Interview

Justin Wolfers

Classical economists such as Adam Smith and John Stuart Mill were interested in a wide range of issues that for later generations of economists were thought to be largely beyond the scope of their discipline. What makes people happy? What gives our lives meaning? How ought we to organize ourselves as a polity? Relatively recently, a number of economists have started to revisit those questions, to place economics squarely within the broader social sciences, where it was once understood to belong, while at the same time not eschewing the formal tools that have given economics so much of its analytical power. The work of Justin Wolfers, an economist at the University of Pennsylvania, exemplifies this broadening scope of inquiry. As stated on his faculty Web page, his research interests include labor, macro, political economy, economics of the family, social policy, law and economics, public economics, and behavioral economics. One research area not listed is monetary economics. However, he also has contributed to that field, both through his academic research and his professional activities. A native of Australia, he has worked at the Reserve Bank of Australia and is currently a visiting scholar at the Federal Reserve Bank of San Francisco. Wolfers also is a nonresident senior fellow at the Brookings Institution in Washington, D.C., where he is co-editor of the *Brookings Papers on Economic Activity*, and a research associate at the National Bureau of Economic Research. Aaron Steelman interviewed Wolfers at his office at the University of Pennsylvania in May 2010.

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RF: Could you please talk about your work with Betsey Stevenson on the recent decline in self-reported happiness among women? What may explain that drop and what does this tell us about subjective measures of well-being?

Wolfers: We organize the alternative hypotheses into three categories of explanations. The first is that women's measured happiness went down following the women's movement — and this shows that the women's movement was somehow a bad thing. The second is that our finding tells us something about measurement problems with happiness research. If most of us believe that the women's movement was good for women, but the happiness data say that it didn't make women happier, then there is a problem using subjective well-being to measure large-scale social change. There are lots of versions of this story. One is that the way women have answered the question over time has changed. Another may be that when you ask people how happy they are, they think about it in relative terms. Perhaps back in the 1970s, women were reporting how happy they were compared to the lonely housewife next door, and today they are reporting how happy they are compared to the man who has the corner office that they should have. Another version would be that when you report how happy you are, your report is heavily influenced by those domains of your life where you feel that you are doing badly. This is Betsey's preferred explanation. The number of things that women are involved in has greatly expanded over time, which means that there are more chances of failing. The third category suggests that there is a puzzle for social scientists. We simply don't know why women's reported happiness has fallen following the women's movement. When you ask most economists how things have changed for women over the last 40 years, most will describe it as a triumph for women. Wages have increased, social and legal protections have improved, technological change has arguably been gender biased in favor of women. The choice set of women has expanded, and according to neoclassical economics this is an unambiguously good thing. But it could be that our finding tells us that there's some other even more important

factor in the background. For instance, declining social cohesion or rising risk could have had a disproportionate effect on women relative to men.

Betsey and I are working on another paper that looks at another great social movement of the second half of the 20th century: the civil rights movement. The women's movement coincided with a decline in self-reported happiness. But for African-Americans, self-reported happiness increased greatly. There was an unconscionably huge gap between the happiness of blacks and the happiness of whites in the 1970s. Today that gap is large, but has declined very substantially. This is interesting, because most of the major civil rights legislation had already been passed by the 1970s, the period where the data begin. So it suggests that changes in attitudes — a decline in racism, for instance — have had a very positive effect on the lives of black Americans.

RF: Should policymakers use happiness as a metric when deciding policy or should they use other measures that we tend to think of as more concrete and which we have traditionally considered to be the proper things to focus on, such as economic growth?

Wolfers: I think the first piece of advice is that policymakers should not abuse happiness research. There was a view, for instance, that economic growth was unrelated to happiness — or actually might impede happiness. That just turns out to be false. So one useful role of social scientists here is to knock over canards. That said, I am still optimistic that there is something useful that can come from happiness research. (Also, I should note that I prefer the term "subjective well-being" to "happiness" because I think it gives a broader measure of how people perceive their circumstances.) The female well-being paper suggests that the trend moved in a puzzling direction during one period of time. But other results are more conventional. If you look across countries, it is absolutely astonishing how closely subjective well-being tracks objective measures. And if you look across countries, the correlation between the level of GDP per capita and the average level of life satisfaction is about .8, which is one of the highest correlations you will see in the social sciences.

In his presidential address this year to the American Economic Association, Angus Deaton made a somewhat obvious but important point. What we normally think of as objective measures of well-being are in some ways subjective. If we want to compare per capita GDP in the United States to that in Burundi, it's easy to measure the number of dollars, but then we have to compare the different price levels. And then do we use the consumption basket of a typical person in the United States or the consumption basket of a typical person in Burundi? And what is the social meaning of owning what is considered a pretty standard good in the United States compared to what is considered a luxury good in Burundi? So there is a level of technical difficulty in getting these things right.

A related point is that the objections we have to subjective measures of well-being are often quite similar to objections we could raise about "objective" measures. How do we measure subjective well-being? We go out and ask people how they feel. How do we measure the unemployment rate? We go out and ask people. You might object that happiness is a social construct. But if you ask someone if they had gone out and looked for work in the last four weeks, there's a lot of ambiguity too. Similarly, corporate profits sound like a pretty objective measure — until you talk to an accountant. So the value of subjective well-being is that it measures something we really care about. Those measures may be flawed and you can point out how they might be improved, but we should inquire whether people are satisfied with their lives.

The first generation of people doing subjective well-being analysis was very motivated by it, and sometimes their work has the feeling of religious revival. But the second generation of people involved in this area of research has been able to take a step back and ask some of the difficult methodological questions we discussed. But why should it necessarily interest economists? One answer is market related: Some people are going to do it. Why not economists? Our friends in psychology, sociology, and political science are doing it. And it's turned out to have enormous political resonance; for instance, consider the Sarkozy Commission. So this will be part of the policy discourse and, as economists, we have to decide whether we are going to be part of that policy discussion. I think we bring two things to the table. We bring very precise and useful models of human behavior that can help us interpret well-being data. And we bring some statistical savvy that, frankly, has been missing.

RF: Your previous answer touches on this, but it may be useful to ask it explicitly: What do you think of the Easterlin Paradox — the idea, broadly speaking, that increases in income are not particularly well correlated with happiness?

Wolfers: In some sense, we all seem to want the Easterlin Paradox to be true. We want to think that people are made happier by seemingly loftier ideals than becoming wealthier. As I noted, it turns out that it's just not true. Income has a huge effect on people's happiness.

It's also been asserted that there is some level of income that satisfies most people's desires — and that there is little point in striving to get above that number because it won't make you happier. That number is often given as $15,000 annually. That's a very widely held view, but as far as we can tell there has never been a formal statistical test of that view. So Betsey Stevenson and I went through every data set we could find to test it, and there is no evidence that an increase in income — at any point — stops making people happier. That's true for the very rich as well as the very poor. A 10 percent increase in income yields the same bump in

happiness, whether it's from $400,000, $40,000, or $4,000.

RF: You noted in a recent blog post that despite being the "queen of the social sciences," books by economists are not frequently cited in scholarly journals across the board. It is true that the economics profession rewards publication of a book less than do many other disciplines — for example, a few good journal articles are more likely to help an economist get tenure than a book — but, still, economists do publish books, both with academic and large commercial houses. Why do you think the citation count for those works is relatively low?

Wolfers: I still believe that economics is the queen of the social sciences. But the metric that leads me to say that is its influence on the world, which is what I think social science should be about. When it comes to almost any public policy problem, you call the economists. This is true in many areas once thought outside of the domain of economics, such as family policy and understanding politics. Economists have been very successful moving into those fields and have provided many important insights.

There are very few economics books that are widely cited across all of scholarship. The likely explanation for this is the body that one calls "all of scholarship" is dominated by the humanities, and people in the humanities don't cite economics very often. But the humanities don't have much influence, either. There may well be a poet laureate of the United States, but there is not a Council of Poetic Advisers. So we are unpopular with those who don't have much influence. I don't see much problem or tension with that.

The broader issue — the reason why I believe economics is the queen of the social sciences — is this movement of economics beyond GDP. It is hard not to think of Gary Becker as the founder of that, and this has been a very good thing. In sociology, I think our biggest influences have been on research about family or crime, as economists have done a lot of empirical work on those topics. With political science, on topics from election forecasting to political economy, we tend to see quite good empirical work from economists. That's not to say that we should ignore other research methods. In fact, I had a sociologist on my dissertation committee, Sandy Jencks. I used to joke with Sandy — and he promised not to be offended — that sociologists have great questions and economists have great answers.

What is interesting to think about are the terms of trade between economics and all these other disciplines. We are clearly a net exporter to political science and sociology. But at this point the trade with psychology is almost all one way. We are a near-complete importer. I wonder why we haven't been bigger exporters to psychology. I think it has to do with the research method. Like political scientists and sociologists, economists are almost all about the analysis of observational data. And then there are second-order differences. Formal political scientists write down a model before they observe data; informal ones don't. Ethnographers

observe four people; survey researchers observe 4,000. But it's all observational. But when I watch and speak with my friends in psychology, very little of their work is about analyzing observational data. It's about experiments, real experiments, with very interesting interventions. So they have a different method of trying to isolate causation. I am certain that we have an enormous amount to learn from them. But I am curious why we have not been able to convince them of the importance of careful analysis of observational data.

RF: Becker and others have long argued that discrimination is costly to firms and that in order to engage in it the leaders or shareholders of those firms must have a "taste" for it. What does your research on the gender composition of CEOs tell us about that claim?

Wolfers: The standard neoclassical approach doesn't fully allow for what I think most people really believe discrimination to be: a mistake. With mistake-based discrimination, imagine that you go to evaluate the future profitability of a firm. One of the things that you are going to look at is the quality of the CEO. You probably have a mental picture of a tall white guy in a pinstripe suit, and if the CEO doesn't fit that image you may have a less positive opinion of that firm. If that is true, firms headed by women should systematically outperform the market's expectations. The first paper was somewhat inconclusive; it wasn't clear whether the firm overall outperformed expectations. Alok Kumar and I are working on a follow-up paper that uses quarterly earnings announcements, which gives us a lot of observations. It turns out that female-headed firms beat analysts' expectations each quarter much more frequently than similar male-headed firms. If you look at which analysts are getting things wrong, it's disproportionately male analysts who have inaccurately low expectations of female-headed firms. That's not true of female analysts; female-headed firms actually do not beat the expectations of female analysts. This, then, suggests what we see are mistakes, not tastes. These analysts do not want to get a reputation for poor forecasts; they are not trying to lose money. In fact, one of the ways you can test whether what we observe are mistakes is to ask people if they would be willing to change their behavior when presented with the data. And whenever I teach this paper to my MBA students, many of whom are former analysts, they say that they are going to change their behavior when they get back to the real world. So this is just a bias that is in the back of their minds, and when they understand the implications of that bias they want to rid themselves of it.

RF: Could you explain what a prediction market is — and in which areas of business and policy you think that prediction markets have the most promise?

Wolfers: It's simply a betting market, really. You choose an event and bet on whether it will occur. The simplest

example is: Who is going to win the next presidential election? The value of this approach is that it is a way of eliciting expectations.

A lot of people ask: Are prediction markets accurate? I think a more useful question is: Are prediction markets better than the alternative? So, for instance, in presidential elections are prediction markets more accurate than the Gallup Poll? The answer is yes. In nearly every head-to-head comparison between prediction markets and some alternative, prediction markets have turned out to be at least as accurate.

Still, a lot of social scientists, policymakers, and businesspeople seem reluctant to use prediction markets. I think there are several barriers to their adoption. One is legal. Betting on events is generally not legal in the United States. So most of the interesting prediction markets are operated offshore. Another is that the United States does not have a gambling culture. In contrast, in Australia, my home country, we will bet on virtually anything. Betting on whether something will happen is simply a natural part of our language. Third, in order to listen to the results of a prediction market you have to be willing to accept that the market is smarter than you are. That requires a lot of humility — and a fair bit of knowledge of how markets work. When someone asks me who I think will win the next election and by how much, I look up the prediction market and I state that number exactly, which means I have to give myself no credit for knowing anything about politics beyond that info embodied in the prediction market price. Most people are not very good at this. They tend to be confident in their individual ability to predict outcomes, even in areas where they may not know much.

In order for prediction markets to be useful in business, for example, the CEO has to be willing to listen to them, and CEOs tend to be men of action who are quite reluctant to admit the limits of their knowledge. Also, think about what middle management is in most firms. They tend to be information monopolists. Their analysts do the research and report it to them and then they decide whether to present it to the CEO. With a prediction market, everyone on the shop floor could give an opinion and that information would go directly to the CEO. That would undermine middle management's role as an information monopolist, so they are reluctant to adopt prediction markets.

As for where prediction markets are useful, I think there is a wide range of opportunities in business. Any business would like to forecast next year's sales, and it appears that prediction markets are very useful at doing that. No company or policy organization has fundamentally changed

Justin Wolfers

➤ **Present Position**

Associate Professor of Business and Public Policy, The Wharton School, University of Pennsylvania

➤ **Previous Faculty Appointment**

Stanford University Graduate School of Business (2001-2004)

➤ **Education**

B.Ec. (1994), University of Sydney; A.M. (2000) and Ph.D. (2001), Harvard University

➤ **Selected Publications**

Author or co-author of numerous papers in such journals as the *American Economic Review*, *Quarterly Journal of Economics*, *Journal of Political Economy*, *Journal of Monetary Economics*, *Quarterly Journal of Political Science*, *Journal of Legal Studies,* and *Science*

its management or operation structure by using prediction markets. But there are some firms like Google that have people researching prediction markets and use them for some purposes. In policy, at the Federal Reserve I assume that Ben Bernanke has a Bloomberg terminal in his office and looks at what's happening with interest rate futures. What are interest rate futures? They are a prediction market on the likely path of interest rates. Similarly, when economists at the Fed want to put together a macro model, they put in some assumptions about oil prices. In order to do this, they look at how oil futures are trading. What are oil futures? They are prediction markets on the future path of oil prices. The same is true with foreign exchange markets and so on. So prediction markets are being used, but we don't necessarily call them prediction markets in these cases.

RF: If prediction markets are such a powerful tool, then why weren't we able to use them to more effectively see that, say, the run-up in house prices was unsustainable or that (related) large problems in the financial markets were likely?

Wolfers: We should acknowledge that all mechanisms of aggregating information are imperfect. So you do see bubbles, manipulation, noise trading, volatility, and so on. Despite that, as an empirical statement, in every head-to-head comparison, prediction markets tend to do better than the alternative. As an illustration, I co-authored a paper a few years ago that looked at a short-lived market called the "economic derivatives market," where you could bet on non-farm payrolls, retail sales, unemployment claims, business confidence. The way we normally forecast these things is we call 30 forecasters and we determine the consensus. It turned out that prediction markets did a better job than the consensus.

Would this be true in housing? I don't know. We could run the experiment and find out. Still, we know that markets were wildly optimistic in predicting the future path of house prices. But think about the alternative: So were most of the analysts. If you had surveyed analysts rather than relying on markets, you would have run into the same problems. So it's not clear to me that markets failed us in the case of housing considering the alternative. They didn't do a great job, but they didn't do worse than the alternative of asking analysts. The evidence so far suggests that markets are the least imperfect forecaster. There may be settings where that is not true, but I have not run across them.　　　**RF**