



## DIALOGUES

### Should Investigations of Consciousness Wait for Better Theory?

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Di Francesco and Marraffa (2013) performed a well-organized exploration of the literature concerned with consciousness. They described how interest in the issue dates back to ancient Greek philosophers, and continues to be of interest. Researchers invest impressive amounts of resources into investigating the issue. My goal is to question whether this is optimal.

Let me commence by clarifying what I do not argue. First, I do not argue that consciousness is uninteresting; on the contrary, it is extremely interesting as the huge literature that has been devoted to it testifies. But that a topic is interesting does not by itself, justify large expenditures of resources. Many are interested in celebrities but it would be difficult to justify large expenditures of research resources on that.

Second, I do not argue that the main issues have been solved. As Di Francesco and Marraffa (2013) made clear, they have not been solved. But the fact that the main issues have not been solved does not, by itself, justify large expenditures to solve them. There is no guarantee that such effort actually will work. Given the past history of such research, it seems difficult to argue convincingly that success is likely in a reasonable amount of time.

To see where I am coming from, it is worthwhile to consider briefly the history of physics, arguably the most successful of all of the sciences. In the two millennia between Aristotle and Galileo, physics largely was stagnant—not because people failed to try various ideas (But-

terfield, 1957)—but because a strong theoretical base had not been proposed. I am not using “strong” in the sense of “correct,” as there are Galilean statements that were not correct. Rather, I am using “strong” in the sense that it provided an excellent foundation for progress. Wolfson (2003) has documented the remarkable effect that Galilean relativity had on the subsequent development of physics, up to, and including, Einstein’s relativity.

Is there a strong theoretical basis for research on the consciousness issue? There certainly is a better, though still weak, methodological basis in the form of improved neurophysiological procedures. But these methodological improvements have thus far failed to result in an impressive improvement at the theoretical level.

It seems unlikely that a strong theory is likely to come from a focus on consciousness because, with such a narrow focus, it is not clear what the general principles are or even where to look for them. It seems more likely that a strong theory will come from a more general interest in how the brain works (or how the mind works), or perhaps a still more general interest in biology. A strong theory might even come from such seemingly unconnected areas as mathematics or physics where the basic principles are clearer.

Well, then, if we learn the Galilean lesson that scientific progress depends on strong theory, it follows naturally that resources for scientific investigations should be focused on areas where strong theory is more likely as opposed to less

likely to develop. Given that despite much research effort, there is a lack of strong theory in the consciousness area, it seems difficult to argue plausibly that more research effort will reverse matters. It might be that progress in understanding the issue of consciousness will be quicker if the indirect route is taken of looking for general principles first, and only then using those principles to investigate consciousness. Sometimes the highway, circuitous as it might be, gets you

to your destination in less time than taking the direct surface road.

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