

## Donald Broady & Yann Renisio, What makes sociology a scientific discipline? An email discussion 31 Jan.–2 Feb. 2020

Subject: What it means that sociology is a scientific discipline  
Date: Tue, 4 Feb 2020 14:20:16 +0100  
From: Donald Broady <donald.broady@soc.uu.se>  
To: sec-nos@lists.uu.se

Thursday last week during a seminar on Bourdieu's sociology of science, and in an email exchange the following days, Yann and I got engaged in a discussion on what it means that sociology is a scientific discipline. A tricky question. Perhaps someone on this list might wish to have a look at our arguments, and maybe intervene. Below are copies of the emails.

/D

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(även bilagor funkar) till [sec-nos@lists.uu.se](mailto:sec-nos@lists.uu.se)  
Listansvarig är [donald.broady@soc.uu.se](mailto:donald.broady@soc.uu.se)  
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Subject: Physics and sociology  
Date: Fri, 31 Jan 2020 14:19:10 +0100  
From: Donald Broady <donald.broady@soc.uu.se>  
To: Yann Renisio <yann.renisio@gmail.com>  
CC: Nubin Ciziri <nubin.ciziri@edu.uu.se>, Astrid Collsiö <astrid.collsioo@edu.uu.se>, Mette Ginnerskov Dahlberg <mette.ginnerskov@edu.uu.se>, Pablo Lillo Cea <pablo.lillo\_cea@edu.uu.se>, Moa Lindqvist <moa.lindqvist@edu.uu.se>, Tina Vrieler <tina.vrieler@it.uu.se>

Cher Yann,  
Cc to the participants in the course Kapital och fält.

Thank you Yann for joining yesterday's seminar on Bourdieu's sociology of science. You really contributed to enrich the discussion.

When we at the end attacked the grand questions about the relationship between natural sciences and human sciences and about what is scientific in sociology, you made the important point that the distance is greater between (theoretical) physics and e.g. biology than between biology and sociology.

Had we not run out of time I would have liked to discuss one of your arguments, namely that the capacity for prediction is a proberstone of a mature science. I think that I disagree, or that I might have misunderstood.

I have never thought of prediction in that way in connection with the sociology of Bourdieu & Co. Quite the opposite, in my lectures I use to tell the students that for example the construction à la Bourdieu of research objects (with the construction of spaces as a prime component) the aim is not the kind of prediction that mainstream sociology (celebrating American models) strives to achieve (a working class daughter will with high statistical probability enter educational programs like the preschool teacher one, and not a program like medicine). The aim of, e.g., the construction of a space is to uncover the forces that impose themselves on everyone who enters or resides there. For example, the trajectory of an individual cannot be predicted in advance since it is the outcome of the encounter between the structure of this field of forces and the properties of the individual, and this outcome is produced within a field of competition. I am simplifying, of course.

Another way of discussing relations between mature natural sciences and maybe less mature human ones is by raising the classical question of laws. Here Bourdieu was an extremist in his belief in sociology's potential to mature. I remember from the memorable conference "Empirical Investigations of Social Space" at the University of in Cologne in 1998 his answer when someone in the audience asked whether or not there could be laws in sociology in the same manner as in physics. Yes definitely, Bourdieu answered, and mentioned his own sociological version of the economists' Gerschenkron effect as an example (that newcomers/parvenus tend to borrow models from the established). And, he continued, in the future we sociologists will discover many more laws that govern the social world.

Looking forward to further discussions.

I am attaching my slides from yesterday  
(200130-broadly-lecture-bourdieu-sociology-of-science.pdf)

/Donald

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Subject: Re: Physics and sociology  
Date: Sat, 1 Feb 2020 19:25:27 +0100  
From: Yann Renisio <yann.renisio@gmail.com>  
To: Donald Broady <donald.broadly@soc.uu.se>  
CC: Nubin Ciziri <nubin.ciziri@edu.uu.se>, Astrid Collsiö <astrid.collsio@edu.uu.se>, Mette Ginnerskov Dahlberg <mette.ginnerskov@edu.uu.se>, Pablo Lillo Cea <pablo.lillo\_cea@edu.uu.se>, Moa Lindqvist <moa.lindqvist@edu.uu.se>, Tina Vrieler <tina.vrieler@it.uu.se>

Hej Donald and everyone,

I really enjoyed the participation to this seminar, it is wonderful that such opportunity is available to us corridor's fellow!

Thank you also for your remarks, I will try to clarify what I meant (excuse my poor English):

- >When we at the end attacked the grand questions about
- >the relationship between natural sciences and human
- >sciences and about what is scientific in sociology, you
- >made the important point that the distance is greater
- >between (theoretical) physics and e.g. biology than
- >between biology and sociology.

Yes, this is one of the results of my thesis. Physicists, biologists and mathematicians are socially waaay closer to each other than they are to social scientists. They share a common trajectory in upper-secondary education (and in France generally two years after that also, via the very intensive "classes préparatoires scientifiques"), a large amount of formal and informal knowledge through this common socialization, they interact more often, understand each other better, share the same buildings, campuses, academies, interdisciplinary journals, conception of science, visions of the world, etc. So the distance I am talking about here is not between the scientists (subjects of the objectivation) but on the kinds of phenomena they study (objects of objectivation): in a nutshell, explanations on plants/animals/ecosystems are definitely more related to the explanation of human practices than to the ones on particles, stars, and other "not living things" (I really enjoyed the Durkheimian take on bees). Disciplines such as anthropology, psychology, medicine, neurosciences, etc., develop strong and direct links between biological and social sciences, whereas relations between biology and physics are generally mediated through chemistry (I showed that empirically). In a nutshell, my research strongly corroborate the Bourdieusian idea that the scientific "problems" of sociology are not so much scientific, but rather sociological.

- >Had we not run out of time I would have liked to
- >discuss one of your arguments, namely that the
- >capacity for prediction is a proberstone of a
- >mature science. I think that I disagree, or that I
- >might have misunderstood.

I could not find what proberstone means, though it sounds like corner stone (in which case I don't think so), but maybe you mean keystone (in which case I kind of think so). I try to develop my answer on these two meanings (but please correct me if I misunderstood) in the end of this email.

- >I have never thought of prediction in that way in
- >connection with the sociology of Bourdieu & Co.
- >Quite the opposite, in my lectures I use to tell the
- >students that for example the construction à la Bourdieu
- >of research objects (with the construction of spaces
- >as a prime component) the aim is not the kind of
- >prediction that mainstream sociology (celebrating
- >American models) strives to achieve (a working class
- >daughter will with high statistical probability enter
- >educational programs like the preschool teacher one,
- >and not a program like medicine). The aim of, e.g.,
- >the construction of a space is to uncover the forces

- >that impose themselves on everyone who enters or
- >resides there.

I certainly agree, Bourdieu's aim was not to predict the position that someone would attain. Though he devoted a substantial part of his work to the explanation of the distribution of these probabilities among and between groups (as in the example we have seen of the higher probability for a physicist with a higher social background to select "risky" research objects than for an equivalent physicist with lower social background). And you are perfectly right, his fundamental contribution to such study was its ability to take precisely into account the structured space of forces that constrain individuals' spaces of (objective and subjective) possible practices.

- >For example, the trajectory of an individual cannot
- >be predicted in advance since it is the outcome of
- >the encounter between the structure of this field of
- >forces and the properties of the individual, and this
- >outcome is produced within a field of competition.
- >I am simplifying, of course.

I agree again, the trajectory of an individual is hardly predictable in the current state of sociological knowledge. I would not use "cannot" though, as it sounds as a universal impossibility, which is somewhat equivalent to the statement of an universal law :). That does not mean, of course, that I am sure that sociology, some day, will predict the trajectory of any individual. Actually I hope not, but I do not exclude this possibility a priori. On the other hand, I think that the example you take is restrictive to what a prediction is. When physicists make predictions, they are extremely rarely based on one small unit : the movement of a molecule of gaz is random (Brownian motion); the motion of a gaz is strongly predictable (Boyle-Mariotte law). On this question, I recommend for those who understand French this video [<https://afs.hypotheses.org/72>] of Yves Gingras (both important and funny; full text is here [<https://journals.openedition.org/socio/2564>]). Regarding the limit of predictability of physics, Yves suggested me a long time ago a book called The Dappled World by Nancy Cartwright's (not Bart Simpson's voice, the other), that I found helpful.

- >Another way of discussing relations between mature
- >natural sciences and maybe less mature human ones
- >is by raising the classical question of laws. Here
- >Bourdieu was an extremist in his belief in sociology's
- >potential to mature. I remember from the memorable
- >conference "Empirical Investigations of Social Space"
- >at the University of in Cologne in 1998 his answer when
- >someone in the audience asked whether or not there could
- >be laws in sociology in the same manner as in physics.
- >Yes definitely, Bourdieu answered, and mentioned his own
- >sociological version of the economists' Gerschenkron effect
- >as an example (that newcomers/parvenus tend to borrow
- >models from the established). And, he continued, in the
- >future we sociologists will discover many more laws that
- >govern the social world.

I would say that Bourdieu was rather an optimist for his discipline than an extremist, but I think this message is more reasonable than the one from the late J.-C. Passeron (in *Le Raisonnement Sociologique*) who, on the contrary, stated that sociology is not and cannot be a science as the other ones (this sounds more extremist to me, and he makes a very weak case in this book).

To answer now on my conception (that I think Bourdieu compatible) of the relation between prediction and scientificity:

History of science can be seen as an attempt from a group of people to gain the legitimate monopoly of the explanation of the world via rational reasoning. Physics had to struggle pretty hard with religious, climatologist nowadays have to face Trumps official (I don't think he is that stupid) positions, etc. Sociology has always been attacked, and a recurrent argument is that it is not a science.

The main way other sciences have succeeded in distinguishing (in the sociological acception) their practices from their rivals (both within and outside science) was in the accuracy of their analysis. This, suppose an agreement in the definition of accuracy, and prediction have frequently been used as a benchmark to compare the validity of two conflicting/competing theoretical statements.

This does not necessary mean that a science must be able to predict in the sense of foreseeing a phenomenon that has not happened yet. Predict, etymologically, means "said before". In that sense I think that prediction is, indeed, of great importance for science as it allows to compare the accuracy of two statements on a given phenomenon. Two sociologists with different theoretical viewpoints should be able to decide an empirical case (past, present or future) and declare in advance (predict) of its analysis the differences in their expected outcomes (not just their interpretation on known outcomes). Under this definition, prediction is indeed, in my opinion, a piece without it the scientific arch cannot stand (a keystone), crucial for the demarcation of science from other activities and within science between competitors. But it is not the first element laid when founding a science (cornerstone). The foundation of science, to me, is discovery, a practice that is, precisely, very unpredictable.

Thank you again for these questions! Attached is the paper I talked about during the seminar on the possibility of a scientific breakthrough at any point of a career.

Happy week-end to everyone,

Yann

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Subject: Physics and sociology

Date: Sun, 2 Feb 2020 19:08:23 +0100

From: Donald Broady <donald.broad@soc.uu.se>

To: Yann Renisio <yann.renisio@gmail.com>

CC: Nubin Ciziri <nubin.ciziri@edu.uu.se>, Astrid Collsioö <astrid.collsioo@edu.uu.se>, Mette Ginnervskov Dahlberg <mette.ginnervskov@edu.uu.se>, Pablo Lillo Cea <pablo.lillo\_cea@edu.uu.se>, Moa Lindqvist <moa.lindqvist@edu.uu.se>, Tina Vrieler <tina.vrieler@it.uu.se>

Cher Yann,

sorry for my Swenglish, the direct translation of the Swedish word "probersten" that I tried was unsuccessful. The correct translation seems to be "touchstone", in French "pierre de touche", i.e. something used to test the validity of something. Literally a stone used to test if something is really for example gold. What I had difficulties to understand was your statement that prediction capacity is a touchstone by the help of which we might distinguish a mature scientific discipline. Thanks to your email yesterday I understand somewhat better what you mean by prediction.

As yet I am not totally convinced, though. I am also sceptical to another statement that we discussed during Thursday's seminar, namely Bourdieu's idea that international proliferation of the research is a criterion of a mature discipline. You were more ready to take Bourdieu's stand.

Here is one example to illustrate my hesitation in both these respects. I myself have many years (fifty to be exact, unbelievable) been fascinated by "die neue Marx-Lektüre", i.e. the reinterpretations of the critique of political economy that Marx developed during his last thirty years, reinterpretations that started with seminal works by Roman Rosdolsky and others published in the second half of the 1960s and revitalised especially by Michael Heinrich and followers from the beginning of the 1990s. Still a vital domain, however almost exclusively within the German language sphere. As far as I know not very much of a reception in France. (When young and naïve in the early 1980s I tried to share my enthusiasm with Bourdieu. In my mind there were some interesting affinities between those Germans' new understanding of Marx' programme and Bourdieu's own programme. Bourdieu knew nothing about it and didn't want to know.) Neither in the Anglosaxon world outside of very limited circles. (Some years ago I gave a lecture at University of East London on the relations between Marx' value forms and Bourdieu's symbolic capital, in the hope for reactions from the audience. There are lots of radical young Marxists at that university. They understood nothing of what I tried to say.)

I have hard to accept that the lack of international spread says anything about the maturity of this German research.

And when it comes to prediction capacity I still have some difficulties to see how it could be used as a touchstone when considering the maturity of research in the disciplinary fields where those Germans reside, such as philology or history of science. But I might be convinced to think otherwise. It might be in line with your reasoning that prediction in those kinds of (sub)disciplines means that, let's say, Michael Heinrich's in my opinion groundbreaking findings concerning some passages in or aspects of Marx' œuvre will be valid for other not yet researched passages and aspects.

Fridens  
/D

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Subject: Re: Physics and sociology  
Date: Sun, 2 Feb 2020 22:53:57 +0100  
From: Yann Renisio <yann.renisio@gmail.com>  
To: Donald Broady <donald.broadly@soc.uu.se>  
CC: Nubin Ciziri <nubin.ciziri@edu.uu.se>, Astrid Collsiö <astrid.collsioo@edu.uu.se>, Mette Ginnervskov Dahlberg <mette.ginnervskov@edu.uu.se>, Pablo Lillo Cea

<pablo.lillo\_cea@edu.uu.se>, Moa Lindqvist <moa.lindqvist@edu.uu.se>, Tina Vrieler  
<tina.vrieler@it.uu.se>

Dear Donald (and corridor fellows),

Below, a try on your tricky (in a good way!) questions. I found this way of answering within the actual email as a good strategy to limit my tendency to digress.

- > sorry for my Swenglish, the direct translation
- > of the Swedish word "probersten" that I tried
- > was unsuccessful. The correct translation seems
- > to be "touchstone", in French "pierre de touche",
- > i.e. something used to test the validity of something.
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- > capacity is a touchstone by the help of which we
- > might distinguish a mature scientific discipline.
- > Thanks to your email yesterday I understand
- > somewhat better what you mean by prediction.
- > As yet I am not totally convinced, though.

So many stones! Thank you for that precision. I was not expecting to convince you so easily, it will take time to reach this... milestone. But if you allow me, I'd like to ask you a question: is there, in your opinion, a good way to hierarchize accuracy between theories in social science? Say, between rational actor theory and Bourdieusian's theory of practice? If so, which criteria would you recommend?

- > I am also sceptical to another statement that we
- > discussed during Thursday's seminar, namely
- > Bourdieu's idea that international proliferation
- > of the research is a criterion of a mature discipline.
- > You were more ready to take Bourdieu's stand.
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- > these respects. I myself have many years (fifty to be
- > exact, unbelievable) been fascinated by "die neue
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- > critique of political economy that Marx developed
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- > 1960s and revitalised especially by Michael Heinrich
- > and followers from the beginning of the 1990s. Still
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- > the German language sphere. As far as I know not
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 >a lecture at University of East London on the relations  
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 >convinced to think otherwise. It might be in line with  
 >your reasoning that prediction in those kinds of  
 >(sub)disciplines means that, let's say, Michael Heinrich's  
 >in my opinion groundbreaking findings concerning some  
 >passages in or aspects of Marx' œuvre will be valid for  
 >other not yet researched passages and aspects.

I understand your skepticism, especially as the "proof administration" in philology and history of science is particularly complex (on these questions, I am a big fan of Norbert Elias developments in his Essays on the sociology of knowledge and the sciences. I have a version in French in my office for those interested).

On the other hand, if we accept the premise of the scientific field as defined by Bourdieu...

"A kind of game whose particular stakes consist in the monopoly of scientific authority (prestige, recognition, fame and so forth), the scientific field owes its main characteristics to the fact that \*the producers generally have no other possible clients than their direct competitors\*." (Bourdieu 1976, my highlighting).

It seems reasonable to me that /over time/, a "more mature" field should broaden the community of its competitors outside of a national space (while increasing its entry cost).

As I am well (and sadly) aware that autonomy of the scientific field, especially in the social sciences, is veeery relative, it is of course possible that relevant progress or breakthrough were wrongfully ignored (it took time for Weber or Elias to be understood). But as a general trend (forgive a statistician's heart), I think that the number of presumed "cursed scientist" (in analogy to the "poètes

maudits") is considerably higher than the ones of actual relevance. And, in my naive scientist optimism, I kind of believe in the long term "justice immanente du champ scientifique", excuse my illuso.

I would be happy (and probably others) to learn more from these new readings of Marx. Do you have any recommendation for something to start with? Preferably in French, English or Spanish, as my German is nonexistent... Perhaps you could share the text of the presentation you were talking about?

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Amicalement,

/Y

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Subject: Re: Physics and sociology

Date: Mon, 3 Feb 2020 13:46:51 +0100

From: Donald Broady <donald.broady@soc.uu.se>

To: Yann Renisio <yann.renisio@gmail.com>

Cher Yann,

Thanks for clarifications and more reading suggestions, and for the link to Yves' lecture and article (fascinating), and not the least for your thesis that is highly relevant in relation to my interests. The discussion will go on.

You asked for something to read on the Germans' "new Marx reading". I have made some attempts to trace affinities between Marx' value forms (as interpreted by those Germans) and Bourdieu's symbolic capital. See below links to the slides from a lecture that I gave in London 6 Nov. 2013, and also a published article on the same matter, unfortunately in Swedish. There you will find references to a number of important authors within "die neue Marx-Lektüre" current. Today's superstar among those Marxist philologists is (and has been since he presented his remarkable thesis in the beginning of the 1990's) Michael Heinrich.

<http://www.skeptron.uu.se/broady/dba-b-20131106-broady-value-form-handout.pdf>

<http://www.skeptron.uu.se/broady/dba-b-20140215-broady-vardeform-tekla-faksimil.pdf>

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Amicalement

/Donald

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