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Geography of Scientific Knowledge: A Proximity Approach

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Abstract

The geography of scientific knowledge is defined as the replication process of locally produced knowledge claims. Proximity in social, cognitive, and physical dimensions promotes the sharing of tacit knowledge. Thus, given the complementarity between tacit and codified knowledge, proximity supports the replication of codified knowledge claims. Distinguishing between controversial and uncontroversial contexts, one can understand the sociology of science as explaining the behaviour of scientists from their proximity to other scientists, and the sociology of scientific knowledge as describing the processes that constitute the proximity between scientists.

Keywords

replication, knowledge claim, proximity, mobility, controversy, incentives, sociology of science, economics of science, geography of science, sociology of scientific knowledge

Geography of Scientific Knowledge: A Proximity Approach

I. Introduction

With the rise of sociology of scientific knowledge in the 1970s, sociologists started the positive study of scientific knowledge production. It marked a break with philosophy of science that took a normative perspective on science. Rather than posing the traditional philosophical question under what conditions empirical knowledge can be said to be scientifically true, sociologists started to analyse the conditions under which a knowledge claim becomes accepted among scientists as being scientific (Gilbert 1976; Shapin 1984; Collins 1985). The rise of sociology of scientific knowledge also marked a break with the classical sociology of science programme (Merton 1973), which looks at the institutions governing scientific activity rather than the conditions under which a knowledge claim becomes accepted within science.

Recently, some sociologists of scientific knowledge have paid more explicit attention to the geography of scientific knowledge production. Following the distinction between space and place (Castells 1996), scientists' activities can be described in terms of the physical sites where people produce knowledge claims ('space of places'), and in terms of communication networks of exchange through which knowledge claim circulate ('space of flows'). It will be argued that a perspective solely focusing on place specifities in scientific knowlegde production is not fully consistent with sociology of scientific knowledge, since a place perspective does not address the central question how knowledge claims become accepted elsewhere (Shapin 1998). To say a new knowledge claim originates from a specific 'place' is to play down the importance of past claims on which a claim builds and, equivalently, the importance of future claims that will build on a claim. A knowledge claim always originates from empirical observations in a particular place and it becomes accepted as scientific only by 'travelling' to other places through texts, instruments and people. The central research question for a geography of scientific knowledge holds under what conditions a knowledge claim originating from one place becomes accepted as scientific in other places (Shapin 1998).

Analogous to the distinction between sociology of scientific knowledge and the classic sociology of science, it is proposed here to use the term *geography of scientific knowledge* for the study of how knowledge claims become replicated across different places, while *geography of science* (or geographies of science) as used elsewhere can be reserved to cover the larget set of questions regarding the local shaping and spatial diffusion of scientitific practices (Barnes 2001; Livingstone 2003; Naylor 2005; Powell 2007). Thus, whereas geography of scientific knowledge – by sociology of scientific knowledge – concerns the process rendering claims scientific, geography of science looks at science in all its facets yet without specific interest in the epistemological question. Note here that the same distinction between science and scientific knowledge has been made regarding the economics of science and the economics of scientific knowledge (Hands 1994).

Thus stated, the central question in the geography of scientific knowledge is a reformulation of the question posed by sociologists of scientific knowledge. This allows one to shed a new light on scientific knowledge production by bringing in concepts and methodologies from geography. In particular, we will make use of the concept of proximity to disentangle the various types of relationships between scientists – cognitive, social, and physical – that support the acceptance of knowledge claims. We aim to make three contributions. First, we want to outline the contours of a geography of scientific knowledge by formulating its research question as a question regarding the conditions under which a knowledge claim originating from one place becomes accepted as scientific in other places. This research question follows from the distinction between knowledge as know-how and knowledge as a claim. Second, we put forward a candidate explanation based on the proximity concept. Our explanation will emphasise the role of physical proximity without privileging it over other forms of proximity that regulate the replication process of knowledge claims. Third, by distinguishing between controversial and uncontroversial contexts, we aim to synthesize the main tenets of sociology of science and the sociology of scientific knowledge into a unified proximity framework. Where the sociology of science explains the behaviour of scientists from their proximity to other scientists, the sociology of scientific knowledge describes the processes that constitute the proximity between scientists. That is, where in the former proximity relations are treated as exogenous, in the latter it is treated as endogenous.

In order to put forward a framework for geography of scientific knowledge, the scope of the the present paper is deliberately limited. The argumentation that follows is confined to experimental laboratory science and we do not claim to cover all forms of scientific knowledge production. Some of the arguments that are being made in the context of laboratories and experimental science may not carry over to knowledge production using other research methods. Obviously, a future geography of scientific knowledge should also cover scientific knowledge production of a non-experimental kind. The focus on how claims are being replicate among scientists constitutes a second major limitation. We are not concerned here in how knowledge claims become replicated by society at large nor in how local social contexts affect the particular content of knowledge claims or the practices of replicating these claims (cf. Barnes 2001; Whatmore 2009). Thus, our framework is not intended to be used for the specific issues concerning the 'science-society interface' even though a fully-fledged geography of scientific knowledge would have to include these questions as well. Rather, we start here by outlining a proximity framework that can be used to study the replication of knowledge claims among scientists who are distribitued in physical, cognitive and social spaces.

We will proceed by focussing our discussion on the problem of replication of experimental results in science (section II). It is then argued that the replication process of experimental knowledge through replication is only *partially* constitutive of the replication process of knowledge claims as knowledge claims can be replicated without the underlying experiment being replicated (section III). We then turn to the role of proximity (in social, cognitive, and physical dimensions) in the replication of knowledge claims (section IV) and how such proximities are constituted (section V). Concluding remarks follow (section VI).

II. On replication in science

The central question in the sociology of scientific knowledge concerns the question how knowledge claims become established as scientific in society. Different from traditional philosophy of science, which treated this question from a normative point of view (when should a claim considered to be true?), sociologists turned to the empirical study of scientific knowledge production and asked the question from a positive point of view (when do people consider a claim to be scientific?). Since scientists' knowledge production is by nature geographically localised in laboratories, the question of how knowledge claims become established as scientific in various places is an immediate spatial one.

In the early stages of what is generally associated with modern experimental science, fellow scientists were indeed often invited to eyewitness an experiment (Shapin 1994). Co-presence of individuals was of importance to multiply witnesses and, hereby, to establish a consensus about what exactly is being observed and how these observations are to be interpreted. Progressively, co-presence became less common, though co-presence still plays a role in team research. Witnessing, instead, has become organised through codification in written reports that are evaluated by peer-review before publication and by reading after publication. The rise of modern science is then to be understood as a new mode of knowledge production in which *empirical* knowledge is communicated in codified form such that fellow scientists can engage in 'virtual witnessing' (Shapin 1984, p. 491). In this manner, witnesses are 'multiplied' and a claim – if judged credible – becomes established as scientific. Without codification of a knowledge claim in a written report, the credibility of a knowledge claim would have to depend solely on the testimonies of those who have witnessed the experiment. In such a system, the production of knowledge claims would be severely constrained by space and time due to the need of physical co-presence. That is, the division of labour among scientists has become possible only through the codification of experimental results in written format by replacing eye witnessing by virtual witnessing.

To develop a geography of scientific knowledge, one can start from the idea that knowledge becomes scientific once a report on specific *experimental* events is accepted as credible by others not co-present at the site of observation. To establish credibility, a written report should not only describe the actual event, but also the laboratory conditions[i] (objects, physical conditions, equipment, protocols, methodologies, etc.) under which the event is generated, as to allow fellow scientists to attempt to replicate the experiment at different sites. Subsequent replications of an experiment by fellow scientists, if successful, lead to the accumulation of confirmations of the original claim. It is common to view the process of successful replications of experiments at different sites as evidence that the original knowledge claim is universally true. Accordingly, scientific knowledge is commonly considered as 'placeless' (Livingstone 2003) in the sense that claims that have been proven replicable in different places hold independently from the observations of scientists who have been involved in the process of successive replications. Even though laboratories have the capability to replicate findings with a high degree of precision, and hereby are able to 'construct predictability' (Nightingale 2004), this does not change the fact that a knowledge claim can become established as scientific without any other researcher ever attempting to replicate the experiment underlying the knowledge claim. Fellow scientists may accept an unconfirmed knowledge claim in the case that they trust that the experiment was carried out correctly as written down in a report. Trust depends implicitly on the belief that if the experiment would be replicated, it would be confirmed. This belief is in essence no different than the belief that an experiment that has been confirmed in the past, will again be confirmed in future replications – better known as the problem of induction.

Following this reasoning, Shapin (1984) argued that the establishment of an empirical knowledge claim as scientific is fundamentally driven by the perceived replicability of the experiment, and not necessarily by actual replications of the experiment, that is, the replication of the *know-how* required to replicate the experiment. The codification of experimental results in a report can thus be considered as a rhetoric act to convince the reader that the experiment described can be replicated in different places and at different times provided that the laboratory conditions as described in the report are replicated as well. The disclosure of the laboratory conditions is essential to suggest that the report is trustworthy in case any scientist would decide to attempt to replicate the experiment. It is for this reason that the description of laboratory conditions in itself may be sufficient for a *knowledge claim* to become established as scientific, that is, as credible.

The distinction between know-how and knowledge claim extends to the distinction between the replication of know-how and the replication of a knowledge claim. In the instance of replication of know-how, the actual replication process of an experiment at a different site leads to the replication of know-how developed elsewhere. In the case of the replication of a knowledge claim, some of the *implications* of the experiment are replicated as an input to design a new research project, without the experiment itself being replicated necessarily. The replication of a knowledge claim occurs when it is used as an input in the production of *new* knowledge claims. Production and replication are thus intimately interrelated (Callon 2002). In evolutionary terms, knowledge claims are replicated in new knowledge claims by recombination.[ii] Through the replication of knowledge claims, particular claims are being gradually selected and hereby become accepted.

The large majority of replication processes in science can be characterised by replication of *knowledge claims* rather than of replication of the know-how required to replicate an actual experiment. Previous knowledge claims guide the production of future knowledge claims through their use in assumptions, methodologies, instruments or interpretations. Research projects build on previous research by extending it to new domains and improving its levels of precision, not by attempting to perfectly replicate past experiments. In doing so, previous claims are invoked, and thus replicated, to support a new claim. Even if a knowledge claim is questioned by the production of a new claim, the claim is still being replicated in that it is regarded as relevant in the scientific discourse. Only by remaining silent about the knowledge claim, the replication of a knowledge claim can be avoided (Collins 1985, p. 151).[iii]

III. Tacit knowledge

The distinction between know-how and knowledge claim relates to the distinction between tacit and codified knowledge. The set of instructions as codified in a scientific paper are generally insufficient for fellow scientists to be able to replicate an experiment. The codified instructions have to be complemented with the relevant tacit knowledge that is required for scientists to fully understand a knowledge claim and to be able to replicate the underlying experiment (Collins 1985; Balconi et al. 2007). As we defined know-how as the knowledge that is produced when a scientist, or group of scientists, carries out an experiment, know-how thus consists of both codified and tacit elements.

The definition of tacit knowledge used here is taken from Collins, who defined tacit knowledge as:

> "knowledge or abilities that can be passed between scientists by personal contact but cannot be, or have not been, set out or passed on in formulae, diagrams, or verbal descriptions and instructions for action." (Collins 2001, p. 72)

The definition of tacit knowledge by Collins (2001) does not imply that tacit knowledge cannot be codified. Indeed, an important part of scientists' activities involves the codification of tacit knowledge such that it can be transmitted verbally or textually. However, the possibility of codification of tacit knowledge in turn does not imply that codified information can be exchanged unambiguously as the receiver of codified information still requires complementary tacit knowledge as 'interpretative skills' to interpret the information content as meaningful in a particular material context (Balconi et al. 2007, p. 836 and p. 842; cf. Nelson 2003, p. 911). In this view, codified information only becomes codified knowledge if the receiver has the complementary interpretative skills that are required to transform the codified information into a meaningful interpretation.

With the production of a *new* knowledge claim resulting from a new experiment, *new* tacit knowledge is produced as well resulting from the experiment. The subsequent publication of the new knowledge claim requires the readers to possess the new tacit knowledge as well, if they are to successfully replicate the experiment underlying the knowledge claim. Since the production of new knowledge claims is accompanied by the production of new tacit knowledge, the problem of replication of experiments lies in the problem of transfer of tacit knowledge. If indeed the replication of experiments involves both tacit and codified knowledge, and the tacit elements of knowledge cannot be transferred perfectly among scientists, scientists can never be fully ascertain that they replicate an experiment perfectly. That is to say, if a laboratory replicates an experiment and comes up with (slightly) different results, the scientists involved cannot ascertain whether the divergence in results means that the original claim has to be rejected or whether the replication experiment has been carried out in the wrong way, that is, in a different way than what has been reported in the original claim.

The fundamental problem in replication experiments lies in what Collins called ‘experimenters’ regress’, which:

> “arises because the skill-like nature of experimentation means that the competence of experimenters and the integrity of experiments can only be ascertained by examining results, but the appropriate results can only be known from competently performed experiments, and so forth. Other ways of testing for the competence and integrity of experiments, such as tests of tests, turn out to need ‘tests of tests of tests’ and so on.” (Collins 1985, p. 130).

In short, good equipment produced correct measurements, but what counts as a correct measurement is whatever good equipment produces. To establish whether equipment is properly working, one can carry out an experiment with a ‘known’ outcome, but this outcome has become established in other experiments using the equipment at hand. Alternatively, one may test the working of equipment using test equipment, but this moves the problem one level up (“tests of tests of tests”). The implication of the experimental regress thesis holds that the truth status of a knowledge claim cannot be decided on the basis of experimental evidence alone, but also involves a process of consensus building among the scientists involved.

Geographically, knowledge claims stem from research carried out in specific local contexts, yet for the knowledge to become established as scientific is has to become perceived as being ‘placeless’. The global network of scientists and their laboratories constitutes a ‘transportation infrastructure’ to replicate empirical findings from one place to another. In this context, Nightingale (2004) speaks of social and physical infrastructures that need to be in place to construct predictability. If replications could be perfect, the geography of scientific knowledge would be trivial (of course scientific knowledge is being produced in certain places) and at the same time irrelevant (for knowledge to become scientific is precisely to become placeless through replication). Such a conclusion can only be drawn if one would hold on to idea that experimental findings can be perfectly replicated, and hence, be attributed a truth status that in independent from place and time.

Since experimental findings cannot be replicated with full certainty, and many findings are never replicated in the first place, the geography of scientific knowledge should not be equated with the replication process of experiments. Rather, scientific practice involves the replication of knowledge claims. Science as a process can thus be considered to be first and foremost as a replication process of knowledge claims. As to become accepted as scientific, a knowledge claim has to diffuse between physical sites, but for this to happen the tacit knowledge that was involved in the experiment reported, need not be replicated between physical sites. The central research question for a geography of scientific knowledge thus becomes: *what determines that a knowledge claim is replicated in different places as to become accepted as scientific?*

IV. Proximity

The problem of experimental regress implies that one cannot explain the credibility of a knowledge claim from the empirical evidence supporting the claim alone. Since empirical evidence can only be invoked as a partial explanation of the credibility of a knowledge claim, the unit of analysis in geography of scientific knowledge can be taken to be the interaction between scientists who mutually judge the credibility of their knowledge claims. More precisely, following Shapin (1995, p. 261), any explanation of the credibility of a knowledge claim has to specify the relationship between the one(s) putting forward the claim (who we will call here 'claimant'), and the one judging its credibility (who is called here 'evaluator').

Since the replication of a knowledge claim takes the form of an input in a subsequent knowledge claim, credibility emerges from the re-use of previous knowledge claims. Re-use here means that a claim is used to think about new research questions, to design new experiments, to interpret its outcomes, and to codify results in written form (Dasgupta and David 1994, p. 500). Since knowledge claims are replicated in subsequent knowledge claims in these manners, the chance of a claim being replicated will depend not only on its credibility, but also on its usefulness in guiding new research projects. Thus, for a claim to be replicated as an input in a subsequent empirical research context, evaluators should consider the claim to be both *credible* and *useful*.[iv] The question becomes how an evaluator established the credibility and usefulness of a claimant's knowledge claim.[v]

We answer this question by focussing on the relationship between the claimant and the evaluator. This micro-perspective may be questioned since, in many cases, the evaluator will not judge the claimant's credibility of a knowledge claim directly, but rather will base his/her judgement on the overall scientific reputation of the claimant built up through past performance. Generally, highly reputed scientists see their claims being replicated much more widely than non-reputed scientists. However, the reputation that scientists built up in the past is based on previous knowledge claims that have become accepted widely throughout their community. Thus, even though reputation plays probably an important role in the assessment of the credibility of a knowledge claim, this reputation has been the outcome of past replication processes of knowledge claims. The question remains how an evaluator established the credibility and usefulness of a claimant's knowledge claim.

To characterise the relationship between claimant and evaluator, we will make use of the notion of proximity as developed in economic geography (Rallet 1993; Rallet and Torre 1999; Boschma 2005; Torre 2008).[vi] Proximity simply denotes the inverse of the distance between two scientists. Proximity can refer to physical proximity in the literal sense of physical distance separating two scientists (which Rallet and Torre tend to call geographical proximity), or it can refer to other forms of proximity. The thesis that is developed in detail below holds that the more distant claimant and evaluator are in physical and non-physical spaces, the less probable the claimant's knowledge claim is being replicated by the evaluator.

To be able to judge a claim, cognitive proximity (Cohendet and Llerena 1997; Nooteboom 1999) seems to be by far the most important factor. *Cognitive proximity* in a narrow sense can be defined as the extent to which two scientists share the code of communication that is used in a particular disciplinary context. Following this narrow definition, cognitive proximity allows an evaluator to de-code the written report containing the knowledge claim as to assess its credibility and usefulness. Cognitive proximity, in the sense just defined, is consistent with the idea that knowledge can be transmitted as information as long as sender and receiver use to the same code to code and decode the message (Cowan et al. 2000). The idea that knowledge can be exchanged as codified information provided that two agents share the same codebook has been criticized on several grounds (Nelson 2003; Nightingale 2003; Balconi et al. 2007). A codebook can be seen as a language that has to be shared between agents to allow for verbal and written communication. However, since languages cannot be fully formalised, the meaning of symbols referring to empirical objects remains to some extent tacit. A language cannot be taught but must be learned in practice by participating in material contexts in which a language is used in a specific way. In this context, Balconi et al. (2007, pp. 840-843) distinguish between tacit knowledge of the physical ('skill-like') type and of the cognitive type. The latter type is often overlooked, but may be more important to understand the replication process of knowledge claims. Tacit knowledge of the physical type can usually be codified relatively easily while the tacit knowledge of the cognitive type cannot. What is more, in the codification process of tacit knowledge of the physical type (e.g., into computer algorithms), new tacit knowledge of the cognitive type is being created concerning the appropriate use of codified information in particular research contexts.

Generally, the evaluator cannot fully judge the credibility and the usefulness of a claim from a text alone. As text length is limited, many details will be left out. What is more, the tacit knowledge involved in the experiment is not transmitted with the text. Thus, an evaluator would benefit from having similar tacit knowledge as was involved in carrying out the experiment. These 'intellectual skills' (Balconi et al. 2007, p. 842) allows an evaluator to 'read between the lines' and to better understand how the experiment what carried out, whether the observational reports are trustworthy, and how likely the experiment can be replicated with a high degree of precision. Thus, cognitive proximity in a broader sense can be taken to mean the extent to which claimant and evaluator share codified and tacit knowledge regarding the experimental context in question. It is cognitive proximity that allows scientists to judge a knowledge claim without any face-to-face interaction between claimant and evaluator; cognitive proximity is what makes Shapin's 'virtual witnessing' possible. From reading a text, a 'competent' evaluator can judge its credibility and usefulness.[vii] Obviously, few people are cognitively close; typically, high levels of cognitive proximity will only be found in small communities that make up highly specialised sub-disciplines that reproduce themselves by training their own successors. This explains why most claims in science are assessed on credibility and usefulness only within these small communities and without any interference from actors outside these communities. Actors outside these communities including fellow scientists and (a large part of) the public, derive the credibility and usefulness of claims indirectly from the peer assessment carried out within these small sub-communities (Shapin 1995, pp. 269-271).

As argued, the *assessment* of a claim in terms of credibility and usefulness is often carried out without any interaction between claimant and evaluator. As along as the claimant and evaluator have a sufficient amount of codified and tacit knowledge in common, claims can be assessed at a distance. Yet, the subsequent *replication* of a knowledge claim as an input in new experiments typically necessitates, or at least benefits from face-to-face interaction as to *transfer* tacit knowledge, instruments, materials, further information and so on. It is for this reason that some form of interaction between claimant and evaluator often precedes the replication of a knowledge claim in a subsequent claim. This can vary from personal correspondence or small talk to more intensive forms of interaction including site visits, temporary exchange of personnel and collaborative research projects. All these forms of interaction are examples of close interaction rendering these interactions fundamentally different from written communication or oral presentation involving a one-to-many form of interaction. In many cases, such meetings also entail the demonstration of (part of) experiments so as to explain better how certain results were reached (Collins 1985).[viii] *Thus, the replication of experiments, or parts of experiments, is not necessarily being done to confirm or disconfirm a claim, but can also be done for the sole purpose to learn how to carry out an experiment in order to exploit the newly acquired tacit knowledge in new experiments to come.*[ix]

The immediate question that arises from this concerning the replication of knowledge claims is under what conditions scientists are willing to share tacit knowledge, information and other resources.[x] Though scientists' main incentive is to see their knowledge claims being replicated (Hull 1988), their have less incentive to see resources being replicated, since sharing resources allows other laboratories to pursue the same research lines and to pre-empt future publications. Since reward in science is allocated on the basis of priority in research findings (whatever the exact ways in which priority is established), the incentive to share resources is limited.[xi] What is more, sharing resources is is a costly affair, especially for what concerns tacit knowledge that requires teaching and on-the-job training, while compensation schemes for sharing activities are rare. It is in this context that Callon (1994) and Dasgupta and David (1994), though from different perspectives, both criticized the idea of science as a public good as a non-rival and non-excludable good. What characterises a scientific knowledge claim is that despite their codified form, the complementary tacit knowledge is rival (because it is embodied in persons) and can be excluded from others (because it cannot be learnt from reading texts alone).

For an important part, the willingness to share resources is internalised as a norm through socialisation known as the Mertonian norm of communalism (Merton 1973). Not only does this norm promote cooperative behaviour as such, but also the confidence that the knowledge that is being disclosed is precise and made relevant to the interests in the contexts of the research project of the receiver. However, as mentioned before, the importance of priority will lead scientists to circumvent the norm of communalism through various tactics, in particular, by partial or delayed transfer of tacit knowledge and other resources.[xii] Dasgupta and David concluded that despite the culture of communalism in science,

> “wastage must be viewed as a regrettable necessity only if the reward system (…) cannot sufficiently compensate scientists to induce them to develop research tools that would be useable (by anyone) in subsequent inquires.” (Dasgupta and David 1994, p. 502)

Apart from the norm of communalism, an important incentive to share resources is the risk of reputational loss. Within the sub-communities in which scientists are operating, those unwilling to share tacit knowledge will run the risk that third parties will no longer share resources with them once they are notified (Dasgputa and David 1994, p. 504). In this respect, the concept of *social proximity* can be used to explain the willingness of scientist A to share resources to scientist B. Social proximity here refers to the number of fellow scientists A and B know in common. The higher this number, the higher the reputational consequences once the word gets out that A does not behave cooperatively, given that B will warn those that A and B know in common about A’s non-cooperative behaviour. Thus, the higher the social proximity between claimant and evaluator, the higher the willingness of the claimant to share tacit knowledge and other resources with the evaluator, the higher the probability the evaluator will replicate the claim in subsequent claims. As for cognitive proximity, social proximity is typically high in small sub-communities in which the members frequently meet, carry out peer-review and engage in collaborative research projects.[xiii] And, given the specialised nature of scientific knowledge production, cognitive and social proximity will tend to be highly correlated (cf. Breschi and Lissoni 2009).

The importance of cognitive and social proximity to the replication of knowledge claims puts the role of geography into perspective. With physical proximity we mean the role played by the physical distance between scientists. For two scientists to get in contact, they have to spend time and resources with these costs roughly increasing with physical distance.[xiv] Since the transfer of tacit knowledge and most other resources involves face-to-face interaction, possibly organised in multiple meetings, the role of physical distance in the probability of a knowledge claim being replicated, is non-negligible. A second advantage of face-to-face interaction holds that it supports the creation and maintenance of cognitive and social proximity. Face-to-face interaction offers the possibility of having thick and complex forms of interaction in which not only language is involved but the entire behavioural complex. Contrary to modern communication media co-presence enables the establishment of common reference frames through rapid feedback, pointing and referring to objects in real space, informal exchanges during breaks, and a shared local context (Olson and Olson 2000). These interactions conditions are supportive of increasing the cognitive proximity, upon which one can rely in future long-distance communication. And, mere exposure to others and informal interaction in itself affects peoples' feelings about one another, which are supportive of social proximity inducing cooperative behaviour. Though alternative communication media exist that support cognitive and social proximity, these are far less effective (Urry 2002).

The role of physical proximity in the replication of knowledge claims is in line with earlier critiques on the popular conceptions of scientific knowledge as 'a view from nowhere' (Shapin 1998) and scientific laboratories as 'placeless places' (Livingstone 2003). Science would only be truly placeless if the probability of a knowledge claim to be reproduced is fully independent of any form of proximity. Put differently, whether someone replicates a knowledge claim should be independent from his/her distance, in physical and non-physical terms, to those putting forward the knowledge claim. This idea has been underlying the notion of science as a *public good*, that is, as information which use is non-rival and non-excludable to anyone is society. Since scientists replicate each other knowledge claims much easier if they also share the resources, and since proximity promotes resource sharing, the conditions of non-rivalry and non-excludability are best met among proximate scientists. Ironically, the most proximate scientists are those working together within the walls of laboratories, from which a knowledge claim should escape as to become accepted as scientific elsewhere.

V. Controversies

From an economics of science perspective, the imperfect transfer of resources is explained from the private incentives of scientists. Imperfect here means that in contexts where there is no incentive to behave cooperatively, tacit knowledge will not be transferred or only partially, while in contexts where scientists have an incentive to behave cooperatively, tacit knowledge will be fully transferred (Dasgupta and David 1994). By contrast, the sociology of scientific knowledge starts from the premise that full sharing of resources is impossible by definition. Tacit knowledge can only be shared partially and in so far tacit knowledge becomes codified as information, new tacit knowledge is required to understand the codified information. This implies, as explained before, that scientists can never be sure to have replicated an experiment perfectly, due to the problem of 'experimental regress'.

The issue of replication is left untouched in the economics of science program as proposed by Dasgupta and David (1994, p. 499), who stated in the context of priority that:

> "among the discoveries (..) made by rivals involved in parallel research only the first is worthwhile to society; there is no social value-added when the

> same discovery is made a second, third or fourth time. [footnote 34: by this we do not, of course, mean independent confirmation of a scientific discovery, which is a different matter altogether]".

In this respect, economics of science explicitly builds upon what Dasgupta and David (1994, p. 492) call the "classic contribution in the sociology of science", particularly those works influenced by Merton (1973), which do not address the epistemological dimensions of knowledge production. The notion of priority follows from the notion of parallel discovery. Parallel discovery in turn assumes that different laboratories can produce the exact same knowledge, which is the same as assuming that a replication of an experiment can be shown to be perfect.

A different way to approach the subject from an economic angle is to ask to question under what conditions scientists have an incentive at all to engage in replications of experiments. Most attempts to replicate an experiment involve a costly investment. The returns to such an investment depend on the status of the knowledge claim that resulted from the experiment in question. When a knowledge claim is considered to be credible and useful for scientists' further research there is little interest in devoting resources to replication attempts. In case such an attempt fails the replicate the earlier reported findings, fellow scientists will generally believe that the replication experiment has been carried out incorrectly. And in case the replication experiment succeeds to replicate the earlier reported findings, it will be regarded as replicating 'the obvious'. In both cases, the results of the replication experiment may well be deemed unworthy of publication in scientific journals. Given that investments in replications of credible knowledge claims have low returns, whatever the outcome of the replication experiments, few scientists engage in such type of research (Collins 1985).

By contrast, if a knowledge claim is considered *controversial*, meaning that acceptance would contradict many previous claims on which scientists have build their research in the past, scientists will have an economic incentive to engage in replication (Collins 1985, p. 19). In such contexts, replication exercises have the explicit goal to replicate an experiment as to confirm or disconfirm a claim. Given the problem of experimental regress, there cannot be a *fully* agreed methodology that distinguishes between correct and incorrect experiments. Given that controversies often arise with the advent of new instrumentalities rather than from theoretical advances (de Solla Price 1984) there typically exist both technological and methodological uncertainties regarding the assessment of experimental evidence. That is why the outcome of controversies is contingent, at least to some degree, upon the entrepreneurial ability of each participant to find resources required to improve experiments technologically and methodologically, as well as upon the rhetoric ability to convince fellow scientists of the methodological soundness of their own experiment (Collins 1985). If through time no agreed methodology emerges, the different positions can lead a research community to fragment into two sub-communities characterised by their own methodology. Alternatively, one particular position, or a synthesis between different positions, may emerge as a consensus and the community remains intact.

A controversial claim changes the normal practice from the replication of knowledge claims along well-defined research trajectories to efforts of actual replication in a controversial setting of contested knowledge. We defined a controversial knowledge claim before as a knowledge claim that threatens the scientific status of many previous claims; controversy arises not so much because of the results of a new experiment *per se*, but because the acceptance of the claim would require established knowledge claims to be revisited.

Depending on the number and importance of the claims that would need revisiting if a contested knowledge claim is accepted, controversies will tend to involve more or less participants. Some contested claims, if accepted, require only few or unimportant claims to be revisited. Controversies stemming from such claims can be lively, but will tend to concern few people. Other claims would require a major revision of an entire field and will consequently involve many more scientists.[xv]

For the purpose of our proximity framework, the distinction between controversial and uncontroversial knowledge claims has important consequences. Whereas the collaborative behaviour regarding the sharing of resources can be explained from cognitive, social and physical proximity in the case of uncontroversial knowledge claims, this explanatory scheme no longer works for controversial knowledge claims. Characteristic of controversy is there is no agreement on the conditions under which replication attempts can be considered true replication due to the problem of experimenter's regress. Even more so than in uncontroversial contexts, the willingness to share tacit knowledge and other resources is crucial to arrive at a common understanding of divergent results in replication attempts. Yet, the proximity framework that can be used to understand the replication of knowledge claims in successive research projects of different kinds does not apply in the context of the replication of actual experiments. Rather, the proximity between scientists becomes endogenous to the process that unfolds during the controversy. The radical uncertainty regarding the outcome of a controversy, which generally last several years, forces each scientist to 'take sides'. To participate in the controversy, a scientist has to allocate its time and resources among the established and emerging research programmes.

Following the spatial analogy implicit in the proximity concept, such repositioning can be termed *mobility*. One can thus distinguish between cognitive mobility (e.g., moving from the established research trajectory to a competing research trajectory), social mobility (leaving the established community to join a new sub-community) and mobility in the literal physical sense (moving between physical sites). The latter form of mobility is crucial here, because the cognitive and social mobility require face-to-face interaction between likeminded scientists. Co-location enables the creation of strong social networks and joint cognitive investments including manuals, textbooks, software, equipment and, of course, laboratories. Ideally, mobility here involves permanent re-location of those who wish to join an emerging research trajectory as to be able to work together on site on a permanent basis. However, other forms of mobility are possible to established *temporary* physical proximity (Torre and Rallet 2005; Torre 2008) such as workshops, summer schools, site visits, *et cetera*.

The mobility of scientists along cognitive, social and physical dimensions in turn affect the knowledge dynamics. The more people join a particular research trajectory, the more resources become available to produce, and more likely that knowledge claims will be replicate in subsequent research projects.[xvi] The reason why critical mass matters is because experiments that are intended to confirm a claim are typically carried out differently than experiments intended to disprove a claim; experiments done by proponents will more often find confirming evidence and experiments done by opponents will more often find the opposite (Collins 1985). This is not to say that empirical evidence ‘in itself’ does not play a role. On the contrary, the accumulation of empirical evidence is generally decisive in settling controversies. Yet, this process cannot be understood as a process of inductive reasoning as scientists have no firm epistemological foundation to agree on what counts as a replication, which would be necessary to aggregate such evidence.

The epistemological uncertainties that tend to arise when knowledge claims become contested limits the scope for the traditional sociological of science, economics of science included. Understanding the behaviour of scientists from private incentives from which strategic behaviour follows is expected to have much more explanatory power in uncontroversial contexts than in controversial contexts. Here, the methodological rules of the game are stable and scientists can act strategically accordingly in their quest for maximum returns. However, in contexts of contested knowledge claims, the problem of replication reveals the stability of the rules of the game 'break down' as the methodological procedures and technical workings of apparatus itself is put into question. In such contexts, the expected returns of investments in certain research projects can no longer be properly assessed ex ante, since any investment in particular theory or methodology, and the experiments based on these, may turn out to be useless if controversies leads to another theory or methodology to become dominant. At that point, the tacit and codified knowledge that a scientist has acquired looses most of its value, since the results of research projects making use of these knowledge bases will no longer be accepted.

To understand contexts of contested knowledge, sociology of scientific knowledge thus provides an alternative framework that is fully complementary to that of sociology of science. The synthesis of sociology of science and sociology of scientific knowledge is evident when the former accepts that its theories do not extend to controversial contexts of scientific knowledge production and the latter accepts that epistemological concerns can be pragmatically ignored in uncontroversial contexts of scientific knowledge production.

VI. Concluding remarks

The geography of scientific knowledge production is understood as a process through which locally produced claims become accepted as scientific elsewhere. Even if scientists can replicate each other's knowledge claims without any tacit knowledge transfer through face-to-face interaction, such interactions remain important in the replication of knowledge claims. By sharing tacit knowledge through face-to-face interaction scientists are better able to judge the credibility and usefulness of a knowledge claim and, hereby, to build upon each other's findings in a cumulative manner. By implication, the mobility patterns of scientists affect the replication of knowledge claims.

The focus on the replication of knowledge claims can be said to be *inherent* to a geography perspective, and to be fundamental to any geography of scientific knowledge approach. If knowledge claims would replicate through independent confirmations – as assumed in the sociology/economics of science – a new knowledge claim would either become universally accepted if independent experiments confirm the initial experiment, or universally rejected otherwise. In such a world, a *geography of scientific knowledge* would be redundant (even if a *'geography of science'* perspective could still ask the question why certain claims originate from certain places). If, however, the replication of scientific knowledge claims is dependent on physical proximity between scientists, the diffusion of scientific knowledge claims will generally be only partial, and will be contingent upon the patterns of mobility of scientists. Mobility is required to transfer the tacit knowledge that is complementary to knowledge claims in their use in future research. This means that to understand the dynamics of scientific knowledge production the analysis of scientist mobility should be much more central in our research. Likewise, in policy debates mobility should be put more central (Mahroum 2000). It follows from our framework that mobility of scientists may well be key to enhancing scientific dynamism as the physical co-location of likeminded scientists is supportive of the establishment new research trajectories.

In the framework proposed, the question why certain knowledge claims that originate from a certain place become accepted as scientific elsewhere is the prime question, while the question why certain knowledge claims originate from certain places is only of secondary importance. In controversial contexts, the geographical outcomes of replication processes are hard to predict as the proximity between scientists in such contexts is likely to be endogenous to the process of replication. One can easily predict that likeminded mobile scientists will co-locate as to enable the creation of the cognitive and social proximity that is required to transfer the new tacit knowledge. Yet, the precise location where they co-locate is less relevant for tacit knowledge transfer. This means that the location of new research programmes, and the geography of scientific knowledge claims more broadly, is subject to path-dependent dynamics in which 'small events' may lead research programmes to prosper in some locations and to become marginalized in other locations (Arthur 1994).

This fundamental contingency in the geography of scientific knowledge claims does not imply that any place is equally likely to develop successful research programmes. One can think of a host of factors that support the creation of new research programmes in certain places including tradition, reputation, related infrastructure and funding. Yet, as 'carriers of knowledge claims', only mobile scientists are capable of promoting the replication of knowledge claims elsewhere as to have them replicated in subsequent knowledge claims. The fundamental driver of new research programmes emerging in certain places is may thus not be found in the particular characteristics of such prospering places, but rather in the complex network of mobility patterns of key scientists. In turn, mobility decisions may be affected by place characteristics, but will primarily be based on finding likeminded colleagues. Either way, science policy can evidently exert a great influence on mobility patterns even if the exact outcomes of mobility policies will remain hard to predict.

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[i] Some use the term laboratory in a broader sense including, for example, field sites (Gieryn 2006). Though much of the argumentation that follows may remain relevant using an enlarged definition, we do not explicitly elaborate our arguments in terms of this broader sense. Furthermore, we do not mean to cover social sciences and humanities. Though some of the arguments hold equally well for these sciences, there are some fundamental differences, in particular, the possibility that knowledge claims about humans influence their self-understanding and behaviour. On this, see Shapin (1995, pp. 257-258).

[ii] I will not go into the question in what specific ways scientific knowledge production can be considered as an evolutionary process (Campbell 1974; Hull 1988).

[iii] In the Dutch language, there is the appropriate verb '*doodzwijgen*', which is literally translated as 'to silence to death'.

[iv] The two main replication criteria are also visible in the almost standard set up of scientific papers. Relevance is dealt with backward in the review section showing how it builds on previous research (replicating the knowledge claims that are deemed relevant) and forward in the conclusion section showing how the new claim can be made relevant for future research. Credibility is mainly dealt with in the methodology section where the procedures are specified that should be followed in an attempt to replicate the know-how itself, even if most experiments are never replicated. Also note that credibility and usefulness are to some extent interdependent. One can draw a range of conclusions from experimental results ranging from an almost literal description of technical findings to a full-blown argumentation that the findings prove or disprove important theories. The first way of reporting will be most credible, but less relevant for future research, while the second way of reporting will be judged very relevant, but will raise doubts about the credibility of the claim (Pinch 1985). It is often during the review process that the "right" level of credibility and usefulness is negotiated with the claimant having to give in by qualifying its claims as being credible only under particular assumptions or particular contexts, and, therefore, being relevant only to a small domain of scientific inquiry. Put differently, the review process decides the level in the hierarchy of claims at which the article should position itself (Meyers 1985). Of course, the findings may still be judged as being more or less relevant and more or less credible than the author has stated in the published version. In the remainder, we will not go into the review process explicitly.

[v] Consistent with the theoretical framework proposed before, one can follow the replication of a knowledge claim through the citations it receives. We do mean to imply that all citations are replications of knowledge claims, but that the replication of knowledge claims can be studied by tracing the citations to a knowledge claim. A citation can involve a replication of a knowledge claim in the sense that a citation invokes a claim in a particular argumentative context within the citing paper. The use of citations in scientific papers makes it possible to condense the description of the experiments, its findings and its interpretation by citing papers containing the knowledge claims that are implied by the experiment. The evolutionary nature of the replication of knowledge claims can now be reformulated in terms of citations. Each new knowledge claim re-combines earlier knowledge claims by citing a sub-set of papers each containing particular knowledge claims. The meaning attributed to citations here, however, does not imply that citations necessarily reflect 'intellectual debt'. Not all knowledge claims implied by the experiment are cited and that not all citations refer to knowledge claims that are implied by the experiment (Gilbert 1977; Amsterdamska and Leydesdorff 1989; Hicks and Potter 1991; Wouters 1999).

[vi] The proximity concept used here follows the Rallet-Torre-Boschma notion of proximity. On different notions of proximity and its history in the economic geography profession, see Carrincazeaux et al. (2008).

[vii] Indeed, anonymous peer review is based on such judgements without any interaction between claimant and evaluator (even if the editor generally, but not necessarily, acts as a mediator interacting with both parties).

[viii] For an illustrative example, see e.g. the study by Hull (1988) on observational evidence supporting chromosome theory in the early twentieth century, in particular Hull (1988, pp. 56-57).

[ix] Essentially, this is also what is being done when training undergraduate students in replicating experiments (Collins 1985).

[x] A related question is under hat conditions scientists want to share material objects relevant for research. There is evidence that scientists are reluctant to share materials if competition is fierce and the cost of sharing is high (Walsh et al. 2007). Exchange of materials differs, however, from the sharing of tacit knowledge since the former does not require face-to-face interaction *per se*.

[xi] For an illustrative example, see e.g. the study by Biagiolo (2000) on Galileo.

[xii] For an illustrative example, see e.g. the study by Atkinson et al. (1998) on myotonic dystrophy.

[xiii] Note that social proximity coincides with the indicator of tie strength proposed by Granovetter as the degree of overlap of two individuals' friendship networks (Granovetter 1973, p. 1362).

[xiv] Alternatively, if one prefers a more precise definition, one can think of geographical proximity as the generalised travel costs between the two places of residences, where generalised travel costs stand for the cost of transportation and lodging plus the opportunity costs of travel time.

[xv] To be more precise, the exact implications of the acceptance of a contested knowledge claim is not a given. Opponents of a knowledge claim can alternate between two rhetoric positions vis-à-vis the new knowledge claim (Collins 1985, pp. 134-135). They can exaggerate the implications of the experiment by arguing that those who accept the claim, should therefore abandon a very large part of received knowledge claims. Or, opponents can play down the implications of the new knowledge claim by arguing that, even if one would accept the claim, its implications for the larger part of knowledge claims are negligible. This implies that whether scientists put forward their new knowledge claim as having major or minor implications for previous knowledge claims, opponents will always have the opposite angle available to attack it. The controversial nature of a knowledge claim is itself contingent.

[xvi] Also known as increasing returns to adoption (Arthur 1994).