HARTSHORNE, PROCESS PHILOSOPHY, AND THEOLOGY

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Hartshorne and the Basis of Peirce's Categories

Kenneth Laine Ketner

Peirce's categories have not received a warm general welcome, either in his day or in ours. However, Charles Hartshorne is one master philosopher who has taken them seriously, and with revisions, used them in his own work.

I do not intend here to look into the categories as Peirce proposed them (initially in 1867) or as Hartshorne has revised them (1983 and 1984). Instead I want to consider the basis for them to be found elsewhere in Peirce's work. This distinction between the categories and the basis for them is one that some students of Peirce have missed, but which Hartshorne knows accurately: the categories are hypotheses within Peirce's metaphysics which are based upon certain suggestive results Peirce obtained in mathematics and the logic of relatives. (These results are hereafter mentioned simply as "basis" or "the basis.") I believe it can be shown that this basis has been misinterpreted by a number of students of the topic who also happen to be persons who may have influenced Hartshorne's understanding of this matter. After presenting and defending my interpretation of the basis, I hope Hartshorne can be induced to comment upon it, and whether it would change the way in which he understands and deploys Peirce's categories.

The categorial basis and cenopythagoreanism

After becoming aware a few years ago that Peirce's Existential Graph system of logic (hereafter EG) was a key for gaining an understanding of many basic points within his work, I began to seek for its roots. I believe I discovered them, in both the intellectual sense (reported in 1986a) and textual sense (reported in 1987b). Textually, the beginning of EG is found in *MS* 482 (following the cataloging system in Robin 1967), wherein Peirce developed it out of graph theory within topology. Under the heading of "Valency Analysis" (VA) I have isolated one aspect of the means for that development (1986a). This is a convenient name which I have coined; VA is not a phrase Peirce used, but it is consistent with his intent and practice.

For some time scholars have complained about being unable to find Peirce's proof for two distinctive theorems, which are (in uninterpreted form) as follows:

NONREDUCTION THEOREM:

No triad can be composed exclusively from dyads.

SUFFICIENCY THEOREM:

Tetrads or higher *n*-ads can be composed exclusively from combinations of monads, dyads, or triads.

In *MS* 482 these two theorems are established within VA, which consists of a formal system of uninterpreted graphs. An entity with one "loose end" is a monad, with two loose ends a dyad, with three loose ends a triad, with *n* loose ends a *n*-ad. The number of loose ends is a property that can also be described as the adicity of the graph. It is convenient to represent such entities with drawings like these.

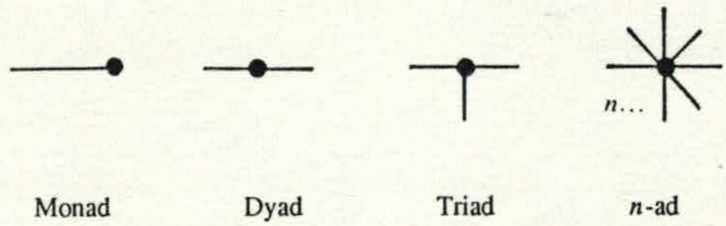
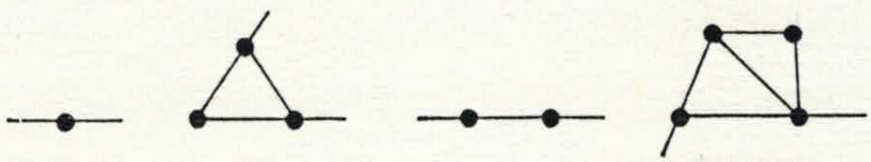


Figure One

Loose ends are places in which any connection that is possible or permissible *may* occur. Connection of loose ends is by the process of bonding exactly two of them at a time. Within VA, one ignores material reasons for joining two loose ends to focus upon formal patterns of composition or rules associated with bonding. Two graphical entities are said to be "valency equivalent" if, no matter what other properties they possess, they have exactly the same number of loose ends. Thus all of the following four graphs are valency equivalent (valency of each is two).



Valency = two

Figure Two

With these understandings, it is easy to prove that Nonreduction and Sufficiency are theorems *of VA* (proofs are in Ketner 1986:377-382). It is important to note at this point that these are not theorems about categories, because so far we are working with just an uninterpreted formal system.

Peirce's next step was to interpret VA over relations. He did it in this manner. He conceived relations rhematically, meaning that he first considered a relation sentence, then conceptually subtracted (precided) all noun-like aspects to produce what he called a rheme (a kind of partial sentence form). In that manner,

"George is a Bolivian"

becomes

"________ is a Bolivian";

"Excess current caused the resistor to melt"

becomes

"________ caused ________";

"Smoke represents fire to the priest in the tower"

becomes

"________ represents ________ to ________."

Peirce referred to the kinds of sentence forms (exemplified by the second of each pair above) as "relatives," from whence is derived his phrase "Logic of Relatives." I will avoid that phrase for now, because in his later philosophy, it became virtually a general name for his entire effort. Let us use a terminology similar to that Peirce employed before "relative" became such a general term for him. We may call these partially-precided relation sentences, for only the noun-like elements have been precided away. A fully-precided relation sentence is one in which the relating aspect is also generalized. That is accomplished by replacing the remaining words, the verb-like elements, with a big black dot. At this level of precision we find that monadic, dyadic, triadic, and n-adic relation sentences look just like corresponding graphs in VA. That is, the graphical entities of Figure One now mean respectively:

"Some object has some property"

"Some one object is in some dyadic relation to some second object"

"Some first object is in some triadic relation with some second object and some third object."

(Of course, 'object' is understood to range over "objects of discourse.")

Now it is clear that Peirce regarded that which we can know as consisting of relations. He rarely mentioned this important aspect of his system, but here are two clear instances (see also my discussion in 1988b):

> . . . Whatever we know, we know only by its relations, and in so far as we know its relations. (From a draft of 1868 in Moore et al. 1984:164)

> In reality, every fact is a relation. Thus, that an object is blue consists of the peculiar regular action of that object on human eyes. This is what should be understood by the "relativity of knowledge." Not only is every fact really a relation, but your thought of the fact implicitly represents it as such. (From a publication of 1892, in Hartshorne et al. 1935-58:3.416)

Once VA is interpreted over relation sentences, it has another contribution to make, for VA in conjunction with Peirce's long interest in the nature of scientific classification (described in Ketner 1981) inspired the Doctrine of Cenopythagoreanism.

> In classification generally, it may fairly be said to be established, if it ever was doubted, that Form, in the sense of structure, is of far higher significance than Material. Valency is the basis of all external structure; and where indecomposibility precludes internal structure—as in the classification of elementary concepts—valency ought to be made the first consideration. I term [this] the doctrine of *cenopythagoreanism.* (*MS* 292:34, 98)

Once VA is interpreted over relations, Peirce—looking with cenopythagorean eyes—found that all relations (and hence all knowables) divide into just three natural classes: monads, dyads, and triads. Dyadic relations so interpreted cannot be constructed from monadic relations; the Nonreduction Theorem shows that triadic relations cannot be constructed exclusively from dyadic relations; and the Sufficiency Theorem shows that relations of adicity n, where n is equal to or greater than four, are reducible to (are valency equivalent to) a combination of n-minus-two triadic relations. That is, a tetrad (adicity four) is reducible to two triads (four minus two is two), a pentad is reducible to three triads, a heptad is reducible to four triads, and so on. Furthermore, EG can be generated out of VA (summarized in Ketner 1986a:384-389). Roberts (1973) has shown that EG is consistent and complete for predicate logic with relations. From this point to the categories is but a short step, but one which will not be taken now, for I want to stay within the basis to see if it can be sustained. (Persons interested in reading two sound studies of Peirce's categories might consult Esposito 1980 and Krausser 1977; see also Peirce's intellectual autobiography in Ketner 1987a.)

Gallant charge of the ordered pairs

When I had what I thought was a good understanding of the topological foundations of Peirce's natural classification of kinds of relations via the Nonreduction and Sufficiency Theorems, it immediately occurred to me that these are important results. For instance, if the interpreted Nonreduction Theorem is correct, then all kinds of reductive explanatory strategies could be shown to fail. For instance, behaviorism or materialism in psychology, which are basically ways of reducing triadic relational phenomena to dyadic causal chains, would collapse. Here I am inspired by Walker Percy (1975), who is an independent re-discoverer of many of the conceptions about relations I have sketched above. Or, for another example, attempts to develop artificial intelligence only from deterministic resources would be doomed from the start (that thesis is defended in Ketner 1988a). I also quickly found that I was not alone in my researches, for two fine Canadian scholars were active in the same general area, Herzberger (1981) and Brunning (1981), of whom more later.

The first thing I wanted to know was what had happened in the logic of relations since Peirce's day. In talking with colleagues specializing in mathematics and logic, a number of persons mentioned that it was well known that Peirce was wrong about Nonreduction, and that triadic predicates could be reduced to dyadic ones. Inevitably I was referred to a paper by Quine (1954) where Reduction was said to have been achieved. That paper is a formal investigation, but it is clear Quine intended to interpret his formal results over relations. That point is further reinforced by some of his later comments (Quine 1981:201). It will be convenient to have the first two paragraphs of Quine's Reduction before us.

> Consider any interpreted theory Θ, formulated in the notation of quantification theory (or lower predicate calculus) with interpreted predicate letters. It will be proved that Θ is translatable into a theory, likewise formulated in the notation of quantification theory, in which there is only one predicate letter, and it a dyadic one.
>
> Let us assume a fragment of set theory, adequate to assure the existence, for all x and y without regard to logical type, of the set $\{x,y\}$ whose members are x and y, and to assure the distinctness of x from $\{x,y\}$ and $\{\{x\}\}$. ($\{x\}$ is explained as $\{x,x\}$.) Let us construe the ordered pair $x;y$ in Kuratowski's fashion, viz. as $\{\{x\},\{x,y\}\}$, and then construe $x;y;z$ as $x;(y;z)$, and $x;y;z;w$ as $x;(y;z;w)$, and so on.

To show that Peirce's Nonreduction Theorem is incorrect, and that Reduction is possible, it would be sufficient to show that there is at least one triadic relation that can be reduced to some collection composed exclusively of dyadic relations. It seems clear that Quine proposes to do that, not only for one triadic relation, but for all relations of adicity three or greater. And one of the essential tools he employed in the proof is the notion of an ordered pair. Let us take a careful look at "ordered pair."

The definition for ordered pair was given by Kuratowski in 1921 (see Kuratowski and Mostowski 1968:59), inspired by Weiner (1914). Pairs sound quite dyadic. However, I shall argue that the concept of an ordered pair is indeed a triadic relation. Therefore, use of it in a proof that many understand as breaking Peirce's Nonreduction Theorem would be a violation of the rules of the game. That is so because a proper reduction would have to constitute a triad from a collection of dyads only, not from a collection constituted by a bunch of dyads plus one triad. If ordered pairs are really triadic relations, then Peirce's Nonreduction Theorem, construed in terms of his definitions, is still standing in view of Quine's results. That is not to say that Quine's results are wrong; but it would mean that Quine's correct results do not break Peirce's Nonreduction Theorem.

Let us begin at a common sense level, and start with the notion of a set that is an unordered pair. I take that step, because a triadic relation seems to be buried even in the idea of a set that is an "unordered pair." I am trying to think of two widely disparate items such that nobody now considers them as a set or collection. Suppose that in the Arbuckle Historical Society Museum in Murray County Oklahoma there exists an object known locally as Mazeppa Turner's pocket knife, and that in Saint Tammany Parish Louisiana exists what people there call General Van Dorn's battle flag. A set is some number of objects of discourse brought together in our conception or imagination. I can say, "Collect or bring together in your imagination those two objects, Turner's knife and Van Dorn's flag; when you have done that, call the result of that process the set **H**." Since the process of bringing these two extremely disparate items together in imagination is an action of a person, a collector who puts a collection together, we can see that even the notion of a set that is an unordered pair presupposes a triadic relation. Based on our example, that relation would be represented in this sentence: "Robert Earl imagined a set composed of Turner's knife and Van Dorn's flag." That can be written rhematically as "________ imagined a set composed of ________ and ________," which is clearly trivalent.

Now let us consider the concept of "ordered pair." Suppose there are two particular rocks on a table in front of us. One of us notes their presence, then imagines them as a set. But suppose further that we wanted to designate one of these as the *first* rock *a* and another as the *second* rock *b*. That is, we want to move from an unordered set to an ordered set. To bring that about, I state, "I order *a* as first and *b* as second." That statement, considered as a partially precided rheme, becomes "________ ordered ________ as first and ________ as second." In other words, it is a triadic relation involving a giver of order to two objects which are thereby ordered. Its fully precided form is that of a VA triad (where O is the above triadic relation):

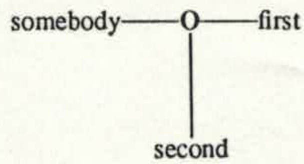


Figure Three

Here is an often-used way of defining an ordered pair (adapted from Halmos 1960:22). We can define the ordered pair $a;b$ as the set composed of the set a and the set a,b, where $\{a\}$ is simply the set one obtains at or before the first location in the order intended in $a;b$, and $\{a,b\}$ is the set obtained at or before the second location in the order intended in $a;b$. This, however, is no better, for a triadic relation is evident in the definition of ordered pair, almost in the common sense way. Furthermore, the set $\{a;b\}$ presupposed in the definition itself presupposes a triad as in the case of Robert Earl's unordered pair.

Some set theoreticians take a more simplified approach. For example Rubin writes (1967:47, adapted into Quine's notation):

> The actual technique used to define an ordered pair is unimportant.... What is important, however, is that it is a set and it has the one property that an ordered pair must have to deserve its name. That is, $(x;y) = (u;v)$ implies that $(x = u \;\&\; y = v)$.

This definition also presupposes Robert Earl's triad. But let us hold that card in reserve, and consider the definition from another aspect. The definition could be understood as a way in which the equality relation can be used to produce the notion of order.

I take this definition to mean that if two ordered pairs are equal, then it follows that the first element of the first pair must be equal to the first element of the second pair (and ditto for the second elements of the two pairs), and that if this condition holds, we therefore have an ordered pair. But one cannot produce order in this way without implicitly introducing the above mentioned triadic relation. Consider two unordered pairs p,q and r,s. And suppose that we are talking about a universe of discourse that does not include the predicate of serial order. We now assert that $p = r$ and $q = s$. In so saying we have not introduced a concept of serial order into this serially orderless universe. However, if we now allow the predicate of serial order to enter our universe, for p

to be equal to *r*, the two must be equal in all predicates, including the serial order predicate. And for a pair to be ordered, someone must combine them according to some principle of order. Therefore, it seems to me that this method of defining an ordered pair cannot smuggle in the requisite conception of order using the notion of equality, so it is no different from the first common-sense case.

Some set theoreticians simply avoid verbiage and get right to the point by unflinchingly biting the bullet in (it seems to me) an unavoidable manner, for example Selby and Sweet (1963:73): "A pair of objects, one of which is designated as the first component and the other as the second component, is called an ordered pair." Here clearly we are asked to consider a designator, a combiner, an orderer—whether a person, an intelligent algorithm, or a Martian—as being the agency which brings two components together and designates one as first and another as second. And if we imagine such a thing, what we are imagining is a triadic relation.

The process of producing a set, whether ordered or unordered, is indeed just that: a process. The process begins with no set present. A set-maker comes on the scene and puts two things together in imagination. At the end of that process we have something new, a set. This set is what Peirce would have called a hypostatic abstraction. By that he meant that it is often profitable within inquiry to give the result of a predictable and already known process a noun or substantive name, and thereby to refer to it as a fact. And to refer to such substantives routinely sometimes leads researchers to forget the original process. Perhaps that has happened in the case of set theory. I believe that one could add a small footnote to Peirce's notion of a hypostatic abstraction by saying that when science is intimately involved in studying a problem for the first time, the language of research is typically full of process phrases. Once a piece of research is complete, known, dependable, often the language of research becomes noun-like in regard to those items just recently mastered, so one can handily refer to the previously established results in the context of the new problems at hand.

One paragraph in *Naïve Set Theory* (Halmos 1960:24-25, adapted to Quine's notation) strikes me as prophetic.

> However important set theory may be now, when it began some scholars considered it a disease from which, it was to be hoped, mathematics would soon recover. For this reason many set-theoretic considerations were called pathological, and the word lives on in mathematical usage; It often refers to something the speaker does not like. The explicit definition of an ordered pair [$((a;b) = \{\{a\},\{a,b\}\})$] is frequently relegated to pathological set theory.

I would not use the term 'pathological.' There is nothing wrong from my point of view about the concept of ordered pair; however, there is good reason to think that there is something inappropriate about conceiving it as being a dyadic relation. Therefore someone who uses the notion to reduce triadic relations is really reducing a triad to a collection composed of dyads plus one triad (ordered pair)—or plus two if one adds Robert Earl's triad. Obviously I have not advanced anything like a formal proof that ordered pairs are triadic. I have tried instead to appeal to the level of reasonable considerations.

Abraham Fraenkel ended his article on set theory for *The Encyclopedia of Philosophy* (1967, 7:426) with these words:

> ... the modern development of set theory seems to shatter mathematics altogether, at least in its analytical parts. New axioms apparently need to be introduced, corresponding to a deeper understanding of the primitive concepts underlying logic and mathematics. Yet nobody has so far succeeded in discovering even a direction in which such axioms might be sought.

One wonders if Peirce's logic of relatives, which accepts triadic relation as a primitive, might be a direction that Fraenkel has considered. In any case, these words seem to suggest that set theory is at a revolutionary phase of its development. Perhaps one of the reasons it has reached a dead end is its almost studious elimination of triadic relations in its explicit fundamentals; one hardly finds overt discussions of triadic relations anywhere in treatises on set theory, and never, as far as I can see, as a primitive element. It is as if they were not real. This means that set theory as it is now constituted harbors a serious implicit metaphysical bias, which might be some part of the cause of its apparent self-limitation.

Other suspicions about Peirce's two theorems

The first case to be considered now may have provided Hartshorne with a false lead in his dealings with Peirce's categories. In 1934, Eugene Freeman published what is still a solid treatise on the subject. Hartshorne wrote its preface. There are a number of prescient insights in this work, an important one being Freeman's recognition that Peirce was a mathematical empiricist (1934:3, where the term is attributed to "my teacher and friend, Professor Charles Hartshorne, under whose inspiring guidance this study was undertaken"). Freeman realized that valency was important in the basis of Peirce's categories, and he proceeded to give an outline of the matter (1934:15). After a good start, however, he made some mistakes. After introducing the graphical forms of monads, dyads, and triads, he forgot to mention that all bonding occurs two at a time. And when he began to present diagrams of actual compositions (bondings) of graphs and relation sentences, he made additional mistakes. For instance (1934:16), composition of two monads is represented as:

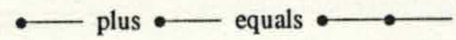


Figure Four

This shows a bond at a place Peirce did not allow, and because of that, a result with the wrong valency is produced. Freeman's bonding of two monads produced another monad. Bonding of two monads in Peirce's system actually produces a medad, a zero-valent graph in which every loose end is bonded, thus:

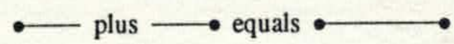


Figure Five

A similar mistake was made in Freeman's next example, which I draw here first in its erroneous form, followed by the correct form:

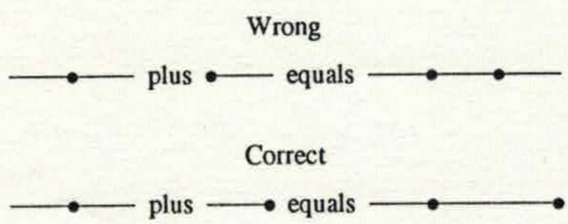


Figure Six

Three other graphs he drew are correct.

In Peirce's system, a monad bonded to a monad always produces a medad; a dyad bonded with a monad always produces a monad; a dyad bonded with a dyad always produces another dyad; a dyad bonded with a triad always produces another triad; a triad bonded with a triad produces a tetrad, and so on.

When Freeman turned from graphs to parallel English sentences, he again made some serious errors. I list below his first such form, followed by the correct form.

Wrong

> The monad "there is whiteness" plus the monad "there is hardness" gives the more complex monad "there is whiteness and there is hardness."

Correct

> The monad "something is white" plus the monad "something is hard" gives the medad "some white thing is a thing that is hard," or "Some white thing is hard," the Aristotelian *I* proposition, in other words (see Ketner 1986a:386).

He gave three additional examples like this one, incorporating relations of higher adicity. But in the additional three cases provided, bonding cannot occur in the way Freeman described, principally because most sentences in his examples are already medads—sentences with no "loose ends"—and hence not further bondable on Peirce's approach.

Freeman was aware that Peirce moved from this basis to a set of hypotheses about metaphysical categories, Firstness, Secondness, and Thirdness. But with this seriously erroneous grasp of the basis, his description of the transition from basis to categories must be suspect.

The next instance to be considered does involve more than a suspicion. Arthur Skidmore, another student of Charles Hartshorne, has argued (1971) for the incorrectness of Peirce's Nonreduction Theorem. His argument, illustrated below in graphical form, is that three dyads *can* be combined to form a triad.

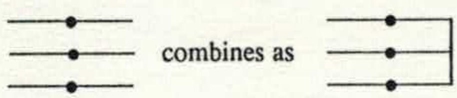


Figure Seven

That this is wrong from Peirce's point of view can be seen from either one of two standpoints. From one, the move is incorrect because it allows three loose ends to be bonded in one step. Peirce allowed two and only two loose ends to be joined in any single act of bonding. So this move would simply not be licensed by Peirce's VA system (Ketner 1986a). From another standpoint, the diagram in the right side of Figure Seven *can* be construed in Peirce's system as the composition of three dyads and one triad, thus:

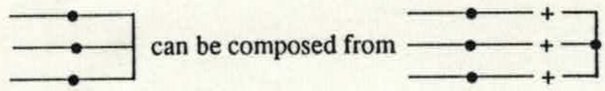


Figure Eight

This, of course, does not break Peirce's Nonreduction Theorem, for here one has composed a triad from three dyads and a triad. The point of triple junction was something Peirce allowed (which he called Teridentity), but he clearly recognized it as a triadic relation (see Brunning 1981). Herzberger (1981:55) has given additional reasons for rejecting Skidmore's account as inconsistent with that of Peirce.

Christopherson and Johnson (1981:241) are two additional persons with suspicions about the Nonreduction Theorem. The reason they give is by now familiar: "Set-theorists know that all relations can be treated as sets of ordered pairs, and that as a consequence *n*-place relations can be reduced to dyadic ones." This claim has been covered above. They go on to consider Thirdness with suspicion, but since this falls outside the basis, it is beyond my present scope.

Hartshorne's revision of Peirce's categories

In his revision of Peirce's categories (1984:77-78) Hartshorne characterizes Peirce's approach in this way:

> Peirce regards the single other as definitive of Secondness, and dependence upon two others (Thirdness) as essentially different, while dependence on more than two can, he holds, be reduced to cases of Thirdness. Thus he *counts the number of items* on which a phenomenon depends, defining Firstness as dependence on zero others, Secondness, on one other, Thirdness, on two others, and dismissing all higher numbers as reducible.
>
> My suggestion is that here Peirce misapplied the numerical model and thereby incurred needless trouble.... The number of items on which a phenomenon depends or does not depend is, I suggest, categorially irrelevant. What counts are the *kinds of relations* of dependence or independence.

Let us display in tabular form the basis Hartshorne attributes to Peirce alongside the basis Peirce actually used (where Roman numerals represent Peirce's categories).

CSH

I = depend on 0 other

II = depend on 1 other

III = depend on 2 other

CSP

I = kind of relations with monovalent external form

II = kind of relations with bivalent external form

III = kind of relations with trivalent external form

Again, I do not want to focus upon the categories proper (keeping them mostly in our peripheral vision), but I do want to inquire what kind of understanding about Peirce's basis Hartshorne might have, as exemplified in the remarks above. It strikes me that the first two sentences of the above paragraph are not at all what Peirce had in mind as the basis of his hypotheses about the categories. It would be helpful to know which passages from Peirce Hartshorne regards as supporting that interpretation. Counting the number of items upon which a phenomenon depends is something that Hartshorne has introduced, and it seems particularly foreign to Peirce's basis. This topic is repeated later in the paper (1984:84) where Hartshorne recommends, "He should not have been so fascinated, almost hypnotized, by the idea of counting, 'One, two, three.' " That is not the point at all within Peirce's basis, instead it is a question of topology interpreted over forms of relation. Simple counting is not the mathematical basis of Peirce's categories. Indeed, had Peirce (with his meticulous concern for terminology) proceeded in the way Hartshorne suggests, Peirce would have named his categories Zeroness, Firstness, and Secondness.

In a somewhat related essay (1983:3), Hartshorne seems to accept the dismissal, supplied by his student Skidmore (1971), of Peirce's Nonreduction Theorem. But he went on to say that his revision of Peirce's categories need not accept the Nonreduction Theorem, for he does not distinguish the categories by counting the number of terms in the dependence or independence relations. Now that seems strange, for the Nonreduction Theorem is at the heart of the basis of Peirce's categories. If one rejected that principle, which is so fundamental for Peirce, I do not see how Hartshorne can speak of a *revision*—it seems much more like a wholesale renovation, of the basis, at any rate. Perhaps in view of the previous discussion Hartshorne would not now accept Skidmore's proposed refutation of the Nonreduction theorem.

I have the impression that Hartshorne wishes to have a categorial set in which each category is separable from the others. That is to say that on his account there would be a clear and pure instance of Firstness (and of each of the other categories). But this was not Peirce's way, for he asserted in many places that his categories were universal: each one and all of them are to be found in any experience in some degree; there is no pure example of any one of them. Indeed, some experiences exhibit more strongly one or another of the categories. For instance, surprise is a favorite example of Secondness, but no experience of surprise on Peirce's view is lacking either in Firsts or Thirds.

That is why Peirce called them the Universal Categories: each of them is in every experience. The phrase 'Universal Categories' does not mean that "some category is to be found everywhere." The only way to gain some understanding of an individual category is to abstract each of them out of any given experience through clues such as valency. This means that to some extent Peirce was a rationalist—not an unexpected result, since his work incorporated a little bit of many things, usually combined in a brilliant new way.

Finally, Hartshorne tells us that what *really* counts are kinds of relations. That is precisely Peirce's point too. But the kinds Peirce had in mind were kinds of forms of relations; Hartshorne seems to have in mind material kinds of relations.

Would the above explorations in Peirce's basis make his approach more acceptable to Hartshorne, or influence Hartshorne to be less inclined to revise Peirce's categories, or perhaps bring him to accept them outright?

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