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Abstract

Until recently, the social-technical process of invention has fallen between sociological investigation of the genesis of a new idea (an ideational phenomenon) and the production of a new technology (a material phenomenon). The advent of post-modernism and post-structuralism offered new avenues for theorising invention, accounting for, on the one hand, its material nature, and, on the other, its ideational nature, through the notion of socio-technical ensembles: phenomena constructed through the co-producing, mutually constitutive action of actants (both human and otherwise). This paper argues that despite its potential, theorising within the sociology of science and technology is hampered by insufficient attention to the role of the researcher and the concept and practice of reflexivity. Reflexive practices within this field of knowledge are explored, and drawing on an empirical case study of an antibiotic preparation, a case is made for the necessity of reflexivity in the production of knowledge about invention.

Keywords

Science, Technology, Reflexivity, and Sociology

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Until recently, the social-technical process of invention has fallen between sociological investigation of the genesis of a new idea (an ideational phenomenon) and the production of a new technology (a material phenomenon). The advent of post-modernism and post-structuralism offered new avenues for theorising invention, accounting for, on the one hand, its material nature, and, on the other, its ideational nature, through the notion of socio-technical ensembles: phenomena constructed through the co-producing, mutually constitutive action of actants (both human and otherwise). This paper argues that despite its potential, theorising within the sociology of science and technology is hampered by insufficient attention to the role of the researcher and the concept and practice of reflexivity. Reflexive practices within this field of knowledge are explored, and drawing on an empirical case study of an antibiotic preparation, a case is made for the necessity of reflexivity in the production of knowledge about invention.
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Studies of science, technology, and the innovation process have enabled significant developments to be made in our understanding of the process of invention. Various disciplines, including sociology and philosophy, have offered theoretical tools for the examination of the intellectual endeavours and technological accomplishments of humankind, and in the process, thrown into relief some of the many difficulties inherent in the historiography of science and technology. Notable here would be, for example, efforts to comprehend the possibility of producing valid knowledge despite the researcher's enmeshment within the social context of which they are themselves a creation (e.g., Mannheim, 1960, pp. 71, 264); or the extent to which it may be possible for conclusions to be drawn that might be other than a mere reflection of the researcher's position within the social structure (Bourdieu, 1994). Yet the compilation of detailed sociological knowledge about the processes of invention has been severely restricted over the past century by disciplinary boundaries and sub-field distinctions. These issues, which are discussed at length in the first section of the paper, have resulted in a fragmented field of knowledge with insufficient emphasis on the role of the sociological researcher. This individual is entrusted with the task of "discovering" past achievements and the "assemblage" of the historical case study - a tool widely used for organising and presenting information about the scientific past - so that they may add to disciplinary knowledge about invention: yet the researcher remains an *invisible* component in any conventional accounting of an invention. Despite the centrality of the sociological researcher to the development of sociological knowledge about the mechanisms of the invention process, critical questions about the researcher's role remain unasked: How are unknown aspects of the past "made known" by the researcher? What enables a researcher to locate "missing" voices, events or achievements? To what extent *can* a researcher "reveal" the historical junctures and pathways of a technology?

The work of the sociological researcher in producing knowledge about the science and technology process is yet to be fully studied or theorised. In order to begin this process, this paper examines the nature of *reflexivity* in the invention process. Reflexivity is essentially concerned with questions about how an individual might come to produce an account of an aspect of reality (Fuller, 1995b, p. 161). An argument is made that studies of invention have primarily conceived reflexivity in moral and philosophical, rather than methodological and epistemological terms, thus obscuring the much more critical role of reflexivity in the research process. Employing an empirical case study of the invention of "Eryc" - an antibiotic product - this paper demonstrates the critical impact of the sociologist, arguing that reflexivity is an essential component of the production of knowledge about the invention process. A new concept - primary analytical reflexivity - is proposed as a means to fully comprehend the missing element in the process through which a researcher can be acknowledged to have taken a legitimate place in the social (or socio-technical) construction of an invention, as well as in the production of historiographical accounts of the past.

Previous Developments and the State of the Field

"Invention" has for some time been a problematic concept, given that it may refer, in common discourse, to the creation of either ideational or material products. For several decades during the latter half of the 20th century, the study of invention fell, rather awkwardly, between two fields of research. One field, that of innovation, technological change, and the social impact of technology, focused on the "material" nature of invention; while the other, the investigation of the world of science, intellectual production, and the laboratory, examined the ideational aspect of invention. The first field of knowledge became popular during the 1970s, and, when applied to medicine, either focused on the *introduction* of technologies to the market and their *adoption* by doctors, hospitals and clinics (e.g., Greer, 1985; McKinlay, 1981); or the social *impact* of an innovation (e.g., Bates & Lapsley, 1985; Faden & Kass, 1993). These studies of technology generally placed little emphasis on the processes which *preceded* the "promising report" or media announcement of a "scientific breakthrough" (McKinlay), and gave more attention to the *diffusion* process, where doctors and hospitals "adopted" technologies which were presumably invented elsewhere (e.g., Evans, 1993; Martin, 1993; Pasveer, 1989). Despite the value of these approaches for revealing the social problems of new technologies, there was a growing recognition within sociology of the need to move beyond an essentialist conception of technology, and consider how technologies themselves might embody social values (e.g., Harding, 1991; Raymond, 1979; Woodward, 1970, p. 14).

The second body of knowledge encompasses a rather diverse field, and includes the Sociology of Scientific Knowledge, the Sociology of Science, Laboratory Studies, as well as sociological offerings from the History and Philosophy of Science. Increasingly prevalent from the mid 1960s, these studies systematically investigated science as a realm of social activity and a form of organisation (e.g., Barnes, 1977; Bloor, 1975; Mulkay, 1979; Traweek, 1988; Whitley, 1982). Eschewing the commercial and industrial orientation evident in the Innovation studies approach (e.g., Abernathy & Utterback, 1978), and the essentialism of the technology studies perspective, these dealt with

"invention" in terms of "conceptualisation", theorising the creation of "mental products" as a social process shaped by social relations and social context. Although no consensus was reached within these fields over theoretical frameworks, program objectives or identifiable boundaries for a sub-discipline; they stood as an alternative to biological or psychological theories of creativity, where inventors were believed to have an "inherent propensity" toward creative behaviour (cf. Bienaymé, 1986, p. 139; Collyer, 1997, p. 195-196). Often, but not consistently ethnographic in method, scholars produced insights into the day-to-day processes of scientific practice through a focus on "unfinished knowledge" or knowledge "that is yet in the process of being constituted" (Knorr-Cetina, 1995, p. 141). Here too was a growing recognition of the paucity of current theories; in this case, in dealing adequately with the material world (e.g., Barnes, p. 25-26; Bury, 1986; Woolgar, 1987). The persisting lacuna in this theoretical field constituted a particular problem for invention, for this phenomena has the capacity to take the form of an idea, concept, theory, technique, "tacit" knowledge, formula, device or machine, and so traverses the many possibilities between the ideational and material worlds.

Theoretical developments in both fields came with the diffusion of post-modernism and post-structuralism. Mirroring developments in other fields of sociology such as gender, sexuality and health, where the perishable, suffering, corporeal, *embodied* individual began to be taken into account (e.g., Harraway, 1991; Turner, 1984); the sociological study of science and technology answered with an array of conceptual frameworks and programs. Though there have been many attempts to delineate the boundaries between their fields, formulate unique identities, and offer coherent principles for future research; there has been a blurring between philosophical, historical and sociological approaches (cf. Shapin, 1995, p. 289), and between theoretical and epistemological frameworks. Thus many of the same authors (such as Callon, Collins, Fox Keller, Knorr-Cetina, Latour, Law, Mulkay, Pinch, and Woolgar), their works, and conceptual frameworks, are claimed equally by several sub-fields or program areas (as found in, for example, Fuller, 1995a; Jasanoff, 2000; Jasanoff, Markle, Petersen, & Pinch, 1995; Pinch, 1993). While no consensus has been, or is likely to be reached on how this diverse field might be identified, and though it may be useful to distinguish between them for other purposes, there is no necessity in this paper to do so. Instead the term "the sociology of science/technology" will suffice to refer to this rather heterogeneous body of literature, which shares little more than a commitment to sociologically explaining either the social or the socio-technical basis of science and/or technology.

One recent approach within this literature is the actor-network theory of Callon, Law and Latour, where the "technical" and "the social" are defined only in relation to one another, as a "seamless web", and not as an interaction between two distinct ontological phenomena (cf. Callon, 1986; Hughes, 1986; Latour, 1987, 1991, p. 129). For the first time since the birth of the modern sociological project with Parsons' 1937 *Structure of Social Action*, and the subsequent exclusion of biology and the physical environment from legitimate sociological inquiry; "the social" lost its favoured status (cf. Collyer, 2010). In this new framework, material elements were conceived not as passive aspects of reality, but "participants" in "social ordering" (Callon & Law, 1997, p. 167). This opened a theoretical "space" for invention, for it was argued that technologies and humans interact upon one another, and in the process, elements are created, modified and

transformed. Thus the inventor and the invented are *co-produced* in a translation process (Knorr-Cetina, 1995, pp. 145-146; also Latour, 1991, p. 116).

The emergence of actor-network theory has also re-invigorated theoretical development regarding social constructionism. The origins of the constructionist perspective are integral to the origins of sociology itself, for its principles were formed in historic debates over the identity and nature of the emerging discipline. Though Marx, Weber, and Durkheim are not constructionists according to modern usage of the term, their work is suggestive of its later principles. For example, Weber proposed knowledge to be a body of value-ideas constructed "in the mind" through a subjective process directed by the value-orientation of the researcher (1949, pp. 80, 94), but given objectivity (i.e., shared meaning) by its production within social interaction (1968, p. 519). Similarly, both Marx and Durkheim theorised ideas and knowledge as reflecting, respectively, the social structure and social organisation of society. These, along with more recent contributions from sociologists such as Alfred Schutz and Pierre Bourdieu, offered a perspective in which knowledge and reality are products of society, and "facts" are historically located and collectively produced.

The constructivist perspective is often targeted for its relativistic stance, for it has traditionally proposed "reality" as not external to social life, but embedded within and (largely or wholly) determined by it. As a consequence, knowledge of the world is "constructed" rather than directly "captured" or "discovered" in the process of "knowing"; and a product of subjective cognition. The development of actor-network theory, in repositioning the concept of "the social", has re-problematised the notion of "the real", spawning debate over realism *versus* idealism, and objectivity *versus* subjectivity, and, perhaps coincidentally, a range of new philosophical and sociological positions, including "subtle realism" (Hammersley, 1992) and "critical" or "reflexive" realism (Layder, 1998; Porter, 1993). There have been many critiques of actor-network theory, despite its popularity and wide application in fields as diverse as computers and information systems, education, management and development studies. This theoretical framework has also been vigorously defended (e.g., Callon, 1998; Callon & Law, 1997; Latour, 1994; Law, 1992). Critiques have focused on its masculinist, Machiavellian and Hobbesian interpretations of human nature; its characterisation of the actor as a self-seeking entity intent on building networks, prestige and power (e.g., Martin, 1998; Shapin, 1995); its inadequate theorisation of power and social structure (e.g., Collyer, 1997; Kleinman, 1998); and the propensity toward colonisation hidden with its liberal-democratic plea for the liberation of the machine (Lee & Brown 1994). However few concerns have been voiced over the epistemological and methodological implications of actor-network theory.

Actor-network theory does not merely replace *social* constructionism with *socio-technical* constructionism, but re-configures the relationships between the "knower", the production of knowledge, and the "known". Within the sociology of knowledge, these relationships are captured with the methodological principle of *reflexivity,* which enables the researcher to produce authentic knowledge even though they are enmeshed within the world they study. Reflexive practices include self-reflection on the part of the researcher upon their presence, and provide the means to assess the extent and nature of social influences on the research process. The notion of "reflexivity" can be traced back to Weber and Schutz with their ideas of philosophical self-reflection and the search for

hidden influences on one's work. Though the concept has a variety of meanings in the social sciences (cf. Lynch, 2000, pp. 27-34), and been challenged by the post-modernist and post-structuralist notion of a plurality of meanings and forms of knowing; scholars generally retain the core idea of the researcher as required to "provide an account of social reality that can explain how the theorist could come to have such an account" (Fuller, 1995b, p. 161).

Given the considerable overlap between the sociology of knowledge and the sociology of science/technology (for both are concerned with the analysis of the nature of knowledge itself), and the recent status of reflexivity as a moral imperative and means for social scientists to access both resources and status (cf. Lynch, 2000; Maton, 2003, p. 54); it is somewhat surprising to find the latter body of literature lacking in reflexive practices. Yet this has been a sustained criticism within the sociology of science/technology (e.g., Fuller, 1995b; Woolgar, 1991; Woolgar & Ashmore, 1988, p. 2). The next section demonstrates how reflexivity has, and has not, been addressed in a sample of studies from the sociology of science/technology. It indicates that these criticisms are at least partially true, for there are some aspects of reflexivity, particularly the "core" principle outlined above, which receive insufficient attention in practice.

Reflexivity in the Sociology of Science/Technology

One of the more significant features of post-modernity for theorists such as Foucault, Derrida, Bauman, Beck, Giddens and Lash, is its reflexivity. As Bauman (1991, p. 272) argues, "Postmodernity is modernity coming to terms with its own impossibility; a self-monitoring modernity, one that consciously discards what it was once unconsciously doing". For scholars working within the field of the sociology of science/technology, reflexivity has not been a significant issue of debate, but nevertheless appears on the agenda (e.g., Bloor, 1976, Collins, 1990; Fuller, 1995b; Woolgar, 1991). This has arisen in part, because of a broader epistemological shift since the 1960s across the social sciences and the arts, with a rejection of positivist and idealised images of the natural sciences, redefined notions of objectivity and value-neutrality, and challenges to expertise and authority. Yet it appears there is a measure of truth in complaints about the lack of reflexivity in the sociology of science/technology, for closer analysis indicates only a limited form of reflexivity is practiced in this field.

In her analysis of sociological approaches to the research process, Stanley (1996) discerns two forms of reflexivity, *descriptive* and *analytical*. The first, arising as a term in the 1960s in feminist critiques of research methodology and the Sociology of Scientific Knowledge, refers to the influences on the researcher from the social context (including the relations of power), and of the social interaction between researcher and the subjects of the research. This form of reflexivity has been taken up with some vigour by sociologists generally, and, despite complaints to the contrary, can also be found within the sociology of science/technology. For example, the studies by Måhlck (2001), Cohen, McAuley, and Duberley (2001), and Roth and Bowen (2001), demonstrate how knowledge, or the processes of knowledge production, are shaped by social context or social structures of power. Similarly, Blume's (2000) study of the cochlear implant reflects at length on how the author's values and interests (as a sociologist *and* father of

two deaf children), influence the research process, his interaction with participants, and his views and knowledge of the technology.

The second form of reflexivity, *analytical reflexivity* (Stanley, 1996), is apparently more difficult to accomplish and rarely found. Analytical reflexivity involves an explanation of the processes through which the researcher reaches an understanding of the phenomena; that is, how they "construct" their knowledge claims. This second form of reflexivity appears to be the concern of Fuller (1995b, p. 161), for in making a case for a widespread lack of reflexivity, points to the works of Lash, Beck and Giddens, arguing that none of the theorists discuss how they came to understand reflexive modernity itself: a crucial issue given they are themselves products of the uncertainties of which they speak. This raises the question of how a researcher might be uniquely capable of obtaining knowledge from which others are barred. This is the conundrum which some other theorists, such as Bourdieu, seek to resolve.

Bourdieu (1994) theorises how a form of knowledge can be produced which is more than the partial, positioned view of the individual scholar, and equally more than the sum of multiple, individual viewpoints. In other words, how the structured, collective nature of science might enable the production of a form of knowledge which escapes the confines of the apparently inevitable positionality of the individual producers of knowledge. For Bourdieu, the escape from the "gravitational effects" of a researcher's intellectual field can be transcended through a process of *epistemic reflexivity* (Bourdieu, 1994; Maton, 2003, p. 57).

According to Bourdieu (1994), the researcher or "knower" enters into an objectifying relationship with the "object" of knowledge in the production of knowledge, and engagement with epistemic reflexivity means "making the objectifying relation itself the object of analysis" (Maton, 2003, p. 57). This procedure is not individualised but a collective practice, for it comprises not just the self-fascinated, individual's values, interests and commitments, but a structured enterprise of the scientific field as a whole (cf. Bourdieu & Wacquant, 1992, p. 72). Epistemic reflexivity thus differs from descriptive reflexivity (that is, the form of reflexivity generally carried out by sociologists), because rather than a focus on the relationship between the researcher (or knower) and knowledge, it focuses on the relations between the researcher and the object of knowledge (cf. Maton). However Bourdieu's theoretical framework, though insightful, does not offer a means to operationalise epistemic reflexivity as a collective, rather than individual research strategy (Maton, p. 58), and moreover, tends to create a boundary between the knower and the object of knowledge which verges on positivism: a boundary which other sociologists are inclined to render more permeable or indeed even abandon.

An alternative to Bourdieu's framework is provided by Weber. Though far less recent, Weber's books, including *The Methodology of the Social Sciences*, remain incomparable for their attention to the form of reflexivity under discussion here. In Weber's theory of knowledge, the process of knowledge production involves the scholar as a historically located actor whose actions are social and thus oriented toward meaning. This means knowledge is never created in the abstract, but an outcome of social processes (Weber, 1968). During knowledge production, the scholar comes to "know" about reality through a subjective process of evaluating the "infinite empirical context" and constructing a view, theory or perspective according to their value-orientation

(Weber, 1949, p. 78). The value-orientation of the scholar directs them toward what is culturally significant, narrowing the field and enabling the actor to "make sense" of the empirical context (Zaret, 1980, p. 1183). This social process has both subjective and objective dimensions. It is subjective because it is always an evaluation. "Every meaningful *value-judgement* about someone else's *aspirations* must be a criticism from the standpoint of one's own *Weltanschauung*; it must be a struggle against *another's* ideals from the standpoint of one's *own*" (Weber, 1949, p. 60). On the other hand, historical analysis is *not subjective*, meanings are shared, and not "valid" only for each individual (Weber, 1949, pp. 83-84). "Objectivity", according to Weber (1949, p. 60), is not where an analysis "captures reality" through value-neutrality, for an "*attitude of moral indifference* has no connection *with scientific* 'objectivity'". Instead, the process of knowledge production can be objective because the scholar obtains a sense of cultural significance from prevailing evaluative ideas (Weber, 1949, pp. 83-84), disciplinary knowledges, and their own experience and analytical training (1949, pp. 79-80).

In this subjective/objective process, knowledge is not "discovered" but a product of human reason (Månson, 2000, p. 79; Weber, 1949, p. 106). The empirical "objects of analysis", that is, "facts" or "the known", are not given, but "constructed" by the social actor (Zaret, 1980, p. 1183), for "we comprehend reality only through a chain of intellectual modifications" (Weber, 1949, pp. 80, 94). In other words, we do not merely "select" which aspect of reality to study, or how to understand a given phenomenon, but "construct" that phenomenon, for "there is nothing in the things themselves to set some of them apart as alone meriting attention" (Weber, 1949, p. 78).

Reflexivity occurs for Weber when the scholar reflects upon what Bourdieu later described as the *objectifying relations* between the researcher and the object of knowledge (Bourdieu, 1994; Maton, 2003). Most scholars, Weber claims, conflate the abstract categories and concepts they use to produce knowledge with reality itself, and forget these are merely tools to understand reality (1949, p. 85; also 1964, pp. 140-145). This principle differentiates Weber's analyses from those of many other scholars, because he remains cognisant of the distinction between an "ideal type" (a "mental construction", see 1949, pp. 101-103), and the "intrinsic" nature of reality; for he argues the former is always partial, distorted, and logically extreme (1949, pp. 72-73, 170-172; also Kalberg, 1997, p. 222). Weber's distinction between mental constructions and empirical reality is the basis of analytical reflexivity, for in practice this requires reflection upon the process through which the scholar produces the object of knowledge. It is this form of reflexivity which enables the collective, structured practice of scholars to escape the "gravitational" effects of positionality.

In the sociology of science/technology, two examples of analytical reflexivity can be provided. Both represent case studies of controversies in science/technology which examine the processes through which researchers construct their arguments. Turner (2001, pp. 477, 499) argues for the need to apply critical reflexivity to ensure an awareness of how an author's own narrative choices "dictate the political and moral arguments implicit in each account ... [and] ... determine the way in which particular accounts of technoscientific controversies work, and what they ultimately mean". For Rees (2001), the main topic of discussion is the methodological practice of observation in a case study of infanticide among primates. Nevertheless Rees includes an insightful, if brief, discussion about how the field workers presented their arguments to form

coherence, what evidence they focused on (and importantly, what they ignored), and how they offered different interpretations, even with access to the same observational reports. For Rees (2001, p. 523), differences emerged because researchers "filtered" "facts" according to "relevance". Although these two examples display analytical reflexivity, their focus is the practice of *other* researchers: not of the author. Thus they represent examples of what we might term *secondary analytical reflexivity.*

Examples of *primary analytical reflexivity* are more difficult to locate, for this requires the researcher to apply the principles of analytical reflexivity to their own work. There is little within the field of sociology of science/technology to compare with the efforts of Weber's (1930) *The Protestant Ethic and the Spirit of Capitalism.* In the book's introduction, and also elsewhere (e.g., Weber, 1949, p. 71), Weber insists that despite its value as an exploration of the ideational foundation of capitalism, it offers an incomplete analysis of historical change. In other words, it disregards the *multiplicity* of causal forces in order to offer an "antidote" to prevailing materialist analyses. With the exception of Weber's book, there is a paucity of primary analytical reflexivity in many literatures. In the sociology of science/technology, this is particularly problematic when investigating the nature of invention. The next two sections will outline the method of the case study research, and then go on to demonstrate how Weber's notion of reflexivity can be applied in a case study of an invention. The case for reflexivity will be made not just on moral grounds, but as a means to better comprehend the phenomena which is the object of study: the invention of technology.

A Case Study of an Invention

Personal statement. This paper draws on a piece of empirical research conducted while a PhD candidate in the Department of Sociology at Flinders University of South Australia in the early 1990s. My interest in the subject of invention had been stimulated in the years prior to my candidature, when working as a researcher in a consultancy firm attached to Wollongong University, the centre for Technology And Social Change (TASC). Clients for the centre came from both government and industry. Much of our research focussed on the impact of new technologies on social relationships and institutions (e.g., predicting the need for a re-skilling of the workforce if new banking or engineering technologies were to be introduced). Although working at the centre gave me the opportunity to utilise, and hone, my qualitative and quantitative skills, and certainly enhanced my project management skills, the clients of the centre were predominantly "Right Wing", "Dry" or "Neo-liberal" in orientation; and the provision of research and consultancy services in this atmosphere became increasingly unsatisfactory for an idealistic and essentially "Left-Wing" sociologist. Having been granted a scholarship for further study, I "escaped" happily to academia where I was less constrained by politics and the iron bands of bureaucracy. I spent the next three years as a post-graduate student exploring the social construction of technologies, free to critically theorise and question *all aspects* of the invention and innovation process, including the client's motives, the policy context, and the influence of the researcher's values and political beliefs. After completing the PhD I spent some years employed on short-term academic contracts, and in 1999 was appointed to the University of Sydney. Although the research from which this paper has been drawn was completed some years ago, these

reflections are very recently composed. In one sense, this paper reflects what I was *attempting* to write in my thesis, but at that time I had insufficient command of the theoretical and conceptual literature to express such complex thoughts. It was only after re-reading Weber, and coming to a more mature appreciation of his reflections on the nature of history and historiography (cf. Collyer, 2008), that the central ideas of this paper could be articulated. In essence, these ideas have been in a "development phase" for the past two decades.

The study. The subject of the case study is the invention of "Eryc", an antibiotic product on the international market. The case study results from data gathered from 20 in-depth, semi-structured interviews with key-informants. Participants cover a broad range of interests, occupations and perspectives; including government officials, industry representatives (marketing, sales, scientists, management), university scientists, researchers, medical practitioners, and members of medical organisations such as the *Pharmacy Guild* and the *Australian Society of Hospital Pharmacists*. Potential interviewees were identified through a "snow-balling" method, and the interviews, which were conducted in Adelaide, Melbourne, Sydney and Canberra, were between one and four hours duration. All were tape-recorded and transcribed with permission of the participants. At the time of the interviews, the university did not require sociologists to seek permission from an institutional ethics committee for non-invasive, non-clinical, social research. Nevertheless, the researcher adhered to the ethical guidelines of *The Australian Sociological Association,* which can be found at www.tasa.org.au/ethical-guidelines/

"Constructing" or "Discovering" an Invention

In the Innovation literature, the process through which a researcher finds a subject for study is rarely problematised, as technologies are perceived as ontological entities defined by market "success". While there are "failed" inventions, these are merely "scientific breakthroughs" which did not fully develop into technological products, or products which did not survive the market process: either as a consequence of inadequacies within the product itself, the structure of the market (cf. Abernathy & Utterback, 1978), or a lack of powerful sponsorship (cf. McKinlay, 1981). The sociology of science/technology literature similarly offers little in the way of reflection on the role of the researcher and the methods used to produce an account of the birth of an idea or technology (cf. Shapin, 1995). Generally speaking, authors do not reflect on how they "selected" a subject for the case study, but begin with their "object of analysis", and proceed to examine its "history", and the cultural, political, economic and/or technical factors which shaped the technology's trajectory over time (e.g., Drake & Purvis, 2001). Studies focusing on "originating ideas" and the context of science, rather than technologies *per se,* are equally culpable in their lack of primary analytical reflexivity. These locate the focus of their research through a process similar to that used in the study of a technology, for scientific activity is considered in many ways to parallel the processes of the broader market. Within scientific networks, "unfinished knowledge" (Knorr-Cetina, 1995, p. 141), and "mere opinion", become new knowledge through formal and informal competitive structures. These structures force scientists to "say

something new", connect people and organisations, and actively "push" discoveries through, "selling" new ideas and techniques to others (Fuchs, 1993). Compared with the Innovation literature, a more critical approach is taken toward the subject matter, for "discoveries" are not ontologically given entities which remain unchanged as they become innovations, but modified by the various agendas and interests of the scientists. Hence their final form may be quite different from the original (Brannigan, 1981). Nevertheless, the researcher is able to unproblematically obtain a focus for their study by examining, for example, the contents of newspapers for evidence of a "scientific controversy", or the scientific journals to locate "breakthroughs" which have excited the interest of scientists and attracted resources. As such, this process is little different from the innovation researcher who might "select" a successful technology from an examination of stock market reports. In both cases, a "history" of a product or idea appears to be based on an abstract, rational process of "history writing"; and working backward from the present "success", "controversy" or "breakthrough", to a possible originating context.

The actor-network approach differs somewhat from this format. Here the focus of a case study is not the "invention" or "breakthrough" itself, but the network of which it is part. Thus the invention is not theorised as an "entity", that is, a distinct object with well-established boundaries, but a "compound reality" and set of relations (Callon & Law, 1997). Nevertheless, the process through which these "breakthroughs" and innovations become the subject of the research (and thus come to constitute "knowledge" of the invention), is not problematised. Researchers produce all forms of knowledge through calibration, measurement and theorising (cf. Latour, 1987, p. 256), but the "critical judgement" of the researcher, though necessary to the research process (cf. Bijker, 1993), is not given specific attention. Instead, the notion of a privileged "outside observer" is rejected as an "epistemological myth" (cf. Latour, 1998). There is therefore, little acknowledgement that the "selection" of a focus for examination is itself a process of knowledge building: of *producing* knowledge *about* knowledge. Amidst the amorphous relations of the actor-network, the researcher, seeking to describe and explain its formation, is not accorded particular significance.

However it can be suggested that the historical and social location of the researcher is of central importance, not just to the relationship between the "knower" and the "knowledge" they construct, but also to the relations between the "knower" and the object of knowledge. Over recent decades, studies within the sociology of knowledge have drawn attention to the inherently social basis of knowledge production, revealing an unequal and hierarchical structure within which core, Western countries dominate (Connell & Wood, 2002). Within this system, case studies of "major breakthroughs", "scientific controversies", and "new technological trajectories", focus on events considered significant to members of the core nations, while other developments are less likely to be documented. A researcher located in either the periphery or semi-periphery (to borrow Wallerstein's terms), is constantly made aware of this facet of knowledge production, and hence ideally positioned to question what may be standard practice elsewhere (though not ideally positioned to successfully challenge the status quo). Moreover, in a country where its own developments are usually over-looked by the core research community, a researcher is rarely in a position to "select" an object for analysis from an existing reservoir of ideas already documented by other social actors. In other

words, while researchers in core countries are busy revising their histories, in the periphery these "histories" are relatively less complete, and a more complex process of construction is required. Despite its disadvantages, the general lack of scholarly knowledge about the past in peripheral, or semi-peripheral countries, represents an ideal context in which to explore *how* an object of analysis is constructed for historical analysis, and can be used to demonstrate reflexivity as a pre-requisite for the production of knowledge.

Self-reflection on the researcher's motivation and interests might be regarded merely as a form of narcissism, where the researcher usurps the object of study in a form of confessional autobiography (cf. Maton, 2003, pp. 55-56). However such self-reflection gives way to primary analytical reflexivity where an opportunity appears to escape the "gravitational effects" of one's intellectual field. The case of "Eryc" offers an example of this, for the semi-peripheral location produced a determination in the researcher to "find" an Australian pharmaceutical invention to demonstrate the relevance of Australian science and contribute to the literature. Important also was discipline-based knowledge about the state of the Australian pharmaceutical industry, which, despite the support of governments during the 1970s and 80s through the construction of state-owned companies (such as CSL and Fawnmac), and research and development tax assistance programs (such as the Factor F scheme), did not survive subsequent privatisations and budget cuts. By the 1990s the industry had returned to its dependence on foreign imports and was once again dominated by the subsidiaries of transnational or international firms (Kot & Petit-Young, 1990, p. 407; PMSC, 1991, pp. 3-7).

Such knowledge of the industry sector provided not an impediment, but an impetus to the search for an "invention" which might "lie in waiting" for a curious researcher. While Australia's position on the semi-periphery of the global economy and its internal political, cultural and economic structure means that it has only 0.7% of the share in patent families and is a net importer of technologies such as licences, patents and technical assistance (OECD, 2000, pp. 52, 54); the output of Australian scientists (measured by the publication of papers) is on a par with countries such as the United States (measured relative to population) (OECD, p. 51). In other words, although Australian's rarely maintain ownership of their "inventions", they are relatively successful in creating them. Such knowledge only encouraged the researcher's determination to "find" an *Australian* pharmaceutical invention and demonstrate the relevance of Australian scientists to the history of science and technology.

Documentary searches and preliminary discussions with members of the medical and scientific communities unearthed little of interest. Driven by a sense of nationalism, and perhaps reformist zeal in the face of perceived global injustice, the researcher persisted with the belief that Australian scientists *must* have been successful at least once, and was spurred into further action (as scientists are when they scent the possibility of "making a discovery"), when it became increasingly obvious that if there had been any inventions, these had not yet been included in the historical record. Eventually a retired scientist suggested an approach to F.H. Faulding, one of the few Australian-based companies (and the only one focusing on pharmaceuticals rather than over-the-counter products). With little conviction in his voice, the scientist stated: "if anyone *has* come up with something, it would be within Faulding".

Initial inquiries at Faulding failed to produce a candidate for research. At first this appeared to be a problem of language, for the words "invention", "breakthrough", "new technology", and "innovation" suggest events of "world changing" magnitude. Given this connotation, participants could not envisage a connection between historic change and the mundane tasks of their daily work environment. Though the terms do not preclude the idea of an invention as merely a modification of an existing idea or process, or new ways of drawing on existing ideas or techniques; they are more often assumed to refer to a radical departure from established knowledge and practice. As one participant suggested, the word technology does not invoke the idea of something that *could* have been achieved 25 years previously with the same instruments or materials:

> When I think of ... [new technologies in medicine] I think of banks of instruments, computers, catheters, wonderful wiring and information, transplants, endoscopic surgery, micro-surgery perhaps ... that sort of thing. That is what I call technology. (Doctor, Interview 7)

The researcher, well-versed in theories of science and technology, encouraged participants to consider technology in terms of modifications and the borrowing of ideas and established practices from outside the firm itself. Participants began to discuss technology in these terms and consider whether there may have been some past events within the firm which influenced medical practice or enabled Faulding to offer a new product or process for the market. Discussion focused on the history of the firm, which had been started by Francis Hardy Faulding in May 1845, and had, over the intervening decades, operated essentially as a warehouse for the importation of both medicinal and non-medicinal products, limiting its manufacturing activities to products which were either generic, or based on licences from overseas companies. Several participants suggested that at some stage, Faulding had ended its "warehousing" role and become active in research and the development of its own pharmaceutical products. This produced some consideration of which product may have been the first of the new era.

Eventually one of Faulding's staff brought up the case of erythromycin, an antibiotic which had been, rather fondly, given the name of "Eryc". Discussion amongst participants focused on whether it was accurate to call this an "invention", and what might have been *new* about Eryc. It was quickly agreed that the active antibiotic substance from which Eryc is made is not the "new" component. Erythromycin products had been available on the market in a variety of forms since the 1950s, and indeed Faulding was, in the 1960s, producing erythromycin stearate under licence to Abbott (meaning that Abbott owned the patent), and selling this product as "Ethryn" on the Australian market. Dismissed also was the idea of Eryc as "new" and "unique" because of the enteric coating process used to protect the drug's active ingredient, or even the substance applied in the coating process. The coating substance had previously been approved for such use by government authorities, and the practice of coating active ingredients was becoming common as a drug delivery system at the time of the Eryc development (cf. Illum & Davis, 1987). Further, the company had previously used the coating method for an earlier product - Prodopa - which was released for hospital use in 1972. Staff also dismissed the idea that Eryc was "new" because the active ingredient had been formed into tiny round pellets and evenly coated with a polymer, weighed and

placed inside a gelatine capsule. This process too had been used in the past. It was eventually agreed that the "new" element of Eryc was the idea of *combining* the three processes with the antibiotic erythromycin.

A focus on the value-orientation of the socially-located actors provides additional insights into how the construction of Eryc as an "invention", was shaped by interaction with the staff of Faulding. Although initial communication faltered over the definition of such terms as "invention", "technology", "new", and "breakthrough", it eventually became apparent there was more to the problem than language, for these were not universal nor value-neutral concepts but context-specific and value-laden. How an actor understands an event or phenomenon from the past will depend on the actor's social and historical location. In medicine for example, clinical practitioners are particularly disinclined to label their work as "new", for this may give the impression techniques are experimental, untested or unsafe. This pragmatic conservatism among practitioners was in evidence in this study, when one participant stated:

> As far as a GP is concerned, even with the new drugs that are released by the government or even those subsidised by the government, there is an old saying, "Don't be the first doctor to try a new drug nor the last." (Interview 2)

Researchers in a *research environment*, on the contrary, have a stronger incentive to label their work "new", and may even exaggerate differences between existing and previous practices or products (Collyer, 1994a); while researchers in a *commercial environment* are inclined to consider technologies to be "inventions" only where they offer direct commercial potential and can attract copyright or patent protection. Moreover, in one social context, minor changes may be considered innovatory, whereas in another, constant change may be the "norm". For example, surgeons working within the speciality of plastic surgery often find themselves facing unique and unusual problems. Plastic surgeons contend they must constantly improvise and "make up operations to suit circumstances" (Collyer, 1994b). Surgeons are thus likely to consider their work *an extension of medical practice* rather than an invention. In some other areas of medicine even minor modifications from standard practice are unusual (and likely to bring censure or litigation), and hence the construction of "new technologies" is a less frequent event. Given this context-specific and value-laden nature of invention, a very different focus for the case study would have emerged if instead of seeking the assistance of research and marketing staff from Faulding, it had begun with participants from the medical community.

The case of Eryc indicates that an object of analysis (such as an invention) is not "discovered" but "constructed", and demonstrates how this might occur in an interactive process between social actors, where at least one of the actors is the researcher. For the research to proceed, Eryc had to be "constructed" as a "social fact", as it had not previously been granted the status of an "invention" in prevailing discourse (either informally or by an historian), and so had to be given salience through social action. In the case of Eryc, "social action" included the interests and value-orientations of the researcher and the participants. Analytical reflexivity indicates these were integral to the research process, for Eryc would not have been "discovered" without the introduction of

prevailing theories of science and technology to prompt actors to reflect on past experience in new terms. This indicates the researcher was not a passive agent in the production of historical knowledge. Analytical reflexivity also reveals Eryc was not an unproblematic, ontological, technical entity "waiting to be discovered", but a socio-technical construction birthed, in large part, during the research process itself.

"Constructing" or "Discovering" the Inventor

Progress with the building of the case study of Eryc met with further difficulties when it became time to identify the source of the invention. It is not uncommon for the identity of the inventor to be disputed when the history of a technology or scientific breakthrough is being recounted. This is no less true in the case of Eryc. Company literature and many participants in this study identified the "inventors" of Eryc as Brian Davies (the Marketing Manager) and Michael Story (a development engineer). However a small number pointed to Bernard Boggiano, an employee of the company, two pointed to individuals outside Faulding, such as Lloyd Sansom, Sidney Bell, or Peter McDonald, and one participant volunteered the name of the then Managing Director of Faulding, Bill Scammell. The identity of many individuals who might also have taken some part in events during the 1960s and 70s, including Richard Handcock and Bill O'Reilly, appear to have largely disappeared from the historical record.

In part, these diverse perspectives on the originator of Eryc, and the disappearance of some individuals from the historical record, can be understood as effects of the differing socio-historical locations of the participants in the study. Engineers or marketing staff for example, were more interested in, and knowledgeable about past events and individuals with a bearing on their own profession. As a result, these participants pointed to the importance of Davies and Story, who handled the process of production of the bulk product and the public announcement of Eryc as a "medical breakthrough" during the mid 1970s. In contrast, members of the scientific and medical community remarked upon the roles of actors within their own fields: such as Bernard Boggiano (a research chemist), Don Sorrell (a microbiologist), Tom Hemmings (a research pharmacist), Michael Gleeson (a chemist), or Peter McDonald (a microbiologist who organised the clinical trial and steered the product through government registration procedures).

However, the identification of a given individual as "the inventor" is also dependent on the intellectual tradition within which the *researcher* operates. From an Innovation Studies perspective, an inventor is one who can be most closely associated with a marketable or patentable product. Scientific theories or clinical problems connected with the product are of less importance, and merely provide context or "background". In the case of Eryc, the focus within this tradition would be on the events of the 1970s (when the product entered the market) and the roles of Story and Davies. In contrast, this researcher took a sociology of science/technology approach, and therefore focused on historic events prior to the development of a market product. There are, as Mulkay (1979, p. 74) suggests, advantages in concentrating on the stages before an intellectual consensus is reached, when knowledge of the empirical world becomes solidified and difficult to conceive in any other form. From inside this tradition,

individuals such as Boggiano and Gleeson, and events of the 1960s become critical elements of the historical narrative.

Once the data has been gathered, the next phase for the researcher is to assemble a case study or "history". From a sociology of science/technology perspective, the conventional approach is to identify a group of individuals who can be closely associated with a "scientific breakthrough", new theory, or innovation; and evaluate their relative contributions. The temporal ordering of events, ideas and actions become an important tool for the researcher in this process. An understanding of the competitive context of science is also of assistance, as it is generally agreed that recognition and reward are not based on merit alone (cf. Clarke & Montini, 1993; Goldberg, 1988). As a result, the researcher generally seeks to critically assess the various contributions, aware of the possibility that not all contributers will have been publicly acknowledged. A case study may then be produced, placing the central figures, ideas and events within a social context in order to explain the social forces determining or shaping the scientific or technological trajectory. The first draft of a case study of Eryc proceeded according to this standard formula, and is briefly narrated below.

The Invention of Eryc

During the 1960s, a number of medical and scientific workers became interested in the problems with the clinical use of erythromycin: a common and readily available antibiotic. Articles in clinical and pharmaceutical journals at the time suggested the absorption of erythromycin into the blood of the patient was extremely low and erratic (e.g., Goodwin, 1967). Even after being given high doses of the drug it was sometimes up to 12 hours before patients could benefit from the medication. Given that the drug was administered for the control of infection, the lack of predicability of the dosage was considered a serious problem, as patients might be left without medication for extended periods during which no supplementary medication could be given. The popular explanation for low and erratic absorption was that erythromycin was destroyed by the body's own protective mechanisms in the blood.

By the mid 1960s, reports were appearing in *The Lancet* and other medical/scientific journals about another product, Levodopa, suggesting its active ingredient was being destroyed by the acidic environment of the stomach. Boggiano, as Product Manager at Faulding, considered whether the reports about Levodopa might be applicable to erythromycin:

> I began to get ideas of what was wrong with erythromycin stearate ... little dawnings of ideas began to come out between Don Sorrell, myself, and Tom Hemmings ... that perhaps erythromycin stearate wasn't all that it was made out to be. (Boggiano, Interview 9)

Boggiano believed the solution to this problem might lead to a new antibiotic product. Leading a small team, Boggiano started work in 1968 on a new erythromycin stearate product which would have an enteric coating to protect the active ingredient against gastric acids. Ethryn Cota-Tabs was launched in 1970. This was not heralded as a "breakthrough", for there was, at this stage, no scientific proof the coated product was

more efficacious, nor sufficient evidence to disprove claims by the rival company (manufacturing the leading brand of erythromycin), that the uncoated product was self-protective against gastric acids. Spurred on by colleagues such as pharmacokeniticist Sidney Bell (an expert on the bodily absorption, distribution, metabolism and excretion of drugs) from the School of Pharmacy at the Institute of Technology; Boggiano and Gleeson set up an experiment in the early 1970s, to provide scientific proof of the destruction of erythromycin by gastric acids and evidence of the lack of therapeutic potential if given in the standard formulation. The results were first presented at *The Chemical Society* meetings and company-sponsored gatherings, and it is alleged attempts were made at these events, and over subsequent months, to intimidate the pair into withholding the evidence. Indeed, as a sales representative of a rival company suggested, they should not "kill the goose which laid the golden egg". Boggiano and Gleeson nevertheless wrote up the results of the study and submitted the paper for publication in 1973. Publication was unusually slow, perhaps because one of the key referees was an employee of a rival company; and it was another three years before the paper appeared in an academic journal (cf. Boggiano & Gleeson, 1976). The case of Eryc is not unique in this regard. Other scientists have reported similar problems, given that it is up to their peers - who are often also their competitors - to "approve" their work and allow it to be made public (e.g., Goldberg, 1988, pp. 56, 63). Moreover, sociological studies of science have documented the misunderstandings and resentments which characterise scientific practice over the circulation or withholding of scientific information (cf. Mulkay, 1979, p. 71).

During the early 1970s, efforts were also made to improve the Faulding product, Syndopa, which was not enteric coated. Bill Scammell had spent most of 1968 in Europe and America and brought back many ideas for the improvement of existing drug products and new developments in equipment for pharmaceutical production. He and Boggiano decided they could improve Syndopa by combining the idea of an enteric coating with a process of pelletisation. Pelletisation had been thought for some time to be a means to improve the absorption of drugs. Boggiano and others argued that the irregularity of the delivery of a drug into the blood system was the result of the action of the pyloric sphincter (a little valve which acts to prevent large particles from passing through the stomach into the intestine). Tablets could be trapped in the stomach until they were broken into small pieces, a process which could take as long as 12 hours. In some instances however, the tablets could escape through the valve by chance; in which case the active ingredient would have an immediate effect. Pelletisation offered a possible solution, for tiny pellets were thought to be treated as a liquid by the body and able to travel at a predictable and constant rate through the pyloric sphincter. In 1972, Faulding launched a new product, Levodopa, which was both coated and pelletised. This product was produced only in small quantities for hospital use and not released onto the international market.

Despite the coating of erythromycin in Ethryn Cota-Tabs, clinical experience continued to indicate problems with absorption of the antibiotic. Between 1971 and 1973, Boggiano and his team at Faulding, supported by company board member Bill Scammell, developed the idea of a new erythromycin product. This time it would be both coated and pelletised, like Levodopa. By the end of 1972, "Eryc" was starting to become more than an idea, and by 1973 Boggiano had a proto-type which he tested on himself

with the help of a neighbour and local general practitioner, Adrian Vanderborch. Engineer Michael Story was busy in 1973 solving the problems of the production process, for the firm's existing pelletisation equipment was not suitable for bulk manufacturing. A small trial of Eryc occurred in 1974 after the finalisation of the formulation (this time on Boggiano and his biochemist colleague Bob Braybrook), and a more systematic clinical trial was led in 1975 by Peter McDonald (McDonald, Mather, & Story, 1977) on staff at the Flinders Medical Centre: a hospital recently constructed and fully staffed but yet to be opened to patients. Eryc was finally launched in Australia in 1977.

Analytical Reflexivity and the Case of Eryc

This account of the "history" of Eryc centres around the historical "moment" in which a scientist or scientists experience a "gestalt" switch; where a solution to a problem appears solvable by applying concepts or experiences from another sphere of activity (cf. Mulkay, 1979, p. 105). It emphasises the *collective* nature of science; the necessity for sustained informal contacts with other scientists (cf. Collins & Harrison, 1975), and the medical community, in order for new knowledge and technologies to be produced. It also demonstrates how new products may be developed as a response, not to scientific "proof" (for this may come later, if at all), but to the "construction" of a socio-clinical *problem* and a building awareness of a possible industrial "solution". A partial "consensus" about a problem with the absorption and acid destruction of the drug was reached within the scientific and medical communities prior to the provision of conclusive "proof", and in the face of contradictory evidence: a not entirely unique event, as Gilbert (1976) shows in his study of radars. It also reveals the historical indeterminacy of a technology; for a very different product may have resulted if, for example, Boggiano and Gleeson had agreed to protect the short term interests of the pharmaceutical industry by failing to disclose their findings on erythromycin efficacy; if Scammell and Boggiano had not convinced other members of the board to support the further development of erythromycin; or if changes in the state-financed *Pharmaceutical Benefits Scheme* (PBS) had not, in 1971, enhanced the possibility of profit from an improved, future erythromycin product.

The above account identifies Boggiano as an inventor of the idea upon which the new product was based, and differs little from many other case studies within the sociology of science/technology tradition. Like these others, however, it fails to incorporate analytical reflexivity, and hence uncritically reproduces an historical narrative of heroic "discovery" which took place without the intervention of the researcher. Consequently, as we shall see, the socio-technical construction of Eryc has only partly been told.

The application of primary analytical reflexivity to the case of Eryc reveals the researcher's significant role in the identification of Boggiano as the "inventor". Admittedly, the extent to which a particular researcher can contribute to the "construction" of the inventor will vary in practice. In most case studies, researchers do not "construct" the inventor, for they do not challenge existing accounts, but merely reiterate past claims about the identity of a given inventor from previous studies or reports (e.g., Hong, 1998). Unless the research is specifically about a case of "mistaken identity", further research on this matter is not considered important to the problem at

hand. In a case such as Eryc however, where the identity of the inventor was previously unknown (or known to only a small number of individuals), the researcher bears greater individual responsibility for "constructing" the inventor *per se*, for in producing a case study or "history", knowledge is being "created" as well as legitimised and transmitted into the future.

Further insights are produced with the application of analytical reflexivity. We can note for example, that the focus of this study, like others in the sociology of science/technology, is on a defined sphere of social, or socio-technical action, and largely excludes the contributions of those *external* to the scientific community. This is because, for the most part, the sociology of science/technology presumes, rather than problematises, the boundaries between "science" and "non-science". Although scientific activity is acknowledged to be collective (cf. Shapin, 1995), analysis is usually restricted to a narrow sphere of action regarded as "scientific work", and little thought is given to how this activity is conducted within a much larger, hierarchically-organised production system in which the relative value and reward for various tasks, skills, roles, and status positions are differentiated and determined. Most accounts do not explore the hidden assumptions which guide the granting of *significance* to specific kinds of actions and individuals in particular roles. The resulting case studies merely reproduce the hierarchical context of science, contribute to the prevailing belief in science as a distinct and highly valuable arena of social action, and fail to problematise its relations of power and prestige. As a consequence, the convention is to prioritise the contribution of "the inventor", Boggiano, over that of Scammell, the laboratory assistant, the individuals who made the test tubes, answered the telephones, or provided security services. Although it might well be useful and legitimate for an individual researcher to focus exclusively on a specific sphere of social action as an analytical exercise (as Weber did in *The Protestant Ethic*), the sociology of science/technology could only benefit from problematising the prevailing assumptions of the field; including the distinction between "science" and "non-science", and the very Western-centred, individualised, liberal-democratic conception of the inventor, which celebrates an individual contribution and prioritises "scientific contributions" over others.

The application of primary analytical reflexivity would, moreover, be a valuable exercise in the sociology of science/technology, because it reveals the theoretical "logic" behind the narrative. The case study of Eryc, for example, is shown to employ a *presentist* approach to history. Presentism means that individuals, events or phenomenon, considered significant in the present context, are traced to an origin point in the past, and a small selection of elements from the empirical past are ordered into a coherent narrative to explain its trajectory. The presentist approach to history has been criticised as an artificially constructed *mythology* in which imputed social actions and causal relationships did not, in fact, take place (cf. Jones, 1974; Seidman, 1985, p. 14; Skinner, 1969). Most sociology of science/technology accounts are presentist because they take the technology or new idea from the present, and trace an historical pathway in which objects or ideas are passed (and perhaps transformed) between actors or sites of interaction over time. Yet the selection of a "point of origin", as well as the object of analysis, are relatively arbitrary. Just as a standard "family tree" restricts the identification of an individual's forebears to the male line, leaves out illegitimate liaisons and offspring, and so disregards half the genetic "history" (and almost all evidence of

social history); a presentist "history" is highly selective and distorted. In the case study of Eryc, the author of the account might have focused their gaze elsewhere, and found, for example, the "origin" of Eryc in the first articulation of a theory of the operation of the pyloric sphincter, the development of the first antibiotic, or even the formation of the PBS. Yet the limitations of this approach to the past are rarely discussed in this literature, and few scholars appear aware of their assumption that there *is* an historical pathway and an historical "root" which can be "traced" and "located" by the researcher.

The conventional alternative to presentism, is to view the past as characterised by disjuncture and disruption, with each idea, event or technology historically located and created anew in an historical "moment". This is the *historicist* conception of history, where the historian seeks to place events, individuals and ideas in their historical context, and to "understand the past, completely as possible, in its own terms" (Jones, 1974, p. 355). In the historicist approach to history, knowledge is pertinent only to a particular problem and intention (Skinner, 1969, p. 50) and is therefore always "new" (Seidman, 1983, p. 85). From an historicist perspective, it is not legitimate to trace the history of a particular idea (Camic, 1997, p. 6): nor indeed the history of a technology or scientific controversy. Hence the historicist approach is rarely found in the sociology of science/technology.

Actor-network theory offers a variation on the conventional, presentist, sociology of science/technology approach to historical case studies. Rather than focus on the technology as it appears over time, there is an attempt to over-come distinctions between object and context, actant and background, and past and present, by defining the essence of an innovation in the *totality* of the innovation network (cf. Latour, 1991, p. 115). The focus on the whole rather than its parts, takes into account the way in which all elements within the network are co-produced and transformed over time, such that at the "temporary" end to the story, none of the original actants can be found (Latour, 1991, p. 117). This enables a problematisation of the concept of invention, for in the invention of the Kodak camera, Latour argues the invention may be the camera, but it might equally be the creation of the patent system, the development in paper processing, or even the emergence of a mass market for amateur photography. In this approach, rather than explaining an innovation's historical path *retrospectively*, an empiricist, historicist approach is assumed, and the researcher takes what an actor *does* to an innovation to define the actors (cf. Latour, 1991, p. 121). Also consistent with an historicist approach is the rejection of the necessity of the researcher's intervention to ascertain causality and explain socio-technical change. For the historicist, authenticity can be found in the integrity of the text itself (Jones, 1974, p. 355). Similarly, for Latour, no "third party" or "external" point of view is essential, for an innovation is explained by "triangulating the many points of view of the actors" (Latour, 1991, p. 124). Admittedly, the researcher has a role in weighing up the importance of each element within the case study (Latour, 1991, p. 116), but it is the examination of the actions of the actor with regard to the innovation that allows the "cause" of the innovation to be found (Latour, 1991, p. 121).

Weber's approach to history offers a compromise between the presentist approach of conventional sociologies of science/technology, and the historicism of actor-network theory. Although most historians perceive the actors from the past to be central to our understanding of the past, Weber's theoretical schema proposes two sets of actors which must be taken into account. These are the actors from the past who were significant in

the creation and transmission of new ideas (or technologies), but also a *second set of actors*, who in the present context "make sense" of the past by "constructing" these past acts and events as "social facts". Both sets of actors are socially and historically located (cf: Weber 1968), and orient their actions to that which is meaningful (cf. Weber, 1949, p. 78). Weber's theories can be readily coupled with the notion of a primary analytical reflexive researcher, who, in constructing a case study and producing a "history", is a significant social actor in the *co-production* of an invention and an inventor. Added to the concept of the analytical, reflexive researcher however, must be Weber's theory of history, for he argues the past cannot be adequately understood either retrospectively, as it is in the presentist approach to history, by constructing the past according to present definitions and concerns; nor can it be assumed, as it is in historicism, that the "facts" of the case study will in themselves explain the trajectory of an idea (or technology) and thus historical change. Weber's proposition is that our understanding of the past will come from combining both empirical study and theoretical reasoning with the critical reflection of the historian. The latter, which has been discussed in this paper under the notion of primary analytical reflexivity, is the process through which the historian reflects on how they have constructed socio-historical facts from empirical reality and social theory, and, importantly, how they use these social facts to explain the past.

Extrapolating from Weber's theories of knowledge and history, it can be proposed that standard case studies from the sociology of science/technology and the translation network approach, are, in effect, "ideal types" illustrating *unique* patterns of social or socio-technical action in a given historical context (cf. Weber, 1949, pp. 101-103). The reflexive researcher will neither conflate these with the empirical world, nor assume the analysis is complete once the "historical sketch" has been drafted. For Weber (1949:101-3), the construction of an ideal type is an essential first step in analysis, but is always partial and distorted; does not capture the "intrinsic" nature of reality (Kalberg, 1997, p. 222; Weber, 1949, pp. 72-73); and is unable to provide the means to demonstrate causality (Kalberg, 1997, pp. 222-223; Weber, 1949, p. 102). Understanding causality for Weber is the *second* step in analysis, within which the ideal type can serve only as a reference point. In other words, while Latour and the historicists seek causality within the bounded arena of action described in the case study, and the sociologists of science/technology find it within the internal dynamic of science or the social context; for Weber (1968, p. 29), historical change is produced through a multiplicity of social forces, but the *imputation of causality* in a case study is an action of the researcher.

Conclusions

The case of Eryc has demonstrated the critical role of the researcher in the construction of a history of an invention, and how reflection on this process is the tool sociology provides for taking scholars one small step beyond the positionality of perspective. While sociology has, for the past century, increasingly provided room for reflection on the relationship between the "knower" and the production of "knowledge", Weber's theories of knowledge and of history re-focus our attention on the socio-historical construction of the "object of knowledge" and the "objectifying relations" which also shape our perceptions. The application of Weber's theories in this field

indicate that reflection on this process is critical to escaping the "gravitational pull" of conventional sociological analysis.

Recent sociology of science/technology approaches to invention indicate the complex interweaving of social and technological phenomena, but have yet to incorporate primary analytical reflexivity into their case studies or theoretical schema. While the concepts of co-production, mutual constitution and the translation process resolve many previous limitations, there is a need to acknowledge the researcher as equally an actant in the co-production of social and technological change. Without primary analytical reflexivity, which necessitates an awareness of the researcher's subjective-objective "choices", the historical trajectory of an innovation, its constituent elements, and the causal forces that brought it into being, are unlikely to be fully expounded.

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