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Response to the Comments

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Response to the Comments

by Richard Swedberg

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Sociologica is to be commended not only for having published so many interesting papers over the years but also for the idea of adding a few comments by other scholars after each paper. This makes the reading of the paper much more interesting for the reader; it also helps the author a lot. For those who do not think that this feature of *Sociologica* represents something rare, I want to point out that the standard format of a journal is to present articles without comments. If you want to discuss something you have read in a social science journal, you have to find a friend or a colleague who also happens to have read the same article.

In the case of my article on Peirce and theorizing, I have had the good luck of getting four very able commentators; and I will address what they have to say one by one. Isaac Reed begins his comment by saying that he wants to contextualize my argument about Peirce and theorizing. This is followed by a discussion of two central elements in Peirce's lecture on how to theorize: the role of surprise and of the economy in the theorizing enterprise.

Reed brings up Kuhn's notion of anomaly and compares it to the Peirce's notion of surprise, but concludes that that they are also quite different. The key to their difference has to do with the impact of scientific cultures on the scientist's working mind. To this I would add that surprise also has a psychological dimension, while anomaly is more cognitive in nature.

In discussing Peirce's ideas about the economy of research, Reed notes that Peirce ignores the fact that resources typically have a political dimension, which needs to be taken into account. Theorizing must not ignore politics.

What Reed means has in mind when he says that he wants to contextualize theorizing is now beginning to take shape. Theorizing does not happen in a void; it is part of a culture and a political economy; and this is crucial to realize. Furthermore, theorizing takes place in a scientific community (Peirce's "community of inquiry"); and the structure of this community needs to be mapped out. One should, for example, be able to lay bare its social mechanisms and how these operate.

Reed's ends his comment with a sentence that is quite poetic and that reminds me of the title of one of Alfred Schutz' article: "Making Music Together." This is what Reed writes, "*Could we move from 'how to theorize' to 'how to theorize together.'*"

Now, Reed and I agree on the point that the project of theorizing will ultimately not succeed without a community and a collective culture of theorizing. In an article on theorizing that was published earlier this year in *Theory and Society*, I wrote:

The project of theorizing can truly flourish only if theorizing becomes a communal and co-operative enterprise among all kinds of social scientists, linked to each other as well as to people around the world. Peirce liked to point out that scientific inquiry is profoundly communal in nature, and that new ways of theorizing and analyzing will only succeed if they are deeply rooted in a universal community of scholars. Inquiry and community, he said, must come together in a true community of inquiry – or into a *general culture of theorizing*, as one could also put it [Swedberg 2012, 35].

So, do Reed and I really agree? Yes and no. Yes, in the sense that we both think that having a community is critical for theorizing to flourish. But no, in the sense that we are partly talking about different matters. What is at issue is an important point to me; and it has to do with the principled difference between the following two approaches: (1) making a social science analysis of situations, in which scholars tend to theorize well and creatively; and (2) providing scholars (and students) with practical tips for how to proceed when you theorize.

There exist quite a bit of research on what makes a social group or an environment creative, but this type of knowledge is typically of little use for the person who wants to learn how to theorize. It is, for example, of little aid for the theorizer to know that around 1900 the epistemic field in the human sciences was wide open for advances in sociology. Or that the network that was to make up German idealism in philosophy had not yet been filled by 1776. The reason for this is simple: the individual cannot develop an epistemic field, a major network and so on. These are simple there or not.

What then can be done? My own view is that each of us can do certain things, especially on a small scale – and for this we need practical tips. One may, for example, cultivate friends who are fun and interesting to hang out with. This is something that Herbert Simon has advocated and also the theme on which he ends his autobiography, *Models of My Life*. He writes:

One heuristic that has been of first importance to my work is missing, however, from the program I have described in this chapter (“The Scientist as Problem Solver”). To make interesting scientific discoveries, you should acquire as many good friends as possible, who are energetic, intelligent and knowledgeable as they can be. Form partnerships with them whenever you can. Then sit back and relax. You will find that all the programs you need are stored in your friends, and will execute productively and creatively as long as you don’t interfere too much. The work I have done with my more than eighty collaborators will testify to the power of that heuristic [Simon 1991, 387].

Passing on to Andrew Abbott’s comment, I should start out by saying that I was very curious about what he had to say. The main reason for this is that Abbott is the author of one of the very few books in social science that tries to give practical advice on how to be creative in your research, *Methods of Discovery* [Abbott 2004].

There exist, however, two differences between the approach of Abbott and that of myself, as I see it. First, and perhaps less importantly, while Abbott speaks in terms of methods, I speak in terms of theory/theorizing. This has to do with my view that modern social science has a tendency to see all research in terms of methods, and to deny that theory/theorizing has its own, independent place in the research process.

The second difference has to do with the idea of a heuristic in general or at least with the most common way of looking at heuristics. In my view, a heuristic of this type has something mechanical about it. It basically says: when you encounter complexity and uncertainty, here is what you should do so that you quickly can proceed to what is important. The advice usually boils down to the creation of a number of tricks to draw on. I have myself suggested some tricks of this type and so have others [see especially Becker 1998].

My own three tricks are these: transform a noun into a verb; pluralize the object of your study; and cast a phenomenon as a social relationship. Some examples can illustrate the general idea: don’t speak of structures, speak of structuring (Gurvitch, Giddens); don’t discuss capitalism, focus on types or varieties of capitalism (Hall, Streeck); and don’t speak of the stranger, speak of the relationship between an outsider and other people in a community (Simmel, Schutz).

In his comments, however, Abbott does not discuss my Peirce article from his view of heuristics. Instead he makes two moves that I like a lot. First, and more

as an aside, he points out that theorizing is one thing, but theorizing social life is another.

My immediate reaction when I read this was to dismiss Abbott's point, but after a few hours of trying to repress what he said, it blew up in my mind and hit me with plenty of power. Yes, Abbott is 100% correct! While Peirce was primarily interested in outlining how to theorize in the natural sciences, what matters to social scientists is something else: how to teach ourselves and our students how to theorize social life.

How this can be done is a huge question, but one that is obviously central to the theorizing project and also exciting to think about. In my *Theory and Society* article I outline some steps for how to proceed when you theorize, that mainly come from literature in the social sciences. These include such things as naming, developing concepts, using analogies and metaphors, constructing an explanation – and all in a heuristic way. But what I do *not* address explicitly is how these steps should be closely informed by the fact that they are to be applied to social life [also Reed touches on this point; see more generally his *Interpretation and Social Knowledge*].

The main part of Abbott's comment is not about the need to theorize social life, however, but about surprise. More precisely, 80% of the text is about surprise: how you can understand what a surprise is and especially how different groups and sub-groups consider different things as surprising.

My guess is that Abbott was somehow struck by Peirce's insistence that the surprise plays such an important role in the discovery process. It is indeed the signal that says to the researcher: *this* is what you need to work on and explain! The reason why Peirce's argument has a certain shock value is related to the fact that most people who speak about discovery do not mention surprise; instead they speak of finding an anomaly; solving a problem; finding a problem; and so on.

What Abbott does with the 80% of his comment is then something I like a lot and something that I also want to claim a certain credit for. He gets inspired, drops the task on commenting on my article on Peirce, and goes theorizing on his own.

Let me explain. If you are of the opinion, as I am, that a text on theorizing should in principle not inspire to a duplication of what already can be found in the text, but should rather inspire the reader to theorize on his/her own, the problem becomes the following. How do you write in such a way that the reader gets liberated and flies off on his/her own?

I will not try to answer this question here. I am just now writing a book on theorizing and am in the process of suggesting an answer. In the meantime I want

to praise myself for somehow having written the article on Peirce in such a way that Abbott felt air under his wings and flew off in one of those inimitable, fractal flights that only he is capable of!

Let me now turn to Mustafa Emirbayer's comment. It should first of all be pointed out that it does not deal with my article on Peirce. Instead the author has made the decision to discuss my article on theorizing in *Theory and Society*. In reading this article, it also immediately becomes clear that Emirbayer is very critical of my view of theorizing. He sums up his view by stating towards the end of his comment that there is an "overall sense of frivolity" to the way I conceive of theorizing.

This is also the point I want to briefly address. There exist many reasons according to Emirbayer for the frivolity of my approach, but it especially comes out in my advocacy of such things as the use of play and free association, in the attempt to be creative when you theorize. There is also the fact that I ignore "the larger pressures and urgencies of social life."

In order to explain my approach I need to return to my earlier argument about the standard take on heuristics, namely the standard perception of heuristics as a set of tricks. My general sense, to repeat, is that there is something mechanical about dealing with complexity and uncertainty in this way; and that we may want to find other ways to proceed.

This is where free association and play come into the picture. Instead of pushing complexity to the side, I argue that that we initially may want to try to play around with various ways of approaching the topic, in order to get a better handle on it. This is why I advocate play. I advocate free association for the very same purpose. It is primarily a technique for seeing how something is linked to other phenomena in surprising ways.

Is this emphasis on play and free association frivolous and does it disregard important social problems and the like? Not necessarily. Throughout my *Theory and Society* article I cite C. Wright Mills and Everett C. Hughes as two exemplary scholars when it comes to theorizing. They also advocate that we use precisely these two ways of approaching things, in order to find a creative angle on what you study.

And what were the topics that Hughes and Mills studied themselves in this frivolous way? Here are two: Hughes used the technique of free association to analyze the genocide of the Jews during World War II ("Good People and Dirty Work"); and Mills used his imagination to study what could become World War III. Plenty of other serious examples can be supplied.¹

¹ The reader may finally find it rare for a sociologist to use a word such as "frivolous" in criticizing somebody else's argument – an impression that JSTOR confirms. A search shows that it is even

If I now turn to the comment by Claudio Paolucci, I should first of all say that I am very happy to have become the occasion of this comment, a bit like I want to take some of the credit for having inspired Abbott. Paolucci starts out by saying that I have simplified and/or misunderstood Peirce's notion of *musement*, and then proceeds to rectify this by showing how it can be better understood. The result is a very fine, short little essay.

My sense is that Paolucci clearly has a better and more interesting understanding of *musement* than I have. More important than this, there exists very little secondary literature to my mind on this concept, something that makes Paolucci's comment so much more valuable.

I will not repeat what Paolucci says but his argument that Peirce looked at *musement* in terms of making a connection strikes me as correct. Also his idea that there has to be a *distant* connection, and one that is not easily made. A *musement*, Paolucci specifies, is for Peirce a connection between different Universes.

What struck me as the most suggestive part of Paolucci's comment, however, was the author's attempt to show that *musement* does not operate according to some rules, but according to "*the law of liberty*." The way that this law operates, according to Paolucci, sounds very much like Marxian dialectics. The example that he uses to illustrate this has to do with a person getting burnt by touching a coffee pot. The pain that emerges comes from breaking a habit and behaving in a way that is not part of one's regular behavior. A *musement*, in other words, represents novelty; and it is the very opposite of a habit. In brief, it is *an abduction*!

By way of concluding, let me thank the editors of *Sociologica* and the four commentators. As I mentioned at the outset, it is very gratifying for an author to have colleagues discuss his or her paper in a helpful and critical manner.

References

Abbott, A.

2004 *Methods of Discovery: Heuristics for the Social Sciences*. New York: W.W. Norton.

less common for sociologists to say that something is "frivolous" than to call it, say, "stupid" or "ridiculous" in a published text. It comes however, very close to "idiotic" (in frequency). On the other hand, more polite terms such as "critique" and "weakness" are about one to two thousand times more common in the sociologist's vocabulary than "frivolity." According to a search for "full text" on the 118 journals that make up "sociology at JSTOR (September 21, 2012), the following numbers appeared: "frivolity" – 208; "frivolous" – 1024; "stupidity" – 1073; "stupid" – 2755; "ridicule" – 2761; "ridiculous" – 2169; "idiotic" – 192; "idiocy" – 273. By way of comparison, words such as "critique" and "weakness" appeared 451,716 respectively 262,298 times.

Becker, H.

1998 *Tricks of the Trade: How to Think about Your Research While Doing It*. Chicago: University of Chicago Press.

Hughes, E.C.

1962 "Good People and Dirty Work." *Social Problems* 10:3-11.

Reed, I.

2011 *Interpretation and Social Knowledge: On the Use of Theory in the Human Sciences*. Chicago: University of Chicago Press.

Simon, H.

1991 *Models of My Life*. New York: Basic Books.

Swedberg, R.

2012 "Theorizing in Sociology and Social Science: Turning to the Context of Discovery." *Theory and Society* 41:1-40.

Response to the Comments

Abstract: As part of the larger project of trying to revitalize social theory by drawing attention to theorizing, I analyze the views of philosopher Charles S. Peirce on this topic. I take my departure in his 1903 lecture called “How to Theorize” and note that for Peirce theorizing was closely linked to his concept of abduction. In analyzing this central concept in Peirce’s work, I suggest that we may want to look at it especially from a practical point of view. More precisely, what can we learn from Peirce in terms of concrete tips and suggestions for how we ourselves should go about theorizing? I also supplement the material from the 1903 lecture with what can be found in Peirce’s later writings.

Keywords: Theory, social theory, theorizing, Peirce, Charles S., abduction.

Richard Swedberg is Professor of Sociology at Cornell University. His two main areas of research are economic sociology and social theory. His books include (edited with Neil Smelser), *The Handbook of Economic Sociology* (1994, 2005), (edited with Peter Hedstrom), *Social Mechanisms* (1997) and *Tocqueville’s Political Economy* (2009). He is currently working on a book on theorizing and articles on the financial crisis.