Of communities and individuals as regards scientific knowledge

The first issue that I will tackle will be the issue of the primary epistemic subject. The thesis which I will implicitly be defending will be the following:

An individual X obtains knowledge of scientific claim p in virtue of being a member of a community A that regards claim p as knowledge.

It should be remarked that this claim includes the originator of claim p, that is the first scientist that will claim p. In other words, what the thesis states is that a claim p only becomes scientific knowledge once it's been through a process of validation by a scientific community. Hence a four-step procedure may be described, consisting of the following events in chronological order:

A) Individual X asserts scientific claim p.

B) Individual X proposes scientific claim p to an other person or persons.

C) Community A, which includes individual X, agrees, (after deliberation) to treat claim p as knowledge at which instant it follows that

D) Individual X, as well as the other members of community A, know claim p.

This is meant to be contrasted with the following two-step picture:

A') Individual X obtains knowledge of scientific claim p through perception or inference.

B') Individual members of community X obtain knowledge of scientific claim p each for themselves through their own cognitive capacities supplemented by X's testimony.

The argument I will present proceeds from the observation of scientific practice and hence restricts itself to scientific knowledge, even though in principle it could be extended to other sorts of knowledge.

The strategy that I will follow will be the following. In the first section I will consider the claim “that collaboration plays a causal role in advancing scientists' epistemic goals, and that its growing popularity is a consequence of its effectiveness in aiding communities of scientists to realize their epistemic goals”(Wray 2002). I will conclude that the claim is rather weak in the sense that it only justifies certain sections of scientific practice and does not establish that in principle scientific knowledge is produced in the manner described above.

An attempt to strengthen the thesis will be made through the presentation of evidence that all

through history in what is widely recognised as scientific activity (the activity which claims as its originators the methodological writings of Bacon and Newton) the scientist is never alone, even if they are the single author of a scientific work. I will draw on certain insights from Latour's (1987) study in the making of scientific knowledge to support the thesis that the individual scientist is necessarily surrounded by allies. However, as opposed to Latour, I will only focus on the human allies.

This attempt will consist of two parts, the first being what the exploration of what I term the intra-laboratory aspect of scientific activity, and the second being the public forum aspect. I will conclude that the latter aspect is the stronger aspect in which a claim that the production of scientific knowledge in principle contains an irreducible social element, that is that the appropriate unit of epistemological analysis of the production of scientific knowledge is the scientific community rather than the individual scientist.

Finally, I will elaborate and discuss the thesis that community agreement is constitutive of knowledge, presenting and arguing for the communitarian account of scientific knowledge (Kusch 2002). I will now begin my account by discussing collaborative work in the sciences.

Section One – Co-authorship

The most spectacular examples of co-authorship one encounters, and the ones where most studies focus on, are papers in the fields of high energy physics or particle physics. Figure one represents a sample picture of the first page of a typical paper in high energy physics, which counts 197 authors affiliated with 21 different institutions. Massive collaborations such as this are associated with what several commentators call “Big Science”. Big science projects are costly long-running experiments run by big groups of scientists.

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Search for new heavy quarks at the CERN proton-antiproton collider

UA1 Collaboration

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Figure One: A typical co-authored paper in high energy physics

As Ziman comments as regards such collaborations, “One naturally thinks of high energy physics or space science, where hundreds of scientists must work together for years around an enormous research instrument just to perform a single experiment” (Ziman 2000, p.69). However, as Figure Two shows, “Big Science” projects typically involving hundreds of collaborators, although historically derived from the field of particle physics, are at present not limited to physics, with the example below concerning the sequencing of the human genome.

THE HUMAN GENOME

The Sequence of the Human Genome

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Figure Two: The author section of the paper on the sequence of the human genome

Modern scientists working in such collaborations or in stable research groups cite, at least in public, the muddling of the issue of individual authorship, as evidenced by the following quotation by the 1980 Nobel Prize winner in Chemistry, Paul Berg: “We work as a unit. Very often we cannot identify the origin of an idea. It will have been adapted and modified and changed so often, then it leads to something else, and finally there is a breakthrough.” (Berg 1980) cited in (Traweek 1992, pp.88-89).A distinction that I will build on in the second section, but is worth mentioning here only to point to the differing nature of the institution of authorship in different sciences, is that between a collaboration and co-authorship. Collaboration, is a broader term used to designate research activities where communication between scientists takes place, whereas co-authorship seems to be a specific reward for a successful collaboration that gets registered and can be traced by bibliometric studies. Laudel (2002) makes this careful distinction between collaboration and co-authorship, giving a taxonomy of modes of collaboration, however other commentators seem to equivocate between the two terms, and even in authored papers (as in Figure One) there is mention of collaborators rather than authors. Both Thagard (1997) and Wray (2002) in the philosophical literature are guilty of this equivocation, with the latter going as far as talking about collective but non-collaborative research when credit and responsibility (I assume as expressed through the process of publication) are shared amongst the authors (Wray 2002, p.152). Hence the following bibliometric data should be read primarily supporting the thesis that co-authorship (rather than collaboration) is prevalent in the sciences.

Cronin (2004) and Cronin et al. (2004) present detailed data, decade by decade, on patterns of co-authorship in the literature of three disciplines, chemistry, psychology and philosophy, as representatives of trends in the natural sciences and the humanities. Over the course of the century, the overwhelming majority of articles in chemistry (88%) have been co-authored, whereas the respective figures for psychology and philosophy have been 26% and 2% respectively, showing the wide disparity among academic disciplines. However, the picture changes dramatically if one considers data from the end of the nineties: 99% of articles in chemistry are co-authored, compared to 77% of articles in psychology and 4% in philosophy (B. Cronin 2004). This indicates a clear overwhelming trend towards co-authorship in the natural sciences. Thagard (1997) presents data drawn from scientific and philosophical journals from the year 1992, where more than 70% of papers in *Cognitive Psychology,* more than 80% of papers in *Physical Review Letters,* and close to 90% of papers in *Science* are co-authored, whereas close to 50% of journal papers in *American Economic Review* and *American Sociological Review* are co-authored. These data support the claim that co-authorship is becoming more prevalent in the natural sciences, as well as in other academic

domains. I will now proceed to present a functional explanation of this increase in co-authorship.

1.1 A functional explanation of co-authorship in science

Wray (2002) gives a functional explanation in three steps for the prevalence of collaborative research in the sciences. In the first step he states that “Collaborative research plays a significant causal role in ensuring that scientific communities are able to realize their epistemic goals” (2002). By epistemic goals Wray seems to imply simply the production of new knowledge claims. The author gives the following five ways in which collaboration significantly helps in the realization of scientists' epistemic goals: co-authored papers are cited more often, something which he takes as an indication of better quality research, collaborative research enables certain types of inquiry to take place, collaboration enables the formulation and development of unifying theories, collaborative research better ensures that data will not be lost, high degrees of collaboration are partially responsible for the rapid growth of scientific knowledge, and finally that collaboration is important in the training of young scientists. Wray takes these five ways to be jointly sufficient to support his claim, however, I wish to briefly criticise three of these five ways, with my aim being to show that this first step is more shaky than it appears.

First of all, it seems that some of the empirical evidence purportedly supporting the first way fails to perform this task. Wray states, citing Hull (1988, p.525) that whilst of all the papers published in the journal *Systematic Zoology* in 1953, 1963 and 1973, less than 21% were co-authored, 39% of the papers published in the same journal and cited more than 50 times in other journals in the period between 1961 and 1983 were co-authored. There are two ways in which this claim may fail to support the conclusion that co-authored papers are better quality papers, that is the correlation between co-authorship and better quality research may not amount to causation. The first is if the number of papers published in general rose, and this coincided with an increase in co-authored papers. The first can be demonstrated through a rough numerical example. Assume that the distribution of papers and co-authored papers was 200 papers out of which 10 were co-authored in 1953, 350 papers in 1963 out of which 40 were co-authored, and 450 papers in 1973 out of which 150 were co-authored. This, coupled with the assumption that new research topics are introduced after (say) 1973 would make a rise in citations of co-authored papers unrelated to any issues of quality.

The second way in which the claim that collaborative research is better quality research may be undermined is the simple consideration of the converse claim, that 61% of the papers published in the journal and cited more than 50 in the period between 1961 and 1983 were single-authored. The persistence of single-authored papers being the majority of most-cited papers would be more

impressive if indeed there was a shift towards more costly research.

Secondly, the development of unifying theories seems to apply, as the author recognises, particularly to cases of interdisciplinary research. However, it is not clear that more collaborations are interdisciplinary than not.

Thirdly, the idea of losing data or ideas seems like a weak premise which does not in principle exclude scientists who work on their own, as long as they keep good records. Hence it seems that the first step in the functional explanation that Wray offers is suspect.

The second step in his explanation is the claim that “collaborative research persists because it is so effective at enabling scientific communities to realize their epistemic goals”. In supporting this claim the author alludes to what he terms the context under which the functional explanation he provides works, which he sums up in two conditions which are the condition of epistemic equality among the scholars involved, and the condition that “research in the relevant fields generally requires substantial resources for which there is fierce competition” (Wray 2002). The latter condition and its link with “Big Science” projects however ought not to be underestimated, in the sense that factors other than the purely epistemic ones, such as sociological pressure and lobbying by groups of scientists play a significant role in the allocation of funds that enables large research projects to get off the ground. Examples of this is the failure of the Superconducting Super Collider (SSC) project and the study by Tournon (1993) on the European Synchrotron Radiation Facility. Hence I conclude that the persistence of collaborative research may be as much a matter of non-epistemic factors as of its enabling scientific communities to realize their epistemic goals.

Finally, the third step in Wray's explanation is the claim that “Collaborative research is initially causally prior to the resulting success that it characteristically gives rise to” (2002) . This intended to establish the beginning of the chain of mutual reinforcement between collaborative research and effectiveness in pursuing epistemic goals. The author takes it that it seems implausible that the initial cause for collaborative research would be the success of researchers in pursuing their epistemic goals separately. However, this can be contested by examples such as the recruitment of successful physicists by university departments in order to create powerful collaborations, such as described by Galison (1997, sec.4.3), or the mobilisation of top scientists for collaboration in the Manhattan Project in WWII, where the separate scientists were invited to the collaboration because they were very effective in pursuing particular epistemic goals.

I hence conclude that Wray's functional explanation for the prevalence of scientific collaborations is questionable. Furthermore its dependence on environmental and other local and

contingent factors means that at best it can establish that in certain domains of science in a certain time-slice (mid-twentieth century to the present) collaborative science, particularly as expressed through co-authorship, is more successful than individual research in pursuing particular epistemic goals, to the point of becoming the norm. The defence of collaboration is more a pragmatist vindication and defence of scientific collaboration rather than an epistemic account of the production of scientific knowledge that prioritises the group above the individual scientists. A stronger argument is needed for the establishment of the latter claim.

Section Two: Presences in the laboratory and in research papers

As Ziman remarks, (2000, p.4), arguing that science is a social institution, "It (science) involves large numbers of specific people regularly performing specific actions which are consciously coordinated into larger schemes. Although research scientists often have a great deal of freedom in what they do and how they do it, their individual thoughts and actions only have *scientific* meaning in these larger schemes". This quote seems to accurately describe the mode of work of scientists working in large "collaborations" such as those encountered at CERN and described by Galison (1997) or by Knorr-Cetina (1995).

Furthermore, from a historian's viewpoint, Shapin provides a detailed study of what he terms "the gentlemanly origins of 17th century English experimental and natural philosophy" (Shapin 1994, p.xviii). Shapin situates 17th century science in the realm of civil conversation among gentlemen, and cites evidence of Boyle's rejection of certain practices as incompatible with the practice of such conversation (Shapin 1994, chap.7).

In the above, two senses in which science is a social activity may be discerned. The distinction between these two senses concerns two aspects of scientific practice, what I will term the intra-laboratory aspect and the public forum aspect. In what follows I will elaborate on these two aspects. I will conclude that whilst the first aspect is not sufficient to prioritise the group over the individual as regards the production of scientific knowledge, the second one can fulfil this role.

2.1 The intra-laboratory aspect of science as a social activity

The intra-laboratory aspect of science as a social activity focusses on the interactions that take place among individuals at the time of their induction into the sciences and during their day-to-day laboratory work. These interactions may range from interactions with specialised technicians and helpers, as in the case of Boyle and his French assistant Papin, or Hooke (Shapin 1994, chap.8), to the specialised division of labour taking place in bigger experiments. It is worth remarking that specialization in science and division of labour took place early on in science, at the same time as it

was becoming it was becoming academicised in nineteenth century Germany (Ziman 1976, pp.59-60). Such an arrangement, which was essentially a master-apprentice relationship, had as a result the formation of a 'school of research', where the students were working on problems selected by their professor, adopting their methods and opinions. It is worth remembering that even scientists purported to work 'on their own' or who published on their own, usually worked with assistants. It is telling that even though Galison portrays the emulsion expert Pierre Demers and the physicist Marietta Blau as 'lone guns' representing an older way of working within physics that was rapidly going out of fashion in the interwar years, he does produce evidence that they both worked with small teams of assistants and doctorate students (Galison 1997, chap.3).

My claim would then, as opposed to the criticism that served as conclusion to the previews section, is that scientific activity has always been a group activity, rather than this being an accident of 20th century practice. However, this claim does not preclude the possibility that certain lines of inquiry could at least in principle be open to the individual scientist. Furthermore, as regards the production of scientific knowledge, it can still be claimed that the individual that heads the scientific laboratory or is the first author in a scientific paper still has priority over the rest of the authors or the collaborators. The head researcher is necessary to the production of scientific knowledge as described above in a way that the assistants are not. I will, however, now move onto the second aspect of scientific work, what I have termed the 'public forum' aspect, which I claim is constitutive of scientific practice, and that necessarily links the production of scientific knowledge with scientific communities.

2,2 The public forum aspect of science

The second aspect of science which I believe necessarily involves an appropriate community into the production of scientific knowledge is that it is primarily a conversation among peers, a dialectic process in which scientists attempt to convince their peers that their representation of nature is the most accurate one. In a real sense of the word, even when Isaac Newton was locked away in his Cambridge study, he was engaged in conversation with Gottfried Leibniz on infinitessimal calculus or Robert Hooke on the nature of light. Ziman, in arguing the constitutive role of conversation in science, states that “Science, by its very nature, is a body of *public* knowledge, to which each research worker makes his personal contribution, and which is corrected and clarified by mutual criticism. It is a corporate activity in which each of us builds upon the work of our predecessors, in competitive collaboration with our contemporaries” (Ziman 1976, p.90). Hence science, even in its most theoretical guise, is conceived of as essentially a dialogue among scholars, both of other times and contemporaries. The social element of science, or at least the

indirectly social aspect of building on previous work, is reflected in Newton's famous dictum “if I have seen further it is only by standing on the shoulders of giants”. By this Newton recognises the debt to previous great minds with which he is in a scholarly conversation.

The more proximate aspect of this dialogue is often reflected in the letters (e-mails in the modern world) and published written articles that scientists use to communicate their findings, as well as the conference presentations and scientific colloquia and other gatherings which play an increasingly important role as face-to-face communication between scientists, present in the original Royal Society meetings (Shapin 1994) is once again becoming prevalent through the ease of transport across the globe. The institution of writing in journals[1] helps also helps formalise the competitive or argumentative essence of science. Scientists present their work in a public forum and thus make it available for criticism and refinement. They engage directly with whoever wants to research on the same subject, by challenging each other to come up with counterexamples and counterarguments that would prove them wrong. It is perhaps no accident that the activity that Boyle and Newton were engaging in was termed natural experimental philosophy (Shapin 1994), that is it was related to the argumentative mode of inquiry of the ancient Greeks, albeit with the empiricist restriction that evidence be drawn immediately through the senses and by the questioning of nature through experimentation.

2.3 Science as an argumentative activity

In the same vein of viewing science as primarily an argumentative or rhetorical activity, Latour (1987, chap.1) rather provocatively argues that in science in action (or science-in-the-making, see Gregory and Miller (2000, chap.6 and 10)) what essentially happens is a mustering of opinions and allies, as expressed through the citing of references in a publication. According to Latour, when one is arguing for a scientific claim in a publication, rather than being alone they are immediately surrounded by allies and potential allies, which they can mobilise to support and strengthen the points contained in the publication. Giving an example, Latour claims that to dispute a renowned scientist's claim, one would have to oppose him and his co-workers at the laboratory, the university board that gave them the funds to carry on their research or any other awarding bodies that conferred their trust upon the scientist, the journal referees and editors who selected the paper for publication, even before moving on to read the paper and look into the scientists' whose claims are mobilised to support sub-points in the argument (Latour 1987, pp.31-38). Hence it can be concluded that a whole community of people is enrolled in the expression of a scientific claim, even if the credit goes to an individual scientist. In a way, the individual is simply the spokesman

1 Which is contemporaneous with the institutionalisation of science.

for a large community of people who bestow their trust upon them, and is arguing against the spokesmen of other communities of scientists who may hold opposing opinions. Furthermore, the whole process takes place in front of an attending jury audience who after examining both opinions decide, according to commonly agreed standards, on the endorsement of one or the other opinion. It is to this audience that I now turn my attention, in an attempt to present a more formal account which accords epistemic priority to the community.

Section Three: Communitarianism and collective knowledge

As noted above, in this section I will focus more on the role of the community as validating knowledge claims that individuals may present to it. Expressing the social and communicable aspects of knowledge that I have gestured towards above, Welbourne (1986, p.5) advances the claim that "Our idea of knowledge, to put it roughly, is the idea of communicable information, information as to the facts, information which is objective in the sense that it is not dependent on any particular point of view, but is available to any one at all, with the capacity to understand the utterances in which it is embodied". Welbourne backs up this claim by arguing that one needs to contextualise the use of the concepts of belief and knowledge by acknowledging that they have evolved hand-in-hand with a certain model of communication of information (1986, p.4).

3.1: The root of the problem: knowledge as individual mental state

Traditional individualistic accounts of knowledge, springing from the Cartesian model of the lone thinker, tend to ignore Welbourne's point about the significance of communication in knowledge acquisition. This is further enhanced by the reductionist position as regards testimony as a source of knowledge. This position takes testimony not to be among the standard basic sources of knowledge, which are taken to be memory, perception, reason and introspection. Modern proponents of such a position are van Cleve (2006) and Audi (2006). Supporters of this position focus their criticism on arguing that the justification of testimonial claims has a non-testimonial basis (Goldman 1999, p.126).

Two further claims add to the downgrading of testimony and of communication in general as regards knowledge acquisition. The first is the idea that knowledge is somehow "denatured"in the process of communication (Welbourne 1986, p.49). Welbourne explains that the received view claims that there is some loss of understanding of some crucial elements required for a claim to count as knowledge during the transmission from speaker to hearer. Welbourne gives the example of John Locke (1975) as an early proponent of such a view[2].

2 Cf. (Locke 1975) "In the Sciences, every one has so much, as he really knows and comprehends: What he believes only, and takes upon trust, are but shreads; which however well in the whole piece, make no considerable addition to

The second is the insistence on knowledge being primarily resident inside the brain/mind, that is of knowledge being primarily a mental state. This assumption seems to go unchallenged and taken for granted by most epistemologists. A particularly pertinent example is social epistemologist Alvin Goldman, who, even though he sketches his project as seeking "social paths or routes to knowledge" (1999, p.4), he intends that to mean that "considering believers taken one at a time, it (his project) looks at the many routes to belief that feature interactions with other agents, as contrasted with private or asocial routes to belief acquisition"(Goldman 1999, p.4). He furthermore elucidates this by maintaining that "Even in this second perspective, (the social one), however, the knowing agents are still individuals" (Goldman 1999, p.4)

The commonly-held view is that possessing knowledge is a mental state, either in virtue of being a species of belief or in virtue of being a separate but nonetheless mental state[3], and that furthermore mental states are primarily (if not only) reducible to individual brain states. Such a thesis may be found to underlie the dialectic of epistemological writings of writers such as Dretske (1991) and is prevalent in Fodor's proposals and defence of the Language of Thought Hypothesis, in which he comes out in favour of a strong Cartesianism (2008, p.12).

Margaret Gilbert lays down two theses, which taken together explain the prevalining tendency to locate knowledge within individual's brains and preclude the possibility of group belief. These two theses she labels as psychologism about belief and anti-psychologism about social groups (M. Gilbert 1992, p.238). The first thesis is taken to reflect the idea that only a being with a mind can have an attitude towards a proposition, such as a belief. The second thesis is meant to block appeals to such entities as group minds[4], hence it simply conveys the idea that social groups do not have minds.

A general criticism for such individualistic accounts is that they overstate the role of the individual mind by considering a lone thinker in the universe, that is they assume that individual mental states are necessary and sufficient to explain individual action, where what at best can be established, at least as regards everyday tasks, is only that individual mental states are necessary in causal explanations of action.

My argument as regards this claim is that a great number of actions can be only be sufficiently explained by supplementing the analysis in terms of mental states with considerations of rule- and norm-following or other activities which have an ineliminable social element[5]. Hence I conclude

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his stock, who gathers them."

3 Williamson claims that knowing that p rather than believing that p is a basic mental state. (1995; 2000)

4 It should be noted, nonetheless, that functionalists about minds, such as Williamson, would not in principle preclude the possibility of group minds.

5 A strong thesis would ground rule-following on the community, see (Kusch 2002, chap.XIV), however a weaker thesis acknowledging that a lot of our behaviour consists of mimesis of past reactions of our peers in similar

that in such situations mental states alone[6] are not sufficient to explain behaviours observed.

From a purely epistemological point of view, the appeal of the idea that the possession of knowledge is a purely individual mental state may be derived from the widely-held belief that it is perception and inference that are the basic sources that generate knowledge, with memory and testimony regarded as merely transmitting knowledge (however see the discussion in (Lackey 2005) for a defence of the claim that memory is a generative source of knowledge). Regarding sense perception and inference as the only basic means of generating knowledge has as a result that knowledge acquisition is treated as more or less instantaneous, in the sense that an epistemic subject *a* comes to acquire knowledge that *p* as soon as *a* becomes aware of or grasps *p*, as described in figure 3.

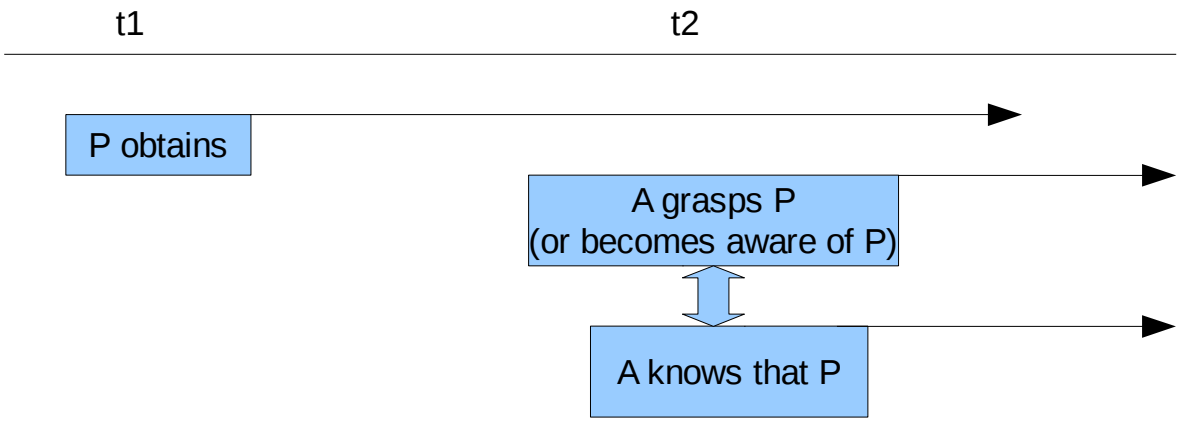

Figure 3: “Instantaneous” knowledge acquisition

My claim is that this schema is an oversimplification of knowledge acquisition and does not hold for most cases of knowledge acquisition. However, before proposing an alternative schema that regards knowledge acquisition as a process spread out in more time-slices than the above schema, I will briefly digress into an account of belief such that could allow for plural epistemic subjects but would also maintain the view of knowledge as justified true belief[7].

In the following section, I will give non-mental interpretation of belief that would allow one to keep the traditional formulation of knowledge as justified true belief but nonetheless cast it as inherently social. This attempt is based on Gilbert's (2000) definition of belief based on joint commitment. In the final section I will elaborate on Kusch's (2002) more detailed account of the

situations would be sufficient.

6 Even states that Williamson labels as 'factive', which are states (or attitudes) that one has “if and only if, necessarily, one has it (them) only to truths” (2000, p.34) would not be sufficient to explain action taking place within a social environment. Considerations of Robinson Crusoe-like examples would be unsuitable as counter-examples as it is an open question whether congenital social recluses would reason and act in the same way as social beings would.

7 See (Rolin 2008) for a defence of scientific knowledge as collective knowledge, with knowledge meaning justified true belief or acceptance held by groups as plural subjects (in Gilbert's sense)

mechanism of the generation of knowledge through testimony.

3.2: Belief as a property of plural subjects

Gilbert (1992), (2000) proposes an alternative account of belief that sidesteps the problems that acceptance of these two theses would cause, by proposing and defending an account of collective belief as jointly accepted view, whereby the term joint acceptance does not entail that any subset (even composed of one person) of the group consists solely by individuals individually holding the belief. In other words, Gilbert stresses that her account is non-summative, that is the joint acceptance of the view does not reduce to the sum of the beliefs of the group members (M. Gilbert 1992, p.306). Gilbert gives the following formulation of her account of collective belief:

"There is a *collective belief that p* if some persons are jointly committed to believe as a body that *p.* These people can then accurately say of themselves that "We (collectively) believe that p." (M. Gilbert 2000, p.38)

In explicating the notion of joint commitment Gilbert stresses the fact that it does not reduce to personal commitments, and that furthermore it cannot be rescinded by any of the parties to it acting unilaterally (M. Gilbert 2000, p.40).

There are two main advantages to be gained from adopting Gilbert's notion of collective belief and the derivative notion of collective knowledge, defined simply as justified true belief or acceptance held by groups of people considered as plural subjects (Rolin 2008). The first is that it provides a definition of knowledge in line with more standard definitions in terms of justified true belief, whilst at the same time taking into account the arguments of the above sections which purport to show that scientific knowledge is the property of plural subjects. The second advantage is that in using the language of joint commitments, Gilbert is in line with the communitarian account of knowledge to be defended below. According to this view, as elaborated in (Kusch 2002), knowledge is a social kind, like money, and the status of the possessor of knowledge as regards a certain claim p is simply a nexus of obligations and commitments held by a community of people towards the claim that p.

A disadvantage of Gilbert's account is that it does not explain the exact relationship between individual brain states and group states. The question of whether the group state of belief that p supervenes on individual states is unaddressed whilst the notion of belief is replaced with the notion of commitment, where the status of the latter as a behavioural trait or a mental state is left unexplained. This however need not affect the communitarian account to be presented below, since the purpose of the account is to shift the unit of epistemic analysis as regards scientific knowledge

to the community rather than the individual, rather than give a complete mechanism of how and if the group states reduce or relate to individual mental states. The latter endeavour is deemed irrelevant, since the stance followed is that focussing on group behaviour has more explanatory power in analysing the production of scientific knowledge than focussing on individuals' cognitive capacities and faculties.

I will close this section by elaborating on the communitarian schema of belief acquisition by contrasting it to figure 3.

3.3: Knowledge acquisition according to communitarian epistemology

As hinted in the conclusion of section 3.2, knowledge acquisition of ordinary claims is not described accurately by figure 3, which describes only special cases of acquisition of knowledge through perception or through solitary inference[8]. Given that most of our knowledge is acquired through testimony, and given the importance of communication stressed throughout the above sections, a more complete schema of knowledge acquisition would drive a wedge between the time of grasping that a state of affairs obtains and being justified in attributing knowledge to the subject, only attributing knowledge when a community of people is formed. Such an account is given in figure 4, which is copied from (Kusch 2002, p.72)

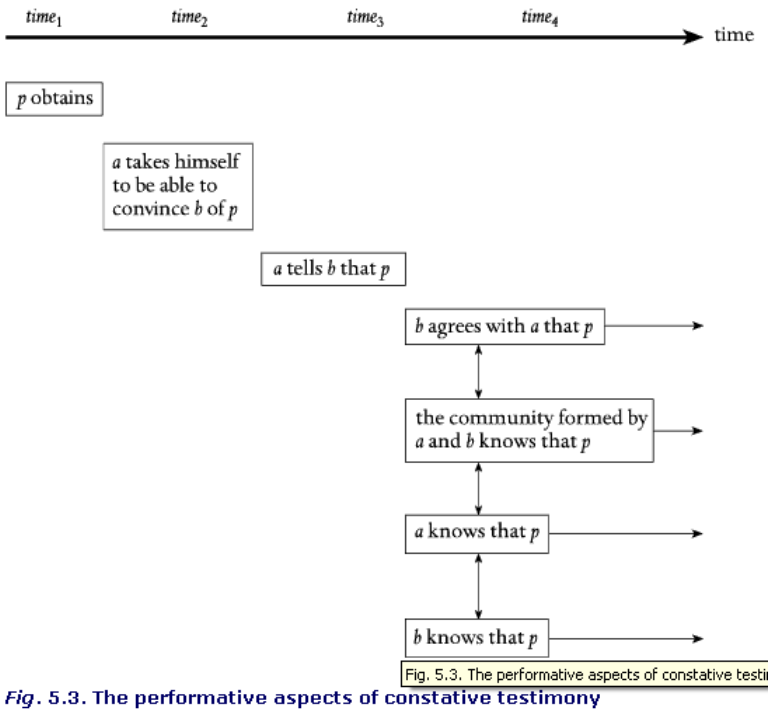

Figure 4: Knowledge production in case of constative testimony (copied from (Kusch 2002,

8 However there is a case to be made to the conclusion that inference is based on rule-following, and hence, if one accepts the communitarian interpretation of rule-following, then would not be available to the social isolate.

p.72))

My claim is that the above schema does justice to the arguments presented above, as to the constitutive role of community validation (sections 1 and 2) in the acquisition of knowledge. Furthermore, it does justice to the idea entertained more formally in section 3 that the epistemic subject is the group and the individual is a becomes an epistemic subject as regards a proposition p only in virtue of there existing a community which accepts p as knowledge. This is demonstrated in the schema by the events taking place at t4, where a community is formed and at the same time, and only then can the two people forming that community, a and b, be said to know that p.

Finally, it is worth remarking that knowing that p as described in figure 4 and as proposed by communitarian epistemology, consists, as noted above, of a nexus of commitments and entitlements, and hence the content of the concept of knowledge may vary from group to group, hence the definition of knowledge proposed is compatible with more formal, traditional definitions of knowledge, as long as the traditional definitions of knowledge do not wish to cast knowledge or belief as purely mental properties. In what follows, I will attempt to further illustrate the concept of knowledge in communitarian epistemology through scientific practice, by focussing on the phenomenon of "distance lending enchantment" as applied to the production of scientific knowledge.

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