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 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 neoclassical 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 they 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 issueed 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 discusses several public policy prescripions 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 companies 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 employee 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 selfish 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 convincing. 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 coauthors) 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 Moffitt (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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