

Subjectivity in Economics

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Abstract

Economics cannot claim to be absolutely objective. There are several significant ways in which economic analysis is subjective, and this should be recognized by the profession to a greater degree.

Like people in general, social scientists are apt to conceal valuations and conflicts between valuations by stating their positions as if they were simply logical inferences from the facts. Since, like ordinary people, they suppress valuations as valuations and give only “reasons,” their perception of reality easily becomes distorted, that is, biased.

Gunnar Myrdal (1969, p. 50)

Mainstream economists often make a distinction between *positive* and *normative* economics, where the former could be described as the analysis of how things work, whereas the latter concerns what should be done. A stated or implied distinction between the two is that positive economics is value-free, but normative economics is not. Economists typically view the bulk of economic research as objective science, not influenced by the values or incentives of the researcher. In this article, I argue that there really is no sense in which economics can claim to be absolutely objective, and that there are several significant ways in which the subjectivity of economic analysis has not generally been recognized by economists.

Consider the following example, which an economist might use to illustrate the influence of another professional’s values on his work. A medical doctor sometimes orders MRI scans for his patients. Together with the other doctors in his practice, he sets up a lab in the same building, where patients may go for MRI scans as well as other diagnostic tests. This is likely to be a considerable convenience for the patients, who would not need to make separate appointments in different locations to have these tests done. However, if



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the doctor in general practice has a financial interest in the lab, he has incentive to order MRI scans more often than is necessary. This is not to say that the doctor consciously orders gratuitous MRIs simply to line his own pockets, but rather that the doctor cannot help being influenced by his own benefit in the form of lab revenues. In a marginal case, where the MRI may or may not be indicated based on the medical assessment alone, the financial consideration could well nudge the doctor in the direction of ordering the MRI. It is because of this kind of influence that some decision-makers, such as public officials, may be urged to avoid even the appearance of impropriety. One could argue that doctors should not have these kinds of financial interests because of the effects on the demand, and thus the prices, of medical tests.

The claim that economics is objective science is essentially equivalent to the claim that economists are immune from these kinds of influences. In fact, the results of a piece of economic research can benefit the researcher in a number of ways, and there is no reason to think that these benefits have no influence on the research itself. An economist may be more likely to pursue research that aligns with his own values or political beliefs. There may be various financial incentives, such as supporting the objectives of potential consulting clients for the researcher, or promoting government policy that benefits the researcher's own socio-economic class. Publishing research of any kind leads to employment, tenure, and promotion, and there is thus an incentive to create publishable work. Since the judgment of what is publishable is in the hands of other economists, themselves subject to their own biases, there is an imperfect correlation between what is publishable and what is true. On the other hand, the quest for greater understanding is itself a motivation for research and may well be the primary incentive behind most economic research. However, it is unrealistic to think that economists are able to disregard all other motivations for the sake of their work, and patently ridiculous to claim that economists are unique in this respect.

This is not a new idea—consider Myrdal (1969), quoted above—and it has sporadically reappeared in the economics literature. Recently, Putnam and Walsh (2011) have argued from a philosophical perspective that the positive-normative dichotomy is untenable. Although there are many who would agree that economics is fundamentally subjective, the profession as a whole has never really embraced the idea; witness the persistence of the positive-normative terminology. Here I focus on the practical issues: how subjectivity arises at essentially every stage in economic analysis, and how any piece of economic research should take this into account.

Carden (2011), in responding to the statement, “Private funding sources often prevent researchers from being completely objective in the conduct of their work,” disagrees and points to *often* as the key word. I, on the other hand, agree and point to *completely*. Of course sources of funding, along with other incentives, influence researchers to some extent. However, this need not be a problem, because any piece of research is part of a larger debate with others in the field. Criticism of a piece of research, whether through peer review or subsequent work, can always focus on the research itself, and does not have to be concerned with the incentives of the researcher. More generally, the presence of

subjectivity does not have to be a barrier to producing compelling research; but it should be acknowledged to a greater extent, especially in the peer-review process.

Methodology and mathematics

Extensive use of mathematics has been the norm among academic economists for decades. Mathematical proof is just a small step away from logical argument at its purest, and as such is commonly accepted as objective. Even this notion can be challenged: the goal of a mathematical argument is to convince someone—reader, grader, referee—that a result is true, and there can be legitimate disagreement about this. I will not press this point, but there are greater potential problems that arise through the use of mathematics as a means of argument.

First, I acknowledge that there is good reason for the use of mathematics in economic analysis. A mathematical argument makes a logical argument explicit and draws attention to any flaws; it is not difficult to make an appealing verbal argument that can be shown to be flawed when translated into mathematical terms. Use of a mathematical model illustrates multiple effects, and aids the researcher in sorting through such effects, when it may be inhumanly difficult otherwise. If an argument can be translated into mathematical terms, and there is some benefit in doing so, then it is worthwhile to perform a mathematical analysis.

One pitfall is that mathematical rigor is often taken to be synonymous with intellectual rigor, when in fact the use of math is neither a necessary nor a sufficient condition for the validity or value of an argument. The math is simply being employed to support the argument. There are many means of argumentation, some of which are qualitative. Currently, the top mainstream economics journals publish virtually no non-mathematical research. One notable exception is the *Journal of Economic Perspectives*, which, according to the journal website, “attempts to fill a gap between the general interest press and most other academic economics journals.” That is, the substance of the journal is viewed as being somewhere between research and journalism. Graduate students and tenure-track faculty face considerable pressure to make their research mathematically complex.¹ There is room for debate over what precisely constitutes intellectual rigor, but surely the use of mathematics should not be the decisive criterion. While qualitative research may have its own pitfalls, any argument can be peer reviewed in its own right.

In some cases, a mathematical model does not provide any insight that a less formal argument would; e.g., if the results are obvious given the hypotheses. It does not necessarily follow that the issue addressed is not interesting, or that it is not important to address it. It may be that the research question is in the hypotheses, i.e. that the argument is not what the model would show us, but which is the right model to use. Other research might focus on variables that are not amenable to parameterization or measurement. Here

¹ Frank (2007) gives an economic rationale for why academic economists have too much incentive to use complex mathematics in their work.

again, this does not mean that the issue is not interesting or should not be addressed. The subject of economics is not defined by the use of a specific set of analytical tools, and it would be a mistake for the profession to ignore some issues because they are not suitable subjects for mathematical analysis. As Firebaugh (2008) says, the method should be the servant, not the master. Research that is in part driven by the desire for mathematical complexity can, at its worst, lead to mathematical conclusions that are not economically meaningful.

Another potential problem is that the use of mathematics, either theoretically or empirically, lends a pretense of objectivity to the argument as a whole. This may be due in part to the analytical similarity to the natural sciences, which are even more commonly viewed as objective. However, ever since Kuhn's *Structure of Scientific Revolutions* (1996), originally published in 1962, philosophers of science have widely acknowledged that even the natural sciences are laden with values.² Whether in the realm of natural or social science, even if we take the mathematical analysis itself at face value, there is always the question of what it means in terms of the larger argument. Consider a study purporting to "prove scientifically that smoking causes cancer." If we assume that the study used appropriate methods to gather and analyze data, and did these things correctly, the only reasonably objective statement that can be made is something like this: in the study group, which is representative of the population of interest, there was a higher incidence of cancer among smokers than non-smokers, and there is only a small probability that these results could have arisen from randomness in the data. How to interpret or apply the results of the study beyond this fundamental statement is open to debate. It is reasonable for a consensus to form among a community of scientists, especially after a number of studies have led to the same result; but any standard for what is required for such a consensus is necessarily subjective. For a piece of economic theory, if we again assume the soundness of the mathematical analysis itself, the reasonableness of assumptions and the validity of conclusions will always be debatable. I elaborate on this point in the next section.

Although mathematical analysis can make a logical argument more cogent, it is not the only means of making an argument, and mathematics can sometimes obfuscate rather than illuminate.³ The crucial point is that we are making arguments, in which mathematics may or may not play a role.

Modeling assumptions and conclusions

A development in one school of economic thought illustrates my next point. The Institutionalist school has for many years focused on the role of institutions in influencing economic behavior. This school has generally been considered heterodox, offering criticisms of mainstream economics and largely using heterodox methodologies. More recently, some mainstream economists have applied mainstream modeling approaches to institutional issues. Spiegler and Milberg (2009) criticize this work, saying that while it

² See also Ratzsch (2001). Both Kuhn and Ratzsch speak to the issues in the following sections as well.

³ Chick and Dow (2001) elaborate on this point.

has “built upon and extended the work of the old and new institutional economics in important ways, it has, at the same time, set back economists’ understanding of institutions by overstating the applicability of its models.” This is because the “formal models are too parsimonious to meaningfully illuminate the complex institutions they ostensibly represent” (p. 290). This kind of criticism, that a model does not apply to the phenomenon under discussion and thus that the results of the analysis do not apply, is not limited to institutional matters. One could potentially make such a criticism of any economic model.

Spiegler and Milberg examine the methodology of the work that they criticize, and again their analysis applies to economic research in general. They delineate the following steps in researching economic phenomena:

1. *Delimiting*, in which the set of social phenomena under study is specified and a research question is formed.
2. *Naming*, in which a mathematical construct meant to be analogous to the social phenomena is introduced, along with a ‘catalog of correspondences’ which links elements of the construct with elements of the phenomena under study.
3. *Solution*, in which the mathematical construct is brought to a solution.
4. *Interpretation*, in which the mathematical solution and its implications are interpreted with respect to the research question.

The authors draw attention to “an important divide in the analysis—i.e. the divide between the realm of ordinary language descriptions... and mathematical language descriptions” (p. 294). Crossing this divide, i.e. proceeding from *delimiting* to *naming* and from *solution* to *interpretation*, is a necessary part of any mathematical economics research. Even if the derivation of the solution is objective, the other steps clearly are not.

Referees do tend to consider these steps, at least implicitly, in their evaluation of a paper submitted for publication. However, the reviewer’s support or criticism tends to focus on whether these steps were executed correctly, as if there were an objective answer to that question. That it is subjective does not mean that the referee need only weigh in as to agreement or disagreement. An important part of the peer review process is the critique of the author’s thinking on these subjective issues. In some cases this warrants as much or more attention than the solution of the model.

A related issue is what we consider to be data or evidence, and what is the most appropriate way to deal with such data. Often it is possible to argue for the existence of stylized facts that cannot be measured with any precision, and where attempts at measurement would be subject to strong potential objections: for example, “The average American does not invest much time or effort in learning about political candidates before voting.” It might then be more straightforward, as well as more compelling, to build an argument upon the stylized facts. Such arguments are often dismissed out of hand, with reference to the evidence as merely “anecdotal.” The evidence may or may not be strong enough to justify the conclusions, but this is one of the very points of argument that peer review must consider.

Selection of topics

Another crucial issue determining whether a piece of research is published, or in how high quality a journal, is the topic itself. Questions of the significance of a contribution, the importance of a topic, and whether it adds significant insight to policy or previous work are all facets of the question of how interesting the research is. Readers and referees of research may willingly acknowledge this. However, the focus of the profession as a whole, in terms of what topics are regarded as worthy of attention, is arguably too narrow, and certainly narrower than it could be. That there is even such a thing as “heterodox” economics, i.e. a considerable body of research outside the mainstream, testifies to this. Students of economics, including graduate students, are unlikely to have much exposure to anything outside the mainstream; and so even PhDs in economics might be only marginally aware of alternative approaches or ideas.

Following on to the subjectivity of what questions are interesting, I would argue that there should be more discussion of what issues we should be studying. If the profession does not take a broad view, we may miss opportunities to understand important economic phenomena, and thereby diminish our credibility. For example, there has been a great deal of research about the financial system that cannot address the recent financial crisis. This in itself is not a problem; there is much we can do to explore the workings of the financial system under the assumption that the system is for the most part working the way it is supposed to. But if we only focus on the workings of the system, and not the potential failings of the system, we can miss the potential for a crisis (not to mention predicting the actual crisis), and then have to scramble to understand it in retrospect.

As another example, consider the history of industrial organization. This field has offered countless insights into the functioning of imperfectly competitive markets, relying heavily on game theoretic analysis. Prior to the 1980s, criticisms of perfect competition came primarily from heterodox schools of thought. It seems that mainstream economics did not take much interest in questions of imperfect competition until good analytic tools for dealing with them were available; i.e. until game theory had been widely integrated into economic thinking. Before this change, it was as if mainstream economists generally regarded markets to be perfectly competitive. The importance of questions of imperfect competition did not change over time, but the mainstream of the profession acted as if this were the case.

Any number of research topics have begun as subjects of heterodox research and have eventually moved into the mainstream of economic thought. That this process happens slowly is likely due in part to the reluctance of the mainstream to accept anything outside the mainstream. Economists are well aware that it is often to insiders’ benefit to keep outsiders out. Any established professional has a natural inclination to shut out potential competitors in the profession. In the economics profession, this inclination can lead to marginalization of some research without regard for the value of the research itself. As in the above example of the physician and the MRI lab, I would not claim that mainstream economists consciously and willfully exclude non-traditional economists, but the incentive is certainly there at some level.

If for no other reason than the inherent complexity of economic phenomena, the study of economics cries out for a pluralist approach.⁴ At the very least, alternative viewpoints should always be welcomed as subjects for debate. Even in a case where a heterodox argument does not hold up in the face of a conflicting mainstream argument, the mainstream argument is strengthened by engaging in this conflict. Of course, assessing the value of an argument and evaluating competing arguments are subjective exercises; but if we are interested in getting at some underlying truth, more open debate is always better.

Policy prescriptions

It is in making recommendations for what should be done that economists are most likely to recognize subjectivity. Still, there are issues that warrant discussion. First, the “positive” conclusions upon which recommendations are made are not entirely objective, as discussed above. Second, it can be difficult for non-economists to distinguish the subjective conclusions from the research that is supposedly objective. Referring to the “normative” part of research as that which makes use of a social welfare function, as is often done, misses an important point. The statement that a particular policy maximizes a given social welfare function is a positive rather than a normative conclusion. The real normative issue is how to think about social welfare: put simply, what is the right social welfare function to use, and why. If, hypothetically, we can all agree on a notion of social welfare, then of course we should maximize it.

Furthermore, it is very easy for non-economists to get the impression that economists only value efficiency. This may well be true for some economists, but it is not in the nature of economics itself. Economic reasoning cannot possibly demonstrate that efficiency is all that matters, or that it matters more than anything else. More generally, one cannot use economic reasoning to prove what is good or right or fair. The question of whether a welfare function is the right one to use is outside the purview of economics.

Consider trade as an example. A large majority of economists support free trade for efficiency reasons. Holding this opinion does not mean that one does not care about the problems that trade potentially generates, such as short-term unemployment caused by the removal of trade barriers. It is easy for a non-economist to think that an economist supporting free trade attaches no weight to equity issues, and this impression can easily detract from the case the economist is trying to make.

Economists do the rest of the world a disservice when they support efficiency aggressively, as if it were the only thing that matters. Of course we will focus our research on efficiency, since our expertise speaks to this. However, as a profession, we must acknowledge that equity does matter also; that there can be considerable disagreement over what is equitable, as well as the relative importance of equity and efficiency; and that, as economists, we do not have any more insight into equity than anyone else.

⁴ See also Kurz and Salvatori (2000).

I have noted that the values of the researcher may affect many aspects of the research. It is, however, completely reasonable and desirable to strive for objectivity in developing “positive” conclusions. In making policy recommendations, on the other hand, it is essential to note what values are being supported.

Conclusion

I have described several ways in which economics research is necessarily subjective. This subjectivity does not devalue the research, but it is something that must be acknowledged, particularly in the peer-review process. As the author of this article, I am naturally influenced by my own values, and I have an obvious interest in publishing this piece. This does not imply that the argument I have presented here cannot be evaluated on its face. The peer-review process is itself subjective, but like a grading rubric or a judge’s instruction to a jury, it is reasonably straightforward to specify the criteria (which are to be subjectively evaluated) concretely. A greater recognition of the subjectivity involved in economics research would potentially influence what research is done, how it is done, and how the results are communicated, all to the benefit of our understanding of economic phenomena.

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