aggregate measure of welfare is derived.

Note that a behavior-based approach also introduces the difficulty that people often miscalculate utility (Gilbert et al. 1998). If this is a generalized phenomenon, calculating the welfare costs of particular events properly is going to be extremely difficult, in part because taking a position on whether there is a “right to be wrong” is controversial (for a discussion, see Oederberg 2000).

Data on Positive Emotions and the Connection to Utility

The direct questions about inflation that Shiller (1997) used are subject to potential criticisms. Diamond and Hausman (1994), for example, worry about strategic manipulation of the answers in contingent valuation studies of environmental costs that use a similar style of questions and believe that a lot depends on the subject’s ability to understand difficult issues (such as the workings of the economy or the state of the environment). An alternative to that approach is to ask subjects about a particular emotion—for example, how happy or satisfied they feel with their life—and then correlate the answers with the variables of interest (in our case, inflation and unemployment). This imposes fewer informational demands, as presumably it is easier to know how one feels than how the economy works.¹⁰

A natural reaction to data on well-being (and other emotions) is to dismiss them as hopelessly noisy. Thus, a first task for the approach we present is to establish some connection between the answers to happiness questionnaires and true utility. The general strategy used by researchers in the field is to correlate happiness and life satisfaction scores with some variable that we can plausibly claim is associated with what an economist would call “true utility.” Note that, traditionally, it has been quite hard to discern true utility accurately. For example, presumably the act of smiling reflects some positive emotion. Yet in some situations and cultures smiling occurs in settings that do not appear to involve high enjoyment or utility. In one famous experiment in psychology, Landis (1924) photographed students while they listened to music, looked at pornographic material, smelled ammonia or observed him decapitate a live rat. Third-party observers were unable to predict the activity by looking at the photographs.

However, more recent research shows that this inability results from a failure to distinguish between different types of smiles. Researchers in this field, particularly Paul Ekman, emphasize the distinction between the smile which mainly reveals teeth (the “Pan American smile” named after the famous American airline of the 1960s) and the Duchenne smile, a type of smiling that involves a muscle near the eye (called *orbicularis oculi, pars laterali*) which can indeed capture true enjoyment. Importantly for us, Duchenne smiles are correlated with self-reported happiness (Ekman,

Friesen, and O’Sullivan 1988; Ekman, Davidson and Friesen 1990). Happiness answers (and Duchenne smiles) are also correlated with left frontal brain activity, which in turn appears to be connected to different forms of what we are calling true utility. Davidson and Fox (1982), for example, show that 10-month old infants exhibit greater activation of the left frontal than the right frontal area of the brain in response to videotapes of an actress generating happy facial expressions. In contrast, asymmetry in other parts of the brain failed to discriminate between the conditions. See Urry et al. (2004) for more recent evidence on the neural correlates of well-being. Useful starting points in the literature on happiness include Diener et al. (1999) and Veenhoven (1993), as well as the recent reviews by Di Tella and MacCulloch (2006b) and Clark, Fritjers, and Shields (2008).

Another argument that has been made to justify a connection between happiness scores and utility is that cross-sectional and panel studies (some of them cited above) reveal that unemployed individuals tend to report low happiness scores. The connection occurs because we think that other adverse life events like divorce, addiction, depression, and violence are correlated with unemployment. Using large samples across many countries, Helliwell (2003) and Deaton (2007) find happiness measures to be positively related to variables that are expected to be associated with high utility like trust and income. Helliwell (2003) and Blanchflower and Oswald (2008) find a positive connection between happiness scores and good health. A related point is that “well-being equations” (where happiness and life satisfaction scores are correlated with the demographic characteristics of the respondents) are broadly similar across countries, an unlikely outcome if the data contained just noise (see, for example, Di Tella, MacCulloch, and Oswald 2003).

To be sure, there are findings in the literature that do not fit our standard economic models, including that conjoined twins are relatively happy, or that money doesn’t buy happiness in the long run (see, for example, Gilbert 2006 and Easterlin 1974).¹¹ Ultimately, happiness research takes the view that happiness and life satisfaction scores are related to true internal utility with some noise, but that the signal-to-noise ratio in the data is sufficiently high to make empirical research productive.

2. Contentment and Macroeconomic Fluctuations

The Contentment Costs of Inflation and Unemployment: Basic Estimates

Once the approach is accepted as potentially fruitful, we run a regression of the form:

$$(1) \text{ Life Satisfaction}_{n,t,j} = \alpha \text{ Unemployment}_{n,t} + \beta \text{ Inflation}_{n,t} + \delta \Omega_{n,t,j} + \gamma_n + \eta_t + \mu_{n,t,j}$$

where *Life Satisfaction*_{n,t,j} is our proxy for a component of utility of individual, *j*, living in nation, *n*, in year, *t*, derived from the survey question that asks, “*On the whole, are you satisfied with the life you lead?*” The four possible answers are “*not at all satisfied*,” “*not very satisfied*,” “*fairly satisfied*,” and “*very satisfied*.” It comes from the Eurobarometer survey series, it is a repeated cross-section, and this particular question is administered towards the early part of the questionnaire (for more description, see Di Tella, MacCulloch, and Oswald 2003). *Inflation*_{n,t} is measured by the rate of change in the Consumer Price Index.¹² The expression denoted by $\Omega_{n,t,j}$ is a vector of personal characteristics (for instance, employment status, including the categories of self-employed, retired, keeping home, or in school; income position; marital status; education; city size; gender; age; and age-squared) and potentially other macroeconomic controls (like GDP or hours). The expression γ_n denotes country fixed effects and η_t are year fixed effects. The error term is $\mu_{n,t,j}$. The standard economic interpretation (meaning one given by somebody who adheres to the assumption that a representative agent exists, that a summary measure of utility exists, and that agents only care about income) is that equation (1) is a reduced-form of a welfare loss function (whereby inflation and unemployment are assumed to affect utility only through their effect on income and possibly on future income).

Finally, several factors conspire against a full treatment of causality. The first is that this is a study about the left-side variable (an emotion). Thus, even if we use several pages to convince the readers that we have clever instruments, most of them will still be wondering what it is that we are estimating. Second, it is hard to think about instruments when the

theoretical literature has done so little to isolate convincing forces that reduce utility when there are macroeconomic fluctuations. Indeed, the most convincing effects involve behavioral costs that have not yet been fully modeled. Thus, specifying what are the omitted variables in equation (1) is a daunting task until macroeconomists produce better models of the costs of macroeconomic fluctuations (see also the discussion of the results in table 5 below). Third, we report some evidence concerning how unemployment arising due to plant closures in Germany is associated with drops in contentment. This evidence, while obviously incomplete for some of our purposes, at least confirms that there is a causal negative effect through which macroeconomic fluctuations affect positive emotions. Finally, we produce some tests that are identified within the context of the Rotemberg and Woodford (1997) model, although we are aware that reasonable people will perhaps see this as too narrow a test.

Column 1 in table 1 presents the results when equation (1) is estimated as an ordered probit and shows that the coefficients on unemployment and inflation are both negative and significant. Column 2 repeats the exercise controlling for country-specific time trends, finding similar results. They are similar to the estimates presented in previous work by Di Tella, MacCulloch, and Oswald (2001, 2003) and Wolfers (2003).

In order to see the size of the effect, note that a 10 percentage point increase in unemployment reduces average life satisfaction by 0.32 standard deviations. A 10 percentage point increase in inflation reduces average life satisfaction by 0.24 standard deviations. Figure 1 illustrates our results graphically. In the base scenario, the cut points leave 3.9 percent of the population in the lowest life satisfaction category, 12.7 percent in the second-to-last category, 55.5 percent in the next one up, and 27.9 percent in the top category. The first scenario shows that when unemployment increases 10 percentage points, the median person is as satisfied as the person at the 43rd percentile in the base scenario (when unemployment and inflation are at their average level in the sample). And when inflation increases by 10 percentage points, the median person is as satisfied as the person at the 45th percentile in the life satisfaction distribution in the base scenario. In an attempt to provide another metric for these changes, Wolfers (2003) focuses on the top categories. The standard deviations of

Table 1
How Life Satisfaction Scores Vary with Inflation and Unemployment in 16 OECD Countries, 1973 to 2002

Dependent Variable	Life Satisfaction	Life Satisfaction	Life Satisfaction	Life Satisfaction
<i>Macroeconomic Variables</i>				
<i>Unemployment Rate</i>	-1.6 (0.4)	-2.3 (0.6)	-1.2 (0.4)	-1.1 (0.4)
<i>Inflation Rate</i>	-1.2 (0.3)	-1.9 (0.5)	-1.9 (0.4)	-2.0 (0.4)
<i>GDP per Capita</i>			0.09 (0.02)	0.07 (0.04)
<i>Hours</i>				-0.03 (0.01)
<i>Personal Characteristics</i>	Yes	Yes	Yes	Yes
<i>Country and Year Dummies</i>	Yes	Yes	Yes	Yes
<i>Country-Specific Time Trends</i>	No	Yes	No	No
Unemployment Inflation	1.3 (0.4)	1.2 (0.4)	0.7 (0.2)	0.6 (0.2)
Number of Observations	609,243	609,243	607,467	607,467
Country-year clusters	309	309	306	306
Pseudo R ²	0.09	0.09	0.09	0.09

Source: Eurobarometer Survey Series (1973–2002)

Note: All regressions control for personal characteristics, including employment status (self-employed, retired, keeping house, or in school), income, marital status, education, gender, and age-squared. Ordered probit regressions with robust standard errors appear in parentheses, clustered at the country-year level. The regressions use as dependent variable the answer to the Eurobarometer question, “*On the whole, are you satisfied with the life you lead?*” The four possible answers are: “*not at all satisfied*”; “*not very satisfied*”; “*fairly satisfied*”; “*very satisfied*.” *GDP per capita* is real GDP per capita in the country, measured in U.S. dollars. *Hours* is the average weekly hours worked per capita.

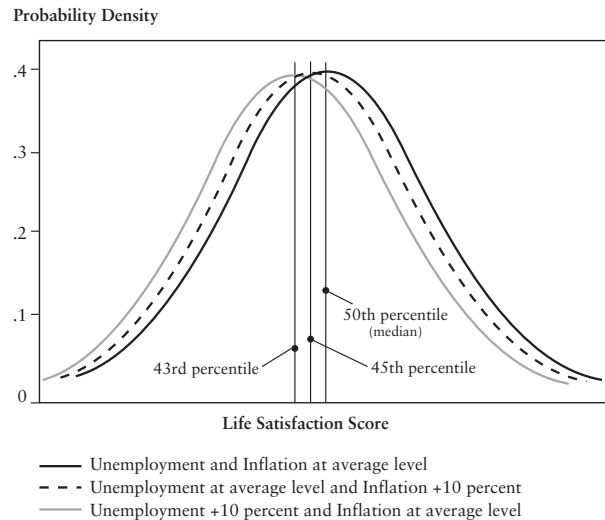


Figure 1
 Distribution of Life Satisfaction in Three Scenarios
Source: Authors' calculations.

unemployment and inflation for the Eurobarometer surveys are 0.035 and 0.045, respectively. Finally, it is worth noting that the 90-percent interval for the ratio of the coefficients on unemployment to inflation is 0.5 to 2.1, which implies a likelihood of more weight on unemployment than on inflation.

We can repeat the exercise with World Values Survey data (see Helliwell 2003). There are four waves and a larger sample of countries (a total of 145 country-year clusters), and a similar set of demographics available. The contentment data also come from a life satisfaction question, but with answers coded on a 1–10 scale, so that presumably fewer people are restricted by the limited number of categories available in the construction of the answer key (still, the top category has almost 14 percent of the sample).¹³ We group the answers into four categories that yield similar proportions to the Eurobarometer sample (although no substantive conclusion depends on this). The coefficient on inflation is

negative and significant, whereas that on unemployment is positive and marginally significant. The sample includes former socialist countries. When the inflation and unemployment coefficients are allowed to take a different value in the former socialist countries, the coefficient on inflation becomes more negative (almost three times in absolute value) and more precisely estimated, while that on unemployment becomes negative (but insignificant). Given that the sample includes countries with very unequal characteristics (different levels of income, of informal family insurance, and so on), it might be advisable to include the log of GDP as a control, which yields similar results. The standard deviations of unemployment and inflation for the World Values surveys are 0.06 and 1.00, respectively. Given this, once the actual variation in inflation is taken into account, the size of the inflation coefficient is comparable to the coefficient in equation (1).

In the European sample the estimate that a percentage point of unemployment causes at least as much discontent as a percentage point of inflation seems robust, although the precise multiple varies in different studies.¹⁴ Note that the coefficient on the unemployment rate in table 1 reflects how the *average* person changes their score when unemployment changes. But the average person is not unemployed. Since the contentment regression in table 1 also includes a control variable for whether each person is unemployed (in the set of personal characteristics), the coefficient on this variable measures the direct cost to those who become unemployed. Therefore to calculate the total cost of unemployment, the cost must be increased by adding the individual cost to the unemployed.

A Narrow View: Aggregating All Social Costs of Inflation and Unemployment

Adding up the total costs of inflation and unemployment (as outlined in section 1) can be quite difficult, especially when we know so little about them (particularly the psychic costs suggested by behavioral economics). One can take a narrow view and take the models developed in the previous literature literally. For example, consider the first paper to derive a social loss function with both inflation and the output gap, Rotemberg and Woodford (1997).¹⁵ They start by assuming a utility function with both consumption and leisure as explanatory variables.

Firms only occasionally get a chance to change their prices and staggered price adjustments lead to oscillations in relative prices. There are two costs of inflation. On the one hand, such oscillations result in direct misallocation of resources, reducing income (consumption). And because there are diminishing returns, the volatility in production means that productivity falls (so more labor input, meaning less leisure, is needed for the same output). This means that if we estimate a social loss function on the output gap (or unemployment) with the hope of capturing the costs of lower consumption, we are missing out on the possibility that sometimes the same level of consumption requires more labor input (due to higher inflation). Thus, the social loss function has the output gap and hours (or inflation) as arguments. A narrow prediction of this model is that, controlling for the output gap (proxied by the unemployment rate) and leisure (proxied by average weekly hours worked), inflation should have no effect on an instantaneous measure of welfare. A test of this is presented in columns 3 and 4 in table 1. It seems that inflation matters to people, even after controlling for the channels that are assumed in the Rotemberg and Woodford (1997) model.

Of course, one can take the plausible position that contentment somehow captures, at least in part, the future. In this case, inflation may enter because it is capturing future values of leisure. One could see if this is indeed the case by checking if future leisure is really predicted by inflation today, once current leisure, income, and unemployment are included (it is not). But perhaps the main point we are making is that simple tests relevant to central banks can be constructed with these data.

In brief, a narrow reading of the literature suggests that with several assumptions—including that a summary measure of utility exists, all channels through which inflation and unemployment matter can be reduced to consumption and leisure, and there is a representative agent—the social welfare loss function can be written as:

$$(2) \text{ Social Welfare Loss} = g(\text{Unemployment}, \text{Inflation}).$$

Conditional on accepting these assumptions, the coefficients in equation (1) provide a way to aggregate all of the relevant costs and benefits of macroeconomic fluctuations. In this view, the regression patterns detected

in equation (1) turn out—unknown to the respondents completing their contentment score sheets—to trace out a welfare loss function defined over inflation and unemployment as described in equation (2).

It is tempting to claim that, even when people mispredict utility (as in Gilbert et al. 1998) the coefficients in equation (1) adequately capture the costs of inflation and unemployment. Although it is possible that they capture all instantaneous costs, there is still the problem that macrofluctuations may affect planning, and hence future utility (and that these costs only register at a later date).¹⁶

Cardinal Interpretation

One straightforward interpretation of the coefficients is that they reveal that individuals find inflation and unemployment costly. This conclusion involves comparing contentment scores of different people and at different points in time. Reliance on the interpersonal comparability of contentment scores, however, is nonstandard for economists. As Hammond (1991) puts it:

Following [Lionel] Robbins, it became fashionable for economists to eschew ICUs [interpersonal comparisons of utility], apparently in an attempt to be scientific. ... And where interpersonal comparisons really have to be made, because the gainers from a change were not going to compensate the losers, the monetary comparisons that result from valuing all individuals' dollars equally still seem to be the most popular among economists, who then wonder why their policy advice does not receive wider acceptance (206).

One possibility is to follow standard practice in macroeconomics and assume that a representative agent exists, with the contentment measures as repeated attempts at getting a reading of his/her utility. Staying with this assumption would be convenient given that it is obviously quite difficult to interpret differences in contentment scores between just two individuals.¹⁷ Consider the question of the importance of income, and the finding that contentment is positively correlated with income in the cross-section. Since energetic/optimistic people tend to work hard (earning high income) and also tend to see the bright side of things, it is implausible that the error term is uncorrelated with income. Although one could theoretically calculate bounds (where exaggeration needs to fall in order to affect the qualitative conclusions) or even find ways to

control for exaggeration (maybe through questions concerning a fixed objective circumstance, as in some vignettes) this problem need not reduce central banks' interest in contentment data. The reason is that several interesting estimates in macroeconomics involve comparing large groups of people. Some of these exercises still require strong assumptions to allow interpretations but others do not. Specifically, in the finding that contentment scores are lower with high inflation, the unit is the country (in a particular year) and it is reasonable to assume that exaggeration and modesty have similar distributions across countries. Importantly, such distribution is unlikely to be correlated with the inflation rate.

Note also that even when cultural differences make the assumption of a similar distribution of "exaggerators" questionable, one could focus on changes over time within countries. Indeed, panel estimates like equation (1) have the advantage of correlating changes in life satisfaction reports with changes in the inflation rate. It is unlikely that countries enter into bouts of collective exaggeration, disconnected from hedonic fundamentals. Of course, booms have an element of collective euphoria. But this is typically genuine, meaning it is unlikely that it results in large groups of people ticking up their scores even when they themselves are not experiencing higher true utility. It is worth noting that another potential interest of equation (1) is the ratio, α/β . In this particular case, even when temporal swings in exaggeration divorced from hedonic fundamentals do take place, the ratio would be unaffected to the extent that these swings are uncorrelated with macroeconomic performance (and instead with variables like the weather) or are correlated in the same way with both of our indicators of macroeconomic performance (unemployment and inflation).

To test the validity of these assumptions it is possible to estimate regressions separately for different groups. For example, if left-leaning individuals use language differently than right-wingers it might be important to estimate these two groups separately. Conveniently, in the Eurobarometer Survey Series respondents are asked: "*In political matters, people talk of 'the left' and 'the right.' How would you place your own views on this scale [from 1 to 10]?*" In Di Tella and MacCulloch (2005), respondents were classified as being "left-wing" if their response was in categories 1 to 3 and as "right-wing" if they answered categories 8 to 10.¹⁸ The main exercise in that paper was to estimate the basic regression in equation (1) separately for the two subsamples. If left-wingers are assumed to use

language similarly, and right-wingers are also assumed to use language similarly, the α/β ratio in each regression does not have a problem of interpersonal comparability. A natural alternative is comparing poor and rich groups of individuals.

One prominent application in macroeconomics involves the comparison of the ratio, α/β , across different groups (for instance, left versus right, or poor versus rich), as in Alesina (1987). The estimation exercise in Di Tella and MacCulloch (2005) is extremely unlikely to be affected by temporal swings in exaggeration that cause some form of measurement error in the contentment data, since these swings would have to be correlated with unemployment and inflation differentially across the two groups in order to affect our results. (See also the discussion in section 3).

Finally, it is worth noting that some of the limitations in these data are not inherent problems with direct measures of utility or its components and that considerable progress could be made if some resources went into designing new measures. For example, Hsee and Tang (2007) have recently proposed asking about happiness in a way that fixes the extreme values at the end of the scales across people (by providing descriptions of the extreme values and intermediate points of reference). Another interesting possibility, particularly for economists, is described by Kahneman and Krueger (2006). They discuss how a focus on the proportion of time people spend in an unpleasant emotional state would allow us to construct an index that is based on an ordinal measure of feelings at the episode level that reduces the impact of individual variability in the use of scales. One reason that such a formulation is significant is because it shows that, in principle, it would be possible to derive direct measures of utility or its components without giving up too much in terms of strict assumptions about interpersonal comparability.

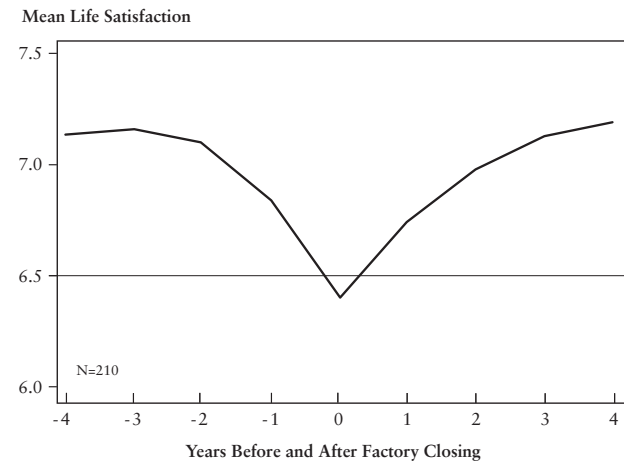
Time Horizons: Instantaneous versus Lifetime Effects

For the approach we are discussing, a serious difficulty for applications in macroeconomics is that ambiguity remains concerning the time horizon used by individuals in framing their answers to the life satisfaction question. When researchers have the ability to design the questions, they have opted to capture what economists would call instantaneous utility. Kahneman and Krueger (2006) have recently argued that well-being measures are best described as "a global retrospective judgment, which in

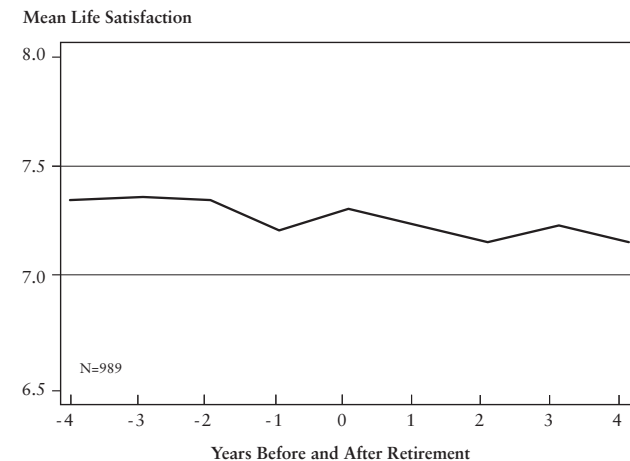
most cases is constructed only when asked and is determined in part by the respondent's current mood and memory, and by the immediate context" (6). They then described the famous dime experiment of Schwarz (1987), whereby subjects "accidentally" find a dime before filling out a life satisfaction questionnaire. The lucky half of the sample reported substantially higher levels of satisfaction with life.

On the other hand, one would expect that such small shocks can be treated as noise in regression analyses. And we know that contentment data react to other shocks in a way that is consistent with standard economic models. As an illustration, consider the life satisfaction response to two shocks that have been observed to have large impacts upon well-being in cross-sectional studies, namely unemployment (a negative correlation) and retirement (a positive correlation). Using the German Socioeconomic Panel we can follow a sample of West Germans before and after an unanticipated shock (unemployment arising from a plant closure) and the anticipated shock of retirement (see figure 2). As we are using a balanced panel the same people are being surveyed in the period before and after the shock has occurred, which means that some of those people who lost their jobs due to plant closure may subsequently be rehired while others may not. Note the large, but temporary, satisfaction drop associated with the plant closing, in spite of the few long-term problems that are revealed by this shock (which is presumably exogenous to the individual), in a country with a generous system of unemployment insurance and with a relatively low unemployment rate.¹⁹ In comparison, retirement is associated with no detectable changes in life satisfaction.

This ambiguity in time horizon has been a serious problem for applications of contentment data, particularly in macroeconomics. Most researchers have opted for showing high correlations, or repeating their estimates using questions worded slightly differently, and claim robustness (see Di Tella, MacCulloch, and Oswald 2001, 2003; Wolfers 2003; Blanchflower and Oswald 2004). Given that this ambiguity seems to be an important weakness for the data presently available, we now provide some preliminary but suggestive evidence bearing on this issue. Our strategy is to exploit the fact that before 1987 most Eurobarometers included (besides the question on life satisfaction described in section 2) a question administered towards the end of the survey: "*Taking all things together,*



Panel 2a: Effect on Life Satisfaction Four Years Before and After an Unexpected Job Loss



Panel 2b: Effect on Life Satisfaction Four Years Before and After an Expected Job Loss Due to Retirement

Figure 2
Life Satisfaction After an Unexpected Job Loss and Anticipated Retirement
Source: Authors' calculations.

how would you say things are these days—would you say you're very happy, fairly happy, or not too happy these days?" (Small "don't know" and "no answer" categories are not studied here.) Let the variable, happiness, be defined as follows: 1="not too happy," 2="fairly happy," and 3="very happy."

Life satisfaction and happiness are strongly positively correlated (Pearson's correlation coefficient equals 0.56). In table 2 we test to see whether the future is also a part of what is being captured in the life satisfaction responses in comparison with the happiness responses by repeating the basic regression using just the sample for which we have both sets of data available and also including the future levels of unemployment and inflation. Although we have less than half the sample (as happiness data are available up to 1986 only), columns 1 and 2 show that whereas the coefficients on future inflation are similar, future unemployment is uncorrelated with happiness data but strongly correlated with life satisfaction data. The difference between the coefficients on the future unemployment rate in these two columns of table 2 is significant at the 5-percent level. One interpretation is that macroeconomic changes matter beyond the moment (perhaps even beyond the next six months) and that life satisfaction, with its reference to "the life you lead" (as opposed to "happy these days") introduces a longer time horizon.²⁰

This ambiguity about the interpretation of these different measures is, perhaps, natural given that they were not developed for macroeconomic applications where intertemporal matters are so important.²¹ It does not, however, seem like an insurmountable problem if some energy went into designing questions that can make the distinction. For new measurement strategies see, for example, Kahneman et al. (2004) and Kimball and Willis (2006).

Although these are relatively new methods (and not yet available across many countries and years), they do remind us that the measurement of emotions in economics is still in its infancy as a research area, and that it is hard to predict how effective this research program will be in the long run, particularly relative to longstanding programs (for example, national accounts) in which economists have convinced society to spend considerable amounts of money. One natural (and cheap) starting point for macroeconomists would be to include two questions, asked in suc-

Table 2
How Life Satisfaction and Happiness Scores Vary with Current and Expected Future Inflation and Unemployment Rates in 16 OECD Countries, 1973 to 1986

Dependent Variable	Life Satisfaction	Happiness
Macroeconomic Variables		
<i>Unemployment Rate</i>	-2.2 (1.3)	-2.2 (1.6)
<i>Unemployment Rate t + 1</i>	-3.1 (1.2)	0.8 (1.3)
<i>Inflation Rate</i>	0.4 (0.5)	0.4 (0.5)
<i>Inflation Rate t + 1</i>	-3.4 (0.5)	-3.3 (0.5)
<i>Personal Characteristics</i>	Yes	Yes
<i>Dummy Variables</i>	Country and Year	Country and Year
Number of Observations	128,722	128,722
Country-year clusters	99	99
Pseudo R ²	0.08	0.08

Source: Eurobarometer Survey Series (1973–1986)

Note: Ordered probit regressions with robust standard errors in parentheses, clustered at the country year level. Personal characteristics include employment status (self-employed, retired, keeping house, or in school), income, marital status, education, gender, and age-squared. The dependent variable in column (1) is the answer to the Eurobarometer question, "On the whole, are you satisfied with the life you lead?" The four possible answers are: "not at all satisfied"; "not very satisfied"; "fairly satisfied"; "very satisfied". The dependent variable in column 2 is the response to the Eurobarometer question, "Taking all things together, how would you say things are these days—would you say you're very happy, fairly happy, or not too happy these days?" (1 = "not too happy", 2 = "fairly happy" and 3 = "very happy"). *Unemployment rate t + 1* and *Inflation rate t + 1* are the unemployment and inflation rates one year into the future.

cession one after the other, with appropriate differential emphasis on the future and the present. We do not know of any questionnaires available at present designed to deal effectively with this problem across countries and years.²²

Contentment versus Other Emotions

As first mentioned in the introduction, there is the possibility that contentment is just one of the components of utility. For illustration purposes, assume that only contentment and regret make up utility. In that case, we have:

$$(3) \text{ Utility} = \text{Contentment} - \beta \text{regret} + \varepsilon,$$

where $\beta > 0$ and we normalize the coefficient on contentment to equal 1. Assume also that we are interested in estimating:

$$(4) \text{ Utility} = -A \text{Unemployment} - B \text{Inflation} + \sigma,$$

which implies that:

$$(5) \text{ Contentment} = -A \text{Unemployment} - B \text{Inflation} + \beta \text{regret} - \varepsilon + \sigma,$$

where $A > 0$ and $B > 0$. We maintain the assumption that shocks to inflation and unemployment are uncorrelated with σ . Now assume that we try to estimate the following regression equation (mistakenly believing that life satisfaction scores, which measure contentment, are also a good proxy for utility):

$$(6) \text{ Life Satisfaction}_{ntj} = -a \text{Unemployment}_{nt} - b \text{Inflation}_{nt} + \psi_{ntj}.$$

Then the error term contains the other elements of what we are calling true utility:

$$(7) \psi_{ntj} = \beta \text{regret}_{ntj} - \varepsilon_{ntj} + \sigma_{ntj}.$$

Let the expected values of the point estimates of the coefficients on unemployment and inflation obtained from estimating equation (6) be equal to $-\hat{a}$ and $-\hat{b}$, respectively. We have the following possibilities:

1. If $\text{correlation}(\psi, \text{Unemployment}) = 0$ and $\text{correlation}(\psi, \text{Inflation}) = 0$, then the estimates we obtain reveal the true size of the effect of unemployment and inflation on true utility, even in levels. In other words, we have $\hat{a} = A$ and $\hat{b} = B$.

2. If $\text{correlation}(\psi, \text{Unemployment}) \neq 0$ and $\text{correlation}(\psi, \text{Inflation}) \neq 0$, then the main coefficients of interest are biased. An example illustrates our point. If regret plays an important role and is raised by inflation, as suggested by Rotemberg (this volume), then our coefficient on *Inflation* might underestimate the true effect of inflation on true utility: $\hat{b} < B$. Indeed, in Rotemberg's theory of regret, $\text{correlation}(\psi, \text{Inflation}) > 0$, so that when inflation goes up, true utility is going to be falling more than life satisfaction (due to the extra effects of regret).

Of course, this is a simple illustration since the bias is hard to pin down, particularly when other emotions (besides regret) are included. With a multiplicity of emotions, a natural question is whether measures appropriate for empirical analyses can be constructed to produce better tests as outlined above. It seems so. A simple theoretical position, for example, is to view emotional expressions as a basic by-product of emotional experience.²³ If emotional expressions provide a guide for the actual experiences, then the expressions themselves are one indicator of the range of emotions available in humans. A large amount of work in this area is due to Paul Ekman (see, for example, Ekman, Sorenson, and Friesen 1969; Hager and Ekman 1983). Facial analysis has been facilitated by a method for coding emotions called the Facial Action Coding System (FACS). Figure 3, adapted from Hager and Ekman (1983), illustrates this method.

Six different types of facial expressions—happy, sad, angry, fear, disgust, and surprise—appear to be the most robust and are depicted in figure 4. It seems possible to argue that other emotions can be reduced to versions of these six types, although there is some contention about contempt (which is arguably a version of disgust), shame, and startle. The facial coding system and these faces are described online at <http://face-and-emotion.com/dataface/emotion/expression.jsp>.

3. Other Contentment Tests in Macroeconomics

Further tests can be informative. First, a natural step is to move beyond the representative agent paradigm and estimate the impact of macroeconomic fluctuations on contentment across groups. This has intrinsic interest (for example, in partisan political economy models) and is also

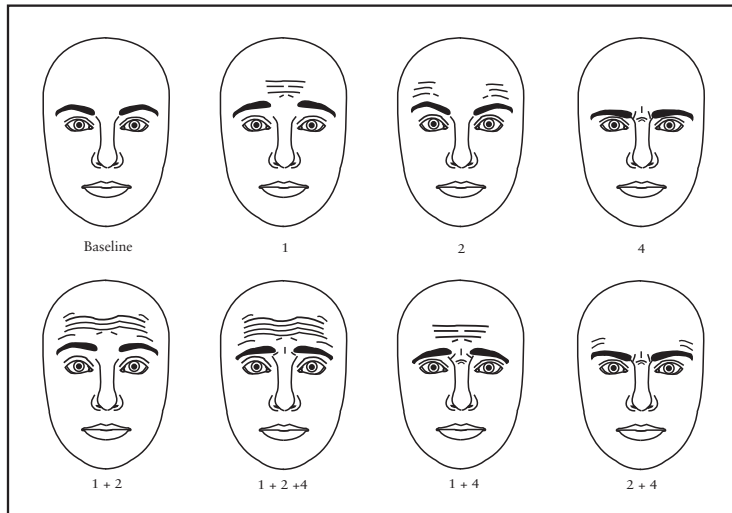


Figure 3
Facial Action Coding System

Source: Redrafted following Hager and Ekman (1983).

Note: The three FAC (Facial Action Coding) units in the brow area and their combinations are illustrated. AU 1 (action of inner frontalis) raises the inner corners of the eyebrows, forming wrinkles in the medial part of the brow. AU 2 (action of the outer frontalis) raises the outer portion of the eyebrows, forming wrinkles in the lateral part of the brow. AU 4 (action of procerus, corrugators, and depressor supercillii) pulls the eyebrows down and together, forming vertical wrinkles between them and horizontal wrinkles near the nasion. The combinations of AUS show how these AUs can act together to form composites of appearances each produces separately.



Figure 4
Ekman's six expressions: Happy, Sad, Angry, Fear, Disgust, Surprise
Source: Authors' illustration.

relevant to the discussions in section 2 above (as it is one solution when there are groups that are suspected of using language differently in ways that may affect the estimates in equation (1) in table 1). Second, we can also use contentment data to help identify some of the channels through which macroeconomic fluctuations matter. And finally, it is possible to study how macroeconomic fluctuations matter, in particular whether there is a significant role for nonlinearities. These three kinds of contentment tests are the focus of this section, and we address each one in turn.

There has been some interest among macroeconomists in studying the costs of business cycles for different groups. In some cases, such differences might even explain different views about the optimal response to shocks, and hence, differences in the experience under policymakers of different color (see, for example, Alesina 1987; Hibbs 1987). One dimension that has received particular interest is income. Hibbs (1987) cites Paul Samuelson as saying:

We tend to get our recessions during Republican administrations. . . .The difference between the Democrats and the Republicans is the difference in their constituencies. It's a class difference. . . .The Democrats constitute the people, by and large, who are around the median incomes or below. These are the ones whom the Republicans want to pay the price and burden of fighting inflation. The Democrats are willing to run some inflation (to increase employment); the Republicans are not (213).

Contentment data can be used to study these questions. Table 3 shows how inflation and unemployment affect life satisfaction responses by demographic groups. In column (1) we present the results for inflation, and note that those on low income display the biggest reductions in life satisfaction. The negative coefficient on *inflation* is monotonically smaller (in absolute value) as we go up the income quartiles, although the effect is not significant. This is consistent with Di Tella and MacCulloch (2005). It is also quite intriguing that the employed (the omitted category) are significantly more adversely affected by inflation compared to the self-employed and those who stay "at home." Males are less affected by inflation than females. Those with little education (less than 15 years, the base category) are more affected by inflation than those with high levels (more than 18 years) and, in particular, those with intermediate levels of

Table 3
The Determinants of Life Satisfaction, Interacting Unemployment and Inflation Rates with Personal Characteristics in 16 OECD Countries, 1973 to 2002

Dependent Variable: Life Satisfaction	(1)		(2)	
	Coefficient	*Inflation	Coefficient	*Unemployment
<i>Unemployment Rate</i>	-2.0 (0.4)		-3.2 (1.5)	
<i>Inflation Rate</i>				
	-2.1 (0.8)		-1.0 (0.4)	
Unemployed	-0.5 (0.02)	0.3 (0.3)	-0.4 (0.04)	-1.2 (0.5)
Self-employed	-0.001 (0.01)	0.3 (0.1)	0.01 (0.02)	0.08 (0.2)
Retired	-0.01 (0.01)	0.2 (0.2)	-0.04 (0.03)	0.5 (0.3)
Keep Home	-0.03 (0.01)	0.6 (0.1)	0.008 (0.02)	-0.02 (0.2)
In school	0.1 (0.01)	-0.01 (0.2)	0.1 (0.02)	-0.3 (0.2)
Male	-0.08 (0.01)	0.3 (0.1)	-0.07 (0.01)	0.06 (0.2)
Age	-0.03 (0.001)	-0.03 (0.02)	-0.02 (0.002)	-0.1 (0.03)
Age Squared	2.9e-4 (1.5e-5)	2.9e-4 (2.0e-4)	2.1e-4 (2.5e-5)	0.001 (2.4e-4)
Income 2	0.05 (0.01)	0.1 (0.2)	0.07 (0.02)	-0.09 (0.2)
Income 3	0.2 (0.01)	0.1 (0.2)	0.2 (0.02)	-0.5 (0.2)
Income 4 (top)	0.3 (0.01)	0.2 (0.2)	0.4 (0.02)	-0.6 (0.3)
Education 15–18 years old	0.07 (0.01)	0.4 (0.1)	0.05 (0.02)	0.5 (0.2)
Education >18 years old	0.2 (0.01)	0.2 (0.1)	0.09 (0.02)	1.1 (0.2)
Married	0.2 (0.01)	-0.3 (0.2)	0.2 (0.02)	-0.6 (0.2)
Divorced	-0.2 (0.02)	-0.8 (0.3)	-0.2 (0.04)	-0.2 (0.5)
Separated	-0.3 (0.03)	-0.5 (0.6)	-0.3 (0.05)	-0.5 (0.6)
Widow	-0.1 (0.01)	-0.3 (0.2)	-0.2 (0.02)	0.2 (0.3)
Number of Observations	609,243		609,243	
Pseudo R ²	0.09		0.09	

Source: Eurobarometer Survey Series (1973–2002)

Note: Ordered probit regressions with robust standard errors in parentheses, clustered at the country-year level (309 clusters), including country and year dummies. The dependent variable is the answer to the Eurobarometer question: “On the whole, are you satisfied with the life you lead?” The four possible answers are: “not at all satisfied”; “not very satisfied”; “fairly satisfied”; “very satisfied.”

education (between 15 and 18 years). There are fewer detectable changes in the basic patterns in column 2, which studies the effect of unemployment across the different groups. The biggest effects are that recessions are particularly costly in contentment terms to those coming of age (see also Bechetti, Castriota, and Giuntella 2007) and those with intermediate and high levels of education.

An alternative approach is to focus on different partisan political beliefs. In several estimates based on contentment regressions, we found weaker differences between these groups than those found in Di Tella and MacCulloch (2005), which focuses on a smaller sample of countries and years (up to 1992) and finds that the unemployment/inflation ratio is higher for left-wingers than for right-wingers.

A second possible use of contentment data is to test the relevance of some of the channels suggested in the theoretical literature. For example, in all of the papers in the literature that we know, being unemployed is associated with large emotional costs, even after controlling for the income losses associated with losing a job (see, for example, Clark and Oswald 1994). The coefficients typically imply very large costs, approximately similar to the well-being difference reported by individuals at the opposite ends of the income distribution in the sample. Assuming these estimates reflect causal forces, they reject the approach used by real business cycle theorists to measure the costs of business cycles, if only because jobless but insured individuals would presumably experience smaller downturns in utility. One could still force a classic interpretation by thinking that these are simply people with unrealistic aspirations about what jobs they can get. However, Clark (2003) presents panel evidence showing that the drop in well-being associated with becoming unemployed is smaller given the higher the unemployment rate is in this person’s reference group (see also Stutzer and Lalive 2004).²⁴

Some general information on these channels can be obtained by looking at the effect of unemployment and inflation across different groups in table 3. Column 2 finds that the coefficient on being unemployed becomes more negative at higher unemployment rates, although it is imprecisely estimated (and does not use the unemployment rate in the reference group). Di Tella, MacCulloch, and Oswald (2003) test the hypothesis that the welfare state has made life too easy for the unem-

ployed. They find a strong, positive relationship between the reported life satisfaction of the unemployed and the generosity of unemployment insurance in panel regressions (see, for example, table 12, column 2 in that paper).

Finally, another possible use of data on emotions is in answering the question of whether nonlinearities exist in the welfare loss function. This is important for answering the question of whether it is more important for the central bank to produce low inflation or stable inflation. Mankiw (2001) notes that “if low average inflation is the goal then the monetary policymakers of the 1990s [in the United States] can be given only an average grade. But if stable inflation is the goal, then they go to the top of the class.” He mentions that there is “little direct evidence of convexity in the costs of inflation. As a result, it is hard to compare quantitatively the benefits of low inflation with the benefits of stable inflation” (9–10).

The assumed quadratic welfare loss function is given by

$$(8) \text{ Social Welfare Loss} = \alpha(\text{Output Gap})^2 + \beta(\text{Inflation})^2.$$

Di Tella, MacCulloch, and Oswald (2001) do not detect nonlinear effects of inflation using life satisfaction surveys. Wolfers (2003) presents a full set of tests for the presence of nonlinearities on both macro variables. He finds that convexities exist with respect to unemployment but are “less easy to detect” with respect to inflation. Consequently his paper finds that “eliminating unemployment volatility would raise well-being by an amount roughly equal to that from lowering the average level of unemployment by a quarter of a percentage point” (1).

Table 4 explores the evidence on nonlinearities by extending the basic estimates using squared terms. We find evidence of a nonlinearity with respect to unemployment but not with respect to inflation. However, unlike the Eurobarometer data, no evidence of a nonlinearity with respect to unemployment is found using World Values Survey data, although given the informal nature of the labor market in some of the countries in the (cross-sectionally) larger data set, unemployment may not be the best indicator of the state of the economy.²⁵

Note that nonlinearities could be coming from a quirk in the reporting function. Although we may estimate a life satisfaction regression that appears to be nonlinear in unemployment, this implies that there is a

Table 4

How Life Satisfaction Scores Vary with Inflation and Unemployment, Testing for Nonlinearities in 16 OECD Countries, 1973 to 2002

Dependent Variable	Life Satisfaction
Macroeconomic Variables	
<i>Unemployment Rate</i>	0.8 (1.0)
<i>(Unemployment Rate)²</i>	-12.4 (4.6)
<i>Inflation Rate</i>	-1.8 (0.7)
<i>(Inflation Rate)²</i>	3.1 (3.2)
<i>Personal Characteristics</i>	Yes
<i>Dummy Variables</i>	Country and Year
Number of Observations	609,243
Country-year clusters	309
Pseudo R ²	0.09

Source: Eurobarometer Survey Series (1973–2002)

Note: Ordered probit regressions with robust standard errors in parentheses, clustered at the country-year level. Personal characteristics include employment status (self-employed, retired, keeping house, or in school), income, marital status, education, gender, and age-squared. The dependent variable is the answer to the Eurobarometer question, “On the whole, are you satisfied with the life you lead?” The four possible answers are “not at all satisfied”; “not very satisfied”; “fairly satisfied”; “very satisfied.”

nonlinear relationship between unemployment and true internal utility only given the (sufficient) condition that the mapping between internal utility and an individual’s self-reported satisfaction is linear. However, when the mapping, g , is nonlinear,

$$\text{Life Satisfaction} = g(\text{True Internal Utility}) = g(\alpha \text{ Unemployment}$$

$$+ \beta \text{ Inflation}) = \alpha(\text{Unemployment})^2 + \beta(\text{Inflation}),$$

then the (true) linear relationship between internal utility and unemployment will not be detected in our life satisfaction regression. Evidence on

the form of the reporting function is limited and may also pose problems when undertaking interpersonal comparisons of contentment, particularly when there is habituation (see below).

Finally, it is possible to use the data to explore the question of adaptation to high inflation and high unemployment. We test for the presence of these effects by including a lagged term. Then we can calculate the long-term effect of a shock in, say, unemployment, by adding the current and lagged coefficients. Table 5 illustrates and finds adaptation to unemployment while little adaptation to inflation. The long-run coefficient on unemployment is only 34 percent of the short-run estimate $\{0.34 = (-4.4 + 2.9) / -4.4\}$. One complication in the interpretation of these effects is that the issue of causality becomes particularly relevant. Indeed, positive theories of inflation predict that central banks may be more tempted to inflate to reduce unemployment when the costs of unemployment are higher or the costs of inflation are lower. What could give rise to these differences? One simple answer is the historical experience (which trains the mind to deal with such uncertainties) and the institutions designed by societies to deal with such shocks. For example, differences in the strength of informal insurance networks, or differences in the welfare state may affect the costs of falling unemployed. Or differences in mental training under high inflation or historical experience with indexation institutions may affect the costs of inflation. If humans design these institutions to deal with macroeconomic policy, then societies might benefit from the joint design of monetary policy and (say) the welfare state and indexation laws.

Di Tella and MacCulloch (2004) provide evidence of a negative relationship between inflation and the welfare state using a panel of 20 OECD countries over the period 1961–1992, controlling for country and time fixed effects, country-specific time trends, other covariates, and using different measures of benefit generosity (for example, the length of time over which unemployed people can claim benefits).

A recent paper by Becchetti, Castriota, and Giuntella (2007) studies employment protection legislation and the age structure of the population with the objective of separating countries with different well-being costs associated with macrofluctuations. They find that the relative cost of unemployment is higher in intermediate age cohorts and in low job pro-

Table 5
Adaptation in Macroeconomics—How Life Satisfaction Scores Vary with Current and Past Inflation and Unemployment Rates in 16 OECD Countries, 1973 to 2002

Dependent Variable	Life Satisfaction
Macroeconomic Variables	
<i>Unemployment Rate</i>	-4.4 (0.9)
<i>Unemployment Rate t - 1</i>	2.9 (0.9)
<i>Inflation Rate</i>	-0.4 (0.6)
<i>Inflation Rate t - 1</i>	-1.0 (0.5)
<i>Personal Characteristics</i>	Yes
<i>Dummy Variables</i>	Country and Year
Number of Observations	597,433
Country-year clusters	302
Pseudo R ²	0.09

Source: Eurobarometer Survey Series (1973–2002)

Note: Ordered probit regressions with robust standard errors in parentheses, clustered at the country-year level. Personal characteristics include employment status (self-employed, retired, keeping house, or in school), income, marital status, education, gender, and age-squared. The dependent variable is the answer to the Euro-barometer question, “*On the whole, are you satisfied with the life you lead?*”. The four possible answers are: “*not at all satisfied*”; “*not very satisfied*”; “*fairly satisfied*”; “*very satisfied*.” *Unemployment rate t - 1* and *Inflation rate t - 1* are the unemployment and inflation rates lagged one year into the past.

tection countries. They point out that this might explain the difference in objectives for the U.S. Federal Reserve (price stability and employment) versus the European Central Bank (price stability only) and their actual experiences with inflation.

4. Which Emotion Should a Central Bank Target?

Economists build their models of monetary policy around the concept of utility. Perhaps the role of this concept is instrumental (allowing research-

ers to structure their thoughts) rather than descriptive. But two natural questions that arise are:

1. Is there an emotion that is in fact close to utility?
2. Is this the correct emotion to target?

On the first question, psychologists have described several positive emotions, including happiness and contentment. Two survey measures seem particularly relevant: the answers to the questions “Are you happy?” and to “Are you satisfied with your life?” The evidence that we presented in section 2 suggests that the former may better capture instantaneous mood whereas the latter has a larger intertemporal component. If we take the plausible position that contentment captures, to some degree, the future, then life satisfaction may be our best available proxy for an overall measure of welfare. Importantly there are large samples of people that have been asked about their satisfaction with life. Validation studies, in particular those involving smiles and fMRI data as briefly discussed in section 1, suggest that they are indeed related to the economist’s concept of utility, so the answer to the first question is yes. Before proceeding, we note that a separate question on which psychologists and economists do not agree is whether any of these two measures can in fact be considered a summary of other emotions, or if they are themselves a component of utility.

The second question is harder to answer. In brief, we believe that it is reasonable to target contentment (for example, as captured in the answer to a life satisfaction question). This is relatively uncontroversial for the implausible case that other emotions are constant. How can we oppose policies that will lead to the “good life”? The complication is that contentment can be quite stable while happiness may be going down in many realistic settings. It is likely that politicians who target contentment will be forced out of office by those that propose policies that raise happiness. The success of populist platforms is one indicator of their appeal.

A more serious problem is that many actors appear to be actively managing other emotions. One example is voters controlling their anger after observing a corrupt privatization (see Di Tella and MacCulloch 2009). Another example, closer to the issues that concern central banks, involves asset bubbles. Indeed, investors often enter markets that have experienced large increases in prices, even when fundamentals do not appear

to be changing (in a positive direction). For example, until very recently people kept on buying houses in the United States even though negative news kept coming in (about the existence of a war, the deterioration of the current account, and so on). One possible explanation is that investors in this market are also trying to minimize regret, as in Rotemberg (this volume). Thus, they enter the market after price increases because the likelihood of “missing out” on the boom and the cost of experiencing regret at the lost opportunity looms larger in their minds than the fear of losing money in the collapse of the bubble. One likely contributor to this asymmetry is the fact that when missing out on the opportunity, the misfortune is experienced in solitude (while the rest are happy). In contrast, when the bubble collapses, the misery is collectively experienced. One hesitates to add that inferences about one’s own ability/identity are harsher in the first scenario than if one can convince oneself that the problems were so tough that even a large collection of people made the same mistake. A central bank focusing on contentment may leave interest rates unchanged while asset prices rise with the justification that contentment had not moved, making economists who worry about bubbles less impressed with contentment as a target for policy.

In summary, a question for future research is to discover which emotion is the most relevant one for economists, in terms of whether it affects market prices and whether it affects (or should affect) government policy choices.

5. Conclusion

We show that direct data on contentment, measured as self-reported overall satisfaction with life for over 600,000 Europeans, are negatively correlated with the unemployment rate and the inflation rate. There are several possible uses of this result. Our preferred interpretation is that it shows that an emotion that is close to utility is affected by macroeconomic fluctuations. This can be a powerful complement to studies restricted to looking at revealed preference. Even if one takes the view that contentment is just one of many emotions that need to be studied, it seems that it should also be possible to construct direct empirical measures of these other emotions.

We discuss two broad applications of our results. First they may help central banks understand the tradeoffs that the public is willing to accept in terms of unemployment for inflation, at least in terms of keeping the average level of one particular emotion (contentment) constant. Of course we will need more work to make sure that we are identifying the causal effects of these variables on emotions, but we believe that the idea of central banks focusing their research efforts on direct measures of emotions as an outcome variable is justified. An alternative application of these data is to study the channels through which macroeconomics affects emotions.

Economists might also see the results presented as an initial step at obtaining the weights in a social loss function that they can compare with those obtained in more traditional models that dominate the design of monetary policy (for instance, Eichenbaum 1992; Rotemberg and Woodford 1997). Some of the assumptions that have to be made for using our results in this context (a representative agent exists, a summary measure of emotions akin to utility exists, and that individuals only care about income and leisure) will not sound extreme to those trained in this area.

The approach we discuss has limitations, but we suspect that several of them arise because so few researchers with experience in macroeconomic policy have studied these data, and because so few resources have gone into perfecting the measures. For example, we still do not know if the contentment data that we have available for large samples of individuals refer to instantaneous utility or if it is an intertemporal measure (although several results appear similar when we use data on happiness—which is arguably closer to an instantaneous measure). The problem, however, does not seem insurmountable as it can be addressed by developing better measures of contentment.

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Notes

1. One area where utility is a poor predictor of choice is moral decisions (see, for example, Greene et al. 2001). Even more narrowly, one can distinguish between positive and negative affect when constructing measures of emotions (see, for example, Watson and Tellegen 1985; and Myers and Diener 1994).
2. See Akerlof (2007) for a discussion of subjectivity and models with more realistic motivations in macroeconomics.
3. Mankiw (2001) outlines four other costs of inflation. One, inflation induces firms to incur more “menu costs.” Two, because the tax laws are not indexed, inflation raises the effective tax on capital income and thereby discourages capital accumulation and economic growth. Three, inflation makes economic calculations more difficult because the currency is less reliable as a yardstick for measuring value. Four, because unexpected price changes redistribute real wealth between debtors and creditors, volatile inflation creates risks that people seek to avoid and makes the use of long-term contracts using money as the unit of account less tenable (see 8–9). Fischer and Modigliani (1980) is a classic paper outlining the costs of inflation.
4. See Atkeson and Phelan (1994). A different approach to measuring the costs of business cycles using asset prices is developed in Alvarez and Jermann (2004).
5. Note that such an exercise is of interest even if one believes that there is no tradeoff between inflation and unemployment in the long run because shocks might still exist and there is the question of how draconian the adjustment path should be.
6. Broadly, on the one hand, prices change more often when inflation increases, so forfeiting a purchase decision in favor of further searching is risky because prices might increase. On the other hand, the fact that there are relative price oscillations means that there are potentially more bargains out there, so additional search is more valuable. For a discussion of the role of markups, see Bénabou and Gertner (1993).
7. While the loss measures derived depend on several details (in particular the assumptions about the timing of the pricing decisions), the point remains that stabilizing the price level (and not just making expected inflation equal to actual inflation) eliminates the main source of the costs of inflation, namely relative price distortions.
8. Economists have long been aware that their approach would be seen as slightly odd by other people: “we shall see that standard characterizations of the policymaker’s objective function put more weight on the costs of inflation than is suggested by our understanding of the effects of inflation; in doing so, they probably reflect political realities and the heavy political costs of high inflation” (Blanchard and Fischer 1989, 567–568).

9. In that case, one would expect that income inequality would have different effects across the United States and Europe, or across people with different ideological inclinations. See, for example, Alesina, Di Tella, and MacCulloch (2004) and Di Tella and MacCulloch (2005).

10. Conceptually, happiness research need not rely on subjective data. For example, economists who want to focus on actions could study suicide rates or hypertension under the assumption that these phenomena are correlated with true internal happiness. See Stevenson and Wolfers (2006) and Blanchflower and Oswald (2007) for examples of work along these lines. For a register of happiness surveys across 112 nations, visit the World Data Base of Happiness: <http://www1.eur.nl/fsw/happiness/>. For a discussion of happiness malleability, see Seligman (2004).

11. In our own estimates of adaptation to income using the German panel we find adaptation to income over 3-4 years. The process of adaptation to income is stronger for left-wingers than for right-wingers (see Di Tella, Haisken-De New, and MacCulloch 2006).

12. There are also indicators of “perceived inflation” that give quite different answers to the official CPI measures. Conceptually we should be able to test whether it is actual inflation rates or perceived inflation rates that matter most to consumers using happiness data.

13. There is a potential problem when life satisfaction scores are at the top of a certain measurement scale, so that they cannot rise higher, or at the bottom of the scale, so that they cannot fall lower. This is more serious in surveys with few categorical answers (the Eurobarometer has four, whereas most new surveys offer a 10-point scale). These bounds can also make it appear that marginal utility is diminishing as consumption increases, when in fact the scores are hitting the top of the scale and for that reason becoming less responsive to rising true utility.

14. Svensson (2004) converts these estimates to a tradeoff between the output gap and inflation using Okun’s Law. He states that “a simple version of Okun’s Law is that a change of the unemployment rate of one percentage point corresponds to a change of the output gap of some 2 to 2.5 percentage points” (6). That is,

$$\text{Output Gap} = -\kappa \text{Unemployment}$$

where $\kappa \in (2, 2.5)$ and both the output gap and unemployment are measured in percentage points. Consequently a one percentage point reduction in the output gap would cause between 0.4 ($=1/2.5$) and 1 ($=2/2$) times as much of a reduction in contentment as an additional percentage point of inflation.

15. Some question the desire to require the central bank to correct all macroeconomic distortions (even when they are aggregate in nature). As a justification, Galí (2002) invokes the principle of division of labor and suggests that “other branches of government are likely to have more suitable tools than those under the control of the central bank to handle many of those distortions. Hence, it

would seem desirable to assign the central bank with the task of correcting the distortions of a monetary nature.” He then assumes that the monetary authority’s mandate is to correct the distortion associated with the presence of staggered price setting (see also Rotemberg and Woodford 1999; and Galí and Monacelli 2005).

16. As an example to illustrate this problem, consider the hypothesis that inflation only matters because it makes people think that they enjoy money more than they do. A one-shot increase in prices at time t leads to plans for excessive work hours in the future being made. However, at time t people would not tick down their happiness scores.

17. The question of whether well being measures can be compared across cultures can be studied using vignettes as anchors, as in King et al. (2003). Helliwell and Huang (2006) use population shares above particular numerical life satisfaction cut-off scores as alternative dependent variables.

18. It is interesting to note that 36 percent of right-wing individuals declare being at the top life satisfaction category (compared with almost 22 percent of left-wing individuals). A second definition based on answers to the question—“*If an election were to be held tomorrow, which party would you vote for?*”—(and the subsequent classification of these parties into “left” and “right” by political scientists) yields similar results.

19. Note that such a large drop seems inconsistent with the small difference between the lifetime expected utility of the employed and the unemployed in some models (for example, Shapiro and Stiglitz 1984).

20. One can presumably reject the hypothesis that the answers to the happiness question are themselves just noise because they are strongly correlated with life satisfaction answers.

21. Note that one can still push the idea that this ambiguity does not affect the relative coefficients on inflation and unemployment (or comparisons across subgroups of the population) under the assumption that changes in these macroeconomic variables elicit a reaction of similar aspects of life satisfaction, and provided that both inflation and unemployment are governed by similar stochastic processes so that both coefficients in a life satisfaction regression are scaled up or down in the same proportion. However, there are many applications in macroeconomics that require more precision in the interpretation.

22. However, within the United States, the University of Michigan monthly consumer survey has recently included the question: “Now think about the past week and the feelings you’ve experienced. Please tell me if each of the following was true for you much of the time this past week: You were happy. You felt sad. You enjoyed life. You felt depressed.” People are asked to give “yes-no” answers to each of those four questions.

23. An alternative position, which originated with Charles Darwin, views emotional expressions as signals in communication games.

24. Another potential application of well being data in the labor market concerns the gains from better matching (see Luechinger, Stutzer, and Winkelmann 2007).

25. There were no episodes of deflation in the sample so there is no dummy variable defined to capture this state. However, conceptually we should be able to measure whether there are asymmetric costs to deflations versus inflations using contentment data.

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Comments on “Happiness, Contentment, and Other Emotions for Central Bank Policymakers” by Rafael Di Tella and Robert MacCulloch

Alan B. Krueger

I thought of a couple of different ways to discuss this provocative paper. The first was very simple; I asked myself:

“All things considered, how satisfied are you with this paper as a whole on a scale from 0 to 10?”

I could give the paper a score of, say, 8. But this metric raises some obvious problems. When I say 8 that might differ from what Greg Mankiw means if he answers 8 to this question. How would you know that I was really satisfied with the paper if I say 8? Even if you can get past worries about interpersonal comparisons of subjective reports and understand that my 8 means that I was more satisfied than dissatisfied, I am not able to communicate exactly what about the paper satisfied me in this approach. What did I focus on in giving my rating—the issues addressed, the data, and/or the econometrics?

This is one of the themes running throughout my comments: the type of survey question that Di Tella and MacCulloch analyze elicits a global evaluation that survey respondents can interpret in different ways when providing their answers. Respondents could aggregate their lives in any way they want. This possibility leads to many concerns, some of which are noted in the paper, such as the time horizon that respondents had in mind. Are people answering for right now or about the future over their lifetime? Are they thinking of their social life or of their financial life?

I think the evidence that psychologists have assembled suggests that global life satisfaction and happiness questions are easily manipulated by subtle changes in wording, changes in question order, and recent irrelevant events (Schwarz and Strack 1999). I do not think that this limitation

means that subjective evaluations should never be analyzed or that they are meaningless, but this limit should be borne in mind. All economic data have noise. Di Tella and MacCulloch raise the right question: is there a signal present in the subjective data? If so, perhaps the noise will average out. And I do agree that there is some signal in the self-reported satisfaction measures. For example, self-reported life satisfaction correlates with health, longevity, and brain functioning. So I do think that there is a signal found in satisfaction data, although I wonder sometimes about exactly what that signal is reflecting.

This leads to my second approach for commenting on the paper. Instead of giving a global evaluation of the paper as a whole, I can report how satisfied I am with different domains of the paper:

“From 0 to 10, how satisfied am I with the paper’s importance, exposition, use of econometrics, interpretation, creativity; and how convincing is it?”

I think there is no question that this paper addresses an important topic, so I give it a 10 on that dimension. On interpretation, I give the paper a 5 or 6, and that is where I will concentrate most of my comments. On exposition, I think some parts of the paper could have been more clearly explained, so I give it a 5. As for creativity, I think this paper takes a clever approach to the problem, and I give it a 9. The application of econometrics could have been improved, and I give my lowest score to that dimension, a 4. Lastly, like some survey respondents, I refuse to give an answer to how convincing the results are because I’m not sure.

As my remarks suggest, an alternative to learning how people view their lives is to ask them how satisfied they are with specific domains of their lives, instead of asking about their lives as a whole. That could be done either by asking about satisfaction with their home life, work life, social life, and so on, or by collecting data moment-by-moment on how people feel about what they are doing. These two approaches—an overall global evaluation versus more specific reporting—sometimes give different results (see Krueger et al. 2009).

What’s more, I think the differences are not random. People use selection bias in recalling domains of their life when making an overall evaluation of their lives. In one study of school teachers, Kahneman et al. (2002) found that teachers in the poorest performing schools in Texas

were fairly unhappy while they were at work compared with teachers in exemplary schools, but they were no less satisfied with their lives as a whole. While work satisfaction was notably lower for the teachers in the poor performing school, apparently the teachers working in these schools chose to ignore a consideration of their work lives when answering the overall life satisfaction question. One way of avoiding this type of selection bias is to ask respondents specifically about what they did with their time in some interval and how they feel about it, or to ask about their satisfaction with different domains of their life to get a more complete picture.

Another concern is that when people answer global life satisfaction questions, they tend to use rules of thumb to form their answers because a question about life satisfaction is not something, like their street address, that they are used to answering all the time. Nonetheless, people have no problem giving an answer to life satisfaction questions; the questions have low nonresponse rates. I suspect that many people arrive at an answer by going through an exercise in which they say, “I’m pretty well-off financially, I should be satisfied with my life” or “I just got divorced or lost my job, I should be unsatisfied.” This is one reason, I suspect, why we find a stronger correlation between income and life satisfaction than we do between income and how people feel from moment-to-moment (Kahneman et al. 2006). This process has been described as a “good fortune heuristic.” If people are fortunate in their financial circumstances, they may use that gauge as their rule of thumb for answering questions about life satisfaction.

There might also be something of “a good economic performance heuristic” to some extent. When people are asked about their satisfaction—especially in a survey where respondents are told that the purpose of the survey is to compare people all across the world or, in the Eurobarometer, where the point is to compare across European countries—they might think, “My country is doing pretty well, I should be satisfied.” It is certainly possible that such a heuristic affects the underlying data in Di Tella and MacCulloch’s paper.

I think another issue with interpretation is that there are many aspects that influence subjective well-being. There’s a whole family of different

measures that can be used to assess subjective well-being. The various measures reflect different factors. The correlation between moment-to-moment measures of mood and life satisfaction is certainly less than one.

I do not believe that any single measure of well-being is best. Any one is just a partial measure of well-being that is, at best, correlated with utility. Indeed, I think the paper would read much better, and be a better paper, if the authors had done a global “search and replace” on the term *true utility* and replaced it with *subjective well-being*. I don’t think the results are any less significant for central banks if the results are just interpreted as reflecting determinants of some component of subjective well-being or one measure of subjective well-being. If it is true utility that the authors believe they are seeking to measure, I think they are raising the bar unnecessarily high.

Turning to the econometrics, where I gave a low score, I think there are strengths and weaknesses in the approach used by Di Tella and MacCulloch. One strength is that country fixed effects are included, which eliminates some possible cultural differences in satisfaction questions. Another strength is that the authors examined many different subsamples and included interactions for different groups.

The main econometric weakness concerns the level of analysis. There are not 600,000 independent observations. There are only 16 European countries or so over about 25 years in the sample.¹ Time-series data tend to move slowly over time, so I would not be surprised if the residuals are serially correlated at the country level. I would recommend proceeding in two steps— first removing individual characteristics and then aggregating to the country level to do the analysis and model some of the time-series properties of the errors. In addition, it would be instructive to present some scatter plots displaying the results across the countries.

I’m a little bit nervous that the results are more sensitive to small changes, such as changes in the sample coverage, because the precision of the estimates is overstated in the individual-level analysis that does not allow for serially correlated country errors. In a version of the paper that used a smaller sample, the ratio of alpha to beta was on the order of 4 to 1, indicating that unemployment is much more important for satisfaction than inflation. Now the ratio is much closer to 1 to 1. It would also be useful to have a standard error for the ratio of alpha to beta. And

it would be helpful to explain why Blanchflower (2007) finds an even larger effect of unemployment than inflation in his related analysis of life satisfaction data.

I also wonder if Di Tella and MacCulloch’s models over-control for covariates by controlling for income and one’s own employment situation. The latter feature of the specification assures that the ratio of alpha to beta understates the impact of unemployment because the direct effect of unemployment on an unemployed individual’s life satisfaction is held constant. Research has found that individuals who become unemployed, especially if they are laid off individually, as opposed to part of a mass layoff or plant closing, suffer a large and lasting drop in life satisfaction.

Another econometric concern is that the substantial differences in the results between the World Values surveys and the Eurobarometer data are not reconciled. I also worry a little about heterogeneity in responses. Maybe countries that have people who worry a lot more about inflation tend to lower it more. And you can make the same kind of argument for unemployment.

In addition to the econometrics issues just mentioned, I have a to-do list for the authors. I think Di Tella and MacCulloch can do more to tease out why the unemployment rate matters. What I take away as the really stunning result in the paper is that the unemployment rate matters much more than real business cycle models say it should matter for people’s welfare. Why? Can you distinguish between the regional unemployment rate and the national unemployment rate—is it what people read in the news media or what they see going on in their more immediate areas that matters more? Likewise, you can use the overall unemployment rate versus a rate that is more specific to an individual’s skill set. Which matters more?

I also worry that a high inflation rate is a marker for something else about a country, especially in the World Values Survey. The standard deviation in the inflation rates across countries in the World Values Survey was 100 percent, which is remarkably high. Countries that have extremely high inflation rates may be basket cases for lots of reasons, so inflation is standing in for those other features of the country.

It would be useful to look for some corroborating evidence to bolster the paper’s assertions. If unemployment matters so much for life satisfac-

tion, why is inflation but not unemployment included in the Fair model of voting? Which variable predicts elections better, inflation or unemployment? This is one way of getting closer to looking at revealed preference.

The finding that inflation has a smaller effect on satisfaction for higher income people than for lower income people is quite striking and surprising. That result suggests to me that tax bracket creep is not the explanation for why inflation matters so much for satisfaction—because higher income people have more savings and should be more concerned that the return on their savings is eroded by inflation.

To connect the paper's results to monetary policy, I thought it would be useful to examine the effects of the levers—interest rates—that a central bank has at its disposal to nudge inflation and unemployment. What I have in mind is a reduced-form model. How does the federal funds rate, or its European equivalent, influence life satisfaction? Of course, one has to overcome endogeneity problems to provide a convincing answer, but ultimately this relationship is what the central bank should be interested in.

To conclude, I think the results in the paper help to explain why politicians are so concerned about unemployment, despite Robert Lucas's protestations. I view this paper as more of a contribution to positive economics than normative economics, even though it is using a normative outcome measure. I think central banks should be interested in understanding the political economy behind inflation and unemployment; that is, why the unemployment rate matters so much for people. Di Tella and MacCulloch's results help to explain why Humphrey-Hawkins gives a dual mandate, and why there's popular support for it.

Regardless of the results, however, the Federal Reserve Board needs to respect its congressional mandate. The paper devotes much attention to discussing what the Fed should do in light of the heavy weight placed on unemployment in satisfaction regressions. That discussion strikes me as irrelevant. The Fed gets its mission from Congress, not from regression results.

A final question pertains to why is this paper considered a contribution to the misnomer known as behavioral economics? If one defines behavioral economics as the integration of ideas from psychology into economics, I suppose the present paper qualifies as behavioral econom-

ics. But sociologists also helped develop and use life satisfaction data. So I do not find that a fully compelling explanation for classifying this as a contribution to behavioral economics.

Here is another justification. The use of self-reports of satisfaction becomes more attractive if there are problems in revealed preference; that is, if one cannot infer preferences from decisions. One of the central implications of behavioral economics is that one cannot always rely on revealed preference to infer preference orderings. People's decisions don't necessarily reflect what's in their own interest, and their decisions can be inconsistent depending on how choices are framed. In that case, our standard practice of relying on revealed preference is not all it is cracked up to be, and the appeal of alternative ways to decipher what people like, such as by asking them, is greater.

Finally, the results themselves lend some support to why behavioral economics is considered a worthy subfield. A major result of the paper is that the national unemployment rate has a profound effect on people's sense of their well-being beyond what would be expected from their personal consumption of goods and leisure. Yesterday George Baker argued that individuals' identities are connected to their jobs and work. I suspect that that is one reason why the experience of unemployment has a lasting effect on individuals, especially individual layoffs. And I suspect that unemployment has an effect on individuals' views of their country as well, a perspective that extends beyond their own immediate situations.

Note

1. Because some of the sample covers a period after European Monetary Union, one can also argue that there is dependence in inflation rates across countries that use the euro, at least for part of the sample.

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Comments on “Happiness, Contentment, and Other Emotions for Central Bank Policymakers” by Rafael Di Tella and Robert MacCulloch

N. Gregory Mankiw

I much enjoyed reading this paper. I have never participated in the happiness literature, although I am often a consumer of the research. And I often find myself intrigued by what I learn. To be more precise, I can report that I am often happier after reading one of these papers than I was before reading it.

Some economists are congenitally skeptical about asking people how happy they are. But I am more open-minded about it. One reason is simple diminishing marginal utility from looking at yet another set of regressions on the conventional macroeconomic time series. Moreover, I find that a lot of the results produced by this happiness research ring true to me.

One finding from this literature is that living with adolescent children makes people less happy. When I first read of this result, my kids were small, and I was agnostic about it. Now, as the father of two teenagers, I am not about to argue. And neither would my wife.

Some of this research finds that happiness as a function of age is U-shaped: happiness reaches its nadir around middle age, and then rises as a person grows older. As someone about to celebrate the half-century mark, I find that result appealingly hopeful.

This new paper takes on the formidable task of asking what central bankers should learn from happiness research. The basic premise is that central bankers are told to care about inflation and unemployment, but are unsure how to weight those two goals. If we can measure how much inflation and unemployment affect happiness, then we can give central bankers the appropriate weights for their objective functions.

There are various assumptions that one has to accept to buy into this analysis. The first is that happiness is the right objective.

My initial reaction is to balk at this assumption. It is tempting for some economists to treat self-reported happiness as utility, which in turn enters the benevolent social planner's objective function. That assumption is appealing mainly because it is so convenient, but I am not fully convinced.

Like many parents, I try to impress upon my children that there is a vast difference between happiness and satisfaction, that a good life is more important than a happy one. This conversation usually takes place when I am trying to explain to my young son that it is time to turn off his Game Boy, and that I am telling him to do this not as a punishment but for his own good. Somehow, he never seems convinced.

I am not entirely sure what this happiness objective really means for central banking. But I do hope that as economists embark on happiness research, we don't forget that happiness and utility are not necessarily synonyms.

The bigger problem I have with this paper involves issues of identification. These concepts are not explored as fully as they need to be in order to make the analysis persuasive. So let's think a bit more about identification in this context.

One way to approach the question is to ask what is causing variation in the right-hand variables, inflation and unemployment, over time in various countries. There are many possible sources of this variation. First, there are labor-market policies, such as minimum-wage laws and unemployment-insurance programs. Second, there are shocks. The productivity slowdown of the 1970s was responsible for, or at least complicit in, the rising inflation and unemployment rates of that era, and the productivity acceleration of the 1990s was similarly a large part of the good news of that decade. Third, there is the general competence of a nation's policymaking institutions to consider, such as the ability of the nation's central bank to maintain independence and low inflation in the face of difficult shocks and political pressure.

With these sources of variation in mind, consider what must be true for the regressions in this paper to make sense as structural estimates. You have to believe that these exogenous disturbances affect happiness only

to the extent that they affect inflation and unemployment. But the more one thinks about this assumption, the less appealing it seems.

Those who advocate labor-market policies such as minimum-wage laws or generous unemployment insurance, for example, think that by helping to create a more egalitarian society, the policies will have some direct beneficial effect on happiness. Conversely, those who advocate scaling back the welfare state think that they will break the culture of dependency and thereby increase overall happiness. Both advocacy groups would expect these policies to affect happiness directly, while incidentally being correlated with unemployment.

Similarly, shocks such as slowdowns and accelerations of productivity could also affect happiness directly. In the late 1990s, the stock market boom probably raised happiness, as everyone enjoyed opening their 401(k) statements. But because this boom also influenced inflation and unemployment, the direct impact on happiness would mistakenly show up as biased coefficients on the included variables.

Finally, inflation and unemployment are correlated with the general competence of policymaking institutions. There is no doubt that incompetent policy can lead to high inflation and high unemployment—history is littered with such examples. For the estimates in this paper to be valid, you have to believe that incompetent policy influences happiness only through the channels of inflation and unemployment. This is surely not the case. Policymakers have a multitude of ways of making people miserable.

In short, the regressions here suffer from the problem of omitted variables that are likely to be strongly correlated with the included variables. For analyses of optimal monetary policy, omitted variables are not necessarily a problem. Central banks can take, for example, labor-market policies as given when they set monetary policy. They do not need to worry about the omitted nonmonetary determinants of social welfare—as long as these omitted variables are additively separable in the social welfare function, they won't affect optimal monetary policy. But the problem of estimating the social welfare function from observed inflation and unemployment is far more difficult. For this problem, you can't get easily ignore the omitted variables, even if they are additively separable.

One approach to dealing with this problem would be to search for plausible instruments. There is a large literature that tries to estimate monetary policy shocks. A reasonable assumption might be that these monetary shocks affect welfare only to the extent that they affect inflation and unemployment. In this case, one could use these shocks as instrumental variables in this kind of exercise.

Until that study is carried out, central bankers will have to take the estimates in this paper with a grain of salt. These regressions establish some intriguing correlations, which invite reflection and storytelling. But we are far from having established a causal connection between macroeconomic conditions and people's overall happiness.

In closing, let me call attention to a passing remark in the paper that may have some direct implications for monetary policy. The authors mention that reported happiness can be manipulated. In particular, in one study, respondents who "accidentally" found a dime on the ground just before they filled out the survey reported being substantially happier than a control group who did not enjoy this "accidental" good fortune.

This finding sheds a new light on the classic metaphor, attributable (I believe) to Milton Friedman, of money being dropped out a helicopter. Maybe this story should be more than a dramatic thought experiment. If the job of a central bank is to make people happy, as this paper presumes, it seems like we have stumbled upon a way to do it. Perhaps someone in the Fed system should be preparing a memo for Ben Bernanke?

7

Behavioral Economics and Economic Policy in the Past and in the Future

Behavioral Economics and Public Policy: Reflections on the Past and Lessons for the Future

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Insights from behavioral economics have already influenced several areas of public policy, and these contributions are likely to continue to have important effects in the future. Regulatory policies and tax policies toward private pensions are examples of this influence. In the last decade policymakers have drawn heavily on findings from behavioral economics in designing regulations for defined contribution retirement saving plans. This is particularly evident in a series of policy changes that have enabled firms to adopt automatic enrollment strategies in their 401(k) plans. My comments will chronicle developments in this area and then describe some of the fundamental challenges that arise in applying behavioral economics to make welfare prescriptions for policy analysis. I close by speculating about future research directions that may be influenced by the growing importance of behavioral economics.

1. The Emergence of Automatic Enrollment Policies

In the mid-1990s benefits managers at many private-sector firms were struggling to encourage low-income and junior employees to participate in voluntary retirement saving programs such as 401(k) plans. Achieving broad participation in these plans is important because the Internal Revenue Service (IRS) has nondiscrimination rules that condition the tax-deductible status of a pension plan on a broad pattern of employee participation, and in particular on the absence of a participation pattern that is skewed toward highly paid employees. Most firms tried to encourage participation of low-income workers by subsidizing their participation with

generous matching contributions or by making specific contributions on their behalf. Both of these approaches can be expensive. Benefit managers searching for low-cost ways to increase participation often tried education programs to inform employees about the benefits of tax-deferred retirement saving, but such efforts were only modestly successful. Moreover, firms were reluctant to make recommendations about retirement saving that might cross the line from providing education to giving financial advice, because the latter could expose them to potential liability risks.

“Automatic enrollment” emerged from the search for low-cost ways to raise 401(k) participation. Several firms experimented with programs that automatically enrolled new workers in their 401(k) plans and that assigned such workers a default asset allocation. These firms sought IRS approval for their approach, and in 1998 the IRS issued a favorable letter ruling. About the same time, the academic research community began to study the effect of these early automatic enrollment plans on worker behavior. Madrian and Shea’s seminal (2001) paper analyzed data from a firm that had implemented automatic enrollment. The study found that automatic enrollment increased 401(k) participation rates by as much as 40 percentage points. Other behavioral economists began designing plans that would use worker inertia to increase saving rates, rather than depress them. Thaler and Benartzi (2004) describe experiences with one such plan, the Save More Tomorrow™ (SMART) program, which commits workers to an increasing savings rate as their job tenure lengthens. A large and expanding literature now examines how various features of 401(k) plans affect participant behavior. Several recent studies, including Beshears et al. (2008) and Mitchell and Utkus (2004), review this literature.

One important feature of the empirical work on firms’ experiences with default plans is that it is relatively easy to describe and communicate these issues to a nonspecialist audience. Simple comparisons of employee participation and contribution rates before and after the adoption of automatic enrollment provide compelling evidence that these programs matter. The findings of such studies can be analyzed and interpreted without complicated statistical tools or a detailed economic model. Anyone who has ever inadvertently purchased something through a “product of the month” club knows that inertia can affect consumer behavior. The surprise finding of these studies, however, is that inertial behavior affects

consumer behavior with first-order consequences, such as choosing the amount to save for retirement, in much the way that it affects less significant decisions such as the purchase of a book or a DVD.

Policymakers’ rapid embrace of the findings from automatic enrollment pilot projects at several firms and the associated academic research reflected a fortuitous coincidence. Private-sector benefits managers interested in satisfying nondiscrimination rules and senior public officials interested in adopting programs that would increase private saving and retirement security had a common interest in moving forward. In the summer of 2000, Secretary of the Treasury Lawrence Summers and Secretary of Labor Alexis Herman held a joint press conference at which they committed their departments to facilitate broader adoption of automatic enrollment and other initiatives that would encourage employee participation in employer-sponsored retirement saving plans. Shortly thereafter, the IRS ruled that firms could apply automatic enrollment to existing workers as well as new hires. The Department of Labor took roughly coincident action to streamline regulations that could discourage firms from adopting automatic enrollment. Support for automatic enrollment and related programs has, if anything, grown since the early years of this decade. The Pension Protection Act of 2006, for example, provides a nondiscrimination testing “safe harbor” for plans that use various automatic enrollment strategies.

The modification of enrollment rules for employer-sponsored retirement savings plans suggests that policymakers have drawn two conclusions from the behavioral economics literature. One is that standard neoclassical tools such as subsidies are not the only way to affect behavior. The other is that modifying the way a decision is framed can have an important effect on the decisionmaking outcome. This insight has generated broad interest in understanding how public policies can affect decision frames.

Recent academic research has explored subtle aspects of how automatic enrollment plans affect the behavior of retirement plan participants. Some studies consider how automatic enrollment influences asset allocation decisions. Others explore the relationship between job tenure and 401(k) participation. The potential of default options to encourage particular types of behavior has also been extended beyond the enrollment context. Gale, Iwry, and Orszag (2005) propose defaults for a

number of stages of the retirement-saving process, including automatic rollover of 401(k) balances to an individual retirement account when an employee changes jobs and automatic annuitization at retirement.

Leading financial services firms have responded to the growth of interest in default policies by offering “lifecycle” mutual funds. These funds have been one of the most popular new mutual fund products of the last decade. Life-cycle mutual funds alter the asset allocation of a retirement saver automatically as she ages, thereby avoiding the need for any active rebalancing decision. Research on participant behavior in retirement saving programs, such as Samuelson and Zeckhauser (1988), suggests that most retirement savers never adjust the allocation of their account. This finding is surprising because asset price fluctuations generate substantial changes in asset allocation, which optimizing households might trade to offset. Moreover, many models of optimal household financial behavior suggest that age-varying asset allocation strategies are appropriate for many individuals.

Several significant factors contributed to make the transformation from research to policy application particularly prompt in the retirement saving case. First, the academic research on automatic enrollment was directly related to a subject of immediate policy concern. Policymakers were searching for tools that could increase private retirement saving, and promoting automatic enrollment in employer-sponsored plans emerged as one of their most effective options. Second, the empirical work on automatic enrollment was straightforward to interpret and very persuasive. Third, academics could offer a theoretical justification, one grounded in insights from psychology that seemed reasonable to non-experts, to explain why the empirical patterns emerged. This may have given managers and policymakers greater confidence in promoting automatic enrollment as a policy innovation.

2. The Challenge of Behavioral Welfare Economics

Behavioral economics was widely embraced by policymakers who were confident that a higher savings rate was an attractive goal. In many settings, however, recognizing the key insights of behavioral economics can make it difficult to draw firm conclusions about what constitutes a

welfare-improving policy. When preferences are subject to modification through education or other means, is there a natural default set of preferences to use for welfare economics? Behavioral economics also suggests that consumers may be altruistic, rather than the self-interested individuals assumed by standard neoclassical economic theory. Bernheim and Rangel (2007, 2008) offer a broad overview of these issues and other difficulties that surround behavioral welfare economics.

The first challenge confronting behavioral welfare economics arises from the finding that consumer choices are frame-sensitive and subject to influence by various environmental factors. This finding undermines the assumption of consistent and stable preferences that is central to neoclassical economics, and the associated reliance on revealed preference that is the touchstone of preference formation in neoclassical economics. One way to find a middle ground between behavioral and neoclassical economics is to search for some decisions for which it is possible to rely on consumer choices, and to embrace revealed preference analysis in these cases. An alternative approach must be developed in other settings, where external factors bearing on preferences seem more important. Some researchers suggest distinguishing between decision utility, the potentially imperfect preferences that individuals use in making their decisions, and “true utility,” the preferences that a benevolent social planner might assign to individuals when constructing a social welfare function. The difficulty with this approach is that virtually any observed set of consumer choices might be justified as coming from a decision utility that differs from true utility. Developing criteria for distinguishing these two sets of preferences in a manner that is neither arbitrary nor vacuous is a key challenge for further research.

The insights of behavioral economics can turn a standard welfare-analytic result on its head. Consider the problem of measuring the welfare cost of taxing a commodity like cigarettes. The standard public finance analysis suggests that the welfare cost of the tax is an increasing function of the absolute value of the compensated demand elasticity. Behavioral economists might argue, however, that cigarette consumption is the result of a past or present divergence between true and decision utility, which admits the possibility that each cigarette smoked has a negative impact on true utility. The case of cigarettes is particularly complicated

because in many cases, as a result of nicotine's addictive properties, the decisionmaking failure occurred when the smoker was a teenager but the choice is difficult to reverse even when he or she is a more rationally thinking but still chemically dependent adult. The behavioral economics approach may imply that a larger decline in cigarette consumption, after controlling for income effects, represents a larger *gain* to consumers rather than a larger welfare loss. Gruber and Kőszegi (2004) develop arguments like this in their analysis of cigarette taxes.

A second challenge to traditional welfare economic analysis arises from the finding that individuals may be altruistic. When utilities are interdependent in the way that most models of altruism suggest, then computing the change in social welfare associated with a policy change requires not only estimates of how the general equilibrium allocation of goods will be affected, but also estimates of the cross-effects of one person's utility on that of another individual. Rotemberg (2003) illustrates the implications of altruistic consumers for designing tariff policy. If the degree of altruism can be affected by framing and other factors, then the challenges to behavioral welfare economics become doubly complex.

Finally, a third challenge to behavioral welfare economics, and one that has received less discussion in the literature, concerns the behavior of policymakers and the way they process information and choose among policy options. The same behavioral biases and decision difficulties that consumers display are also likely to affect policymakers. This idea suggests that how policy choices are framed may affect policy outcomes. Some empirical evidence suggests that fiscal institutions such as balanced-budget rules and legislative supermajority provisions for the passage of bills that increase indebtedness affect fiscal policy. The position of a candidate's name on a ballot may affect the likelihood of winning an election. One suspects that the analog of automatic enrollment in the political setting, identifying a "default" vote recipient for all voters, would have substantial and much-decried effects. Standard choice-based microeconomic approaches to collective decisionmaking may not offer easy solutions to the challenges of ranking alternative policy outcomes, which suggests that behavioral political economy is a promising area for future research.

3. Future Directions

I shall conclude by offering several speculations about future developments in the field of behavioral economics and the role that behavioral economics will play in enhancing the development of public policy. First, I am confident that empirical researchers will continue to document behavioral anomalies that are inconsistent with the standard neoclassical paradigm of decisionmaking. This evidence will probably become ever more persuasive as a result of a move toward using controlled experiments within economics. This process will in all likelihood draw research attention to places where decisions do not fit the standard postulates of neoclassical economics. In some settings, these anomalies may be of little consequence, but in others, such as the retirement savings field, they may be substantively important. The significant findings should serve as starting points for future research.

Second, I expect that theoretical research on the foundations of behavioral economics will continue to advance. Researchers will continue to search for simple but general models that can explain a large fraction of the behavioral anomalies we observe. Today some view behavioral economics as a collection of interesting and convincing anecdotes about economic choices and a set of corresponding theoretical models that lack a well-organized unifying framework. There is a clear need to develop theoretical models that not only explain what we observe but that also can make predictions about outcomes in settings we have not yet observed. Whether such a unified model can be developed for behavioral economics remains an open question.

Finally, neoclassical economics will, and must, contribute to the advance of behavioral economics. The critical role for neoclassical economic theory is to find ways to expand the standard paradigm to see whether behavioral anomalies can be explained with minimal disturbance to traditional postulates. The case for modifying the standard neoclassical paradigm, or potentially replacing it, must hinge on a body of empirical findings which are extremely difficult to reconcile with standard theory. Traditional neoclassical theorizing is essential to determining just how difficult such reconciliation may be.

The retirement saving context illustrates the importance of trying to explain observed behavior in standard models. At least two potential mechanisms, one behavioral and one neoclassical, might be advanced to explain the substantial impact of default options on retirement saving. The behavioral explanation argues that decisionmaking costs lead to substantial inertia and that workers find it too expensive to invest in resolving whether or not saving in a 401(k) plan makes sense. This approach suggests that education about the importance of saving, or training that reduces the cost of making choices in a retirement plan, might affect retirement saving outcomes. The second explanation for observed behavior, which appeals to standard microeconomic theory, is that prospective 401(k) participants recognize that they do not know very much about retirement saving and that when offered a default to opt into the plan, infer that some more informed person—a human resources manager or a financial planner—has evaluated typical saving needs and distilled these findings into the default recommendation. If this explanation—which follows from neoclassical models with asymmetric information—is a more accurate account of why default options matter, then the natural way to affect decisionmaking may be to provide employees with more detailed information on optimal saving rates for particular age, income, and family structure combinations. The choice between these alternative explanations of what we observe thus matters for the design of policy to influence discretionary saving decisions.

There are many exciting research challenges that remain to be addressed using the tools and insights of behavioral economics. Policymakers are likely to rely increasingly on the empirical findings that emerge from studies in the behavioral economics field. I am confident that the future will bring continued fruitful interplay between research in behavioral economics and research in more traditional neoclassical economics, and that behavioral economics will continue to inform the design of public policy.

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Implications of Behavioral Economics for Monetary Policy

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I want to congratulate the Federal Reserve Bank of Boston for organizing a fascinating and thought-provoking conference. I applaud the Bank's decision to establish a center to promote and support research in behavioral economics and concur wholeheartedly with the judgment that motivates this initiative—namely, that research in behavioral economics is broadening and enriches our understanding of decisionmaking. This research has the potential to strengthen the conceptual and empirical underpinnings of macroeconomic policy.

The Federal Reserve is one of a growing number of organizations that have already taken to heart some implications of behavioral economics research. This year, we began to automatically enroll new employees into the Federal Reserve System's retirement savings plan, defaulting them into an asset allocation fund that includes fixed income, domestic, and international equity investments. Employees who do not want to participate can, of course, easily opt out of the program. But our early experience mirrors well-known research findings: so far, an overwhelming fraction of employees who were defaulted into the savings plan remain enrolled in it. Of course, this default choice reflects our System's appreciation of the striking findings of behavioral economics concerning the sensitivity of saving decisions to automatic enrollments.

In terms of the Federal Reserve's public policy responsibilities, I can easily envision other ways in which explorations in behavioral economics could be of practical use. For example, one of the Federal Reserve's duties is to design consumer disclosures, including the information that borrowers receive from lenders when they take out a mortgage, apply for a credit

card, or lease a new vehicle. As we have unfortunately seen recently, such disclosures have not always been effective in conveying the key information relevant to such decisions in a salient, understandable, and timely way. Indeed, recent research by the Federal Trade Commission¹ documents that a large fraction of mortgage borrowers fail to understand the financial implications of prepayment penalties and other complex loan features. To improve the effectiveness of such disclosures, the Federal Reserve has begun to use consumer testing techniques to redesign and refine these disclosures,² but there remains substantial scope for behavioral research to contribute to the design and implementation of more effective practices in the consumer disclosure area.

Today, however, I would like to focus on some implications of behavioral economics for the conduct of monetary policy. I will concentrate on the implications of behavioral research for the Phillips curve, though the other papers delivered at this conference demonstrate that behavioral economics has implications for many other aspects of macroeconomic modeling. These include the behavior of housing and other asset prices, as well as the specification of crucial components of aggregate demand, such as the consumption function.

The Phillips curve is a core component of every realistic macroeconomic model. It plays a critical role in policy determination because its components importantly influence the short- and long-run tradeoffs that central banks face as they strive to achieve price stability and, in the Federal Reserve's case, maximum sustainable employment—our second congressionally mandated goal. I will argue that behavioral economics can enhance our understanding of the Phillips curve, and that this refinement is important for two reasons. First, better models of the inflation process help improve our forecasts and clarify limitations on what monetary policy *can* achieve. Second, the theoretical underpinnings of the Phillips curve are important in understanding what central banks *should* do. In other words, beyond determining the constraints governing what monetary policy is feasible, macroeconomic models underpinning the Phillips curve have implications for the way in which central banks should interpret their price stability mandate and for assessing the welfare costs of fluctuations in output and inflation.

The New Keynesian model provides theoretical microfoundations for a Phillips curve that relates actual inflation to expected inflation one period ahead as well as to marginal production costs.³ This model has become a standard workhorse for policy analysis and provides loose justification for empirical implementations of the Phillips curve. These implementations typically relate actual inflation to lags of inflation (as a proxy for expected inflation), to a measure of the output or unemployment gap (which proxies for cyclical fluctuations in marginal cost), and to other variables reflecting supply shocks (such as the prices of energy and imported goods). The coefficient on the unemployment gap in the Phillips curve determines the slope of the short-run Phillips curve relationship between unemployment and inflation. This parameter is crucial for monetary policy because it influences the sacrifice ratio—the cost in terms of unemployment or lost output due to lower inflation. Virtually all empirical research on the inflationary process finds that the short-run Phillips curve is flat enough to generate a significant short-run tradeoff.

Of course, the existence of this empirical short-run tradeoff between inflation and unemployment also helped motivate the development of the New Keynesian model in the first place. In particular, with no frictions and with fully maximizing agents, markets should always clear, and the labor market should be no exception. Thus, the short-run Phillips curve “should be” vertical.⁴ This divergence between theory and reality was the original motivation for New Keynesian economics. But in contrast to the ad hoc behavioral assumptions underlying old-style Keynesian theory, modern researchers have amended the neoclassical model with well-specified assumptions concerning the nature of preferences, the process of decisionmaking, the frictions characterizing markets, and the details of expectation formation. The objective has been to build macroeconomic models on sound microfoundations that are not only rigorous but also realistic.

Viewed in this light, the now-standard New Keynesian approach explains the short-run Phillips curve tradeoff by introducing a key friction into neoclassical theory, namely, price stickiness; such a friction is often justified as a menu cost of changing nominal prices. The consequence is that firms change the prices they charge only periodically, not continu-

ously. With staggered decisionmaking across price-setters, the aggregate price level exhibits inertia, thus rationalizing the short-run Phillips curve tradeoff. Other frictions, such as wage rigidity and habit persistence in consumption, are typically added to improve the fit of the model.

Behavioral macroeconomic models have extended this agenda, both by providing new justifications for wage and price rigidity and by incorporating additional departures from the frictionless benchmark. Of course, the jury is still out on which modifications are most important empirically for understanding the macroeconomy. Nevertheless, the evidence presented throughout this conference regarding how individuals and firms behave is too compelling to simply ignore. Let me discuss a few examples of how behavioral macroeconomics augments our standard models used for policymaking.

Some behavioral models assume that people follow simple heuristics or rules of thumb that require relatively little cognitive effort or time, such as focusing on only a few salient details of a problem. Indeed, the psychology and economics literature that builds on the work of Kahneman, Tversky, and others generally concludes that people do not make decisions in the fully rational way commonly envisioned in standard economic models. As Benjamin and Laibson (2003) summarize the findings of this literature: “economic agents make good decisions but not perfectly rational ones” (2).

Other behavioral models, including those surveyed by Fehr, Goette, and Zehnder and by Rotemberg (both in this volume), go much further, arguing that individual behavior is affected by a reliance on nominal frames of reference and by considerations such as envy, fairness, social norms, and social status. As Rotemberg makes clear, such assumptions can also rationalize the phenomenon of price stickiness embodied in the Phillips curve.

Of course a logical question is why such additional complexities are worth incorporating into macroeconomic models if the New Keynesian approach, based on costly price adjustment, is empirically satisfactory. The problem is that the New Keynesian Phillips curve is *not* fully satisfactory. For example, it is not consistent with contractionary disinflations or with the inflation persistence observed in the postwar period. It also

is not consistent with empirical estimates of the joint responses of unemployment and inflation to monetary shocks.⁵

Behaviorally based macroeconomic models help address these concerns about the New Keynesian Phillips curve, notably by modifying the process of expectations formation, the feedback between expected future inflation and current inflation, the link between labor-market conditions and firms’ marginal cost, and the impact of supply shocks on the inflation process. These behaviorally informed macroeconomic models also offer new insights. For example, Mankiw and Reis (2002) assume that decisionmakers form expectations using sticky or stale information, an assumption they justify on behavioral grounds. To keep their model more tractable, they assume that all agents act as if they had rational expectations, but that most agents use outdated information when forming these expectations. With this amendment of the standard New Keynesian model, the Mankiw-Reis version generates a short-run Phillips curve that is downward-sloping and that is consistent with inflation persistence and costly disinflation.

Of course, the assumption of rational expectations, which Mankiw and Reis maintain, is a clear but probably unrealistic benchmark. Ball (2000) suggests, based on near-rationality, that perhaps people forecast with optimal univariate estimation rather than acting as if they knew the entire model.⁶ For the postwar period, this approach makes expected inflation close to being last period’s inflation—so expectations depend heavily on recent experience. Inflation is thus persistent, but this persistence is not structural. An important implication for policy is that, if policymakers change their behavior, the empirical dynamics of inflation could change markedly.

Let me next turn to the long-run properties of the Phillips curve. Most macroeconomists accept that the long-run Phillips curve is vertical, so that steady-state unemployment is unaffected by the average level of inflation. Intriguingly, some behavioral models raise the possibility that steady-state unemployment might depend on the inflation rate.⁷ For example, Akerlof, Dickens, and Perry (2000) explore the implications of a model with money illusion, a phenomenon which, according to surveys and other empirical evidence, appears to be both widespread and significant

in decisionmaking. In their model, when inflation is sufficiently low, most agents do not focus on the difference between real and nominal variables, so inflation is relatively unimportant for nominal wage bargaining and for prices. As real inflation rises, however, it becomes salient to a growing fraction of agents who take it fully into account. This hypothesis gives rise to a long-run Phillips curve that is bowed in at very low inflation rates, backward-bending at slightly higher rates, and ultimately vertical at the “natural rate” when inflation is sufficiently high. The implication is that a very small amount of inflation may lower equilibrium unemployment. Beyond a point, however, higher inflation raises equilibrium unemployment since inflation becomes an increasingly salient factor in decisionmaking. Akerlof, Dickens, and Perry (2000) argue that, in the late 1990s, as inflation fell to low levels, it became less salient to wage bargaining, reducing the effective natural rate of unemployment.

Closely related to the idea of money illusion is downward nominal wage rigidity which, as Fehr, Goette, and Zehnder (this volume) discuss, may reflect considerations of fairness. Pervasive evidence of such nominal rigidity was identified, for example, by the International Wage Flexibility project (see Dickens et al., 2007). As Tobin (1972) originally showed, such downward nominal wage rigidity means that at sufficiently low inflation rates, a significant fraction of firms would optimally cut nominal wages. This possibility is explored in another paper by Akerlof, Dickens, and Perry (1996). In their model, if productivity growth and steady-state inflation are low, then long-run unemployment might be relatively high. The reason is that some firms might need to cut real wages which, at very low inflation, requires nominal wage cuts. If they are unwilling or unable to implement such cuts, then these firms may lay off workers instead. This reduction in labor demand leads to an increase in unemployment. Of course, if productivity growth is high, as it has been on average since the mid-1990s, then downward nominal wage rigidity becomes a less important issue.⁸ Behavioral considerations thus point to the possibility of a long-run tradeoff between inflation and unemployment at very low inflation rates.

Downward nominal wage rigidity, as well as downward real wage rigidity, may also affect the linkages in the Phillips curve among unemployment, marginal cost, and inflation. In particular, norms governing

the pay increases that are deemed fair may result in a short-run Phillips curve that is convex rather than linear. The nonlinearity is due to the fact that even with high unemployment rates, firms are unwilling to treat workers in ways they consider unfair—either by cutting nominal wages or by raising nominal wages by less than workers think they should receive, causing inflation to “bottom out” as unemployment rises. For the United States, Clark, Laxton, and Rose (1996) find evidence of nonlinearity, although tests to discriminate among alternative functional forms of the Phillips curve suffer from extremely low power, making a reliable assessment of the degree of convexity impossible. The degree of convexity of the short-run Phillips curve is potentially important, however, because the volatility of unemployment and mean unemployment are inversely related along paths with constant expected inflation. This means that policies to stabilize unemployment produce the payoff of lowering it on average.

Another implication of behavioral economics for the Phillips curve relates to the impact of productivity growth on equilibrium unemployment when real wages exhibit some rigidity, a phenomenon found by the International Wage Project to be prevalent in many countries. Ball and Moffitt (2001), for example, have shown that shifts in productivity growth, like other supply shocks, can shift the Phillips curve and thereby change, at least for a time, the equilibrium unemployment rate, or the nonaccelerating inflation rate of unemployment (NAIRU). Behavioral economics suggests that social norms may govern the real wage increases that workers consider fair, and that these norms or aspirations may be historically rooted. Shifts in productivity growth make it easier or more difficult for firms to meet these norms, altering, at least for a time, the unemployment rate that is consistent with growth in real wages that is in line with productivity. During the 1990s, faster productivity growth enabled firms to more easily meet norms for real wage growth that were depressed by the post-1973 productivity decline. In this view, the sluggish upward adjustment of norms enabled unemployment to fall to 40-year lows without igniting inflation. In essence, the short-run NAIRU was below its long-run level. By contrast, the poor experience of the 1970s reflected the collision of inherited norms for rapid real wage growth with the unpleasant reality of a sharp productivity slowdown.

Let me conclude these remarks on the implications of behavioral research for the properties of the Phillips curve by noting that at least some of the behaviorally based insights have already crept into our internal analysis and forecasts. For example, Federal Reserve policymakers often attributed favorable inflation performance in the late 1990s to fast productivity growth and its effect on the short-run NAIRU. And policy simulations with FRB/US, the Board of Governors' main model, sometimes assume that agents form expectations by estimating reduced-form vector autoregressions rather than using model-consistent expectations. Moreover, issues related to communications and credibility figure prominently in Federal Open Market Committee discussions, because members recognize that well-anchored inflation expectations, as we have had in the United States since the mid-1980s, can reduce the sacrifice ratio and the sensitivity of inflation to supply shocks. More generally, the Federal Reserve recognizes that public understanding of its reaction function can help people form expectations in ways that are likely to enhance the stability of the economy. Given the importance that expectations formation plays in all aspects of modern macroeconomic models, I see a high payoff to further behavioral research on how people actually form expectations. Moreover, behavioral research could be very useful in helping us understand how best to communicate our views on the economy and on policy.

Having outlined how behavioral research affects our understanding of what monetary policy can do, I now want to address the question of what policy should do. Specifically, what we can learn about the appropriate objectives of monetary policy?

I will start with inflation. In the long run, everyone agrees that inflation primarily reflects the actions of the central bank. But what inflation rate should we strive for as a long-run policy objective? Existing theoretical work, grounded in neoclassical models, provides surprisingly little guidance. It points to the importance of shoe-leather costs, since individuals tend to economize on their use of cash as inflation rises. However, these costs are probably small at low to moderate rates of inflation. More important, in all likelihood, is the impact on the incentive to save and invest stemming from the interaction of inflation with the tax code. But findings from behavioral economics bring other considerations into play. Empirically, the evidence from surveys performed by Shiller (2007)

and those discussed by Di Tella and MacCulloch (in this volume) reveal that individuals strongly dislike inflation. It appears to reduce reported happiness. Such evidence, along with research suggesting that individuals heavily rely on nominal frames of reference in decisionmaking, reinforces the desirability of keeping inflation rates quite low. After all, zero inflation, correctly measured, means that the distinction between real and nominal variables is unimportant; indeed, targeting a constant price level would make it easier for people to plan for the future. However, some policy considerations highlighted by behavioral research point in the opposite direction. For example, the tendency of workers to ignore inflation in wage bargaining until it becomes salient and the prevalence of downward nominal wage rigidity suggest that there may be potential benefits from choosing an inflation target that is low but positive. These arguments reinforce a case for maintaining some small inflation cushion to guard against deflationary risks due to the zero nominal bound on interest rates. Although empirical work suggests that downward nominal wage rigidity is prevalent in the United States, its importance diminishes when productivity growth is high, as it has been since the mid-1990s.

Let me next turn to some implications of behavioral economics for the Federal Reserve's role in stabilizing the real economy. Along with price stability, output stabilization has been an important policy objective during the postwar period, and fluctuations in both output and unemployment have diminished. The questions for policymakers are how large are the welfare losses that result from such output volatility and how beneficial would further reductions be?

Perhaps surprisingly, standard economic theory suggests that the losses associated with output volatility of the magnitude experienced during the postwar period are quite small. Lucas (1987, 2003) spawned a large literature by arguing that the welfare gains from *additional* stabilization of the economy are tiny. Given standard preferences and the observed variance of consumption around a linear trend since 1947, he calculates that the representative American consumer would be willing to reduce his average consumption by a trivial amount, only one-half of one-tenth of a percent, to eliminate *all* remaining consumption volatility.⁹ Lucas concluded that stabilizing output, even if possible, should not be a macroeconomic priority.

If Lucas's calculation was correct, then the average person in the United States would value consumption stabilization (complete insurance) by only around \$16 a year.¹⁰ Compared with the premiums we pay for very partial insurance (for example, collision coverage on cars), this seems implausibly low. The New Keynesian model offers one basis to conclude that the stabilization costs may be larger. For example, Galí, Gertler, and López-Salido (2007) argue that because of wage and price markups, steady-state employment and output are inefficiently low. In their model, the welfare effects of booms and recessions are asymmetric because marginal increases in employment result in diminishing welfare gains. In good times, with low unemployment, the marginal gain from additional job creation may be low, because marginal employees may be close to indifferent in choosing between work and leisure. In contrast, job creation in bad times may yield a sizable welfare surplus. As a result, recessions are particularly costly—welfare falls by more during a business cycle downturn than it rises during a symmetric expansion. If good policy can reduce the frequency and severity of recessions, then the analysis by Galí, Gertler, and López-Salido suggests that the resulting welfare gains from stabilization may be substantial.

Behavioral considerations suggest some additional reasons why output stabilization may raise welfare. In particular, some of the behavioral phenomena already discussed create the tantalizing prospect that a more stable economy may benefit from higher average levels of employment, output, and consumption. As DeLong and Summers (1988, p. 434) once put it, stabilization might “fill in troughs without shaving off peaks.”¹¹ Or, as in Barlevy (2004), stabilization might increase the economy's long-run growth rate. In contrast, both the neoclassical model, analyzed by Lucas, and the New Keynesian model, analyzed by Galí, Gertler, and López-Salido, predict that mean consumption, output, and unemployment are unaffected by the volatility of these variables.

One behavioral reason that a more stable economy might enjoy lower average unemployment relates to the convexity of the short-run Phillips curve. If this relationship is convex, rather than linear, higher volatility in unemployment is associated with a higher mean unemployment rate. Recall that such convexity could reflect the influence of downward rigidity in either nominal or real wages. Interestingly, using U.S. data for

the period 1971 to 1995, Debelle and Laxton (1997) estimate that the increase in mean unemployment associated with the volatility in unemployment over this period amounted to a nontrivial 0.33 percent.¹² Yellen and Akerlof (2004) show that a similar argument applies if the long-run Phillips curve is not vertical at low inflation rates.

For policymakers, the bottom line of such research is that behavioral economic models tend to reinforce the priority that policymakers should attach to the goal of stabilizing output. But the magnitude of any potential welfare gains is difficult to infer from existing empirical estimates of the Phillips curve. In principle, the happiness literature might give us some more direct evidence on these benefits. As Di Tella and MacCulloch emphasize, there is persuasive evidence that people's happiness is inversely correlated with both unemployment and inflation. The finding that lower unemployment raises satisfaction even when it is fairly low to start with is consistent with the New Keynesian assumption that equilibrium unemployment is inefficiently high. But this finding sheds little light on how policymakers should assess the welfare consequences of business cycle fluctuations—a wider assessment hinges on the more subtle issue of how volatility in unemployment affects well-being for a given mean. Regarding this point Wolfers (2003), using subjective measures of satisfaction, found evidence of nonlinearity in the relationship between life satisfaction and unemployment, a result implying that unemployment volatility does undermine a sense of well-being. Even so, Wolfers found that the welfare benefits of reducing volatility are subject to rapidly diminishing returns, so that further reductions in the volatility of unemployment would raise welfare by only a relatively small amount, albeit by more than Lucas's estimate.

There is a lot more work to be done to validate and confirm that happiness responses do correspond to well-being.¹³ In addition, we care about more than just whether people are happy; we'd like to understand why they are happy. There is considerable scope for additional refined survey evidence that focuses more precisely on what it is that individuals dislike about unemployment and inflation, and the reasons behind this aversion.¹⁴

Let me conclude by summarizing what I think policymakers can learn from behavioral research bearing on the Phillips curve. This research pro-

vides clear-cut evidence that people's behavior often deviates from the way that benchmark neoclassical theories assume they will act. Individuals have money illusion, follow heuristic rules of thumb, and care about issues like fairness and equity. As I've discussed, there is a growing body of literature showing that macroeconomic theories built on behavioral foundations have strikingly different implications from those predictions that follow from more standard theories. Behavioral research thus offers the promise of unified theories that can explain microeconomic behavior as well as the movements of macroeconomic aggregates.

With respect to the Federal Reserve's dual mandate, behavioral research supports the view that inflation is costly, although very modest inflation might help protect against downward nominal wage rigidity. Behavioral macroeconomic models also provide theoretical underpinnings for the view held by most policymakers that, in the short run, monetary policy can and should strive to stabilize the real economy.

In sum, research on behavioral economics is as exciting for policymakers as it is for academics. It helps policymakers understand what they should care about and improves the quality of our economic models. The work at this conference highlights some of the progress that has been made, but also suggests that the marginal product of further research in behavioral economics is still likely to be very high.

■ *I am deeply indebted to staff in the Economic Research Department of the Federal Reserve Bank of San Francisco, and most particularly to John Fernald, for their help in preparing these remarks.*

Notes

1. See Lacko and Pappalardo (2007).
2. See Kroszner (2007).
3. The New Keynesian intuition for such a relationship is that firms that are readjusting their prices today will want higher prices if the marginal cost of production is relatively high—but they are also concerned that they might be unable to change their price in the future. Hence, if they expect inflation to be high in the future, they will want to raise their price by more today in order to keep from being stuck with a price that is too low.

4. In a simple version of the New Keynesian Phillips curve, Mankiw (2001) shows that the slope of the curve is $\alpha\lambda^2/(1-\lambda)$, where λ is the fraction of agents that adjust their prices each period and α is the response of the desired real price to movements in the unemployment gap, with a small value of α reflecting greater real rigidity. With perfectly flexible wages and prices, $\lambda = 1$ and the curve is vertical.

5. Mankiw (2001) highlights these critiques.

6. A key motivation for Ball (2000) is that inflation appears very persistent in the postwar period but not persistent under the gold standard, which was a very different monetary regime. Common features of New Keynesian models, such as backward-looking agents or price indexing, can yield more persistence but not its apparent regime-specific nature.

7. Technically, in standard Phillips curve models, this relates to whether the coefficient on expected inflation is 1.00.

8. Recent productivity data have been, on balance, weaker than the average since the mid-1990s. But most, if not all, estimates of trend productivity growth remain above the average growth rate from 1973–1995.

9. As Lucas (2003) makes clear, even taking his estimates at face value, such a calculation does not imply that the Federal Reserve should ignore fluctuations. Very long, very deep downturns, such as the Great Depression, are costly, and policy has avoided such episodes during the postwar period, presumably averting sizable welfare costs.

10. Reis (2007) suggests this way of framing the benefits of stabilization.

11. Yellen and Akerlof (2004) survey this literature.

12. 0.33 percent is the estimated difference between the average historical rate of unemployment and the deterministic NAIRU, defined as the unemployment rate consistent with nonaccelerating inflation in the absence of shocks.

13. Responses do appear correlated with things like income, employment status, education, marital status, and so forth. And there is some evidence that these measures are, in turn, mirrored in suicide data (see Daly, Wilson, and Johnson 2007), which is clearly of a very objective nature.

14. Shiller (1997) took this approach in asking people about inflation.

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Behavioral Economics as “Psychologically Informed” Economic Inquiry

Lawrence H. Summers

Central banking is about being consistent with expectations, and so I will endeavor to be mildly provocative in my comments. First, a comment stimulated by what Jim Poterba said, nothing which I exactly disagree with, but I will present a different and simpler perspective on it: people don't actively tend to opt in or out because they find it costly to do so. This is the case in a vast range of settings and was actually quite widely known before research in behavioral economics began. To take just one concrete example, why was the book club invented 75 years ago? And why does the book club, which basically pays you a bunch of money up front by giving you four books for free and then stipulates that you are not required to pay for a single thing, persist to this day? The rules are that they send you the book or they send you the slip saying they are going to send you the book, and if you don't send the slip back, which means you are opting out, you are in the book club. You get the book they send. People are willing to pay you a small fortune to get you in a situation where you have to opt out of what they want you to do.

The ability to opt in or opt out is a crucial issue in modern financial legislation and is a major point of contention between the Americans and the Europeans. Everybody agrees that you have a right to privacy with respect to your financial information. Everybody agrees that some people would rather be marketed to intelligently rather than unintelligently. So if my credit card bill reveals that I'm interested in tennis and I'm not interested in fashion, then I would prefer being marketed products that are related to tennis. So the question is, can your credit card information be used for direct marketing appeals? The American position is that opt out is a very good policy. If I want to have the information remain

private, I can choose to have my information remain private—but the default position is that in the absence of my opting out, my credit card transactions can be used for marketing purposes. The European position holds that actively opting in is the right policy. I will assert that there has been no important context where it has ever been observed that people are not reluctant to opt in or out for which the framing of when opting takes place does not matter. If this conjecture is close to being right, it makes me much more skeptical of the context-by-context attempt to find rather tortured neoclassical economic explanations for this behavior when I think there's a simple overriding rule that actively opting in or out is costly to individuals.

My second observation is that I have always been puzzled by the term “behavioral economics.” I understand what macroeconomics is because there is an alternative which is microeconomics. I understand what international economics is because there's an alternative, which is studying a closed economy. So I understand what most of the economic subfields constitute. Yet I have kept trying to figure out what “nonbehavioral economics” would be about, and the best I can do is to assume that it would seem sort of goofy to call your subdiscipline “irrational economics.” But there is an oddity in the name “behavioral economics,” since I would surmise that all fields of economic inquiry essentially study behavior. Paul Samuelson could talk about this point in a much more learned way, but I will assert that what is really important about scientific revolutions and new paradigms is not so much the new answers they give to old questions, but the new questions that become possible to investigate, examine, and debate in systematic ways. One of the reasons why Keynes's *The General Theory of Employment, Interest, and Money* was such a successful book was that two generations of graduate students could go estimate consumption functions, investment functions, and all kinds of functions that economists could not investigate before. Reading the papers presented at this conference, I was a bit struck that many of them seemed to be oriented towards asking the kinds of questions that economists have talked about for a long time. In effect, the modus operandi is to give a different answer by appealing to behavioral economics, which considers issues that economists traditionally have thought they didn't have much to say about—but that might be amenable to an analysis that is psychologically

informed by something other than the classical model predicated on utility functions. I've got five examples to discuss.

First, the question of transparency in central bank communications. Here's what an anthropologist who came from Mars would observe: virtually everyone who deals with the subject speaks in favor of something called transparency and clear communication. The second thing the Martian anthropologist would observe is that there is a near-complete taboo among even the most enthusiastic advocates of transparency regarding the quantitative calibration of uncertainty. Some great discussions would delve into whether an event was possible, probable, had a high likelihood, or was a plausible outcome but that no responsible soul in an official capacity would be prepared to provide a numerical calibration of the likelihood of any potential event. You never hear anyone from the Federal Reserve say that in our judgment, there is a one-third chance, or there's between a 33 and a 50 percent chance, of a recession—even the people who are most enthusiastic about transparent communication regarding monetary policy. It is, I believe, the policy of every central bank in the world, whether admitted or not, that the central bank's governor speaks publicly about policy and speaks anonymously (if somewhat less frequently perhaps), off-record to a reporter like John Berry, or their journalistic equivalent of Berry, with the explicit understanding that the views will be disseminated but not in the name of the central bank. Are these practices wise? I don't know the answer. What theory of achieving an objective is served by this dual policy of direct versus anonymous communication? I don't know the answer to that question either. If I had to guess, since these practices have evolved fairly universally and separately, they have a fair degree of functionality. Whether that functionality is in achieving national shared objectives or more particular objectives of the central bank is less obvious to me, but the question seems highly amenable to investigation using the perspective of what I will call psychologically informed economics.

Here's the second puzzle. What about the very great transitory efficacy of what might be called “cheap talk?” Say that the U.S. Treasury Secretary made the following observation, which would follow naturally from standard economics textbooks (whether written by Paul Samuelson, Greg Mankiw, or Robert Barro): the economy has slowed, the

dollar has fallen, and some of the lost demand from the slowing economy will be made up by increased exports coming from the weaker value of the dollar. I promise to a moral certainty that if the U.S. Treasury Secretary said that, roughly \$100 billion of wealth measured against a global numeraire would be lost by those who are currently holding dollar assets within the space of 20 seconds. Why? Because they thought he might trade in the dollar on a scale that would be a quarter of the normal size of the fifteenth largest hedge fund in New York? Because he might whisper something to the independent central bank that might do something about it? I don't know, but there are many such examples of talk influencing the markets. When the Dow was at 6,400, why did Alan Greenspan's observation that markets sometimes have a tendency towards irrational exuberance have a profound impact on the level of the stock market? It is clear that exhortation and commentary are thought to be an important part of the arsenal of financial policymakers. What is that all about? Behavioral economics should have something systemic to say about this question.

As the third example, Alan Blinder has been engaged in some research that points to what the questions are but for which dispositive answers have yet to be provided: Why is it that we think that the job of providing civilian control of the armed forces should be done by one person, but that the job of setting monetary policy should be done by a committee of seven headed by one figure who is deemed to be dominant though given almost no statutory power? Is there something different about monetary policy? Should everything be done by a committee? Is it wrong in the case of monetary policy to do the things in the way we do them? If we're going to do it this way, should we do what the previous governor of the Bank of England famously did, which was to observe that he always voted last and never lost, or what the current governor of the Bank of England does, which is proudly announce that it's a committee process, and sometimes he gets his way, and sometimes he doesn't? What is the right way to achieve our objectives? This is a similar situation where it seems to me that some understanding of the less purely neoclassical aspects of human behavior would shed light on the issue.

Fourth, there is the choice between multiple equilibria. Here's a game for all of you to play. I want each of you to think about this question. You

can play strategy A in which case you will neither pay nor receive any money or you can play strategy B. If everyone in the room plays strategy B, everyone will get \$100. If anybody in the room does not play strategy B, then those who play B will lose \$500. Everybody understand what the game is? How many would choose strategy A? How many people would choose strategy B? Both outcomes are Nash equilibrium if everybody does them. Strategy A is the so-called dominant Nash equilibrium strategy, and yet it is not what all of you play. Now let me change to a game where the set of Nash equilibria is essentially the same. If less than 15 people in the room choose strategy A, strategy B pays off \$500. Only if more than 15 people choose strategy A, will those who play strategy B will lose \$500. Now, what would you choose? How many people would choose A? How many people would choose B? This kind of reasoning must have something to do with why when a bank has more reserves, a multiple equilibria run is less likely, and when a bank has less reserves, a multiple equilibria run is more likely. But what's special about 15? How does it depend on the full context of factors? As I just illustrated, it's clear that using a dominant Nash equilibrium truly gets the wrong answer. But ascertaining what decisionmaking process determines what happens must be an important behavioral aspect as well.

For my fifth and final example, how do we really control principal-agent problems in the real world? I don't know whether it's a one-dimensional infinity or a two-dimensional infinity of stuff about incentive structures and the design of an optimal contract optimally deployed, or a principal-agent equilibrium with one principal and three agents, or seven principals and two agents, or whatever. What Weber had to say about the subject was that it was really important to have professions with professional norms and professional ethics because then doctors would not be respected by other doctors if they performed more operations on their patients in order to make more money—so the imposition of norms would control the principal-agent problem. The paper that had the greatest influence on my thinking about banking regulation in the last 20 years was the one by George Akerlof and Paul Romer that made the point that I wasn't worldly enough at the time to appreciate. Namely, that for every bank that decided to take advantage of the FDIC put and ramp up its volatility so that it could earn higher profits—because it was heads we

won, tails you lost—for every case like that, there were 50 banks where nobody was watching very closely, and so the guys running these banks decide to lend a lot of money to their brothers-in-law or to pay a really big dividend or to sponsor the mother of all golf tournaments. How do we control for that kind of behavior? How do we set and establish norms that reduce those kinds of inherent risks? I don't know the answer to that question either, but what these five examples all have in common is that they raise really important issues. These are questions that you almost cannot begin to talk about within a neoclassical paradigm, and yet it seems to me that the kinds of considerations that might or might not be important for the types of behaviors we consider standard are almost dominant here.

A final observation: if the Federal Reserve Act were being legislated today, there would be no consideration of having 12 regional banks. But one of the virtues of having 12 regional Federal Reserve banks has been that over time it has been possible for some of the banks to develop distinctive research perspectives and to become centers of thought of a particular kind. My own view that the monetarist St. Louis Fed has said much that was new and much that was true, and that something close to that impact could be said of the Minneapolis Fed acting as a center of rational expectations in economics. I have no doubt that they have made an enormous contribution to the quality of the policy debate and the range of the perspectives that are open for consideration by the economics profession and in the setting of monetary policy. I humbly submit that the Boston Fed, having made an excellent start with this conference, consider adopting a distinctive thrust around behavioral economics through what they do in their research department.

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