BOZZE / PROOFS:

Prof. Mario Alai
Completare indirizzo

Mario Alai

THE "NO MIRACLES" JUSTIFICATION OF INDUCTION

There is a striking contrast between the unquestioned confidence we have in inductive inferences both in science and in everyday life, and the apparently unquestionable thesis advanced by Hume and many other philosophers that, short of circularity, we cannot provide even the slightest justification for induction: past inductive inferences mostly proved correct, but from this one could conclude that future inductive inferences will also (probably) prove correct only by an inductive inference, hence circularly. Alternatively, inductive inferences could be justified by adding as a premise the Principle of Uniformity of Nature; but this is a synthetic universal statement, and as such it needs to be established by induction, thus involving circularity again.

It has been suggested[1] that in fact, assuming the uniformity of nature would even turn inductive inferences into deductively valid inferences. For example, suppose we observed that a member of kind A has the property B, or perhaps that an event of type A is followed by an event of type B, or that a property A is a certain function f of property B: henceforth, I will schematize any of these cases as

(1) $A = fB.$

[1] For instance, by Musgrave ([1993], ch. 9, § 1).

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Further, suppose that (1) held in all instances we observed up to now: symbolically,

(2) $\forall oi(A = fB)$

(where '*oi*' stands for 'observed instances'). If nature is uniform, this would seem to imply that the same will happen in *all* instances, observed or not: in symbols

(3) $\forall i(A = fB)$

(where '*i*' stands for 'instances'). But this could not be a strictly speaking deductive inference[2], unless one assumed the Principle of Uniformity of Nature in an implausibly strong form, such as, e.g.,

(4) Nature is uniform under *all* respects.

Otherwise, one might assume that

(5) Whenever a regularity has been observed, this regularity will hold in all instances

(in symbols,

(5′) $\forall \mathcal{A}, \mathcal{B}, f\,[\forall oi(\mathcal{A} = f\mathcal{B}) \supset \forall i(\mathcal{A} = f\mathcal{B})]$,

where '$\mathcal{A}$', '$\mathcal{B}$', 'f' vary over types of individuals, properties, events, and functions). But assuming (5) would be just the question-begging assumption of the Principle of Induction:

(6) All inductive inferences are justified.

Setting aside the attempt to turn induction into deduction, one might assume some less committing form of the Principle of Uniformity of Nature, such as

[2] See Wright [2004], p. 170. This was brought to my attention by Giorgio Volpe.

(7) Nature is uniform,

or

(8) There are universal regularities in nature,

or

(9) Some events, properties or individuals will be always connected in the same way to given events or properties

(symbolically,

(9′) $\exists \mathcal{A}, \mathcal{B}, f\,[\forall i(\mathcal{A} = f\mathcal{B})]$.

But then, as I said, these are still universal synthetic statements, which apparently cannot be established except by induction. So, there seems to be no non-circular justification of induction.

It has been claimed[3] that the circularity involved here is not vicious, since the conclusion that induction is reliable is reached by using induction as a rule of inference (rule-circularity), not by assuming the reliability of induction as a premise (premise-circularity): so, the conclusion is not assumed among the premises. But if it is not *logically* vicious, it is *epistemically* vicious, for we don't need the conclusion for its own sake, but in order to warrant our use of induction as a rule; so, we are not entitled to use it as a rule already within the argument[4].

In particular, the most relevant inductive inferences, those to universal statements (like scientific laws), not only seem to lack justification, but might appear most likely incorrect: if we think of the probability of such generalizations as the ratio of a series of invariantly positive past cases to the infinite possible cases, they must al-

[3] By Braithwaite ([1953], pp. 274-278), Carnap [1952], Papineau [1992].

[4] One might suggest (as Enzo Fano did in conversation) that some version of the Principle of Uniformity of Nature can be arrived at *abductively*. In fact, this is going to be my strategy as well; but first, it is necessary to clear the way by appreciating that *inductive* justifications of induction are unsuccessful strategies.

ways get zero probability. Of course, one could give up universal generalizations, and be content with an inference to the next case. But this would be giving up scientific laws in the proper sense. Again, it may be observed that the most basic and general laws, as Newton's laws of motion, or Maxwell's equations, are not arrived at by induction from particular instances, but through "a creative act of an intellect which 'sees' beyond the empirical data"[5], or by *abduction*, a backwards reasoning from a wide body of phenomena and phenomenological laws to an underlying explanation, cause or common principle[6]. As it will turn out, I am frankly sympathetic with this observation, for I shall call on abduction even for a justification of genuinely inductive inferences; yet, we must ask: what about phenomenological laws, like the second law of thermodynamics prior to the development of statistical mechanics, Ohm's law, etc.? They are clearly generalizations from particular observable phenomena, so, how can we justify not just having a *high* confidence in them, but even having *some* confidence at all? Of course, it is not in question here that induction has the decisive role in the "context" of the "discovery" of such laws, but whether it can also play that role in the "context" of their "justification". For, if induction cannot, what else could?

From the actual success of induction, Hilary Kornblith ([1993]) has argued to the reality of natural kinds, understood as clusters of properties permanently coexisting in the same substance: in other words, to the existence of objective uniformities in nature. Howard Sankey ([2008], pp. 79-87) takes this cue, but he supplements it with Brian Ellis' account of natural laws as grounded in natural kinds, and above all, he reverses the sense of Kornblith' inference: taking for granted a Principle of Uniformity of Nature couched in Kornblith's and Ellis' terms, from it he argues to the justification of induction. This move may be considered unsatisfactory, since the uniformity of nature is at least as dubious as induction, and it would

[5] Agazzi [2006], p. 86. Enzo Fano also called my attention to this point.

[6] Concerning abduction see Peirce [1931-58], Vol. 5, p. 189: "The surprising fact, *C*, is observed. But if *A* were true, *C* would be a matter of course. Hence, there is reason to suspect that *A* is true"; Hanson [1958], ch. IV.

seem to require induction for its own justification. Now, I submit, the problem can be overcome by chaining Kornblith's and Sankey's respective arguments into a two stages argument: in the first stage the uniformity of nature is established abductively, as the best explanation of the past success of induction, and in the second stage induction is justified by showing that it is warranted by the uniformity of nature. Although this idea is not new, having been proposed in different versions by Armstrong ([1983], [1995]), Foster ([1983]), and BonJour ([1998], ch. 7), I wish to elaborate on it to some extent[7].

When looking at the infinite series of possible cases of a generalization, impressed by the probability rapidly approaching zero, we may forget about another series, finite but equally significant: that of past invariantly positive cases. Suppose we observed the phenomenon $A = \pounds B$ always happening in the same way up to the present time t (i.e., suppose '$\forall oi(A = \pounds B)$' is true at t). If so, either

(a) this was a pure coincidence,

or

(b) there is a reason why this should happen: either

(b_1) an underlying mechanism, or simply

(b_2) a permanent disposition of nature to produce *A* as an effect of *B* (or a function of *B*, or endowed with *B*).

But the longer the positive series, the unlikelier that it is a mere coincidence. When the series gets long enough (as in the case of scientific laws), a coincidence becomes so unlikely to resemble a miracle. But it is reasonable to discard extremely unlikely events, just as it is methodologically correct to discard a third conceivable alternative, i.e. that the observed regularity was literally produced as a miracle. So, (b) is left as the best explanation, in fact the only reasonable one; hence, (b) must be the case[8]. Now, both in sub-case (b_1) and

[7] Harman, instead, has claimed that inductive inferences actually *are* inferences to the best explanations: see Harman ([1965]; [1967]; [1968]).

[8] This "no miracles" version of the inference to the best explanation has become

(b_2), the universal generalization '$\forall i(A = \pounds B)$' (claiming that $A = \pounds B$ will always be the case) is warranted: this is what accounts for our commonsensical (and highly successful) confidence in induction.

One might object: why is our explanation (b), i.e. the principle of the Uniformity of Nature, the best explanation of the uniformities we observe, as opposed to explanation (a), i.e. pure chance? I claimed that an invariantly uniform series of events produced by pure chance is too unlikely to make for a viable explanation. But, the objector might insist, why is it so unlikely? BonJour, the latest proponent of an abductive justification of induction, answers that although it is possible that one or many such chance regularities occur, possible worlds in which they happen ("counter-inductive worlds") are extremely rare within the total class of possible worlds ([1998], pp. 208-209). To this James Beebe replies that since the two classes of counter-inductive worlds and of total possible worlds are infinite, the only possible way to compare them is by speaking in terms of relative frequencies, but these can only be defined through ordered sequences of worlds; now, it seems that BonJour could not appeal to any privileged ordering, except a random sequence (Beebe [2008], pp. 161-162). However, Beebe does not see "why should the limiting frequency of counter-inductive worlds in every random sequence of worlds be less than the limiting frequency of normal worlds" (p. 163). This is another way to ask: why is it extremely unlikely that the great number of phenomenal uniformities of nature happen by mere chance?

It may be answered that, for all we know, we should deem as extremely improbable that in a purely chance world there occur exceptionless regularities as we actually observe (i.e., that so many generalizations of the form '$\forall oi(A = \pounds B)$' are true). A chance world is one where each member of a set of mutually exclusive and collectively exhaustive possible atomic events happens with an equal limiting frequency. So, if such a set were {$A = \pounds B$, not($A = \pounds B$)} (e.g., {water boils at 100° at 1 atm., water *does not* boil at 100° at 1 atm.}, the limiting frequency of possible chance worlds with a straight se-

popular since it was employed in defence of scientific realism in Smart ([1968], p. 150), and Putnam ([1975], p. 73).

ries of n cases of $A = fB$ would be $1/2^n - 1$, i.e. very low, and getting lower as the observed regularity gets longer. In fact, this frequency would be even much lower, since in general 'not($A = fB$)' does not describe an atomic event, but a complex one: there is not one, but infinite possible atomic events which falsify '$A = fB$' (viz., $A = fC$, $A = fD$, … etc., where C, D, etc. are mutually exclusive with B). For instance, there are thousands of possible atomic events in water would not boil at 100° at 1 atm.: if it boiled at 99°, if it boiled at 98°, etc; or if it boiled at 101°, at 102°, etc. (where each figure includes any decimal fraction below the next integer, to account for the limits of instrumental precision, actual observational fluctuations, etc.). So, the relative frequency of possible chance worlds in which at any given occasion water boils at 100° is $1/m$, where m is at least in the order of thousands, and the limiting frequency of those in which it does so n times in a row is $1/m^n - 1$, i.e., rapidly approaching zero. In fact, in the actual world we do not witness just one exceptionless regularity, but a very high number p of them; therefore, the limiting frequency of chance worlds in which this happens is $1/(m^n - 1)^p$, i.e. infinitesimal, and this answers Beebe's question. Hence, the probability that our world is a chance, or "counter-inductive" world are practically null: it is reasonable to assume that we live in an "inductive" world, and that assumption (b) is the best explanation of its regularities.

Admittedly, we face a parametrization problem, here: how should we divide up the stream of happenings in possible atomic events? Or, in the water example, how should we divide up possible boiling temperatures? Still, the probability of an exceptionless regularity in a chance world is going to be very low whichever division we adopt, except for divisions which are *very* weird in the face of all we know[9]. For instance, the probability of always observing water boiling at 100° would be high if the scale of temperatures were divided up as follows: (1): 1°-99°; (2): 100°; (3): 100,001°; (4): 100,002°; …(1001): 100,999°; (1002): 101° or higher. According to this scale, water would boil at 100° in 1000 possible atomic cases, and fail to

[9] As suggested by Magidor ([2003], pp. 13-14, *passim*), background knowledge can yield quite sensible solutions to parametrization problems of this kind.

do so in only two of them. But this is not a "natural" scale, not one that "cuts (our) world at its joints": let us call 'the physical context' of the fact that an object has temperature *t* its causes, effects, and any other relevant circumstance. Well, in almost all known cases, the physical contexts of having a 100,001° temperature are practically undistinguishable from those of having a 100,999° temperature, while there are very significant differences between, say, the physical contexts of having a 1° temperature and those of having a 99° temperature. In other words, 100,001° and 100,999° are very similar from every viewpoint, and may be considered the same event, while being at 1° or at 99° are very different, and should be considered different events.

A different possible objection is that the above explanation (b) need not be true, for an alternative explanation is possible: namely, that

(c) there was an underlying mechanism or a permanent disposition of nature to bring it about that $A = fB$ *up to time t*;

and obviously, this explanation does not licence the universal generalization '$\forall i(A = fB)$'. Replying to this objection requires a short detour. To begin with, the sketchy account just given needs qualifications: very few descriptions as simple as '$A = fB$' are invariantly confirmed *prima facie*: water does not always boil at 100°, only at one atmosphere; pressure is not always a function of temperature, only if the volume is constant; etc. This is often acknowledged by granting that generalizations hold *ceteris paribus,* or *all things being equal*; but again, it is never the case that *all* things are equal from one instance to the other. Hence, what we need is determining, by repeated trials or by background knowledge, which are the relevant circumstances: they are then added to the description of the phenomenon either as necessary conditions, or as further arguments of the function, or both:

$$(10) \qquad (C_1 \;\&\; C_2 \;\&\; \ldots\; C_m) \leftrightarrow (A = fB)$$

(e.g.: If (and only if) pressure is one atmosphere, water boils at

100°);

(11) $A = f(B, D_1, D_2, \ldots, D_m)$

(e.g.: The pressure of gases is proportional to temperature and to the inverse of volume);

(12) $(C_1 \,\&\, C_2 \,\&\, \ldots\, C_m) \leftrightarrow A = f(B, D_1, D_2, \ldots, D_m)$

(e.g.: If (and only if) a gas is an ideal gas, its pressure is proportional to its temperature and to the inverse of its volume). As the symbolism shows, each relevant circumstance C_i is assumed to be a necessary condition, but all of them are assumed to be collectively sufficient. Of course, even such descriptions are in principle open to failure, for occasionally a new relevant circumstance C_n or D_n may be found: but then the new conjunction $(C_1 \,\&\, C_2 \,\&\, \ldots\, C_n)$ is taken to be sufficient and necessary, or the new function $A = f(B, D_1, D_2, \ldots, D_n)$ is taken to be the *actual* function. Occasionally, on may also find that a condition C_i is not actually necessary, for an alternative condition C_j is sufficient; in this case, however, the disjunction $(C_i \vee C_j)$ will take the place of C_i as a necessary conjunct in the left-hand side of biconditionals of form (10) or (12).

In principle, this process of adjustment is open-ended; however, in most cases, (α) once a circumstance C_i or D_i has been found to be relevant in one case, it invariantly remains such in subsequent cases, and (β) *a posteriori* it can be seen to have been relevant even in earlier cases in which it had not been acknowledged as such. For instance, after failing to observe water boiling at 100° we may notice that the only difference with previous cases was a pressure different from one atmosphere, thus understanding that this is a necessary condition; if this is right, not only in subsequent cases we will invariantly observe that its absence prevents water from boiling at 100°, but recalling past observations we can readily see that it was indeed present in all earlier cases in which water boiled at 100°. Moreover, (γ) the longer an inductive generalization is tested, the less frequently new relevant circumstances C_{n+i} or D_{n+j} are discovered, and (δ) the smaller their impact (i.e., the less their absence causes the actual values of the function to deviate from the predicted ones); finally, (ε) *in practice*, after a certain time t_c no further relevant circumstances are found anymore. In this sense we may speak of invariant regularities: when a simple description of the form '$A = fB$' is not simply rejected after a few tests, but either confirmed or refined through a series of successive descriptions of form (10), (11) or (12) conforming to conditions (α)-(ε), at time t_c we may say that the series *converges* on the description we hold at that moment. At this point, by inference to the best explanation we may assume that there is some underlying mechanism or permanent disposition of nature to produce the described regularity, or one closely approximating it; from this we may then infer to the universal generalization of that description, and call it a *law*.

Now, coming back to the above objection, in most cases observations, tests and background knowledge soon make it clear that *the time* at which a phenomenon has been observed (as well as *the place*, and other circumstances of our observations) is neither an argument D_i of the function, nor a necessary condition C_i that must remain equal. Even if a particular time span (or place, etc.) figures as a necessary condition (e.g., 'Given it is the last decade of June, and it is not too cold or too hot, cornflowers blossom in the Castelluccio Plains'), or even if time is an argument D_i in the function (e.g., 'Acceleration of free falling bodies is a function of time'), *this* is so *at all times* (and in all places, etc.). Hence, the observed regularity cannot be explained by the explanation (c) proposed by the above objection, even if it is modified as

(c′) There was an underlying mechanism or a permanent disposition of nature to bring it about that $A = f(B, D_1, D_2,\ldots, D_n)$, if (and only if) C_1 & C_2 & … C_m, and *up to time t*.

Such an explanation would not be adequate, because it places on the uniformity of nature a limit which is both unnecessary and crippling: unnecessary since the time being no later than t has not be shown to be a necessary condition for $A = f(B, D_1, D_2,\ldots,D_n)$, and crippling since (c) and (c′) would explain positive cases until time t, but leave all the subsequent ones as miraculous coincidences. Thus, neither (c) nor (c′) can be assumed as a candidate best explanation of an observed regularity in alternative to (b) or to its refined version

(b′) There is an underlying mechanism or a permanent disposition of nature to bring it about that $A = f(B, D_1, D_2, \ldots, D_n)$, if (and only if) C_1 & C_2 & … C_m.

Incidentally, this is just another way to show that the old humean riddle of induction and the new goodmanian[10] one have a common root, and may be tackled by similar strategies[11]. Attributions of properties may be seen as explanatory hypotheses about the causes of observed phenomena: after observing that emeralds are invariantly green until time *t*, we can explain this either by suggesting that emeralds have the property of being green, or by assuming that they have the property of being *grue* (i.e., green if observed until *t*, and blue otherwise). Both explanations are coherent with past observations, but only the former is *the best* explanation, as such deserving to be taken as true. The latter is not a satisfactory explanation, since it introduces both time, which has not been shown to be a necessary condition for being green, and an unnecessary hypothesis (that emeralds unobserved until *t* are blue). Of course, this view presupposes that being green is an determined (atomic) property, being blue is a different (atomic) property, and being *grue* is not a (atomic) property at all, as well as that position in time is a determined property of events: in sum, it presupposes at least a moderate realism on universals, which Goodman rejects. But this moderate metaphysical assumption is much weaker than Sankey's assumption of the uniformity of nature, understood as constant coexistence of some properties in the same substance as a ground for natural laws.

Recapitulating, we get both a justification of particular inductive inferences and of induction in general in the following way:

[10] See Goodman ([1946], [1954], ch. 3).
[11] See Blackburn [1969], Alai ([1991], [2005]).

Justification of particular inductive inferences

(I) After noticing a phenomenal regularity, by repeated observations and tests we find circumstances $C_1, C_2, \ldots, C_n$ and $D_1, D_2, \ldots, D_n$ such that at a time t_c our descriptions of that regularity *converge* on a description of form

$$(10') \qquad (C_1 \,\&\, C_2 \,\&\, \ldots\, C_n) \leftrightarrow A = fB$$

or

$$(11') \qquad A = f(B, D_1, D_2, \ldots, D_n)$$

or

$$(12') \qquad (C_1 \,\&\, C_2 \,\&\, \ldots\, C_n) \leftrightarrow A = f(B, D_1, D_2, \ldots, D_n).$$

(II) We reason that unless this regularity is a miracle or an utterly unlikely coincidence, it must be the effect of an underlying mechanism or permanent disposition of nature ((b_1) or (b_2) above). This mechanism or disposition must be apt to bring about (most probably) a series of invariant phenomena conforming to the description (10′) or (11′) or (12′), or (less probably) a series slightly diverging from it by some factors C_{n+i} or D_{n+j} we have not observed so far (the greater the deviation, the less probable). So, by inference to the best explanation, we conclude that there is such an underlying mechanism or disposition.

(III) From this, we deductively infer that the corresponding universal generalization of form

$$(10'') \qquad \forall i[(C_1 \,\&\, C_2 \,\&\, \ldots\, C_n) \leftrightarrow (A = fB)]$$

or

$$(11'') \qquad \forall i[A = f(B, D_1, D_2, \ldots, D_n)]$$

or

$$(12'') \qquad \forall i[(C_1 \,\&\, C_2 \,\&\, \ldots\, C_n) \_ A = f(B, D_1, D_2, \ldots, D_n)],$$

is most probably correct, with only small deviations from it having probabilities practically different from zero, and we call it a law. So, the inductive inference to this law is justified.

Notice, in spite of the deductive character of step (III), an occasional failure of generalizations of form (10′) or (11′) or (12′) is in principle allowed by this justification, for two different reasons: first, step (II), yielding the premise for (III), is an inference to the best explanation, and so an ampliative inference: the content of its conclusion exceeds that of its premises, so the conclusion is *probably* but not *necessarily* true; the same, therefore, holds for the conclusion of (III), which is drawn from it. Second, since in principle, in spite of convergence, we cannot altogether exclude the existence of unobserved relevant factors C_{n+i} or D_{n+j}, our description of the underlying mechanism or disposition as producing a given phenomenal regularity, as opposed to some very similar regularity, may be only probabilistic. Nonetheless, it is in the nature of induction to yield probabilistic conclusions; so, this justification is successful, *qua ampliative* justification of an *inductive* inference.

General justification of induction

(I′) We observe the following: in the past, whenever by repeated observations and tests our descriptions of a given regularity at a time t_c converged, the description on which they converged has invariantly been confirmed even by all observations after time t_c.

(II′) From this, by inference to the best explanation, barring miracles or unlikely coincidences, we argue that

(13) There exist underlying mechanisms and/or permanent dispositions of nature producing the series of invariant phenomena conforming (most probably) to the descriptions on which earlier descriptions converge, or (less probably) to slightly diverging descriptions (the greater the deviation, the less probable).

(III′) Hence,

(14) The universal generalizations of descriptions on which earlier descriptions converge are probably true (and approximations to them even more probably true), and we may call them laws. So, inductive inferences to those laws are justified.

Here, (13) is the form assumed by the Principle of Uniformity of Nature, and (14) is the form assumed by the Principle of Induction. Unlike the obviously false

(6) All inductive inferences are justified,

(14) specifies that only inductive inferences to universal generalizations of those descriptions on which there has been convergence are justified. In a similar vein, (13) differs from various unsatisfactory versions of the Principle of Uniformity of Nature:

(4) Nature is uniform under *all* respects

is clearly too strong to be tenable, and

(5) Whenever a regularity has been observed, this regularity will hold in all instances

is still too confident as it stands, and it needs qualifications. On the other hand, since everything is similar to everything else in countless respects,

(7) Nature is uniform

is empty until one specifies in which respect(s) nature is supposed to be uniform. In turn,

(8) There are universal regularities in nature,

and

(9) Some events, properties or individuals will be always connected in the same way to given events or properties

are too vague to be useful in drawing any particular inductive conclusion. Instead, (13) specifies that nature is uniform (at least) under the respects outlined by the descriptions on which there has been convergence, and that the regularities which will hold in every (future) instance are (most probably) those conforming to such descriptions, or (less probably) to close approximations to them.

Obviously, like the justification of specific inductive inferences, even this general justification of induction is an ampliative inference, so, in principle, it allows for individual failures. It might be objected that, being ampliative, the "no miracles" justification is also inductive, thus running into circularity. However, not all ampliative inferences are inductive: induction is the inference from a number of instances of a certain phenomenon to more (and possibly infinite) instances of *the same* phenomenon. The "no miracles" justification, instead, consists of (i) an abduction, or inference to the best explanation (steps (II) and (II′)), and (ii) a deduction (steps (III) and (III′)); now, abduction does not proceed from some instances of a given phenomenon to more instances of the same phenomenon, but from some instances of a phenomenon to its underlying causes or common principle[12]. A justification of induction may be circular in two ways: first, if from the past successes of induction (premise) one argues that induction will be always successful in the future (conclusion). This would be an inductive inference, from instances of a certain phenomenon to further instances of the same phenomenon, and so it would be circular. Second, a justification of induction is circular if from the fact that nature behaved uniformly in the past (premise) one argues that it will always behave regularly in the future (Principle of Uniformity of Nature, intermediate conclusion), from this deducing that induction is warranted (conclusion). Here, the step from premise to intermediate conclusion is again inductive, hence circular. Instead, the "no miracles" justification also deduces the Principle of Induction (conclusion) from a version of the Principle of Uniformity of Nature (intermediate conclusion), but it establishes the latter principle abductively, not inductively: it derives it

[12] On the difference between induction and abduction see also Musgrave ([1988], p. 238); Musgrave ([1999], p. 234); Sankey ([2008], p. 84, footnote 1). See also references at footnote 6 above.

from the presupposition that observed regularities must have an explanation, and from the fact that the uniformity of nature (as above qualified) is the best (in fact, the only admissible) explanation. So, the idea that future instances must resemble past instances is not a premise of this justification, and no circularity is involved.

Of course, this is not an *absolute* justification: it relies on the correctness of abduction, which is usually taken for granted both in everyday reasoning and in philosophy, but is questioned by some philosophers. According to BonJour, for instance, the cogency of the abductive argument in favour of induction can be rationally grasped a priori ([1998], pp. 203 ff.); however, whoever rejects abduction can also reject this justification of induction. But this is so for every argument we might give for *any* conclusion: not all premises can be established at once; and above all, being non-absolute is quite different from being circular.

Armstrong does not believe we can answer Hume's sceptical challenge by justifying induction, for the correctness of induction is implied by numberless strongly held beliefs, such as that bread nourishes, water suffocates and fire burns. He calls them 'Moorean' beliefs: like Moore's belief that "this is a hand", they cannot be proved, because there are no other beliefs of equal or greater strength which may serve as premises of the purported proof. So, in his view, it is not possible to argue *that* induction is justified, but at most to show *why* it is justified ([1995], pp. 46-47). But the Principle of Induction does not seem to be among our *most* certain beliefs: for instance, the belief that a particular inductive inference (or induction in general) has been successful *until now* (an observed phenomenon) is no doubt *more* certain than the belief in the Principle of Induction (i.e., the belief that induction will also be successful in the future). Thus, since the "no miracles" justification derives the latter belief from the former, it counts as an argument *that* induction is justified, although it would not satisfy a sceptic seeking a purely deductive or in principle infallible proof.

Dipartimento di Filosofia
Università degli Studi di Urbino "Carlo Bo"
E-mail: mario.alai@libero.it

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Mario Alai

L'ARGOMENTO DEL "SE NON È UN MIRACOLO..." A GIUSTIFICAZIONE DELL'INDUZIONE

Riassunto

Il problema apparentemente insolubile di una giustificazione non circolare dell'induzione diverrebbe più abbordabile se invece di chiederci solo cosa ci assicura che un fenomeno osservato si riprodurrà in modo uguale in un numero potenzialmente infinito di casi futuri, ci chiedessimo anche come si spiega che esso si sia manifestato fin qui in modo identico e senza eccezioni in un numero di casi finito ma assai alto. È questa l'idea della giustificazione abduttiva dell'induzione, avanzata in forme diverse da Armstrong, Foster e BonJour: serie talmente regolari di fenomeni sono talmente improbabili che se il mondo fosse puramente casuale esse non potrebbero verificarsi se non per una coincidenza miracolosa. Se dunque non vi sono miracoli, tali regolarità si spiegano solo assumendo che siano prodotte da meccanismi o necessità nomiche; ma se questo è il caso, è corretto concludere che tali regolarità persisteranno anche in futuro senza eccezioni, e dunque le inferenze induttive su di esse sono giustificate. Anche Kornblith argomen-

ta dall'effettivo successo delle inferenze induttive all'esistenza di regolarità oggettive in natura, mentre Sankey parte dall'uniformità della natura per giustificare l'induzione. Congiungendo dunque le due argomentazioni, si giustifica di nuovo l'induzione in base all'argomento che le regolarità osservate non possono esser dei miracoli.

Certo, bisogna chiarire in che senso la natura sia uniforme (dato che ovviamente non lo è in ogni suo aspetto), e di quali regolarità possiamo aspettarci che persistano senza eccezione anche in futuro (dato che evidentemente non tutte lo fanno: l'acqua *non* bolle sempre a 100°, la pressione *non* è sempre funzione della temperatura, e così via). Ma le conoscenze di sfondo e la ripetizione delle osservazioni in condizioni diverse ci mostrano quali circostanze siano rilevanti al verificarsi del fenomeno dato. In tal modo le nostre descrizioni iniziali delle regolarità naturali *convergono* su descrizioni che specificano sia le condizioni individualmente necessarie e congiuntamente sufficienti, sia tutti gli argomenti delle funzioni che costituiscono tali regolarità. Ciò consente di formulare i principi di Uniformità della Natura e di Induzione in modo non generico (e dunque, a seconda della formulazione, troppo forte o troppo debole), ma circostanziato: così essi asseriscono, rispettivamente, che la natura è uniforme negli aspetti evidenziati dalle descrizioni su cui ci fa convergere l'osservazione ripetuta, e che solo queste descrizioni possono essere induttivamente generalizzate. Così si risolve anche il seguente enigma Goodmaniano: poiché abbiamo osservato il verificarsi di una data regolarità (solo) *fino al momento presente t*, come possiamo presumere che essa si verificherà anche dopo *t*? La risposta è che osservazioni e conoscenza di sfondo non ci dicono che vi sia alcun limite temporale tra le condizioni necessarie della regolarità data.