Reply to Zwirn & Zwirn

[David Miller]

I am indebted to Zwirn and Zwirn [1989] (hereafter Z&Z) for their extended and careful comments on the arguments of Popper & Miller [1983], [1987], and also for friendly and illuminating conversations. Their judgement seems to be that although Popper and I fail to make a satisfactory case for our conclusion that inductive probability is impossible, that conclusion is nonetheless defensible on quite other grounds. I don't really agree with this, as I shall explain.

The principal thesis of probabilistic inductivism is, presumably, that it is possible (and desirable) for evidence to exert a positive probabilistic influence on what is not known; that is, that $p(h, e)$ can exceed $p(h)$ when h is a hypothesis that goes beyond the evidence e. The paradigm case is said to be that in which e is deducible from h (and extreme probabilities are absent): we then invariably have $p(h) < p(h, e)$. But whereas inductivists see induction here, and (it seems) in all other cases where positive support is registered, Popper and I assert that the effect is a simple deductive one: e is not supporting what is unknown, but what is already known; it is doing no more than lending support to the part of h that it implies, namely $h \vee e$. Once we cut away from h this common part, leaving the genuinely novel content $h \leftarrow e$, we find that the effect of e is standardly to depress the probability (in extreme cases the effect is null); never to increase it: $p(h \leftarrow e, e) < p(h \leftarrow e)$. All non-deductive support, that is to say, is countersupport.

A good part of Z&Z is devoted to our identification of the excess content $Ex(h/e)$ of h over e with the material conditional $h \leftarrow e$. Since they take this to be the cardinal point of dispute, it is perhaps as well if some unimportant misunderstandings are cleared away first.

Our point always was that since $h \vee e$ is the common content of h and e, $Ex(h/e)$ has to be a proposition that shares no content with e and, in the presence of $h \vee e$, is equivalent to h. It follows easily that $Ex(h/e) = h \leftarrow e$. (*Proof*: Write x for $Ex(h/e)$. What is required of x is that $x \vee e$ be a tautology; that xe imply h; and that h imply x. It is immediate that x implies $h \leftarrow e$; moreover, $-e$ implies x, so $h \leftarrow e$ implies x. Thus $Ex(h/e) = x = h \leftarrow e$. The proof of uniqueness given in Z&Z, section IV.B, is unnecessarily| laboured.) Although $Ex(h/e)$, so |150

A comment on Zwirn & Zwirn (1989), first published in *Cahiers du CREA*, 14, 1990, pp. 149–153. Paris: École Polytechnique. Digitally remastered January 2008, with a small typographical correction (replacing a period by a comma) December 2010.

defined, is a truth function of h and e, it was a bit misleading of Jeffrey [1984] to suggest (see Z&Z, pp. 62, 70f.) that this was an additional requirement we placed on $Ex(h/e)$. By no means; $Ex(h/e)$ is fully determined by the above conditions alone. We mentioned also in [1983] that each of $h \vee e$ and $h \leftarrow e$ is the weakest proposition that, in the presence of the other, implies h; and that these two consequences of h are probabilistically independent when e is given. But in spite of what Z&Z claim on pp. 60f., it was never intended by us that $Ex(h/e)$ was to be obtained by taking any two propositions that are (a) each just strong enough, in the presence of the other, to yield h, and (b) probabilistically independent given e; and then discarding the factor [if there is one] that is deducible from e. It is obvious that this method of construction would generally not yield a unique result, since $h \vee f$ and $h \leftarrow f$ satisfy (a) for any f, and satisfy (b) also if f is implied by e. (That $h \vee e$ must be one of the two propositions into which h is factored is clear from the first full paragraph after formula (8) in our [1983]. That the factors must be maximally independent of each other [subcontraries] is clear from footnote 2 (p. 326) to $*15$ of Part II of Popper [1983]. For the utter irrelevance to this business of the condition of probabilistic independence, see p. 586 of our [1987].)

As far as I can see, the only firm objection that Z&Z have to our identification of $Ex(h/e)$ with $h \leftarrow e$ is this (p. 31): if h and k are competing hypotheses, each of which amplifies on the evidence e, then surely the amplifications $Ex(h/e)$ and $Ex(k/e)$ themselves ought to be in competition — that is, incompatible. But they are not: $Ex(h/e)$ and $Ex(k/e)$ do not contradict each other, since each of them is implied by $-e$. This is indeed so, and flickeringly surprising. But no more than that. In the presence of e, after all, $Ex(h/e)$ and $Ex(k/e)$ do contradict each other. The matter is perhaps best appreciated in reverse: if we start with the hypotheses $h \leftarrow e$ and $k \leftarrow e$, which disagree only conditionally, we shall not be astonished that they disagree unconditionally when the condition e for their disagreement obtains. If I avoid paying this week's rent, I can afford both a bottle of wine and a new hat; if I pay it, I can afford only one of them. The options *rent + wine* and *rent + hat* are therefore incompatible, even though the disbursements on which they differ are together perfectly compatible. Is this puzzling?

Our definition of $Ex(h/e)$ is not of course the only one that may be given, or even the only one tolerated by the simple spatial analogy of Z&Z, section V.A (pp. 69–72). (For an explanation of how this analogy may be inappropriately exploited — as, indeed, it seems to be by Z&Z — see p. 583 of our [1987].) But of the various definitions Z&Z contemplate ours is the only one that (i) identifies $Ex(h/e)$ with a proposition implied by h and (ii) is applicable to any but | the most primitive cases of |151 purportedly inductive inference [induction by simple enumeration]. In other words, anyone who seriously wants to hold that in real science evidence can positively support hypotheses that genuinely transcend it will have to characterize excess content in a way quite different from any proposed so far. (Mention should be made here of Mura [1987], which refines in an interesting manner the relation of complete independence.) That, we feel, is a problem for those who believe in induction, not for us.

Despairing of pinning down a satisfactory definition of $Ex(h/e)$ themselves, Z&Z proceed (pp. 73f.) to attack our view that the positive support (if any) that e gives to h is deductive in origin, being in reality deductive support for their common content $h \vee e$. At one point we are accused of arguing in a circle (that is, *validly*) where no argument as such, or demonstration, was ever intended. What we tried to do was not to justify or prove anything, but to explain why positive support for h evaporates when h has no content in common with e: that is, when $h \vee e$ is tautological. As far as I can see, Z&Z offer no alternative explanation of this curious fact. Instead, they dismiss our explanation on the extraordinary grounds that it collides with current usage: inductivist usage, needless to say (pp. 74f.). A change in terminology, they concede, can be valuable if it leads to a more sensible system of classification (such as not counting whales as fishes) but our proposed reform preserves none of the examples dignified by usage, and consequently it deserves to be rejected. To this it can be said only that we no more recommend a change in terminology than does a person who withdraws the label *miracle* from all those incidents once stigmatized as miracles. We are simply claiming that what has been so ostentatiously advertised as inductive support is not inductive support at all, but purely deductive. And as the titles of our [1983] and [1987] indicate, we are not committed to describing as inductive every relation between maximally independent propositions. On two occasions (pp. 74, 79) Z&Z insinuate that we have to call the falsification of $-e$ by e an inductive inference. This is playing with words.

Z&Z propose that the crucial idea of inductive logic is embodied in their principle of weak induction (P.I.F.), according to which evidence e reporting that *all observed objects possess property P* supports the hypothesis that *all objects possess P* more strongly than it supports any rival hypothesis about the unobserved objects (pp. 76f.): and they conclude, apparently with regret, that no such principle can be established from the probability axioms alone. This is scarcely to be wondered at. But it might be worth comparing P.I.F. with the principle of positive instantial relevance of Carnap [1971], section 13, which is proved by Humburg [1971] and Gaifman [1971] to hold under certain conditions. | |152

In conclusion (p. 79) Z&Z challenge us to explain the mysterious counterinductive character of the effect we have discovered; it must, they say, be as irrational to Popper as a positive inductive effect would be. I think not. The plain truth is that neither the probability function $p(h,e)$, nor the support function $s(h,e) = p(h,e) - p(h)$, adequately measures how much h depends deductively on e, as neither equals 0 whenever h and e are deductively independent. A much better measure is to be found in the function $q(h,e) = p(-e,-h)$, which, like $p(h,e)$, equals 1 if e implies h (for then $-h$ implies $-e$), but equals 0 if h and e are maximally independent. (The function q is investigated in detail in Miller & Popper [1986].) We suggest that inductivists have unwittingly conflated the functions s and q. It is $q(h,e)$, which increases monotonically as the content of e increases, and can be positive even when e and h contradict each other, that grades the backing or support that h receives from e; in contrast, $p(h,e)$ grades the unadventurousness of h given e, on a scale that changes as e changes. (Unless $p(e) = 0$, $p(h,e) = 1 - ct(h \leftarrow e)/p(e)$.) What $s(h,e)$ grades, except for $p(h,e) - p(h)$, is anybody's guess. It is absolutely characteristic of inductivism to imagine that the functions s and q are measures of the same thing. But they are not.

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*[The paper was published in *Philosophy of Science* **57**, 2, 1991, pp. 278–289.]

†[The correct concluding page number is 591.]