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# THE CAUSALIST PROGRAM. RATIONAL OR IRRATIONAL PERSISTENCE? *

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## 1. Introduction

I intend to discuss some disturbing problems concerning the notion of *causation* and those of *rationality*, *objetivity* and *progress* in scientific knowledge: they deal with what I will call 'The Causalist Research Program' or 'The Causalist Paradigm'.[1]

Two problems —intimately connected but basically different— have often been confused in former times:

* A summary of part of the contents of this paper was read at the *Terceras Jornadas de Epistemología y Metodología de la Investigación y de la Acción* (Universidad de Belgrano), held in the City of Buenos Aires, Argentina, in September 1985. A summary of other parts was read at the *Jornadas de Pedagogía y Epistemología* (Facultad de Ciencias Exactas y Naturales, Universidad de Buenos Aires), held in the City of Buenos Aires, Argentina, in June 1985. Another summary was read in the Universidad Nacional de La Plata, in the City of La Plata, in December 1988. Section 3 of this paper is a reformulation of some parts of the Introduction of my thesis [2] presented in SADAF (Sociedad Argentina de Análisis Filosófico) in December 1985. Parts of that Section have been published in Spanish, included mainly in Sections 2, 5 and 8, in [3]. I am grateful to Gregorio Klimovsky, Alberto Moretti, Raúl Orayen, Gladys Palau, Agustín Rela and, very specially, to Dorothy Edgington, for very helpful comments on earlier drafts of this paper. Many thanks too, to the participants in the Seminary I directed in SADAF in 1986, for the illuminating discussions on this and related items. I am also grateful to Horacio Abeledo and to Dorothy Edgington for checking my English of my next before last draft. For the errors introduced in this last version I am solely responsible.

[1] See [6], especially section 3. See also [5]. I will not worry about the important differences between the notions of a *Scientific Research Program* —in Lakatos's sense— and that of a *paradigm* —in Kuhn's sense— because they are not relevant for the present discussion.

a. The explicatory analysis of natural laws and of the nomological connections between events.

b. The explicatory analysis of the relation of event causation (or the cause-effect relation between events).

Research programs have often confused these problems. Modern discussions, from Humean times up to our century, have discussed important issues, such as, for instance, the physical necessity of those relations, making no relevant differences between nomologic connection and the relation of causation.

In 1912–13, Bertrand Russell[2] launched his famous attack against the possibility of an analysis of the relation of causation *in a scientific context*. At the same time, he firmly pressed for an explication of the notion of nomologic connection or natural law. Russel proposed a new research program which would deal exclusively with the epistemological analysis of natural laws. On the other hand, he rejected the old causalist program as vitiated by inaccuracy and anthropomorphism.

Nonetheless, the old causalist program stayed alive, and even progressed, but *only in relation to the progress of its nomological component*, within that confused mixture. It did not seem to be of great interest to the philosophic-scientific community to follow Russell's proposal.

In 1928, Ramsey[3] made an advance in the explication of the notion of natural law. He is followed by Popper[4], Nagel[5] and especially by Hempel and Oppenheim.[6] But the old causalist program goes on, trying to analyse the notion of causation. During the last decades, that program has been constantly shaken

[2] See [14].

[3] See [7], chapter 3, pp. 73–74 and note* at p. 73.

[4] See [13].

[5] See [11], especially chapters 3, 4 and 5.

[6] See [4].

up, because of its exceptionally important negative heuristic. But it does not make any progress: it has no positive heuristic. Sophisticated theories are developed in order to analyse event causation. They teach that the relation of causation is a nomological connection *and something more*. We find, depending on the theory, the part of that *something more* played by: necessary conditions, sufficient conditions, different sorts of combinations of these. We find the condition of manipulation. We find also the relation of temporal precedence.

All these analyses are refuted again and again by counterexamples. The classical difficulties, in which counterexample shooters take delight, are the problems of effects (or of asymmetry), of epiphenomena and of overdetermination (symmetrical and asymmetrical). It will be sufficient for us here to deal with the first one.

The relation of causation is asymmetric in most cases, perhaps in all of them. Most examples of that relation are intuitivelly asymmetric and a good explication must produce an analysis which ought to explain that asymmetry. For instance, if we say that the gun shot caused Peter's death, we would not accept as good an analysis which allows us also to say that Peter's death caused the gun shot.

I believe that event causation is intuitivelly asymmetric in *all* cases. In other words, I think that it is logically asymmetric, or *type*-asymmetric. But, inasmuch as some authors[7] do not agree about this point, I will work only with asymmetric *cases*. That is: I will not deal with a logical asymmetric relation, but with cases of individual asymmetry.

A good analysis must indicate conditions for an event $a$ to be a cause of an event $b$, such that, for the large set of asymmetric cases, it precludes the possibility of event $b$ being a cause of event $a$. The problem of finding such conditions is the problem of effects or of asymmetry. From now on I will use the

[7] For example, see [12], chapter VII.

term 'asymmetric' and similar ones, meaning *case-asymmetric* for the large number of examples of asymmetric event causation relations (perhaps, as I suspect, of all of them).

Now, returning to our story, we face the fact that each new analysis of the notion of causation —just at its birth— is surrounded by a plethora of counterexamples. As a consequence, each analysis becomes more sophisticated, with more restrictions. And, immediately, new counterexamples appear again, so that the old analyses are replaced by new ones and the new ones by newer ones, each one more sophisticated than the previous one. But there is no progress in the paradigm.

Meanwhile, again in 1966, Carnap[8] rejected the causalist program on scientific contexts, and propounds a nomologic one. But he goes on using the expression 'causation relation', corresponding to the nomological connection; perhaps, because of the old confusion relative to both ideas. Carnap considers that the causation relation (prior to the explication) is the production relation. That relation has a strong anthropomorphic content, which disappears during the explicatory catharsis, leaving behind only the pristine and transparent post-explicatory nomologic relation.

I do not mean that Carnap's proposal is similar to Russell's. Carnap accepts the possibility of facing the study of natural laws by means of modal logic and of counterfactual conditionals. But he is propounding, as Russell does, an acausalist paradigm, even if he unfortunately uses for it, as so many other authors do, the expression 'causation relation'.

Finally, during the last decade, incredibly sophisticated analyses were developed in order to explicate once and for all the notion of causation. They are the counterfactual analyses of event causation, based in the possible worlds semantic for

[8] See [1], chapters XIX to XXII.

modal logic. David Lewis[9], Marshall Swain[10], John L. Pollock[11] and other authors have developed very interesting theories.

But even these theories relapse into the difficulties they intend to avoid.[12] Here I will just give one counterexample to Lewis's theory. My main intention is to sketch an introductory explanation of why I believe that the causalist program is a typical degenerative scientific research program, in Lakatos's sense. Its protective belt is a factory which produces theories and modifications of theories, each time more complex and sophisticated. Its only end is to protect the hard core from the counterexamples.

I will first sketch Lewis's theory of causation. Then, I will try to answer some questions about rationality, objectivity and progress in connection with the causalist and acausalist research programs.

## 2. Lewis's Analysis of Causation – A Counterexample

Let us assume that two events, $a$ and $b$, occur. We will express that as: '$O(a)$'; '$O(b)$'. Lewis says that *a causes b* or that *a is a cause of b* if (but not only if) the following counterfactual is true: '*If a had not happened then b would not have happened*'. The counterfactual is expressed symbolically as: '$\sim O(a) \Box\!\!\rightarrow \sim O(b)$'. This is (for Lewis) a sufficient condition for causation; but not a necessary one. A necessary and sufficient condition is the following: $a$ is a cause of $b$ if and only if there is a causal chain between $a$ and $b$ (even if $\sim O(a) \Box\!\!\rightarrow \sim O(b)$ is false). That is, if and only if all links $\sim O(a) \Box\!\!\rightarrow \sim O(d_1)$; $\sim O(d_1) \Box\!\!\rightarrow \sim O(d_2)$; ...; $\sim O(d_n) \Box\!\!\rightarrow \sim O(b)$, are true.

[9] See [8] and [9].

[10] See [15].

[11] See [12], chapter VII.

[12] See [2].

Let us look at the following example:[13]

Let $P_r$ be the event corresponding to the fact that the atmospheric pressure is $P_r$. Let $p$ be the event corresponding to the fact that the barometer indicates $P$. And let $b$ be the event corresponding to the fact that the barometer is functioning well. I assume that $O(P_r)$, $O(p)$ and $O(b)$ are true propositions (in the actual world). Of course, it is implicit that I am referring to a time $t$, a place $x$ and a certain barometer. Just for simplicity, and because it does not change the argument, I will suppose that there are only two possible atmospheric pressures in the actual world: $P_r$ and $P'_r$ and that there are only two possible indications of the barometer: $P$ (correct for $P_r$) and $P'$ (correct for $P'_r$). Then, $\sim O(P_r) \equiv O(P'_r)$ and $\sim O(p) \equiv O(p')$. Also, $\sim O(b) \equiv O(b')$, where $b'$ is the event corresponding to the fact that the barometer is out of order.

The counterfactual

(1) $\sim O(P_r) \mathrel{\square\!\!\rightarrow} \sim O(p)$, that is: $O(P'_r) \mathrel{\square\!\!\rightarrow} O(p')$ is true;

because, according to Lewis's analysis of counterfactuals,[14] in any possible world where $\sim O(P_r)$ holds (that is, in any $O(P'_r)$-world) sufficiently similar to the actual world (in the actual world, $O(P'_r)$ is false), the barometer will indicate $p'$. That is to say that in all close enough worlds (to the actual world) where the antecedent $O(P'_r)$ holds, also the consequent $O(p')$ holds. According to Lewis's analysis of counterfactuals, all this means that (1) is true. And this is in agreement with intuition.

Therefore, according to Lewis's analysis of causation, $P_r$ causes $P$. And this is also in perfect accordance with intuition.

Up to this point the analysis works. But, how does Lewis impose asymmetry on the relation? His method consists in den-

[13] Taken, with some changes, from [8], pp. 188 (last paragraph) and 189 (first paragraph and note 10).

[14] See [7], [8] and [9]. Lewis introduces some changes to his analysis in [10]; but I will not deal with them, because they do not modify my conclusions.

ying counterfactual reversibility for standard contexts.[15] (When I mention counterfactual irreversibility, I mean the same as the *asymmetry* of the causation relation: *case*-irreversibility for the large number of (purported) irreversible examples.) Mention of *standard* contexts takes into account the fact that counterfactuals —following Lewis— are intrinsically vague. Such vagueness is related to the over-all comparative similarity relation among possible worlds. I will glance at the previous example in order to clarify the idea.

Let me state the counterfactual which is the inverse of (1):

(2) $\sim O(p) \Box\!\!\rightarrow \sim O(P_r)$, that is: $O(p') \Box\!\!\rightarrow O(P'_r)$:

If the barometer had not indicated $P$, then the atmospheric pressure would not have been $P_r$.

Lewis says that (2) is false in standard context[16] (the most usual, the strongest one). In that context we consider that the atmospheric pressure is —in the $O(P')$-possible worlds close enough to the actual world ('*a.w.*', from now on)— the same as the actual atmospheric pressure $P_r$. He says that it is easier to blame the broken barometer (and not to blame a different atmospheric pressure) for its divergent indication. In other words, he says that

(3) $\sim O(p) \Box\!\!\rightarrow \sim O(b)$, that is: $O(p') \Box\!\!\rightarrow O(b')$,

is true in standard context: If the barometer had not indicated $P$, then it would have been out of order. Lewis remarks that we are able to accept (in a special context) the truth of (2) and the falsehood of (3), by means of something similar to a *gestalt switch* of intuition. It would be a different way of ordering worlds by means of a different apprehension of the similarity relation among them. In such an ordering, all worlds close enough

[15] See [9]. See also [8], pp. 188–191.

[16] Lewis does not use the term 'standard' in this example (see note 13) because he introduces that word in a later paper: [9], pp. 455–458. But is precisely the same notion. See [8], note 10.

(to *a.w.*) where $\sim O(p)$ holds are worlds where the barometer functions well. We have changed certain premises, cotenable[17] with the antecedent: We had $O(P_r)$ before. Now we have $O(b)$. Both premises cannot be cotenable with the antecedent at the same time.

That was a typical example of the intrinsic vagueness of a counterfactual. Lewis takes as standard the context for which (2) is false and (3) is true, because he says that it is usual to consider that the breakage of the barometer involves a lesser departure from *a.w.* (where the barometer functions well) than the change in the atmospheric pressure.

The validity of Lewis's general (noncontextual) theory of counterfactuals is not involved in the decision about the identification of the standard context. On the other hand, his theory of causation is involved; because Lewis denies in most cases the possibility of reversibility of counterfactuals in standard context, with the desired asymmetric result for causation relation.

If he had to accept the reversibility of (1): that (2): $\sim O(p) \; \Box\!\!\rightarrow \sim O(P_r)$ is true in standard context, it would result that $p$ is the cause of $P_r$: an absolutely unacceptable result which could not derive from a good explicatory analysis of the causation relation. As a matter of fact, I think that it may be shown, within the general framework of Lewis's counterfactual analysis, that (2) is true in its own standard context. I am working on that idea.[18]

But even if we accept Lewis's claim, that (1) is irreversible in the standard context and that, consequently, (2) is false in such a context, his theory of causation is not safe. We must recall that Lewis says that (3): $\sim O(p) \Box\!\!\rightarrow \sim O(b)$ (if the barometer had not indicated $P$, then it would have been out of

[17] See [7], chapter 2, p. 57 and chapter 3, pp. 68–70. Lewis says that $A$ is cotenable with the antecedent $E$ iff: (i) $E \Box\!\!\rightarrow A$ and (ii) $A$ is true in the actual world. Or (stronger definition) iff: $A$ holds in all possible worlds as close as the close enough worlds where $E$ holds and in all closer ones. Or $A$ holds in all possible worlds. For our example, either of the first two definitions works.

[18] See [3bis].

order) is true in the standard context.[19] But accepting that, we conclude (following Lewis's theory of causation) that $p$ causes $b$. And this is absolutelly unacceptable. We can accept that the event corresponding to the fact that the barometer indicates $P$ is a sign of its well-functioning. But we would never accept that it is a cause of its well-functioning.

I will now try to hint how it could be shown (against Lewis's opinion) that (1) is reversible: that (2) is true in its own standard context.[20] If I succeed, the result will be that $p$ causes $P_r$, an absolutely unacceptable conclusion. We would never accept that the barometer's indicating $P$ is a cause of the atmospheric pressure being $P_r$. This conclusion would be another counterexample to Lewis's theory of event causation.

We begin by looking at counterfactual (3): $O(p') \boxdot\!\!\rightarrow O(b')$. It is true in its own standard context, because the close enough (to *a.w.*) $O(p')$-worlds are $O(P_r)$-worlds where $O(b')$ holds. In that context, counterfactual

$$(4) \sim O(p) \boxdot\!\!\rightarrow O(P_r), \qquad \text{that is: } O(p') \boxdot\!\!\rightarrow O(P_r),$$

is also true, because, as we have just seen, the close enough (to *a.w.*) $O(p')$-worlds are $O(P_r)$-worlds.

Nevertheless, we would never accept that (4) is true in its own standard context. We would never accept (in standard context) that: If the barometer had not indicated $P$, then (anyway) the atmospheric pressure would have been $P_r$.

There is a serious problem here for Lewis's interpretation of his own analysis of standard contexts. I think that the interpretation can be modified without changing the basic frame of his notion of a standard context. We must accept that (3) and (4) have different standard contexts, even if they share the same antecedent $\sim O(p)$ or $O(p')$.

Consequently, even if (4) is true in the standard context of

[19] See note 16.

[20] See [3bis].

(3), that standard context of (3) is not the standard context of (4). It is a non-standard (or special) context of (4). On the other hand, (4) is false in its own standard context (in perfect agreement with intuition). But the standard context of (4) is also clearly the standard context of (2). In that context, the close enough (to *a.w.*) $O(p')$-worlds are $O(b)$-worlds. We conclude that it may now be easily accepted that (2) is true in its own standard context, which is not the standard context of (3), but which is indeed the standard context of (1).

The immediate conclusion is the reversibility of (1), with the absurd consequence that, following Lewis's analysis of event causation, $p$ causes $P_r$.

## 3. My Proposal

### A. *The Argument*

I intend to sketch the possibility of opening a line of research which might be able to show that counterfactual *possible worlds* analyses of event causation fall into the same difficulties as the previous ones. And we might be able to conclude that the attempts to explicate the notion of causation in a scientific context, belong to a degenerative research program, in Lakatos's sense, or belong to a paradigm, in Kuhn's sense, with persistent anomalies, which compel us to leave it and to replace it by another one, which uses only nomological relations.

First of all, I point out the existence of a relation: the *production relation* ($p$ relation), as an essential component of the meaning of the notion of causation. The cause *produces* the effect, or it is a *producing factor* of the effect. The other essential component is the *nomologic connection* between cause and effect, which derives from the natural laws and from the initial conditions and other particular circumstances involved.

(I mean by '*essential* component of the meaning of a notion' a mostly pragmatic idea: a component of the meaning in everyday language, whose *adherence* to it —to the meaning— is

such that we do not want to lose it during the explicatory process which is carried out to obtain the technical meaning or explication of the expression.)

Secondly I note that the *p* relation, which I will call 'the original *p* relation', does *not*, in the first instance, correlate *events* to each other; on the contrary, at least the first argument of the relation is an individual,[21] not an event: the painter *produces* the picture. Consequently, we may say that the relation *p*, in the first instance, correlates *individuals* with their products (individuals, events, things). On the other hand, the nomological connection correlates events with each other. Also the relation of causation (at least the type of causation relation we are discussing here) is a relation between events: the gun shot (event) caused Peter's death (event). Note that we would never say that the painter *caused* the picture; because the first argument is an individual, not an event.

How, then, is it possible for the original *p* relation to be a component of the meaning of the causation relation? How is it then possible to say that the gun shot (event, not individual) *produced* Peter's death?

My answer is that the causation relation *anthropomorphizes* the event which constitutes its first argument. It turns into a hybrid: an *event-individual*. Therefore, when we look at it as a component of the meaning of the causation relation, the *p* relation is not the *original p relation* (whose first argument is an individual). It is an *expanded p relation* (expanded into events), such that it preserves an umbilical cord which relates it with the original *p* relation, because the event in the first argument is actually an event-individual.

The characterization just sketched produces no serious damage in the context of everyday language. But if we want to develop an explicatory analysis of the notion of event causation in scientific contexts, we must delete or avoid any trace of ant-

[21] By 'individual' I mean the anthropomorphic sense of the word.

hropomorphism in its technical meaning. And the big problem is that any such *disanthropomorphization* of that notion eliminates not only the original *p* relation, but also the expanded *p* relation, because the events-individuals must turn decently into humble and simple events.

It is at this point that the explicatory analysis of causation in scientific contexts fails: *it is indeed the p relation which generates the asymmetry of the causation relation.* When I mention the asymmetry of the *p* relation, mean the same as for the corresponding notion of the causation relation (asymmetry *case* for the big set of asymmetric examples —perhaps all, as I believe— of the *p* relation). So it is the essential presence of the *p* relation in the meaning of the notion of causation which secures the asymmetry of this last one.

When *p* is eliminated during the explicatory process, the possibility of *symmetrization* is introduced in the causation relation. And we cannot retain in the causation relation a *p* relation purified of anthropomorphism, because anthropomorphism is essential to it, as we have seen above: it is indeed the condition of an individual, or —better— of an event-individual, of the first argument, which confers on it the notion of a producer.

*What is the response of the philosophers who want to explicate causation at any cost, when they face such difficulties? Any attempt to solve the problem turns into an attempt to find* surrogate *relation, substitutes of p, free from any type of anthropomorphism and which would be able to give back to the causation relation the lost asymmetry.*

*An important part of the research program I am propounding, consists in working towards being able to find what those surrogate relations are, and towards showing the difficulties concerning each case.*[22]

[22] See [2].

B. *The Methodology*

I will now sketch the methodology I propose in order to show, for each particular theory of causation, that the surrogate relation which substitutes *p* cannot really replace it. Examples of surrogate relations presented explicitly or implicitly by different theories are: counterfactual dependence, the relation of overdetermination, the temporal precedence relation, necessary condition, sufficient condition, different combinations of the last two, etc., etc.

Just because the surrogate relation is not identical with the *p* relation, the set of ordered pairs of arguments which satisfy the first will not, in general, be the same as the set of ordered pairs which satisfy the second. Therefore, there will be non-overlapping instances of the expanded *p* relation and the surrogate relation of the corresponding theory. There (in those non-overlapping instances) we must look for the counterexamples.

It is very interesting to observe that in such cases (at least, in all cases I have examined), the expanded *p* relation is favored in relation with the surrogate relation: the expanded *p* relation overlaps completely, even for those special instances, with the causation relation, while the surrogate relation overlaps, neither with the expanded *p* relation nor with the relation of causation in those instances. Therefore, those examples are counterexamples for the corresponding theories of causation.[23]

We can repeat the same methodology in those cases in which there could be a complete overlap between the expanded *p* relation and the surrogate relation of the corresponding theory. This could happen in cases in which both relations differ only intensionally, but not extensionally. I will now try to explain the basic idea.

The essential components of the meaning of a general notion must hold, not only in the actual world, but also in any

[23] See [2].

sufficiently similar or *accessible* possible world (I will explain below the meaning of such *ad hoc* notion of accessibility). This may not hold for very specific notions; but it is always valid for general notions, something like metatheoric or metaparadigmatic notions, such as the notions of explanation, explication, natural law, causation, etc. This is necessary, in order to be able to speak with coherence about different possible worlds, even if they are incommensurable in the sense given by Kuhn or Feyerabend. The notion of causation exists in the different scientific theories throughout history, hardly depending on the changes of the theories themselves. Or, at least, we can study the old theories, the present accepted theories and those which are not unanimously accepted, using always our present notion of causation.

In order to discuss this notion, every world which could have been reasonably accepted as the actual world, will be considered as an accessible world, even if it finally turns out to be different from the actual world. For instance: the Newtonian universe.

Although we *do not* know the *exact* natural laws and concrete circumstances of the actual world, we *do* know a very large variety of worlds which we *cannot* reasonably consider as candidates for being the actual world. The remaining possible worlds are those which we consider as accessible, without excluding those whose research program (in Lakatos's sense) has degenerated.

Even if the surrogate relation coincided extensionally with the expanded *p* relation in the actual world, it would not coincide intensionally with it (because they are not the same relation). So there must be accessible possible worlds, where they do not coincide extensionally. In that case, we may look there for counterexamples: *those cases where the expanded p relation, and not the surrogated one, coincides with the causation relation.*

Let us suppose, for example, that we work accepting the fo-

llowing classical assumption: that the basic laws of nature are reversible with respect to time. In such a world, which we now know is probable not ours, the production relation *is not* extensionally identical to the overdetermination relation. For instance, we could look at cases where the expanded *p* relation and the causation relation hold, but where the overdetermination relation does not hold. Such a result destroys the overdetermination relation, seen as an intended surrogate for *p*. But it destroys it not only in the classical world of reversible processes, but also in the contemporary world of irreversible processes, in spite of the fact that in this last one, the example is not valid (because in the world of irreversible processes the expanded production relation and the overdetermination relation coincide extensionally).

The reason for this is that we can distinguish clearly in the reversible world the conceptual (intensional) difference and the extensional difference between the two types of relation. And it becomes clear that the expanded *p* relation and the causation relation are valid in cases where the other relation does not hold.

I think that the expanded *p* relation is tied to the causation relation in every accessible possible world. On the other hand, the surrogate may coincide extensionally with causation in some possible worlds; but not in all of them.

Just the same happens with the idea of the physical possibility of causes which are subsequent to their effects. We do not know if this really happens in the actual world. But it is a genuine hypothesis. The expanded *p* relation is tied to the causation relation in such a possible world (perhaps the actual world) and in any other accessible possible world. On the other hand, the temporal precedence relation does not coincide with the causation relation in the examples of backward causation.

Therefore, we can find cases in such a world —which may or may not be the actual world— where the relation of causation and the expanded *p* relation hold, while the temporal prece-

dence relation does not hold (either it is reversed or the simultaneity relation holds).[24] We are thus compelled to reject the temporal precedence relation as an essential component of the meaning of causation, even if in another possible world (perhaps the actual world) they could be tied together. I think that we can repeat the same idea for any surrogate relation proposed up to now.

The whole argument is related to the fact that the *present* meaning of the notion of causation must not change when we pass from one accessible possible world to another one. (This is not opposed to the idea that in different historical periods the meaning of that notion could have been different.) We can say that if a characteristic is essential to the meaning of a notion, it cannot disappear in other possible (accessible) worlds. I think that the expanded *p* relation is a characteristic of causation in all possible (accessible) worlds, because it seems to be an essential component of the meaning of causation. On the other hand, the surrogate relations may or may not seem to be tied to causation, according to the world analyzed in each case. Therefore, if that were the case for all surrogated relations, we would be able to state with certainty that they are not essential components of the meaning of causation.

### C. *Applied Methodology*

The surrogate relation (tacitly) chosen by Lewis is the relation of counterfactual dependence (to state its asymmetry is equivalent to stating counterfactual irreversibility). To apply to this case the methodology sketched above, is to show that that relation is not asymmetric in simple and clear cases where causation is undoubtedly asymmetric.

On the other hand, Lewis considers that such counterfactual irreversibility is explained by the asymmetry of another (surro-

[24] See [2], Introduction and chapter IV. See [3], pp. 46–50.

gate) relation: that of overdetermination.[25] If that consideration were correct the previously discussed strategy would be sufficient to reject overdetermination also (as a successful surrogate relation). It would be interesting to apply the methodology directly to this case, finding the corresponding counterexamples, because it could be wrong to state that asymmetry of counterfactual dependence is explained by asymmetry of overdetermination. In fact, I think that it is wrong.[26] It is not difficult to find direct counterexamples for overdetermination.[27]

Swain propounds an alternative counterfactual theory of event causation. It is not easy to discover which surrogate relation is implicit in his theory. I think that it is the same overdetermination relation suggested by Lewis.[28]

Of course, I do not exclude the possibility of the existence of a new surrogate relation (not yet discovered) which would succeed: such are surrogate relations (and not the extended $p$) would coincide with the relation of causation in any possible accessible world. In such case my thesis would prove to be wrong. But not one of the surrogate relations which I find implicit in the different theories proves to be successful. And I do not believe that such successful surrogate relations exist. The anomaly of the persistence without any solution of the problem of asymmetry points in that direction. The causalist research program, in the scientific context, is refuted in each of the particular theories proposed; which are replaced by new theories, which are refuted once more. It is always possible to find valid counterexamples. So the support of the hard core of the causalist program becomes fruitless. There is no progress. We relapse always in the same difficulties. It seems wiser to develop a new

[25] See [9], pp. 472–475.

[26] See [2], chapter V.

[27] See [2], chapters VIII and IX.

[28] See [2], chapters VII and VIII.

acausalist program, working only with nomological connections between events.

We have an alternative program: the nomologic acausalist program. Such a program is open to all oportunities of proving its fruitfulness. There is an enormous research field waiting for researchers. The concept of natural law, its relation, if any, with the counterfactual analysis, its demarcation from accidental generalizations, are not resolved problems. On the other hand, the causalist program shakes, but it does not progress.

## 4. Rationality, Objectivity and Progress: Causalist and Acausalist Programs

It is said that the *rational* attitude of a scientific researcher and of a philosopher of science must be to give up a degenerative program after a prudential testing time, and to adopt a fresher and more fruitful program. That would be a sign of *progress.*

*Is it possible that the ancestral anthropomorphic weight of the notion of cause-effect could be so strong a restraining force, sufficiently powerful to be able to waste so much time and courage?*

*Or are we perhaps on the verge of finding the correct explication of the notion of causation, explication which will once and for all put an end to the dance of counterexamples?*

*Is there any other rational way of deciding?*

If we follow Popper's ideas and we start striking at all causation theories, all of them become rejected just newborn, because, as we have seen, we are not short of counterexamples. Is it correct to abandon the causalist boat? Could the case not be, perhaps, that a good analysis appear after so many wrong ones? If we answer affirmatively to the last question, it would seem that we must go on trying. Nevertheless, if we divide the field, separating the causalist theories on one side and the acausalist program on the other side, we can see that each successful stroke against a causalist analysis is a corroboration of the acausalist thesis. Therefore, even if there is no sense in

confirming acausalism inductively, it seems that its degree of corroboration is high.

From the Lakatosian point of view we have already seen that the circumstances are the same.

And if we state the problem from a Kuhnian view, we can look at it in the following way: so long as the causalist paradigm faces severe and persistent anomalies, and provided that there is another paradigm which, even if it does not solve, does dissolve such problems, how is it possible that there has been no conversion from the first paradigm to the second? We can find also in Kuhn a notion of rationality and of progress.

I have no sure answer to that question. But I will state below some conjectures. I think that perhaps in the time of Russell's attack (1912–13) or of Carnap's proposal (1966), there was not sufficient work done with the causalist research program. But I believe that now that work has been done. I do not see reasons to delay any more our joining the program proposed by Russell and Carnap.

Let us see if the circumstances I have presented, about the difficulties of the causalist program, are comparable with the persistent difficulty endured by the Newtonian program, with regard to the movement of Mercury's perihelion. In this last case too, there was a very big resistance to giving up the accepted paradigm.

I think that the situations are not comparable. The Newtonian program was —in spite of the persistent mentioned difficulty— a very fruitful one, with an important positive heuristic and enormous predictive success. There was no reason to doubt that a solution to the Mercury perihelion problem was going to be found *within* the Newtonian program. The resistance was justified. Furthermore, up to the moment in which General Relativity appeared, there was no serious alternative which could manage to solve or dissolve the problem.

In our case we have, at least from 1912–13, the possibility of an alternative program. But, on the other hand, the old cau-

salist program has shown no positive heuristic. When I say: 'It shakes, but it does not progress', I mean that only the negative heuristic works. There is no fruitfulness. There is no predictability.

To finish sketching the problem, let me present a mythological metaphor, which is not only anthropomorphic, but also theomorphic. The Hindu god Shiva is very often represented fighting with a demon. He pierces the demon, brandishing a spear with one of his many arms. But each drop of the demon's blood which reaches the floor, generates a new demon. Shiva, using another of his arms, supports a vessel with his hand under the demon's wound, in order to receive the blood, avoinding its reaching the floor.

The demon represents the protective belt of the causalist program. Shiva's lancer arm is Popper, the Exterminator. We only lack somebody to put the vessel in the correct place.

I'll try finally to answer the question.

I think there is no fixed rational criterion to decide when to give up a research program and admit another one. It does not seem profitable to use the notion of rationality in order to fix rules to be followed by the scientific community. Fashion, interest, ancestral influences, are often factors which decide the course of action to follow. It is certain that the *objective* way of progress consists in choosing the program which, on the one hand, offers the best possibilities of fruitfulness and, on the other hand, is able to solve the problems already solved by the previous programs and also those not solved before.

But it is impossible to know beforehand what is going to happen with a new program. No research program which deserves its name is able to preview with certainty its own success or failure.

Only History will be able to judge retrospectivelly which was the correct way: the way followed by those who continued fighting within the old paradigm or by those who converted to the new one. But is it correct to say that the rational way to follow

was the one pointed out by History? To judge rationality retrospectivelly seems nonsense.

Perhaps there is no other way out but to accept the rationality of both ways, provided they are followed with good sense. Within the acausalist program, which I adopt, we consider that the persistence and stubborness in working with a causalist degenerative program is due to the fact that the ancestral anthropomorphical weight, a *non-rational* consideration, is such a powerful restraining force, that it precludes the possibility of seeing the sterility of the program. It is not seen however as *irrational*, as against reason, but as influenced by non-rational motives.

On the other hand, if we make a gestalt switch, and convert to the other side, we will consider that there has been much progress in the search for an explication of the notion of causation and that, even if the research has not yet reached the point where the analysis becomes free from counterexamples, the way leading to that goal is open. Therefore, it does not seem rational to give up the search and to turn to a poorer program, as the acausalist one is, when we are just about to reach our goal.

We see that, even if we accept the rationality of both ways, such acceptance is not simultaneous. It depends on the gestaltic switch. It depends on the frame of reference from which we argue. If we argue from the causalist view, the acausalist program is non-rational. And if we argue from the acausalist view, then the causalist program is non-rational and the acausalist one is rational. It would seem that *rationality* and *non-rationality* are relative to the frame of reference. But that is not the case for *irrationality*. For it, the rule would be much more absolute.

Therefore, History will not judge about rationality, but about which was the program which turned out *objectively* to be the best one: that is, which solved more problems and was more fruitful. History will give us a criterion of *objectivity* and of *pro-*

*gress* with respect to the change of research program. But the criterion of *rationality* or *non-rationality vs. irrationality* must and can be fixed in the present. Also, the criterion of *rationality vs. non-rationality* is relative to the conceptual frame, and must and can be fixed as well in the present, without any help from History.

My conjectures related to the question at the heading of this paper may be stated in the following way: *The causalist research program does not involve an irrational persistence. But it does involve a non-rational persistence, in relation to the acausalist conceptual frame, which I favor. Nevertheless, I accept the rationality of the causalist program from the point of view of those who favor it. History will not decide about the rationality or non-rationality of such program; but it will decide about the final and objective result of it, with respect to its accelerating or restraining power for the progress of scientific research.*

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* 1. The first date shown in each item is the date of the first edition in which the reference material is included.

2. The sign '(*)' shows the consulted edition. The page numbers pointed out in the notes correspond to those editions.

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RESUMEN

El análisis de las conexiones nomológicas se confunde a menudo con el de la causación. Mi propuesta, que se basa en las ideas de Bertrand Russell, plantea tratar el primer problema (programa acausalista) en contextos científicos, y rechazar el viejo programa causalista, viciado de imprecisión y antropomorfismo. Intento explicar por qué aun los análisis más sofisticados de la causación recaen en las dificultades que pretenden eludir (por ejemplo, en el problema de la asimetría). Mi intención principal es tratar de explicar por qué creo que el causalista es un típico programa degenerativo, en el sentido de Lakatos. También intento contestar ciertas preguntas acerca de *la racionalidad, la objetividad y el progreso, en relación con los programas causalista y acausalista. Línea argumental*: 1. La relación *p* (de producción) y la conexión nomológica son componentes esenciales de la causación. 2. Pero *p* tiene componentes esenciales irreductiblemente antropomórficos. Y *p* introduce la asimetría en la causación. 3. Señalo que ningún análisis de la causación que prescinda de *p* ha podido resolver problemas como el de la asimetría. 4. Y que *p* se resiste a cualquier análisis que pretenda eliminar su antropomorfismo. 5. Al eliminarse *p*, ésta debe ser reemplazada por relaciones sucedáneas asimétricas que no son esenciales a la causación. 6. Por lo tanto, siempre podemos encontrar contraejemplos en los que se da la relación sucedánea y no la causación (ni *p*) y viceversa.