A blind spot in economics? Unjustified claims about reality

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An essay based on Rogeberg and Melberg (2011) “Acceptance of unsupported claims about reality: a blind spot in economics,” Journal of Economic Methodology, 18(1):29-52 Most of the stuff you read below has references, quotes, graphs and supporting evidence there - though some stuff has been added here because we thought of it after the article was published.

Science is about supporting claims

One of the big deals about science is that its claims need to be justified in an appropriate manner. You wouldn’t conduct an opinion poll to establish the truth of Pythagora’s theorem, and you wouldn’t conduct a linguistic analysis of texts to find the boiling point of a specific liquid. As a basic rule, if you want to justify claims about reality you will need to present relevant empirical evidence. This sounds obvious, but isn’t always. At least not in economics.

Consider the different models of addictive consumption within the subfield of economics working on rational addiction theories. The original paper by Nobel Laureate Gary Becker and Kevin Murphy from 1988 is cited ever more often, and it inspired a number of related models published in top 5 theoretical economics journals as well as more applied journals. They’ve been called a standard tool in the economic analysis of addictive consumption, have been seen as relevant to discussions of human rationality and the weakness of will, as well as for practical policy design such as the right size of a cigarette tax. But are the claims that come out of this supported by relevant evidence? How do we tell?

Our answer is that we first need to agree on what kind of claims we are looking at. Are they claims about the formal properties of mathematical systems, about successfully predicting market data, about the underlying mechanisms that are actively at work in the real world, or about the welfare of real people? These are the four types of claims we’ve identified that are both common in economics and require very different kinds of evidence.

Four types of claims in economics

The first type we’ve called claims of “conceptual innovation.” It’s a kind of work that is sometimes also called pure theory or “economic theory proper”, and that consists of logical, mathematical or conceptual systems that need to be internally consistent. They’re evaluated by checking that the maths is right, that the system does not generate inconsistencies, and by criteria relating to beauty, such as whether the model is elegant or beautiful or messy. Our main point here should be uncontroversial: No matter how elegant, consistent and logically coherent a model is, it may have nothing to tell us about the real world around us. This is true even if
we label different parts of the model with something that sounds “real world`ish”, calling one function “a consumer,” perhaps, another one “a firm,” this variable “capital” and so on. At this point it’s just labels, and all claims are about formal and logical and aesthetical properties.

The second type we’ve called claims of “as-if prediction.” We can think of the world as a giant machine and consider someone who just sits there “watching the wheels go round and round.” After a while, he’ll notice patterns, persistent regularities that show up in the empirical data he observes and jots down. When A happens, B usually happens soon afterwards, and so on. As long as these patterns stay the same, any formal model that reproduces the same patterns will be able to predict: If I now tell you that my data contains A, you can predict that there’s an increased chance of B - even if you have no idea why this is so. And for predictive purposes, as long as the patterns remain the same, any “story” is as good as another: You might say that the sun-god rides his chariot across the sky each morning because he is chased by the dark forces of the night wolf. No matter how crazy that is, the sun will seem to rise in the morning and travel across the sky and be followed by darkness. In that sense, you could say that it is “as if” the sun is a god riding across the sky. At the same time, as is often the case when we say something is “as if,” we know that this need not actually be the case: A puppet so lifelike that it is “as if” it were alive is still just a puppet.

Even so, such “as if” models can be useful tools if we want to predict and summarize patterns in data. Also, if we want to study the consequences of different people or firms interacting in some way, simple “as-if” models that are memorable and easily summarized in mathematical form can provide us with simple stand-ins - toy-robots or fantasy firms - that can be used in place of real people and firms in our models.

In any case, to justify such claims of “as if prediction” you would need to establish that you can successfully predict: Any model that successfully and repeatedly predicts well in a specified context can be seen as a useful tool for prediction in that context, and it will be a useful predictive tool for as long as the empirical regularities happen to remain in place.

This brings us to the third type of claim, which deals with the reasons things happen. We’ve called these claims of causal insight, and this is where good justifications get tricky. In medicine, the set of minimal conditions required to establish causality are referred to as the Bradford-Hill criteria. This extensive list illustrates well how difficult it is to reliably and credibly establish that A caused B in the sense that B would not have occurred (or occurred with less probability) if A had not been the case. Unfortunately, we learned about these criteria only after our paper was published and so could not discuss them there.

We don’t have anything similar to the Bradford-Hill criteria in economics, at least as far as we are aware of, but still: there are parts of economics where claims of causality are cautiously and rigorously discussed, where discussions revolve around questions of identification, discussions of randomized experiments, selection issues, the instrumental variables method, regression discontinuity designs, and so on. Having established causality in this way, though, is just a first step: Even if you’ve done this well, even if you’ve established that you can robustly affect one thing by manipulating something else, the mechanism that makes this happen may still be difficult to identify.
Clarifying the mechanism that would generate the causality you have established could be done using a model. But this model now needs to be more than just an elegant mathematical system or a weird, memorable as-if story that can reproduce patterns in the data. You’re now trying to put variables into your model that mirror some real empirical counterparts. You are trying to show - in your model - how those counterparts - out there in the real world - actually interact. This means that you need assumptions that are reasonable approximations supported by empirical data. In other words, the realism of the assumptions matters.

Finally, the fourth type of claim we’ve identified involves welfare. We call these claims of welfare insight, and they are claims that you can tell whether some change in policy or economic circumstances will raise or reduce people’s welfare. These are claims about real effects in the real world, and that means you need to have empirical evidence. You need to show an empirical measure, some proxy for welfare, that is credible and open to discussion. You can argue that the right measure is how much income people have, or how happy they claim to be when you ask them, or their estimated risk of suicide, or how often they smile, or whatever, but you need to tell us what you mean by welfare and how we can measure it. We can’t judge whether the “welfare” you’re talking about is something we care about unless we know what kind of welfare-in-the-real-world you are talking about. And if you claim to be able to predict how it will change, then we also need to know how to measure it so we can tell whether your predictions are good or not. If you can’t tell us how to measure the welfare you are predicting, then why should we believe you can predict it? In fact, why should you think you can predict it?

Some economists might respond to this argument that the welfare effects follow logically from the preferences of the individual. That may be true, but if it is true, then it has to be preferences in the sense of “what makes people better off” and not preferences in the sense of “what people choose.” When people worry that smokers, junkies, gamblers, and so on are making poor choices, their worry is precisely that “what people choose” is not the same as “what makes them better off.” Showing that there exists a mathematical model of a rational agent who acts in this way because weird preferences or some specific decision problem makes it optimal does not establish that people are wrong to worry about junkies. To support your claim that someone is maximizing their welfare you would need to show us that they really do face the decision problem you’ve specified, and that your assumed preferences really do capture what makes them better off in some substantive sense. To do that, you still need to tell us what kind of welfare you are talking about and how we can credibly measure it in practice.

Against this view, we sometimes hear the argument that this makes it pretty damn hard to do welfare economics. If so, then that’s because it is pretty damn hard to do welfare economics. You wouldn’t trust an engineer who invited you into a plane that had been built by skipping the hard calculations usually involved in designing them. Why should you trust an economist who asks you to implement policies designed by skipping the hard work involved in identifying policies that actually would make people better off? It’s really quite a simple point: If someone makes claims about empirical reality, about something in the real world, even if it’s people’s welfare, then we need to ask for empirical evidence.

That’s our “framework,” if you want to call it that. It might seem simple and uncontroversial (we
hope it does), but we believe economic research could be better assessed if we became better at sorting out the different claims involved in a piece of research and discussing each type of claim separately. This would hopefully make it easier to agree on what a piece of economic research does and does not achieve. To show that this has real “bite,” we use the theory of rational addiction. In our experience, using a real example triggers a lot more disagreement. Before you dismiss the discussion below, however, think about what it is you disagree with. Sometimes, people will dislike our critical discussion because they themselves believe some claim that rational addiction researchers have made. That’s irrelevant. You are free to believe a lot of things, but the question here is whether the claim has been justified in the relevant way by the people stating it in the research literature. A second response frequently encountered is reinterpretation of some claim. Put bluntly, these guys are actually saying that “If we try really, really hard, we could interpret this claim as something innocuous that no one would find reason to disagree with.” Our response: If the researchers “really” only meant something innocuous, why are these claims often written in a way that suggests something more? Why invite misinterpretation? Shouldn’t we just restate the claim to clarify that we’re not saying anything interesting at all? And if we do so - is this (smaller) claim supported and justified by the research work in question?

Unsupported claims - examples from rational addiction theory

Addictive behavior seems inconsistent with the standard assumption that consumer preferences are constant. After all, a beginning smoker gradually smokes more over time as he “gets hooked” even if prices are stable and his other consumption more or less constant. The original article on rational addiction theory from 1988 solved this conceptual problem. It proved that you can derive unstable consumption patterns theoretically from a rational agent who is making optimal choices from stable preferences. You do this by making the rational agent aware of the delayed, or lagged, effects of his consumption on his future tastes. Each cigarette the agent smokes now changes how much nicotine withdrawal he will experience in the future, it affects his future health, how much enjoyment he will get from a future cigarette and so on. The agent is aware of all these lagged effects of today’s choices on the future, and designs a plan to change his body and mind gradually using cigarettes, cocaine, heroin or whatever in an optimal way. Having made this plan that includes optimal investment in future tastes, he implements it over time. As a result, his consumption changes as his taste-technology and his incentives to further invest in this technology are gradually altered by his actions, even though his underlying preferences are stable. He cares more about cigarettes not because cigarettes in themselves are more highly valued, but because they are gradually becoming more useful ways to get the types of pleasures he has always cared about.

This is the underlying logic of the different rational addiction theories: The agent has beliefs about how a good will change his or her tastes and body, and identifies how these effects can be best used to maximize utility. By changing the model’s assumptions about how these lagged effects work, we can generate consumption patterns that are quite different: Rising, falling, suddenly dropping, cyclic, even chaotic. This can be interpreted as getting hooked, quitting, going cold turkey, binging or diet cycles, and as an explanation of why it’s impossible to predict individual behavior (because the behavior depends on details so nuanced in the decision
problem there is no way we can measure them). In other words: The conceptual claim that the theoretical construct of a rational agent can display unstable consumption paths while making optimal, forward looking choices from stable preferences is correct. The rational addiction model gives us new results within a consistent formal framework.

How about claims of as-if prediction? According to many economists, these models successfully predict, perhaps especially aggregate data (how many cigarettes the inhabitants of a country smoke all together, for instance). Several studies also indicate that when cigarette prices in the future are expected to rise, people reduce their smoking immediately - just as the rational addict would. At the same time, it should be noted that the rational addiction model also fits consumption of milk, eggs and oranges, and that there are empirical results less favorable, both on aggregate data and individual level data. Overall verdict on the claims of as-if prediction? They predict some things well in some datasets.

How about causal insight? Again, the economists have claimed that their research on rational addiction supports such claims. But in this case we’re less convinced. We’ve seen that the models generate very different consumption patterns when you change the type of lagged effects you get from consuming the good. This tells us the models are sensitive to the lagged effects we assume. These effects, however, are typically very simple ones, chosen to make it possible to solve the decision problem mathematically. The size and type of effects are not linked to real empirical studies of health effects or effects on taste development. And if we look to the research on such effects, it tells us that not only are the effects much more complex than this, but they are also so difficult to identify that we don’t really know precisely what they are. Not even for smoking. That’s a problem if your claim is that it is sophisticated exploitation of precisely estimated effects that is causing real people to behave like addicts.

It would also seem reasonable to ask for evidence that people have beliefs about these effects that are similar to what you assume. The evidence on how well-informed about risks, say, smokers are is quite mixed - some methods indicating that they overestimate the risk, others that they are accurate or underestimate the risk.

Since your causal mechanism assumes that people accurately predict and even rationally plan their future tastes, you should also provide some evidence that people do this. We’re not aware of any such evidence, but we’ve seen articles arguing the opposite - that people may systematically mis-guess future tastes and are unaware of how these will change - even when this happens in a systematic, predictable way(e.g., here and here).

Since your explanation for addiction is that they are optimally solving a difficult problem, you should also give some evidence that people are able to solve such problems if they faced them. But when you seat students in front of a formally equivalent investment problem and link the payoffs of the model to real monetary payoffs, the students prove unable to find the correct paths (see here and here for two write-ups of relevant research).

Finally, if your smoking is actually the implementation of a plan, you should also be able to predict your future smoking. Again, what evidence we’ve seen suggests that people don’t do this very well. For instance, half of high school senior smokers said they’d quit within five years,
but in a follow up study five to six years after graduation only a fifth of the daily smokers actually had quit.

So all in all, we don’t find the causal claims convincing. People are not addicted because they are implementing an optimal forward-looking plan based on correct information about how these goods will affect them down the road. Or if they are, the case for it has not yet been established.

Finally, how about welfare insight. Again, the claims are made. And again, we don’t find the evidence convincing. As we just discussed, the authors do not give us any evidence that their assumed preferences and decision problem are empirically accurate or sufficiently good approximations to empirical counterparts. And they don’t identify any credible, empirical measure of welfare or prove that they can accurately predict how this welfare measure will change with circumstances. One study that did make an attempt to empirically specify a welfare measure, concluded in the opposite direction of most rational addiction theories, and found that higher cigarette taxes stopped some people from smoking and that these people reported being happier than they would otherwise have been. Our case is not built on that study, however. Our point is more that if you want to make the surprising and counter-intuitive claim that smokers and junkies and the morbidly obese were and are just implementing optimal plans that serve them better than any other available options, then this requires you to provide evidence. The burden is on you.

**Nice model fits the data - let’s speculate freely about causality and welfare and call it research**

And this brings us to our final hypothesis. Why do economists sometimes sound so sensible, and at other times so insane? We think it might have to do with these groups of claims. We think maybe most economists learn not to make claims of conceptual innovation or as-if prediction if they can’t support them. If they do, they get shot down in seminars, by referees and editors. If you support these claims, on the other hand, you may be allowed to tack on claims of causal and welfare insight without supporting them with evidence. They’re icing on the cake. If you’re challenged by someone who says your assumptions seem unrealistic, for instance, you can say that it’s “standard assumptions,” that it’s “just as-if,” that it’s “necessary to make the model tractable” and so on. We believe such incantations may serve in some parts of economics as accepted ways of deflecting and neutralizing challenges to your claims of welfare and causal insight. Of course, this varies with the field of economics you are looking at. Some empirical parts of economics are very careful and sober in examining claims of causal insight, for instance. But in more theoretical work, our gut feeling is that this might be part of the explanation. Your gut feeling may be that we’re wrong. So we also tried to test it.

A few years ago, we sent a survey to all authors of peer-reviewed rational addiction articles, asking them what kind of claims they felt the literature supported, and what sorts of evidence they felt the models should be, and actually were consistent with. The details are in the paper, and we would stress that the results should only be seen as indicative, but the results seem at least consistent with our hypothesis: Take all the researchers who say the literature supports a claim of conceptual innovation. Out of these, how many of these agree that the models should
be consistent with evidence relevant to that kind of claim? How many of those also believe this evidence actually exists and in fact supports the claim? This gives you the share of what we called crudely rational researchers in that group. Put differently, out of all the researchers who agree that the literature supports some claim, this is the percentage of them that understands what kind of evidence you need to support such a claim in principle, and who also believe that this evidence exists in practice. The data indicate that the share of crudely rational researchers is higher for those believing in the claims of conceptual innovation and as-if prediction than it is for those believing in claims of causal and welfare insight. If correct, this means that these researchers are better able to judge claims of conceptual innovation and as-if prediction, and poorer at judging claims of causal and welfare insight.

And that’s it. The most important take-away, we hope, is that you should evaluate different claims in economics differently, and that you shouldn’t allow researchers to make claims of causal and welfare insight unless they’ve provided appropriate justification for these. If you agree to that, we’re happy. We also believe that economists in practice sin against this principle, and that we shouldn’t take claims of causal and welfare insight based on, for instance, rational addiction models seriously. If you don’t agree to that, we’d be interested in seeing the evidence relevant to supporting these claims. And finally, we suggest that economists may sin against this principle because they learn in practice that it’s fine and even common in parts of economics to just tack on claims of causal and welfare insight when you have an elegant, consistent model that matches stylized facts or some patterns in empirical data. If you don’t agree that this is acceptable, then we look forward to seeing you join us in reacting next time it happens.