BAD LOTS, GOOD EXPLANATIONS*

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SUMMARY: Van Fraassen's argument from the "bad lot" challenges realist interpretations of inference to the best explanation (IBE). In this paper I begin by discussing the replies suggested by S. Psillos and P. Lipton. I do not find them convincing. However, I think that van Fraassen's argument is flawed. First of all, it is *a non sequitur*. Secondly, I think that the real target for the scientific realist is the underlying assumption that epistemic justification results from a comparative assessment among rival explanations. I argue that justification for believing an explanation does not depend on comparison, but on the extent that criteria of explanatory goodness are fulfilled. Therefore, in addition to offering more or less intuitive IBE-tailored arguments, realists fond of IBE should have to analyze the implicit standards of explanatory goodness. In the last section I distinguish between contextual and transcontextual criteria concerning explanatory goodness. Concerning the latter, I focus on consilience, simplicity, analogy and conservatism.

KEY WORDS: inference to the best explanation, van Fraassen, scientific realism

RESUMEN: Un argumento empleado por van Fraassen contra las interpretaciones realistas de la inferencia a la mejor explicación es el argumento "del mal lote". El artículo comienza discutiendo las réplicas de S. Psillos y P. Lipton. Aunque ninguna me parece convincente, pienso que el argumento de van Fraassen no resiste un examen cuidadoso. En primer lugar, la conclusión antirrealista que él extrae es un *non sequitur*. Además, el argumento parte de un supuesto muy cuestionable, a saber, que la justificación de una explicación es resultado de una comparación con sus rivales. En mi opinión, la justificación no depende de esto, sino del grado en que la explicación satisface ciertos criterios de bondad explicativa, independientemente de que haya o no comparación. En consecuencia, la socorrida estrategia de defender el realismo científico basándose en la IBE pasa necesariamente por un análisis de los criterios implícitos de bondad explicativa. Mi propuesta es distinguir entre criterios contextuales y transcontextuales. En los últimos incluyo: la diversidad de la evidencia explicada, la simplicidad, la analogía y el conservadurismo.

PALABRAS CLAVE: inferencia a la mejor explicación, van Fraassen, realismo científico

Inference to the best explanation (IBE) is a favoured policy among scientific realists. The argument from the "bad lot" is one of van Fraassen's latest attempts to cast doubt on IBE. Firstly, I discuss the replies offered by S. Psillos and P. Lipton. I do not think they are successful, so I develop a different approach in section 2. I conclude that we have no rationale to believe that, as a rule, there is —or it is likely to be— a better explanation outside the lot of currently available explanations. I reject also the comparative notion of justification implicit in the argument from the bad lot and I claim that justification of IBE has to do mainly with the explanatory goodness possessed by the explanation in question. Finally, although I accept that the strength of IBE depends on the particular context where it is applied, in section 3, I consider some trans-contextual criteria for explanatory goodness.

1. *The Argument from the Bad Lot*

The argument is stated by van Fraassen as follows:

[IBE] is a rule that only selects the best among the historically given hypotheses. We can watch no contest of the theories we have so painfully struggled to formulate, with those no one has proposed. So our selection may well be the best of a bad lot. To believe is at least to consider more likely to be true, than not. So to believe the best explanation requires more than an evaluation of the given hypothesis. It requires a step beyond the comparative judgment that the hypothesis is better than its actual rivals. While the comparative judgment is indeed a 'weighing (in the light of) the evidence', the extra step —let us call it the ampliative step— is not. For me to take it that the best of set X will be more likely than not, requires a prior belief that the truth is already more likely to be found in X, than not.[1]

Van Fraassen's complaint is addressed to the ampliative step. "The best" is unavoidably relative to the set of available theories, and the set of available theories is restricted by the scientists'

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[1] Van Fraassen 1989, p. 143.

imaginative power and other historical contingencies. Unfortunately, we are bound to choose from an incomplete set. Putting the matter in another words, if J is the set of all hypotheses which entail the evidence and K is the subset which contains the lot of *available* explanations, to believe that the theory considered best in K is true would be justified only in case we had some grounds to believe that truth lies somewhere within K. But that is precisely what is at stake.

Stathis Psillos and Peter Lipton have recently argued that the argument from the bad lot is not sound. Roughly speaking they argue that the background constrains scientists' theoretical assessments, and, since it is at least approximately true, scientists' choices are well-grounded. Their conclusion is that, contrary to van Fraassen, we are justified in believing that the theory considered best in the lot is, at least, approximately true. I shall take up both replies in turn.

1.1. Psillos's Reply

According to Psillos, the argument from the bad lot misconceives the way scientific evaluation takes place. The key question to support the ampliative step in IBE is to realize that "theory-choice operates within and is guided by a network of background knowledge".[2] Psillos argues that:

 (i) Theory-choice operates within —and is guided by— a network of background knowledge.

 (ii) The background knowledge suggests hypotheses which are compatible with it. The best theory is selected from this reduced set of potential explanations.

(iii) The background knowledge is true.

Conclusion: the best explanation in the lot is true.

Psillos's main point is that compatibility with the best confirmed theories suffices to assure that the potential explanations

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[2] Psillos 1996, p. 38.

can not go astray and, consequently, that the lot of available ex-
planations is a good one. Although, from a logical point of view,
the number of theoretical hypotheses which entail the evidence
may be infinite, the range of explanations that scientists consid-
er is not unlimited. At the very outset the background knowl-
edge "can drastically narrow down the space for hypotheses that
provide a potential explanation of the evidence at hand".[3] A
potential explanation entails the evidence at hand and enjoys
some initial plausibility. And, when there are several hypothe-
ses which fulfil these requirements, "explanatory considerations
are called forth to select the best among them". (Id.) In dis-
cussing a historical episode —Fresnel's hypothesis about the
transversal propagation of light—, Psillos briefly mentions what
kind of considerations determine the choice: all other things
equal, scientists prefer the hypothesis that does not force us to
make *ad hoc* adjustments and generates less additional problems.
In the nineteenth century the interference fringes produced by
parallel polarized light-rays could only be explained appealing
to light-waves propagating in different ways (either longitudinal
or longitudinal and transversal). Explanatory considerations led
Fresnel to favour the first hypothesis, since they were empiri-
cally equivalent according to the available evidence. But both
explanations assumed the wave theory of light, which, at the
time, had superseded the corpuscular theory and, supposedly,
both are approximately true.

   Psillos emphasizes the effect of constraining operated by the
background knowledge. He agrees with van Fraassen that the lot
of available explanations is reduced. But the size of the lot does
not threaten justification because, whatever the hypotheses it
includes, they are all fairly good. Obviously, the process depicted
by Psillos is reliable provided the background is sound. The
rationale for believing this is that

> Even though evidence does not entail theoretical beliefs, it can *sup-
> port* some theoretical beliefs up to a high degree, so that it would
> be unlikely that the beliefs are outright false and the evidence that

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[3] Ibid., p. 39.

it is. [. . .] Those beliefs for which scientists acquire overwhelming
supporting evidence augment the mass of warranted background
beliefs and become the pivots for new warranted beliefs.[4]

Sometimes the evidence is so irresistible that the falsity of the
hypothesis which entails it is hardly conceivable. In the end,
the background is sound because the hypotheses included in
it are overwhelmingly supported by the evidence. And this is
why they lend justification to the potential explanation selected
under their guidance.

Unfortunately, I do not think that Psillos's reply is valid
against the argument from the bad lot. In principle, an over-
whelming evidence and no available competitors count in favour
of a hypothesis. However, Psillos's argument could offer some
comfort if it were possible to infer directly the truth of some the-
oretical beliefs solely from the evidence entailed by them. But
this possibility is ruled out by the first premise of his argument:
the lot of potential —i.e., available— explanations results from
a reduction operated by the background. So, the hypotheses
included in the present background knowledge were themselves
suggested by the extant background knowledge at the moment,
and the hypotheses contained in the previous background were
suggested by an older background, and so on. Van Fraassen's
objection reappears at whatever temporal point we decide to
stop. What reason do we have to believe that the available lot
at that point was a good one? Psillos's reply simply begs the
question.

On the other side, sometimes the scientific community en-
dorses hypotheses which conflict with the background knowl-
edge. It is hardly disputable that some of the most fruitful sci-
entific hypotheses were proposed against the set of established
truths. Einstein's hypothesis about light-quanta, for instance,
clashed with the prevailing view about the nature of light, i.e.,
the undulatory theory, which was —as Psillos remarked above—
part of the background at the time. When there is no hypothe-
sis in the lot which explains the evidence, novel alternatives are

[4] Ibid., p. 40.

required and some of the background assumptions must be put
aside. Psillos could reply that his model of science can accom-
modate these episodes because it does not claim that compat-
ibility with the background is the only source of justification.
Exceptionally, evidence alone may justify the endorsement of
a particular explanation which overtly clashes with the back-
ground. But then, in order to accommodate such episodes, we
have to acknowledge that the lot was not a good one, and the
justificatory import of the background is in question.

I suspect that Psillos himself is not very confident about the
privilege he confers to the background knowledge. That is the
reason he puts forward a further *tu quoque* reply. Recall that
van Fraassen's constructive empiricism claims that the aim of
science is *empirical adequacy*. Acceptance of a theory does not
require belief in its truth. It suffices with believing it is em-
pirically adequate. And believing that a theory is empirically
adequate equates to believing that *all* its observational conse-
quences —past, present and future— are true.[5] Psillos points
out that accepting a scientific explanation as empirically ade-
quate assumes that the lot of available explanations is a good
one. Hence, constructive empiricists cannot dispense with a priv-
ileged background knowledge and are not in a better position
than realists, otherwise "how do they know that the real empir-
ically adequate theory does not lie in the spectrum of hitherto
unborn hypotheses?"[6]

However, "the real empirically adequate theory" is an unfor-
tunate definite description, because there are many empirically
adequate theories concerning a body of evidence, while there is,
supposedly, just one true theory. I do not want to enter into
details about the extent of the background's privilege because it
seems to me that this is not an essential point in the discussion.[7]

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[5] Van Fraassen 1980, p. 12. Concerning the unobservable apparatus of
the theory, the wiser attitude is, according to van Fraassen, agnosticism.

[6] Psillos, op. cit., p. 41.

[7] The point has already been discussed. See Ladyman, Douven, Horsten
and van Fraassen 1997, and Psillos's counterreply (pp. 369–372) in the same
issue.

What is at stake is whether we are justified in inferring the truth of the best available explanation. Showing that constructive empiricism needs some kind of privilege for the background does not reinforce the realist position, since to consider one theory in the lot as empirically adequate does not involve any commitment to its truth beyond the observational level. Neither does it involve a commitment to the background's truth. Hence, the appeal to the background knowledge does not give an advantage for the realist. Certainly, once belief about unobservables has been bracketed, the justification for belief in empirical adequacy still remains as an embarrassing issue for the constructive empiricist, insofar as it involves beliefs about the future, that is, about unobservable situations. But, even though the justification of the belief in empirical adequacy depends on the correctness of the background knowledge involved, the kind of correctness required may be just empirical adequacy.

1.2. Lipton's Reply

According to Lipton, the argument from the bad lot grants that the ranking of explanatory rival hypotheses elaborated by scientists is a reliable indicator of their *relative* truth content. The hypothesis considered best is, in fact, the truest one. The challenge, then, is closing the gap between comparative and absolute evaluations concerning the truth content of rival explanations.[8] Here is his argument:

  (i) Scientists are reliable rankers concerning the relative truth content of the rival hypotheses.

 (ii) If scientists are reliable rankers about the relative truth content of the rival explanatory hypotheses, then, the background knowledge is approximately true in an absolute sense.

(iii) If the background knowledge is approximately true in an absolute sense, then, the best explanation in the lot is also true in an absolute sense.

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[8] Lipton 1996, p. 97.

Conclusion: the best explanation in the lot is true (in an absolute sense).

Likewise Psillos, Lipton emphasizes the role played by the background knowledge. The way the data are characterized, their relevance, and the fact that the technology employed in testing predictions is devised according to scientific theories already accepted, show rather convincingly that we are bound to rely on background theories. Besides, the background is actually true in an absolute sense, as the step (ii) states.[9] In order to support this claim, Lipton suggests a counterfactual statement:

> If most of the background theories were not even approximately true, they would skew the ranking, leading in some cases to placing an improbable theory ahead of a probable competitor, and perhaps leading generally to true theories, when generated, being ranked below falsehoods.[10]

This is a sophisticated version of the "no miracle argument". This well known argument infers the approximate truth of scientific theories from its observational success: truth is the best explanation of the impressive empirical success of contemporary science.[11] Lipton applies the same pattern to a different *explanandum*, namely, the "ranking success". But, in any case, he makes an inference from success to truth: scientists could hardly make successful rankings, if their guiding principles —the background theories— were not accurate at least to some degree. Therefore, provided that rankings are fairly good, we have some rationale to believe that background theories are approximately true.

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[9] Lipton acknowledges that (i) could be rejected by a radical skeptic. However, he rightly points out that it is a plausible claim granted by van Fraassen himself. Recall that van Fraassen's constructive empiricism is a sort of skepticism concerning the non-observational realm, but constructive empiricism does not rule out talking about truth when dealing with observational entities. Certainly, (i) could be disputed from an overtly skeptical standpoint, but I shall not argue the point here.

[10] Ibid., p. 100.

[11] For a detailed discussion of this well-known argument, see Iranzo 1999.

I grant that, if this argument is sound, we have a non comparative assessment of truth. The question is whether the ranking might reliably mirror the comparative truth content of rival explanations, in spite of the falsity of the background theories. An affirmative answer would block Lipton's argument and I must confess that my intuitions are unclear. Anyway, the argument has not been completed yet, since Lipton does not assert only the approximate truth of the background theories, but the likely truth of the best *extant* theory (see above step (iii)). His remarks on this point are surprisingly brief. According to him, "the best of the theories now being ranked will form part of tomorrow's background, . . . Hence, if scientists are highly reliable rankers, . . . , the highest-ranked theories have to be absolutely probable, not just more probable than the competition". (Id.). So, in the end, the best —past and present— is approximately true.

Nonetheless, even if it sounds reasonable that the best theories now ranked will be taken into account in future assessments, it is not very complicated to list examples of very well ranked past theories —according to the standards of those days— that have been discarded. Lipton takes the point: "Even the most fervent realist cannot afford to claim that scientists are completely reliable rankers, since this would require that all their background beliefs be true, a hopelessly optimistic view, and one that is incompatible with the way the scientific background changes over time."[12] He notices, however, that the argument may work without infallible rankers, for "even moderately reliably ranking is not compatible with the claim that scientist's methods may leave them with theories that are arbitrarily far from the truth".[13]

In the end, Lipton's answer to the argument from the bad lot depends on the existence of a direct correlation between success in ranking and success in attaining truth.[14] The better the rankers, the truer the theories. And he does not believe that

[12] Lipton, op. cit., p. 106.

[13] Ibid., p. 102.

[14] See his example at p. 101.

there is a substantial change in moving from a perfect ranking to a more or less accurate one. It can still be inferred the approximate truth of the hypothesis best considered. Certainly, the fact that a correct ranking based on an outright false theoretical background seems very unlikely gives intuitive support to the existence of a correlation between success in ranking and success in attaining truth. This is an extreme situation: perfect reliability on one side, outright falseness on the other. It sounds convincing because it seems clear that from a collection of falsities we can get no more than a mistaken ranking. I pointed above that my intuitions are not clear beyond extreme situations like this. Now I shall argue that the existence of the correlation is hardly defensible as we move away from extreme examples toward intermediate ones.

An approximately accurate ranking —in Lipton's sense— is a list of the available explanatory hypotheses. The position in the list approximately reflects their content of truth. Imagine that the rankers are confronted with four rival explanatory hypotheses (H_1, H_2, H_3, H_4). Let us suppose that the perfect ranking is H_1-H_2-H_3-H_4. What kind of results could be obtained by "non completely reliable rankers"? The list H_2-H_1-H_3-H_4 seems a fairly good ranking. It misplaces only the two first positions. But, although the ranking is rather good, to endorse H_2 instead H_1 may be disastrous if H_2 is much worse than H_1.

Lipton could reply that rankers who commit such errors are not reliable at all, excluding the possibility of interchanging hypotheses notably different as to their truth content. Reliable rankers do not make always perfect rankings but they *usually* make fairly good rankings, that is, rankings where the pool position is occupied by the truest hypothesis in the list. Perhaps he could add that, in case scientists misplace the first position, there will not be a substantial difference between the hypothesis actually endorsed and the truest hypothesis in the list.

It is worth noting, however, that in the most common situations where scientific decisions take place, the ranking very likely will include few hypotheses. A common situation includes only two hypotheses, and more than three is rather unusual. Even though there is only one perfect ranking, the number of

approximately accurate rankings depends on the total number of *rival* hypotheses, that is, the number of hypotheses seriously taken into account by the scientific community. But, as the number of hypotheses in conflict diminishes, the proportion of good rankings —that is, rankings where the first position is occupied by the truest hypothesis— increases in comparison with the total number of possible rankings. Thus, in the common situation where there are only two rival hypotheses, the odds for ranking them successfully by chance are 1/2. When there are three hypotheses in conflict, the odds are 1/3. And, if we define "approximately accurate ranking" as encompassing rankings where the first hypothesis in the rank is not the truest one but the closest to it, then odds for success in ranking would increase even more.

A consequence we might draw from these remarks is that an approximately accurate ranking does not assure that we are close to the truth, because the probability of getting it by chance is high enough not to be dismissed. The point is that a good ranking may be obtained rather easily from faulty background theories, granted a very small number of ranked hypotheses. Undermining the reliability of the rankers has undesirable consequences on the alleged soundness of the background. Lipton's argument collapses inasmuch as establishing the likely truth of background theories is a necessary step to infer the approximate truth of the best extant theories.

At this point, Lipton could argue that we have misconstrued the *explanandum*. When we attribute reliability to scientists, particular success does not count decisively. Rather, we are pointing at a tendency, and it is this tendency that demands an explanation. Although it may be fairly easy to stick at a good ranking, it is unlikely to attain a high *rate of success* in a large row of decisions —say, 80% rankings are approximately accurate—, despite of the fact that scientists are bound to choose from a very small lot of hypotheses. The occurrence of such unlikely state of affairs is conditional upon the occurrence of another fact, i.e., the approximate truth of the background knowledge. It is the (approximate) truth of the background that

makes likelier continuous ranking success, otherwise highly unlikely.

But, if reliability is understood in the long run, Lipton's argument yields no more than a general —and weak— presumption in favor of realism. According to him, "modest realists" believe that some of our best considered hypotheses are not really the truest in the lot. This is a plausible claim insofar as a fallibilistic stance about scientific knowledge seems unavoidable. But fallibilism is not very informative and modest realism turns into "blind realism" because we are completely at a loss about which are the more suspicious explanations —and, neither do we know which are the more credible of them— in the wide range of endorsed hypotheses. There is just a vague presumption in favour of the scientific world-view, as a whole. This is not a surprising result since Lipton's argument focuses on a general feature of scientific practice —ranking reliability. Yet I think that the position in the list is not the crucial factor concerning the hypothesis's credibility. Sometimes the best available hypothesis may be so poor that it does not deserve to be believed. Then, the confidence attached to top-hypotheses does not seem to depend on scientists' ranking skills, but on the virtues they actually possess *qua* explanations.

To sum up, Psillos and Lipton base their replies on the fundamental role played by the background knowledge in science. I agree with their common assumption, but I do not find their replies convincing. In the next section I shall try another way of coping with van Fraassen's objection.

2. *Lots and Comparisons*

Recall that van Fraassen distinguishes two steps in IBE: the comparative and the ampliative. According to him, the ampliative step is not justified because the comparative step is circumscribed by the set of available explanations (K). The argument from the bad lot threatens justification because it suggests that it may be a better explanation outside K, that is, in the set of now unavailable explanations (J). In fact, in some places van Fraassen states his view in a more radical way. It is not only that

the contingencies of the context of discovery *could* have been so different as to lead us to unforeseen explanatory alternatives. Rather he asserts that "*there are* many theories, perhaps never yet formulated but in accordance with all the evidence so far, which explain *at least as well* as the best we have now".[15] He adds that explanations in accordance with all the evidence obtained so far may disagree in many ways about statements that go beyond this evidence. As a consequence, "most of them by far must be false" and, since the best theory is just a random member of this class, it is likely false. So, we are not justified in believing it. Thus, he intends to reinforce the case against the best explanation by pointing out that: (i) there are always alternative explanations —even though they have not been formulated, and (ii) they are serious competitors so long as they are as good, at least, as the best explanation we have now.

If we could be able to compare all the possibilities, the final assessment would render the best explanation in an absolute sense and we will be justified in believing it. But, how to compare explanations which have not been even formulated yet? Both Psillos and Lipton try to avoid exhaustive comparison by identifying some feature for K which entitles them to infer the truth of the best explanation it contains. Psillos emphasizes "affinity with the background", while Lipton stresses "ranking adequacy". I agree with them on the general policy: an exhaustive comparison is not required. But my reasons for dismissing it are different. Firstly, we have no rationale to believe that there are always better explanations than the best available one. Secondly, believing that there *may be* a better explanation than the best available one does not undermine justification. Thirdly, the thesis that justification is related to comparison is an underlying wrong assumption in the argument from the bad lot. Rather, justification depends on the "explanatoriness" possessed by the hypothesis. In the next section I shall develop the first and the second topics. The relation between justification and comparison will be addressed in section 2.2.

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[15] Van Fraassen 1989, p. 146 (my emphasis).

2.1. Justification and Unavailable Explanations

I shall assume the general point that our doxastic states about the likelihood of finding better explanations in the future may defeat justification for believing the best available explanation. Let us begin with van Fraassen's strongest claim (i.e., there are better explanations in the set of "never yet formulated" ones). This claim seems to him so obvious that he does not even argue for it. I think that such a strong statement deserves a more detailed scrutiny. Three kinds of reasons to believe that there are so many and so good unknown explanations come to my mind:

(a) *Logical*. Given that the entailment relation between the empirical consequences and the theory runs only in one direction, the possibility of elaborating empirically equivalent theoretical hypotheses stems from purely logical considerations. Nevertheless, the claim that there may be different theories with the same empirical content concerning a certain body of evidence, does not entail that there are many equally powerful theories, from an explanatory point of view. Being empirically equivalent does not mean the same as being explanatory rivals because explaining an evidential corpus is not just to entail it. Tricky adjustments on a current theory may render an empirically equivalent version —adding extra baggage without observational consequences, for instance. Putting aside the question as to whether the modified version is as well confirmed as the original theory, it is quite clear that it does not offer an explanation of the evidence at hand as good as the original theory.

(b) *Psychological*. Broadly speaking, explaining involves setting up relations. Human cognitive powers are not only limited but they have been shaped under particular evolutionary pressures as well. Then, are we completely certain that the relations established by our best theory are the only possibility to account for the phenomena? Although it may seem hardly conceivable an alternative to a very good explanation, that could be only the effect of being constrained by our imaginative capabilities. Perhaps an alien race endowed with super-minds could find a much

better way to explain a certain body of evidence. However, this unspecific doubt is not damaging. The best available account is also the best for us but this does not imply that there are many accounts better than the best we have now. Neither the weakened claim "it is very likely that there are many accounts..." follows from the plain truth that the best available account is the best one that humans have been able to formulate.

(c) *Historical.* Granting that most scientific changes are progressive, we are bound to accept that extant scientific explanations will be eventually replaced by better ones. Even though the newer theory usually explains a larger body of evidence than the replaced one, it presumably will explain the phenomena covered by the old explanation. The relativistic explanation of planetary orbits is better than the newtonian explanation. It affords a better explanation for the same evidential corpus explained by the newtonian theory —and, obviously, for further evidence. Therefore, belief in scientific progress involves believing that better explanations will be worked out in the future. To affirm that *there are* undiscovered better explanations would overstress the point, but perhaps it could be argued, at least, that the set of unavailable explanations *likely contains* better explanations than the best extant one.

This argument is a version of the skeptical meta-induction addressed to explanation rather than truth. But it is unclear that it has to be an infinite sequence of explanations each of them better than the previous one. And, in any case, I find highly suspicious the claim that since an explanation now endorsed could be a poor explanation tomorrow, we are not justified in believing any extant explanation, whatever good it may be for us now. As I see it, the "historical concern" is no more than a reminder of both scientists' fallibility and science's incompleteness. However, I do not think this generic reminder rules out the possibility of believing justifiedly a good explanation. When we go into details concerning the justification for believing a particular explanation the historical concern is normally overridden by further factors which are far more important (the subject's beliefs about the

quality of the evidence at hand, the degree of explanatoriness possessed by the hypothesis, . . .).

This all goes to show that van Fraassen's claim about the actual existence of rival explanations is unacceptable. Logical, psychological and historical reasons are not enough to settle such a strong view. But, what about the weaker claim (namely, "it *may be* better explanations in the set of 'never yet formulated' ones")? Would not it be encouraged by both psychological and historical reasons? And, could not it be enough to defeat justification? I do not think so. Believing that it may be a better explanation outside the lot is compatible both with believing it is not probable there is a better explanation and with believing it is probable there is a better explanation. Justification will be defeated depending on which additional beliefs are held by the subject. But then, van Fraassen needs further grounds for the undermining belief "It is probable there is a better explanation than E" since the mere possibility that it exists does not defeat justification. The same arguments employed against his stronger assertion are effective against the weaker version, as we have seen above. So, even if it follows from van Fraassen's objection that, for every top-hypothesis it may be a better explanation outside the lot, the justification for believing the former has not been ruled out. In the end, his pessimistic conclusion is a *non sequitur*, because neither the actual existence nor the likeliness of better unknown explanations have been established.

I conclude, then, that exhaustive comparison is not required for justified belief because there is no rationale to believe that, as a rule, the best available explanation has —or could have— unknown dangerous rivals. I am not claiming, however, that the best available explanation is always justified. Believing that E is the best available explanation does not rule out the possibility of holding an undermining doxastic state because sometimes the best available explanation may be very poor, even in scientific contexts, as I pointed out above. In the next section I shall develop a further argument to reject exhaustive comparison. It may offer a positive case to infer the best explanation given that certain conditions are fulfiled.

2.2. Justification and Comparison

The thesis that justification is related to comparison is an underlying assumption in the argument from the bad lot. Recall that van Fraassen considers the best explanation as a "random member" of the class of explanations which explain the evidence to date. In fact, the requirement for an exhaustive comparison makes full sense from such assumption. Unfortunately, the assumption is flawed. If it were right, either justification could not be attributed unless an exhaustive comparison had been carried out —if justification is understood as an all or nothing affair—, or justification would depend on how many comparisons are made —if justification is understood as a gradual notion. I shall argue that both disjuncts are unpalatable because the justification of an explanation does not mainly depend on the comparisons it has been subjected to.

Firstly, we may be justified in believing a hypothesis which has not been compared to any rival. It may be that we have just one explanation concerning a range of phenomena. But this fact by itself neither makes it unlikelier nor prevents us from believing justifiedly in it because the explanation may still be a very good explanation. Moreover, as some writers have emphasized, having just one explanation that fits the available evidence is not an unusual predicament in science.[16] The historical record contains examples of temporary empirically equivalent explanations, but very often scientists feel content when they find just one explanation to connect the data. If comparison were necessary, we would never be justified in such situations, in spite of the fact that there were a scientific consensus about the explanatory goodness of the only available explanation (it connects apparently unrelated data, it unifies lower-level hypotheses, it suggests successful predictions, it does not force us to make great modifications in the background, . . .).

On the other side, when an explanatory hypotheses is better than its rivals but is selected just because it is the less bad from

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[16] See, for instance, Horwich 1991, Leeds 1994, Lipton 1991 and Norton 1994.

the batch, perhaps the most reasonable epistemic attitude is not to believe it. It is not difficult to think out situations where the best available explanation does not deserve great confidence. In that case, we will not perform the inference to truth unless the supposed explanation seems likely or reasonable to us. Turning again to scientific contexts, when there is comparison and selection, choosing a theory is usually accompanied by believing it, but sometimes this does not happen. Epistemic agnosticism is more popular, for example, among "frontier physicists" than among geologists. Correspondingly, belief and tentative acceptance —acceptance without believing that the theory is true, just because of purely pragmatic reasons—, may be justified in some scientific contexts.[17]

I conclude that, concerning the justification of believing a theory, comparison is neither necessary nor sufficient. Perhaps all this sounds too obvious to be spelled out. However, the apparent strength of the argument from the bad lot is due to its implicit assumption. If van Fraassen's objection were simply that being the best available explanation by itself does not entitle us to infer its truth, I would have no complaint. Inference to truth is not an automatic consequence of being selected as the best of a lot because the best available explanation must be an explanation good enough to infer its truth. But from this claim he infers the skeptical conclusion that we are never justified in believing the best explanation. I demur and suggest, instead, that inference to the best explanation is *sometimes* justified, and that justification decisively depends on the virtues possessed by the explanation at issue.

This said, it must be noticed, however, that while van Fraassen's objection assumes a "comparative" notion of justification, neither Psillos nor Lipton think that the justification of an explanation depends on comparison. They precisely try to show that there is something more —namely, the background's

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[17] Readers familiar to van Fraassen's works will notice here a resemblance to his distinction between acceptance and belief. I grant the distinction —albeit in a weaker form—, but I do not think that van Fraassen's rejection of belief in the existence of unobservable entities follows from it. For the view that belief is the same as acceptance see Horwich 1991.

truth— than mere comparison and selection from a lot. But they are not successful in settling it. Besides, they try to offer a vindication of IBE without taking into account the particular features of the explanations involved. Perhaps this is a natural response to an undiscriminate objection like that of van Fraassen, but I do not think it is an appealing policy to vindicate realism. The justification attributed to explanatory hypotheses may differ greatly from one to another context. Sometimes the best available hypothesis is no more than a guess. Sometimes we are very confident about it. Coarse-grained arguments that lead at most to a kind of general presumption overlook the fact that explanations, even when they are the best, do not deserve the same confidence. It seems to me that Psillos's inability to accommodate the endorsement of hypotheses against the background stems from missing this point. And Lipton's reply, on its turn, does not allow us to go beyond a vague presumption in favour of realism.

Therefore, being the best explanation does not guarantee justification, because there is no justification in believing the best explanation unless it is a good explanation. Turning both to the properties which make an explanation good and to the contextual factors involved in such an assessment seems the only way to do justice to a wide array of situations. Also, it may be the only way to justify our preferences. The following section is devoted to ascertain which are the factors involved in explanatory goodness.

3. *Explanatory Goodness*

I have tried to show that the justification of the best explanation is mainly a question about the explanatory goodness possessed by the best available explanation. Moreover, it sounds plausible that, in order to determine whether a particular instance of IBE is justified, it has to be taken into account the context wherein it is proposed.[18] But, even if it is true that there are contextually-bounded constraints on explanatory goodness —the pursuit of mechanistic or deterministic explanations, for instance—, there

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[18]   The contextuality of IBE has been emphasized in Day and Kincaid 1994.

may be also general criteria of explanatory goodness. In the remainder of the paper I shall explore this view. I shall follow an approach suggested by Paul Thagard.[19] He proposed three criteria for *theory* choice (consilience, simplicity and analogy). Let us see to what extent they could be applied in ordinary contexts.

Consilience. It is the ability to explain a variety of facts. Everyday explanations hardly fit this criterium as Thagard states it, but we can make sense of it: diversity is more important than quantity. He takes an example of C.S. Peirce. We may infer that a man is a Catholic priest from the fact that he wears a black suit and white collar. The goodness of my explanation does not increase because I see him dressed in black every morning. But my explanation will be strongly reinforced after discovering that he knows Latin because this is a novel fact. Certainly, scientists intend to explain observational regularities appealing to wide-encompassing theoretical laws, while the most usual request for explanation in ordinary contexts is explaining particular facts (the man in a black suit, my car did not start this morning, my best friend did not invite me to his birthday party, . . .). Such a contrast, however, makes no great difference concerning consilience. Although we dislike considering regularities as facts, consilience may be easily accommodated. The idea is that explaining instances of novel regularities counts more than explaining instances of known regularities.

Simplicity. Thagard defines simplicity as a function of the size and nature of the set of auxiliary hypotheses needed by a theory to explain facts. They are statements —not part of the theory— which are assumed in order to help explain a particular fact. An auxiliary hypothesis may become into an ad hoc hypothesis if ongoing research fails to discover either direct supporting evidence or new *explananda* for it. However, I see no equivalent for auxiliary hypotheses in everyday explanations. To say that our everyday explanations assume general claims about physical reality, human behaviour, and so on. . . is just a platitude, but I

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[19] See his 1978.

suspect that trying to count the number of "auxiliary hypotheses" in everyday contexts hardly makes any sense because of the vagueness of the assumptions involved.

In spite of this, it should be noticed that we tend to favour simplicity. When trying to find a suitable explanation, we usually begin by the simpler alternatives. And the reason is not only that they may be easier to test, but that they seem likelier to us. Anyway, it is not fully clear what we mean by simplicity in these contexts. Appeal to the naturalness of the related predicates does not solve the problem but some kind of "common sensical ontological economy" is probably involved. Concerning explanation of other people's behaviour, it could be said that we also tend to simplify. Whenever we have no reasons to attribute strange motives, or exaggerated passions, we do not appeal to them. But when we discard an explanation of a particular human action because it seems very complicated —in this sense—, what we actually mean is that the explanation attributes unusual motives to people in that situation. And that notion of simplicity fits better with the next criterium proposed by Thagard.

Analogy. According to Thagard, an analogical inference is an inference from similarities to the equivalence of the explanatory mechanisms which sustain those similarities. Thus stated, this is a rather sophisticated notion of analogy, since in everyday contexts very often we are not interested in internal workings or something like that. However, the idea that observational resemblances point at the same explanatory factors —let them be underlying structures, causes, reasons, . . . — is an heuristic pervasively applied in everyday explanations. Even in explanation of human behavior, it could be said that external similarity suggests underlying alike motives. Sometimes we discard an explanation because it attributes fool or uncredible motives, and we mean that it is no explanation at all (why is he pressing the lift's button?: just because he likes pressing lifts' buttons, because he wants to clean his finger, and so on. . . these are not putative explanations unless he have further reasons or evidence to discard the plainest answer: "because he wants to take the lift"). Roughly speaking, we judge the similarity between the motive attributed

to the agent and the motive we would attribute to us if we were in the same situation. The more similar the motives, the better the afforded explanation. Analogy could, perhaps, account for this.

So far, the criteria put forward by Thagard. However, I would add a further criterium, namely, *conservatism*. Even though it may be related to analogy, I think it deserves an isolated treatment. Other things equal, the more modifications needed on the set of background beliefs when endorsing an explanation, the less tempted we feel to embrace the explanation. This is not more than the maxim of minimum mutilation. Likewise in consilience, quantity is not all that matters. There may be very basic items in the background, perhaps because of their role in powerful explanations previously accepted. Rejecting them may reverberate through large parts of our worldview.

To sum up, consilience and analogy may be extrapolated to everyday contexts. Simplicity is an important factor in assessing explanatory goodness, but it is less appropriate to be understood in the same way in both kinds of contexts. Finally, conservatism is an added trans-contextual criterium. These general criteria prevent contextual relativism concerning explanatory goodness while allowing a role to further constraints related to specific contexts. In everyday life, it seems that general criteria may suffice because to ascertain what is a good explanation does not require an appeal to domain-specific principles. By contrast, in scientific contexts they have to be properly supplemented by further specific constraints.

It may be objected that a good explanation for the available evidence could turn into a bad explanation when further evidence is gathered, even though it *now* satisfies to a good extent the general criteria. After all, the rivals discarded now may be much better concerning future evidence.[20] In order to minimize this, *dynamic* consilience is required. As evidence increases and new classes of facts are discovered, new challenges arise. When the hypothesis successfully explains new kinds of evidence, it

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[20] Day and Kincaid 1994, footnote 26.

is dynamically consilient.[21] Of course, it is a logical possibility that future evidence could radically change in a way that the best explanation lost its consilience, and its simplicity (due to ad hoc adjustments to cope with novel facts, . . .). But, in spite of this, we may have very good grounds to believe it is highly unlikely that future evidence runs counter to the explanation favoured. Think, for instance, about the explanation of australian mammals' peculiarities, i.e., a long period of evolution in isolation from other mammals, as a consequence of the continental drift.

I claim that IBE is a justified pattern of reasoning iff the explanatory hypothesis inferred according to it fulfils the general criteria to certain extent and maximize the particular domain-specific constraints. The fulfilment of the criteria may be the decisive factor for believing it.[22] Certainly, it does not seem easy to work out an algorithm to assess the degree of explanatory goodness. The general criteria may even point at opposite directions (an explanation may clash with the background while enjoying a high degree of consilience, for instance). Anyway, if the afforded explanation does not fulfil any general criterium of explanatory goodness there is no justification, even if it fits with the available evidence. I guess that a hypothesis which violates all of them will be considered no explanation at all.

I admit that this is still a vague proposal. It would be desirable to detail the relations between the general criteria and the particular ones across different contexts (could the latter be opposed to the former?). Examples taken from actual scientific practice are also required and the list of general criteria may be implemented. But, in spite of all these shortcomings, an appeal to the general constraints on explanation urges for a new —and qualified— reassessment of IBE as a general pattern of reasoning. Besides, it gives some help to avoid some difficulties

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[21] Thagard 1978, p. 83.

[22] Stephen Leeds underlines the crucial influence of the informational —namely, explanatory— virtues upon the plausibility attributed to the General Theory of Relativity, even before it had confirmational support. See his 1994, p. 207.

of Psillos and Lipton's vindications of realism. Recall that they interpret IBE as follows:

"Infer the best of those explanations suggested by the background knowledge" (Psillos)

"Infer the top-ranked hypothesis" (Lipton)

I noticed that Psillos overstresses the privilege for the background. My proposal does fit better with the fact that sometimes we come to believe explanations which forces us to reject parts of the scientific background. Thus, conservatism is contained in explanatoriness, although it may be overriden by the remaining criteria even before decisive evidence is obtained. Concerning Lipton's views, I noticed that sometimes the best available explanation may be very poor and that fine-grained discriminations among top-ranked hypotheses are required. Then, even though there is no algorithmic procedure to assess explanatory goodness, justification could be, in principle, differentially attributed to top-ranked hypotheses so long as they fulfil the criteria of explanatory goodness to a different extent.

Lastly, the common assumption of Psillos and Lipton's replies —the decisive role played by the background knowledge— is not rejected. Conservatism and analogy do the job. I would emphasize that the background required to justify belief in good scientific explanations encompasses not only the best scientific theories but deeper layers in our cognitive machinery, that is, the set of criteria according to which we judge an explanation as a good one.

4. *Some Final Remarks about IBE and the Realist's Case*

The fulfilment of explanatory criteria is, on my view, a more fundamental cognitive aim than the mere coherence with the scientific background. But the fact that we do not perform inferences when criteria about explanatory goodness are not fulfiled is a fact concerning our inferential practice. From a realist point of view, however, justification has to be linked to a non-epistemic notion of truth. So, if IBE transmits justification through the criteria aforementioned in the way required by scientific realism,

it is due to an alleged link between the criteria and the truth. Insofar as this connection is not explicitly argued, belief in a good explanation may still be arbitrarily far from truth. Notice that this is not the argument from the bad lot but a more general qualm. The antirealist's concern now is not about the existence of better explanations outside the lot of available ones, but about the epistemic import of explanatory goodness. He now objects that, although "explanatoriness" is obtained within the lot, it does not lead to truth.[23]

To meet this challenge adequately would require an essay in its own right. Certainly, I have not offered a foundation for the criteria. Rather, I have tried to show that appealing to criteria of explanatory goodness makes some progress against the argument from the bad lot. I think also that we get a deeper insight into IBE, since the contextuality and pervasiveness of IBE may be better understood when we take notice of the operating criteria in our inferences. Besides, since explanatory criteria are supposedly more stable than explanations themselves, they constitute, in principle, a more suitable basis for the realist in order to account for turbulent periods in the historical record. The moral is that, in addition to offering more or less intuitive IBE-tailored arguments, realists fond of IBE should have to analyze the implicit standards of explanatory goodness.

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[23] Van Fraassen himself suscribes this objection. Both his pragmatic approach to explanation —1980, chapter 5— and a Dutch-book argument developed in his 1989, chapter 7, are addressed to the epistemic import attributed by realists to explanatory virtues. However, see Kitcher and Salmon (1987) for a criticism of van Fraassen's pragmatic account of explanation, and Day and Kincaid (1994) for a reply to his Dutch-book argument.

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