

What Legal Scholars Need to Know About Economic Research on Taxation: The Evidence Thoroughly Debunks the Conventional Wisdom

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Peter Diamond & Emmanuel Saez, [The Case for a Progressive Tax: From Basic Research to Policy Recommendations](#), 25 *Journal of Economic Perspectives* 165 (Fall 2011).

Too often, policy research relies more on the misleadingly elegant results of economic theory than on actual evidence. Tax policy discussions, as I will describe below, are especially prone to being infected by this evidence-free approach to analysis. Fortunately, two of the best public finance economists in the world, Peter Diamond and Emmanuel Saez, have recently provided a much-needed antidote: *The Case for a Progressive Tax: From Basic Research to Policy Recommendations*. To understand just how important their article is, it is necessary to appreciate the deep roots of the problem that their article addresses.

As an economics graduate student, and later as a young economics professor, I often felt a deep sense of unease about the disconnect between economic theory and the empirical research that was relevant to evaluating that theory. Overwhelmingly, we learned in classes (and from theoretical scholarship) a series of “known results” that followed from the manipulation of economic models—results that, however nicely derived from the assumptions of those models, either were not backed up by any empirical research, or the magnitude of which turned out to be quite trivial.

For example, in macroeconomic theory courses (both those that were required of all first-year graduate students, and those that were only required of those of us who planned to specialize in macroeconomics), we learned that nearly all macroeconomic models included a deceptively simple assumption: Increases in the real interest rate (r) cause decreases in real business investment (I), and decreases in r lead to increases in I . If I is desirable, therefore, r should not rise.

This was, moreover, presented to us as not merely an assumption that “makes the math work,” but as an intuitively obvious matter as well. The real interest rate is a “price,” that is, it is the cost of borrowing money to finance business investment, and every good economist simply *knows* that an increase in the price of anything leads to a decrease in its demand. In a revealing attempt to invoke the imprimatur of the hard sciences, this was confidently referred to as an application of “the law of demand.”

But *how big* is the response of I to r ? Can it be entirely unresponsive? Can it, like some well-known “paradoxes,” respond positively to changes in r , rather than negatively? When I began to look at the empirical research, I found not just that the evidence of a negative relationship between I and r was weak, but that decades of research had failed to turn up credible evidence of any relationship at all between the two variables (even ignoring questions regarding the direction of causality). Almost as a hobby, I found myself collecting quotations from top-tier macroeconomists, all of them confessing in various ways, “We keep looking for that elusive negative relationship between r and I , but we can’t find it.” Even so, if you ask almost any economist today, “What is the relationship between r and I ?” he will almost surely say, with great confidence, “They’re inversely related.”

After I became a legal scholar, I focused on tax law and policy. This, too, is an area in which the “law of demand” is supposed to give us definitive answers to important questions. Taxes alter the prices that people face, and therefore, economic theory should supposedly be essential in telling us how to think about tax issues.

What makes the landscape of legal scholarship different, of course, is that most legal scholars have not been trained in economic theory. Perhaps understandably, therefore, they tend to defer to the views of economists on many issues. If an economist confidently states that “Economic theory tells us _____,” the temptation for many legal scholars is to say, “Well, I don’t know the economics, so I am in no position to say otherwise.”

This deference to economists, of course, is hardly limited to legal scholars. Journalists, including business journalists, tend to regurgitate a standard set of statements about the world that are, in fact, nothing more than unexamined conclusory statements that the journalists have heard from orthodox economists. As one of many examples, the top economics writer for *The New York Times* confidently [claimed a few years ago](#) that “higher tax rates discourage work and investment, two crucial ingredients for economic growth. But higher taxes on consumption don’t have nearly the same effect as taxes on incomes or companies. If anything, consumption taxes encourage savings, which lifts investment.” How did he know any of this? The author did not even feel the need to justify his assertions. Apparently, he felt that he was saying nothing more controversial than claiming that the sun rises in the East.

Such assertions of cause and effect are part of what many public finance economists are happy to tell us are “known results” from economic theory. After hearing these claims by economists, legal scholars who study taxation have, at least by my observation, generally (and quite erroneously) concluded that those results must be backed up by evidence and are uncontroversial among people who “know the economics.” Those claims are, many people seem to believe, simply the Economic Truth. Other statements of supposed truth include the “double distortion” argument and the claim that inefficiency rises as the square of the tax rate, which are used to support the ideas, respectively, that we should not tax capital income, and that increases in marginal tax rates are increasingly harmful to the economy.

Similarly, it is often taken as received truth that subsidizing poorer people’s wages creates a disastrous work disincentive. If a person believes that statement to be true, he can then insist that he does not *want* to ignore the plight of the poor, but that “economic theory tells us” that indulging our soft hearts by helping the poor would be pure folly.

The problem is that none of those cause-and-effect relationships is established by economic evidence—and, for most of them, the evidence leads to precisely the opposite conclusions. Just as I had found that the supposed relationship between interest rates and investment was not what was so often assumed, the empirical research on all of these tax-related questions does not support the received wisdom, either. By my reading, in fact, the evidence for those assertions is so weak that continuing to believe in them is little more than an act of faith—or superstition.

What I had not done, and what I had not seen from any other scholar, was to summarize the state of knowledge—both theoretical and empirical—about taxes and their effects on important social outcomes, to show that there is no “there there” to back up these off-heard claims. Luckily, Diamond and Saez decided to do that difficult and important work. In the Fall 2011 issue of the *Journal of Economic Perspectives*, they summarized the best available research and, based on that evidence, made three specific recommendations:

- (1) “Very high earnings should be subject to rising marginal rates and higher rates than current U.S. policy for top earners,”
- (2) “Tax (and transfer) policy toward low earners should include subsidization of earnings and should phase out the subsidization at a relatively high rate,” and
- (3) “Capital income should be taxed.”

These recommendations are not the personal opinions of the authors. They are the conclusions that can be drawn from the existing body of economic research. In short, three of the key assumptions that too many public finance economists have successfully exported into legal tax policy research, and that legal scholars had no particular reason (or standing) to question, are unsupported by the evidence.

What makes the Diamond and Saez paper especially helpful is that it was published in an economics journal that requires authors to minimize the use of mathematical exposition, which makes the results comprehensible to non-economists. (For those who care about credentials, it also helps that Diamond recently won the the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, commonly—though inaccurately—known as the Nobel Prize in Economics.) The exposition, while still tough sledding in places, allows interested readers to follow the logic and evidence that the authors present.

Notably, Diamond and Saez do not merely say that the evidence is lacking to support the conventional superstitions about taxes, but that the evidence supports the *opposite* conclusions: that marginal tax rates on high incomes should be increased, that lower incomes should be subsidized, and that capital income should be taxed.

Diamond and Saez are not writing on a blank slate. They are debunking received wisdom, exposing shibboleths that legal scholars need to un-learn if scholarship in taxation is to move forward, fulfilling its promise to guide policymakers toward wise decisions. For legal scholars with interests in tax policy, this is the essential article to read.

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