Novel science; or, How contemporary social science is not well and why literature and semiotic provide a cure*

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Part One

The social and human sciences are in a sorry state today. You don’t have to take my word for it. Consider some others who have reached the same conclusion.

Percy’s Antinomy

For instance, consider Walker Percy’s Antinomy, stated in its most general form (Percy 1958: 240).

The functional method of the sciences is a nonradical method of knowing because, while it recognizes only functional linkages, it presupposes other kinds of reality, the intersubjectivity of scientists and their assertions, neither of which are space-time linkages and neither of which can be grasped by the functional method. Therefore, when the functional method is elevated to a total organon of reality and other cognitive claims denied, the consequence must be an antinomy, for a nonradical instrument is being required to construe the more radical reality which it presupposes but does not understand.

I’m fallibly certain that I understand what Dr. Percy (the first hero of this essay) meant by the distinction between radical and nonradical methods of knowing; it goes something like this. A radical science is one in which no limits are placed upon the kinds of knowledge sought by scientific intelligences. Thus, chemistry would be a nonradical science; for chemists seek only chemical knowledge and, as professional chemists, have no interest in knowledge about art, religion, psychiatry, or literature. Thus, in a nonradical science such as chemistry, when a question about human intersubjectivity arises, chemists using the functional method can only say, ‘We don’t study that’. But human and social sciences would be radical, at least in regard to the inclusion of human intersubjectivity or

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other issues involving human nature, for that is precisely something they must study.

The phrase 'functional method' here means the technique of conceiving 'dependent variables' E as a function f of 'independent variables' C. We could express that as:

\[ E = f(C), \]

or in plain American,

C causes E to be the way it is.

It should be clear that the functional method presupposes that all explanations of phenomena being considered will be given exclusively in terms of functional or causal statements or facts. Moreover, a practitioner of such a method does not ask whether there are other kinds of facts or data in the phenomena to be studied.

Culture, Percy argued, is largely a tissue of assertions. That appears to be obvious if one conceives an assertion as a kind of communication event, and even more evident if one is considering scientific culture, the community of persons capable of learning from experience. This brings us to an interesting and shocking fact. *Assertions and other intersubjective communication events, including those among scientists, are inexplicable if one's explanatory resources are self-limited solely to information about causal or functional linkages.* Naturally, you want the evidence for this claim. I promise you will have it, but for the moment, let us be clear about the antimony.

It occurs when in science a strictly causal method is focused upon cultural realities such as assertions or communication events. We should carefully note that it is not a difficulty of science as such, but of science conducted exclusively according to a particular methodological conception; for reasons which will become clear, let us refer to a science, *limited by its practitioners* to strictly functional or causal resources, under the name of Dyadic Science. Such a science would only find functional data, for it would have *limited itself* to seeking only such phenomena. Percy's antimony, a fundamental but heretofore hidden internal contradiction, becomes apparent as the habit (widespread in the physical sciences) of limiting one's explanatory resources to those of Dyadic Science is carried into the social or human sciences, where it becomes self-defeating. For cultural realities are inexplicable solely in terms of functional or causal information. The antimony arises equally whether the social realities studied are those of scientific culture itself or of some other culture conceived as alien.
Here is another way to grasp the point. Imagine there is a particular society of scientists who have a formal initiation requirement for every scientific intelligence wanting to join their community. Suppose you have just been initiated and, like all beginners, you have signed an oath that you will always use only the causal, functional methodological conceptions of Dyadic Science in studying any real phenomenon. Soon after taking the oath, at a beer-and-pizza reception in your honor, a senior member whose hobby is repairing antique radios remarks, ‘Some of those old dual-function AC/battery sets from the 1940s were amazing; when run from AC, a 35Z5 was on line in series with a large dropping resistor, and the I-square R through it was unbelievable’. That comment, we must carefully note, is a real phenomenon, and for certain kinds of activities it would be important to have a full understanding of it. But as a beginner, you don’t understand it. What is a 35Z5? What is I-square R, and why does it seem to be so important? However, if intersubjective communication were a strictly causal matter, you should now understand the remark upon being exposed to it as a causal factor. You don’t understand it. Therefore, intersubjective communication is not a strictly causal matter. Topics (such as the understanding of a fellow scientist’s remark) that involve more than causal considerations cannot be fully studied by a method that is self-limited to causal processes.

That is, one might say that the life of scientists working together is a cultural or community life, which has a reality in addition to that of the phenomena these scientists are studying. So, if in our method of studying culture we limit our explanatory resources strictly to causal relations, then the culture which is essential for that very scientific community, and the general nature of science itself, would always remain a mystery. This is the unhappy result because the culture of science, its assertions and communication events, involve social phenomena other than just causal linkages. While the focus of science was nonradical, was upon nonsocial realities such as chemistry or physics or astronomy, the antinomy would probably remain unnoticed. And in such situations, it is easy for one tempted by the false doctrines of Dyadic Science to follow its principles. But the moment one turns the gaze of science upon human and social realities, either within science itself or within subject matters such as psychology or anthropology in which intersubjectivity and community life are obviously fundamental, Percy’s antinomy must eventually work its way to the surface, like a putrid ancient winter bubble of swamp gas rising in a hot April thaw.
Turing’s machine constitutes another testimonial about the sorry state of the social and human sciences.

At the turn of the century, David Hilbert, the eminent Göttingen mathematician, expressed (1900) his faith that it was but a matter of time before procedures would be found in mathematics that would enable most if not all the outstanding problems of that field to be solved by means of algorithms. An algorithm is a definite procedure such that from explicitly defined starting conditions, a clearly stated result may be obtained using a known method requiring a finite number of steps. For instance, an algorithm for obtaining ten percent of any positive number is: ‘Write down the number including its decimal point, then move the decimal point one place to the left; the resulting second number will be ten percent of the original number’. Of course, Hilbert’s dream of a general algorithmic method is precisely an example of the mechanistic functional method of Dyadic Science applied to mathematics.

Turing made a number of clear definitions for such algorithmic or machine methods and, through a series of insightful observations of the consequences of his definitions, showed that there are some mathematical results that cannot be obtained algorithmically (that is, by means of a deterministic machine). One might summarize it this way. Finding algorithms, or finding new computer programs, is an important task in mathematics. But Turing’s results suggest that the process of finding new algorithms or of developing new theorems cannot itself in all cases be an algorithmic or automatic technique. On the contrary, it seems likely to be a procedure that requires human creativity, serendipity, experimentation, and inventiveness. In other words, mathematics is not a Dyadic Science (compare Ketner 1988).

If that is the case in mathematics, the clearest and simplest of sciences, it is likely to be the case in any science, including the social and human sciences. That is, it seems likely that there is no set of algorithms for doing all that is to be done in the social and human sciences. But how many social scientists automatically start their work by assuming a kind of algorithm or noncreative recipe, often one that involves statistical research design procedures? If social science is based upon the notion (as it is frequently assumed to be) that you gotta have an algorithm to do science, social science will shoot itself in the foot, because it will begin its efforts by throwing away human creativity, experimentation, and inventiveness, both as resources used by social scientists and as factors inherent in the realities such sciences propose to study.

Dean Swift was a great wit whose humorous sentences often penetrated
to the heart of an issue better than a long theoretical essay. He described a crank-powered thinking machine he represented a Laputian Professor as trying to make, then remarked (1977: 179):

Every one knew how laborious the usual Method is of attaining to Arts and Sciences; whereas by his Contrivance, the most ignorant Person at a reasonable Charge, and with a little bodily Labour, may write Books in Philosophy, Poetry, Politicks, Law, Mathematicks, and Theology without the least Assistance from Genius or Study.

The science of Artificial Intelligence has been occupied by partisans of Dyadic Science. However, it now shows signs of realizing that it cannot accomplish its goals with such a self-imposed limitation which omits important non-dyadic phenomena from study. This is the principal upshot of Turing's results.

It seems to me that Alan Turing and Gulliver's father came face to face with one and the same reality.

Some problems with statistical social science

Social scientists often use statistics, so if there are problems in the way such operations are carried out, that might be another reason for the sad state of affairs in social and human science. I can detect a few such general problems. Perhaps you can think of others.

First, ask yourself this: What do weather forecasters mean when they say there is an 80 percent chance of rain tomorrow? Do they mean drops 4/5 the usual size, or that we ought to carry 4/5 of an umbrella? One meteorologist commented that it meant that there were five forecasters working at the local National Weather Service office.

Here is what I think it means. From the billions of years of past history of weather events at some particular location, the weather folks have a microcosmically small sample running from, in the United States, certainly no earlier than 1492. Of all the days in the sample in which weather conditions were like they are now, in 80 percent of them it rained the next day. Notice an important thing about that sentence: the phrase 'in 80 percent of them it rained the next day' describes a property of a sample from the standpoint of its collective properties; it is not a property of any one single day from the days sampled. First one forms the sample, then one notices that the sample as a collective has certain properties. That is, fleas are small, but if I had a metric ton of live fleas in my back yard, you would say that Ketner's collection of fleas is large. Suppose someone argued that Ketner's fleas weigh a ton, and this is one of them, therefore
this one weighs a ton. Someone uttering such an argument pattern perpetrates an equivocation known among logicians as the Fallacy of Division. This involves an argument process that moves from premisses that mention a property possessed by a collective as such to a conclusion that mentions a similar-sounding property possessed by an individual. This is a fallacy, for although the phrases presenting a collective property and an individual property often use the same words, they are very different in meaning. For example, a critic might assert that a play is good, and conclude that a particular actor in the play was therefore good. But we all know that a play as a collective thing can be good even though some individual actors in it are bad. I remember seeing a fine production of *Hamlet* that included one actor who had not overcome his native southern Oklahoma dialect (a noble language, to be sure, full of fine Choctaw and Chickasaw and Elizabethan resonances), so he uttered lines like ‘*Git* Thee tew aiy *Nun*-ner-reel!’ That of course is music for an Okie like me, but it tended to disrupt the performance for some folks in the audience.

Sometimes when a statistical social scientist makes a prediction, something like the weatherman’s technique is used. And if it is, a division fallacy occurs. Thus we have the result that the collective statistical features of a sample of some social phenomenon logically cannot be used to guide a concrete individual issue, such as some person’s decision. A political scientist’s prediction that 53 percent of the voters sampled would vote for a third term for Ronald Reagan is of no use to any one of us in making a personal decision about how to vote on such a matter.

An obliging objector (or an objecting obliger) might say, ‘But such statistical information about collective properties of samples could be useful for someone attempting to manage a large social organization which is itself a collective’. Such a line of thinking might be represented by an imaginary manager of an imaginary corporation who has been ordered to lay off 40 percent of the workforce of a particular division. He has read of a recent statistical study that says that only 3.5 percent of workers sampled who were laid off by a female executive become verbally violent, whereas if the executive bearing the bad news were male, 68 percent of the affected workers become violently rude during the exit interview. This manager probably is thinking that here is some good and useful scientific information. I ask, ‘Useful to what end?’ Surely the answer is: ‘To control the situation according to the Big Picture that a manager has in mind’. And when this purpose is made explicit, do we not see clearly what image of individual humankind it presupposes?

Here is a second kind of difficulty that can happen when one uses statistics. Imagine that I want to conduct something like an opinion poll
about some topic I am researching, so I prepare a questionnaire according to standard and accepted techniques. Next I select a random sample from the target population. Let's not quibble about whether the original sample is sufficiently large or sufficiently random. Let's agree that it is large and as perfectly random as you wish, through any technique you wish. So I now mail the questionnaire to persons in the selected sample. I'm told by those who do this sort of thing that something like a 30 percent return rate is typical and quite acceptable. At this point should I go on to assume that my sample, 70 percent smaller than before, is still random? Many researchers appear to assume that. But how can the remainder still be considered random? Surely it has been de-randomized by any number of relevant factors, an obvious one being that many of those nonreturners don't care for questionnaires. The reason samples are made random in the first place is to avoid bias; but now our reduced sample is exactly biased, in that it has an insufficient representation of questionnaire-haters. And if we note that persons who dislike questionnaires might plausibly be busy and intelligent persons, that could be a significant bias.

Here is a third difficulty, again with questionnaires. Often these documents ask us what we think about some issue, or ask us to describe our feelings. If you are a researcher and you ask me what I think about something, you might be assuming the doctrine of privileged access, namely that whatever description I give about my thoughts is automatically correct. After all, you might say, they are HIS thoughts. The difficulty with that kind of procedure is a simple one: I might be suffering from the peculiarly human and non-mechanistic malady of self-deception, which is to say that I might be systematically misdescribing my own thoughts. I once had a friend who described himself as a neat person, but every time I visited his apartment it was a mess. It always looked like Sam Spade's apartment after the crooks had searched it for the missing Maltese falcon. On every visit he had some excuse why it was not clean, and I am sure he would have answered a questionnaire about neatness to the effect that he believed in it and practiced it. We might put it this way: if it is so damned easy to describe one's thoughts correctly, why have great persons in every age, from Pythagoras to Percy, repeatedly urged, in every imaginable way, that in spite of the difficulties, we should struggle to know ourselves?

But these considerations do not get at the principal problem of statistical social science; that is, they illustrate the problem, which is more general than any one of the difficulties mentioned. Each of these matters is either a retreat from achieving an understanding of persons, or assumes erroneously that persons are only complex machines, discountable as individuals and collectively quite suitable to be controlled or manipulated
by a statistical social scientist who has, or who has been hired to have, the Big Picture in mind. This last option, of course, returns us to the unyielding grip of Percy’s antinomy.

We could summarize this by saying that many of the practitioners of contemporary statistical social or human sciences (as well as other persons attracted by the false lure of Dyadic Science) appear to take for granted an erroneous philosophical anthropology, the theory of what it is to be a human being.

The history of Manicas

We can get another extended testimonial from Manicas’s recent book, A History and Philosophy of the Social Sciences (1987). I regard this work as extremely important. I believe Manicas has convincingly shown that the methods and assumptions of typical contemporary social science, what he calls ‘mainstream social science’, are based upon an uncritical, almost historically accidental acceptance of some rather questionable assumptions of technology and twentieth-century logical empiricism. In other words, I think he has shown that contemporary mainstream social science is a methodological dead end. This is ironic, for positivism as a philosophical approach is one of the most reductive in history, one with a philosophical anthropology than which there is no weaker. And its assumptions about the nature of science itself are a model for constructing a Dyadic Science. The historical component of the Manicas thesis is extremely important, but I have space to put forward only a few aspects of the philosophical part of his fine essay.

A central feature of his book is the claim — a correct one, I think — that the logical empiricist account of the logic of explanation, the so-called covering law thesis (the ‘standard view’ of explanation), remains culturally engrained in social science at a time when all the central points of that thesis have been disconfirmed and rejected by philosophers of science. Or, in words well chosen by Manicas (1987: 242):

Einstein, often ahead of most people, began his 1933 Herbert Spencer Lecture at Oxford with a significant prescription: ‘If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: Don’t listen to their words, fix your attention on their deeds’. But if the practices of physical scientists bear little resemblance to the dominant philosophy of science, it is no exaggeration to say that, in consequence of their relatively late beginnings as ‘sciences’, the practices of mainstream social science have long since been constituted by it.

Yet in the last several decades, every key tenet in this ‘standard view’ [the
covering law thesis] has either been abandoned, liberalized to the point of triviality, or thoroughly undermined.

One of the features of the covering law account of science is that explanation (the process of achieving understanding) and prediction are logically parallel. Accordingly, an explanation (an occasion of having achieved understanding) occurs when from a law plus some instantiation conditions we deduce the event to be explained. Moreover, once we know the law, and can bring the instantiate conditions to pass, we can predict that a given result will be present. This would mean that if one could explain, one could predict, and vice versa. But as Manicas indicates (1987: 289), we often predict lacking an explanation, or explain without being able to predict. Humans could predict rain in the middle ages by viewing certain kinds of ominous clouds nearby without having any correct understanding or explanation of the truth of how rain occurs (condensation, or dew point, or the like). Or we can understand or explain very well the process of combustion, yet fail to predict a terrible hotel fire. In other words, prediction and explanation on the covering law thesis presupposes a highly controlled, well-isolated environment.

Manicas argues (1987: 289) that devotion to this standard view of science by mainstream social scientists (who have been socialized in it, and who continue to socialize their students in it) is intimately connected with the historical accident that saw social science in its formative years aping technology. For on the covering law approach, understanding and explanation collapse into control. It is true that control is the purpose of technology. Thus the technological Dyadic Social Sciences of today have as their ultimate purpose the control of something or someone — or, perhaps better expressed, the control of collectives of someones.

At this point I add that a person who assumes that such widespread control is possible and desirable will most likely be someone who also is committed to a kind of general determinism. In such a cultural atmosphere, the person as free agent is lost, discarded, factored out, made into a ghost in the social/industrial/economic machine. It should be clear that this tendency in contemporary thought is starkly opposed to what has been called the fundamental principle of Western Democratic Society: the absolutely basic worth of each individual free person. I suggest that anyone who wants to meditate upon this point should read Percy’s novel, Lancelot (1978). There one finds a world in which there is no evil, only illness brought about through due cause. If you take up the book, be sure to select a chair with a good seat belt; the volume has been certified by the Logician General as an established hazard to current assumptions.
Part Two

My theme now is that we stand on the edge of a new era in the social and human sciences.

We may escape the difficulties noted above and find a new way to conduct social science by rethinking some rather basic points. My proposal is that the culprit is not the general idea of science, but the narrow and self-confining conception that science, any science, is equivalent to Dyadic Science. Whatever knowledge we have gained of causal or functional processes retains its value and reality, but (as we shall see) we now have strong reasons for concluding that such knowledge alone can never be fully sufficient for a complete social science. However, it is unscientific to reinvent the wheel; the process of rethinking began some time ago in the work of Peirce, the second hero of this story.

Peirce’s triads

Charles Peirce made a number of interesting and original claims, but the first appearance in his thought of the one I shall focus upon here can be traced at least to 1866, when he was 27 years old, sitting at his desk in Cambridge, Massachusetts, thinking about the glaring gaps he had just discovered in the logic of Kant, which is how Kant became Peirce’s fallen hero. He duly published a defense of this claim in 1868 (first read publicly in 1867 — see Ketner 1986a: 3–4), then elaborated and defended it in many publications and manuscripts over the years, almost until the day of his death in 1914.

This notion was rediscovered by Walker Percy in this century, when he was in his mid-thirties in the 1950s, while sitting at his desk one summer day in Louisiana thinking about an event in the life of Helen Keller on another summer day in Alabama in 1887 (Percy 1975: 3). Recently, as the eighteenth Jefferson Lecturer for the National Endowment for the Humanities, with deadly accuracy Percy focused this particular notion upon the contemporary social sciences (a slightly revised form of the lecture was published as Percy 1989; see also commentary in the next issue of The Wilson Quarterly, plus a correction, 13:4 [1989], 143–144). In proper scientific lingo this proposal ought to be known as the Peirce/Percy Conjecture. By the way, this conjecture is a way of providing a defense of Percy’s observation, noted earlier, that assertions are not something one can study with a method that admits only functional or causal explanations; thus I am about to redeem my promissory note from
Part One. If we express Peirce’s claim in terms appropriate to our present discussion, it would read like this:

It is logically illegitimate, as many contemporary social and human scientists attempt to do, to construct triadic relations from collections of relations containing only dyads.

To aid in appreciating the strength of this, a little explanation is in order. We can grasp the sense of relation that Peirce meant by means of a generalizing technique he used. Here are two sentences, the first one describing a dyadic relation, and the second describing a triadic relation.

George pushed Mary.     John sold his car to Howard.

If we generalize the noun locations in these sentences by replacing them with blanks (which he also called ‘loose ends’), we get

____ pushed ____     ____ sold ____ to ____

If we convert the blanks to lines and call the remaining words the verb parts of the sentences, and generalize those verbs by replacing those words with big black dots, we get Figure 1. These diagrams, which Peirce called Valental Graphs (see Ketner 1986b), have the following meanings:

Some two unspecified entities (ordinary things, concepts, any objects of discourse) are in some kind of as yet unspecified dyadic relation with each other.

Some three unspecified entities are in some kind of as yet unspecified triadic relation with one another.

Figure 1.

In other words, these two diagrams are highly generalized models of the forms of dyadic and triadic relation.

Peirce defined connections between relations represented by diagrams such as these in terms of what he called ‘relative product’ (see Brunning 1981 and Herzberger 1981), which is a way of showing how relations combine. For instance, if we wanted to represent the sentence ‘John pushed George who sold his car to Howard’, it would look something like Figure 2. The relative product identifies the recipient of John’s push as the same person that sold a car to Howard. In the most general
graphical form, that is represented as in Figure 3, where '+' indicates relative product (which Peirce also called composition or bonding). Basically, bonding is a way of showing that two objects (in the sense of objects of discourse) previously thought to be separate are now to be considered as equal, and so treated as one.

Peirce considered two graphs equivalent in valency if, whatever other properties they might have, each had exactly as many loose ends as the other.

With this as a background, we can state Peirce's claim as follows:

No genuine nondegenerate triadic relation forms can ever be constructed from combinations of only dyadic relation forms.

This sentence can be demonstrated informally, within the body of assumptions Peirce made, in the following way. The valency of any unbonded Valental Graph is equal to the number of blanks (also known as loose ends) in the graphical form. If a new graph is constructed from some old graphs plus one or more new acts of bonding two blanks together, the valency of the new graph will be given by this rule:

Valency of New Graph
equals
Sum of valencies of the Old Graphs
minus two times the Number of New Bonds.

In this context, constructing a triadic relation from a collection of dyadic relations would require that the triadic relation be made up from bonding a number of dyadic relations. But the above rule, through a simple *reductio*, shows that this cannot be done. In such a case the Valency of the New Graph would be three or any other higher odd positive whole number. The Old Graphs would all be dyadic, which means that their valency sum would be an even number. The Number of New Bonds multiplied by two will always be an even number. An even number subtracted from an even number is always an even number. An odd
number is never equal to an even number. These considerations show that a triadic relation cannot be constructed from a collection containing only dyadic relations.

‘But it could be done’, says a careful objector. ‘If we have three dyadic relations like this [see Figure 4], we could join them like this [see Figure 5]. But notice that Figure 5 violates the provision that a triad be made from dyads only, for there is a bootleg triad — namely, the trivalent graph to the right of the three plus signs. Or, if the figure to the right of the plus signs is disallowed as a triad, the proposed maneuver violates one of the basics of Peirce’s set of assumptions: that each bonding involves joining exactly two loose ends, never three.

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Figure 4.

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Figure 5.

These results obtained by Peirce (see Ketner 1986b and 1989 for fuller discussions) are extremely important, for they show that the preference of contemporary Dyadic Social Scientists for explanations based exclusively upon dyadic relations is logically illegitimate. This is true because communicative and social phenomena involve genuine triadic relations, so any decision to allow only dyadic relations in the explaining material is ipso facto an attempt (bound logically to fail by the Peirce/Percy rule) to reduce such triads to dyads only. It might be said that I am not supporting my claim that communicative or social phenomena involve triadic relations. It is true that I don’t provide a supporting argument. It seems obvious that in communication we essentially have triadic relations such as ‘John gave a note to Susie’, or ‘Howard made a photograph of The Louvre’, or ‘Watson understood Holmes’s wave as a gesture of urgency’. The same would apply to social phenomena, since communication is presupposed in society; and, the same would apply in any science, social or otherwise, because every science presupposes and makes essential scientific use of social phenomena, even the so-called ‘hard sciences’ such as physics and chemistry. One of the conclusions here, then, is that the
social sciences are the logically-first sciences. The typical reductionist claim by Dyadic Scientists that physics (or some other hard science viewed dyadically) is the first or basic science is exactly upside down, 180 degrees out of phase, precisely backwards.

‘But’, the objector persists, ‘Quine (1954) has, within contemporary formal logic, shown that an interpreted theory having triadic (or higher adicity) predicates can be reduced to (constructed from) a theory containing only dyadic predicates’. This is a crucial article, because logicians inevitably refer to it in the course of saying that the Peirce/Percy Conjecture is simply false.

First, it is important to notice the broad structure of Quine’s reduction. There are three general elements in it. First, there is the target of reduction: a body of relations that are triadic or higher in valency. Second, there is a group of tools for performing the reduction of the target: call these his resources. Third, there is a group of relations exclusively dyadic which under the procedures of the resources rewrite the target. Quine has successfully and correctly used his resources to rewrite the target relations as strictly dyadic. Logicians have widely assumed that this is a wholesale reduction of any triadic relation to dyadic relations only: call this Claim Two. Quine himself, as far as I know, has not made such a claim; as far as I know, Quine has only claimed (and quite correctly) that with his resources he can reduce relations of valency three or higher to expressions containing just dyadic relations (call this Claim One). It is obvious that Claim One is not equivalent to Claim Two. And, for Claim Two to be true, Quine’s resources must also be exclusively dyadic. It can be shown that Quine’s resources are not completely dyadic, and that in at least two ways.

First, a careful examination of Quine’s presentation shows that among the resources he assumes in executing the reduction is the notion of an ordered pair. A pair is a set containing two entities, for instance, \{b,z\}. An ordered pair is a set of two entities with a specified order, for example \{c;m\}, where c is first and m is second. In other words, \{c;m\} and \{m;c\} are not identical, for while the members of each are the same, the order of each is different.

The interesting point here is that the concept of an ordered pair incorporates a genuine triadic relation. One can appreciate that by considering two entities not currently joined together in a set, but which by means of an act of imagination are placed together in a set. Notice that without such an act of imagination on someone’s part, there will be no set before us for consideration, not to mention an ordered set. That is to say, no matter whether a set is a natural being or a created being, a set imaginer is essential; if natural, the set imaginer is needed to direct our
attention to THAT set, and if created, then the set imaginier creates it through an act of imagination. Imagining sets is second nature for mathematicians, for they begin their most formal discussions of them with phrases like: ‘Let \( \mathbb{H} \) be the set \( \{0, s\} \)’, which is clearly an act of imagination, or an invitation for one. In any case, if neither the mathematical writer of such a sentence nor any reader of it undertook such an act of imagination, there simply would be no set present for study. Suppose I say, ‘Imagine a set composed of George Washington’s toothbrush and Andrew Jackson’s wooden comb’. As yet the set is an unordered pair, and I must engage in an additional speech act before it becomes an ordered pair. I do that by saying something like, ‘Imagine Jackson’s comb as the first item in an ordered pair in which Washington’s brush is the second item’. If we describe that speech act of mine in terms of Peirce’s generalization technique for relations, we get: ‘Ketner ordered Jackson’s comb as first and Washington’s brush as second’, or ‘_______ ordered _______ as first and _______ as second’. This is obviously a triadic relation. Actually, we don’t even need to consider the case of ordered pairs, for just the very notion of a set that is an unordered pair involves a triadic relation: ______ imagined ______ and ______ as a set’. Additional features of set theory relevant to these points, as well as responses to other objections to the position here taken, are discussed in Ketner (1989). Probably the most tempting additional objection involves invoking a property of ordered pairs: namely, that \( \{x; y\} = \{u; v\} \) implies \( x = u \) and \( y = v \). But I believe I have shown (1989: 139–140) that this objection does not succeed, because this implication states a necessary condition or consequence of that which is already an ordered pair, not a sufficient condition which makes something an ordered pair; my point is that a triadic relation is indispensable to create an ordered pair in the first place.

My objecting obliter at this point says, ‘But you are mixing psychological issues with mathematical matters; mathematicians don’t get involved in the psychic activity of imagination or of speech acts’. If this is true of mathematics, then it seems to me that mathematics is simply impossible, which is a kind of reduction to absurdity of this objection. If no mathematician ever imagined anything nor ever communicated with anyone else about imaginary concepts, then mathematics as a practice and a science would simply cease. Actually, here we are face to face with what is probably the most abstract form of Percy’s Antinomy (we might call this its mathematical form): to wit, as long as mathematicians are discussing some other subjects using mathematics, no antinomy arises; but when the discussion is about the nature of mathematics or the foundations of mathematics, topics such as acts of imagination by individual mathematicians or social phenomena such as intersubjective communication between
mathematicians are unavoidable. And if a mathematician refuses to discuss such topics, then mathematics as a human activity becomes inexplicable or full of contradictions. This situation is parallel to the predicament of the behaviorist psychologist who discusses stimulus and response all day at work, but goes home in the evening to the same old behavioristically inexplicable love/hate relationship with her own false true love.

In a recently completed book, Robert Burch (1990, and also in his presentation to the Peirce Sesquicentennial International Congress, September 1989), in inventing a powerful formal system based upon Peirce’s work, has achieved a major result with broad consequences; he has provided strong support at a high level of confidence to the Peirce/Percy Conjecture about the nonreduction of genuine triads, and in a manner corresponding to the highest standards of mathematical rigor. He has also provided convincing evidence (at an equally rigorous level) for Burch’s Thesis, an important new proposal, which states that Peirce’s logic of relations, with its irreducible genuine triadic relations, is adequate to express all relations. Furthermore, he has shown, at the same level of rigor, that while Quine’s reduction proof is correct, it does indeed assume at least one genuine triadic relation among its resources — quantification, for example; therefore Quine’s work is not a disproof of the Peirce/Percy Conjecture, as proponents of Claim Two above have thought (this is the promised second way of discarding Claim Two). And by the way, in view of Burch’s breakthrough, we must adopt the correct mathematical terminology for this matter and henceforth refer to it as the Peirce/Percy Principle, for it has been mathematically established. Burch’s work is a major new finding of sweeping importance.

It is possible to confuse my way and Burch’s way of freeing the Peirce/Percy Principle from the claim that Quine’s reduction refutes it, so it would be wise to be quite clear about the differences in these two ways. Burch has proved, within standard assumptions about the nature of mathematics and mathematical signs, that the Principle is correct and that Quine’s reduction to the dyadic is consistent with the Principle, because in achieving his results Quine makes use of constructive resources (quantification, for instance) that are irreducibly triadic. In other words, Burch’s contribution is sufficient to show that even on Quine’s own assumptions about the nature of mathematics and mathematical signs, Quine’s reduction does not contradict the Principle. On the other hand, I have gone slightly beyond (or perhaps behind) Burch and have made an additional point that is consistent with his results by showing that the formalistic assumptions made by Quine about the nature of mathemati-
cians and mathematical signs amounts to a hand-waving attempt to bypass what is irreducibly a triadic relation: the relation between (1) the mathematical mind that assembles or orders two objects into a set or ordered pair and (2) those two objects. And my claim is that this relation between the mathematical mind and objects it considers cannot, by a wave of the formalist wand, be excluded in this case, nor in mathematics in general. 'There is no such thing as the silly notion of the mind of a mathematician', you say? Is my mathematician friend Tom then a formula, or equation, or an ordered pair, or a set? I do really talk with him and he with me, and sometimes we come to a meeting of our minds. Talking, agreeing, socializing are realities on the basis of which the edifice of science is erected; they are not scientifically dispensable 'extras' that can be left aside. I am doing philosophy of mathematics? Perhaps (and it is nothing to be ashamed of), but only in the sense that I am pointing out a truth without which mathematics, no matter how austere or pure, would be impossible.

Burch's book, together with the other considerations cited above, strongly implies the indefensibility of Dyadic Science approaches in another contemporary topic of considerable interest for students of human and social science. I refer to semiotics, a conglomerate of reductionist approaches that includes themes from such arch-Dyadic-Scientists as Saussure and Morris. The foundational role of dyadic relations in the work of both figures is well documented in recent books by Milton Singer (1984) and Eugene Rochberg-Halton (1986). In Burch's Thesis we now have a striking new mathematical result, one with consequences as important as those of Turing's Thesis or Gödel's Proof. One such consequence is that no theory of signs is possible that reduces all communication to a series of dyadic relations. That such a thing is possible is precisely the claim and the aim of Saussure and Morris and disciples. Their principal hypothesis, that all communication is fundamentally and basically and exclusively composed of dyadic relations, has now received a strong disconfirmation. This is, on the other hand, a strong confirmation for Peirce's semiotic, the principal hypothesis of which is that all communication is fundamentally constituted of genuine triadic relations. These considerations also show that scholars who would like to mix dyadic semiotics with semiotic are attempting to mix oil and water. This state of affairs will also serve as a means of discovering whether contemporary semiotics is really a science or a mere world-view. For the test of a genuine science is whether its practitioners can acknowledge when a previously valued hypothesis is disconfirmed and change their procedures accordingly.
But I digress. Back to social science proper.

Here is a list of events usually described as dyadic relations. A bumped into B; A reacted chemically with B; A flowed into B; A is connected to B; A stimulated B; B responded to A; A caused B.

Here is a list of events typically described as real triadic relations. A understood B to mean C; A interpreted C as B’s message; A promised C to B; A bought C from B; C inherited B from A.

In terms of the Peirce/Percy Principle, no item in the second list is constructable from combinations of items in the previous list. Triadic relations cannot be reduced to (constructed or composed from) sets comprising exclusively dyadic relations. It now follows that each member of a third list, containing explanatory strategies that have been popular in Dyadic Human and Social Science, is logically inadequate: materialism, behaviorism, the social physics of Comte and others, or any program such as that of Saussure/Morris semiotics or behavioral psychology which relies exclusively upon efficient causal connections or other such strictly dyadic explanatory schemata.

Then what explanatory strategies are available to social science, given that the Peirce/Percy Principle is correct? How can we analyze thought, or mind, or communication, or society, or culture, or literature, if these are essentially triadic? For one thing, we know how not to proceed: it is a non-option to explain triadic activities such as understanding, interpreting, promising, buying, narrating, or inheriting exclusively in terms of dyads. That is, there might be some dyads in a successful social scientific explanation, but there must be at least one triad. Consider it this way. Socrates might have suicided with an Ivers Johnson gun. If so, any dyadic causes that could be given by physics and chemistry, of the pistol’s action and effect, would be relevant. But a social scientist would necessarily have to take up other topics, such as reasons for the act, its significance in the culture, or whether it represented a social trend.

Thus, we stand at a bold juncture. Having disestablished an old explanatory habit, we must now find a new one to replace it. No bandaid will suffice. We require a conversion experience, a fundamental change in basic practices.

How then shall we analyze thought, or mind, or communication, or literature, or society, or culture if these are essentially triadic? In this era we seem to have an urge to analyze. That seems innocent enough. But if analyze always means ‘reduce to dyadic forms’, then a hidden (and false) assumption accompanies this use of the word. On the other hand, if analyze x means ‘come to have a better understanding of x’, then the answer seems to be in this principle:
We must analyze triadic relations by means of other triadic relations.

In particular, if there is a matter about which we lack understanding, we can use a set of triadic relations we already comprehend reasonably well to model the relations in the area of relative ignorance. Once such a model is constructed, we can study or manipulate it to discover new things about the relations within it, hoping that the newly discovered relations there will be analogous with undetected relations in the area modeled. If they are analogous, then we will have increased our understanding of the area modeled. Stated in a very abstract fashion, that is Peirce's general method of diagrammatic thought, to which we now turn.

I hope this account doesn't contain too many loose ends.

Diagrammatic thought

In one of the most remarkable theoretical paragraphs ever written, Peirce brought together the concepts we need to make sense of diagrammatic thought as a nonreductive technique for modeling, and for thereby gaining an understanding of triadic phenomena. It is from a draft of his article, 'The logic of relatives', which appeared in The Monist in 1896 (published in broken form in CP 2, pp. 134–135; for a full account of this draft, see Ketner 1984). Semeiotic, of course, is the study of semiosis or sign action (triadic action) as opposed to the study of dynamic action. Where in this extract Peirce mentioned signs, read 'genuinely triadic phenomena', and for diagram read 'model'.

Logic, in its general sense, is, as I believe I have shown, only another name for semiotic (σημειωτική), the quasi-necessary, or formal, doctrine of signs. By describing the doctrine as quasi-necessary or formal, I mean that we observe the characters of such signs as we know, and from such an observation, by a process which I will not object to naming Abstraction, we are led to statements, eminently fallible, and therefore in one sense by no means necessary, as to what must be the characters of all signs used by a 'scientific' intelligence, that is to say, by an intelligence capable of learning by experience. As to that process of abstraction, it is itself a sort of observation. The faculty which I call abstructive observation is one which ordinary people perfectly recognize, but for which the theories of philosophers sometimes hardly leave room. It is a familiar experience to every human being to wish for something quite beyond his present means, and to follow that wish by the question, 'Should I wish for that thing just the same, if I had ample means to gratify it?' To answer that question, he searches his heart, and in doing so makes what I call an abstructive observation. He makes in his imagination a sort of skeletal diagram, or outline sketch, of himself, considers what modifications the hypothetical state of things would require to be made in
that picture, and then examines it, that is, observes what he has imagined, to see whether the same ardent desire is there to be discerned. By such a process, which is at bottom very much like mathematical reasoning, we can reach conclusions as to what would be true of signs in all cases, so long as the intelligence using them was scientific. The modes of thought of a God, who should possess an intuitive omniscience superseding reason, are put out of the question. Now the whole process of development among the community of students of those formulations by abstractive observation and reasoning of the truths which must hold good of all signs used by a scientific intelligence is an observational science, like any other positive science, notwithstanding its strong contrast to all the special sciences which arises from its aiming to find out what must be and not merely what is in the actual world.

I ask you to note carefully several things about this remarkable paragraph. First of all the sheer power of it will grow on you, so please give it a chance to serenade you. One instance of its power is the connection it makes between mathematics and novels! Second, by reading sign as ‘triad’, we get the result that semeiotic is the study of triadic action, a study accomplished by constructing and observing models! Abstractive observation is of course observation of relations in models. Also, a sign or representation, this paragraph encourages one to infer, is itself some kind of model of that which it represents (its object) to that which interprets it (its interpretant). Notice that Peirce called to mind a common-sense example of something we are constantly and routinely doing — making mental models of our life situations (often miniature narratives) and experimenting upon those as a way of planning our affairs. (He might just as easily have used examples such as Märchen, or folktales, or proverbs, which in effect often say to interpreters thereof, ‘life is like this’.) For future reference, let us refer to one of these as a — here I was going to make up a new word, but a perfectly accurate one already exists — self-image.

In diagrammatic method, Peirce preferred visual diagrams, probably because they appealed to sight, which is our most evolved sense. But he also recognized diagrams based upon auditory or other sensory channels, for instance speech as an auditory diagram (Peirce 1892, reprinted in CP 3. p. 259). By the way, old radio dramas are outstanding examples of auditory diagrams — remember how Molly would say, ‘Don’t open that closet!’ but Fibber did anyway, followed by a memorable sound effect?

But Peirce thought sight was probably best adapted for detecting new features of relational patterns in diagrams that model triadic relations presently under study. People sometimes say, when they want an explanation, ‘Draw me a picture’. To ‘draw a picture’, then, would be to proceed in the way Peirce would have recommended in response to the question,
'How can we study phenomena rich in triadic relations if dyadic considerations alone cannot exclusively do the explanatory job?' If we add that Peirce recognized algebras and other arrays of symbols as visual diagrams, then we can state that mathematics — not in the narrow sense in which it is usually understood today, but as the science that models (diagrams) relations in areas under study — would be among the finer tools for 'drawing pictures' that humankind has yet developed. Peirce said as much in the paragraph above.

But he did not limit the concept of visual diagram to sketches or marks. He clearly allowed for mental diagrams, in a way that would make a behaviorist blush, as being important intellectual resources. Some contemporary psychologists, in a way quite consistent with Peirce's ideas, have made good empirical progress on the notion of mental diagrams (Block 1981).

But I think there are even further consequences we can extract here. The principles of diagrammatic thought extend even into art. An exploration of this notion for visual art may be found in Scott's recent essay (1985).

And we could easily and profitably extend these insights to nonvisual art: novels, for instance. Percy has observed (1977: 360–361; compare Percy 1972), in regard to novels, that

art is cognitive, that is, it discovers and knows and tells, tells the reader how things are, how we are, in a way that the reader can confirm with as much certitude as a scientist taking a pointer-reading.

A corollary to the proposition that art in general and the novel in particular is cognitive is that the stance of the novelist in the late twentieth century is also diagnostic. The implication is that something has gone wrong, which it certainly has, and that the usual experts cannot tell us what it is — and indeed that they may be part of the problem.

Suppose we answer our question about how to proceed as scientists in the essentially triadic world studied by social and human science by taking this insightful suggestion in only a slightly different way than Percy intended. Suppose that Novel Science is one of the secrets of further progress in social and human science.

What is Novel Science? I'm saying that we now have in our hands all the necessary elements for what may be a new and very fruitful way of conceiving the method of social science. I'm trying to connect those basic parts.

For instance, we could think of a novel as a tool for aiding readers to construct mental (or nonmental) diagrams. These models would then be
available to readers who can perhaps learn something about (have an insight about — an In-sight!; see also Percy 1977: 367) an area of relative ignorance within their persons (within their personal self-knowledge, in the Socratic or Gnostic sense) by exploring the relations that are partially understood within the world (the diagram, the relational patterns) of a novel. Perhaps we could say that a novel is a large sign, a triadic relational pattern on a large scale, that can be a tool in diagrammatic thought (which is to say diagrammatic inquiry), the technique whereby one triadic relation that is relatively well understood is used to model, by mental diagrams, some other relations (often personal) that are not as well understood. If so, within the social and human sciences, novels might be outstanding tools for analysis, the process of achieving deeper understanding.

Alternatively, we could say that a good novelist provides materials with which readers construct artificial self-images. Once these are constructed, the novel scientist tries to offer ways a self-imager can manipulate such images to discover new relations in them. And if artificial self-image manipulation is successful, the self-imager’s understanding of whatever is being addressed is thereby increased.

There could be many variations on this general theme, many subtechniques within it. A more detailed pursuit of the problem, which cannot be undertaken now, would include consideration of Peirce’s three kinds of iconic representation (see the redaction of Peirce manuscript 478, which incorporates these matters within one of his best accounts of the fundamentals of semeiotic, in Ketner 1987: 72–78). There is only space for a brief outline.

His idea was that a quality an icon has as thing renders it fit to represent. He distinguished three kinds of iconic representation: images, diagrams, and metaphors. Images represent through having the same qualities as their objects. A diagram represents the mainly dyadic relations of parts of its object by analogous relations in its own parts. Metaphors represent the representative character (triadic character) of a representation by representing a parallelism in something else. I take it that Peirce meant that the object of a metaphor is the representative character of some sign presently not as well understood as one might wish. By showing that this representative character, probably a law or symbol, is parallel to the representative character of some better known sign (another law or symbol), the target or object representative character is interpreted, explained, better understood. Probably there is no example of a single pure image, pure diagram, or pure metaphor, but some experiences have one of the three factors more strongly, hence such cases are useful examples.
Obviously my description of Peirce’s much fuller discussion of these matters is only a sketch, but perhaps by its fruits we will know it.

Conclusion

Given all the difficulties of social science, one is tempted to ask some fundamental questions, such as: What is the aim or object of social science? What would it look like if it succeeded? At this point we can provide a few negative answers and propose some positive hypotheses.

I think we can safely propose that a sound social science would not have control or prediction as its aim, and that its object should be to increase our understanding of social realities. Such a notion arises directly from Peirce’s conception of science in general. Stated negatively, no one who desires dictatorial control over other persons can be a scientist (see, for instance, Peirce 1869; see also CP 2. pp. 397–400).

I had often thought that the social sciences might be secure if only one could arrive at a dramatic breakthrough, a convincing new crucial experiment or observation based upon some technique derived from a lightning-flash of insight by some genius. Mired in that rut, I would often mutter to myself, ‘Be patient, a Newton of the social sciences will arrive some day’. I now think this way of thinking is a mirage. For if such a new technique were based upon dyadic considerations, as this thought seems to presuppose, it would fail for the reasons already noted.

Then what is to be done? In fact, that which is to be done is in kind already being done. Every day each of us accomplishes multiple acts of understanding. The requisite breakthrough occurred many thousands of years ago when one of our ancestors, instead of merely reacting to the course of events, somehow first understood something. The Newton of the social sciences is that unknown ancestor who first understood and conveyed that ability to descendants.

This seems to imply that what has been called ‘common sense psychology’ or routine experience is a likely starting point in social science. However, beginning should not include reducing these phenomena to dyadic patterns, a tendency which has been dominant among social scientists in our age. That will fail. Instead, operating out of everyday and routine experience, in a spirit of fallibilism, let us begin to use novel science and other nonreductive procedures that will improve and correct common sense and routine experience. Although it cannot be argued here, I suspect that unanalyzed common-sense abilities, which are rich in triadic relations, lurk in the background everywhere in contemporary science of all kinds, and that these factors constitute irreplaceable contri-
butions and indeed make the otherwise unintelligible Dyadic Science approach (which doesn’t recognize these factors) semi-palatable and serviceable in a jury-rigged sense.

If we do that, what end or object might we envision? We might understand understanding better. At the very least we could avoid destructive reductionism. The brightest hope is that we might understand ourselves and others better. That is to say, the aim or object of social science would be seen not as prediction, or control, or reduction, or statistics of the collective properties of samples, but as the kind of improved self-understanding that necessarily includes a better understanding of other selves and of community.

My colleague Shelby Hunt has suggested that I am proposing to discard all causal knowledge. To say the least, that is an inaccurate description of my hypothesis, which is a way of countering the chief underpinning of Dyadic Science — the strange identification of all knowledge with knowledge of causes. By all means, let us retain and employ all causal knowledge, and let us seek to obtain new knowledge of that kind. But in the Peirce/Percy Principle, as supported by Burch’s results, we now have the strongest scientific evidence to support the claim that social science is not possible with only dyadic or causal knowledge. It is our duty as scientists to take notice of this result and to adjust accordingly in our future work. It would not be good science if persons who are tenaciously in love with the fundamental hypothesis of Dyadic Science were to refuse to consider these new findings. After all, the basic requirement for life as a scientist is the will to learn, which presupposes a capacity to change one’s mind in the face of new evidence. And these results for social science have serious implications for the ‘hard’ sciences, for as Percy’s antinomy shows, the practice of hard science is inescapably social, so the practice of hard science presupposes social science.

An insistent voice intones, ‘But your proposal isn’t science — it’s philosophy or literature at best and obscurantism at worst’.

I suppose that for a dyad addict, notions like those advanced here are unpleasant, even repugnant (which in itself is a phenomenon worth serious study — why should many of our best and brightest have a serious phobia about stepping outside the house of causation?). But they certainly don’t amount to obscurantism, although I would be the first to admit that I am treating understanding as primitive, as basic, as underived. And it seems to me that literature and other fine arts, plus our friends from the art and science of Nursing, have preserved that chunk of wisdom through the ages, sometimes against rather serious attack. Every explanatory hypothesis requires a primitive component, an unjustified starting point in terms of which everything else is discussed. My
suggestion is that we make the primitive element of social science equal to those irreducibly triadic abilities that are unique to human social life. And I don’t say that understanding must be permanently a mystery. Nonreducibility does not equal unintelligibility. Perhaps through diagrammatic thought we will deepen our familiarity with understanding. That sort of thing has begun, as represented in some of the topics mentioned in the story I have been telling.

Ironically, it seems clear that Dyad Addicts are the obscurantists here, for they refuse to investigate any noncausal phenomena. Moreover, on a closely related matter, they also adopt a certain eschatology: when confronted with a problem, their usual move is to say that further research into causation will eventually produce the desired solution; in the Peirce/Percy Principle, we now have solid reason for regarding this particular eschatology within social science as simply false. To borrow a distinction from Peirce, the real is everything having properties that are independent of any individual’s whim or wish or desire as to the nature of such properties. Causal knowledge then is one species of reality. But phenomena such as communication, rich in triadic relation — what Percy described as nonlinear, nonenergetic, natural phenomena, or what Peirce identified as semeioses — are also real in the same sense. If I really convey ownership of my ancestral farm to my nine-year-old son, something more than a series of causes has occurred, and this something more is also real. (This, by the way, is the principal logical point that Patricia Poteat — in her 1985 study — seriously misapprehended in a way that brought her to propose a radically distorted and inaccurate interpretation of Percy.)

Along about here someone usually charges me with resurrecting vitalism, the old notion that there are ghosts which biologists study. Nothing could be further off the mark. Nothing!

I will accept the accusation that what I conceive for the social sciences is philosophy, but not in the sense of some pleasant line of bull, which is unfortunately what some folks today think it amounts to being. I accept philosophy in the sense that it is the Greek word for that phenomenon for which science is the Latin word — one’s devotion to the goal of increasing one’s understanding of human beings and their relation to the cosmos. If we understand those two concepts in that sense, philosophy is not just pleasant talk; and literature or other art forms are disclosed as its intimate friends.

Note

* I have learned many lessons through discussions with Robert Burch, Vincent Colapietro, Shelby Hunt, Thomas McLaughlin, Walker Percy, and Hilary Putnam. I have their
acknowledgment that they don’t assume responsibility for a single one of my imperfections, but they surely have my heartfelt gratitude and admiration.

Just as this essay was completed, the saddest words were spoken — news that Walker Percy was lost to us in death.

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