11 Social Capacities

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INTRODUCTION

Nancy Cartwright is commonly held to advocate the capacities concept as a central tool for the philosophical analysis of practice in natural and social science alike. But it would be wrong to ascribe to her the view that social phenomena are governed by causal factors with stable capacities (or *social capacities* in short). Her point is rather that the methods many social scientists use presuppose, in order to be successful, the existence of capacities. But since in her view the record of success in employing these methods is at best mixed, to be consistent she cannot believe that the social world is actually governed by capacities.

Already in *Nature's Capacities and Their Measurement*, which originally introduced the capacity concept and, in fact, used econometric practice more than once to prove a point, Cartwright employs John Maynard Keynes in order to express scepticism about the reality of social capacities. According to Keynes, the universe consists of bodies 'such that each of them exercised its own separate, independent and invariable effect' (Keynes 1957/1921: 249, quoted from Cartwright 1989: 156). However:

> We do not have an invariable relation between particular bodies, but nevertheless each has on the others its own separate and invariable effect, which does not change with changing circumstances, although, of course, the total effect may be changed to almost any extent if all the other accompanying causes are different. (Keynes 1957/1921: 249, quoted from Cartwright 1989: 156)

The contribution a factor makes to a situation is thus dependent on the arrangement of all other factors. In other words, the Keynesian world is "holistic". As a consequence, John Stuart Mill's methodology of analysis and synthesis cannot be applied.

In a different context, Cartwright uses ideas of members of the German Historical School to shed doubt on the reality of capacities in social phenomena. In an encyclopaedia entry on capacities, she writes:

> Just as the analytic method of Newtonian physics was challenged by Goethe and others in his particular German tradition, the analytic method of classical economics laid out so clearly in Mill was rejected by the Historical School dominant in German political economy at the end of the nineteenth and beginning of the twentieth century. [. . .] Gustav Schmoller, in particular, is famous in methodology for his insistence against Carl Menger that history and political economy could not employ exact universal laws as physics does. For Schmoller, as for Mill, economic phenomena are brought about on each occasion by a myriad of interacting causes. But for Schmoller, the role each cause plays depends on the total context in which it is set. So although separate causal factors can be identified and reidentified from one context to another, the separate causes do not have stable capacities. (Cartwright 1998: 45, emphasis added)

The aim of this chapter is to investigate how well-founded Cartwright's scepticism is. In order to do so, I first review some essential aspects of the capacities concept and its application to social science in the next section, 'Capacities'. In the following section, 'Are there social capacities?' I play devil's advocate and present three case studies that give good reason to believe that our prospects for finding social capacities are very grim indeed. In the penultimate section 'How well-founded is scepticism about social capacities?', I then discuss whether Cartwright is right. For that I distinguish two forms of scepticism, atheism and agnosticism. I argue that there is good reason to be agnostic but little for being a fully fledged atheist. I conclude by pointing out a number of methodological modifications social science could adopt to make it more likely to find social capacities.

CAPACITIES

Cartwright is a causalist in the sense of believing in the reality of causal relations or the reality of properties with genuine causal efficacy. In her conceptual framework, causal concepts are primitive: They cannot be further analysed in terms of laws of nature, relations of counterfactual dependence, or the like. Among the causal concepts, the capacity concept adds to the idea of causality the ideas of potentiality and stability. Saying that some X has the capacity to Ψ tells us something about what X does potentially: When X operates unimpeded, it produces Ψ. However, even when this process is interfered with, X will tend to or try to do Ψ. In other words, if there are causal factors present that impede on X's action to do Ψ, X will still contribute to the overall result. Secondly, the ability of X to Ψ must be stable across some range of circumstances if it is to count as a capacity.

To give a hypothetical example from social science, let us assume that economists have established that the growth of money in an economy has the capacity to raise the general level of prices. According to the conception that Cartwright defends, this means (a) that the growth of money is not only correlated with the level of prices but it actively produces its increase, (b) that this says nothing about the actual behaviour of the price level, since there can always be factors such as technology shocks or international trade which can interfere with the operation of the money stock; however, even in such a situation, money will *contribute* to the actual behaviour of the price level, and (c) the ability of the growth of money to increase the price level is stable across some range of situations, for example across different capitalist economies, under different monetary regimes, etc.; however, it is possible that there are situations (in systems with radically different economic constitutions, say) where money does not have this capacity.

What is the relevance of capacities for social science? The answer is conditional: If there are social phenomena that are governed by factors with capacities and they are epistemically accessible, then the social scientist and/or engineer is helped in realising his epistemic and pragmatic aims. The aims of social science, I take it, can be described by the tripartite expression explanation-prediction-control (see, for example, Menger 1963). Knowledge about capacities helps in explaining social phenomena. This is evident from their causal nature and the wide acceptance of causal models of scientific explanation. Capacities can also help in predicting phenomena. It is an analytic truth that capacities allow to make true conditional predictions about phenomena of the form: 'If nothing interferes, doing *X* will result in Ψ.' This is an exact prediction but nonvacuously true only if the antecedent is fulfilled. On the other hand, if disturbing factors do occur, the prediction will be inexact and about the contribution of *X* to the overall result.

Knowledge about capacities may also help in planning and control. The matter is, however, slightly more complicated here. When Mill originally introduced the tendencies concept, after which Cartwright modelled her own concept, he did so in order to save the truth or universality of natural laws in the light of apparent disconfirmations due to intervening factors.[1] Suppose we have a particle which is subject to two forces, one that pulls in *x*-direction and another that pulls in a 135° angle to it. When each force operates on its own, the law that describes its behaviour is true. But when both forces operate jointly, the law, understood as a function from forces to actual *results* (in this case, motions), is literally false. This is because part of the motion in *x*-direction that would have occurred had the first force operated on its own is upset by the second force and vice versa. The motions do not occur in the way the (individual) laws predict. But if we understand the law as an ascription of a tendency or capacity, its truth is salvaged. This is because, although the motion that actually occurs is not the one predicted by either law, each law is true of the *tendency* to produce its characteristic result, and each force contributes to the overall result.[2]

Therefore, the kind of stability the concept requires is a stability "under interferences". But this does not imply that the *X*-Ψ relation must be stable under *all* interferences and, in particular, under interferences which destroy the causal structure on account of which the capacity arises. An economic example will illustrate this claim. Let us assume that the Phillips curve has a true causal reading in the capacities sense. That is to say, let us assume that the unemployment gap (the difference between the actual and the "natural" unemployment rate) has the capacity to decrease inflation. This means (a) that the unemployment gap causes inflation, (b) that any precise formulation about the exact relation between the two quantities is true only potentially, that is, in the absence of interferences, and (c) that when other factors capable of affecting the inflation rate are co-present, the unemployment gap still contributes to the overall result. But it does not mean that the capacity must still be there if the economy has been intervened on, for example by changing the wage-setting process. Imagine, for example, an intervention that changes the labour market from a purely centralised and unionised system to a localised and free system. This will surely have effects on the rate with which the unemployment gap changes the inflation rate, i.e. on the strength of the capacity, but possibly even on its existence (i.e. it may change its strength to zero).

The ability to plan and control presupposes knowledge not only of capacities but also of a second kind of stability, which in econometrics is called autonomy. An autonomous relation is essentially one which remains stable under (some range of) interventions. Stability across some range of circumstances is part of the concept of capacity, but the difference between the circumstances does not have to involve interventions. All interventions change circumstances though, and hence, all autonomous relations involve a capacity but not necessarily the other way around. However, thus construed, knowledge of capacities again helps in realising (this time, pragmatic) scientific aims; it is just that the economic planner needs to know something more too.

ARE THERE SOCIAL CAPACITIES?

If there are stable capacities in a domain of interest, research is aided in a number of ways. Most importantly, claims that have been established with respect to a certain test situation *X* are exportable outside *X*. For example, if we judge on the basis of the Stanford/NASA gyro experiment (see Cartwright 1989: Ch. 2) that coupling has the *capacity* to affect precession in the way the experiment tells us, then we assume that coupling affects precession also outside the experimental situation. True, outside the test situation coupling may *result* in no precession at all. But that means that there is an inhibiting cause which prevents the capacity from being exercised.

Although Cartwright has voiced her scepticism in a number of papers, talks, and in personal communication, she never really defends it with respect to modern social science (with one exception that I will discuss below). The purpose of this section is to examine a number of significant methods of causal inference in social science that indeed give reason to believe that the causal claims established by them are not claims about capacities. My argument then is straightforward. Were there (knowable) social capacities, we would (probably) be able to find out about them with our best methods. However, analysis of our best methods in social science shows that these methods are not capable of finding capacities. Therefore, (probably) there are no capacities.

Exhibit I: The Vanity of Rigour

In the "Vanity" paper (Cartwright 1999), Cartwright argues that the thought experiments or "toy models" we find every so often in theoretical economics do not provide evidence for capacities. This is due to the fact that these models employ many "non-Galilean" idealizations, which implies that one cannot attribute the effect to the cause of interest as its "characteristic effect".

The concepts of "Galilean" idealization and capacity are closely linked. An idealization is Galilean if it helps in learning about operation of a causal factor free from disturbances. Galileo's own thought experiments on falling bodies are good examples. In one thought experiment, Galileo asks us to imagine two bodies falling from a tower without air resistance, a heavier cannon ball and a lighter musket ball. According to the Aristotelian tradition, the heavier ball falls at a faster rate and hits the ground first. However, if we now suppose that we join the two balls with a string, the Aristotelian theory falls into a contradiction. This is because we can derive that the amalgam falls both faster as well as slower. On the one hand, it falls slower because the lighter ball pulls the heavier one upwards and thus slows down the ensemble. On the other hand, the two balls together are heavier than the heavy ball alone and thus should fall faster. Hence, Galileo argues, in a vacuum all bodies fall at the same rate.

The assumption of no air resistance, then, is a Galilean idealization as it helps us learning what the Earth's pull does to falling bodies in the absence of disturbing factors. In other words, it helps us in learning about the capacity of the Earth to attract heavy bodies. Ernan McMullin, in his paper 'Galileian Idealization' (McMullin 1985), distinguished a number of kinds of idealization, but in the present context the kind he calls 'causal idealization' is most relevant. McMullin writes,

> And it is this sort of idealization that is most distinctively "Galilean" in origin. His insight was that complex causal situations can only be understood by first taking the causal lines separately and then combining them. [. . .]
>
> The move from the complexity of Nature to the specially contrived order of the experiment is a form of idealization. The diversity of causes found in Nature is reduced and made manageable. The influence of impediments, i.e. causal factors which affect the process under study in

> ways not at present of interest, is eliminated or lessened sufficiently that it may be ignored. Or the effect of the impediment is calculated by a specially designed experiment and then allowed for in order to determine what the "pure case" would look like. [. . .]
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> Galileo is convinced that he has discovered the motion that "nature employs for descending heavy things". [. . .] It is "natural" in the sense that it defines what the body would do on its own, apart from the effects of causes (like the resistance of air) external to it. These latter are to be treated as "impediments", as barriers to an understanding of what the "natural" tendency of body is.
>
> (McMullin 1985: 265)

It is a commonplace that the models characteristic of theoretical economics are highly idealised. Cartwright points out that many of the idealizations employed are not of the Galilean kind. Consider Akerlof's famous lemons model (Akerlof 1970).[3] Akerlof's aim was to explain the phenomenon of a large price differential between new cars and cars that have just left the showroom, or, in more general terms, that markets where quality matters often experience lower than expected prices and exchanged quantities. The second-hand car market is an instance of this more general phenomenon.

Akerlof explained the phenomenon by pointing out that in such markets there is an asymmetry in the information distribution: sellers know more than buyers. After they learn about the quality of their cars, owners of lemons (bad-quality cars) will want to sell their cars and exchange them for new ones, whereas owners of good cars will keep their cars. Because the quality of the car is not observable to buyers, cars are priced at some average rate, which further increases the incentives of owners of bad cars to sell their bad cars and of owners of good cars to keep their good ones. Hence, quality, prices, and exchanged quantities drop.

To lend credibility to his story, Akerlof provides a mathematical derivation of the result in addition to a more intuitive thought experiment. As is very common in investigations of this kind, Akerlof makes a large number of assumptions in order to derive the result in the mathematical model. Cartwright points out that making these assumption is in fact a methodological prerequisite. This is, she claims, because the basic principles of economics (the equivalent to "laws" in physics) are both few in number and meagre.[4] As a consequence, there is not a lot of deductive power built into them. But this in turn means that many additional structural assumptions must be made if results are to be deduced mathematically.

Among the assumptions Akerlof makes is that there are two types of traders with distinct utility functions, and both types are von Neumann–Morgenstern maximisers of expected utility, that the cars' quality is distributed uniformly between zero and two, that goods are infinitely divisible, and that the price of "other goods" is one. Few of these are Galilean in nature. That is, few of the assumptions help us learn what asymmetric information does on its own. If one attempts to trace back responsibility for a result, one finds that not only the factor of interest is to blame, but so are all of the assumptions made—otherwise no result would have been obtained. But this in turn means that we have not isolated a tendency.

So what did Akerlof establish? In my view, he measured the causal effect of asymmetric information on quality and volume in the system he envisaged. To see this, note that his method of proof resembles very closely Mill's "method of difference". Remember that the method of difference infers the causal effect of a factor *F* by comparing two situations which are identical except *F* is present in one and absent in the other (and *F*'s effect if there is any). The difference *F* makes to the situation, then, is its causal effect. Akerlof does exactly that. He models a situation with symmetric and a situation with asymmetric information, and the difference in the market result is then attributed to the difference in the information distribution. But because since the result crucially depends on the assumptions made, we can judge it to be present only in systems of which Akerlof's assumptions are true.

The point is this. If we aim at establishing that a factor *X* has the capacity to Ψ, we had better make our conclusions as independent of the test conditions as possible. This is the lesson one can draw from McMullin's treatment of Galilean idealizations. Due to the particular manner in which results are determined in models such as the lemons model, however, conclusions are highly dependent on test conditions—in this case the model assumptions. Therefore, in themselves, they cannot establish a capacity claim.

One might object that the lemons model is a nonstarter as a tool for establishing capacities anyway. The model is a piece of theory after all, whereas a capacity claim is a claim about a particular kind of causal relations in the world. The reason I include theoretical models in my brief survey of methods in social science is that they are frequently taken—in themselves—to provide evidence for capacities. For instance, an argument why third-world labour markets fail might go as follows. In third-world countries labour markets often fail. In these markets quality matters. In markets where quality matters, asymmetric information can lead to market failure (we have established that with Akerlof's model). In third-world labour markets, the quality of labour cannot be observed by employers, i.e. there is an asymmetry in the information distribution. Hence, in third-world labour markets asymmetric information causes market failure.

This argument is obviously fallacious. One cannot argue from the fact that in a specific situation a causal factor is responsible for a result to the conclusion that it does so too in the envisaged situation. The least we need to do is to rule out all alternative explanations for the result. But worse, the result has been established only for a patently unreal situation (one where there are only two types of agents, both von Neumann–Morgenstern maximisers of expected utility, distinguished only by their respective utility functions, cars have only one property, viz., "quality", which is uniformly distributed, etc.). Thus one would first need to establish that the conclusion holds in a real experimental situation too before one exports it to other situations. Nonetheless, arguments of this kind can be found, so I wanted to include theoretical models here. Exhibit II examines a case where the conclusion is established experimentally.

Exhibit II: Natural Experiments in Economics

There is a movement in contemporary econometrics which has been labelled 'natural experiments movement' (see Heckman 1999). Its basic strategy can be summarised as follows:

> *Natural Experiments.* To measure the causal effect of *C* on *E*, find a set of economic units on which one can measure *E* such that one can partition them naturally, i.e. without intervention, into treatment group (where *C* is present) and control group (where *C* is absent) in a way that resembles a controlled experiment. That is, the distribution of factors, which are causally relevant to *E*, is identical in treatment and control group and the assignment of a unit to a group is independent of any factor that may be causally relevant to *E*. Then measure the causal effect by taking the difference between the averages of the *E*-values in treatment and control groups.

An example illustrates this. Economic theory predicts that a rise in the minimum wage leads employers to cut jobs. David Card and Alan Krueger challenged this (universal) prediction with an analysis of a natural experiment that occurred in New Jersey in 1992 (Card & Krueger 1994, 1995). In that year, New Jersey's minimum wage rose from $4.25 to $5.05 per hour. In order to measure the causal effect of the minimum wage rise (*C*) on the change in employment (*E*), Card and Krueger surveyed 410 fast-food restaurants in New Jersey and eastern Pennsylvania before and after the rise. The economic units of interest (the fast-food restaurants fall naturally into two groups, the ones in New Jersey, which form the treatment group, and the ones in eastern Pennsylvania, which form the control group). Several items of background knowledge allow the authors to judge that the natural setup resembles a controlled experiment sufficiently. They argue, for example, that 'New Jersey is a relatively small state with an economy that is closely linked to nearby states' (Card & Krueger 1994: 773), and therefore, one has no reason to believe that the distribution of factors that could be relevant to employment differs between New Jersey and eastern Pennsylvania. This choice of control group is further tested by means of a second control group, viz. restaurants in New Jersey, which initially paid at least $5.00 per hour wage, and thus should not be affected by the rise. In particular, they observe that 'seasonal patterns of employment are similar in New

Jersey and eastern Pennsylvania, as well as across high- and low-wage stores within New Jersey' (Card & Krueger 1994: 773.), such that the "natural development" of employment, which could confound their result, should be controlled for. There is furthermore no reason to believe that the change in the level of employment is dependent on the assignment to groups, that is, whether the restaurant is in New Jersey or Pennsylvania.

Card and Krueger present their results as follows:

> . . . we find no evidence that the rise in New Jersey's minimum wage reduced employment at fast-food restaurants in the state. Regardless of whether we compare stores in New Jersey that were affected by the $5.05 minimum to stores in eastern Pennsylvania (where the minimum wage was constant at $4.25 per hour) or to stores in New Jersey that were initially paying $5.00 or more (and were largely unaffected by the new law), we find that the increase in the minimum wage increased employment.
>
> (Card & Krueger 1994: 792, emphasis added)

I do not want to comment on whether Card and Krueger are successful in their analysis of the natural experiment (but for a discussion, see e.g., Neumark and Wascher 2000). They surely *try* to replicate the structure of a controlled experiment. The point to draw attention to is rather that *if* their results are valid, they cannot be understood as an ascription of capacity. What I believe they can claim is that *ceteris paribus*, raising the minimum wage increases employment. But since one way of understanding statements of *ceteris paribus* law is as an ascription of capacity (Cartwright 2002), the difference I point out requires elaboration.

To ascribe a capacity to a causal factor means that one believes that certain inductive inferences are licensed. Surely the usual inference to all situations that are relevantly similar to the test situation is made. But, importantly, we know more: Even when the conditions of the test situation are not fulfilled, if the causal factor is present, it will still "try" or "tend" to produce the result. If it does not succeed, then there must be a very good reason for it, viz. a countervailing capacity. For example, saying that the Earth has the capacity to attract heavy bodies means that it will still try to do so even when gravity does not operate on its own. Now, if the Earth does not succeed in attracting a given heavy body, we ascribe this failure to the presence of another capacity, for example a strong magnetic field above the body that pulls it upwards.

Inferences that are licensed by a *ceteris paribus* law in the sense used here are narrower in scope. They allow only the usual induction to cases that more closely resemble the test situation. My point about the Card and Krueger paper is that they can make (at best) only the latter kind of inferences, not the former, broader kind. They did not find a general truth about minimum wages (nor do they believe they did). Rather, what they found (again, of course, if their results are valid) is a law that under certain conditions raising the minimum wage will increase employment. What these conditions are is difficult to say. Crucially, however, the failure of raising the minimum wage to produce more employment in a very different situation (e.g., when the minimum wage is already very high, when the rise is very large compared to its level, when economic conditions are radically different, etc.) will not induce us to seek for a countervailing tendency. Rather, we will attribute the failure to a relevant difference between the two situations we have compared.[5]

One may object that this relevant difference is exactly a countervailing tendency and that the difference is only terminological. However, I think that reading the claim as a capacity claim would be highly unnatural. Let us suppose that there is a second natural experiment involving two different states with characteristics very similar the Card and Krueger case. One difference is that the minimum wage in these states is initially much higher, say, $10.00. Let us also suppose that raising the wage to $12.00 results in a *de*crease of employment. Now it seems to me that it would be absurd to say that there is a capacity of raising the minimum wage (of the first $5.00?) to increase employment, which is offset by a countervailing, stronger capacity (of the second $5.00?) to decrease employment, such that the overall result is negative. Rather, one would say that the situation differs in crucial respects and that the law we found in the first case is not at work in the second case.

This discussion, I believe, points towards a more general feature about thinking in capacities and thinking in *ceteris paribus* laws. Thinking in capacities presupposes a method of analysis and synthesis. Situations are broken down to tractable parcels, the behaviour of these parcels is analysed severally, and finally, the bits are synthesised to let us know about the initial situation. Among other things, the method of analysis and synthesis presupposes that it makes sense to investigate what the parcels do on their own. Many cases Cartwright examines have this property. It makes perfect sense to talk about bodies subject to no other force than gravity. Even certain physically impossible scenarios, e.g., the behaviour of bodies that have charge but no mass, are relatively easily conceptualised.[6]

In the social sciences, by contrast, the method of analysis and synthesis (in this sense) seems less applicable. No factor produces anything on its own. It does not even make sense to ask, for instance, what a minimum wage does in the absence of everything else. We need a thick network of causal conditions to produce any result. Furthermore, the result that is actually produced very often depends crucially on the conditions that are present when the factor operates. It seems then, that in such situations the language of *ceteris paribus* laws is more applicable than the language of stable capacities.

Exhibit III: Singular Causal Analysis

The example in the preceding discussion was one where the causal effect of a single event (the raising of the minimum wage in New Jersey on April 1,

1992) on a property measured on a population (employment in fast-food restaurants in New Jersey) was measured. At the microlevel, i.e. the individual restaurants, employment is caused by a variety of factors, many of which probably escape subjection to causal law. The characteristic effect of increasing the minimum wage was extracted using, among other things, the assumption that the distribution of all other causes of employment was identical between the "treatment group" in New Jersey and the "control group" in eastern Pennsylvania. In many cases in social science, however, we will not be so lucky as to have such favourable circumstances. In particular, in many cases both relata of the causal statement of interest will be singular events (when the questions are, e.g., whether a certain decision or a certain battle stopped a war or whether the decision of the Fed to increase interest rates on a particular date triggered the financial crisis in Asia). So how do we establish singular causal claims? Cartwright tells us that in many cases in the physical sciences such claims can be established by bootstrapping (see for example Cartwright 1989, 2000). In general, the bootstrapping methodology allows us to infer a hypothesis deductively from the data and background knowledge (Glymour 1980). In the Stanford/NASA gyro experiment example I have alluded to above, the relevant hypothesis is whether space-time curvature causes relativist precession of amount x, which is predicted by general relativity theory. Our background knowledge consists of a disjunction of hypotheses about the various sources of precession different from curvature coupled with the knowledge (or assumption) that all such sources have been controlled for successively. The data consists in the measurement result that precession is indeed x. Thus, we can derive the hypothesis from background knowledge and data deductively. More importantly, our background knowledge assures us that we have established a singular causal claim. Since nothing else *in this particular case* could have caused precession, space-time curvature must have been responsible for it.

In social science, unfortunately, the requirement about background knowledge seems unduly restrictive. This is for at least three sets of reasons. First, it seems impossible to find a disjunction of factors that could cause the phenomenon of interest that which exhausts all possibilities. Not only does experience tell us that such a list would be very long, it is also open ended. Nobody can predict the rise of the dot.com industry, but once that phenomenon is extant, it will serve in many causal explanations of other phenomena. Second, in social phenomena there is less room for manipulation. Very often the aim is the explanation of a historical event, where manipulation is impossible to begin with. But even disregarding that problem, experimental control is often out of reach for ethical, practical and economic reasons. The third difficulty is associated with the second one. In physics, even if we cannot literally control for a confounding factor, we can very often either calculate precisely the contribution of that factor or at least run simulations and thereby calculate upper limits. Most laws in social science, by contrast, are of a highly qualitative nature. Therefore, if the question is, say, whether a certain event has triggered a financial crisis, and we know that another factor that can contribute to financial crises was present, it is hard to tell whether the presence of that latter factor by itself would have been "enough" to trigger the crisis or whether the particular event we focus on was necessary in the circumstances.

These three sets of difficulties notwithstanding, a number of authors have attempted to tackle the issue of singular causation. To my knowledge, however, only Max Weber has developed a systematic account of causal inference in a single case. In my view, Max Weber is the only methodologist who has developed an account of singular causal inference tailored to the epistemic situation social scientists are often interested in. So let us examine whether his ideas can be exploited.

Two concepts are central to Weber's ideas about singular causal analysis: that of "objective probability" and that of "adequate causation". Objective probability is a term Weber originally borrowed from the German physiologist and statistician Johannes von Kries, who himself developed a tradition in the German legal philosophy (Ringer 1997: Ch. 3). Broadly speaking, an event is objectively probable[7] if the range of possibly relevant conditions under which it will occur is greater than the sum of further conditions under which it will not occur.

Weber first notes that, as I have discussed above, social phenomena are usually brought about by a vast number of factors[8], all of which are necessary in the circumstances for the result. In particular, against Mill Weber emphasises:

> Rather it is to be emphasized once and for all that a concrete result cannot be viewed as the product of a struggle of certain causes favouring it and other causes opposing it. The situation must be seen as follows: the totality of *all* the conditions back to which the causal chain from the "effect" leads had to "act jointly" in a certain way and in no other for the concrete effect to be realized. In other words, the appearance of the result is, for every causally working empirical science, determined not just from a certain moment but "from eternity". (Weber 1949: 187)

The first step in his causal analysis is that a number of factors of interest are isolated from the network of interacting causal factors.[9] When, for example, Eduard Meyer asks whether the battle of Marathon was significant for the subsequent development of Western civilisation, we notice that a myriad of factors is responsible for the development of our civilisation as it actually occurred, but we single out a particular event of interest *C*, the battle of Marathon, and ask whether it was significant for the phenomenon of interest *E*, viz., the development of Western civilisation.

The essential mechanism to answer a causal question of the form 'Did event *C* cause event *E*?' is to ask oneself if *E* would be expected had *C* not occurred, or in Weber's words:

> in the event of the exclusion of that fact [C] from the complex of the factors which are taken into account as co-determinants, or in the event of its modification in a certain direction, could the course of events [E], in accordance with general empirical rules, have taken a direction in any way different in any features which would be *decisive* for our interest? (Weber 1949: 180)

Thus, we ask whether either subtracting C from the course of events or modifying it to C' would have made a difference to E. Now in order to judge whether the change in C would have made a difference, we ask in a second step whether the occurrence of E was *objectively probable* given only the conditions or factors F that were co-present with C but now without C. For Weber, the question is thus whether the event $E \mid F.{\sim}C$ was "to be expected". If the answer is Yes, then C is judged to be causally insignificant, and if it is No, then C is causally significant. Weber then uses the term "adequate causation" to label cases where C did change the objective probability of *decisive aspects* of E, while he reserves the term "chance" or "accidental" causation to cases where C may have changed aspects of E that were not essential or decisive from the point of view of the inquiry of interest.

The details of Weber's analysis are not relevant for my argument, so I will not discuss them here. Let me just point out a worry. Weber takes it to be a necessary condition for causality that cause-events should make a difference to the probability of effect-events. However, there may be cases where the cause-event leaves the numerical probability of the effect-event unchanged but still is causally connected with it (and in fact responsible). The standard example discussed widely in the literature on causation is that of birth control pills and thrombosis. Pills cause (directly) blood clotting, but they also prevent it by preventing pregnancies, which themselves are one of the major causes of blood clotting. Now, the probability of a particular woman's getting thrombosis may be the same whether or not she takes pills because the probability-rise due to the direct effect exactly cancels the probability-lowering due to the indirect effect. Hence, Weber's method would wrongly conclude that (in this particular case) birth control pills did not cause thrombosis.

Let us assume, however, that Weber's method is sound. The point I make about it is that it does not help us in any way to learn about social capacities. The method is tailored to suit cases historians are interested in, that is, cases of singular causation. Whether or not a particular event did indeed raise the objective probability of another event is as good as irrelevant to the question whether it does so in other circumstances. Weber's method presupposes that all factors but the one we focus on behave regularly and that knowledge gained about them in other contexts is applicable to the case at hand too. But the knowledge it yields is tied to the one context under scrutiny.

The lesson of this section is this. Very widely used and important methods of causal inference in social science fail to yield knowledge about social capacities—for a number of different reasons. We might infer from this that Cartwright's scepticism is warranted. But there is a missing link in the argument. The inference presupposes that these are the best methods indeed to find capacities. In the next section I argue that there is something wrong with the way (at least some) social scientists use these methods. If that is true, one may grant Cartwright that there is no good (positive) reason to believe in the existence of social capacities. But I add the caution that there is no good (negative) reason to believe in their nonexistence either.

HOW WELL-FOUNDED IS SCEPTICISM ABOUT SOCIAL CAPACITIES?

So far, I have tried to give meat to Cartwright's scepticism about the existence of social factors with stable capacities. As any other form of scepticism, this variety can be read in two basic ways: as a positive disbelief and as a suspension of judgement. In this section I argue that Cartwright has good reason for the latter but little evidence for the former. In other words, I think agnosticism is a sensible stance regarding the reality of social capacities, and full-blown atheism is ill-founded.

Cartwright herself seems to oscillate between the two forms. Pretty dire sounds a joint statement with Jordi Cat in a paper on the German Historical School:

> The analytic method supposes that the causes of the phenomena of interest can be conceptually separated into distinct factors each of which has its own characteristic law of action. [. . .] Physics has been able to make effective use of this method in the study of motions; but political economy does not seem to lend itself to treatment by the analytic method.

And this is because:

> [The judgement about the above claim] is based on looking at cases of what is judged within the sciences themselves to be good practice. . . . (Cartwright & Cat 1998: 2)

I offer two arguments for the weaker reading, according to which it is more sensible to just suspend judgement rather than to claim positively that 'political economy does not seem to lend itself to treatment by the analytic method': a cheap and nasty one and a more involved one.

The cheap and dirty argument is that philosophers of science often make an unfair comparison of social with natural science. I would always tend to agree with Cartwright and Cat that '[p]hysics has been able to make effective use of [the analytic] method in the study of motions' and, in fact, in the study of many other phenomena. However, the claim that economics (or social science more generally) has failed to make use of the analytic method seems inequitable.

The social and those parts of the natural world where the analytic method has been applied most successfully differ in a number of important (and well-known) respects.[10] Let me mention just a few. Social phenomena (of interest) tend to be complex while natural phenomena (of interest) tend to be simple. Social phenomena (of interest) tend to be unstable and evolve over time while natural phenomena (of interest) tend to be stable and immutable. Social kinds tend to be interactive while natural kinds tend to be inert.[11] Social systems tend to prohibit experimentation while natural systems tend to allow it.

In my view, none of these differences motivates a principled distinction between natural and social science, but they tend to make causal inference in social science harder. Coupled with the (contingent) fact that social scientists tend to be interested in relatively young phenomena (such as the capitalist economy), it seems unfair to demand from them results comparable to those of their physics colleagues, who have had thousands of years to analyse their phenomena.

This argument is cheap and nasty indeed. Let me provide a second, more involved, argument. One of Cartwright's methodological principles for finding out about the nature of a subject matter is to investigate the best methods employed in the science that studies the subject matter and make inferences on that basis. This explains the selection of theoretical modelling in economics, the natural experiments movement, and Weber's singular causal inference scheme which have all been discussed above. I think that this methodological principle, defensible or not for other sciences, fails in the case of economics. The reason is simply that economics' so-called "best" methods are still characterised by a methodological oddity natural science was able to overcome in the seventeenth century.

The argument, in short, is as follows. Most economists, and, increasingly, other social scientists as well, presuppose a lot of theory in their empirical work. This, in my view, results in a certain disability to establish the existence of or facts about social phenomena.[12] Knowledge about capacities, however, is parasitic upon knowledge about phenomena. Hence, the theoretical bias also impedes learning about social capacities.

Let us therefore examine how social scientists establish phenomena. The best place to look for a sound methodology of social observation and measurement should be the early work of the Cowles Commission. Jakob Marschak, Tjalling Koopmans, Ragnar Frisch, and others here established econometrics as a proper branch of economics through a combination of mathematics, statistics, and economics. Importantly, at least in the early years they regarded measurement as central to economics and adopted Kelvin's dictum "science is measurement" as the motto for the Commission.[13]

However, as much as they were interested in empirical investigation, theory was to play a strong part. In particular, they rejected the institutionalists' attempts to base economic analysis on empirical and historical investigation without recourse to theory. By contrast, they were aiming at a combination of theory and measurement in which the most fruitful use of both could be made. A good statement of this agenda can be found in Koopman's review of Arthur Burns and Wesley Mitchell's *Measurement of Business Cycles* (Burns & Mitchell 1946). He writes,

> . . . this reviewer [Koopmans] believes that in research in economic dynamics the Kepler stage and the Newton stage of inquiry need to be more intimately combined and to be pursued simultaneously. Fuller utilization of the concepts and hypotheses of economic theory . . . as a part of the processes of observation and measurement promises to be a shorter road, perhaps even the only possible road, to the understanding of cyclical fluctuations. (Koopmans 1995/1947: 492)

Although I accept that theory sometimes can play a role in observation, measurement and experimentation, I deny that it is necessary, and in particular I reject the dogmatism with which economic theory is acknowledged as *sine qua non* of economic measurement.

To see that theory is not necessary, consider William Stanley Jevons's investigation of the phenomenon of monetary inflation (Jevons 1863). Without an essential use of theory, and surely not of economic theory in the modern sense (which he was to co-invent), Jevons successfully establishes that the gold discoveries of the 1840s in Australia and California led to an increase in prices of about 13 percent. True, Jevons believed in the quantity theory. But with his investigation he tested the quantity theory at best and never presupposed it or used it in the construction of the measurement procedure. Further, Jevons makes use of the fact that prices are caused by what he calls 'the conditions of supply and demand'. Again, one might think that economic theory—in some sense—is sneaking in here, but in fact all that amounts to is a conceptual divide of causal factors into two groups.[14]

Popper is famous for stressing the principle "theory before observation". Now, if we accept—pace Jevons—that in order to make sense, observation must be made in relation to *some* theory, even Popper would regard it as pure dogmatism if it was taken for granted that it must be *a particular* theory and that that theory is beyond questioning. Economic theory (in the sense of a canon of general presuppositions and methods), however, does have such a position. It is not very surprising, then, that the empirical results of the early Cowles Commission have been disappointing.

The Cowles Commission is not the only place to look for this theoretical bias. I used their example because they are the inventors of modern economic measurement and therefore should speak with some authority. To turn to a more contemporary example, reconsider the natural experiments movement. On the face of it, it seems that by following this approach one can do solid empirical work without much theory.[15] However, a main criticism that has been levelled against it is exactly that the results cannot be interpreted in the light of economic theory and are therefore very limited in their usefulness. Even James Heckman, who himself is a proponent of the natural experiments movement and an ingenious developer of its methods, writes:

> Applications of this [natural experiments] approach often run the risk of producing estimates of causal parameters that are difficult to interpret. Like the evidence produced in VAR accounting exercises, the evidence produced by this school is difficult to relate to the body of evidence about the basic behavioural elasticities of economics. The lack of a theoretical framework makes it difficult to cumulate findings across studies, or to compare the findings of one study with another. Many applications of this approach produce estimates very similar to biostatistical "treatment effects" without any clear economic interpretation. (Heckman 2000: 85)

Why do economists get so excited about "theory" and "economic interpretation"? One reason is pointed out by Margaret Morrison: Sometimes there is a link between theory and our ability to carry experimental results to other contexts. Commenting on Cartwright's analysis of the Stanford/NASA gyro experiment, she writes:

> What the experiment shows is that in space the dragging effect produces gyro precession but that tells us nothing about frame dragging in other contexts; the theory tells us that this is a global effect and since the experiment bears out what the theory predicts will happen in space we consider it confirmed.
>
> (Morrison 1995: 168)

Morrison disagrees with Cartwright about whether we need the capacities framework in order to understand such exportability of results. Their disagreement need not concern us in the present context. What is important is Morrison's claim that if we have a theory which is universal throughout the specified domain, and we have good reason to believe our theory to be confirmed by a particular experiment or series of such experiments, then we also have good reason to believe that our experimental results are exportable to other contexts within that domain.

The complaint recorded by Heckman is, then, because we cannot bring the results achieved by proponents of the natural experiments movement to bear on economic theory—which, after all, is universal in its domain—we do not have an off-the-shelf mechanism that tells us how to export our claims beyond the particular experiment that established it in the first place. Metaphysically, my reaction to this obstacle was to bite the bullet and accept that in economics there may be truths that are entirely local and not at all exportable: i.e. to accept that we must understand many of the results of the natural experiments movement as *ceteris paribus* causal laws, where the *ceteris paribus* condition ties the result to the experimental population.

However, this reaction may have been too hasty. There are probably many causal factors in economics that are not as stable as the universal capacities we know from parts of physics but are more stable than a *ceteris paribus* causal law. One possible aim of future research in methodology is to find a number of "off-the-shelf" principles that are informative about how to export claims established by a natural experiment to other contexts. For example, we may ask whether it matters that Card and Krueger's study investigated fast-food restaurants, that the study was conducted on the East Coast, or that the initial minimum wage was $4.75. Geoffrey Hodgson, I believe, made some advance on this question. In his book *How Economics Forgot History* (Hodgson 2001), Hodgson attempts to answer what he calls the 'problem of historical specificity', viz. the problem of knowing how historically (and geographically) specific a claim about socioeconomic systems must be to have the potential to be valid. His response consists essentially in relegating concepts and principles to the right level of abstraction, five of which he distinguishes (see his Table 21.2: 326–327). Certain concepts and principles pertain to *all* "open, evolving and complex systems". At this level, theorising is informed by evolutionary theory, general systems theory and complexity theory. At the second level, concerning all human societies, human instincts and psychology as well as general anthropological principles govern theorising. The usual laws of supply and demand come into play at the third level, which concerns only "civilised and complex human societies", and the fourth and fifth levels differentiate between kinds of socioeconomic systems.

I understand this schema to be a schema for exporting claims beyond the experimental population. Certain properties are shared by, say, all open, evolving, and complex systems. If an experiment establishes a new result about such a property, we should be able to export it to all other open, evolving, and complex systems and similarly for the other levels.

In my view, Hodgson's schema fails for a variety of reasons.[16] But what is immensely valuable about it is that it provides a starting point for research on a topic which I believe to be of fundamental importance for methodology. In their empirical work, economists have usually attempted shortcuts that exploit economic theory in order to, for example, identify causal parameters in an econometric regression or aspects of a measurement procedure. The methodological point of view put forward in this chapter suggests that no such shortcut is possible. We need a methodology that is informative about empirical ways to determine how projectible claims established on the basis of experiments are.

Therefore, I do not believe that the current state of economics is a good place to examine what is possible in economic analysis. The empirical road has not been walked yet and we do not know what fruits it will bear. Tjalling Koopman rightly distinguishes a "Kepler stage" and a "Newton stage" of scientific inquiry—one of empirical generalisation and one of fundamental law. But he errs that one needs to pursue them simultaneously. Empirical laws do not require fundamental laws to be found. By contrast, fundamental laws are void unless established on the basis of a range of empirical laws. There is no shortcut to fundamental laws that bypasses empirical laws.

The lesson of this section is that, pace Cartwright and Cat, there is no reason to lose hope. Yes, the record of finding social factors with stable capacities is poor. But it is poor because much empirical work done in social science has presupposed a particular theory about human behaviour. In my view, this has incapacitated the ability to establish real social phenomena, which in turn makes learning about social capacities a near impossibility. Giving up reliance on economic theory, and allowing social science to be a more empirical, more Baconian science, may result in learning about real social phenomena governed by real social capacities. To be sure, no one can predict that one day we will find only a single social capacity. However, I believe that there is no reason not to try.

CONCLUSION: HOW WE MIGHT FIND SOCIAL CAPACITIES

The previous section ended with a mild optimism regarding the existence of social capacities. My hope to someday find such things is rooted in the conviction that there is something wrong with the way in which much of social science achieves its results. What I think is wrong is a certain dogmatism in an area one might label "phenomenal inference". Phenomenal inference is the establishment of phenomena on the basis of observations and measurements. Phenomena, the object of scientific explanations, do not lie around to be collected by the scientist, neither natural nor social. To take a hopefully uncontroversial natural scientific example, consider Newton's method of "deduction from the phenomena". What are the things Newton took as a basis for inferences? Surely not the naked observations of dots of light in the night sky (to take an example). Rather, he would construct a phenomenon such as the trajectory of a planet on the basis of observations or measurements made. Bas van Fraassen has recently remarked:

> Patrick Suppes had long emphasized that theories do not confront the data bare and raw. The experimental report is already a selective and refined representation, a "data model" as he calls it. This is especially true today, as Fred Suppe has emphasized, now that scientists routinely process gigabytes of data. It was already true in Newton's time when he claimed to deduce laws from the phenomena—for of course he used

> as basis very smooth functions distilled from thousands of astronomical observations. But it is true even of the idealized, simple observation report discussed by the logical positivists, as they themselves came to agree after some debate. (van Fraassen 1997: section 3.2)

There is no one way to infer a phenomenon on the basis of "thousands of observations". Especially in economics, choices such as the formula used to construct an index number or the specification of an econometric regression matter. As I have described in more detail elsewhere (Reiss 2002a: Ch. 4), in my view too much use of theoretical considerations is made in these inferences. It is as if Newton had used the laws in the process of constructing (or inferring) the phenomena from which he was to deduce his laws. Moreover, relatively theory-free approaches such as the natural experiments movement are regarded as deficient exactly for the fact that they cannot readily be connected to economic theory.

A more empiricist stance in economics would attempt to make inferences to phenomena with as little explicit reliance on theory as possible. Unlike proponents of natural experiments in econometrics themselves, I see nothing wrong with, say, finding out that, under certain conditions, an increase in the minimum wage causes employment to rise—even if that does not tell us much about elasticities. In a second step, research would proceed to investigate the stability of such a law. It would ask under what conditions minimum wages have what effects on employment. Finding a range of different conditions that affect the wage-employment relation differently, research may further proceed to drawing up hypotheses about mechanisms responsible for the different relations and thus explain them.

Effectively, this is partly what David Card and Alan Krueger do in their 1995 book. After the natural experiment in New Jersey, they analyse a second one in Texas; they reanalyse previous evidence from California, statewide, as well as international evidence from Puerto Rico, Canada, and Britain. Their aim, however, is only to undermine economists' traditional belief in the universal adverse effect of minimum wages. Hence it is enough for them to present a single case where an increase actually raised employment and to cast doubts on the validity of studies that find evidence for the opposite claim. They stop short of a systematic empirical investigation into the conditions and mechanisms responsible for the wage-employment relation. Further, they do try to explain their results by means of models of the kind discussed under "Exhibit I". Even for Card and Krueger, economic theory is sacrosanct. All they do is amend the simple model that predicts the negative effect slightly such that the resulting model predicts a positive effect.

Natural experiments à la Card and Krueger (as well as other models of causal inference that make little use of theory, such as Kevin Hoover's [2001] or the Bayes' Nets approach) do, nonetheless, provide a starting point. On their basis, a range of phenomena can be established, phenomena of the kind "under conditions *xyz*, increases in the minimum wage lead to higher employment", "for population *P*, schooling increases earnings" or "in system *S*, money causes prices". Phenomena can then be classified according to similarities and dissimilarities as well as compared and analysed. Again, on the basis of such a classification and analysis, attempts can be made to explain them with reference to underlying mechanisms. If we are lucky, such mechanisms have parts that can be used in the explanation of a range of different phenomena. They may be factors with (relatively) stable capacities.

This Baconian vision of social science is not new. It is essentially what social science would have looked like had the discipline followed Gustav Schmoller's methodological principles (see Schmoller 1998/1911). Ironically, then, I ask to use Schmoller's ideas to achieve what he himself thought would be impossible. As we have seen near the beginning of this chapter, Schmoller argued against Mill that social factors do not have stable capacities that can be moved from situation to situation and that, in general, the analytic method is not applicable to social systems. But Schmoller may have been overly hasty in his conclusion. There has never been a prolonged attempt to do social science the way he envisions it. Nonetheless, if we want to find social capacities, I do not currently see any better way.

NOTES

1. There seem to be differences, however, between the nineteenth-century concept of tendencies and Cartwright's concept of capacities. For a discussion, see Schmidt-Petri (this volume).
2. It is important to notice that we cannot salvage a law-as-regularity view by claiming that the account presented here simply misdescribes the actual situation because the "true law" is the *combined* law. The reason is that there are many cases in which the intervening factor cannot be brought under a more comprehensive law. Suppose that the motion in the second direction is brought about by a sudden gust of wind. According to Cartwright, there is no law that describes the operation of this kind of intervening factor in the regularity sense but the capacity (of the first factor) still holds.
3. The choice of the lemons model as an example is mine rather than Cartwright's.
4. Principles are few in number indeed: "self-interested actors maximise their utility" being one in microeconomics; "models should be solved using expectations derived from the model itself" being one in macroeconomics. They are meagre, as very little real-world behaviour is constrained by them.
5. It is important to note that the issue is not one of regularities versus causal powers. I do not want to defend a regularity view of law against a capacities view but rather indicate that the causal powers we find in social phenomena seem to be more fragile than the causal powers we find in many physical phenomena. Social causal powers seems to interact more frequently with other powers when they bring about a result.
6. This verdict is not an artefact of the choice of examples from simple mechanics. Even in more complex systems, such as systems described by particle physics, the general method employed by physicists remains the same. Often the synthetic step is more involved than adding forces by means of vector addition.

But still, the laws of the individual parts contribute in a principled way to the solution of the complex.

7. The original German term is "möglich" (possible) rather than "wahrscheinlich" (probable); I will stick with the usual translation, however.
8. Weber in fact thinks that there is an infinite number of such factors.
9. I sidestep issues about describing events *C* and *E* here. This is done in order to focus on the causal relation between *C* and *E* and not because these issues lack importance.
10. My comparison here involves paradigmatic cases on both sides. This is not to say, of course, that a vast number of cases from the less fundamental "natural" sciences (meteorology, geology, engineering, epidemiology . . .) more closely resemble my characterisation of the "social" sciences.
11. This is Ian Hacking's terminology; see for instance his 1999 article. His claim is that entities examined by social sciences tend to be responsive to our conceptions of and theorising about them in a way natural entities are not. Atoms do not care whether we have a good or bad theory about them while Marxism has changed a lot in the world.
12. By "social phenomena" I mean the social equivalent to Duhem's experimental laws or Hacking's or Bogen and Woodward's phenomena: stable features of the world that can be predicted (with some accuracy) and/or manipulated (with some accuracy), and/or explained (with some accuracy), or simply low-level social laws. (See Duhem 1991/1914; Hacking 1983; Bogen & Woodward 1988.)
13. This was later (1952) replaced by the diluted "Theory and Measurement". The reasons for this move will be apparent momentarily.
14. I have defended this interpretation of Jevons (Reiss 2001).
15. I am not saying that one can learn about causal relations from statistics without background knowledge. But that background knowledge can come from a variety of sources, including knowledge about institutions, previous econometric studies, common knowledge, etc. There is no requirement of economic *theory* here.
16. For a detailed discussion, see Reiss (2002b).

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