Why do Temporary Invariances Explain in Biology and the Social Sciences?*

The issue of whether there are laws in biology and the "special science"[1] has been of interest owing to the debate about whether scientific explanation requires laws. A well-warn argument goes thus: no laws in social science, no explanations, or at least no scientific explanations, at most explanation-sketches. The conclusion is not just a matter of labeling. If explanations are not scientific they are not epistemically or practically reliable.

There are at least three well-known diagnoses of where this argument goes wrong. First, the argument that there are no laws in social science adopts an account of laws that is too stringent, one that not even the physical sciences satisfy (Cartwright 1983, Mitchell 2000). On a less stringent definition, there are plenty of laws in social science (and biology). These laws are, ***sensu*** Fodor, "non-strict," as opposed to the "strict laws" (if any—vide Cartwright 1983) of physics. Second, scientific explanation does not require laws, and when laws do explain, they do so because they satisfy some other requirement on scientific explanation, for example unification, or the identification of causal difference-makers (Friedman 1974, Kitcher 1989, Salmon 1984, Strevens 2009).

A third view, increasingly attractive among philosophers of social science and biology is due to James Woodward (2000, 2003). This view, like the second one eschews laws and identifies causes as difference makers. On this view explanations do require regularities, but these regularities need only satisfy a requirement of "invariance" under certain specified circumstances, in order to be explanatory, and

* Thanks to James Woodward for many helpful comments on a previous draft.
[1] Fodor's [1976] name for the social and behavioral sciences seems to have stuck, in spite of its implicit suggestion that they are some how retarded, like the recipients of "special education."

there are many such invariant regularities expressed by generalizations, models, and theories in the social sciences

As an account of what scientists take to be explanatory in their own disciplines, Woodward's account has much to be said for it. It seems to explain the intuitions of philosophers and specialists in the "special sciences" all the way from biological science to mortuary science, and certainly including economic science, about when regularities are explanatory and to what degree.

In this paper I try to show that Woodward's analysis does not escape from the need to appeal to laws, that when it comes to biology, the laws it requires as those of natural selection, and that these very same laws are required to underwrite the explanatory power of invariances in at least some of the special science. I go on to explore the implications of the Darwinian foundations for the fate of invariances in economics and history—two disciplines at the extremes on the spectrum from quantitative to qualitative among the special sciences. If the arguments to follow are cogent, they have implications for the account of regularities in the special sciences that Woodward shares with many of those who deny the need for laws (strict or non-strict) in their explanations.

1. Invariance as evidence or diagnosis: an example from Woodward

Here is an example from "Explanation and invariance in the special sciences" [Woodward 2000] which will enable us quickly both to understand Woodward's approach to explanatory regularities and some of their problems. Consider the "Philips Curve," which names a generalization about the relationship between inflation and unemployment: there is an inverse relationship between the level of money-wages and the level of unemployment. This claim, first made by A.W. Phillips [1958] in the late 1950s provides a basis for Keynesian fiscal and/or monetary policy, in particular that unemployment can be lowered by inflationary government action, for example, ***interventions*** such as increasing the money supply or incurring a budget deficit.

In order for the Philips curve relationship between unemployment and money wages to explain the original data which supported it and subsequent phenomena to which it was applied, the generalization about their relationship must satisfy Woodward's requirements [2000, p. 201]:

M1. The intervention—in this case, for example, increasing the money supply, changes the rate of inflation from what it would otherwise be.
M2. The Phillips Curve generalization asserts that increasing the rate of inflation really does reduce the unemployment rate from what it would otherwise have been.
M3. The increase in the money supply lowers the unemployment rate only through its impact on the inflation rate, and not by any other means—for example, making workers feel rich enough to reduce their labor supply and thus the rate of unemployment.
M4. The increase in money supply is not correlated with other causes of unemployment increases besides inflation, so that we can exclude the possibility that money supply increases and inflation are both the joint products of some other independent change. This condition rules out the possibility that some other variable may work independently of either one to change the rate of unemployment.

After the late 60s monetarist economists, following Milton Friedman [1968], challenged the Phillip's-curve-regularity's invariance. They did so by denying M2 on the basis of another relationship deemed by them to be invariant, the relationship between beliefs about real wages and prices, instead of money wages and prices, and economic choices (holding preferences constant). Once agents recognize that inflation does not change real prices while changing money prices, changes in it will cease to have any impact on supply and demand in any market including the labor market.

Here one regularity's claim to explanatory power is undercut by the argument that it is not invariant under changes in the beliefs of economic agents, employers and workers. And this failure is due to another invariant relationship, one that figures in rational choice theory. since rational employers and workers recognize that changes in money prices may not reflect changes in real prices, such changes by themselves can have no impact on their choices. The only invariant relationships are ones between real prices and economic choices which obtains for rational agents. Since the rate of unemployment is the result of economic choices of employers and workers, changes in money prices will not have an impact on unemployment for rational agents who recognize that such changes do not reflect changes in real prices.

But suppose that the alleged invariance in the relationship between real prices and economic choices of economic agents is itself subject to an objection of the same kind. That is, suppose that experiments in cognitive science for example provide-evidence that the choices of actual economic agents persistently respond to changes in money prices, even when they are acquainted with the relevant economic theory which vindicates the rationality of indifference to changes in money prices that leave real prices unchanged. There is both anecdotal and experimental evidence that suggests such a conclusion. Of course such experimental data, if replicated and otherwise reliable, would lead one to conclude that the regularities of rational choice theory were not invariant in the way that Woodward requires explanatory generalizations to be. That is, the claims of rational choice theory must be denied (at least some) explanatory power, and/or they may not provide a basis to deny the invariance, and thus the explanatory power, of the original Phillips' curve relationship. (I take it that under the supposition you are making about the experimental results, the Phillips relationship would be invariant after all under some interventions on the inflation rate—hence would describe a genuine causal relationship)

At this point in the dialectic, there seem to be one move open to a defender of the Philips curve and a response open to economists who deny its explanatory power:

Defenders could insist that the statistical and anecdotal historical evidence for the Phillips' curve outweighs an argument like Friedman's appeal to rational choice theory. They might even invoke the operation of "animal spirits" to which Keynes himself adverted in one of his digressions into informal micro-theory in ***The General Theory***.[2] This appears to have been among the consideration that gave continued life to the Phillips curve and its explanatory force through the 1960s and early 70s. As Keynesians did not give much credence to rational choice theory nor seek microfoundations for their macroeconomic generalizations, they paid little regard to Friedman's argument.

---

[2] In the General Theory, Keynes wrote only a little about this notion: "a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectations, whether moral or hedonistic or economic. Most, probably, of our decisions to do something positive, the full consequences of which will be drawn out over many days to come, can only be taken as the result of animal spirits - a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities." Keynes, 1937, pp161-162

On the other hand, economists suspicious of Keynesian economics owing to its lack of microfoundations could of course seek another general relationship, besides those expressed by rational choice theory. that is invariant over a broader range of changes than is rational choice theory, and which still undercuts the invariance of the Phillip's curve regularity.

What eventually happened in the debate reflected both of these developments to some extent. First, theorists like Friedman, and Phelps began to exploit a distinction, common to economics, between short-run and long run phenomena, allowing that there is a short run Phillips curve, in which inflation and unemployment are inversely related, and a long run during which the relationship between inflation and unemployment is vertical—that is, inflation has no effect on unemployment. Over the short run economic agents may wrongly infer changes in real prices from changes in money prices, but eventually they catch on.

This notion became increasingly attractive over the 1970s when fiscal policy designed to decrease the unemployment rate by increasing inflation began to fail. This period was characterized by "stagflation" since employment was stagnant while inflation was increasing.

At this point, in part owing to the failure of accelerating inflation to be correlated with reduction in unemployment, Robert Lucas [1976] and other rational expectations theorists began to offer a microeconomic critique of even the short run Phillip's curve, suggesting that over the long run that actual economic agents are rational at least to the extent of learning from experience. Learning about governmental Keynesian fiscal policy over the previous 15 years led them to adjust their choices thereafter to bring them more closely in alignment with Friedman's original observation that rational agents concern themselves only with changes in real prices. Thus, according to Lucas, if there was a short run Phillips's curve, then over time it became steeper and steeper, ultimately arriving at the vertical—i.e. no increase in employment for any change in money prices, no matter how great: no more Philips' curve at all—either short or long run.

The Lucas critique arose after the empirical disconfirmation of the Philips curve by 1970s stagflation. It was then that economists began to seek explanations for a failure of invariance. Woodward says, "The interesting question for economists is not whether the Philips curve is a law of nature, but rather whether it is invariant under certain specific kinds of changes and interventions." (2002, p. 221) If my

history of the Philips curve is right, then the order of events in the process of abandoning the Philips curve was: first, direct disconfirmation, second casting about for an adjustment in economic theory that will explain or explain away the disconfirmation, finding one by reasserting the importance of microfoundations and reasserting the rationality of economic agents—they don't after all fall for mere changes in money prices. Woodward writes, "judgments about the significance or importance of the intervention over which a generalization is invariant…play an important role in the ***construction*** and ***assessment*** of explanatory generalizations."

This does not always seem to be the case. Philips offered his curve after undertaking the study of a correlation, one which Keynesian theory might lead us to expect. But in many of the cases included in Philips' data, governments did not intervene to raise money prices, and the price raises were due to a variety of unintended economic causes. Philips was not identifying interventions when he constructed the curve. As for assessment, the stagflation of the 1970s provided empirical disconfirmation of the Philips' curve, because governments were relying on it to stimulate employment by increasing the rate of increase in money prices. So, in retrospect it was clear that it the failure of policy-interventions which assumed the Philips curve correlations that led to its reassessment. But governmental intervention played little role in the assessment of the Philips curve as an explanatory generalization. The Lucas critique identified quite different interventions as significant in its assessment of the failure of the Philips curve as an explanatory generalization.

It does not appear that considerations of "the significance and importance" of interventions to change money price levels under which the Philips curve might be invariant had a role in its construction. There were no such interventions before 1937 at the earliest. Changes in money prices were retrospectively recognized as the consequences of governmental interventions in some cases, but these interventions were unintended consequences of other policies. It also did not appear that governmental intervention played a role in the assessment of the Philips curve's explanatory power. The explanatory failure of the Philips curve was traced to its failure to be invariant over interventions in rational agents' information were diagnostic considerations to which economists turned once the Philips curves'

explanatory power was called into question by good old statistical evidence of correlations, without treating changes in money prices as "interventions."[3]

Moreover, there was a serious problem with the diagnosis of the Philips curve's failure under interventions in information of economic agents, offered by the rational expectations theorists. It is now well-know from the work of Kahneman and Tversky [1982] and many others, that economic agents are far from rational in many respects, so many that we can hardly have much confidence that the invariance of the components of rational choice theory correctly diagnoses the failures of the Philips curve to be invariant. Indeed, in [2000, p. 232-3], Woodward himself sketches considerations that have led economists and political scientists to doubt the invariance of rational choice theory under relevant changes to its variables. If rational choice theory itself fails to be invariant under changes in agent's expectations and preferences, it is difficult to see how it can reliably explain why the Philips curve fails to be invariant.[4]

The problem this history of the Philips curve raises for understanding how explanations work in economics (and all the special sciences, I hold), is this: The Philips curve actually explained the relationship between changes in money prices and employment over some period up to the 1970s. This period may even have been a long one, going back to the beginning of fiat currency or even earlier if currency debasement and vast new discoveries of gold and silver result in inflation. During the whole time it obtained and really did explain how inflation enhanced economic activity—including employment, the Philips curve could have broken down had people realized that money prices are not real prices. There was no causal barrier to such realization through out the whole time the Philips curve held good. But if the Philips curve regularity could have broken down at any time, was it not merely an accidental regularity lacking explanatory force? "Surely not!", we might want to say.

---

[3] If intervention is synonymous with change, then of course the disconfirmation of the Philips curve was the result of noticing that interventions on (changes in) money price levels ceased to co-occur with changes in levels of employment. Here I assume that in macroeconomics, interventions are policy-directed changes.

[4] Experiments of the sort Kahneman and Tversky and others inspired by them have run do not directly address "money illusion" phenomena, but they do show that there are asymmetries in the dollar valuation of goods depending only on whether the subject has ownership or not. So, there is good reason to think that economic agents persistent violate the strictures of rational choice theory, contrary to the assumptions of the Lucas critique.

But consider, the Philips curve won't support the counterfactual that had people realized that money prices are not real prices, changes in the former would still be followed by changes in employment levels. Once people actually came to the relevant realization, it ceased to have explanatory power. So here is the question: why should the Philips curve ever have had explanatory power, given that it could have been made false at any time prior to the stagflation of the 70's? This is the question of why spatiotemporally restricted regularities are explanatory.

2. Why are spatiotemorally restricted generalizations explanatory?

Woodward writes,

> It is perfectly possible for a generalization to be invariant only for changes and interventions that occur within a limited spatial or temporal internal and to break down outside that interval. Suppose that, contrary to actual fact, the Philips curve turned out to be invariant under government interventions that changed the inflation rate between say, 1870 and 1970 in the United States, although not invariant outside this interval. If this had been the case, then (I would claim) despite the limited spatio-temporal scope of this relationship, one could appeal to it and to the fact that the US government intervened to raise the inflation rate in 1915 to explain why unemployment fell after the intervention. More generally, in contrast to the traditional law based account of explanation, the notion of invariance allows us to talk about explanatory relations that hold only over limited spatio-temporal intervals or which make reference to particular objects, events or processes….Many explanatory generalizations seem to have exactly these features and this is one reason why the notion of invariance is particularly well suited to understanding their character. (2000, 224-5).

As Woodward rightly says, we treat spatiotemporally restricted regularities as invariant and therefore explanatory, even when they could have been made false at any time during which they held. For example, in the case of the Philips curve, "interventions" drawing economic agents attention to the differences between money prices and real ones could have been undertaken at any time before the 1970s with the same effect that the Lucas critique alleged to have destroyed the correlation after 1970.) My question is, given this possibility, why do we treat spatiotemporally restricted regularities about human actions and institutions as explanatory?

We have a fairly good idea of why the regularities of common sense- or every day- or folk-physics are explanatory, in spite of their spatiotemporally restricted invariance. The trouble is that this approach won't work to enable us to see why the spatiotemporally restricted invariant regularities of economics or other social sciences are explanatory.

In the case of a variety of folk-physics regularities about specific objects, or processes such as iron sinks, wood floats, free falling bodies move towards the center of the earth, it is fairly easy to check for invariance by experiment, either casual or systematic. Regularities that pass such tests are accorded explanatory power in ordinary life and they are the starting point for refinements that systematic science seeks. In the social and behavioral sciences, it is the generalizations of folk psychology that are the starting points for explanations (think of Thucydides). But our confidence in them is not similarly the result of informal experiments that identify the range of variations in causal variables over which they are invariant. Rather, the folk-psychological regularities we begin with in social science are vouched safe by some combination of introspection, the meanings of words, and a bit of informal conceptual analysis. It is hard even to know how to set about testing these regularities for the range of values of their causal variables over which they are invariant. Almost all tests of invariance advanced in these cases are either indecisive because their results are unreplicated, subject to alternative interpretations, or else the tests themselves are rejected as indecisive or irrelevant.

Empirically driven social science adds some explanatory regularities to those of folk psychology, and also provides some formalization of the folk psychological regularities it adopts. The evidence for these regularities is often statistical, econometric, or (increasingly) based on data-mining. It is sometimes anecdotal, and rarely based on laboratory-findings, controlled experiments or quasi-experiments, or natural experiments. But none of these regularities comes up even to the level of predictive reliability or explanatory power of the spatiotemporally restricted regularities of folk physics and folk biology.

In the absence of unambiguous experimental or observational results, how can a generalization true only within a certain period really be judged invariant and thus explanatory during that period? Here is one answer that vindicates Woodward's approach. It actually requires some claims that the passage seems to deny. First, the Philips curve probably was invariant for government interventions that increased the

rate of inflation in the period 1870-1970.[5] (Woodward's use of the counterfactual, "If this had been the case" suggests he disbelieves it.) On several occasions during this period government action increased the rate of inflation, for example, during the panic of '93, during First World War, after the mini-depression of 1937, and in each case unemployment decreased. Of course the government intended neither action—increasing the rate of inflation, or decreasing the unemployment rate (except in the 1937 case), but its interventions had the effect the Philips curve describes

If we are to adopt a Friedman- or Lucas-inspired analysis of why the Philips curve held over this time, the invariance was due to facts about economic irrationality, the fact that economic agents responded to money price changes even when real prices were not changing (or changing more slowly). After 1970 economic agents "got wise" to the government's willingness to exploit the Philips curve relationship and agents' mistakes about money prices v real prices. At this point the Philips curve disappears—the short run curve gets more and more vertical until it coincides with the long run vertical curve and we have stagflation, the impotence of deficit spending and expansionist monetary policy. In short, the Philips curve explains changes in employment up to 1970 because it really was invariant with respect to "microfoundations"—individual economic choice, until that date. Thereafter, changes in microfoundations deprive the Philips curve of its invariance. In particular, agents began to take government policy into account, and came to realize that its actions were moving money prices without changing real prices (by the same amounts). The result was the economic agents came to have new and more correct beliefs, and changed their strategies, rendering governmental fiscal and monetary policy ineffective

If this account of the matter is right,[6] the change in the Philips curve from being invariant, and therefore explanatory up until 1970, to no longer obtaining after 1970, is the consequence of what evolutionary biologists and game theorists will recognize as an "arms race": a strategic interaction between two or more players, in which each searches for strategies that will attain conflicting aims, and in which temporary equilibria between strategies in play are broken by the implementation of a

[5] See James 1985, "Shifts in the Nineteenth-Century Phillips Curve Relationship"

[6] Woodward accepts it (personal communication), but it may be challenged by historians of economic theory.

new strategy by one side or the other, in a persistent cycle.[7] In an arms race an equilibrium among strategies will obtain for a period of time, sometimes a very long time, and then will be broken, by one party finding a better response to the strategy of the other.[8] In the US the Philips curve is the product of strategic interactions over a long period, one that began when, for example, fiat money was introduced in the US with the introduction of "green-backs" during the Civil War, in 1862. Apparently it required 70 years or so before governments began to exploit this regularity in its own full employment-stimulation strategies, and another 30 years before economic agents recognized the impact of government monetary strategies on the outcomes of their economic choices, and began to take action to frustrate the government's strategy.

So, now we have an explanation for the explanatory power of a spatiotemorally restricted invariance. If we understand the way generalizations in the social sciences obtain, on an analogy with the way they obtain in biology, we will have an attractive argument for Woodward's conclusion that even when spatio-temporally restricted, some regularities have explanatory power, owing to their temporary invariance.

The trouble with the argument is that it is too strong. For if it is right, it turns out that spatiotemporally restricted regularities are invariant and therefore explanatory because they are underwritten by less spatiotemporally restricted invariances, and these in turn by others, until we reach bed-rock with strict laws of the sort which Woodward certainly does not think we need to back up explanations in social science.[9] To see why this is so, we need to turn to the home base of arms races,

---

[7] I employ the terms strategy, arms race, player here with the meanings common to evolutionary game theory, which does not require that the players be intentional agents knowingly employing strategies they can express. Hawks and doves can play hawk/dove without knowing it, so can governments and citizens.

[8] As argued below, the persistence of arms races will itself require some invariant regularities. Some economists (for example Lucas) and economically influenced social scientists will assume that these regularities will be found in rational choice theory. Others will argue that nothing stronger than regularities about environmental learning will be required. Such learning may result in behavior economists would recognize as approaching their definition of rationality.

[9] That there will be strict laws at some deep level or other underwriting and explaining spatiotemporally restricted regularities is not disputed by Woodward (personal communication). But strict laws of physics, say, on which all other processes, including those treated in social science, supervene, will not do the work which I will argue Woodward's theory requires. No one supposes that physical laws have any significant role in social science's explanations! Woodward's claim is that

biology, and consider how spatiotemporal invariances can be explanatory in that discipline. This is however not so much of a digression as a deepening of our understanding of how economic and other social scientific regularities explain, since they turn out to be biological regularities, about the causes and effects of the behavior of one particular species, ***Homo sapiens***.

3. Invariance and arms races in biology

It is widely accepted among philosophers of biology that there are no completely invariant regularities in biology except for those reported in the Darwinian theory of natural selection. A variety of arguments have been offered for this consensus view, of which perhaps the most influential is John Beatty's "evolutionary contingency thesis": All other regularities in biology—from the most invariant to the least—obtain only as a result of the operation of natural selection on initial conditions that have obtained in the history of the Earth, and are subject to abrogation by the operation of natural selection on later conditions. [10]

The general argument is obvious. Since nature builds adaptations by a process of environmental filtration of random variations. When environments change adaptations can become maladaptations and vice versa; variations neutral in fitness in one environment can become adaptive or maladaptive in another one, and since nothing is forever, even environmental conditions, no regularity thrown up by the process of natural selection is immune to breakdown as a result of environmental change. And once environments come to include creatures and their effects on one another, the life-times of regularities about creatures' adapted traits fall from the scale of billions of years (archebacteria—whose environment has not changed for 3 billion years) to multiple geological epochs (oxygen-respirators) to hundreds of millions of years (vertebrates) to weeks and months in the case of others (the AIDS-virus). These evolutionary interactions between biological lineages are the original "arms races." Owing to the role of environmental change, even the most invariant of regularities in biology is not as invariant as any well established regularity of physical science.

---

spatiotemporally restricted regularities in social science are explanatory even in the absence of any less restricted regularities, still less strict social scientific laws to underwrite them.

[10] Some philosophers of biology even deny that any invariant regularities operate in evolution. Beatty's argument and conclusion can be rephrased to accommodate this denial.

Consider what was until recently thought to be the most invariant of biological regularities: all genes are composed of DNA. For a long time this regularity was subject to no exception. But because it remained invariant over a very long period, its operation provided an environment that would allow for the selection for any new biological system that could take advantage of the fact that all genes are composed of DNA. Such a system eventually came into existence—the RNA viruses, whose genes are made of RNA and which parasitize the machinery of DNA replication (the HIV virus is the most notable example of these viruses).

Thus, the regularity that all genes are made of DNA gives way to the regularity that they are all made of nucleic acids (either RNA or DNA). But we can be sure that the arms race of evolutionary competition will eventually undermine this new invariant regularity, by producing an alternative means of genetic transmission that exploits the regularity (unless it already has done so, by bringing about the prion protein that transmits Mad Cow disease). The same arms race between DNA and RNA and priors also disposes of another invariance of molecular biology, the so-called Central Dogma (in its strong form) that the flow of genetic information is always from DNA to RNA to proteins.

The evolutionary contingency of all biological regularities is relatively easy to establish, once we recognize that the natural kinds related by these regularities are functional, that is, are individuated by their adaptational roles. Biological kinds are always individuated at least in part by their causal roles in the adaptational economy of the systems in which they are embedded. So even when a biological regularity reports the co-incidence of pairs of traits of the same organism that have no adaptational role with respect to one another, or reports the co-incidence of pairs of traits of different organisms, the regularity obtains only in virtue of more fundamental adaptational regularities about the organisms. Correlations between traits of different species are the result of selection for successful mutualism, or parasitism, or predator/prey stability, or other forms of co-evolution. It is well known that mother nature is continually searching through design space to find stratagems which will enable parasite or host, predator or prey, or commensualist species to take advantage

of one another's correlated traits by breaking the correlation. And mother nature always finds what it is looking for, provided it has enough time.[11]

So, in the biological realm there are spatiotemporally restricted invariances. In fact all regularities in biology (except those of the process of natural selection itself) are spatiotemporally restricted invariances, and invariances to greater or lesser degrees. There are no laws in biology. Once we get beyond regularities about the functions of macromolecules that are invariant under almost all values of their environmental parameters, the regularities of biology become strikingly less invariant under changes to the values of their variables. It should be no surprise that all regularities about particular species will be spatio-temporally restricted in their invariance.

Owing to the slowness of changes in the environment, regularities about individual species can obtain over geologically long periods. Changes such as the shift to an oxygen-rich atmosphere, or continental drift or the on-set of ice ages will

---

[11] Natural selection even produces locally invariant regularities between traits that are not adapted at all, but are correlated as the by-products of traits mutually selected for. For example, consider a remarkable discovery of Darwin's: In all mammalian species subject to domestication at least some examples are "piebald"—i.e. have spots, usually white on dark—and this trait is heritable. Darwin's observation has since been widely confirmed, even in "natural experiments," cases in which experiments in domestication of hitherto wild and non-piebald species such as the mink have produced this trait. Presumably, being piebald is not an adaptation, and in general animal breeders do not select for it. The relationship between being domesticated and being piebald is nevertheless invariant, or has been hitherto. However, we pretty much know why. Domestication has always proceeded by allowing the tamest, least aggressive young to reproduce with one another. Tameness is a hereditary trait. At least some of the genes involved in tameness behavior are probably located close together on the same chromosomes as recessive genes that control for variegated coat color. Repeated interbreeding always brings out the recessive trait of piebald coat in at least some descendants. So long as those chromosomes are not broken in meiosis at points between the herd genes and the piebald genes, the regularity that domestic species have some piebald members will be invariant. But of course this generalization is evolutionarily contingent. There are several obvious circumstances—human and natural interventions—that can and some day probably will break it down. Besides a suite of mutations, a founder effect recombination that breaks the chromosomal link, or (equivalently) a persistent program of artificial selection to breed non-peibald domestic animals, there is the possibility of a new move in some arms race we have not noticed breaking the invariance. Being peibald may become an adaptational disadvantage owing to the conspicuousness or other fitness lowering effect of such marks in an evolutionary arms race with predators or parasites.

break up some invariances and create new ones. Other more rapidly occurring species-making processes such as earthquakes, major droughts, will have similar results. But the invariances produced will be hard to break down for the same reason that massive changes that were necessary to produce them.

However, matters begin to change quite radically once any species becomes part of the selective environment of another species. In these circumstances regularities about the latter species become comparatively shorter lived, more temporary and more spatially restricted as well. The reason of course is that species become part of one another's selective environments when they compete, or one predates or parasitizes the other, or both are predated by a third species, or both predate a third species, or both cooperate for that matter. Under all these circumstances, nature begins to search through design space seeking variations in both species that will provide them with a selective advantage over the other. In doing so, selection will break up regularities and establish new ones, some that provide adaptational advantages and correlations among them, it will destroy maladaptional properties and correlations among them, establish and destroy adaptively neutral correlations the adaptations carry along with them. When traits are genetically coded, the arms race process will be relatively slow, though much faster than non-arms race adaptational change. Owing to the rarity of favorable mutations, biological invariances between genetically encoded traits will be often be locked as the result of some relatively long term stable equilibrium in the arms race. Besides interspecific competition, there is also a great deal of intraspecific competition, arms races between lineages within a single species, which also makes and breaks invariances at what is often a faster rate than intraspecific competition does. What is more as the rate of change in the properties of groups or individual organisms begins to change even more rapidly, the average "life-time" of biological invariances among the traits of other species begins to shorten even more, as does their geographic ranges.[12]

---

[12] A nice example is the different fates of Eurasian and American megafauna. As Eurasian megafauna evolved together with the various homonim species, there was strong selection for flight and other traits and strategies that made hunting them more difficult. In the absence of homonins in the western hemisphere, megafauna were not selected for such protective traits. Once humans arrived 30,000 years ago, all the American megafauna were killed off within a few thousand years. The results included among others, the absence of traction animals which had a further impact on what local invariances arose in North and South America, including invariances in human affairs.

The upshot is that all invariances among genetically encoded traits are spatiotemporally restricted. [13] During the periods that they obtain, they are vulnerable to being undermined by random variations that break up adaptational equilibria. The differences between those regularities which are explanatory and those which are not, reflects the dependence of explanatory regularities on adaptational factors identified by the theory of natural selection. As the rate of variation increases, the average life span of an invariant regularity will decrease as will the spatial range over which it obtains.[14]. . In the case of very fast breeding species—say, parasites and their host-targets—for example virus and bacteria on the one hand and humans on the other, regularities may remain invariant only for a few years, months or even weeks. (Consider the life-times of antibiotic effectiveness).

Now consider what happens as a result of arms races between individuals and lineages in a species in which traits are subject to strong adaptational selection, can be transmitted nongenetically, and are open to wide and rapid variation. Under these circumstances, the life times of invariant regularities will be remarkably short and regionally restricted. Of course the individuals and lineages in which traits are transmitted nongenetically will be restricted mainly to primates—including a small number of ape species and ***Homo sapiens*** who have acquired culture. These organism can transmit traits vertically, to off spring, horizontally to one another, and diagonally to the off-spring of other conspecifics. Once language is acquired such transmission becomes vastly more efficient and accurate as well. Variation in these traits also accelerates as intelligence flourishes. As a result the natural selection of traits accelerates and the life-span of invariant regularities drops more and more.

But why suppose, as evidently I have, that all the regularities about humans are the result of the operation of natural selection—genetic or cultural—in which arms races persistently break down invariances between traits and behaviors? The answer is simple: all of the traits of humans that are of interest in the human sciences are adaptations or the result of adaptations. Accordingly, the only explanation of their

---

[13] Interestingly, even the genetic code's universality has been violated on one or two recently discovered cases of bacteria changing the coding of one or two amino acids to gain an advantage in an arms race with parasitic virus and phage.

[14] Sometimes a regularity may carry on obtaining in a spatiotemporal region even after it has ceased to be explanatory owing to the emergence of traits whose manifestation ***would*** falsify it, but which have not yet been exercised. In these cases the regularity loses its explanatory power because it will no longer support counterfactuals, even as its positive instances continue to occur

emergence and persistence available is Darwinian. In brief, excluding divine design, immanent teleology, future causation, or intentional human design, there really is no alternative to a Darwinian process of blind variation and environmental filtration to explain the emergence and persistence of all the adaptations (and their by-products) that the social science concern themselves with. Even those adaptations that are the result of intentional human design will have ultimately to be explained by Darwinian processes operating in the head. After all, like every other science psychology must exclude explanations of cognition that appeal to God, immanent teleology, future causation, and intentional design (homuncularism).[15].

There really is no other alternative to the Darwinian mechanism to explain human behavioral adaptations. The only real alternative is to give up the description and conceptualization of human life in terms of functions, goal directed behavior, actions, norms, and institutions. No social scientist is going to do this.

The role of the environment and of the onset of arms races in making and breaking biological invariances explains clearly the differences in the successes and limits of biological models. Among the most widely used models in biology is the Hardy-Weinberg model which is employed to describe gene and genotypic frequencies of sexual species under a set of well known assumptions, and can reliably be employed to identify sources of evolutionary change. There are only a small number of values of its variables for which the Hardy Weinberg model fails to be invariant. This is largely because intergenomic arms race conflicts are few and far between, most having devolved into very stable equilibria long ago on evolutionary time scales. A somewhat less invariant relation is described Fischer's model of why sex ratios remain equal in sexually reproducing species. There are well understood environments in which the optimal sex ratios are far from equal and the model fails to apply—for example where the environment prevents female off-spring from dispersing. Relationships among predators and prey of the sort modeled by Lotka Volterra simultaneous differential equations are likely to be more invariant than those

[15] The absence of "memes" or other particulate carriers of information is sometimes raised as an objection against this Darwinian account of social behavior and cultural change. Suffice it to say that the absence of anything like memes in human affairs does not undermine a Darwinian account of culture. For there is no alternative to it. Rather, the absence of memes simply makes more daunting the problem of understanding the details of the Darwinian processes that must be at work in human affairs.

between bacterial parasites and their hosts described in Nicholson-Bailey models of a mathematically similar type. For the much more shorter intergenerational time and more rapid rate of mutation in the latter case will result in one or the other of the parasite or host much more quickly breaking out of the relationship between them altogether than will predator/prey relations among much more slowly varying metazoans break down.

The next section explores the impact of accelerating arms races on the modeling of invariant relations in the social sciences.

4. Implications for Theories, Models and Case Studies in Social Science

It's obvious that people, and groups of people are parts of the selective environment that filters both for the traits and for the behaviors of groups and individuals that the traits dispose us and to. That's because the traits and the behaviors are adaptations or the immediate consequences of adaptations. Since the components of the environment under human control change quickly, and since human nongenetic traits and behaviors are subject to equally rapid and very wide variation, the process of evolutionary change in the traits and behaviors of individuals and groups will be much faster among humans than any other species. These facts will have significant consequences for the life span and geographic range of regularities about human behavior, social institution, and their interactions. Let's consider just a few of them.

The first and most obvious consequence of the environmental contingency and arms race character of human affairs is that regularities will have very short lifetimes and very restricted ranges of application. So, we cannot expect general theories about human affairs anything like those we find in physical science. The most general theory in principle possible for human affairs will be no more general than, indeed will be identical to the most general theory operative in the biological sciences, viz. Darwin's theory in its latest biological variants.

Of course, theorists in most social sciences don't search for general theories any more in the way that Durkheim or Talcott Parsons might have done. Instead their interest is modeling. But here too the consequences of the spatiotemporal limits on regularities imposed by natural selection, are evident. Consider what was for a long time the most influential model in economics, and perhaps even all of social science, the LM/IS graphs and equations of Keynesian macroeconomics.

This set of graphs and equivalent equations outlined a set of curves that enabled economists of the third quarter of the 20th century to provide a model of the relationship between sets of macroeconomic variables, including investment and savings, consumption and gross national income, the interest rate and the money supply, and with one another. There was considerable debate as to whether the IS/LM model actually conveyed the conception of Keynes' ***General Theory of Employment, Interest and Money*** (1936), but this was a matter for historical scholarship during a period when the model's equations provided a set of tools for macroeconomic intervention. The Philips' curve discussed above, having been grounded in macroeconomic data, was often explained by appeal to the Keynesian IS/LM model.

The stagflation of the 70's put an end to the model's general acceptance, and resulted in its replacement by newer ones, including the rational expectations model. This model, though not as neat and simple as the IS/LM model, explained why the superceded model was no longer a basis for effective intervention. The reason of course was that its widespread dissemination, or at least the fact that economic agents had become acquainted with the governmental interventions it guided, resulted in a change in their choices, one which rendered the Philips' curve inoperative and fiscal policy ineffective. The subsequent monetary theory models of the economy more explicitly included microeconomic foundations which the LS/IM model ignored. That is, it implicitly incorporated alleged invariants from rational choice theory—agents' expectations and preferences--and added these choices to the model to show why they made fiscal policy ineffective, if it ever had been in the first place.

Monetarist models too proved to have restricted lifetimes, for much the same reason that the Keynesian model did. Economic agents, especially those in a position to arbitrage between currencies came increasingly to exploit the opportunities provided them by a predictable governmental policy of money supply. as the environments in which the agents they modeled found themselves changed.

Other models in social science will certainly share the fates of the IS/LM models, its successors (the AD/AS and AD/IA models) and rivals. The moral to draw from these fates is not that such models should not be sought or employed. But that owing to their limits, social scientists framing them ought to recognize that they are not searching for invariable laws of human affairs, or even first approximations to them. If, as Woodward suggests, social scientists seek invariant regularities, the ones they find will always be spatiotemporally restricted invariances. Therefore they need

to work quickly enough in framing them so that their use will not be overtaken by events. If the length of time needed to find a model extends very long we can be confident that by the time it is formulated, it will be too late to exploit the temporary invariance it relied on.

In effect, I am arguing that there is in social sciences a continuum from models of the generality of Keynesian macroeconomics all the way to the case studies of historical sociology. Historical case studies are self-consciously more modest in their claims of generality than are those associated with the sort of mathematical models familiar in Keynesian economics. But their claims are particularly well suited expression in terms of invariances of Woodward's sort. Consider a contemporary and widely influential instance of historical case studies in sociology: the work of Theda Skocpol. Like her little remembered predecessor in the field, Crane Brinton (author of ***The Anatomy of Revolution,*** Skocpol sought to identify the causes of revolutions in a comparative study of important historical cases. Brinton identified a cyclical process of historical change, of which revolutions were a stage. By contrast Skocpol's ***States and Social Revolutions: A Comparative Analysis of France, Russia and China*** seeks to show that revolutions are the consequence of the "structural" features of states and their relations—in particular their susceptibility to military collapse when subjected to pressure from more developed states, and the sociopolitical structure of their agrarian economies. These two macro-social structural features are the important constants in the three cases of Skocpol's title. Her comparative study led to the claim that in these three revolutions the relation between these structural factors and violent political overthrow was the invariance that explained their occurrence and their nature as revolutions.

There is in such historical case studies no pretence of extracting mathematically expressible functional relations that could be formulated as a model. And presumably a historical sociologist such as Skocpol would reject the notion that her causal analysis uncovers an invariance that is not spatiotemporally limited. What is clear however is that insofar as Skocpol's work reports invariances that hold only within a historical period, we now have at least one clear understand of the nature of their historical limits. And we see that their prospects of their explaining other revolutions or similar patterns of institutional change that occur distant in time and space—either much earlier or much later—is very probably going to be overtaken by events. There are bound to be changes the human or natural environment to make the

causes Skocpol sites inoperative or ineffectual in producing the sort of changes she credits them with in the French Russian and Chinese revolutions. In fact, the probability of such changes is increased by the very dissemination of her theory, as I shall argue below.

Theorists like Skocpol and indeed many others who reject the economist's mantra of "methodological individualism", will argue from case-studies like hers that social structure at least sometimes trumps individual choice in shaping the course of history, and some structures always win against individuals. But besides assuring us that a model like Skocpol's will have only limited invariance, the theory of natural selection can also be harnessed to show that its claims about the causal independence of structure in human affairs need seriously to be tempered. And this skepticism about social structural causation will hold for all of the models in the long history of holistic social science that stretches from Hegel and Marx through Durkheim and Levi-Strauss to latter day structuralists like Skocpol just as it does for macroeconomic models that lack micofoundations.

Darwinism assures us that systems and structures will always succumb to the actions of individuals setting out to exploit or even undermine them. Just as the process of natural selection makes all biological and social invariances inevitably local and temporary, so to does it assure us that groups, social structures and their institutions cannot permanently resist the inroads of fitness-maximizing or preference maximizing individuals. Modern day group selection theory has come increasingly to be accepted by evolutionary biology, but only on the understanding that between-group selection trumps within group individual selection only temporarily and under conditions too stringent to be realized most of the time. To this extent biology again assures the social scientist that macro-models will always have an only temporary shelf life.

Does this mean that rational choice microfoundations are the fundamental regularities in the social sciences that explain the spatiotemorally restricted invariances of the rest of social science, in the way that natural selection explains the temporary write of regularities in biology? If it were so, this would still be a troubling conclusion for a view like Woodward's if his view really eschews a role for strict regularities in social science. But for reasons we have already seen rational choice regularities are not going to do for the equilibria and the arms races of social science what natural selection regularities do for the equilibria and arms races

of biology. And it is these very regularities of natural selection operating in human cognition and behavior that insure this difference.

Thus, it would be a mistake, characteristic of an economic research program such as Gary Becker's, to suppose that at the foundation of human affairs lies a set of really invariant regularities about individual rational choice, and that these invariances can be used to show both why macro-models work over limited periods and why they always eventually unravel. As noted above, work in cognitive psychology has shown that so far from invariant, the regularities of rational choice theory have only a limited role in actual human choice. Instead, it appears that the invariants of human choice are a variety of heuristics that have been selected for owning to their ability to provide fast and frugal strategies for creatures with limited calculating ability, strong risk aversion, and a large number of environmental threats to cope with.

Still there is after all something to the imperative of rational choice theory. The heuristics are themselves invariant over spatiotemporally limited ranges! The heuristics that people employ instead of rational choice theory in making choices are themselves vulnerable to arms race exploitation. As recent evidence in the financial markets and elsewhere shows, there has been an arms race between consumers and suppliers of financial products of all sorts in which consumers have been exploited by strategies that honor rational choice theory. This selective environment will move some populations' behavior closer to the rational choice model at least for a period. But, unless the theory rational choice is treated as a prescription or a set of implicit definitions, it must also and repeatedly be overtaken in its descriptions of behavior by arms races of various times.[16] I will return to this point below.

Though we have not noticed it, our confidence that there are no really spatiotemporally unrestricted regularities in the human sciences turns on the principles of the theory of natural selection. If, however, they are themselves merely spatiotemporally restricted invariants with particularly extensive life-spans and universal terrestrial ranges of application, the conclusion that there are no spatiotemporally unrestricted invariances in social science will be undermined. For my argument to work, the principles of Darwinian evolution will have to turn out to

[16] The semantic account of theories, according to which they are sets of models that are implicit definitions together with empirical domains , makes for a complication in this claim which I shall not discuss here.

be nomological generalizations of the sort familiar from physics, regularities that are invariant under all changes in the values of their variables and parameters. If it turns out that natural selection is a process that is itself only locally invariant, then there will have to be more fundamental invariances, presumably uncovered in physical science that together with local conditions establish the range of natural selection's invariance. And for all we know, its at least possible that these more fundamental invariances may underwrite invariances in human affairs.

5. Conclusion

Winston Churchill once wrote, "The farther backward you can look, the farther forward you can see." This watchword, like so many others about the lessons of history turns out to be quite false, and false for the rest of social science as well. Or at least it must be false if we take the arms race character of adaptational change seriously in human affairs. The further back in history one goes, the slower the rate of arms race change. A million years ago, at the point where hominims emerge, the pace of arms race change is glacial, and almost entirely genetic. As homonims population density increases, interactions with other megafauna accelerate arms races between them and homonims, and eventually between individuals and groups of homonims and one another. Once proto-linguistic interaction emerges, say 250,000 years ago, the rate further increases. By the time you get to recorded history, changes have become so fast that there is almost nothing genetically transmitted behavior has to teach us about human affairs. And of course the arms races of the hunter-gather period, between tribes, within tribes, between genders, generations, and individuals within all these classes, have little to tell us about human affairs after the onset of agriculture.

Wind the tape forward faster through hydraulic empires, feudalism, renaissance, the industrial revolution, the informational revolution, and the role of the past, even the very recent past, as a basis for understanding the present and the future becomes smaller and smaller. The reason is the ever accelerating rate of arms race interactions, and increasingly the role of science and technology in changing the size and the dimensions of the space in which mother nature selects for alternative strategies.

In one respect the arms race character of human interactions, together with the decreasing cost of acquiring, and employing information to make instrumentally

rational decisions, may have a reflexive impact on social science, mathematical modeling and rational choice theory. As we now know from the work of cognitive psychologists, evolutionary game theories and others, human behavior has more frequently been the result of heuristics, fast and frugal inferences, preferences that do not honor von Neumann Morgenstern axioms of expected utility, and norms that emerged as a result of natural selection in pre-modern settings. The explanations for these systematic and nonsystematic departures from what rational choice theory would predict about the behavior have been various. And in the last half of the 20th century some people ("financial advisors") have been able to employ rational choice theory and the reduced costs of information, in order to take advantage of the behavior of others ("financial planning" consumers) that fails to satisfy the strictures of rational choice theory. The activity of these "behavioral arbitrageurs" will in the medium run have the same effect on all the rest of human behavior that they have had in the financial markets. Arbitrage imposes the law of one price, that is, it forces every one to behave more rationally in the rational choice theory's sense, or become extinct. As the costs of employing quick and dirty heuristics in decision making rise, and the trade off between cost and speed in algorithmic information processing declines, the incentives to engage in classically rational behavior increase. When the incentives become high enough and the costs become low enough, almost all behavior of interest in the social sciences—political behavior, business strategy, social cooperation, will begin to satisfy the models of rational choice theory, at least for a while. Coalitions will form and create institutions that will provide their members and their non-members incentives to act in accord with the dictates of rational choice theory.

It's reasonable to predict that at least for a time the arms race character of a great deal of human affairs will settle down into a series of equilibria, at the level of international and intranational relations, and in smaller scale human communities. Some of these equilibria may be quite novel. Certainly, cooperative institutions that emerged in past environments as evolutionarily stable Nash equilibria among strategies for individuals engaged in fairness-norm-governed choice will have to undergo substantial redesign. Otherwise they will fall prey to arms race subversion, and fail to protect participants from perfectly rational free riders operating in the new environment created by the changed costs and incentives. Institutions that don't change to protect their participants will be undermined and will become extinct, along with the regularities in behavior that they support. Institutions that do change, and

new institutions, embodying new norms may persist in equilibria for varying lengths of time. But eventually they will simply set new design problems for participants and others with incentives to exploit them, and thereby set off a new sequence of arms races.

If the costs of coming up with good new ideas about how to beat the system increases or the benefits to doing so decrease enough then the inevitable arms race that nature imposes on all its creatures will come to an end and models in social and behavioral science will start to work for longer periods of time. But don't count on its happening any time soon.

Alex Rosenberg
Duke University

References

Becker, G., 1976, ***The Economic approach to Human Behavior***, University of Chicago Press
Cartwright, N., 1983, ***How the Laws of Physics Lie***, Oxford University Press (August 1983
Fodor, J. "Special Sciences (or the disunity of sciences as a working hypothesis), ***Synthese***, 28: 97-115
Friedman, Milton, 1968 "The Role of Monetary Policy." American Economic Review 58: 1–17.
Friedman, Michael, 1974, "Explanation and Scientific Understanding", ***The Journal of Philosophy,*** 71: 5-19
James, J., 1985, "Shifts in the Nineteenth-Century Phillips Curve Relationship" ***NBER Working Paper No. 1587***
Kahneman, D., Slovic, P., & Tversky, A. ,1982, ***Judgment under uncertainty: Heuristics and biases***, New York: Cambridge University Press.
Keynes, J. M. , 1936, ***The General Theory of Employment, Interest and Money***, London: Macmillan
Kitcher, P., 1989, 'Explanatory Unification and the Causal Structure of the World', in ***Scientific Explanation***, ed.,P. Kitcher and W. Salmon, 410-505. Minneapolis: University of Minnesota Press
Lucas, R., 1976. "Econometric Policy Evaluation: A Critique," ***Carnegie-Rochester Conference Series on Public Policy***, 1: 19–4
Mitchell, S., 2000, "Dimensions of Scientific Law," ***Philosophy of Science***, 80: 242-265.
Phillips, A., 1958, "The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957." ***Economica***, n.s., 25: 283–299.
Salmon, W., 1984, ***Scientific Explanation and the Causal Structure of the World***, Princeton: Princeton University Press
Samuelson, P., and Solow, R.M., 1960, "Analytical Aspects of Anti-inflation Policy." ***American Economic Review*** 50: 177–194.

Strevens, M., 2009, ***Depth: An Account of Scientific Explanation***, Harvard university Press
Woodward, J., 2000, "Explanation and Invariance in the Special Sciences," ***British Journal for the Philosophy of Science***, 51: 197-254.
Woodward, J., 2003, ***Making Things Happen: A Theory of Causal Explanation***, Oxford: Oxford University Press