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CHANGING CONCEPTIONS OF BASIC RESEARCH?

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1. Introduction and summary

'Basic research' is a term that is widely used but with little apparent consensus on what it actually means. This background document analyses the concept of basic research, drawing on interviews with nearly 50 scientists and policy makers on the definition of the term and on the challenges that basic research is facing in today's funding environment. The aim of this document is exploratory and descriptive. It does not attempt to provide 'answers' but to increase awareness of the various dimensions of basic research and of the many ways in which the term is used.

The term 'basic research' developed in a specific historical context which is summarised in section 2. Section 3 then turns to the definition of basic research. Interviews with scientists and policy makers show that the concept is defined in several different, often inconsistent, ways. Six of these methods of defining basic research are outlined in section 3.1 and compared with official definitions. The aim here is not to establish which of these is the 'correct' way of defining basic research, but to draw attention to the various definitions that are currently in circulation. Section 3.2 addresses the highly-valued ideal of 'science for its own sake', and the associations basic research has with status and value – issues which should not be overlooked in any analysis of the concept. Several alternative categorisation schemes and new terms have been developed that attempt to overcome the perceived shortcomings of current research classification terminology (notably Stokes 1997), and these are described and assessed in section 3.3. The justification for the public funding of basic research, which is closely related to the definition of the term, is the subject of section 3.4. Here the aim is to explore briefly the various justifications that are currently in use. Changes in the funding environment and their implications for basic research are examined more broadly in section 3.5. There are increased pressures for instrumentality in research in today's climate and these have had effects on the way scientists present their research. The actual day-to-day utility (and lack of utility) of the concept of 'basic research' is discussed in section 3.6, and section 3.7 addresses some existing ways of measuring basic research. Finally, the policy implications of our changing conceptions of basic research are examined in section 4, which asks whether our current terminology should be retained or modified.

2. A brief history of the term

The value of the pursuit of knowledge 'for its own sake' can be traced back to the Ancient Greeks, for whom this type of pursuit was associated with social status. The scientific revolution drew on the knowledge of the Greeks so the idea of the superiority of 'pure' knowledge was retained. When universities in Europe and the United States were established they incorporated these ideas (Brookman 1979 and Stokes 1997), which remain important constituents of our current notion of basic research (see section 3.2). However, it was the Second World War and the Cold War that followed which were crucial in establishing the institutional arrangements and ideological commitments that we now associate with basic research. During the Second World War the military and academics joined forces (Elzinga and Jamison 1995) and public funds became essential to science. Funding for science compared with that in earlier periods was extremely generous (Ziman 1994:94).

Perhaps because of worries about increased government intervention, the important notion of basic research as an *autonomous* pursuit, free of interference by sponsors, was asserted at this time, one of the major influences being the US presidential science advisor, Vannevar Bush (1945). Bush managed to instil the idea of a generously funded yet self-governing scientific establishment. By stressing the importance and inevitable benefits of basic research, he helped legitimate the 'linear model' where inputs in basic research would eventually feed into technological innovation. Bush argued that it would be self-defeating to attempt to constrain the creativity of basic research, and that science was most fertile if it was not under direct governmental control (National Science Board 2000).

The Cold War period is also important because it was during this time that the *justification* for the public funding of basic research emerged. This justification was based on the arguments of economists that, because of the unique characteristics of basic research, the private sector would always under-invest in it – i.e. that it represented a case of 'market failure' (see section 3.4). Here we see that the justification for the public funding of basic research is closely linked to the definition of the term.

The first attempts to measure basic research were made in the early 1960s, and some argue that this measurement is the main reason why basic research became a central category in science policy (Godin 2000; see also section 4.1). The US National Science Foundation (NSF) was, in the 1960s, the most advanced in classifying different types of research, so the NSF definitions were an important resource when the OECD was formulating its own definitions, which it codified in the Frascati manual in 1963 (Godin 2000). This classification of basic research became crucially important for the persistence of the concept over subsequent decades. Once it became something that could be measured, trends could be identified and international comparisons drawn.

The 1950s and 1960s were periods of considerable growth in funding for scientific research. During this time there was a belief that investments in science would generate innovation (Gummett 1991). Towards the end of the 1960s, the belief in the inevitably benevolent potential of science began to be challenged (Salomon 1977), as some of the negative consequences of scientific and technological development on the environment and wider society became apparent. During the late 1960s and 1970s many countries experienced a reduction in government expenditures on science (Elzinga 1985:192).

The 1980s saw a large shift in governmental attitudes towards basic research in many countries. With the ending of the Cold War, military incentives for funding research were no longer so pressing.[1] What became important instead was the promotion of technology and economic competitiveness. At the same time, academic studies of innovation began to question the simple linear model of the relationship between science and technology (see Mowery and Rosenberg 1989, Freeman 1974). The changes that began in the 1980s are still continuing today, although we are also seeing the emergence of new issues. We now have a situation where "basic research has become intimately intertwined with the production of goods and technological development of relevance for all realms of society" (Elzinga 1997:420). Moreover, by the 1990s most European countries had entered an era of 'steady state' science (Ziman 1994), where funding was not keeping up with the rapid pace at which research costs were growing.

To summarise, the history of the funding of basic research from the 1950s to the 2000s has shown a move away from the idea that scientists should be supported as autonomous truth-seekers towards the idea that they should orient their work rather more toward social and economic objectives. Simply put, there has been a parallel decline in autonomy and an increase in accountability.

[1] Additionally, the decline of nuclear power in the 1980s led to reduced funds to associated fields of physics and engineering research (Martin and Etzkowitz 2000).

3. Present use of the concept of 'basic research' in the science policy context

3.1 What is meant by basic research?

Following this brief history of the concept of basic research, we need to consider exactly what is meant by the term. At first glance 'basic research' appears to be used in a rather confused manner. Although it is often invoked, the concept itself is rarely analysed. A symptom of this confusion is that there are many terms which refer to 'basic' research, and which may or may not mean the same thing – for example, 'pure science', 'fundamental research', 'curiosity-driven research' and 'blue skies research'.

In order to tackle this problem, 49 interviews were undertaken with scientists (biologists and physicists) and policy makers in the US and UK on the concept of basic research and changes it is facing in the current funding environment.[2] This section outlines the various ways in which interviewees defined the term, illustrating problems with each definition.

Before we discuss the various definitions of basic research it is helpful to reflect on how we might intuitively define the term. Should we define basic research as research that is 'fundamental' and underlies other types of research? Is it research that is guided purely by the curiosity of the researcher? Is it research that is produced for publication and free dissemination in the scientific community? All these different concepts of basic research were found in the interviews as the rest of this section will show. The six criteria that interviewees employed to distinguish basic research and the number of interviewees who used each criterion are shown in Table 1.

Table 1 – Definitions of basic research given by interviewees

Criteria for distinction	No. interviewees
Epistemological	33
Intentional	32
Distance from application	15
Institutional	8
Disclosure norms	7
Scientific field	3

Note: Total number of interviewees = 49

Only seven interviewees used a single criterion to define basic research, the others all using multiple criteria. This is preliminary evidence that most scientists and policy officials do not have *one* clear idea about what basic research is, but that they draw on many different features when describing the term. The diversity of ways in which basic research is defined in practice shows that it is ambiguous and has different meanings for different individuals, and that it can potentially incorporate a range of different characteristics. 27 interviewees defined basic research in both epistemological *and* intentional terms, the two most important criteria used to distinguish basic from other

[2] The interviews formed part of the doctoral research carried out by the first author. Full details can be found in her doctoral thesis *Goodbye Blue Skies? The Concept of 'Basic Research' and its Role in a Changing Funding Environment* Brighton: SPRU, University of Sussex, available 2002.

types of research. Table 2 below summarises these six different ways of distinguishing basic research. The rest of this section discusses each of these distinctions in detail.

Table 2 – Summary of the definitions of basic research

Definition	Example	Problems with using the definition
Epistemological *Unpredictability/novelty* Basic research is unpredictable research.	"What you're trying to do is find a new concept or push the boundaries of existing knowledge" (UK physicist).	• Conservatism in the research agenda • Basic research inherently predictable (puts forward hypotheses that predict outcomes).
Generality Basic research is done at such a 'level' that it applies to a wide range of instances.	Basic research gives an overall vision and an ability to connect disparate pieces of information (US biologist).	• Some interviewees thought that basic research was actually specific research.
Theory Basic research involves statements of general principles about how things work.	"The more basic the research, the more it's driven by the internal theoretical dynamics of the field" (US policy maker).	• 'Theory' has a specific meaning in some scientific fields.
Reductionism Basic research describes things in reductionist terms.	"I suppose what I mean is partly in a reductionist sense – looking at things at a *molecular level*" (UK biologist).	• Some interviewees thought that the more basic the research the more broadly applicable it was.
Intentional If the intention behind the research is simply to further knowledge or pursue curiosity then the research is basic.	A basic researcher is "someone who is just following their curiosity" (US policy maker).	• Basic research becomes something that is defined in negative terms (where you do not know what you are going to do with the knowledge). • Whose intentions matter? The scientist or the agency funding the research?
Distance from application In basic research the research will *not* be useful in any way.	"The certain work I do has no applied use so therefore it is basic research" (UK physicist).	• Trivial research (e.g. bouncing neutrons off coke bottles) would have no immediate application, but would not be 'basic' research. • Difficult to tell in advance whether something will have an application or not.
Institutional *Where* the research is done determines the type of research.	"If you walk into a laboratory how do you know whether they are doing basic or applied research? And I would say that the first clue is probably the name on the building" (US policy maker).	• Basic research *cannot* be undertaken in industry according to this definition.
Disclosure norms If it is published it is basic, if it is patented it is applied.	"Research by scientists that is directed at an audience of other scientists meaning that it is intending to be *published* to gain rewards in the scientific reward system" (US policy maker).	• Assumes that firms do not publish. • Assumes that applied research is not published.
Scientific field The scientific field determines the type of research.	Basic science is "astronomy and particle physics and nuclear physics" (UK policy maker).	• Varies considerably among individuals.

3.1.1 Distinguishing the term epistemologically

Basic research is often thought to produce a certain *type* of knowledge, and a distinction is labelled 'epistemological' when it refers to the *properties* and/or the *nature* of the knowledge basic research is said to produce. There are several sub-categories of this epistemological distinction – unpredictability, generality, theory and reductionism.

Unpredictable/novel research

In interviews, both scientists and policy makers distinguished basic research in terms of its unpredictability or novelty. According to a UK physicist, in basic research "what you're trying to do is find a new concept or push the boundaries of existing knowledge". Unpredictability of this kind can lead to truly novel outcomes; for example, a UK policy maker observed that "the word processor didn't come about through research on a quill pen".[3]

Some interviewees, however, disagreed with this definition of basic research as unpredictable or novel research (a reminder that it is difficult to formulate a finalised definition of basic research). They talked instead of conservatism in the research agenda, which discourages spectacular novel outcomes. Indeed, a US biologist described scientific methodology as being inherently *predictable*, in that it puts forward hypotheses which predict certain outcomes. Similarly Ziman (1998) argues that much research, such as astronomical research, can be very mundane and predictable because its results are dependent on routine, repetitive observations. One biologist from the UK described the Human Genome Mapping Project as "quite predictable and dreadfully boring". He went on to assert that funding agencies liked it precisely *because* of its predictability.

General

According to some definitions, research is 'basic' if it is general rather than specific. General research refers to research done at such a 'level' that it applies to a wide range of instances or phenomena, whereas applied research will help solve the particular problem being investigated but little else. A UK biologist suggested that the generality of basic research provided an overall vision and an ability to connect disparate pieces of information.

As with the definition in terms of unpredictability, there were some critics of the idea that generality is the defining characteristic of basic research. A US molecular biologist said she thought that basic research was actually *specific* research because in her field it involved working out how a specific gene functioned and hence it became difficult to generalise this to the level of the organism.

Theory

Theory is closely related to generality, since theories involve statements of *general principles* about how things work. One US policy maker said "the more basic the research, the more it's driven by the internal theoretical dynamics of the field". However, the words 'theory' and 'theoretical' drew a contrasting response from physicists. A UK physicist argued that "theory and experiment's nothing to do with basic and applied". A UK policy maker agreed, because "a theoretician will postulate something that can be tested by an experimentalist" or give an experimentalist greater insight into the data that they have collected. This was a specific understanding of theory that made most sense in the context of large-scale physics experiments.

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[3] Kuhn (1962) and Lakatos (1970) also argue that risky 'paradigm-breaking' science, or science that goes beyond the sphere of a dominant 'research programme', is likely to have the greatest impact.

Reductionism

A notion often found in epistemological definitions of basic research is that knowledge is built on top of knowledge, and that knowledge at the 'lowest' level, which explains phenomena by reducing them to their smallest constituent entities, is by implication the most important. This is a *reductionist* perspective that some interviewees drew on. A UK biologist, when asked what he meant by basic research, said that it involved "looking at things at a *molecular level*". Physicists, not surprisingly, had a 'deeper' notion of reduction in terms of "the properties of matter" (US physicist). Reductionism is very closely related to generality since reductionists claim to understand a diversity of phenomena in terms of smaller and more homogenous constituents, making it possible to draw *general* conclusions about those phenomena.

3.1.2 Aims and intentionality

The second most common way of defining basic research after the epistemological distinction is in terms of the *motivations* driving those engaged in it. According to this distinction, a basic researcher is "someone who is just following their curiosity" (US policy maker). This intentional definition was often put forward in interviews.[4] It was explained that if the *same* research is done with *different* intentions, it should be classified differently. For example, "understanding how a cell grows would be basic, understanding how to grow it in your fermenter would be applied" (US biologist). Similarly, one could map the stars either for navigational reasons or to learn about the solar system, and the research would be defined differently on these two occasions.

Intentional and epistemological definitions will not necessarily result in a piece of research being classified in the same way; if the intentions behind the research are to produce something that will result in an application, some would argue that, no matter how 'fundamental' the research may be in a epistemological sense (i.e. unpredictable or general), the research should not be classified as basic. If we adopt this strict intentional definition, we may conclude that pressures on scientists to be aware of the potential applications of their work mean that basic research is indeed being threatened as an activity (see section 3.5).

Several commentators have remarked that the accepted (intentional) definition of basic research has come to be the *absence* of concern with practical applications rather than a positive concern for understanding the properties of phenomena (Vavakova 1998, Rosenberg and Nelson 1994). This leads to a situation in which "Policy studies get themselves into a twist over whether it is possible to put a quasi economic value on the cultural role of research that is avowedly useless" (Ziman 1998:164). Interviewees often defined basic research in this negative manner; for example, basic research can be thought of research that is done for "no good purposes" or "merely Friday's entertainment" (UK policy maker).

With a definition based on intentions we also have the problem of *whose* aims we are talking about. Those of the researcher may differ from those of the funder, and those of one researcher may differ from those of another. There is a danger that one's definition of basic research will eventually become individual and even solipsistic. This problem was recognised by interviewees. One US policy maker pointed out that "something could be basic to the person who's doing it, and could be applied to the sponsor. So partly it's a matter of perspective." Yet despite these problems with the aims-based definition it is still very widely used.

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[4] Intentional definitions were not clearly separated from epistemological distinctions by interviewees, who would often switch between the two.

3.1.3 Distance from application

It may seem more logical to try to classify different types of research in terms of their *outputs*. The distinction in terms of 'distance from application' is separate from the intentional categorisation because the results of research can be distant from an application without the *intention* being for them to be so.[5] Nevertheless, this definition is closely related to the intentional definition so the distinction is a somewhat arbitrary one.

An example of the 'distance from application' classification comes from a UK physicist: "The certain work I do has no applied use so therefore it is basic research". It is interesting that this same physicist justified government funding of his work in terms of its potential practical applications. His inconsistency shows how the heterogeneous properties of basic research can be drawn on to make it simultaneously "practically useful but useless" (Gieryn 1999:19).

A limitation of this definition was pointed out by a UK physicist, who noted that it was possible to do work which did not have immediately obvious applications (such as "bouncing neutrons off coke bottles") but that this would not mean that the research qualified as 'basic'. There are other problems in defining research in this way. A medical science policy maker from the UK worried whether it is possible to categorise *in advance* if a piece of basic research is likely to be useful. There are many familiar anecdotes about how basic discoveries can have unforeseen applications (for example, lasers led to the development of CD players), while more applied and directed research can often fail to provide the expected useful outcomes (such as the nuclear fusion programme) or may even become important in inspiring another area of basic research (for example, steam engines gave rise to the science of thermodynamics).

3.1.4 The institutional distinction

According to the institutional definition, *where* the research is done is central in defining the type of research. A US policy maker used this distinction:

> If you walk into a laboratory how do you know whether they are doing basic or applied research? And I would say that the first clue is probably the name on the building. But if you don't know what building you're in, it gets a lot harder to distinguish it from the mechanical activities that go on.

The example of the private initiative for sequencing the human genome was cited by another US policy maker. He argued that if the work was being done in a university it would be basic research, but since it was being done in a private company for profit it could not be. We can see the complications which might arise with this definition when we ask how this interviewee would categorise research which is done for companies, on contract, by researchers in universities.

3.1.5 Disclosure norms

Open publication is usually related to the institutional arrangements in universities so institutional location and disclosure norms are often closely related. Those who use the disclosure criterion to distinguish basic research will focus on "whether it's publishable" (UK physicist). This physicist commented that he would not work with NATO because he would then be dallying with a military agency that does closed research. For him, publishable research distinguishes the type of research he would want to be involved in as a basic researcher. A US

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[5] The 'distance from application category' also implies that because basic research results are further from application the research will be *longer-term*.

policy maker likewise defined basic research as "research by scientists that is directed at an audience of other scientists meaning that it is intending to be *published*". However, there is an assumption here that firms do not publish, an assumption heavily criticised by Hicks (1995). We can also question whether one would want to call *everything* that is published in a peer-reviewed journal 'basic' research. These complications demonstrate that there is not a straightforward relationship between basic and applied research and the public and private arenas (see also section 3.7).

3.1.6 Scientific field

Three interviewees defined basic research by scientific field. For example, a UK biologist classified biotransformations as applied research. The problem with a distinction based on this criterion is that it varies considerably among individuals. This is clearly exemplified by one particular UK physics policy maker who maintained that basic research consists *only* of "astronomy and particle physics and nuclear physics".

3.1.7 Basic research in different fields

The distinction in terms of scientific field leads to the question of whether basic research is defined differently in different fields. The interviews discussed here were with biologists and physicists, but within the areas of biology and physics a wide range of scientists were included,[6] several of whom, for example, carried out research that had clinical applications. Because of the diversity the analysis here can probably be extended, with caution, to the rest of the natural sciences and perhaps to the social sciences.

We might ask how basic research would be defined in the humanities. It would be very difficult to say something comparable about this area because the humanities are different from the natural and social sciences in many respects. First, even the distinction between 'research' (original work) and 'scholarship' (re-analysis and synthesis) in the humanities is often difficult to make. Second, the humanities have never been treated as if they were autonomous and isolated from the rest of society, as the natural sciences were before the late 1960s (Gibbons *et al.* 1994), and, as discussed above, this autonomy was very important in establishing the concept of 'basic research' (section 2). Third, natural and social scientists attempt to be objective and rigorous in their work and to separate themselves from the object they are studying, while in the humanities "the distance between creation and contextualisation is minimal, and the construction of meaning is considered the essence of what the humanities do" (Gibbons *et al.* 1994:92). This contextualisation means that a separation between the pursuit of knowledge 'for its own sake' and the application of knowledge is much more difficult to make in this field. For these reasons an analysis of the different types of research in the humanities would be a separate project in its own right.

3.1.8 Official Definitions

Having covered the different ways in which the scientists and policy makers interviewed defined basic research, it is helpful at this point to turn to the current official definitions used by the OECD, to show how they correspond to the above categorisation scheme. Table 3 shows the OECD definitions with the terminology adopted here for the type of categorisation being used bracketed in italics.

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[6] The scientists ranged from ecologists, theoretical physicists, astrobiologists, physical chemists and molecular biologists to those doing cancer research and medical physics.

Table 3 – Frascati definitions of basic research (OECD 1994)

Basic research "is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundations of phenomena and observable facts *(epistemological – general/reductionist)*, without any particular application or use in view *(intentional)*" (p.68) It "analyses properties, structures, and relationships with a view to formulating and testing hypotheses, theories or laws *(epistemological – general/theoretical)*. The results of basic research are not generally sold but are usually published in scientific journals or circulated to interested colleagues. Occasionally, basic research may be 'classified' for security reasons *(disclosure norms)*" (p.68). It is "is usually undertaken by scientists who may set their own goals and to a large extent organise their own work *(intentional)*." (p.69). **Pure basic research** "is carried out for the advancement of knowledge without working for long-term economic or social benefits and with no positive efforts being made to apply the results to practical problems or to transfer the results to sectors responsible for its application *(intentional)*" (p.69). **Oriented basic research** "is carried out with the expectation that it will produce a broad base of knowledge *(epistemological – general)* likely to form the background to the solution of recognised or expected current or future problems or possibilities *(intentional)*" (p.69)

The 'pure basic' category in this list of Frascati definitions caused problems for some interviewees. A UK policy maker said that she could not classify any of her funding agency's work as 'pure basic' because they were, by definition, a mission-driven agency. The intentional definition is explicitly written in here, and it inevitably becomes problematic to justify funding for research where scientists make "no positive efforts" for their research to be useful. 'Oriented basic' research is also problematic in that it becomes difficult to establish unambiguously whether there is an 'expectation' that the work will be useful or not.

3.1.9 Summarising the various definitions

We have seen that there are good reasons against using any one particular definition of basic research. However, following the discussion above, we could perhaps attempt to summarise these six different definitions of basic research by amalgamating at least some of them. Starting with the epistemological definition, we noted above that reductionism and generality are not opposites because both describe phenomena in terms of a smaller number of properties. Theoretical explanation also aims to explain phenomena in terms of general laws so this could potentially be subsumed under the same definition. If we combine these definitions this leaves us with two broad epistemological characteristics: unpredictability and generality. The intentional definition and the distance from application definition were separated above because they could potentially conflict.[7] However, both engage with similar aspects of the concept of basic research (a future-directed emphasis on what the research is intended towards or what it produces), so potentially these two definitions could also be combined. The institutional definition and the definition based on disclosure norms do refer to different characteristics of the research (since university research can be patented and industry research can be published – see section 3.7), but disclosure norms and institutional location are usually correlated to some extent so these two definitions could also be merged. The definition in terms of scientific field is

[7] Research could be intentionally driven purely by curiosity but result in a useful application, or could aim to produce a useful application but have no practical outcome.

probably best excluded because it varies so dramatically between individuals. To summarise, we could say that basic research can be defined in each of the following four ways:

- epistemological: unpredictability/novelty;
- epistemological: generality/theoretical/reductionist;
- intentional/distance from application;
- institutional/disclosure norms.

In some situations, however, it may be more helpful to be aware of the more subtle distinctions highlighted by the original six criteria. We should also remember that several different criteria are often used simultaneously. This issue will be discussed further in section 4.2.

3.2 Basic research as an ideal

A point that must not be overlooked in any analysis of the concept basic research is the importance of the *ideal* of basic research to scientists. Many of those interviewed would describe basic research as autonomous and driven purely by curiosity when defining it in the *abstract*, but when they came to describe their own research in *practice* they often described it rather differently.

For example, although there was a strong definitional emphasis on curiosity in basic research, in discussing their own research several scientists demonstrated that they were motivated by much more pragmatic aims. A US biologist said, “I like seeing the fruit of the research and what it means to society”, and a UK biologist described how curiosity, ambition, peer approval, a desire to serve humanity and his enjoyment of conferences in attractive locations all motivated him in his scientific work. Many of those scientists who present an idealised image of science as the unadulterated quest for greater understanding do not describe their own work in this manner.

There was a similar ‘idealising’ in the case of autonomy. Some scientists described themselves as autonomous researchers as one might expect. A UK policy maker said that the kind of people attracted into academic research in the first place are those who like a “free-ranging life”. Many scientists, however, would initially say they had complete autonomy in their work and then go on, when considering grant applications, for example, to admit that in reality they did not have so much. One UK physicist summarised this in one sentence; “in principle I have autonomy, in practice there’s quite a lot of constraints”. It is this ‘in principle’ that is interesting here. ‘In principle’ refers to the idealised image of what basic scientific work comprises. Again it seems to be important for scientists to maintain that they do, in some respects, have autonomy, even while at the same time admitting that, when it comes to obtaining funds, this autonomy is often restricted.

3.2.1 The value of basic research

The importance of the *value* of this ideal of basic research is shown by the association of basic research with other culturally valued concepts. Basic science was explicitly tied to *ethical* values by two physicists; one argued that the methods of science themselves engender scrupulous honesty and integrity, claiming that doing science makes one become a better person. *Status* also often came up in discussions about the relation between basic and applied research. A US biologist noted that:

> The elitism of basic science still hangs around. There’s still a lot of people, mainly older people, who still look on industrial collaborations as being slightly tainted and dirty, and it’s ‘prostitution’ to do applied research.

A UK physicist supported this point, saying that some of the research council committees "still think it's undignified to do anything which is useful". The status of basic research is clearly important and has an impact on the ability of scientists to obtain funding.

These associations of basic research with ethics and status demonstrate that there are deep cultural values embedded in the notion of basic research. These historically important values should be acknowledged, especially if we are considering altering long-standing terminology. When describing their research as 'basic research', scientists are implicitly drawing on these attributes associated with the concept.

3.3 Other terms

Having covered the most important ways in which interviewees distinguished basic research, and the values they attached to this type of research, it is necessary to briefly mention other terms that are occasionally used in place of the dominant basic/applied dichotomy:

- *Pure* – The term 'pure' was often used in place of 'basic'. Usually an interviewee's definition of this term was covered by one or more of the categories discussed above as defining basic research. However, one US biologist said, "I would be very unlikely to use that term...in some ways it carries with it very positive connotations if you say 'pure research' ".

- *Fundamental* – One UK physicist preferred to describe his work as 'fundamental' rather than 'basic'; when asked why, he said: "Fundamental sounds like it's more profound, I suppose. It's a different level of research".

- *Blue-skies* – One UK biologist said that Watson and Crick's work was 'blue-skies' because "it was more inspired and creative", whereas for him 'basic' is more mechanical and mundane.

Most interviewees in their definitions of 'pure', 'fundamental' and 'blue-skies' research were referring to similar ideas to those associated with 'basic research' above. They were, however, pointing to subtleties in the distinction (indicating that there is no single clear distinction), and illustrating that scientists have very different ideas about the specific term 'basic research'.

3.3.1 Alternative categorisation schemes

Several recent categorisation schemes have attempted to deal with ambiguities surrounding the definition of basic research by re-classifying research types and developing alternative terms. Three of these alternatives will be addressed briefly.

Use-inspired basic research

Stokes (1997) has introduced the category 'use-inspired basic research'. Adopting an intentional definition of basic research, he questions the distinction between research pursued for a specific aim and research pursued simply for deeper understanding. Stokes gives many examples of research driven both by some very practical concerns and by a desire to understand the phenomena in question more deeply, such as Pasteur's research into microbiology. Stokes argues that research can simultaneously be driven by considerations of use and by a quest for fundamental understanding. He terms this type of research 'use-inspired basic research'. He argues that if we acknowledge the 'use' element in most basic research, the benefits of basic research will be more apparent to all.

Stokes' model can be criticised for exclusively focusing on the *aims* behind the research, arguing that different aims (for understanding and use) do not have to be mutually exclusive. He does not address the issue that the aims behind the research might *not* be the most important feature to distinguish basic research from other types of research. His argument is that a statement about the (epistemological) characteristics of a piece of research "is only a statement about an empirical correlate of the goal patterns envisaged by the Frascati definitions" (p.82), an argument one might question in the light of the importance of epistemological distinctions apparent in scientists' definitions of basic research. Despite these criticisms, Stokes' model of research-types has been very influential, especially among policy makers in the US. Similar ideas have been drawn on recently by other science policy analysts (see in particular Branscomb *et al.* 2000).[8]

Strategic research

Stokes' contribution has been to add an extra category to the basic/applied categorisation scheme. However, this is something that had already been done in the formulation of the category of 'strategic' research. Irvine and Martin (1984) argued that the terms 'basic' and 'applied' lacked operational utility, so they proposed dividing basic research into 'pure or curiosity oriented' research and 'strategic' research. Strategic research is long-term research but it is more directed than 'pure or curiosity oriented' research. The impetus can come either from applied scientists when it is recognised that more background research is required, or from basic researchers when it is realised that a discovery has potential applications (Senker 1991).

Stokes recognises that use-inspired basic research shares the same conceptual home as 'strategic research'. Stokes explains that, although this term was present in the 1980 revision of the Frascati manual, the category of strategic research was not taken up by governments because they wanted to allocate it to the pre-existing categories of *either* basic *or* applied research in order to preserve the existing statistical series. He argues that "the conceptual issue of strategic research has been taken hostage by problems of measurement and has remained unsolved" (p.67).

Rip (1997) also argues that strategic research is a useful concept. He maintains that the advantages of strategic research are that it allows scientists to set the research agenda and to retain autonomy and quality control, but that it also encourages a productive relationship between science and wider society. Rip notes that the concept of strategic research is particularly suited to the current political and financial situation facing science.

'Strategic' was an interesting term in interviews in that there was very little consensus about its definition. Six interviewees spontaneously introduced the term. According to a UK policy maker, "it's where you put things you can't decide whether they're applied or basic". In the US a policy maker explained that when the Democrats were in power in the Congress "they were pushing very hard for what's called 'strategic' research", but this term was not very popular any more because the present[9] Republican-dominated Congress objects to the involvement of government in industrial research."

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[8] Branscomb *et al.'s* (2000) concept of 'Jeffersonian' research is slightly different from 'use-inspired basic research'. Jeffersonian research is not driven by motives for both understanding and use at the *researcher* level, but the motivation for supporting useful research comes from the *sponsor* of the research (usually the government), and the motives of the researcher may simply be to further their understanding.

[9] US interviews were carried out between September and December 1999.

Comroe and Dripps
Comroe and Dripps (1976) in their study of biomedical science do not just suggest one new research category but set up an entire new categorisation scheme. They define basic research in an epistemological manner as research where the investigator "attempts to determine the *mechanisms* responsible for the observed effects" (p.42, emphasis added). Their epistemological distinction is combined with an *intentional* distinction – whether the research is clinically oriented or not. Research is clinically oriented "if the author mentions even briefly an interest in diagnosis, treatment, or prevention of a clinical disorder" (p.108). They combine these two dimensions and end up with a categorisation scheme of six types of research (including 'basic: not clinically oriented', and 'basic: clinically oriented'). They clearly recognise that there are different dimensions one can use to distinguish research types and do not rely on intentional criteria alone. More questionable is their assertion that their categorisation scheme is 'the correct' one, and there is also very little discussion of potential ambiguities.

One needs to be aware of the similar concepts these alternative categorisation schemes are trying to encapsulate.[10] The fact that there is a plethora of terms implies that there is some dissatisfaction with the way the research landscape is mapped out. This may encourage us to believe that there are pressures to change the current terminology. However, as will be demonstrated below (section 4.1), there is resistance against change for several reasons.

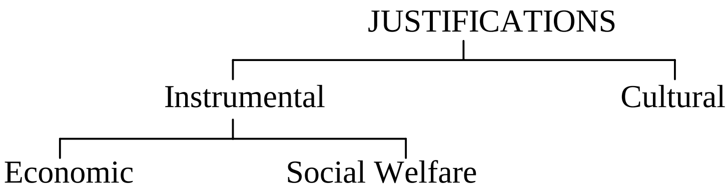
Although the work of Stokes and others has helped us to understand better the dimensions of basic research and provided much material for further analysis, they have not resolved the problems concerning the precise definition of basic research. This supports the suggestion that there is actually no *one* way to distinguish basic research. Basic research may be an intrinsically ambiguous category, with different features highlighted by different people at different times. Since there are many ways of defining the term, this gives it an inherent flexibility and the potential to serve many functions.

3.4 The funding of basic research

It is necessary to evaluate briefly the justifications for the public funding of basic research, which, as mentioned above (section 2), are closely linked to the definition of the term. Governments are ultimately interested in funding basic research because of the benefits that it is perceived to bring to society. The justifications for funding basic research can be broadly classified into instrumental and cultural justifications. Instrumental justifications often refer to *economic* benefits which can be either direct or indirect. These include: increasing the stock of knowledge, training graduates, creating new instrumentation and methods, developing networks and creating spin-off companies (Salter *et al.* 2000). The instrumental benefits from basic research can also be broadened to include other types of benefit that can be loosely termed contributions to 'social welfare' such as improvements in healthcare. Publicly funded basic research can also have consequences for environmental sustainability and safety (see Senker 1999), and can be a source of independent scientific advice for governments. Figure 1 summarises the different types of justifications used for funding basic research.

[10] Such concepts can also be seen in other terms present in interviews such as 'translational research' and 'basic technology'. Both these terms represent attempts to 'bridge the gap' between basic and applied research.

Figure 1 – Justifications for funding basic research



The 'cultural' justifications are the most elusive. They are based on the idea that basic research has an intrinsic cultural value. It is considered necessary for a 'civilised' society to fund the pursuit of knowledge for its own sake (Stokes 1997). The problem with this argument, however, is that it is not sufficient to justify the large amount of spending on science to which scientists in Western society have become accustomed. To maintain these levels of funding, a justification is needed which shows that science provides economic benefits. But even a very convincing argument that science produces substantial economic benefits does not lead to the conclusion that science should be *publicly* funded. The argument that it is public funds (via government) which are needed to support the scientific enterprise is the 'public good' argument, according to which there is a 'market failure' if the production of scientific knowledge is left to the private sector alone (see Nelson 1959 and Arrow 1962).[11] The faith that public investments in science will necessarily be for the general good is increasingly being challenged (see section 2 and section 3.5). We are, however, seeing new arguments concerning the importance of science in today's 'knowledge economy' used to justify the public support of basic research.

3.5 Changes in the funding environment for basic research

The importance of science in the knowledge economy is one indication that the role of basic research is changing. Not all changes are perceived as positive; indeed, there is considerable concern about the role of basic research today. Trends in the climate for basic research funding, outlined above (section 2), tend to lead to a stress on the *instrumental* aspects of research.[12] Many analysts argue that those trends are having a detrimental effect on basic research. Brooks (1993) talks of "the *declining prestige* of the kind of curiosity-driven, freewheeling research that was emphasised in the Bush report as a basis for a thriving economy and a healthy and peaceful society" (p.206 emphasis added). These notions are even infiltrating science policy itself: "It is now a question of 'science policy for what?' rather than 'science policy for science's sake'" (De la Mothe and Dufour 1995:209).

A feature of today's funding environment is an increasing emphasis on applicability, accountability, relevance and measurable research outcomes (Ziman 1994, Guston and Keniston 1994). Most interviewees noted a recent emphasis on applicability – a pressure to show that scientific work is potentially useful to a group outside the scientific profession, usually industry. Scientists often complained about these trends. A US biologist talked about "basic research creep", saying that everything has to have more of a "tinge of applicability" nowadays. There

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[11] The public good argument has been criticised for not recognising the 'tacit' nature of scientific knowledge (see Callon 1994 and Pavitt 1991). Furthermore, Hounshell (2000) has argued that Arrow and Nelson were actually providing a "*justification* of the special arrangements which grew up around the Cold War regime of science planning" (p.37).

[12] Analytical approaches to the changing nature of the research system can be found in Gibbons *et al.* (1994) on 'Mode 2' and Etzkowitz and Leydesdorff (2000) on the 'Triple Helix'.

were worries about delayed publication and suppression of publication, and fears that the very nature of free inquiry might be bent out of shape subtly and unintentionally. One US biologist thought that today's short-term perspective "will in the long run be detrimental to science".

Geuna (1999) argues that because of disproportionate incentives for short-term research, there is a lack of incentives for 'path breaking' research. Part of the reason why risky research might be becoming less attractive is because such research is, by necessity, difficult to measure, its outcomes being unpredictable. A UK policy maker also commented that "over the past few years we think we have less adventurous speculative research in our portfolio".

Accountability is linked to the rise in importance of the *public* in decision-making about science. Even advocacy groups who may be opposed to an emphasis on economic competitiveness (e.g. those who want less military research or more environmental research), may nevertheless champion applied science, science which *does* something. Even the welfare and public safety aspects of science and technology are areas that have a strongly applied dimension to them (Senker 1999).

Some policy makers maintained, however, that these changes are *not* negatively affecting basic research, arguing that, although there may be a shift towards making scientists think a little more about how their research might be used in the future, this did not pose any real threat to basic research. They maintained that the research had not really changed in character "but the people have just become more savvy about its potential for applications" (US policy maker).

Some interviewees were rather sceptical about any talk of changes and several claimed that they had seen it all before. One US policy maker said that the basic research issue was one that resurfaced every couple of decades and there was no special criticism of it now; another remarked that "the acronyms change, nothing else much does".

It should be stressed at this point that most 'new' aspects of the current funding situation have been present in the past. Rip (1997) notes that there were pressures for research to be relevant in the early 1970s. Even in the late nineteenth and early twentieth century the American States funded much university research because of its 'relevance' to agriculture, and German universities often engaged in collaborations with industry during this time (Martin and Etzkowitz 2000). The point is that one can trace relevant science back very far, while "Pure science might be the aberration" (Rip 1997:634).

3.5.1 Is basic research becoming more important?

Some interviewees judged that conditions were getting *better* for basic research in the current climate. Several scientists in the UK commented that the political climate for basic research is better than it was in the 1970s and 1980s. One reason given for the increased importance of basic research is the emergence of certain new technologies (such as biotechnology) which require very basic research but then quickly produce marketable products (Elzinga 1985) – now a 'fundamental' breakthrough can simultaneously be a commercial breakthrough (Crook 1992). This is how 'strategic' research is often described. A UK policy maker observed that now it is often difficult to make a distinction between basic and applied research

> Because the speed of research is increasing, because the speed of moving from discovery to exploitation is increasing, and because the same individual people can be involved in any point of the cycle.

One US policy maker ascribed this phenomenon to more advanced instrumentation; because tools are better, it is possible to go straight from the modelling stage (often involving computer imaging) to development, without having to go through the traditional intermediate phases. The interviewee mentioned pharmaceuticals in this context but implied that this was occurring more generally. This could also feed into the justification for the funding of basic research; a UK policy maker pointed out that because of the rapid pull-through from basic research into application it was now easier for the public to accept the importance of basic research. Yet if these suggestions are correct and basic research is becoming more important, it may be that because of its closer links with technology the research itself is changing in subtle ways.

3.5.2 Changes in the way the term is used

Scientists perceive that there are increased pressures for applicability in grant proposals, and this can filter through to the way they portray their research. This is a demonstration of how changes in the process of knowledge generation and creation affect the characterisation of basic research and the way in which the term is used. Scientists described how research could be made to appear *either* basic *or* applied depending on the circumstances. As a US physicist said of his research, "for me it's very beautiful fundamental physics but it's so close to a lot of industrial processes that it's very easy to write a grant that looks strictly applied". However, scientists maintained that the actual *content* of their research was not being affected at all, and that the changes they had to make to their research were merely cosmetic. In this way the ideals of curiosity-driven, autonomous research which were described above (section 3.2) are protected from adjustments that some scientists feel they have to make in order to emphasise certain features of their research to get funding.[13] The objective of 'knowledge for its own sake' is retained, even in the face of pressures for relevance.

3.6 How useful is the term?

Discussions of the *use* of the term with interviewees revealed a wide range of views about the importance (or, most surprisingly, the lack of importance) of 'basic research' itself. Overall, the concept seemed to be most useful in situations where the scientific and policy communities had to interact.

Several interviewees thought the term was completely unproblematic. For example, it was defined as what is described by the US Office of Management and Budget or the Frascati manual (this reminds us of the importance of the codification and use of basic research as a statistical category). As a US policy maker put it, "most people sort of agree what basic research is".

Other policy makers and scientists stressed how they were obliged to use the term when often they would rather not. Some *resisted* making distinctions between different types of research. They thought that forcing research into categories of basic and applied could be unhelpful. A UK biologist said it "can be used to damn you if people want to", and a US biologist worried that when the distinction enters the funding arena "it can really lead to misuse". This reminds us that the categorisation of types of research is crucially important since it can determine whether a piece of research is funded or not.

Many people described how the term could be unhelpful. A UK policy maker stressed that the whole activity of trying to classify research types was rather pointless ("on a bad day it's

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[13] Rip (1997), however, argues that changing the emphasis of one's research in this way will have more substantial effects.

counting angels on the head of a pin"). A US policy maker also doubted the importance of the term, saying "It's just a flashback to an earlier way of thinking, it doesn't mean much".

When comparing scientists and policy makers and their attitude towards basic research, questions that arise are: Who *owns* the term? Who has authority over its definition? Many policy makers said that the terms 'basic' and 'applied' were used by scientists, and they used them in order to communicate with scientists. Scientists, however, frequently stressed how they do *not* use basic/applied terminology on a day-to-day basis, but only when they have to present themselves for assessment or when they are seeking funding. Rather than talking about 'basic research', scientists are usually more involved in discussing the particular problem that they are currently working on. The above examples suggest that it is when the worlds of science and policy have to *interact* with each other that the term is drawn upon, and that the concept is less of an issue when such an interaction is not taking place.

This hypothesis was supported by a US biologist who said that in his daily work the term was not important to him, but that he would use it if he thought there was an expectation that his work would produce applications:

> If I'm talking to someone who's from a commercial concern I will very quickly in the conversation use the term 'basic'. Just because I just want to make it clear to them that I don't foresee I'm going to have something patentable or anything else during some reasonable time span of my grant.

By referring to his work as 'basic', he is signalling that he does not have anything commercially profitable to offer. This example also demonstrates how recent changes in knowledge ownership and intellectual property have affected the way the term is used. In today's climate, where there are increasing pressures for university scientists to produce patents, it is important for them to define themselves as doing 'basic research' (see section 3.7).

A similar phenomenon was apparent at the broader programme level. One US policy maker gave the example of the US Department of Energy whose "fusion program used to be applied and now it's all basic". This was because it was realised that there would not be an application for fusion power in the next 40 years. Since practical outcomes were not forthcoming, it seemed more suitable for the Department to classify their research as basic research.

It appears that the definition becomes useful when there are specific external pressures to describe research in a certain way, for example in applying for funding, or when the applications of a piece of research are being demanded. The term 'basic research' is invoked in circumstances when scientists and funders interact in decisions about research.

3.7 Different ways in which basic research is measured

This background document has mainly focused so far on the ways in which the term 'basic research' is used by scientists and policy makers, but it is necessary to briefly mention some of the efforts which have been made to measure basic research for science policy purposes.

Publications, and in particular articles in international peer-reviewed journals, have traditionally been used for measuring the output from basic research. In using publications as a proxy measure for basic research, one is implicitly adopting a definition of basic research in terms of its disclosure norms. It should be remembered, however, that industry may publish the results of its research quite extensively and that applied research can also be published (see section 3.1.5). In order to make the analysis of publications more accurately reflect the *type* of research, one can attempt to categorise journals according to the nature of their contents – for example, Narin *et al.* (1976) classify journals into four research 'levels' depending on how basic or applied each journal is.[14]

Another frequently used indicator of basic research is the number of citations received by a publication in subsequent scientific work which is often taken to reflect its impact and perhaps even its quality. This indicator evidently relates to the intentional definition of basic research – that the aim is to further knowledge so success in achieving this will be reflected in the use made of a particular research finding in the subsequent scientific literature. It is also associated with the 'generality' version of the epistemological definition of basic research in that the more basic (or the 'better') the research the more likely it is to be found relevant to a wide range of phenomena. Empirical support for this comes from the finding of Narin *et al.* (1976) that more basic publications tend to be more heavily cited, with applied publications tending to cite more basic ones but not so much *vice versa*.

More direct measurement of basic research, or more specifically of the inputs to it, can be found in the figures that are produced by governments on the amount of spending on basic research. In the UK, where levels of spending on both 'pure basic' and 'oriented basic' are collected, it is the funding councils who are responsible for calculating spending on the basis of official government definitions. This means that the interpretation of the definition can vary depending on the policy official who is responsible for collecting the data (see for example the problems some policy makers have in allocating research to 'pure basic' described in section 3.1.8). This suggests that similar problems in allocating different types of research to categories occur in other countries.

To examine whether the nature of basic research is *changing*, it is possible to examine the different institutions involved in writing scientific papers. Private firms are producing greater numbers of publications than previously and there are also more collaborative publications (both internationally and between different types of institution). This shows that non-academic actors publish and to a growing extent (Katz *et al.* 1995, Iversen and Kloudis 1999). University research is increasingly seeking patent protection and patent-to-paper citations are growing rapidly, implying that the links between science and technology are becoming closer in certain fields (Narin *et al.* 1997). This is further evidence that the patent/paper divide which has been used in the past to signify the basic/applied boundary is becoming increasingly less appropriate.

4. Policy implications

4.1 Advantages of retaining the current terminology

We can see that there are many complications involved in both defining and measuring basic research. Should we attempt to develop a *new* categorisation of research-types to replace the existing way of differentiating research activities? Discussions of this issue with interviewees suggested that many would be unhappy with changing their categories; they thought re-classification would be too demanding because the current terms are so well established. It was argued that the current terminology provides a useful 'shorthand', and even if it is "rough around the edges" (US policy maker), it allows trends to be analysed and international comparisons to be made. The NSF uses the longevity of the basic/applied scheme to justify its use of this

[14] This categorisation was originally based on citation patterns in the biomedical literature. It was shown that journals tend to refer preferentially to research which is more 'basic', and in this way a hierarchy of journals can be generated.

terminology, stating that these terms "have been in place for several decades and are also generally consistent with international definitions" (National Science Board 1998). It was thought to be very hard to get any agreement on alternatives, since if a new suggestion is put forward it is not clearly "prettier than the one on the stage" (US policy maker).

Many policy makers mentioned Stokes' (1997) book, *Pasteur's Quadrant*, which obviously had made an impact, especially in the US policy community. However, what was surprising was that generally these policy makers thought that it would be impossible to operationalise Stokes' scheme. Although Stokes and others have pointed to a conceptual gap in the way research is classified (detailed in section 3.3.1), and despite a level of dissatisfaction with the basic/applied dichotomy (see section 3.6), it appears that many actors want to retain the current methods of distinguishing between research types. This may be partly due to the importance of the ideal of basic research among scientists, discussed above (section 3.2).

Another reason why the terminology does persist, despite its shortcomings, is because the basic/applied distinction came to be used in statistics and measurement of R&D (Godin 2000). It has been a recognised category in the Frascati manual since its first edition (OECD 1963 – although here it was called 'fundamental' research). It has also been used as an accounting category for the NSF since its formation. Godin (2000) argues that because the category was measurable it became stabilised, and because it is stabilised there is tremendous inertia and it would be very difficult to get a consensus to change it. He claims that "Without surveys and numbers the concept would probably never have congealed – or at least not in the way it did: criticisms were too frequent" (p.18). These arguments suggest that there will be pressures to retain a category whose boundaries are blurred, simply because it can be measured, however imperfectly. This point is also supported by Porter (1997) who holds that statistics regulate economic and social life, rather than just describe it. He says that "even the collection and publication of statistics tends to alter or at least to crystallize what it sets out to describe" (p.101), and we see a prime example of this 'crystallization' with basic research.

4.2 Advantages of changing the terminology and tentative suggestions

Alongside this reluctance to alter existing schemes, there are simultaneous pressures to change the current terminology so that it more accurately reflects changes in the funding environment (see section 3.3.1).

The suggestion here is that rather than trying to pin down basic research, we should perhaps see it as a "family resemblance concept" (see Dupre 1993:242). If a concept is one of family resemblance, it is impossible to specify a set of necessary and sufficient conditions which will guarantee membership in this 'family'; instead we find overlapping similarities. Family resemblance can be defined more precisely as a 'polythetic concept', a term from biological taxonomy (Sokal and Sneath 1963). Here members of a group share some characteristics but no one characteristic is essential. Figure 2 illustrates this point (feature A might be that the research is epistemologically general, feature B that it is driven by the curiosity of the researcher and so on).

Figure 2 – A polythetic concept

Various definitions of basic research

	1	2	3	4
Features	A		A	A
	B	B	B	
	C	C		C
		D	D	D

After Sokal and Sneath (1963).

It may be that actors and analysts have overlapping ideas about the definition of basic research but very few of them would ascribe *exactly* the same set of features to basic research. In short, it might be more helpful to think of ways of understanding a concept which does not have one set definition, but which can have flexible boundaries and multiple possible definitions. It appears that it is the ambiguity and the complexity of the term which make it useful to actors. For example, as a scientist, if you describe yourself as doing basic research you can show that applications should not necessarily be expected and associate yourself with certain institutional norms. As argued above (section 3.3.1) basic research may be such a pervasive concept precisely because it has many different attributes and can be made use of in many different situations.[15]

5. Conclusions

This paper has demonstrated the flexibility of the concept of basic research, and illustrated the many dimensions of the term. Six different ways in which basic research is defined in the current science policy context were identified, showing the wide variety of definitions of basic research that are in circulation, and the limitations of each of these definitions. Additionally, the importance of the *ideal* of basic research, which often contrasted with the reality of research practice, was demonstrated. Alternative categorisation schemes (such as that of Stokes 1997) were helpful in revealing the dimensions of basic research but did not ultimately resolve the problem of the definition of the term. Several justifications for funding basic research under the headings of cultural and instrumental justifications were outlined. There were concerns that the emphasis of today's funding environment on increased instrumentality and accountability would have negative implications for basic research, although some interviewees argued that basic research is actually becoming *more* important because of its close links to new technologies in certain fields. It was shown that the term 'basic research' itself is most commonly used in interactions between scientists and policy makers regarding decisions about research. These complexities concerning the definition of basic research mean that it is not simple to measure basic research. The *values* attached to basic research persist, as does the *utility* of the term as a category for science policy, partly because of its flexibility but also because of its 'crystallisation' in statistics, leading to a reluctance to modify current terminologies. It was suggested that in any future attempts to define basic research we should note that the term has many dimensions and that it can be defined differently in different situations.

In summary, we need to be aware of the diversity of definitions of basic research that can be drawn upon, the value of the term in the self-image of scientists, and the persistence of the term.

[15] In this way basic research can be described as being used for 'boundary work' (see Gieryn 1999). This is where the boundaries around science are drawn differently in various circumstances depending on the interests and objectives of the relevant actors.

With a greater awareness of the extent of the ambiguity in the concept of basic research it would not be automatically assumed that everyone is referring to the same set of ideas in discussions about basic research. Misunderstandings could be more easily clarified by identifying the manner in which different actors were distinguishing basic research in particular circumstances.

A further point is that our understanding of basic research will depend on prior ideas about what basic research should be for. For example, if we conclude that basic research should be funded primarily to increase economic growth and innovation, we might define basic research as unpredictable research and justify it on the basis that it will result in profitable novel inventions. A problem with this perspective, however, is that the pressure to make basic research more relevant to private industry is a development which many scientists find threatening (section 3.5). There are alternative views on the purpose of science which could be adopted instead. We could conceive of a society where science was *only* funded for cultural reasons. Here basic research would be defined as ‘knowledge for the sake of knowledge’ and it would be justified because of its intrinsic value. As discussed in section 3.4 above, however, this would almost certainly lead to greatly reduced funds for basic research. This perspective on basic research might also be problematic because maintaining that basic research is ‘knowledge for its own sake’ implies that it can ignore the needs of wider society, meaning that demands for accountability would inevitably arise.

An alternative ‘instrumental’ reason for the funding of science is social welfare (see Figure 1), and if this was the objective of science, then basic research could be defined as independent research and justified because it can provide neutral advice to governments and other interest groups. But even research that is ‘democratic’ and involves the public may be still considered by some scientists to be a threat to ‘basic’ research, as discussed above (section 3.5). In the case of both the democratisation and the commercialisation of science, wider society has a greater role in making decisions about science and the role of the autonomous scientist is brought into question (see also Nowotny 1997, Crook 1992, Slaughter and Rhoades 1996). Hence commercialisation and social welfare objectives might have similar effects and give rise to similar worries. However, there could be potential benefits from being aware of this third reason for funding science. It would give us a means of assuring the public that basic research is accountable, but not narrowly accountable in a commercial sense. These are tentative suggestions, but the argument being put forward here is that the definitions and justifications of basic research that are used will be dependent on pre-existing ideas about *why* science should be funded. These ideas are, in turn, dependent on society’s values, aspirations and priorities.

To conclude, the overall aim of this background paper has been to demonstrate the complexity of the concept of basic research, its flexibility, utility and persistence. In raising these issues we hope to initiate fruitful discussion and debate on our changing conceptions of basic research.

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