



Revisiting the Basic/Applied Science Distinction: The Significance of Urgent Science for Science Funding Policy

Jamie Shaw¹

Accepted: 27 June 2021 / Published online: 28 January 2022
© The Author(s), under exclusive licence to Springer Nature B.V. 2022

Abstract

There has been a resurgence between two closely related discussions concerning modern science funding policy. The first revolves around the coherence and usefulness of the distinction between basic and applied science and the second concerns whether science should be free to pursue research according to its own internal standards or pursue socially responsible research agendas that are held accountable to moral or political standards. In this paper, I argue that the distinction between basic and applied science, and the concomitant debate about freedom and social responsibility, require revision. I contend that the distinction can only be maintained in cases of *urgent science*. I go on to elucidate the notion of urgent science using a case study from research of the climate refugee crisis.

Keywords Basic and applied science · Freedom of science · Socially responsible science · Urgent science · Climate refugee crisis

Preamble

Whether scientists should be free to set their own priorities or guided towards socially desirable ends has been an important debate in the formation of modern science funding policy. Oftentimes, this debate has been entangled with one concerning the aims of science: should science aim at understanding nature or socially relevant knowledge? In more common parlance, should scientists seek *basic* or *applied science*? If we accept the former, some contend, scientists should be free to pursue whatever research is likely to be important by their internal methodological standards. Scientists should be left alone to do science. If we accept the latter, others claim, science should be directed by exogenous interest groups to ensure the social relevance of their work. To complicate things, many have argued that the pursuit of basic science predictably leads to gains in applied science while some have argued for the opposite view, namely, that basic science regularly emerges out of applied science. This makes the connection between the aims of science and the kinds of projects we fund more intricate than it may appear at first glance. In this paper, I hope

✉ Jamie Shaw
jshaw222@uwo.ca

¹ Institute for History and Philosophy of Science and Technology, SSHRC Postdoctoral Fellow, Toronto, Canada

to shed light on this topic by rethinking the basic/applied distinction and its implications for science funding policy. Specifically, I argue that the terms ‘basic’ and ‘applied’ can be coherently used only within cases of urgent science. By doing so, I also hope to show the limitations of debates concerning the freedom of science and provide a preliminary framework for identifying urgent science. I briefly discuss cases of research on the climate refugee crisis to elucidate the complexities contained therein.

The structure of this paper is as follows. In Sect. 1, I clarify the relationship between freedom and social responsibility and the basic/applied distinction. Since the former debate is not entirely predicated on the latter, I specify the precise sense in which the basic/applied distinction is thought to have implications for freedom and social responsibility. In Sect. 2, I consider two criticisms of the basic/applied distinction. In Sect. 3 I reframe the ways in which we should approach the basic/applied distinction. Following this, I show what implications this revised distinction has for science funding policy and debates about the freedom of science. Lastly, in Sect. 4, I briefly describe some of the funding considerations in the sciences on the climate refugee crisis to elucidate the notion of urgent science.

1 The Freedom of Science and the Basic/Applied Distinction

There have been many formulations of the basic/applied distinction. Indeed, the term “basic science” has a difficult history where some use it interchangeably with “pure science”, “fundamental science”, or “basic research”, while others see these terms as conceptually distinct. Similarly, the term “applied science” is sometimes equated with technology, itself a nebulous term, even though there are many applications science may have (see Kline 1995). More broadly, as Désirée Schauz (2014) has demonstrated, the constitutive terms and the basic/applied distinction has been put to many uses in different contexts. Still, I think, there exists a common core for the analytic purposes the basic applied distinction is meant to serve. It is this analytic function I wish to assess.

One famous description of basic science, echoed by many, comes from J.J. Thompson where basic science is “made without any idea of application to industrial matters but solely with the view of extending our knowledge of the Laws of Nature” (quoted in Rayleigh 1942, 198). Basic science is usually evaluated in terms of its significance. As Kitcher (2001) points out, basic science is not simply *any* knowledge of the world, such as the gravitational force my pinky toe exerts on Jupiter, but *significant* knowledge. Philosophers have debated what counts as ‘significant’ for millennia. Thompson mentioned one above: knowledge is significant if it extends our understanding of the laws of nature. To give another example, Poincaré offers a distinct criterion through his notion of simple facts:

We all know that there are good experiments and poor ones. The latter accumulate in vain; whether there are a hundred or a thousand, a single piece of work by a real master, a Pasteur for instance, suffices to make them fall into obscurity... What then is a good experiment? It is one which teaches us something more than an isolated fact; it aids us to predict, and enables us to generalize... [Hence,] it is necessary that each experiment should allow the greatest possible number of predictions... The problem is, so to speak, to increase the efficiency of the scientific machine (Poincaré 1902, 517–518).

Whether Thompson, Poincaré, or someone else is correct does not concern me here. It may be the case that there is no general notion of significance, and criteria vary from domain to domain and across history. The salient point is that basic science should be evaluated by some notion of *epistemic significance*. Basic science can be evaluated as better or worse insofar as it offers a coherent proposal that aims to advance the frontiers of knowledge in some important respect.

Applied science, on the other hand, aims towards practical ends. That is, “in applied science proper applications are singled out by other than purely cognitive goals” thus giving rise to an “extra-scientific criterion of relevance for what counts as an answer or a good answer” (Sintonen 1990, 24). While, historically speaking, applications have been narrowly construed as marketable goods or technology, there are a wide variety of kinds of applications that basic science could have such as medical treatments, uptake in legal norms, policy-design, influences on the arts or popular culture, uses in education, or prompting reimaginings of who we are and our place in nature (see Toulmin 1965; Jasanoff 2015). Applied science can be more or less significant depending on what problems the application solves, how it solves it, or what novel problems it creates. In any case, our criteria for what counts as significant applied science will require non-epistemic values.

At times, the definitions of basic and applied science refer to the *intentions* of the researchers (see Bud 2012). We often hear the basic/applied distinction referring to the ‘motivation’ of the research. For the purposes of science funding policy, though, this characterization is unhelpful. Grant reviewers are uninterested in whether researchers *think* their research is basic or applied but what the research actually is. A more ‘objective’ definition has also been frequently offered instead:

- *Basic Science*: Significant scientific knowledge that advances our understanding of nature.
- *Applied Science*: Significant scientific knowledge that informs practical applications.¹

These definitions approximate those employed by science funding agencies across the world. This distinction does not focus on the motivations of researchers but characterizes the purported value of the research products. Moreover, these definitions refer to the *aims* of the research proposal and not the *research process itself*, as is often suggested by terms such as basic or applied *research*.

The use of the term ‘science’ may evoke the ghosts of paltry attempts to demarcate science, be it basic or applied, from other intellectual ventures. Sometimes, this provides the motivation behind more generic expressions such as “basic (or applied) research.” The chimeric demarcation problem philosophers have traditionally preoccupied themselves with is often translated on the ground level into the establishment of boundary conditions (Gieryn 1983). While there are many clear-cut cases of scientific ventures, there are also many situations where it is not clear whether or to what extent a particular research proposal can be called ‘scientific.’ For example, as Solovey (2020) documents, the ‘scientific’ status of many of the social sciences was highly contentious in the United States during the Cold War and this affected the availability of funds for research in their fields. There is very little that can be said in general about how to establish boundary conditions. Establishing boundary conditions is an often highly (local) political process and should not be fixed in

¹ This approximates Niiniluoto’s (2013, 267ff) formulation.

advance. For example, debates in the U.S. during the Cold War about whether sociology was ‘scientific’ was attached to questions whether it deserved funds from the National Science Foundation, when there were few other American institutions dedicated to supporting sociological research, the polarized political climate during the Cold War, the perception that many social sciences were progressive ideologies in disguise, and so forth. It also depended on the stage of theoretical development of various social sciences and their relationships to the biological and hard sciences. Managing these boundary conditions required attention to these details and should not be determined by a context-independent criterion of what counts as ‘science.’ How these boundary conditions are established in particular cases will play a role in determining what projects count as basic or applied *science*, but this does not impact the coherence or usefulness of the basic/applied distinction so long as it is consciously used within these limits. In other words, an all-things-considered approach to assessing what counts as basic or applied science will require settling boundary conditions. But once these are fixed, in whatever way, we still must distinguish between what counts as basic or applied. This will be my focus here.

It is often argued that the basic/applied distinction, when combined with a view of the proper aims of science, has profound implications for the freedom of scientists to set their own research agendas. Oftentimes we hear two conflicting arguments that connect the *goals* of research, be they basic or applied, to the *organization of science* as either free or directed by exogenous groups. A historically common argument for the freedom of science runs like this:²

Argument for the Freedom of Science:

F1: The goal of the sciences is to advance basic science.

F2: Basic science is best pursued when science is free.

∴ Science should be free.

Both premises have multiple lines of defense, making an in-depth summary of either argument multifarious. Some defend F1 on the grounds that scientific knowledge is intrinsically valuable. This view has been defended by Plato, Aristotle, Russell³, Moore, and many others.⁴ These arguments are also frequently within the context of science funding policy.⁵ Alex Stern, for example, argued for the “intrinsic goodness” of scientific knowledge such that “the pursuit of truth and the passion for understanding give a dignity and nobility to man” (Stern 1944, 356).⁶ Another argument, which will become clearer later on, is that basic science is necessary for the development and justification of applied science. On this view, scientific knowledge is justified instrumentally due to its role in promoting welfare more broadly.

The argument for F2 is often presented in an ambiguous manner. Even though freedom is often presented as ‘non-interference’, more sophisticated pleas for the freedom of

² A similar framing is provided by Kitcher (2001, 109).

³ Though Russell’s view is more complicated, since he argues that the pursuit of knowledge is valuable even where knowledge is not possible. As such, we could imagine Russell supporting the pursuit of basic science even if the goal of attaining is unlikely or impossible (see Russell 1912, ch. 15).

⁴ See Douglas (2014) and Kvanvig (2003) for more detailed discussions.

⁵ Plato and Aristotle’s arguments for the intrinsic value of knowledge have been raised in the context of debates about science funding policy by Bernal (1939, 4) and Holbrook (2018, 27) respectively.

⁶ For similar sentiments, see the quotations collected in Douglas (2014, 57–58) and Wilholt (2006a, 254–255).

science are not *mere* demands for the right of scientists to operate in isolation from other parts of society. Consider the following claim by Lakatos:

In my view, science, as such, has no social responsibility. In my view it is society that has a responsibility—that of maintaining the apolitical, detached scientific tradition and allowing science *to search for truth in the way determined purely by its inner life*. Of course scientists, as citizens, have responsibility, like all other citizens, to see that science is *applied* to the right social and political ends. This is a different, independent question, and, in my opinion one which ought to be determined through Parliament (Lakatos 1978, italics added, 258).

The purpose of the freedom of science is not to protect scientists from doing *whatever they want*, but to provide scientists the intellectual space necessary to pursue knowledge *according to accepted scientific standards*. An exemplar of this attitude can be found at the heart of the manifesto of the ‘Slow Science’ movement:

We do need time to think. We do need time to digest. We do need time to misunderstand each other, especially when fostering lost dialogue between humanities and natural sciences. We cannot continuously tell you what our science means; what it will be good for; because we simply don’t know yet. Science needs time (The Slow Science Academy 2010).

No reasonable defender of the freedom of science would insist that science should be free if scientists habitually committed fraud, used public funds to buy sport cars (that were not necessary for their research), outwardly projecting false racist or sexist tropes, or some other practice that did not advance knowledge of the world. Leaving scientists alone does not guarantee that truthful norms, rather than other kinds of norms, will be followed.⁷ Rather, as Polanyi put it, freedom concerns the “freedom of the systematic branches of science *to pursue their own scientific aims*” (Polanyi 1940, 9).⁸ Merely being free *from* external forces does not guarantee that scientists will pursue these aims (see Shaw 2021a for more detail on this issue as it appears in Feyerabend’s thought).

Rather, the freedom of science is more charitably characterized as the freedom of *properly structured scientific communities*. It is the thesis that scientists need a peculiar kind of the intellectual space to advance scientific knowledge according to the standards of the sciences themselves (see Reydon 2019). Debates about what those standards should be are fierce and seemingly unending. Philosophers of science used to boldly claim that they had uncovered such standards to impose upon scientists to ensure the security of their epistemic products. Nowadays, most claim that there are many standards across the sciences, while others deny that scientific standards can be put forward verbally. Regardless, these debates are orthogonal to the view that the sciences have peculiar tasks required of them and which should be their sole focus (within the constraints of the law and standard ethical oversight committees).

On the other side of the aisle, there is a similar argument for a socially responsible science:⁹

⁷ That science will self-organize in truth-productive ways is a questionable hypothesis with many moving parts. See Butos and Koppl (2003), Kummerfeld and Zollman (2015), and Smaldino and McElreath (2016) for empirically grounded analyses and Laudan (1981, ch. 14) for a more philosophical engagement.

⁸ Polanyi notes exemptions where practical and scientific projects are interwoven (see Polanyi 1956, 232–233). However, these collaborations are only required at the scientist’s discretion that this will advance *her* task of furthering scientific knowledge.

⁹ To be clear, the phrase ‘socially responsible science’ sometimes applies to the view that scientists should do no (or minimize) harm to society (e.g., Douglas 2003). I am using the phrase in the sense that scientists should aim to achieve goals that are, in some sense, accountable to society.

Argument for Socially Responsible Science

SR1: The goal of the sciences is to advance applied science.

SR2: Applied science is best pursued when science is directed.

∴ Science should be directed.

Proponents of SR1 contend that the immediate instrumental value of science is what makes it an activity worthy of public funds. Otherwise, as it was before science became a proper profession in the late nineteenth century, science should remain a hobby and largely be funded privately.¹⁰ Some, such as J.D. Bernal, justify the practical aims of science on Marxist grounds. This argument attacks the very coherence of the notion of basic science—all science is applied science, properly understood. As Marx states,

The mode of production of the material means of life determines, in general, the social, political, and intellectual processes of life. It is not the consciousness of human beings that determines their existence, but, on the contrary, it is their social existence that determines their consciousness (Marx 1977, preface).

Scientific reasoning, however ‘basic’ it may appear, is genuinely determined by the material conditions under which scientists attempt to thrive. Science is already a practical endeavor, one whose essence must be understood by looking at its economic functions. Bernal argues that the history of science confirms this theory: “the most elementary reading of the history of science shows that both the drive which led to scientific discoveries and the means by which those discoveries were made were material needs and material instruments” (Bernal 1939, 6).¹¹ The goals of science are *already practical*; to deny this is to engage in a false consciousness about the purpose of science. Related arguments could be extracted from pragmatists who argued that science properly done is simply an extension of practical problem-solving inquiries. The search for knowledge with no practical impetus is to be repudiated as idle. As Dewey put it, “‘Science’ is converted into knowledge in its honorable and emphatic sense only in application. Otherwise it is truncated, blind, distorted” (Dewey 1927/1954, 174).

A more popular line of defense for P1 is grounded in democratic considerations; if science is publicly funded, then it should serve the public. The public, therefore, gets a say in the priorities of scientific inquiries (Kitcher 2001). According to this view, scientific institutions should just be like all other public institutions and held accountable to public wishes. Of course, the public *may* choose to fund science for its own sake. Philosophers could certainly imagine dreamworlds where discoveries about the origins of mass, for example, trump all other intellectual pursuits. Regardless of how realistic such scenarios may be, funding for research just because it is scientifically interesting could not be expected as a matter of course. The argument from democracy usually, though not necessarily or exclusively, supports P1 insofar as the public has needs and desires that science can help address.¹² While each of these arguments for P1 are interesting and deserve

¹⁰ Although, hobbies are also often funded publicly simply because some citizens find them enjoyable. This would justify publicly funded science in the same way that we justify public funding for basketball courts. However, this justification would likely only allow for an extremely frugal budget that would not allow much of modern science to proceed without stable private sources of supplementary income. For an interesting, related anecdote, see Gadagkar (2011).

¹¹ See Kuhn (1971) for a criticism of this approach.

¹² Often, the satisfaction of curiosity is mentioned side-by-side with the claim that scientific knowledge is intrinsically valuable. It seems to me that the satisfaction of curiosity is an instrumental reason for pursuing research, but I will leave this point aside for now.

attention in their own right, they are not my focus since they all arrive at the same conclusion: publicly funded scientific knowledge is only instrumentally valuable.

The argument for P2 is fairly straightforward once we accept P1. If we agree that science should aim at practical knowledge, it should aim at the *right kinds* of practical knowledge. How we should discern what counts as the 'right kinds' is rife with controversy. Kitcher, for example, provides a model of deliberative democracy, where scientists and citizens discuss what those goals should be, and those goals determine the kinds of research that are engaged in (Kitcher 2004). Others provide straightforwardly moral arguments. Consider the arguments of AIDS activists on the composition of clinical trials (Epstein 1996). Here, the internal scientific standards dictated that trials should eliminate as many confounding factors as possible. This, according to traditional double-blind methods, is necessary to securely establish causal relationships. However, these trials do not provide any immediately applicable treatments for the majority of people suffering from AIDS, who normally have other comorbidities. Some activists argued that we have a moral responsibility to conduct trials differently to provide desperately needed treatments that are admittedly less reliable, rather than allowing people to suffer until a sufficiently comprehensive causal framework is available. A different moral argument comes from Reiss and Kitcher (2009, 43), who argue that biomedical research should be allocated according to the "fair share" principle where "the proportions of global resources assigned to different diseases should agree with the ratios of human suffering associated with those diseases." The upshot of these arguments is the same: science should be directed in accordance with particular non-epistemic values.

Before moving on, it is important to note that there are arguments for the freedom of science and socially responsible science that do not involve the basic/applied distinction. Torsten Wilholt nicely summarizes one such argument where the freedom of science is essential for the functioning of democracy:

It is in essence a political argument and arises from the consideration that scientific knowledge has become an important input for the democratic process. In making their political choices, citizens are in many ways relying on their beliefs about what the world is like, and ever so often they turn to science in order to resolve uncertainties. On the basis of this observation, it can be argued that the practices and institutions generating the scientific knowledge that citizens rely upon should enjoy independence from the major political powers. Otherwise, the democratic process would be undermined, in a similar fashion as it would be if the press, for example, was subject to the control of the government (Wilholt 2010, 177).¹³

Elsewhere, he discusses a distinct argument where the freedom of science safeguards continuous funding across regime changes which is necessary for long-term, politically controversial research projects like space exploration or stem cell research (Wilholt 2006a). A parallel argument for socially responsible science suggests that science should be held accountable as all democratic public institutions should be. On this view, publicly funded science must "view the relationship between government (respectively the public) and science as one between contractors, where the principal (i.e., the public) must somehow make sure that the agent (science) pursues the delegated task rather than his own interests" (Wilholt and Glimell 2010, 355). Of course, other arguments could be mentioned. The pertinent point here is that since these arguments aren't tethered to the basic/applied distinction,

¹³ This argument was also enunciated by Polanyi (1940, 11). See also Wilholt (2006a, 238).

arguments about the proper aims of science cannot settle the question of whether science should be free or socially responsible.

2 Problems With the Basic/Applied Distinction

Many are skeptical that there is a coherent distinction between basic and applied science. This has led to many claiming that the terms “basic” and “applied” research are merely rhetorical terms signaling ideological allegiances, rather than useful analytic concepts (e.g., Tijssen 2010). Jean Calvert, for example, argues that the terms merely facilitate “boundary work by actors to gain authority and resources” (Calvert 2006, 200).¹⁴ Others claim that the distinction was valid, once upon a time, but has since become obsolete. The director of the Office of Science and Technology under George W. Bush, John Marburger III, for example, had this to say:

Globalization and changing modes of science that have blurred disciplinary distinctions have undermined the value of traditional science and engineering data and their conventional interpretations. The old budget categories of basic and applied R&D, still tracked by the U.S. Office of Management and Budget, do not come close to capturing information about the highly interdisciplinary activities thought to fuel innovation... More attention, however, is needed to definitions and models that suit current needs of policy (Marburger III 2005, 1087).

If no distinction can be made, then P1 in each argument makes no sense. While the literature on this topic is massive, two common objections can be extracted. The first goes like this: every research result is a mixture of basic and applied and therefore there is no such thing as pure ‘basic’ or ‘applied’ science. In other words, “Critics of the traditional basic/applied distinction tend to hold that supporters imply separation and isolation of two kinds of scientific activity. And since this is an unrealistic description both of present and history the distinction is obviously invalid” (Roll-Hansen 2017, 536). Moreover, a sharp distinction disguises the mutual dependencies between basic and applied science:

It is sometimes claimed that this distinction [basic/applied] cannot be upheld within present-day science, either because hardly any branch of today’s science is completely bereft of practical relevance, or because contemporary scientific research is so heavily dependent on technology that applied and basic science have become inextricably interwoven (Adam et al. 2006, 437).

This claim has been confirmed by recent studies that has found that most studies, across disciplines, are mixtures of basic and applied science (Bentley et al. 2015). Some research falls on extreme ends of the spectrum. The discovery of the Higgs Boson substantially advanced our knowledge of the origins of mass but has little practical applicability. The ability to transport the COVID-19 vaccine at moderately cold temperatures was enormously important, practically speaking, but we did not learn anything fundamental in virology (or anything else) as a result (at least not yet). However, these cases are typically

¹⁴ Roger Pielke (2012) has argued that the term “basic science” can be understood as a political symbol. This is compatible with the idea that it is *also* a useful analytic concept. Elsewhere, though, he claims that the term “hinders productive debate on science policy” (Pielke and Byerly 1998, 43).

outliers. If this is right, it is difficult to identify what research would be uniquely supported by a particular argument for the aims of science.

This criticism, despite its prominence, does not pose a serious challenge for us here although it may well pose problems for descriptive studies. The debate about the aims of science concerns what is *valuable* about scientific research. These debates are normative, not descriptive. We can happily accept that research may have basic and applied components, and debates about aims determine *which* components make the research *worthy of support*. Consider the example of the isolation of the *poliovirus* in 1908. There are basic components to this discovery. For example, it provided a missing link in the evolution of viruses. But the implication, realized by Jonas Salk in 1953, that a vaccine for polio could be developed is what makes it worthy of funds from a social responsibility perspective. If we hold that the aims of science are applied, then the discovery of the missing link is relatively unimportant. If the aims are basic, then the implications for a polio vaccine are a happy side-effect that should not affect its evaluation. Of course, despite the simplistic dichotomy we are often presented with, no major science funding body in the world exclusively supports basic or applied science. Totalizing arguments for the aims of science are rare in practice (and for good reason! – see below). However, this only adds the admittedly challenging complication of how to balance these conflicting goals (see Quaglione et al. 2015). It is this difficulty that many science policy makers and scholars, such as Marburger III, struggle with when collecting data to construct models of epistemic growth. While this complication is extremely difficult to manage in practice and deserves further attention in its own right, it is irrelevant for the arguments under consideration here.

A second, more theoretically troublesome, challenge may be called the ‘problem of change.’ Since characterizing knowledge as ‘basic’ or ‘applied’ (or more or less significant) changes as our epistemic (and sometimes social) situation changes, we cannot identify at a given point whether a piece of research is (primarily) basic or applied. Without being able to identify what counts as basic or applied, the distinction becomes useless. The history of science and technology is replete with examples of basic science that became applied or applied science that eventually led to fundamental insights. In the early twentieth century, Max Planck’s interest in blackbody radiation was significant due to its importance for the quantization of light. This discovery would have profound implications for atomic physics and quantum mechanics. In 1902, his research would be characterized as basic. However, by the late twentieth century, Planck’s research provided the foundations for crucial parameters in climate models. Planck’s research is, now, basic *and* applied. On the other hand, in WWII, technicians struggled to interpret radar images and communications with noisy signals. This constituted a practical problem. However, approximately 20 years later, reflections upon this research on these topics led to detection theory which became influential in psychology and psychophysics (see also Wilholt 2006b). In either case, the identification of research changed significantly as research went on. This is important since a proponent of the socially responsible argument would not have supported Planck’s research and vice versa for proponents of the freedom of science for research on radio signals.¹⁵

¹⁵ More often than not, proponents of the freedom of science would retort that the research on radio signals should be supported but by different institutional means (e.g., Bush 1945). It is (some) proponents of socially responsible science who tend to be more restrictive and claim that we should not support basic science.

The most common solution to the problem of change is to argue for predictable patterns of the downstream consequences of basic or applied science. If these patterns exist, we can characterize the aims of a proposal as basic or applied and anticipate its future payoffs within our calculus. One model of such patterns, frequently called the ‘linear model’, states that applied science is an eventual consequence of basic science.¹⁶ If we pursue basic science, we will oftentimes accumulate applied science as an offshoot of the initial basic discoveries. This view was famously defended by Vannevar Bush:

Discoveries pertinent to medical progress have often come from remote and unexpected sources, and it is certain that this will be true in the future. It is wholly probable that progress in the treatment of cardiovascular disease, renal disease, cancer, and similar refractory diseases will be made as the result of fundamental discoveries in subjects unrelated to those diseases, and perhaps entirely unexpected by the investigator. Further progress requires that the entire front of medicine and the underlying sciences of chemistry, physics, anatomy, biochemistry, physiology, pharmacology, bacteriology, pathology, parasitology, etc., be broadly developed (Bush 1945, 237).

This view has sometimes been codified in philosophical conceptions of basic science where knowledge (of the world) is power (to reach desirable goals).¹⁷ Others defend the ‘emergent model’ which “takes technology development to proceed in large measure independent of science” (Adam et al. 2006, 437).¹⁸ Proponents of this view contend the reverse of the linear model: basic science emerges out of applied science. For some, this is because nature just is a local patchwork of facts, so the best descriptions of nature (basic science) would necessarily involve detailed descriptions of isolated systems:

The phenomena in different areas are rich in their details and distinctive in their nature. The emergentist contention is that, for this reason, the phenomena escape the grip of highbrow theories that merely address the generic features of the situation. Descriptive adequacy is only accomplished by small-scale accounts; comprehensive approaches lose touch with the wealth of the phenomena (438; see also Carrier 2006).¹⁹

If this is the case, basic and applied science will often go hand-in-hand. But one does not need to hold this metaphysical picture to accept the emergentist account of scientific progress. For a different example, consider Donald Stokes’ infamous depiction of Louis Pasteur:

The problem of deriving alcohol from beet juice makes this point well. Pasteur’s work on this problem is... a distinguished example of applied science, a highly suc-

¹⁶ See Egerton (2004) and Godin (2006) for informative, though skeptical, overviews of the history of the linear model, especially Bush’s views.

¹⁷ On my view, this association is misleading. Views that knowledge is power, such as those in Bacon or Spinoza, concern the *logical* priority of basic science rather than its *historical* priority (though see Carrier 2006, 15–16).

¹⁸ Notice that this employs a narrow understanding of ‘applied science’ which equates applications with technology.

¹⁹ Adam et al. (2006) hold what they call ‘moderate emergentism’ which is more of an interactive approach than the linear model or emergentism. On this view “it is granted to emergentism that applied science may give rise to genuinely new knowledge that is adjusted to the relevant local circumstances. On the other hand, it is conceded to the cascade model that the knowledge gained is not purely local” (443).

cessful effort to improve the technology of fermentation. But [this research] was at the same time a distinguished example of basic science. This blend characterized virtually the whole of Pasteur's later career. He probed ever more deeply into the processes of microbiology by accepting applied problems... (Stokes 1997, 13).

While Pasteur's work was geared towards an understanding of pasteurization, it also revealed basic insights into plant structure and botany and was foundational for edaphology for years to come. While much mission-oriented research, or research that "draw[s] on frontier knowledge to attain specific [concrete] goals" (Mazzucato 2018, 804), has applications as its primary goal, it also has spawned basic science down the road (see McCarty 1984; Kealey & Al-Ubaydli 2000, 7).

If we accept the linear model, then the following argument becomes attractive:

Argument for the Freedom of Science (Take 2)

F1: The goal of the sciences is to advance applied science.

F2: Applied science is best pursued when science is free.

∴ Science should be free.

On the other hand, if we accept the emergentist model, then a different argument becomes plausible:

Argument for Socially Responsible Science (Take 2)

SR1: The goal of the sciences is to advance basic science.

SR2: Basic science is best pursued when science is directed.²⁰

∴ Science should be directed.

These arguments allow us to continue to use the basic/applied distinction coherently while recognizing that our knowledge of how these categories may be applied can change. They are also attractive since they allow proponents of conflicting axiologies to agree upon whether science should be free or directed. Indeed, the former argument offered a politically appealing "social contract" for science that provided scientists their desired freedom while promising society useful knowledge for their investments (Guston 2000).

While these are not the only models under consideration,²¹ the debate has reached a stalemate (despite the heavy rhetorical insinuations to the contrary). There are many examples to support both views. What is more troubling, though, is that they both generalize from a small group of case studies to a more general view that basic and applied science evolve.²² This generalization involves the *assumption* that knowledge has and will continue to grow in predictable ways—that there are patterns that these models latch onto that will reliably repeat themselves. This assumption seems false for numerous reasons (see Feyerabend 1975;

²⁰ This premise, though, assumes that applied ends themselves should not be pursued without particular purposes in mind. See Pirtle and Szajnarfar (2017, fn. 9, 105) and Drexler (2013) for further discussion.

²¹ See Kline and Rosenberg (1986), Gibbons et al. (1994), Etkowitz & Leydesdorff (2000), and Akrich et al. (2002) for additional models.

²² Systematic studies on the relationship(s) between basic and applied science are rare, and even more rarely cited. One relatively famous study was Project Hindsight which found little correlation between basic science and military defense technologies. However, as the counter-study by the NSF demonstrated, this study relied on a historically narrow definition of basic science (which was ambiguous in other respects) (see Pirtle and Moore 2019). It is also difficult see how these results can be generalized, even if valid, to other kinds of applied science, within different institutional settings, different research cultures, and so

Gomory 1995; Acuna et al. 2012; Penner et al. 2013; Shaw 2021b). As Polanyi writes, “It is difficult enough to see how society can do anything to adjust what is admittedly unpredictable, to the service of its welfare” (Polanyi 1940, 18). Many have thought that the unpredictability of the growth of knowledge lends support to the freedom of science:²³

Prior judgments about the fruitfulness of research projects are generally fallible. Even projects that hold little promise of success from the point of view of the current scientific mainstream may therefore turn out to be groundbreaking. Consequently, scientists should choose their approaches and projects freely, such that a wide variety of approaches end up being pursued. Some of them will prevail and lead to new knowledge, but it is impossible at any time to predict which ones these will be (Wilholt 2006a, 261).

However, the converse is correct as well: we cannot tell in advance whether practical research will lead to fundamental discoveries. Most proponents of the unpredictability argument are also proponents of the linear model, but these two views come apart. One can acknowledge that basic discoveries can emerge from practical tasks in unpredictable ways. If we hold that both models are wrong to assume that we can predict how basic or applied science will evolve, then our conception of the aims of science has no implications for what sorts of projects will realize those aims. If we have no argument to pursue basic or applied science, then the arguments for freedom or social responsibility that follow therefrom are baseless. In plain terms, if knowledge grows in unpredictable ways, then no argument for the proper aims of science can support the view that science should be free or socially responsible.

The upshot is significant: the problem of change makes it impossible to identify which research proposals have basic or applied aims. What we fund may seem like basic science but, as our knowledge changes,²⁴ turns out to be applied. The opposite may be true as well. Therefore, it is not obvious, from the perspective of any science funding body at a given point in time, what implications any given piece of research has. Basic science may perpetuate crucial applied advances and applied science may spawn foundational insights. This poses a serious problem for the applicability of the basic/applied distinction. In the next section, I will address this criticism by revising our conventional understanding of the basic/applied distinction.

3 Revising the Basic/Applied Distinction

The traditional conception of the basic/applied distinction is problematic if the growth of knowledge is unpredictable. This problem can be addressed, however, by taking a closer look at the unpredictability argument. The examples that are lauded for it usually involve *long term projections*. Take Polanyi’s example as a typical one:

It is generally accepted that in the last 40 years physics have advanced on a scale which is unsurpassed in any previous period of similar length. This advance has, no

Footnote 22 (continued)

on. Some studies look at citation patterns from patents to study basic/applied dynamics, but these studies are limited in what they can tell us (see Pavitt 1998 and Meyer 2000 for further discussion). See Salter and Martin (2001) for a somewhat different approach to the problem. This is not to say that a systematic approach to the problem is impossible but has yet been done in a satisfactory manner and would be extremely difficult.

²³ Though this point is disputed (see Bedessem and Ruphy 2019).

²⁴ Things get more complicated once we consider radical theory change, as it is unclear what value contributing to a “graveyard of theories” truly has. See Patton (2015) for related thoughts.

doubt, enlarged the outlook of industrial physicists and has in many unspecifiable ways and has assisted them in their inventive tasks. But it seems to me that the only invention which may be said to have arisen directly from this era of discoveries is the modern discharge lamp which is now coming into use for the illumination of roads. Now the theory which has been utilised for this invention was built up between 1900 and 1912 in a series of giant strokes by Planck, Einstein, Rutherford and Bohr. Suppose then that ‘the socialised, integrated, scientific world organisation’... would have existed in 1900, with its ‘unified and co-ordinated, and above all, conscious control of the whole of social life’. How would this organisation have ‘adjusted’ the inclination of Planck, Einstein, Rutherford and Bohr to discover the atomic theory to the increased need for street lighting which was to arise 20 years later in connection with the popular use of motor cars, undreamed of in 1900? Would scientific world control have foreseen, not merely this future need but also the fact that it might be satisfied by a discharge lamp based on the discoveries which were about to be made? And, then the crucial question. Supposing the likely case that the scientific world controllers would not have performed this miracle of foresight, would they then have had to reduce their support of the investigations which were leading to the discovery of atomic structure? (Polanyi 1940, 18-19).²⁵

The lack of our predictive powers concerning the practical importance of the atomic theory is due to the series of changes within science and society including the development of roads, cars, electric wiring, mass production of tungsten, amongst many other things. These changes took decades and several distinct social forces to occur. Changes in our epistemic and/or societal situations rarely happen overnight, nor do they happen in straightforward ways. These changes give rise to new epistemic and practical underpinnings necessary for assessing research as “basic” or “applied”, “significant” or “insignificant.” Now, let us compare Polanyi’s example to predictions about whether we could build a cost-effective battery for commercial planes. Given what we know and current institutional arrangements, we can reasonably say this result is possible within 5 years. There are many factors that play into the confidence in this prediction: heavy safety regulations on plane travel restrict ‘DIY’ approaches thus limiting alternatives that may defuse the value of battery-driven engines, large-scale battery constructions are at a relatively advanced stage of development, no new basic aeronautical knowledge would be needed to accommodate the additional weight, the necessary chemical compounds are abundant (both locally and from importing countries with low tariffs), there is some economic demand for them and future markets will not drastically change over five years, and so on. However, if we were asking this question 20 years ago, the research would have seemed much more speculative and unclear. I conjecture that this result generalizes: projections about the likelihood of success are more reliable in short-term scenarios²⁶ but not in long-term cases.

From this, it is a small step to the view that in times of *urgency* the problem of unpredictability becomes relatively muted. Conversely, unpredictability is the norm when we are looking at long-term research. Therefore, the problem of change does not pose a problem for the applicability of the basic/applied distinction in cases of urgent science though it

²⁵ See Polanyi (1951) for detailed discussion.

²⁶ Though see Fujimora (1988, 263) for the claim that even short-sighted changes in research can be unpredictable. In a rough meta-study I conducted, it seems as if scientists’ abilities to predict future research dramatically decreases after roughly 5 years (Shaw 2022). See also Kondo (1999) for related discussion.

remains formidable when we look at the growth of knowledge in the long-term.²⁷ In the latter case, we can concede that we do not know how this or that research project, be it ‘basic’ or ‘applied’ from our current perspective, will turn out. In these cases, the basic/applied distinction is useless. In short-term research, though, the basic/applied distinction can be valuable when combined with the arguments for the appropriate aims of a particular research project. While NASAs ‘nuking the sky’ program of spraying various chemicals to increase the albedo effect on cloud tops is applied in its aims, would we gain fundamental knowledge in, say, meteorology as a result? Only time and future research will tell.

Many grant applicants claim that their research is urgently needed. Usually, the term ‘urgency’ denotes the need of particular research results with relative expedience. However, this straightforward categorization of urgent science is unsatisfactory and far too simplistic. It is not enough to *need* knowledge urgently, but we need to know that we *can* realize those results within the specified timeframe. That is, that the infrastructure, institutional connections, personnel, etc. that are necessary for completing the project are available and that there are no insurmountable legal, material, or social restrictions that would impede its progress. As a preliminary account of urgent science, I offer three conditions for characterizing a research project as ‘urgent’:

- (1) The Feasibility Condition
- (2) The Epistemic Condition
- (3) The Moral Condition

By presenting things in this fashion, I do not wish to suggest that these conditions are separable from each other. As will become clear, they are deeply intermixed. The feasibility condition is satisfied when we *can* pursue a research project. Research projects can require training new researchers, mobilizing infrastructure, hospitable political climates, the availability of particular equipment, and so forth. These all contribute to the feasibility of a piece of research.²⁸ Moreover, there are various practical deadlines that impact scientist’s ability to carry out research: researchers have limited contracts at particular locations, they want to publish in a timely fashion, government budgets must be balanced on an annual basis, some research requires legal changes to take place (e.g., psychedelics in therapy), and so forth. Decision-makers must consider these day-to-day formalities to know whether the goals of a research project can be realized within a specified timeframe. Feasibility considerations have evaluative and epistemic dimensions as well. For example, increasing government expenditures during the ‘booms’ of business cycles is justified on the Keynesian grounds that this will minimize economic depressions. Even if this is right, perhaps we are willing to go through a period of depression, or increase debts, if the promised goals are significant enough. This highlights how complicated and intertwined these conditions can be. The bottom line is that we must know whether we have the resources to pursue this or that research before we should say that we should.²⁹

²⁷ Of course, the difference between long-term and short-term research is one of degree. So-called ‘moon-shot’ research projects incorporate elements of both where they give extremely coarse-grained goals that can be interpreted in many incompatible ways with loose criteria for what research follows from those goals.

²⁸ See Gardiner (2016) for a focused discussion on the political dimensions of feasibility debates concerning action on climate change.

²⁹ Considerations such as these are entirely absent from Kitcher and Reiss’ (2009) recommendations for reorienting medical funding.

The epistemic condition constitutes whether or not we can predictably obtain the desired knowledge within the allotted timeframe. This involves evaluating the state of the field and identifying possibilities that lie on the research horizon. Consider the following hypothetical scenario.³⁰ Imagine Trump wants to create a bomb that, when detonated, will exclusively damage foreign spies. Trump wants this bomb by next Saturday because that is when he conjectures that the spies will have left the Pentagon with valuable information. Trump then demands that the scientific community constructs such a weapon that can be used by next Saturday. In this case, such a device cannot realistically be made within that timeframe. Perhaps in the distant future, this device could be invented—but certainly not by next Saturday. In this case, the knowledge is needed urgently (for Trump), but it cannot impact science funding policy as the knowledge needed to accomplish the goals of that research is not available within the specified timeframe. A related issue may concern the *reliability* with which a result can be assured. Since reliability comes in degrees, and establishing reliability itself takes a great deal of effort (see Carrier 2017), whether or not we can come up with a reliable *enough* result in a specified timeframe is another dimension of the epistemic (and possibly moral) condition.³¹ Similar points could be raised for other theoretical virtues (i.e., making theories or models more accurate, simpler, etc.).

Finally, there is the moral condition of when knowledge is needed urgently. Oftentimes, the argument that a piece of research is urgently needed is justified on the grounds that it alleviates intermittent suffering or is necessary for preventing future harms. The kinds of considerations at stake are complicated and require addressing difficult moral questions such as the moral status of future generations (Massimi 2020; Mulgan 2008), pursuing research with unknown possible consequences (Lenman 2000; Logan 2009), balancing incommensurable goals (Chang 2014), and identifying for whom the research must be deemed to be valuable (see e.g., Klopprogge & van der Sluijs 2006). The appeals to the moral urgency will be complicated in practice. All I want to claim for now is that moral questions such as these must be answered to determine whether a piece of knowledge is urgently needed or not (or whether it is *comparatively more* urgent than alternative research). Moreover, the criterion of urgency redirects the role of non-epistemic values in determining the pursuitworthiness of a given piece of research. The social responsibility approach contends that non-epistemic values should directly determine what (feasible) research should be prioritized. But if I am right, this approach only makes sense in urgent cases making the value of urgency paramount and other value judgments are only helpful within urgent cases.

The idea of a ‘timeline’ suggested by the concept of urgent science often requires an assessment of the *speed* at which research can be produced. Research speed is often a function of the size of research communities, communication networks, bureaucratic green lights, institutional seals of approval, and so on.³² When we are looking at urgent science, we are often concerned with the possibility of mobilizing research resources adequately by a specific date. Sometimes, research speed is limited by chronological time (e.g., a

³⁰ This example is due to Chris Smeenk.

³¹ In an off-hand remark from Lakatos, he claims that “If two teams, pursuing rival research programmes compete, the one with the most creative talent is more likely to succeed... the direction of science is determined primarily by human creative imagination and not by the universe of facts, that surrounds us” (Lakatos 1970, 158). This suggests that the epistemic criterion reduces to the feasibility criterion. I will leave this as a topic for future research to settle.

³² There are interesting differences between long-term research that requires greater degrees of institutional flexibility, and short-term research within the *in-situ* environments (see North 1995; Davis 2010).

particular amount of days, months, years, etc.) in cases required longitudinal experiments where a specified amount of chronological time must pass. For example, the “Building a New Life in Australia” research program which focuses on the settlement of humanitarians in Australia required collecting data over a 5-year period. This study could not be expedited without sacrificing the integrity of the study—as long as the study is necessary or methodologically appropriate, we must wait for 5 years to pass. Here, chronological time limits the speed of the research. But in many other cases, what we are genuinely concerned with is research speed.

Whether a piece of research can be characterized as urgent science changes our ability to assess whether research is basic or applied. Because of this, the notion of urgent science is essential for the applicability of the debate about freedom or social responsibility insofar as either view is grounded in a view of the proper aims of science. Actual analyses of individual cases will be incredibly complicated, involving a wide variety of values and contextual resources to assess. As an implication of this, a piece of research may be urgent and not urgent in different contexts (see below). This complexity, though, can still be guided by the framework offered here. In the next section, I will elucidate this framework further through the example of research on the climate refugee crisis.

4 Case Study on Climate Refugees

The impacts of climate change, environmental and otherwise, are being felt now. Coastlines are flooding, desert regions are limiting land-use, and increased extreme weather events are causing all kinds of destruction. More alarmingly, recent research suggests that we are approaching large-scale irreversible tipping points with the melting of the Antarctic and Greenland ice sheets and the subsequent release of trapped methane. In this section, I will focus on the example of the current climate refugee crisis to both illustrate the framework provided in the previous section and illuminate complexities with how it may work in practice.

Millions of people have been forced to evacuate their homelands as a result of climate change. Accommodating refugees in a responsible way requires a great deal of research on the social, economic, and political consequences of allocating refugees amongst different countries and determining who counts as a climate refugee. While few would deny that this is a morally pressing issue, since the well-being of the refugees and their potential host communities are put at greater risk without these studies, whether it meets the moral criteria may vary from funding agency to funding agency depending on the comparative urgency of this research and its national relevance. Take Sri Lanka as an example. Its primary science funding body is the Ministry of Science and Technology (MS&T) that operates with a miniscule 0.17% of net GDP (which is already extremely low). Moreover, many of the supercomputers needed to run simulations on complex immigration models are not readily accessible to researchers at Sri Lankan institutions. Additionally, Sri Lanka is not likely to be a host country of climate refugees (the opposite will likely be the case, see De Silva et al. 2007) so the research is not needed for their national agenda. There are also many more pressing issues facing Sri Lankans that require large amounts of research, such as food scarcity, extreme coastal flooding, and increased monsoons. To the MS&T, the climate refugee crisis does not seem to be an urgent problem. The opposite very well may be the case for countries in different geopolitical and economic situations, such as the United

States or Germany, suggesting that the crisis is an urgent science for, say, the National Science Foundation (NSF) or the Energie und Klimafonds.

Assessing whether the feasibility condition is satisfied is particularly difficult since the refugee crisis is an international issue. While particular funding bodies will have their own local agendas that may take precedence, the possible cooperation of dozens of major funding bodies such as the European Research Council, NSF, the Japan Science and Technology Agency, and many ad hoc multilateral institutional arrangements allows for a plurality of research centers and loci of institutional support for developing knowledge relevant to the placement of refugees. Moreover, large amounts of the research require migration and/or immigration models; the data comes from demographic studies which, for the most part, have already been done (see Hugo 2013). Some of these models require supercomputers, which limit the amount and ability of researchers to contribute. However, beyond this the infrastructure needed to develop these models is relatively minimal especially given increased international communications and networking facilitating globally shared expertise and collaboration. While fractures of open science and distrust between some countries create obstacles to these collaborations, they appear to be few enough that individual funding agencies that are rich enough and have a sufficiently well-situated labor force can afford to promote research on the climate refugee crisis even if it is not directly relevant to the host countries national priorities. While this analysis is admittedly superficial, it provides a semblance of whether the feasibility condition can be satisfied.³³

Whether the epistemic condition is met is difficult to assess due to numerous layers of fierce disagreement. There is a debate surrounding which countries should bear the responsibility of taking on larger portions of refugees. Some argue that historically prominent global emitters should be held responsible and therefore proportionately contribute to pooled funds for managing damages (Donhauser 2017; see also Frisch 2012). Others argue that we should forgo questions of responsibility and offer a consequentialist approach whereby we should place refugees in whatever countries can best support them (Docherty & Giannini 2009). Answers to this moral question determine what research is relevant. If the former is accepted, then we should consult models that determine which emissions were likely responsible for particular local events (or series of events) that forced migration (i.e., ‘attribution models’) (see Hulme 2014). Some, such as Myles Allen, argue that this entails that we “urgently [need] to develop the science base to be able to distinguish genuine impacts of climate change from unfortunate consequences of bad weather” (quoted in Gillies 2011). If the latter is accepted, knowing whether causes of particular weather events were anthropogenic in origin is irrelevant; we should focus on ‘best fit’ analyses to see where climate refugees should settle:

Whether a particular risk event was triggered by human or natural meteorology, there is an ethical imperative to build social resilience and institutional capacity to deal with all weather-related risks. The crucial point is that climate adaptation investment is most needed where vulnerability to meteorological hazard is high, not where meteorological hazards are most attributable to human influence (Hulme et al. 2011, 765).³⁴

³³ Some of these considerations are mentioned, *en passant*, in Ferris (2011).

³⁴ For a general oversight of the ethical issues involved in weather attribution models, see Thompson and Otto (2015).

In the case of climate refugees, we need to know what countries can host unprecedented levels of immigration, how particular immigrant groups would adapt to and change political, social, and economic dynamics in their host countries, and the reasoned preferences of the climate refugees. According to some, this is the kind of research that is morally required.

Research on weather attribution models is controversial.³⁵ While it is generally agreed that there are difficulties with them, whether they meet the epistemic criteria for urgent science will depend on our assessment of how numerous, fundamental, or legitimate we take these difficulties to be. Given the range and levels of disagreement on the viability of weather attribution models, it is unclear whether the epistemic criterion is met. However, as mentioned, given the global interest and possible venues of support for this research, perhaps whatever ‘challenges’ they face can be overcome relatively quickly. On the other hand, research on the latter is also needed, since existing models for immigration focus on controlled, consistent, and low levels of incoming peoples which are not suited for the climate refugee crisis (see Barnett & Webber 2009). However, as previously mentioned, much of the data is already available making the development of these models relatively cost-effective. In the former case, therefore, there is disagreement about whether the epistemic condition is met. In the latter case, it seems as if the epistemic condition is met.

This case study clearly underlines the interdependence of the criteria listed in the previous section. However, there are a few additional wrinkles that are brought out by this example. Since the climate refugee crisis is an international development, it brings new considerations to the forefront such as international collaboration and data sharing. More experimental sciences would struggle in these situations given national competition limiting usage of rare equipment and the cost and feasibility of large amounts of travel needed for the hands-on collaboration. Since the research needed for the climate refugee crisis is largely the construction of models, these struggles are relatively moot in this context. This would not be the case in other kinds of international collaborations on, say, geoenvironmental projects that require sharing materials. This case study also brings out the importance of disagreement when assessing whether the epistemic condition can be satisfied. The literature disagreement is vast, and I do not have the space to discuss them here (see Beatty 2006; de Melo-Martín & Intemann 2018; Shaw 2020 for starters). However, this literature focuses on questions of what to *accept* in light of disagreement. In this case, we are not only interested in disagreement about the viability of weather attribution models themselves but also whether obstacles, insofar as they are genuine obstacles, to their viability can be overcome in the near future. This is a distinctive kind of disagreement about pursuitworthiness, rather than acceptance. It requires assessments of what criticisms are legitimate, how serious those criticisms are, and an optimism or pessimism about whether those criticisms can be overcome.

I do not doubt that a closer look at the climate refugee crisis or other case studies will reveal additional complexities and nuances for identifying urgent science. Here, I hope to have taken the first step in the direction of a larger task. Perhaps our understanding of urgent science will have to remain general and leave the complexity to questions about whether this or that research is urgent in a particular case. Regardless, I will leave such issues for future research.

³⁵ See the report on weather attribution models from the National Academy of Sciences, Engineering, and Medicine (2016) for some details.

5 Concluding Remarks

Despite the fact that science funding decisions are massively complicated with seemingly endless amounts of context-sensitive considerations, this does not mean that we cannot carefully design science funding policies. Funding bodies are normally so overwhelmed with day-to-day exigencies, more abstract considerations and their downward implications are rarely given the attention they deserve. Philosophers of science can play an important and much needed role here. However, philosophers must also be careful that their analyses are not so detached that they lose sight of how issues in science funding policy play out in practice. My hope is to have taken the first steps in this direction by articulating the beginnings of a framework that is both articulate enough to assist science funding bodies and flexible enough to accommodate the many context-sensitive factors that govern their functionality on the ground. This represents the first step towards creating philosophically informed and morally justifiable means of distributing funds in scientific communities. Hulme et al., (2011, 764) rightly claim that “allocation principles for... climate adaptation funds remain both underdeveloped and politically contested.” Given this, I hope the framework suggested in this paper can contribute to this need.

Acknowledgements An early version of this paper was presented at Queen’s University Colloquium Series. I would like to thank the audience (in particular, Josh Mozersky, Sergio Sismondo, David Bakhurst, Will Kymlicka, and Jared Houston) for their helpful feedback. Thanks also go to Kareem Khalifa, Justin Donhauser and 3 anonymous reviewers. This paper is dedicated to the memory of D.L.C. (Lorne) Maclachlan who also commented on a presentation of this paper with his usual exuberant charm.

References

- Acuna, D., S. Allesina, and K. Kording. 2012. Predicting Scientific Success. *Nature* 489(7415): 201–202.
- Adam, M., M. Carrier, and T. Wilholt. 2006. How to serve the customer and still be truthful: methodological characteristics of applied research. *Science and Public Policy* 33(6): 435–444.
- Akrich, M., M. Callon, B. Latour, and A. Monaghan. 2002. The Key to Success in Innovation Part II: The Art of Choosing Good Spokespersons. *International Journal of Innovation Management* 6(02): 207–225.
- Barnett, J., and M. Webber. 2009. Accommodating Migration to Promote Adaptation to Climate Change. *A Policy Brief Prepared for the Secretariat of the Swedish Commission on Climate Change and Development and the World Bank Report 2010 Team*. <http://www.ccdcommission.org/Filet/documents/Accommodating%20Migration.pdf>.
- Beatty, J. 2006. Masking Disagreement Among Experts. *Episteme* 3(1–2): 52–67.
- Bedessem, B., and S. Rupy. 2019. Scientific Autonomy and the Unpredictability of Scientific Inquiry: The Unexpected Might Not be Where You Would Expect. *Studies in History and Philosophy of Science Part A* 73: 1–7.
- Bentley, P., M. Gulbrandsen, and S. Kyvik. 2015. The Relationship Between Basic and Applied science in Universities. *Higher Education* 70(4): 689–709.
- Bernal, J. 1939. *The Social Function of Science*. London: George Routledge & Sons Ltd.
- Bormmann, L., R. Mutz, and H. Daniel. 2010. A Reliability-Generalization Study of Journal Peer Reviews: A Multilevel Meta-Analysis of Inter-rater Reliability and its Determinants. *PLoS ONE* 5(12): e14331.
- Bud, R. 2012. “Applied Science”: A Phrase in Search of a Meaning. *Isis* 103(3): 537–545.
- Bush, V. 1945. Science: The Endless Frontier. *Transactions of the Kansas Academy of Science (1903-)* 48(3): 231–264.
- Butos, W., and R. Koppl. 2003. Science as a Spontaneous Order. In *The Evolution of Scientific Knowledge*, ed. H. Jensen, L. Richter, and M. Vendelo, 164–188. Northampton: Edward Elgar.
- Calvert, J. 2006. What’s Special About Basic Science? *Science, Technology, & Human Values* 31(2): 199–220.

- Carrier, M. 2006. The Challenge of Practice: Einstein, Technological Development and Conceptual Innovation. In *Special Relativity*, ed. J. Ehlers and C. Lämmerzahl, 15–31. Berlin: Springer.
- Carrier, M. 2017. Facing the Credibility Crisis of Science: On the Ambivalent Role of Pluralism in Establishing Relevance and Reliability. *Perspectives on Science* 25(4): 439–464.
- Chang, R. 2014. *Making Comparisons Count*. New York: Routledge.
- Davis, L. 2010. Institutional Flexibility and Economic Growth. *Journal of Comparative Economics* 38(3): 306–320.
- de Melo-Martín, I., and K. Intemann. 2018. *The Fight Against Doubt: How to Bridge the Gap Between Scientists and the Public*. Oxford: Oxford University Press.
- De Silva, C., E. Weatherhead, J. Knox, and J. Rodriguez-Diaz. 2007. Predicting the Impacts of Climate Change: A Case Study of Paddy Irrigation Water Requirements in Sri Lanka. *Agricultural Water Management* 93(1–2): 19–29.
- Dewey, J. 1927/1954. *The Public and its Problems*. Ohio: Ohio University Press.
- Docherty, B., and T. Giannini. 2009. Confronting a Rising Tide: A Proposal for a Convention on Climate Change Refugees. *Harvard Environmental Law Review* 33: 349.
- Donhauser, J. 2017. The Value of Weather Event Science for Pending Climate Policy Decisions. *Ethics, Policy and Environment* 20(3): 263–278.
- Douglas, H. 2003. The Moral Responsibilities of Scientists (Tensions Between Autonomy and Responsibility). *American Philosophical Quarterly* 40(1): 59–68.
- Douglas, H. 2014. Pure Science and the Problem of Progress. *Studies in History and Philosophy of Science Part A* 46: 55–63.
- Drexler, K. 2013. *Radical Abundance: How a Revolution in Nanotechnology will Change Civilization*. Public Affairs.
- Edgerton, D. 2004. The Linear Model Did Not Exist. Reflections on the History and Historiography of Science and Research in Industry in the Twentieth Century. In *The Science-Industry Nexus. History, Policy, Implications*, ed. K. Grandin, N. Wormbs, and S. Widmalm, 31–57. New York: Science History Publications.
- Epstein, S. 1996. *Impure Science: AIDS, Activism, and the Politics of Knowledge*. Sacramento: University of California Press.
- Etzkowitz, H., and L. Leydesdorff. 2000. The Dynamics of Innovation: From National Systems and “Mode 2” to a Triple Helix of University–Industry–Government Relations. *Research Policy* 29(2): 109–123.
- Feyerabend, P. 1975. *Against Method*. London: Verso Books.
- Ferris, E. 2011. Climate change and internal displacement: A contribution to the discussion. *Brookings-Bern Project on Internal Displacement February 2011*.
- Frisch, M. 2012. Climate Change Justice. *Philosophy & Public Affairs* 40(3): 225–253.
- Fujimura, J. 1988. The Molecular Biological Bandwagon in Cancer Research: Where Social Worlds Meet. *Social Problems* 35(3): 261–283.
- Gadagkar, R. 2011. Science as a Hobby: How and Why I Came to Study the Social Life of an Indian Primitively Eusocial Wasp. *Current Science* 100(6): 845–858.
- Gardiner, S. 2016. The Feasible is Political. In *Debating Climate Ethics*, ed. S. Gardiner and D. Weishbach. Oxford: Oxford University Press.
- Gieryn, T. 1983. Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists. *American Sociological Review* 48(6): 781–795.
- Gibbons, M., C. Limoges, H. Nowotny, S. Schwartzman, P. Scott, and M. Trow. 1994. *The New Production of Knowledge: The Dynamics of Science and Research in Contemporary Societies*. London: Sage.
- Gillies, J. 2011. Heavy Rains Linked to Humans. *The New York Times*. <https://www.nytimes.com/2011/02/17/science/earth/17extreme.html>.
- Godin, B. 2006. The Linear Model of Innovation: The Historical Construction of an Analytical Framework. *Science, Technology, & Human Values* 31(6): 639–667.
- Gomery, R. E. 1995. An Unpredictability Principle for Basic Science. In *AAAS, Science and Technology Policy Yearbook*, ed. A. Teich, S. D. Nelson, and C. McEnaney, 5–17. Washington: AAAS.
- Guston, D. 2000. Retiring the Social Contract for Science. *Issues in Science and Technology* 16(4): 32–36.
- Holbrook, J. 2018. Philosopher’s Corner: What Is Science in the National Interest? *Issues in Science and Technology* 34(4): 27–29.
- Hugo, G. 2013. *Migration and Climate Change*. Cheltenham: Edward Elgar Publishing Limited.
- Hulme, M. 2014. Attributing Weather Extremes to ‘Climate Change’: A Review. *Progress in Physical Geography* 38(4): 499–511.
- Hulme, M., S. O’Neill, and S. Dessai. 2011. Is Weather Event Attribution Necessary for Adaptation Funding? *Science* 334(6057): 764–765.

- Jasanoff, S. 2015. Imagined and Invented Worlds. In *Dreamscapes of Modernity: Sociotechnical Imaginaries and the Fabrication of Power*, ed. Sheila Jasanoff and Sang-Hyun. Kim, 220–228. Chicago: University of Chicago Press.
- Kealey, T., and O. Al-Ubaydli. 2000. Should Governments Fund Science? *Economic Affairs* 20(3): 4–9.
- Kitcher, P. 2001. *Science, Truth, and Democracy*. New York: Oxford University Press.
- Kitcher, P. 2004. What Kinds of Science Should be Done? In *Living with the Genie*, ed. A. Lightman, D. Sarewitz, and C. Desser, 201–224. Washington, D.C.: Island Press.
- Kline, R. 1995. Construing “Technology” as “Applied Science”: Public Rhetoric of Scientists and Engineers in the United States, 1880–1945. *Isis* 86(2): 194–221.
- Kline, S., and N. Rosenberg. 1986. An Overview of Innovation. In *The Positive Sum Strategy: Harnessing Technology for Economic Growth*, ed. R. Landau and N. Rosenberg, 275–304. Washington: National Academy Press.
- Klopprogge, P., and J.P. Van Der Sluijs. 2006. The Inclusion of Stakeholder Knowledge and Perspectives in Integrated Assessment of Climate Change. *Climatic Change* 75(3): 359–389.
- Kondo, M. 1999. R&D dynamics of creating patents in the Japanese industry. *Research Policy* 28(6): 587–600.
- Kvanvig, J. 2003. *The Value of Knowledge and the Pursuit of Understanding*. Cambridge: Cambridge University Press.
- Lakatos, I. 1970. Falsification and the Methodology of Scientific Research Programmes. In *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave, 91–197. Cambridge: Cambridge University Press.
- Lakatos, I. 1978. The Social Responsibility of Science. In *Philosophical Papers, Volume 2: Mathematics, Science and Epistemology*, ed. J. Worrall and G. Currie, 256–259. Cambridge: Cambridge University Press.
- Laudan, L. 1981. *Science and Hypothesis*. Dordrecht: Springer.
- Lenman, J. 2000. Consequentialism and Cluelessness. *Philosophy & Public Affairs* 29(4): 342–370.
- Logan, D. 2009. Known Knowns, Known Unknowns, Unknown Unknowns and the Propagation of Scientific Enquiry. *Journal of Experimental Botany* 60(3): 712–714.
- Kuhn, T. 1971. The Relations Between History and History of Science. *Daedalus* 100(2): 271–304.
- Kummerfeld, E., and K. Zollman. 2015. Conservatism and the Scientific State of Nature. *The British Journal for the Philosophy of Science* 67(4): 1057–1076.
- Marburger, J., III. 2005. Wanted: Better Benchmarks. *Science* 308(5725): 1087–1088.
- Marx, K. 1977. *A Contribution to the Critique of Political Economy*. Moscow: Progress Publishers.
- Massimi, M. 2020. Investing in Fundamental Research: For Whom? A Philosopher’s Perspective. In *The Economics of Big Science*, ed. H. Beck and P. Charitos, 113–116. Cham: Springer.
- Mazzucato, M. 2018. Mission-Oriented Innovation Policies: Challenges and Opportunities. *Industrial and Corporate Change* 27(5): 803–815.
- McCarty, M. 1984. On the Unexpected Fruits of Mission-Oriented Research. *Proceedings of the American Philosophical Society* 128(1): 20–26.
- Meyer, M. 2000. Does science push technology? Patents citing scientific literature. *Research Policy* 29(3): 409–434.
- Mulgan, T. 2008. *Future People: A Moderate Consequentialist Account of our Obligations to Future Generations*. Oxford: OUP Catalogue.
- National Academies of Sciences, Engineering, and Medicine. 2016. *Attribution of Extreme Weather Events in the Context of Climate Change*. Washington, D.C.: National Academies Press.
- Niiniluoto, I. 2013. On the Philosophy of Applied Social Sciences. In *New Challenges to Philosophy of Science. The Philosophy of Science in a European Perspective*, vol. 4, ed. H. Andersen, et al., 265–274. Dordrecht: Springer.
- North, D. 1995. Institutions and Economic Theory. In *The New Institutional Economics and Third World Development*, ed. J. Harriss, J. Hunter, and C. Lewis. New York: Routledge.
- Patton, L. 2015. Incommensurability and the Bonfire of the Meta-Theories: Response to Mizrahi. *Social Epistemology Review and Reply Collective* 4(7): 51–58.
- Pavitt, K. 1998. Do patents reflect the useful research output of universities? *Research Evaluation* 7(2): 105–111.
- Penner, O., R. Pan, A. Petersen, K. Kaski, and S. Fortunato. 2013. On the Predictability of Future Impact in Science. *Scientific Reports* 3(1): 1–8.
- Pielke, R. 2012. “Basic Science” as a Political Symbol. *Minerva* 50(3): 339–361.
- Pielke, R., and R. Byerly. 1998. Beyond Basic and Applied. *Physics Today* 51(2): 42–46.

- Pirtle, Z., and J. Moore. 2019. Where Does Innovation Come From? Project Hindsight, TRACEs, and What Structured Case Studies Can Say About Innovation. *IEEE Technology and Society Magazine* 38(3): 56–67.
- Pirtle, Z., and Z. Szajnfarber. 2017. On Ideals for Engineering in Democratic Societies. In *Philosophy and Engineering. Philosophy of Engineering and Technology Series*, vol. 26, ed. D. Michelfelder, B. Newberry, and Q. Zhu, 99–112. Berlin: Springer.
- Poincaré, H. 1902. Relations Between Experimental Physics and Mathematical Physics. *The Monist* 12(4): 516–543.
- Polanyi, M. 1940. *The Concept of Freedom: The Russian Experiment and After*. London: Watts & Co.
- Polanyi, M. 1951. *The Logic of Liberty*. London: Routledge & Kegan Paul.
- Polanyi, M. 1956. Pure and Applied Science and Their Appropriate Forms of Organization. *Dialectica* 10(3): 231–242.
- Polanyi, M. 1962. The Republic of Science: Its Political and Economic Theory. *Minerva* 38(1): 1–21.
- Quaglione, D., A. Muscio, and G. Vallanti. 2015. The Two Sides of Academic Research: Do Basic and Applied Activities Complement Each Other? *Economics of Innovation and New Technology* 24(7): 660–681.
- Rayleigh, L. 1942. *The Life of Sir J. J. Thomson: Sometime Master of Trinity College, Cambridge*. Cambridge: Cambridge University Press.
- Reiss, J., and P. Kitcher. 2009. Biomedical Research, Neglected Diseases, and Well-Ordered Science. *THEORIA. Revista de Teoría, Historia y Fundamentos de la Ciencia* 24(3): 263–282.
- Reydon, T. 2019. What Attitude Should Scientists Have? Good Academic Practice as a Precondition for the Production of Knowledge. In *What is Scientific Knowledge?*, ed. K. McCain and K. Kampourakis, 18–32. London: Routledge.
- Russell, B. 1912. *The Problems of Philosophy*. Oxford: Oxford University Press.
- Roll-Hansen, N. 2017. A Historical Perspective on the Distinction Between Basic and Applied Science. *Journal for General Philosophy of Science* 48(4): 535–551.
- Salter, A., and B. Martin. 2001. The Economic Benefits of Publicly Funded Basic Research: A Critical Review. *Research Policy* 30(3): 509–532.
- Schauz, D. 2014. What is basic research? Insights from historical semantics. *Minerva* 52(3): 273–328.
- Shaw, J. 2020. Feyerabend and Manufactured Disagreement: Reflections on Expertise, Consensus, and Science Policy. *Synthese*. <https://doi.org/10.1007/s11229-020-02538-x>.
- Shaw, J. 2021a. Feyerabend, funding, and the freedom of science: The case of traditional Chinese medicine. *European Journal for Philosophy of Science* 11(2): 1–27.
- Shaw, J. 2021b. Feyerabend's Well-Ordered Science: How an Anarchist Distributes Funds. *Synthese* 68: 419–449.
- Shaw, J. 2022. On the very idea of pursuitworthiness. *Studies in History and Philosophy of Science* 91: 103–112.
- Sintonen, M. 1990. Basic and Applied Sciences: Can the Distinction (Still) Be Drawn? *Science & Technology Studies* 28(2): 23–31.
- Smaldino, P., and R. McElreath. 2016. The Natural Selection of Bad Science. *Royal Society Open Science* 3(9): 160384.
- Solovey, M. 2020. *Social Science for What? Battles Over Public Funding for the Other Sciences at the National Science Foundation*. Cambridge: MIT Press.
- Stern, A.W. 1944. The Threat to Pure Science. *Science* 100: 356.
- Stokes, D. 1997. *Pasteur's Quadrant: Basic Science and Technological Innovation*. Washington, D.C.: Brookings Institution Press.
- The Slow Science Academy. 2010. The Slow Science Manifesto. <http://slow-science.org/slow-science-manifesto.pdf>.
- Thompson, A., and F. Otto. 2015. Ethical and Normative Implications of Weather Event Attribution for Policy Discussions Concerning Loss and Damage. *Climatic Change* 133(3): 439–451.
- Tijssen, R. 2010. Discarding the 'Basic Science/Applied Science' Dichotomy: A Knowledge Utilization Triangle Classification System of Research Journals. *Journal of the American Society for Information Science and Technology* 61(9): 1842–1852.
- Toulmin, S. 1965. The Complexity of Scientific Choice II: Culture, Overheads or Tertiary Industry? *Minerva* 4: 155–169.
- Wilholt, T. 2006a. Scientific Autonomy and Planned Research: The Case of Space Science. *Poiesis & Praxis* 4(4): 253–265.
- Wilholt, T. 2006b. Design Rules: Industrial Research and Epistemic Merit. *Philosophy of Science* 73(1): 66–89.

- Wilholt, T. 2010. Scientific Freedom: Its Grounds and Their Limitations. *Studies in History and Philosophy of Science Part A* 41(2): 174–181.
- Wilholt, T., and H. Glimell. 2011. Conditions of Science: The Three-Way Tension of Freedom, Accountability and Utility. In *Science in the Context of Application*, ed. M. Carrier and A. Nordmann, 351–370. Dordrecht: Springer.

Publisher's Note Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.