II) Handmaiden, version 2: Philosophy serves the purpose of furthering scientific discoveries. This is the model implicitly adopted by Weinberg, Hawking, Krauss, and deGrasse Tyson in the quotations mentioned above. It explains why—in the same interview—Tyson went on to say: "Philosophy has basically parted ways from the frontier of the physical sciences, when there was a day when they were one and the same. … So, I'm disappointed because there is a lot of brainpower there, that might have otherwise contributed mightily, but today simply does not. It's not that there can't be other philosophical subjects, there is religious philosophy [sic], and ethical philosophy, and political philosophy, plenty of stuff for the philosophers to do, but the frontier of the physical sciences does not appear to be among them." (Levine 2014)

III) Separate Magisteria: Science studies the world, while philosophy (of science) studies science. This has largely, though not exclusively, been the model within philosophy of science since the famous debate between John Stuart Mill and the above-mentioned Whewell on the nature of induction (Forster 2009). It has also characterized the "golden era" of philosophy of science, from logical positivism at the beginning of the twentieth century to Popper, Kuhn, Lakatos, and Feyerabend (Godfrey-Smith 2003), when philosophers of science were preoccupied with producing grand theories of the nature of science.

IV) Overlapping Magisteria: Science studies the world and philosophy of science studies science, but there are mutually beneficial areas of overlap. As I mentioned above, there are a number of these areas that are frequently addressed by both scientists and philosophers, including, but not limited to: species concepts in biology (refs. above); definition and uses of "gene" (Hall 2001; Griffiths and Stotz 2013); discussions of the concept of race (Pigliucci and Kaplan 2003; Hacking 2006); the nature of evolutionary theory (Pigliucci and Müller 2010; Laland et al. 2015), epistemic limits of evolutionary psychology, medical research, neuroscience, and social science (Kaplan 2000; Fine 2017); neuroscience of consciousness (Rose 2006; Block 2007); metaphysical interpretations of quantum mechanics (Barandes and Kagan 2014); desirability of a "post-empirical" science vis-à-vis string theory (refs. above); and several others.

It seems to me fairly clear that the most viable model in contemporary science and philosophy of science is (IV) above. Model (I), as noted, did characterize much of the history of philosophy and of natural philosophy/science, but gradually—beginning with the scientific revolution of the seventeenth and eighteenth centuries—different sciences have successfully emancipated themselves from philosophy. Physics first, then chemistry, biology, and finally social sciences such as economics and psychology. The cognitive study of consciousness is the latest entry in the catalog, gradually but fairly rapidly cutting its umbilical cord from philosophy of mind.

Model (II), as we have seen, is apparently popular among certain scientists, who use it to demonstrate that "obviously" philosophy is dead or irrelevant. But insofar as I can tell, that view of the relationship between the two fields is a fact-free construct of their own

minds, with no counterpart in either the historical or the current literature in philosophy of science.

Model (III) does describe what happens most of the time in academic journals: the majority of philosophy of science papers treat science as an external object, to be studied and understood from a distance, so to speak; while the overwhelming majority of science papers simply don't mention philosophy (and take philosophical baggage on board unexamined, as famously observed by Dan Dennett 1996, 21).

Model (IV) has the advantage of accommodating the standard practices described by (III), and yet of leaving enough room for science to directly contribute to some philosophical disputes, as well as for philosophy to help scientists clarify certain issues at the boundaries of their discipline, or when problems are sufficiently underdetermined by the empirical evidence (Stanford 2013) as to accommodate alternative philosophical interpretations.

In the following, I will briefly examine four examples of debates about causality and interpret them as instances of model IV at work. These are: the discussion concerning the distinction between proximate and ultimate causes in biology; the issue of (alleged) top-down causality, in both physics and biology; the nature of functional explanation in biology and the social sciences; and the broad debate over the very nature of causality. I will then attempt to draw some general lessons, hopefully illuminating both my concern about the relationship between science and philosophy, as well as some aspects of the specific scholarship in philosophy on the nature of causality. A fairly large caveat needs to be kept in mind throughout the following, however: I am not attempting a general review of the huge field of the philosophy of causation. That would be unwieldy, redundant with currently available entries in that literature, and especially besides the main point of my analysis, which is focused on using causality as a conduit for a better understanding of the science–philosophy debate. In particular, I will not be discussing causal graphs, which present a semantic approach to causal analysis, where the ontology is implied (Woodward 2016), or interventionist accounts of causality, which are practical, not ontological in nature (Duncan 1976; Morgan and Winship 2007)—characteristics that explain why both of these approaches are especially popular among social scientists and statisticians.

Example I: Proximate vs. Ultimate Causes in Biology

In evolutionary biology, the distinction between so-called proximate and ultimate causes has been common practice since its introduction by Ernst Mayr (1961), one of the most influential figures of twentieth-century biology. Recently, however, the distinction has been under sustained attack in philosophy of science, and for good reasons (Ariew 2003; Amundson 2005; Haig 2013; Laland et al. 2012).

Mayr introduced it in order to explain why developmental biology had famously been left out of the Modern Synthesis of the 1930s and 1940s (Mayr 1993): "The functional biologist is vitally concerned with the operation and interaction of structural elements, from molecules up to organs and whole individuals. His ever-repeated question is 'How?'... [The evolutionary biologist's] basic question is 'Why?'... To find the causes for the existing characteristics, and particularly adaptations, of organisms is [his] main preoccupation" (Mayr 1961, 1502).

A number of years later, he elaborated: "The suggestion that it is the task of the Darwinians to explain development... makes it evident that Ho and Saunders [critics of the Modern Synthesis] are unaware of the important difference between proximate and ultimate causations.... Expressed in modern terminology, ultimate causations (largely natural selection) are those involved in the assembling of new genetic programmes, and proximate causations those that deal with the decoding of the genetic programme during ontogeny and subsequent life" (Mayr 1984, 1262).

As mentioned above, however, a number of recent authors have critiqued the sharpness of Mayr's clean separation of proximate and ultimate causes. For instance, Amundson (2005, 176) wrote: "In order to achieve a modification in adult form, evolution must modify the embryological processes responsible for that form. Therefore an understanding of evolution requires an understanding of development." Laland et al. (2011, 1512) explain: "In reciprocal processes, ultimate explanations must include an account of the sources of selection (as these are modified by the evolutionary process) as well as the causes of the phenotypes subject to selection."

So far, the two contrasting pictures are on the one hand of ultimate causes acting across generations and proximate causes acting within generations (Mayr), and on the other hand of no meaningful distinction at all to be drawn between the two sorts of causes, in the name of a principle of causal completeness (Mayr's critics, especially Amundson).

Building on these recent re-analyses, Raphael Scholl and I (Scholl and Pigliucci 2014) have proposed to strike a balance between Mayr and his most harsh critics (in line with my more general suggestion that the Extended Evolutionary Synthesis is an expansion, but not a radical revision or rejection, of the MS [Pigliucci and Müller 2010, 3–19]). Our thesis is that it is a mistake to think about the proximate–ultimate distinction in terms of allegedly omitted biological causes, as some of Mayr's critics do. It is much more fruitful to think in terms of the necessary partiality of causal explanations: the issue is not whether certain types of causation (e.g., between genotype and phenotype, or between phenotype and selective environment) exist, but whether these causal paths carry much weight in the explanations we give. For each individual case, we should ask what motivates the foregrounding or backgrounding of some parts of a complete causal account of any given evolutionary transition. The key concern is not causal relevance, in other words, but explanatory salience.

Our approach builds on S. J. Gould's (1985) point that developmental processes sometimes (Most of the time? How frequently?) are not isotropic, as they have classically assumed to be within the Modern Synthesis. When they are, then developmental causes