Section IV. Reply to Kyle Stanford

To put Kyle's criticism of what I've said about the atomic hypothesis into perspective, let me say schematically how my argument about scientific realism goes. It has been widespread in the realism-anti-realism debate to appeal to claims about how evidence works without investigating what evidence is directly. I investigate the claims that empiricists and realists make about evidence *directly* using confirmation theory, and find first that the crucial claims are measure-dependent; whether they're true depends on how you measure the *degree* of confirmation. On further investigation it turns out that Constructive Empiricism and other empiricist-style anti-realisms aren't coherent according to *any* familiar probabilistic measure of confirmation. However, the Likelihood Ratio makes this point in a way that's most congenial to the arguments that historically the realist is inclined to make. So that's the measure the realist should embrace for his own best interest. (Chapter 5 provided independent reasons for this choice, but the current point doesn't depend on those.) Once we do that we see that this standard of evidence has not as yet been fulfilled for any high-level theory. So one of the standards that defeats the anti-realist also undermines the claim realists tend to make. That said, it's just a standard, and the standard can't say that we will never meet it. For that we'll have to wait and see. This is one part of my view that distinguishes me sharply from the traditional anti-realist.

The other part that distinguishes me from traditional anti-realists is that the Likelihood Ratio used in my approach makes no mention of observable vs. unobservable--so this distinction is not per se relevant in my view. However, one wonders whether it might just happen to yield the same line for where our achievements have *so far* given

out that the empiricist thought was where they *had* to give out. I give the pregnancy test example, and the examples of blood tests for disease, to show that we have already got beyond the observable/unobservable line, so the answer is 'no'; that doesn't even *happen* to be the line. At this point my view about realism and anti-realism is done and defended, and what we may think about the atomic hypothesis or any particular case doesn't matter to it. My discussion of Perrin and atoms is what I think the facts about that case and my standard of evidence imply, namely that we have lent significant confirmation in particular to the modest hypothesis that there are atoms (which is not a piece of high theory but is a hypothesis beyond observables). However, nothing about my general view stands or falls with the verdict on this particular case. So, it's inappropriate to call this a flagship case for me. Nevertheless, have taken a stand so I owe a defense of it.

Kyle disagrees with the stand I take that Perrin's experiments with gamboges particles, combined with all subsequent experiments on the atomic/molecular level (he leaves out that part), lend significant absolute confirmation to the modest atomic hypothesis that there are spatially discrete subvisible entities moving at random. He cites the possibility that what explains the random motion of the gamboges particles that Perrin confirmed is that they are uncaused. Now, Fine is clearly right that this hypothesis was available at the time, but Kyle's claim that this opens up a yawning chasm where I saw a razor-thin gap is false. I didn't treat that gap as razor-thin, since in my calculation I assumed that the probability that something other than the atomic hypothesis explains the random walk is fifty per cent. Even on that assumption, I got a posterior probability of 60% for the atomic hypothesis out of Perrin's experiment alone. To make the alternative hypotheses undermine my modest claim they're going to have to have a greater than 50%

probability of explaining the evidence. (Note that 50/50 seems a fair distribution over the alternatives: uncaused vs. caused by randomly moving entities.)

I'm flattered that Kyle assimilates my view to his own approach in terms of unconceived conceivable possibilities, but there are reasons why it's fundamentally wrong to do that. Kyle likes the likelihood ratio because it's hard to evaluate the denominator--the probability of the evidence on the catch-all-- for theories, and that makes life difficult for the realist. However, he conveniently ignores other aspects of the Likelihood Ratio approach. For example, that it is not concerned per se with whether scientists have *imagined* seriatim all of the alternative hypotheses that are imaginable. First, the set of *conceiv**able*** possibilities isn't big enough. All logical possibilities incompatible with the hypothesis matter to the probability of the evidence on the catch-all, not just the ones ***we*** could *conceive* given infinite time. On the other hand, as I explain in the book--and this is the saving grace--*conception* of a hypothesis is not necessary for ruling it out and thereby taking a step forward in the evaluation of the likelihood ratio. We rule out hypotheses *without* conceiving of them when we test a pregnancy or blood test on a large random sample of the relevant population, because we don't have to know what all the variations in the relevant population are in order to know that all that matter are represented and tested.

Similarly, advances in statistics allowed Perrin to take the study of the atomic hypothesis to a dramatically, and qualitatively new level of generality because it allowed him to rise above the seriatim imagination of alternative hypotheses. Perrin's predecessors were ruling out hypotheses about causes of the Brownian motion that came from outside the gamboges preparation *one by one* as they conceived of them, including

vibrations, light, temperature gradients, magnetism, electricity, shaking, evaporation, air currents, capillarity, motions of the observer's hands, etc. Perrin ruled out these hypotheses and every hypothesis like them because using statistical models he could rule out large *categories* of hypothesis, corresponding to broadly described *types* of causal mechanism. He didn't have to conceive of the individual hypotheses in those categories in order to rule them out when their characteristic behavior didn't match that of the Brownian motion.

In this sense, partly because of the development of statistics, scientists have gotten much better over the last 150 years at evaluating the probability of the evidence on the catch-all, and thereby the logical space of alternative hypotheses. This causes trouble for any pessimistic induction over the history of science because such an argument always depends on our not being able to say how we are doing better than our predecessors with regard to alternative hypotheses. But this is a clear sense in which we have done and *are* doing better. We didn't have and couldn't have had the e. p. t. pregnancy test a hundred years ago. That required a kind of control of alternative hypotheses that we do now have. And we have no reason to think we can't get even better.