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### Commentary on Powers

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**In Response to:** Lawrence H. Powers' *Statistical Syllogistic, Part I*

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Powers' bold project is to get at fundamental concepts for informal and formal logic through an intense study of statistical syllogism. When we have a statistical syllogism the first premise states the frequency with which objects of a given kind have a property e.g. 90% of professors like Bach, the second premise states that an individual is a member of that kind e.g. Joan is a professor, and the conclusion is that this individual has the target property. Powers will then say that the conclusion is 90% valid relative to the premises. The project is to look at various versions of this kind of reasoning, and no one, so far as I know, has attempted to appreciate the complexities of the statistical syllogism, while relating it to broader issues of informal logic, as much as Powers.

There are two problems related to his choice of frequency probability (and related to each other) that I want to mention, although neither points to any defect in Powers' approach, only either clarification or an issue to be handled down the road.

First, Powers focuses on percents, rather than fractions. This raises no formal problems, nor any problems for his analysis, since trivially these are elementary school equivalents. But the focus, though generally advisable and often necessary (when, trivially, one does not know the number of cases) obscures a distinction that has too long been in the shadows. This is the distinction between the force of the evidence and what Keynes called the weight of the evidence. (The importance of weight of evidence in understanding induction and probability has been made most clear in Cohen 1977. In that work, he also takes on two issues that Powers discusses in his paper: convergence and the negation law for probability.) Keynes writes:

One argument has more weight than another if it is based upon a greater amount of relevant evidence;...It has a greater probability than another if the balance in its favour, of what evidence there is, is greater than the balance in favour of the argument with which we compare it...(Keynes, 1962: 77)

The crucial difference between these formats is that in a frequency format information is explicitly presented as to how many cases have been sampled (e.g. 80 out of 100 vs. 800 out of 1000), which is lost in a percent format (80%). But then the frequency format makes explicit a basis for assigning differences in weight of evidence and correlatively, differentials in confidence. So although the fractions  $8/10$ ,  $80/100$ , and  $800/1000$  are equal, the increasing number of cases sampled are overtly increases in the weight of evidence. Consequently, we can have much more confidence in the assigned probability in the latter than the former cases.

The second problem is a version of the treacherous reference class problem which Powers addresses, in part. One facet of the problem he does not address. Frequency probabilities are standardly defined as limits in a long or infinite run of experiments. The probability that the coin will land heads is  $2/3$  is really a poor way of expressing that in a long run of, say, a thousand flips around 660 will be heads. Strictly the original is a bad way of speaking because on a single toss the coin either lands heads or tails and the two-thirds probability is not a happy expression.

Now both of these problems are inter-related in recent discussions of the significance of psychological experiments on reasoning. In the most central of these studies on base rates, the

standard finding is that subjects do not integrate base rate information with standard, individuating or biographical data. Roughly, if you are told that a sample of biographies is of 70 lawyers and 30 engineers, and you are given a biographical sketch of someone who sounds like an engineer, you will judge it highly likely that the sketch is of an engineer. By so doing, you seem to ignore the base rates altogether. Now evolutionary psychologists have argued that these results occur because use of frequency data as percents rather than fractions are unfamiliar to our hunter-gatherer ancestors. Additionally, although this is not sufficiently emphasized, in fractional form it is possible to estimate the weight of the evidence. When the experiments are redesigned to accommodate these two factors subjects do considerably better. (See, for example, Gigerenzer and Hoffrage, 1995.) I do not think that this shows that the original experiments fail to expose a weakness or defect in our reasoning. But that's for another occasion. (For some brief remarks see Adler 2000).

Powers is very careful in circumscribing his project, but on one count I am not sure if he ought to be so modest. He tells us that he is concerned with "validities of arguments", not with "detachment". But if the idea is supposed to be that deductive validity will be the limiting case where the premises render the conclusion 100% valid, at least for the finite case (see p.4), there remains a significant difference. In the deductive case, detachment follows automatically with no further premises. But, for the less than 100% cases, Hempel and Carnap have taught us that detachment requires some further claim--premise--about the sample such as the evidence is the total available or that it is representative. (In a part of his paper that he did not read Powers distinguishes between statistical syllogisms and similar seeming inductive arguments and sampling arguments as well. But the ampliative nature of inductive argument does require the additional premise about the adequacy of the evidence, and I think this is necessary for not only its ampliative, but for its counterfactual, force that Powers also rightly notes.) I wonder what Powers makes of this disanalogy, and in general how he thinks about what we learn about all-out validity as the limiting case, if this is how he thinks of it.

His main claims in this paper concern the chain and convergence arguments. I have not examined these arguments with the care that they deserve, so I will not comment.

### ***References***

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