INTRODUCTION

The thesis will examine the relationship between relations of force and the construction of credible knowledge in a laboratory. Some laboratory researchers occupy loci of relatively greater trust and authority and exercise their superordinate powers over others, who are expected not to demur when they receive commands. It is also the overwhelming case that they do agree, or at least acquiesce to such commands, as they are professionally socialised to conjoin trust, relations of force, and professional intercourse, and to regard these as in a desirable agreement with the requirements of science. The field study that provides ethnographic data for this thesis was conducted between February 1, 2004–June 1, 2004, in a bioscience laboratory. It is part of a research centre (hereafter called the Institute) located in the Southwest of India. The Institute is a highly regarded centre of research in investigative areas with a biological accent.

The laboratory is conducting research in intersecting areas of oncology and virology. It is under the supervision of a professor (Sridhar hereafter) who is a published authority in cell biology. Although young, he has acquired a reputation as an expert in the molecular analysis of cervical tumour formation. The population of the laboratory included PhD candidates, postdoctoral fellows (postdocs hereafter), and research fellows who doubled as technical assistants. In addition, there were MSc students and undergraduate summer interns from other institutes, completing research projects or acquiring some laboratory exposure. The stated mission of the laboratory of fieldwork, is the study of 'the molecular pathogenesis of cervical tumours.' (Hereafter, I shall refer to the laboratory as CCL, or Cervical Cancer Laboratory) The mission statement means that the researchers are investigating, at a molecular level, the generation of cervical tumours as a result of infection by Human Papillomavirus (HPV) types 16 and 18.¹

The principal focus is on the human cellular processes through which HPV infection can cause cervical epithelial tumours, that is, how the host gene products interact and collaborate with the viral DNA so as to permit the unchecked activity of Notch, a pathway in cell-growth signalling. Notch signalling is the term

¹ H. Zur Hausen (1991) 'Human papillomaviruses in the pathogenesis of anogenital cancer'. Virology 184, pp. 9-13. The work of this scientist is regarded and followed as an exemplar by CCL.
for a biological pathway normally associated with organismic development and differentiation. The work of CCL has been drawing on the possible ways in which the viral action subverts Notch signalling, thereby leading to transformation of epithelial cells so that they exhibit cancerous behaviour. The projects of the researchers in CCL are variations on this theme. The researchers have been doing work on selected and related aspects of the effects of the over-activation of this particular signalling pathway.

Prior to the main fieldwork I undertook pilot studies at two Delhi laboratories. One of these is engaged in research in molecular cardiology and the other in molecular medicine. The duration of each pilot was about two months, and the studies were conducted between July 2003-November 2003. The observations from the pilots will serve as 'controls'. I shall be using them to occasionally complement and clarify the data from the main fieldwork. The pilot studies showed that different laboratories may exhibit local differences. Thus one sees differing supervisory styles of the PIs, differing modes of restriction of laboratory space and working hours, and differing forms of social activity. However, the similarities are far more striking than the differences. Where necessary, any significant differences between the pilot and main data will be highlighted in the thesis.

PRINCIPAL QUESTIONS

It may be pertinent to first address the question: why do authority, power and capital (symbolic, financial, material and human) in a research laboratory merit the researcher's attention? One should address the objection that one could select any organizational unit, from any of the modern professions for study of these factors. After all, the laboratory is a 'planned organization of producers' like many others. As a

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2 The use of the term 'space' here is deliberate. I conceive of the laboratory as a site whose work and longevity are imbricated and co-productive with space considered in its physical, disciplinary and epistemic aspects. In this regard, the physical element of these spatial arrangements is never left in a pristine state; it cannot be thought of as 'purely' physical. Conversely, the epistemic and disciplinary aspects of laboratory space do not exist in a dematerialized void. In this regard, Henri Lefebvre's idea of 'abstract' space is both useful and misleading. See Henri Lefebvre (1998) The Production of Space Blackwell. Oxford UK and Cambridge. Massachusetts. Lefebvre believes that space becomes abstract when concepts and representations annex the space of lived experience. This idea is useful insofar as it encourages critical reflection on, for instance, the ways in which the irregularity of the quotidian experiences of laboratory research are flattened out in the making of credible inscriptions. It is misleading when it seeks to irreconcilably oppose experience and conceptualization, material concreteness and abstract thought. I would argue that the laboratory space is a rational governable space insofar as it brings ideas and programs of control and calculation to bear on a material location, thereby making it the site for the production of restricted experience that can be inscribed within and judged 'outside' the laboratory.
professional group, it has a division of labour, relations of superordination-subordination, 'inner solidarity coupled with exclusiveness towards the outside' such as are also 'found in the state as well as in a religious community'.

In response, one may say that the play of relations of force is taken so much for granted in the case of all organizations that there is a tendency to ignore the specificity of association in particular contexts, such as the specific context of knowledge production. I would like to see how the intersubjective, force-driven relations in a scientific work unit and the work produced by it form a particular modality. The organizational features of a particular laboratory may be understood as the mutual conditioning of forceful association and work-content. The identification of such features at a specific professional locus, that is, in the laboratory, must accompany any attempts at generalization across the professions. Simmel holds that a certain interactional form, such as competition or super ordination-subordination can be found in sociations with differing interest. He adds however, that such general knowledge yields little. One must study the specificities first, in order to attempt a later generalization. While the form and content of laboratories present certain similarities to the form and content of other organizations, it is profitable to identify and investigate the features that are distinctive to scientific work as a mode. According to Michael Oakeshott, human experience can be recognized in terms of its modality, that is, as a variety of independent, self-consistent worlds of discourse, 'worlds of ideas'. Each of these modes is to be understood as 'an arrest in human experience.'

A mode modifies on-going activity, enabling us to experience the world historically, practically, scientifically, poetically, as we choose. The role of the social scientist is to discern the leading ideas that each mode postulates. Thus one must examine how each mode looks at the world through its particular abstractions. History, science, and practice all look at the world through their limited sets of organizing ideas. Thus one may argue that scientific research-work has a specific modality in its practices of

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calculation and control, modes of perception and inscription, technical vocabularies, types and distributions of authority, physical locations, object domains, and forms of knowledge. The sociological investigation of laboratory life is a task as specific as its subject matter. Oakeshott’s later work sees any settled activity as a practice. Thus he speaks of the practice of history or of science. As a result, he is able to see the possibility of play between modes, which does not injure their integrity. Adopting this view, one accepts that modes of association in scientific work are conditioned, like other professions, by their situation in modern capitalism. But it is still the case that features derived from the macro-structures of modern capitalism gain a distinctive modality, a parochiality, in the actual practices of each profession.

As the working units of scientific research are predominantly the laboratories, I focus on the associations in laboratories. In order to address questions of the specificity of the link between credible, impersonally trusted scientific research and relations of force and of trust, that is, personalised acknowledgements of institutionalised charisma, one must identify and investigate the locus, the field of positions, where these factors condition actual, everyday, lived practice. This locus is unmistakably the laboratory. Through participatory observation of laboratory life, I have acquired data on the modulation of laboratory association and practice. This data will be distributed through the four chapters of the thesis. While adopting the laboratory as a fieldsite, I do try to remain aware of the implications of presenting the laboratory as some exotic terra incognita or thinking of the sociologist as a heroic stranger with a ‘god’s eye view.’ Second, although I incline to the view that laboratory activities often exhibit profound similarities to activities in other professions and even in everyday life ‘beyond’ the laboratory boundaries, I hope to avoid the attitude of astonishment that segues into a sort of forced banalisation in studies such as by Bruno Latour and Steve Woolgar. Such attitudes contribute to and are sustained by the natural-social/science-sociology demarcation; this is ironical considering that laboratory ethnographies seek to undermine this demarcation. The relatively low number of ethnographic studies of the laboratory is in part a result of suspicion and determined disengagement between sociologists and scientists. The will to

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5 Bruno Latour and Steve Woolgar (1979) Laboratory Life, Sage, California and London.
exoticise or banalise the ethnographic ‘object’ is an ‘alter’, not the antonym of the suspicion of the sociologist to the scientific laboratory. The frequent refusal of laboratory directors to admit the sociological observer is the effect of the scientist’s reluctance to tolerate the danger of objectivity turned inwards, to endure the stranger, the hostile gaze of the non-scientist.

There is at present a dearth of ethnographic research on the role of relations of force in ordering knowledge production in the laboratory. Sociologists generally accept that the laboratory, in its social and cognitive aspects, is to be understood wholly or largely through the influence of ‘wider social factors.’ This perspective unites sociologists who ground science in a socio-economic structure with those who ground it in social norms, culture or ideology. Unfortunately, most such studies focus on so-called extra-scientific aspects such as the social regulation of technoscientific applications. Sociology tends to accept the separation of contexts traditionally seen as internal and external to science. This inhibits studies that would effectively synthesize ‘interactional’ ‘institutional’ and ‘cognitivist’ and ‘externalist’ orientations, while also effectively challenging the validity of the separations that set up such orientations. Insights from political science and economics should be strongly integrated with sociological and ethnographic perspectives on the ‘interior’ context of science, on laboratory work. As long as we think of the laboratory as a physically bounded location, we will fail to see how its workings are constituted and sustained by ideas and institutions of ‘the public’. The laboratory may be physically enclosed; but it would be unwise to think that this sort of physical enclosure seals the laboratory from sociopolitical contexts or makes it an intrinsically ‘personal’ concern. The laboratory supervisor may act in extremely discretionary fashion, so much so that the laboratory may seem like his personal fief, a domain ‘ruled’ by his choices, which may seem unconstrained by factors external to themselves. As will be discussed in Chapter 1, the choices and

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b One may recall Kant’s prefatory remarks in *The Critique of Pure Reason*. Kant holds that any hold on public allegiance had to survive the test of free scrutiny. He makes a similar point in ‘What is Enlightenment?’, where he says that unfettered critical discussion would facilitate freedom from blind allegiance to traditional ways of thinking. Kant’s notion of Enlightenment relies on a distinction between the private and public aspects of reason. The private aspects involve the protection of self-interest or the narrow meeting of obligations such as obeying the law or paying taxes. The public aspect of reason implies the individual’s right, or rather his obligation, to engage in free public criticism. A just society was characterised by free public criticism, a stable society, adherence to the law by the members of civil society, and the consonance between the law-maker’s will and public opinion.
creativity of the supervisors and the subordinate researchers may be strongly personalized. However, insofar as these choices invoke and refer to public fora of accountability and legitimation, they cannot be deemed personal in a fundamental sense.

It is important to identify and analyse the ways in which academic supervisors exercise authority in establishing their specialty areas, in dispersing their authority, through the social networks of associates in the laboratory and beyond it, networks which they constantly work to extend and protect. The extension of networks is linked to the extension of alliances, of chains of credibility and command. PIs are micro-actors who seek to occupy important nodal point in a network of influential actors. They seek to strengthen the depth and extend of their command and influence over subordinates, techniques, and object domains. To this end, they seek to control the application of the technologies that shape researchers and research practices at a micro-level, the particular locale of the laboratory. I draw here on Michel Foucault's discussions of the constitutive effects of power. He sees power as different from the subject-destroying use of violence or the forced production of consent. The specificity of the exercise of power is in that it conducts ('leads' with varying strictness) the possible 'ways of behaving' (forms of conduct). Truly effective power is subtle and not warlike. It quietly orders the possible field of action of others. With the calculated, efficient and parsimonious deployment of means and resources, it ensures that actions are shaped to meet objectives. The rationalized schemes, programmes and devices wielded in this way realize and maximize the 'probability' of domination, the prospects that commands will be obeyed, even making explicit commands largely superfluous.

Cognitive centralisation reflects the unequal distribution of symbolic and economic capital in the scientific field. Certain scientists, possessing a preponderance of such capital, are the elites of science. The statement that science has elites means that in scientific communities there are some practitioners who have

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7 Specialty area may be understood in the sense that Richard Whitley defines it: as 'focussed on explanatory models and definitions of the phenomena under consideration. Membership of specialities implies commitment to particular types of accounts and preferred ways of formulating the underlying object of concern' (See Whitley (1976) 'Scientific Disciplines' Social Studies of Science, 6, p473.) If we see the laboratory as the site for the sustenance of specialty interests and activities, then we can also see how the working group is bound together by quotidian organizational and technical exigencies and activities in an ad hoc but nevertheless cohesive manner.

a 'special control over scarce resources.' This is possible because in science the rewards are distributed in markedly unequal fashion. The practitioners who are members of the elite have a proportionally greater ability to control the activities of others and also to regulate the entry of new potential recruits to the elite corps, that is, to control the investiture of symbolic capital. They do this in a variety of ways. As PIs, they are heads of large research groups and are directly involved in the shaping of the junior scientist's careers through the control they exercise over the use of apparatus and the directions of research projects. They tend also to dominate the committees of funding agencies. Their open exercise of authority and power is complemented by the exercise of informal, personalized, and uncodified influence. They are institutionally, even if sub-textually, deemed fit to pass just judgements on the adequacy, value and relevance of new research. Mulkay says 'Thus those scientists who wish to advance their careers and/or to produce acceptable contributions to knowledge must comply with the cognitive standards set by these leaders; whilst those who aspire to elite membership must be prepared to undertake the same prolonged commitment as their leaders to the solution of problems defined as significant in accordance with the culture of the research community.'

Elites invest their reputations into certain areas of research; their position as elites is contingent upon the maintenance of reputation. The eponymous association of the elite scientist and 'his' research is strongly connected to the transversal estimation of research and researchers. Although the intellectual professions are supposed to be democratic, they are not so in practice. Not all research communities have the same prestige. Not all scientists within any research community speak with the same authority. Each of these communities is constituted in part by the actions of individuals with influence, with the power lent by fiduciary estimations. The authority of novel ideas and of the legitimating institutions, such as the publication system and the learned societies of peers, cannot be separated from the actions of these power-

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9 See Michael Mulkay (1976) 'The Mediating Role of the Scientific Elite' in Social Studies of Science, 6, pp 445-470. (See pp. 446-447)
holders, who constitute the 'establishments' in each community. These distributions of power are co-productive with the 'collective aspirations and goals, procedures and criteria' of the community.

If we take the laboratory as a micro-level instance of such macro-level power configurations, the role of the PI as a linchpin in laboratory life gains considerable sociological interest. Individual authority-holders, especially those with a preponderance of prestige, claim to speak for disciplines, understood as 'a framework... a set of scientific values [applied to] some domain or field of reality.' There is sociological interest in examining the implications of the location of the PI as an authority-bearer in a configuration of variably authorized scientists. Some of the scientists in this configuration are elites, but most are not. The configuration of scientists, composed of elites and non-elites, may be identified at different levels. First, one may identify the local level of the laboratory, which is my focal interest. Beyond the laboratory, one may identify the level of the research institution of which the laboratory is a part, and further, the configuration of spatially scattered laboratories, who regard each other as peers operating in affiliated specialties. The different loci in these configurations are invested with levels of transversal estimation and authority that vary not only at particular time-points but also over time-periods. There are configurations encompassed within larger configurations. That is to say, if we consider the PI as a member of a group of visible and influential elites, we must also remember that among the 'sub-configuration' of elites there are core elites with a particularly high level of visibility, reputation and influence. One can investigate the implications of power-relations for knowledge-construction without making out a case for or against the desirability of elites in science. Thus, for instance, one may see whether interests in the longevity of certain paradigms may be entwined with the protection of authority and power associated with research along those paradigms. It would be inadvisable to label such action as unscientific. The defence of research should instead be investigated to see if it supplies evidence of organised enterprise-interests in science. Thus a

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11 See Jerome Ravetz (1971) *Scientific Knowledge and its Social Problems*, Penguin, Harmondsworth. p. 82. The author points out that the self-discipline of scientists and the sustainability of their respective research communities are most 'sensitive to the leadership' they receive.


focus on paradigmatic science may be conjoined with perspectives on the corporate organization of research units, a phenomenon that has been markedly on the increase since the Second World War.\textsuperscript{14}

THEORETICAL ORIENTATIONS

In Chapter 1, I will draw from the work of RK Merton, Thomas Kuhn, Karl Popper, Michael Polanyi, Michel Foucault, Gaston Bachelard, and Edward Shils,\textsuperscript{15} whose contributions, even with their radically divergent perspectives, indicate the relationship between relations of force and the making of credible knowledge. What clearly emerges from a comparison of these perspectives on science is that there is scope for investigating the significance of relations of force and prestige in the performance of research.

\textsuperscript{14} The Second World War, often called the physicists' war, was a major turning point for the fortunes of science. For instance; Hiroshima and Nagasaki were proof of the potential of atomic physics to safeguard national interests. The military-political insistence on useful research in the post World War period led to the transformation of research into a quasi-industrial affair. This gave a huge push to the elaborate set-up of nuclear research, and later to 'big science' projects. Big Science is science in which a giant research task is divided for study by several teams. The teams operate with highly sophisticated apparatus: indeed, today, research done without such apparatus is regarded as being of negligible value. The teams operate in secrecy about research progress and results. They are subject to supervisory surveillance and control. The elaborate technology and personnel in Big Science projects imply that the projects are capital-intensive. The inputs generally come from government coffers. As such, the funding body insists on quick and impressive returns to justify investments. Big Science is designed with an eye to usefulness and profit. The extra-scientific control implied in research supported for its applicability implies that the control extends right into the internal contexts of scientific research. Science started to move out of its cloistered pre-war university setting; its new scale of operation and production put it beyond the capacities of the autonomous scientist. Scientific work moved from its earlier leanings toward natural history and 'disinterested' analysis towards explicitly interested synthetic-experimentalism. The operation of Mertonian norms was always doubtful even in 'pure' research; with contemporary science, they have been conspicuously obviated. Instrumental views of experimental areas were firmly institutionalised in the new research environment. Research groups became increasingly specialised and competitive. The drive for results put continually produce results exchanged for grants and visibility, the training pressures and costs to form laboratories, made for a new work culture. This fostered an instrumental attitude in biosciences, as well as attitudes of secrecy and rivalry. The institutional magnitude of the ethical-legal-social controls over nuclear and genetic research are indices of recognition that Big Science is about big funds to generate big, profitable, and socially applicable results. Such control therefore extends over the inside of the laboratory and the outside, which is the arena of scientific application. Western contexts of research and epistemic legitimation have become the reference models for contexts of research in the non-Western world. For useful discussions on the rise of Big Science in the postwar period, with focus on Western contexts, see A. Weinberg (1967) \textit{Reflections on Big Science}, Pergamon, Oxford; John Ziman (1967) \textit{Public Knowledge}, Cambridge University Press, Cambridge, John Ziman (1982) \textit{Reliable Knowledge}, Clarendon Press, Oxford, England; D. Dickson (1984) \textit{The New Politics of Science}, Pantheon, New York; D.S. Greenberg (1969) \textit{The Politics of American Science}, Penguin, Harmondsworth, D.S. Greenberg (1967) \textit{The Politics of Pure Science}, Dune NEL, New York; J. Haberer (1969) \textit{Politics and the Community of Science}, Van Nostrand, New York; J.J. Salomon (1973) \textit{Science and Politics}, Macmillan, London.

in the laboratory and its preparation for legitimation in contexts beyond. It is possible to use sociological tools to study the role of interests in power and recognition in the making of scientific knowledge.

According to Merton, science has standards that are universal and objective. He has portrayed the contexts of research as inseparable from the untainted operation of certain scientific norms. These norms form a cohesive set. They are universalism in the evaluation of research; communism, or the unfettered public ownership of knowledge; disinterestedness, or research undertaken for its own sake; originality in choice of research; and the norm of scepticism, which means that the scientist takes nothing on trust. Elites are central to science because of the interest of scientists in seeking recognition; in any society there must be a way to ensure that the best and the most superior hold command positions. But this actually conflicts with his perspectives on the universalism of science. Kuhn’s work on tradition and change in science presents an interesting corrective to Merton’s universalistic and homogeneous depiction of science. The tension between continuity and revolution in science emerges from the Kuhnian discussion of changing paradigms and the role of these in scientific appraisal, of the ‘gestaltist’ thinking whereby scientists seek to resolve anomalies till these become an insupportable burden for the paradigm. However, both Kuhn and Merton seem to agree that elites are essential for the health and growth of science. Thus they also seem to subscribe to the view that elites are capable of a superior sort of intuition in guiding research, a point opposed by Karl Popper who believes that science needs rules and not elites, openness and intersubjectivity rather than powerplay.

Polanyi’s work seeks to provide an integrated, albeit theoretically unsophisticated, perspective on the sociological and methodological aspects of science. The sociology dominates the methodology, in a manner of speaking. He holds that science is not autonomous of the wider culture but is a part of it. His work is ultimately concerned with answering questions relating to the place of science in society, the question of science as a resource for social welfare, and the exogenous or endogenous regulation of science. Polanyi holds that science is characterised by tacit knowing and ‘personal knowledge’. Scientists work through knowledgeable intuitions to produce credible and reproducible knowledge. This, in Polanyi’s
view, gives science its sociological and epistemological uniqueness. Polanyi’s ‘subjectivist’ perspective seeks to explain how knowledge is actually produced in its living context. It tries to play down the traditional emphasis on the objectivity of science. The subjectivism of science may mean that scientific discoveries never reveal the true nature of the world, but this is the only way to proceed with research. Apprenticeship and tradition play an important part in the tacit generation of knowledge. In this regard, the exercise of authority, also tacit and frequently arbitrary, must be recognised.

Polanyi’s views present a sharp contrast to the work of Merton, whose work is generally criticised for its subordination of the actual workings of science to its normative façade. Polanyi anticipates Thomas Kuhn in speaking of tradition, innovation, and the negotiation of anomalies through psychosocial processes. Both Polanyi and Kuhn also agree on the significance of authority-bearers in transmitting research traditions. It may be possible also to bridge Polanyi’s view on tacitness, experiential knowledge and apprenticeship with Bachelard’s views on science. Bachelard holds that scientific knowledge takes on objectivity. But this objectivity is not one born of the sensualist, romantic submergence of the subject in the object. For knowledge to take on the quality of experience, it has to involve separability, however tenuous and variably sustained and defined, between the scientist and the object of research. This means that the engagement with the research object is such that there is a loss of individuality, but not a total loss of identity, for the subject. Scientific work is not characterised by a purely immediate, experiential quality. It is discursive, and as such it occupies a social dimension in that the researcher’s performances invoke other subjects who are also similarly placed. The researcher is reflective, monitoring his work, and exercising stringent discipline over himself and others. This reflectivity is related to orientation to significant others; self-consciousness in science, as in the sectors of everyday life, is inseparable from other-consciousness.

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16 See G. Simmel (1971) ‘Domination’, pp 98-99. As Simmel says, ‘A person of superior significance or strength may acquire, in his more immediate or remote milieu, an overwhelming weight of his opinions, a faith, or a confidence which have the character of objectivity. He thus enjoys a prerogative and an axiomatic trustworthiness in his decisions which exceed by a fraction the value of mere subjective personality…. By acting “authoritatively” the quantity of his significance is transformed into a new quality: it assumes for his environment the physical state—metaphorically speaking—of objectivity.’ The exercise of the authority of super-ordinates such as the PI in the laboratory, in matters of data evaluation, discussion of new published research etc., tends to guarantee itself. By virtue of its exercise, it seals itself off to interrogation by subordinates.
This consciousness of the other as 'an object of understanding and as an interlocutor' exercises a strong effect on the work of the scientist. It creates the possibility of doing work that can be individuated because it is characterised by a sense of responsibility to and for the other, that is, the audience of peers constituted and considered in terms of its ability to govern the institutions of credible knowledge.

From this comparison of the tacit and discursive dimensions in scientific understanding and meaning-making, we may be able to understand the general character of 'trans-subjectivity' in science as one sector of life similar to others, and the specific character of transversal appeals for credibility in science. I have appropriated the term 'transversal' from Michel Foucault who originally uses it to describe the proliferation of oppositions against 'the effects of power linked with knowledge, competence and qualification—struggles against the privileges of knowledge.' I believe that oppositions often closely resemble what they oppose. So 'transversal' might also be an apt adjective for the trans-border movements of knowledge, which are implicated in its legitimation and are also constitutive of its 'power effects.' As Foucault says:

"Truth" is to be understood as a system of ordered procedures for the production, regulation, distribution, circulation and operation of statements. "Truth" is linked in a circular relation with systems of power that produce and sustain it, and to effects of power which it induces and which extend it — a regime of truth."

I prefer the use of the term transversal to 'universal' because it does not neglect the local element in the extra-laboratory scrutiny of laboratory inscriptions. These examinations of new research are

17 D. Gupta (2003) 'The Domesticated Public', in (Eds) Gurpreet Mahajan and Helmut Reifeld The Public and the Private: Issues of Democratic Citizenship, Sage, New Delhi, p.64. The author discusses the emergence of modern conceptions of the public. These conceptions are characterised by a regime of injunctions that enforce a consideration of the other in terms that uphold the individuality of the other. This is possible because institutions of public accountability uphold considerations of private choice, creativity, individual freedom, and equality. These terms of consideration are therefore fundamentally different from those dictated by modes of status ascription. In the author's view, traditional modes of action are characterised by a fundamental nullity of regard for the other, and therefore also by a lack of regard for the things that one cannot do as a member of a public constituted of technically free and equal individuals. The private can emerge as a zone of choice and creativity for all only when public accountability ensures that no particular person or group misappropriates modes of choice at the cost of others.

18 See Michel Foucault (2000) 'The Subject And Power' p329.
19 Foucault (2000)'Truth and Power' p 132
concerned less with questions of universal truth and falsity than with an interest in the contingent and conditional testability and applicability of statements of theoretical relevance. Concepts are formally universal, but they are examined for the particular, identifiable use that scientists can make of them in their explanatory activities. One may speak of the non-local presence of research, rather than of its universality, as a sign of its achieved legitimacy. Transversality as a concept is useful because it posits neither absolute locality nor absolute universal homogeneity all across science.

Transversal movements of research are of two types. Taking the laboratory as a point of orientation, transversal movements are centrifugal and centripetal. Research is sent out of the laboratory for scrutiny, and in a reverse movement, legitimised research is parochialised, invested with local nuances in specific laboratories. Research products circulate across diverse milieux; 'regimes of value'\textsuperscript{20} are co-productive with these circulations and are identifiable in particular locales. I would emphasise here that this discussion takes the laboratory as the point of orientation in using the notions of centrifugal export of its own products and the parochial consumption, in a centripetal movement, of the legitimised, published products of other laboratories. To avoid confusion, with strict reference to the laboratory, I shall refer hereafter to the centrifugal movements of research as simply transversalisation, and the centripetal movements as parochialisation. Nevertheless, I do recognize that the fora to which research is exported for legitimation can also be seen as parochial, with members who work through their own local perspectives. In that sense of course, if one takes the specific forum, such as a journal or a conference, as the point of orientation, the movement of new research into them is a case of parochialisation. Chapter 2 will focus on the centrifugal movements of research into contexts of scrutiny and legitimation. The centripetal movements, disciplinary enclosures, and parochial aspects of the work and constitution of the laboratory are discussed in Chapters 3 and 4.

In this regard, I draw on the work of Edward Shils in understanding scientific legitimation. Shils' argument develops on Durkheim's work on the coercive power of society. Such coercive power is linked to

allegiance to a central cluster or core of values. Society has a core value system, which is an ultimate source of legitimations, a centre of sacredness, which legitimates the distribution of legitimacy and other social rewards. In order to understand the constraining effectivity of central values, Shils suggests that social constraint is exercised through the relationship between the centre and its periphery. The elites are associated with the centre whereas the other social groups are at the periphery. The peripheral groups vie with each other for the rewards that flow from the centre. Further, the members at the periphery are drawn into the sacral presence of the central values through their participation in ceremonial occasions. The emotional and psychic pitch of these occasions is such as to strengthen attachment to the core. However, in everyday life, these attachments do not have any obvious, explicit translation. Personal attachments, close relationships, quotidian tasks and routines shape behaviour into conformity with central values. The values remain invisible, untheorised.

Drawing from Shils, I take the idea of the centre as indicative of a context of legitimation for research from the laboratories that orient themselves to it. However, I would not argue that one can speak of a single forum, a single central context of legitimation in science. Research is far too diverse in its subject matter and audiences to have single legitimating fora. Thus the elites who manage research in the laboratory will not all subscribe to the same forum for recognition of legitimacy. Such fora of subscription and recognition of subscription are field-specific. Thus there are a multiplicity of centre-periphery relations and negotiations in science. It may be said that there are multiple locations of economic power and legitimacy in the global economy of scientific production and distribution.

Again drawing from Shils, one may say that the everyday life of the laboratory is devoid of the affective tensions associated with the activities of legitimation, as observed in a research seminar or else at the time of having a paper published. Nevertheless, there is a constant though muted reference to central values, which are reinforced by the ceremonies of legitimation, the occasions of ‘consecration’ of research, researchers, and the value-system of science. In Shils’ work on the relationship between the centre and the periphery, one may also identify similarities to the work of Talcott Parsons. Parsons also points out that
The most general values of the highest level are articulated at successively lower levels so that norms governing specific actions at the lowest level may be spelled out... At the lower levels, norms and values apply only to special categories of units of the social structure, unless they are the norms most general to all 'good citizens...' The central values, the most general norms are thus translated in the everyday context, where they are strengthened through tacit usage. Further, the paucity of theorisation is tied to the generation of cultural specificities, bounding scientific areas, 'curling' them up into self- and 'internally referential' contexts, characterised by 'multiple instrumental, linguistic, theoretical, organizational, and many other frameworks.' These porously bounded locales are characterised by the thickness of symbolic and expressive aspects, of shared beliefs and values that theorists such as Clifford Geertz associate with culture.

Drawing from these perspectives on the inter-translation and co-production of the 'grand' and the 'common', one may see a certain complex mode of association in science. The laboratory is an enterprise with associates who are goal-oriented with 'substantive purposes and wished-for satisfactions' that are achieved in a 'Janus-faced' mode, which is both formally, publicly, civil and privately, substantively strategic and polemical. This mode governs and is fortified by the flows of research and legitimation between the laboratory and the forum of legitimation. One may argue that each of these contexts partakes of an ideology of civility, and speaks and interprets the language of civil intercourse in scientific legitimation. This civility is nuanced in a very specific way in the context of the laboratory. This brings us to a consideration of the relevance of the work of Michael Oakeshott on enterprise and civil association, in the contexts of research performance and legitimation.

From Oakeshott, I draw my view of the 'consecrated' existence of published science and the 'recognition-oriented' conduct of laboratory research. Oakeshott recognises 'modes of association' in social life, of which the most notable is 'civil association' or the rule of non-instrumental law, which is actually

25 Michael Oakeshott (1975) pp102-184
Oakeshott's treatment of the state in natural-law tradition. Oakeshott defines the minimal conditions for the rule of law from an extremely conservative viewpoint. For him there is only one postulate of civility: a body of formal equals who recognise their obligation to obey the law, however disagreeable, and regardless of any divergent individual purposes. Law (lex) provides a language of civil intercourse among citizens (cives) in agreement with certain public conditions (respublica). Civil association presupposes human beings with purposes, plans, and projects of their own who recognise the law as not sharing these. However, while human beings can choose their own personality and aims (or at least think that they can), they cannot choose their civility. In civil association, they must accept and obey the lex of civility. In enterprise association by contrast, the associates have no such obligation. They have divergent purposes and where they co-ordinate their purposes through managerial activities it is for reasons of gain rather than civility. Oakeshott's distinction between civil and enterprise association is close to our distinction between state and civil society, the latter conceived of as 'the totality of private and particular interests.'

Unlike Oakeshott, who draws a firm line between the civil and the enterprise modes at least in their ideal character, I will engage in delineating the tension and mutual translation in the actual relations between these modes in the domain of research. To this end, I will regard civil association as providing scientific research with a certain 'ideological' sanctity that appears to be at variance with its enterprise mode in the laboratory, but actually supports it by yielding for it rewards of recognition and legitimacy that translate into symbolic and economic capital. Conversely, the enterprise interests and activities of the laboratory are oriented to gaining such civil sanctity in the sphere of scientific legitimation. In other words, I intend to explore the possibility that the laboratory is a thoroughly mediated part of society, and as such

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27 By mediation one refers to the operation of money, power, desire, and so on, as forms of coordination between parts of the social system. Mediation has the effect of inter-relating the differentiated parts of modern society. One would therefore do well to see science as mediated, as being tightly embedded in society and closely connected to politics and economy. This understanding derives from Talcott Parsons, the first to develop a theory of generalized symbolic media. (See Talcott Parsons (1969) 'On the Concept of Value Commitments' in Politics and Social Structure, Free Press, New York. (1977) 'The Social Systems' in Social Systems and the Evolution of Action Theory, Free Press, New York; (1977) 'Social Structure and the Symbolic Media of Interchange' in Social Systems and the Evolution of Action, Free Press, New York.) Parsons speaks of 'interchange' media. Media means the building of links between differentiated parts, each with a certain functional imperative. Parsons sees the properties and workings of money as
very far from being alienated from civil association. I will show that the laboratory as an enterprise, marked
by power and interest, is viable only insofar as it is conspicuously civil.

The laboratory will be examined as a special instance of enterprise association, with associates
who are interested in certain gains in reputation and ‘symbolic capital,’ that is, publicly acknowledged
gains of credibility, and therefore seek to achieve and maintain mutual understanding and shared horizons
in their work. However, as discussed in the thesis, despite a tendency to enclosure in spatial, intellectual and
other ways, the laboratory is not a hermetically sealed unit populated by associates with limited and local
interests. The work of the laboratory is meant for the domain where other scientists will assess the research
for its subscription to rules and norms. Research is incorporated into science only when there is a
provisional consensus on its subscription to procedure. These forms of procedure, as I understand them, are
somewhat less formalised than in Oakeshott’s depiction, with perhaps greater tacitness and flexibility.
Nevertheless, it is useful to imagine a relationship rather than an analogy between, on the one hand,
enterprise association and lex, and on the other, the laboratory and the contexts of legitimation of research.

The logic of the ‘symbolic market’ and the logic of civil legitimation are not separable as
Oakeshott conceives. I draw here on Karl Marx’s idea that those ‘who establish social relations’ in
conformity with their material power of production, also produce principles, laws and categories in

paradigmatic of the workings of other media, including power, influence, and value commitments. Interchange refers
to the twin aspects of exchange relationships and equivalence of exchange relationships between differentiated parts.
Media are characterised by ‘symbolization’ and ‘generalization’. The first refers to the exchange-value of money and
the symbolic use of language. The second term implies that every medium can affect several objects from various
contexts. Each medium is attached to a specific subsystem by norms. Parsons points out there are generalized
‘symbolic media’ associated with different social subsystems. Each medium facilitates the autonomous working of its
subsystem, while interpenetrating with other media, thereby achieving functional integration. Media can circulate
between actors in their respective subsystems and also beyond those boundaries. The ‘medium–system–functional
imperative’ relationships include money–economic subsystem–adaptation, power–political subsystem–goal attainment,
influence–societal subsystem–integration, and value commitments–fiduciary system–system maintenance. Media can
circulate between actors in their respective subsystems and also beyond those boundaries. Jürgen Habermas follows
the work of Parsons. Habermas makes a distinction between ‘steering’ and ‘communication’ media, in line with his
distinction between the goal-oriented workings of ‘system’ and the ‘communicative’ workings of the lifeworld.
Steering media include money and power. In contrast, influence and value-commitments are the media necessary for
the communicative understandings that sustain and reproduce the lifeworld. See Jürgen Habermas (1981) Lifeworld

24 I have adopted the use of this term from the work of Pierre Bourdieu, whose work has extensively discussed the
significance of the flows of capital between its economic and non-economic forms.
25 Pierre Bourdieu (1993) (Ed) Randal Johnson The Field of Cultural Production: Essays on Art and Literature,
conformity with their social relations. I will take liberties with Oakeshott’s concept of *lex*, which I shall read and use not as a system of law, unitary and uniform, but as a quasi-juridical context for the legitimation of research as a crucial step in its circulation in a symbolic economy. Further, I plan to combine the idea of *lex* with the notion of multiple fora of legitimacy, to speak of scientific *lex* spread and variably interpreted and enacted across many such fora. In this thesis, wherever I may use the term *lex*, I use it with the qualification that scientific *lex* is enacted and administered in dispersion across fora. The term is used as an abbreviation but with no intent to speak of it in association with any single particular centre of scientific legitimacy. Each of these fora of legitimation can be imagined as operating within a discursive field, ‘characterized by a shared vocabulary within which disputes can be organized...by mutually intelligible explanatory logics, by commonly accepted facts, by significant agreement on key...problems.’ In this regard, they can be conceived of as macro-actors akin to the laboratory conceived of as a micro-actor. *Lex* fora, which determine rational-legal authority in general, and the authority of scientific research in particular, are grounded in certain modes of ‘truth-telling’. These modes are characterised by distinctive procedures and rhetorical devices for making ‘veridical’ statements and for examining statements for their ‘veridicality’, to use Georges Canguilhem’s term for the quality of scientific discourse. These fora administer regimes of intelligibility and enunciation, who can speak with authority and who cannot, what counts as credible and therefore as rational knowledge and what does not.

**CROSS-BORDER MOVEMENTS OF RESEARCH**

One may investigate a seeming paradox: how is it that scientists, so actively interested in recognition and success, as individuals and in groups, can nevertheless win non-local recognition by

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30 Karl Marx (1847) *Poverty of Philosophy* MEGA 1, pp. 179-80 Although I shall draw on Marx’s and Marxists’ views at several points of this thesis, and although I regard Marx as a sociologist of knowledge par excellence, I do not hold with Marx’s notion that civil society is a Hobbesian nightmare of isolation, greed, and aggression, of individuals bound only by a cash nexus. Nor do I agree with formalistic separations of supposedly apolitical ‘enterprise association’ from political ‘civil association’, such as found in the liberal thought of Oakeshott. These separations are also implicit in the scientist’s claim of being ‘apolitical.’


Introduction

publicly disavowing their local enterprise interests? How is it that the local interests of the laboratory, and
the authority structures that are identifiable in it, are maintained by a presentation that is altogether civil,
disinterested and depolemicised? These questions are the concerns in Chapter 2. An associate in the domain
of research is also a producer in a complex and very specific economy. This economy is characterised as
much by the movement of knowledge, force, symbolic capital and credibility within and across boundaries
of local contexts, as by movements of money. The production of knowledge in the laboratory has a
reciprocal relation with the production and maintenance of credibility. Credibility and credible knowledge
are intersubjectively created and maintained. Relations of force are inextricably part of this intersubjective
process. The generation of credible and moveable, knowledge commences in a local, specific, and
restricted context, which characterises each laboratory in its being and doing. However, the knowledge so
produced is deemed knowledge only if it is accorded a non-local, transversal recognition and acceptance.

The laboratory experimentation, procedures and objects gain their identity and coherence through
the work of the associates. The local work of the scientist endows research objects with coherence and
identity. Procedures and experimental materials must first be rendered malleable, susceptible to being
worked upon in local conditions. At the same time, credible science must be testable, which in turns
requires that the experimental work be exportable and reproducible elsewhere. This emphasis on
reproducibility is tied to an emphasis on the achievement of transversality of the work done by the
laboratory. The sociology of scientific production could therefore attempt to understand the co-productive
and inter-translative movements of research activities and products. These may be seen as proceeding from
and towards the two termini in a reciprocal process of material production and symbolic recognition. At
one terminus, one may consider the laboratory, an ‘imperatively coordinated enterprise association’ for the
material production of the scientific work. At the other, one may consider the context for the legitimation of
the work, for the validation of belief and credibility in its value, the context for the ‘symbolic production’ of
the work. In art as in science, there are critical audiences, publishers, funding agencies, all of whom are
implicated in the production of objects then identified as works of art or science. The activities of all these
interactants animate and constitute the context of research legitimation, to which I shall refer as 'lex'. The _lex_ of science is constitutive of certain 'foregrounded practices organizing its normative institutions.' These foregrounded practices are linked to local practices, which are routinely rendered invisible through the process of legitimation and de-linked to the publicly visible practices.

I owe the idea of _lex_ to the work of Michael Oakeshott on civil association. However, I will be disrespectful of Oakeshott's legal-formalist refusal to see the domain of civility as anything other than its impersonal _lex_ strictures. While using the notion of _lex_ for a sociological study of research legitimation, I shall also polemicise it by inducting perspectives from the work of Bruno Latour, Pierre Bourdieu and Michel Foucault, among others. These perspectives inform my view that the relationship of the _lex_ and the laboratory is a case of 'antagonistic balance,' of adversarial collusions, of unceasing circulation and translation of actants and capital between two termini. One may think of a dialectical relationship between the laboratory and its context of legitimation, as in Michel de Certeau's use of the adjective: 'the sixteenth century sense of the movement of relations among different procedures on the same stage, and not in the sense of the power assigned to a particular place to totalise or "surmount" these differences.' This emphasis on inter-translative movements is not intended to provide a picture of a closed universe of research. It seeks to illustrate the restless movements between the 'enterprise association' and the 'civil association.' There are chronological _changes_ in the relationships created through these movements. There are elevations or drops in the reputation and symbolic capital of a laboratory. The shifts in the networks in which reputational gains or losses occur indicate the 'radical indeterminacy' of the actors involved, who are networked and therefore malleable.

Through movements and translations the laboratory takes form as a 'parochial' entity, with a specific physical existence and local practices, marked through and through with relations of force and interest in capital-maximization. At the other end, _lex_ is delocalised and despatialised. The symbolic

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34 Michel de Certeau (1988) p 43
products of the laboratory enter it, seeking credibility and legitimacy in transversal movements away from the parochial and local. The symbolic products arrive invested with perlocutionary resources to gain credibility and legitimacy for their locutionary statements. Thus lex thrives on the thoroughly interested efforts of enterprise associations to gain credibility for their 'disinterested' products. The ideology of civility and credibility is shored up in this collusive process.

Scientific research may be seen as meaningful intersubjective performances, based on intelligent, mutually communicable and negotiable subscription to procedures that administer the generation of credibility. The subscription to procedure will be viewed at two inter-related levels, within the 'internal' laboratory context of research and within the 'external' context of research legitimation. As mentioned above, the domain of legitimation of research, of the administration of the formal procedures that demand such rule-oriented subscription, will be termed lex, following the work of Michael Oakeshott. This chapter will focus on the ways in which research is invested with symbolic resources, preparatory to its export into the lex, for the investiture of credibility. The consonance between event and structure, research and lex, or between practical and theoretical reason in science is an effect that is achieved strategically and performatively. This achievement is dependent on the exercise of relations of force. These relations underlie, and are evident in the accomplishment of research performances and their eventual trial for facticity, credibility, and innocence in the rule-oriented contexts of legitimation, or as Bourdieu says, 'consecration'. These contexts can be traced 'outside' the confines of the laboratory. Knowledgeable audiences who are involved in the formal administration and allocation of credibility populate and animate these contexts.

The research of the laboratory also appears to be the focus of contestation between two types of orientation to two different audiences: first, to the requirements of other producers in the restricted market for symbolic goods, and second, to the demands of the external agencies who wish research to have some applicability, some exchange-value apart from the requirements of other producers in the restricted market for symbolic goods. Following Bourdieu, the first orientation may be termed 'autonomous' and the second,
'heteronomous'. In the autonomous orientation, the research is first put up for evaluation by peers, for examination in \textit{lex}. Success in this effort at transversalisation would imply that others might then use the work. It could then gain longevity through its ‘sighting’, local examination, and parochial adaptation and acknowledgement in the research of other laboratories. These are the intermediations that effect the ‘persistence of memory’ and a successful, because chronologically shallow, ageing of research through use; they effect the translation of ‘formalities’ to ‘modalities’.\textsuperscript{36} Thinking of parochialisation as a process of consumption, we may consider its poietic\textsuperscript{37} character also as ways of consuming the products imposed by a dominant economic order. It is not inappropriate to associate patterns of consumption with the resourceful use of intellectual property. The practices of consumption of published research can be linked to the making of credible knowledge, with the qualification that all such practices are identifiable only within networks of restricted and localised production and consumption. Acts of consumption are legitimised in the form of the published paper that has used the publications of others in the same or similar specialty areas. In this way, these acts are finally turned into positive judgements of the ‘fitness’ of the consumer’s judgements and choices.\textsuperscript{38} The consumption of symbolic goods has a power to shape the identities of the producers and the consumers. In this regard, citation indices and citatologies can be thought of as calculations of desire, production and consumption similar to those produced by market researchers. The indices of symbolic consumption are narrative forms that

\textsuperscript{36} Michel de Certeau (1988) p 29
\textsuperscript{37} See Henri Lefebvre (1968) \textit{The Sociology of Marx}. Allen Lane, London, pp 44-45. The author says, ‘Poiesis gives human form to the sensuous; it includes man’s relations with nature—his labors as a farmer, craftsman and artist—and more generally, the appropriation of nature by human beings, both of the nature external to themselves and that which is internal to themselves. Praxis comprises interhuman relationships, managerial activities, and the functions of the state as they come into being. In a broad sense, praxis subsumes \textit{poiesis}; in the strict sense, it only designates the \textit{pragmata}, the matters actually deliberated by the members of society.’ In \textit{The Production of Space} (Lefebvre, 1998), the author says that it is incorrect to say that the world contains anything that can be called natural and separate from the social. Synthesising these views, I would argue that the scientist’s engagement with material and symbolic instruments that supposedly open the way to insights about Nature, is always and indissociably an engagement with people and with political interventions. The crucial point is that scientific knowledge validates itself with a denial of its political investments, with defining such investments as burdens and hindrances to knowledge. Scientific \textit{poiesis} justifies itself through a dissociation from political interventions, from all elements of the praxis within which \textit{poiesis} is possible. These interventions structure and penetrate people and things in such a way that the dichotomous use of terms such as the ‘natural’ and ‘social’ is invalidated. At the same time, it is pointless to dwell on ideas of the nature that lies within or external to humans or other life-forms, since every attempt to understand and pin down aspects of the natural is an act that socializes what it would like to think of as natural.

establish a consonance between the transversal norms of scientific civility and virtue and the parochial interest and gratification of the laboratory. Moreover, if we see researchers as consumers of symbolic goods marked with names of their 'creator-owners', we can understand their compulsion to attend conferences, seminars, workshops, to invite speakers and visitors, to accept invitations to speak and lecture. We can contextualise their drive to 'give marking services and get marking services...to be present at other people's rituals of consumption to be able to circulate [their] own judgements of the fitness of the things used to celebrate the diverse occasions.'

Parochialisation can be viewed as a process analogous to the 'construction of individual sentences with an established vocabulary and syntax.' It can be likened to the relationship between langue and parole, protocol and strategy, rule and maxims. Consider the localised use of protocols in molecular biology. These are written instructions that spell out the proper use of apparatus, chemicals, and procedures in making technical preparations in experiment. There is a difference between the prescriptions and the practice of the protocol. The practice, that is, actually doing protocols, disseminating them from text to practical context, involves a degree of re-writing and adaptation to local conditions. These adaptations are the intertwinnings of transversal text and local know-how.

Taking the perspective of scientific researchers, we may say that scientists do their research primarily, but not exclusively, for the audience of 'peers,' the audience of antagonists-competitors-collaborators. Research publications, and the expectation of winning credibility, are oriented to this audience. The acts of production are affirmations of this goal; this goal is part of the inscriptions generated to realize it. In an important sense, this reliance on the judgment of peers encloses the system of research production, giving it an almost reflexive history. This reliance is a decisive influence on the relation between the producer and his work, and thereby also on the fates of the producer and the work. The researcher must define his career with reference to the transversal meaning of his research, a meaning that is negotiated and tentatively settled on via the play of associate-institution relations and interactions which

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39 Mary Douglas and Baron Isherwood (1996), p. 56
40 Michel de Certeau (1988) P xiii, italics in text
animate the economy of credibility, the play of positions on the ‘field of restricted production.’ In this play of positions and relations, the peers must exercise and revise not only their evaluations of other peers, but also try and take stock of themselves, of their shifting positions and fluctuating strengths, via the assessments that others impose on them. Borrowing from Mead’s idea of the emergence of the social self, ‘me’ and ‘us’ replace ‘I’ and ‘we’ in this process of stocktaking though the assessments of others.

The heteronomous orientation is to a non-peer audience. This audience is composed of the agencies that provide the funding for research, which is part of the scarce resources and instruments for research. The nature of the appeal and of the evaluation differs from that of the autonomous orientation. There is neither perfect exclusion nor perfect complementarity between heteronomy and autonomy; the two orientations are in a tilted complementarity rather than mutual exclusion. Heteronomy is stronger than autonomy, as scientific research is so finance-intensive that laboratories are not in a position to resist the demands of funding agencies beyond a point. It is not possible to precisely identify the points of resistance and yield. However, it is the case that applicability of research is a prime criterion for research funding. At some point external pressures on scientific research agendas generates compliance with heteronomous pressures. Spokespersons for science, such as Michael Polanyi, have vociferously condemned the post-World War state and industrial intervention in the choice and direction of research agendas. Polanyi says that the mesh of communicating neighbourhoods of scientists would suffice to ensure the integrity and smooth working of the ‘Republic of Science.’ This view of ‘Science for Science’s sake’, so similar to ‘Art for Art’s sake’ invokes the principle of autonomy of scientific production from the extra-scientific market. The de-realised scientific paper, which makes no mention of these mutually antagonistic pulls and pushes, makes a similar subtextual invocation of the principle of autonomy. In its effort to gain credibility in lex, it must be deemed to forego statements of opinion, theorem, purported statement of fact, doctrine, creed, dogma, and so on, which liberal thought holds to be incompatible with true civitas.

In this discussion, it is important to note that the boundaries between laboratory and _lex_, benchworkers and reviewers, contexts of peer-dependent autonomy and of non-peer-dependent heteronomy are highly permeable, shifting, and both transitory and historically determined. In the movements across the boundaries, and in the historical shifts in these boundaries, one sees struggles to maintain or shift them. These are struggles of on one side, orthodoxy and its 'power to consecrate certain producers and products' and on the other, heresy, views and works against the paradigm or legitimate view. They involve attempts to preserve or wrest a certain power to decide, the institutionalized discretion to authorize oneself or others as authors, or else to confer or divest researchers of the symbolic or material resources to create works for consecration or use.

The researchers move their productions through various stages from the 'internal' contexts of the laboratory to the 'external' contexts of legitimation. At each of these stages, there are performances and negotiations to invest the research work with credibility and reliability. These evaluations of research are imbued not only with an assessment of their status as propositional truths about existing states of affairs in the world, but also their normative rightness and expressive sincerity. Strategy, rhetorical and physical performance, and the audience-dominated application of procedure mediate the successful attachment of these different validity claims to research. However, it is only in the contexts of legitimation through peer review that research is accorded enough credibility to be incorporated, provisionally, in the body of useful resources for the future generation of other such concretions of credibility.

THE CONVERSION OF RESEARCH TO SCIENCE

Within the local context, the research is _worked on_ so as to enhance its non-local recognizability and acceptability. Latour and Woolgar's _Laboratory Life_ takes a pioneering constructivist approach to science. Their constructivist approach is that only by studying science-in-the-making, may we effectively deconstruct science, exposing the power of rhetoric in the making and presentation of whatever is called

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42 Jürgen Habermas (1981) _The Theory of Communicative Action: Reason and the Rationalization of Society_, Volume I, Heinemann, London. The author follows J.L. Austin in discussing different types of validity claims associated with statements, and the association of these claims and statements with types of action.

truth. The analysis claims to open up scientific ‘black boxes.’ The authors seek to ‘penetrate the mystique’ of science, in its crafting at the laboratory workbench, to give an exposé of the ‘internal workings of scientific activity.’ They present scientific knowledge as socially constructed by negotiation and the marshalling of material and social allies in defence of facts that are presented as ‘uncovered’ rather than made.

Such views pose a revolutionary challenge to the entire Western tradition of science. The extravagant deconstructionism in the work has come in for a good deal of criticism. The authors have been accused of adopting double-talk to evade criticism — for instance, when they say that their own text (like the science they describe) has no real meaning, being ‘an illusory, or at least, infinitely renegotiable concept’. They have been accused of over-generalising from stray instances of laboratory practice. Another charge is that they justify their complete ignorance about laboratory operations as a sort of cerebral hygiene and, worse, present it elsewhere as the result of deliberate and even malicious scientific esotericism. They have also been accused of turning their own incomprehension of the constitution of meaning and truth in science into an assertion of the inherent meaninglessness of science. The charge is that they see the work of science as the factory production of inherently meaningless ‘traces, spots, and paints’ because they themselves provide only a new way of knowing nothing at all. It would be wise then to ask what Latour and Woolgar’s work offers to the sociologist of science.

The thesis of ‘constructing’ facts permits a sensible reading according to which the theory or description of a natural phenomenon, are settled upon and ‘socially negotiated’ in a certain sense. Research is one mode of meaningful action. The process and outcomes of research hold meaning, for scientists at least. That should suffice for the sociologist. His interest is ultimately not to establish the truth, facticity, or meaning of the results of research. Projects to establish that laboratory phenomena are natural or just the products of elaborate social contrivances are both inherently dogmatic and rhetorical. Further, the sociologist need not take the stance that scientists concoct something inherently meaningless that is paraded as meaning. The sociology of science would be utterly trite if the inquisitorial procedures it should
problematize were to inform all its projects. It is always possible to mould facts to fit a theory, especially if one seeks to validate a theory of culpability. The selective appropriation that one may make from the constructivist sociology of science is recognition that social interventions give research its direction; science does not propel itself. The sociologist may start with identifying which lines of research are favoured and which are not. He could then investigate the sociological bases of the valuations that condition the formulation of research priorities. He could investigate the reasons for decisions regarding research priorities and the delegation of work within the laboratory. He could examine the bases of the power and the authority to take decisions and give commands. Science requires material and non-material resources to maintain its procedural coherence. The sociologist can investigate the pre-conditions for and effects of the relative availability of these resources. The focus on credibility-making also provides a strong purchase on understanding the making of scientists and the conjunction of their careers with the careers of their products. It will help me to trace the movement of knowledge, force, capital and credibility within and across the bounded local contexts of the laboratory, an ‘amalgam of arrangements and mechanisms — bonded through affinity, necessity, and historical coincidence.’ I recognize that the activities of the laboratory, like those of other groups, are both self- and outward-referential. In consequence, these activities are registers of ‘several levels of cultural complexity.’ A purist focus on either micro-features or macro-structures would be skewed and infructuous, given the synergy of these dimensions. Instead, I will examine the processes by which events at micro-levels are converted into statements with macro-level credibility and significance. I will speak about the construction of credibility through the deployment of strategies and resources in the laboratory. I will discuss how scientists are engaged in the pursuit of attributes of being believed in and being accredited, and thereby also in the pursuit of the formal authority and under-articulated influence that flow from credibility. My focus is on the production of cognitive resources that are deployed to gain belief in knowledge and knowledge-producers. I shift my attention to

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the processes by which legitimation is achieved by a careful forgetting of epistemic production and a shift of focus to finished works of knowledge.46

My view of science as characterised by an economy of credibility follows the work of Thomas Kuhn and is therefore at odds with the Popperian idea of science as an economy of doubt. Doubt, scepticism and scrutiny do not enjoy a monolithic strength in science. The suspension of disbelief supports the credibility invested in the ‘paradigms’ of Science, and in the ‘micraparadigms’ that govern the research pursuits of a particular laboratory. Although research findings are subjected to intensive evaluation and scrutiny, the scepticism that drives scrutiny is suspended in the case of the canonical concepts of research. In the case of a biological laboratory such canonical elements include the double-helix of DNA, the idea that genes code for proteins, and the idea that the organismic functions can be seen as the result of molecular interactions. They are ‘black-boxed’, as things that have become a matter of indifference and are no longer to be interrogated.47

In choosing a bioscience laboratory as the source of insights about the making of credible knowledge, I do not aim to impose a field-specific view on all of science. As the title of the thesis indicates, my aim is to identify certain aspects, possibilities, and movements in science. There may be a blurring of margins between research specialities, between orientations to subject matter, between principal fields such as physics or chemistry or biology. Therefore, my study may identify features in one area that may be identified, in varying degrees of conspicuity, elsewhere. I would also qualify the term ‘biological’ because in many areas of research such as biophysics or neurochemistry or

46 See Henri Lefebvre (1998) The author draws a distinction between the dynamism of acts of ‘production’, and the ossification of these acts into ‘works.’ Similarly in Lefebvre (1968): ‘Praxis, result of a division in the body social, is in turn divided. Productive labour (agriculture, crafts, and later industry) is devalued in relation to creative activity proper or, more accurately, the activity that is alone regarded as creative, namely, that which an individual pursues in producing a work. The thing, the product, and the work come to be distinguished from one another.’ (p. 45)

47 For a significant discussion of ‘black boxing’, see Bruno Latour and Steve Woolgar (1979). According to Latour and Woolgar, rhetoric plays an important part in deciding the acceptance or rejection of observations and theories. The settlement of scientific debates is about the victory of a viewpoint through the extraction of compliance and not about the discovery of truth. The proponent of a particular theory tries to rally as much support as possible. Technical arguments are mobilised so as to blunt the force of counter-arguments and falsificatory attempts. Theories invoke black boxes in their defence: a resistant theory may eventually become a black box itself. A feedback loop emerges between the invocation and the generation of black boxes.
Drosophila genetics, the work can only very roughly be dubbed 'biological'. In their work, they use proteins or extracts of an animal origin or breed fruitflies. However, I would say that the bench-preparation of these materials, the advanced research tools and conceptual schemes subsequently brought to bear on them, and in sum, the positioning and perspective of the work in these areas situates them sufficiently close to or right within the camps of physics and chemistry. The contemporary 'biological sciences' are extremely physico-chemical. On the surface, CCL looks no different from the biophysics or the membrane-ion-channel laboratory at the Institute. I saw machinery and procedures to subject 'living substances' to physico-chemical transformations and analyses.

At the same time, there is a marked propensity to achieve distinction through the growth of exclusivism between specialty areas enclosed within similar research areas, which Whitley defines as 'collectivities based on some degree of commitment to a set of research practices and techniques.'48 Exclusivism also exists between different research areas. This means that there is a constant endeavour to establish and maintain boundaries around and coherence within research areas and even within specialty groups in each area. Chapters 3 and 4 focus on the parochialism and boundary-work in research communities and in the laboratory.

Parochialisation and the curtailment of universal scientific fraternitas are co-productive with what Gieryn calls 'boundary work.'49 Boundary work involves the self-presentation of a scientific community in different ways to different audiences. This self-presentation is linked to the cohesion and integrity of the community. Boundary work and communitarian cohesion are also co-productive with the transversal borrowings and parochial consumptions of epistemic products. All these aspects of cohesion are intimately associated with a perception of threat from 'rival' areas and perspectives, to present each area and perspective as of relatively greater importance. A perception that emerges is of discontinuous and porous inter-community boundaries, such as may be associated with neighbourhoods, with their aspects of

48 Whitley (1976) p 472
parochial clustering, insularity and rejection of strangeness beyond a certain radius of interaction and exchange. Such neighbouroods are quite different from the catholic embrace, the grandly encompassing reach of Polanyi’s picture of the Republic of Science.

In closing, I would clarify that the thesis uses masculine pronouns on occasion but strictly in a generic sense, although of course even this is suspect. Although I do not focus intensively on the import of gender, religion or caste in the orderings of power in the laboratory, I remain aware that these are powerful cultural influences on interactions, memberships, and activities of laboratories. Even the scientist’s denial of their effects or presence in work is a reiteration of the politics by which certain deletions and absences must be constantly ensured, and strategically concealed or made conspicuous in the making of credible knowledge.
I. ENTERPRISE ASSOCIATION IN SCIENTIFIC RESEARCH

INTRODUCTION

The laboratory is a site for imperatively co-ordinated enterprise association.\(^1\) Super-ordinates and subordinates, who draw on an array of epistemic resources for constructing credible knowledge in a restricted space, animate and sustain this site. Relations of force penetrate and structure the modes of association and the performance of research in the laboratory. In the following discussion, my use of the concept of ‘force’ will draw on its varied connotations, each of which has a specific situational grounding and manifestation. I shall occasionally refer to the superordinate researchers as actors and the subordinates as agents. It must be clarified that these distinctions are entirely relative. Each actor may be an agent and vice versa, depending on situation. Thus actor-agent relationships in the laboratory are not fixed and formalised in a rigid sense. These relationships are subject to situational redefinition. The concept of force encapsulates a range of means by which an ‘actor’ can exert a situational and positional influence on the actions of another, less forceful ‘agent’. The relationship of the actor to the agent is that of superordinate to subordinate. The actor is not an agent insofar as he can exercise force and is entrusted with a level of autonomy and discretion, which the ‘agent’ cannot contest or similarly exercise.

While actor-agent relationships between co-workers are not peculiar to science, and may certainly be identified in other professions such as medicine, law, and government, they gain a specific character in science. In the laboratory, the associate who is the actor is accorded a comparatively higher level of authorisation and autonomy to re-imagine and extend the possibilities of scientific benchwork and discourse. Their subordinates have comparatively less autonomy in this regard. Drawing from Lévi-Strauss,\(^2\) the actor is an *engineer* who can draw from the already existent, but also has the privilege, endowed by the possession of symbolic capital, to exceed the limitations imposed by it. Although the actor must operate within disciplinary norms, he can partially re-imagine those norms and extend a discursive

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\(^1\) This is the translation of Weber's term *Herrschaftsverband* to denote the domination of groups.
domain. He can change the theoretical world instead of merely recycling previously available meanings. In contrast to the engineer-actor, the agent may be thought of as a *bricoleur*. The French verb, *'bricoler,'* refers to the kind of activities that are performed by a handy man. The *'bricoleur'* performs his tasks with materials and tools that are at hand, from 'odds and ends.' His activities of the present are an accretion of the past. The engineer or scientist, according to Levi-Strauss, seeks to transcend conventional wisdom: 'The scientist creating events (changing the world) by means of structures and the *'bricoleur'* creating structures by means of events'. The *'bricoleur'-agent* deals in signs; hence his activity only reconfigures previously available meanings. The engineer-actor deals in concepts that open up new possibilities of meaning.

The actor's means of exerting influence on an agent may have been made explicit in official regulations, and can be viewed as synonymous with authority. The means of influence may also be seen to be unofficial, impromptu, tacit, or even violent. In this case, I shall refer to it as power. The interest that guides the exercise of power is the extraction of compliance. Violence implies coercion and this is particularly observable in situations where a crisis is felt to be real or impending. In these situations of crisis, the exercise of violence significantly divests the weaker performant of subjecthood or independent action. The unofficial exercise of *'power'* is not seen as illegitimate in the context of research. The exercise of power in ways that are not officially stipulated is possible in the laboratory because of the trust, the charisma deriving from credibility, invested in the persons who are authorised in impromptu manner to exercise such power.¹ Such power is sub-textual, and weakly or not at all mediated by rules. However, it is not thereby purely personal or clearly illegitimate. The laboratory associates see these tacit exercises of power as appropriate to meet the constantly varying requirements of experimental research.² Within the

¹ Steven Shapin (1994) *A Social History of Truth: Civility and Science in 17th Century England,* University of Chicago Press, Chicago. Shapin makes the interesting point in this work of historical sociology that science is still characterised by a powerful element of personalized trust between fellow researchers, an aspect that is complementary to impersonal systems trust. In this he presents an alternative to the Mertonian viewpoint that local patterns of trust and recognition have yielded to the dominance of structures of impersonal credibility and the authority of experts with institutionally validated credentials.

laboratory, the situational achievement of meaning in the mutually-oriented research actions and performances is not regulated by the invarying application of official codes. Actors in laboratories seek several types of solution to grapple with the risks and task uncertainties of research. As Michel Callon remarks, these uncertainties concern states of the world, on the nature of possible actions and the consequences of these actions. The solutions involve flexibility in the negotiation of events and arrangements. Flexibility characterizes the laboratory situation, which is marked by a paucity of explicit, rule-bound procedures of the bureaucratic sort. The flow of information is conditioned by the sharing of routines and conventions. These routines are subtextual, and as such they demand interpretative labour and a sort of bounded intersubjectivity. Interpretation is achieved in the course of interactions and conversations. Just as frequently, it is achieved in a tacit manner, with a minimum of explication or elaboration from the participants.

Trust, obedience, and authority in the subjective expectations between actors-agents, superordinates-subordinates are co-productive with the continuity and community of the associates’ shared horizon of understanding. Experience is nothing without the intersubjectivity that makes it cognizable and re-cognizable as experience. Thus experience is interpreted only in the light of shared understanding; the steadiness and consistency of understandings comes from the transmission and preservation of traditional schemes of thought. As Parsons observes,

'There is double contingency inherent in interaction...communication which is the precondition of cultural patterns could not exist without both generalization from the particularity of specific situations (which are never identical for ego and alter) and stability of meaning which can only be assured by 'conventions' observed by both parties.'

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5 See Michel Callon (1999) p. 185
Thus one may say that the perception of evidence and causal connections is the result of a practical mastery. The researcher is trained to inculcate certain perceptual schemes. He learns to perceive and interpret phenomena in terms of the categories and objectives of his research area and even more importantly, his specialty area. This derives in part from transmission, reception and re-activation of customary practices, the reception of tradition and authority and accession to seniority. Practical mastery and the reasoning therefrom set up the subjective expectation and perception of what will or might come next in interactional sequences.

THE EPONYMOUS ASSOCIATION OF THE PI AND THE LABORATORY

In the laboratory the official director of the research group is the prime integrating element. The PI is institutionally endowed with a ‘super-individual’ power, a reputation, and power of decision, which would never flow from his unconnected individuality. In this regard, we may take note here of the significance of eponymy, the phenomenon of eponymous identification of junior researchers and an entire laboratory with a PI. This is institutionalized at the Institute. I observed that the laboratory associates are identified not by their common area of research, but by ‘whose’ lab they are in. While I was conversing with Vandana, a research assistant, sitting in a room full of expensive machinery such as microarray gene analyzers, she said, ‘Latin is the one who owns all this.’ This remark is representative of the general acceptance of an eponymous association of PIs and ‘their’ laboratory resources.

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7 See Jerome Ravetz (1971) p.115
9 See Marcel Mauss (1979) ‘A Category of the Human Mind: The Notion of Person, the Notion of Self’ in Sociology and Psychology: Essays. Routledge and Kegan Paul. London. pp 59-89 A reading of Marcel Mauss on names provides the insight that names and naming are as important, though differently created and protected, in the ‘ultramodern’ domain of the laboratory as among the Zuni, the Chinese, or the Romans. The Roman cognomen, and the Chinese ming, were names treated as conferred on privileged individuals. The possession of a name was the privilege of some persons, who were not legally defined as individuals equal to other individuals. In Roman society the citizen, unlike the slave, had a nomen, praenomen, or cognomen, which was attributed to him by the personal laws of the gens. Under the personal character of the law that placed patrician over plebeian, and citizen over the slave, slaves could not have a cognomen, possessive rights over his body, or a persona. Only a usurper would adopt a cognomen that had not been legitimately attributed to him. In modern society the name of each individual is protected as his or her intrinsic and inalienable possession. This name is not defined as
The PI's name is a collective noun for the working group of the laboratory. Although the researcher is an individual, during his apprenticeship, he is tied to a name that has acquired a public estimation and weight that his own name is yet to gain. However, although the PI and the apprentice researcher have differently valued names, each must compulsorily append his or her signature to documents that are to be scrutinised for their legitimacy. Thus, the signatories give their consent to scrutiny of symbolic goods to which they stake a proprietary claim. Once they have signed, they cannot evade scrutiny for the legitimacy of the goods that they set in circulation and with which they desire a publicly validated eponymous association. Thus, the name in modern society is related to an idea of legitimate membership in a collective of formally free and equal individuals. The individual is regarded as having a claim on his or her 'own' name. The individualist ownership of the name is collectively protected. This protection is enunciated and enacted in terms that valorise the rightful individual possession of names and censure misappropriations of these names by others.

It is interesting that eponymy is made possible by the production of texts that are supposed not to be authored at all, that is, works of truth and anonymity, wholly unintentional and cleansed of an author's or inventor's desire. Names are appended to these works, and this appendage is seen either as a convenient appellation for the theorem, syndrome etc. or else as a mark of ownership of the discovered effect. Ownership is then held to be other than authorship of an effect-as-invention. The author of the scientific linked to a persona in the same sense as the cognomen or the ming. It is ineluctably united in a non-personal legal sense with a person. The legality of the name is inescapably affixed to individuals and their works. In legal terms, it cannot be situationally detached or appended according to personal whim. This is because the name is a means to ensure the public accountability of actions.

\[\text{See Friedrich Nietzsche (1954) The Gay Science in Ed Karl Schlechta Werke Carl Hanser Verlag, Munich, Volume 2, pp77-78.}\]

Nietzsche speaks of the association between things and the names they are given; 'unspeakably more depends on what things are called than on what they are. The reputation, the name and appearance...each being in origin most frequently an error and arbitrariness, thrown over things like a garment...have gradually by belief therein and growth from generation to generation, so to speak grown on to things and into things and have become its very body. The names 'J.D. Watson' and 'Francis Crick' are eponymously associated with a thing called DNA. Rhetoric, such as in Watson's triumphal memoir The Double Helix, and power distributions in science have enhanced the strength of this association. These enhancements are allied with the weakening of Maurice Wilkins' association with DNA and the near erasure of Rosalind Franklin from public record of the discovery of DNA structure. Anne Sayre provides a trenchant critique of Watson's belittlement of Franklin and his deliberate lack of acknowledgement of the significance of her crystallographic work in the DNA breakthrough. \(\text{(Anne Sayre (2000) Rosalind Franklin and DNA, W.W. Norton and Company New York.)}\)
paper is a revealer. He is supposedly solitary and separated from his epoch. He is supposedly alone and away from the field of positions and functions that make authorial acts, identities, and functions possible. The point is that this field makes scientific author-functions and the non-authorial author simultaneous and co-productive possibilities. The 'intellectual property' of the scientist is not alienable from him, in the sense that commercial property is alienable. The research work is owned but the ownership is of a sort that is on the one hand denied, or on the other stated firmly but *sotto voce*, in a way that strengthens and perpetuates it. The researcher’s eponymous association with ‘his’ research is accorded a fiduciary estimation of being disinterested, unintentional, non-interventionist, ‘non-authorial’ and not privately owned.

Researchers demand, commandeer, request or draw on the skills and work of others, present and absent, in their daily work. Within the laboratory, however, researchers have a highly proprietorial attitude to their work, which strengthens the individual-oriented identification of most intellectual property. This attitude is quite marked in the case of authorship. The graduate researchers are convinced that they will and should also eventually be recognized as primary owners of intellectual property, the ‘senior authors’ of ‘their’ research paper, when they are seen to be its senior authors. Citation of the work of others is conjoint with projects that alter work in parochial use. But citations are not a mark of transferred or alienated ownership of the cited work. They are a form of invoking credible witness. Successful citations are those that can be read in works deemed credible and publishable. These citations are a successful reciprocation of the invocations of other research. They are so because they attest afresh to the eponymous association between the invoked research and the researcher. This everyday, routine eponymous identification of associates as members of a ‘quasi-owned’ unit is strengthened by the institutional insistence that the PI validate the smallest actions of the ‘member’ of ‘his’ laboratory. Thus, the researcher cannot get photocopies and print-outs of journals without the PI’s signature, which is also necessary for getting an identity card, a library membership, a hostel room, a fellowship renewal, an Institute web-mail account.

The notion of science as driven by individual efforts is not at odds with the collective dimension in research. PI-laboratory eponymy can be understood as operating through the co-adaptation of an imaginary
of individual genius\footnote{Sec Chandra Mukerji (1996) ‘The Collective Construction of Scientific Genius’ in (Eds) Y. Engestrom and D. Middleton Cognition and Communication at Work. Cambridge University Press. Cambridge. pp 257-278. The author suggests that the chief scientist has assumed much importance as a figure. She relates this to the Euro-American privileging of the achievement of the individual mind. As a result, collective labour in research is concealed through the attributions of individual authorship.} and the collective authority-laden performances of any experimental research done today. Research financing on the basis of credibility and reputation, systems of accountability, the reputations congealed in scientific publications and positions, the eponymous identification of the laboratory with the PI, are all part of this co-adaptation of collectivist organization with the persistent cult of the free, individual scientist. Thus it would not be correct to say that this cult is a mere survival from the older ethos of science. The ethos has taken on a new and potent effectivity in a rational organization of research effort, wherein the name is a mark of credibility and a means of audit and accountability. The collective trust in the notion of individual creativity secures the power of that notion to obscure the significance of the collective construction of knowledge. At the same time, this obscuring firmly locks the researchers into a collective system of accountability on the basis of names, a system of governance on the basis of signatures and signed consent. Eponymous identifications are integrated with techniques of research, with the modes of control of objects and of researchers, with the etiquette and rules of parochialisation and transversalisation. This integration sustains and enforces credibility and civility in scientific research and legitimation.

The culture of civility and credibility, of ‘gentlemanly’ science, and the accountability that is of crucial importance in bureaucracy are adapted to each other. The ideology of civility is effectively part of a system wherein the means of ascertaining, policing, allocating or withdrawing credibility are of prime importance. Credibility, eponymy and civility are not mere masks; they can be regarded as practically effective conditions of existence. Eponymy and the notion of individual knowledge-creation also actually strengthen the authoritarian elements in the organization of relations among researchers, the relations that inform their scientific work. The discretionary exercise of power characterizes a relationship of power-asymmetry, in which the superordinate’s creative and decisive ability and right are accepted and
perpetuated in a tacit, non-procedural way. This is the principal aspect of the relationships between the actors and agents in the laboratory.

**CIVITAS IN RESEARCH**

In the laboratory, all those features of research are visible that are disguised and invisible in the research product that is to be exported out for legitimation. Self-interest, to gain recognition or to protect it, and the ever-present interest in gaining recognition so as to translate it into financial capital inputs for further research are absent from the published research. Scientific research presents a form of economic interest that is denied in *lex*. It would be appropriate to discuss the aspects of this ideologically-driven denial, taking Michael Oakeshott’s work as the primary source of insights. Oakeshott defines civil association in a series of negatives as:

‘This is an ideal character...not an association of ascertainable persons identifiable in respect of a place and a time, by the signs it uses to recognize itself, or specifiable in terms of common beliefs or of its own rules and arrangements. It is a certain mode of association...it necessarily excludes relationships contradictory of itself...but while it is distinct from relationships contrary to itself, it does not exclude persons who enjoy those relationships. I shall call it the relationship of civility.’

The civil association, *civitas*, is a ‘relationship of equals.’ It has no instrumental purpose, and cannot be a means for anything beyond itself. Its most salient feature is a ‘system of rules’. Oakeshott provides a discussion on the ‘postulates’ of civil association. Free intelligent agents, *cives*, engage in acts of self-disclosure and self-enactment, orienting themselves to the responses of other such agents, ‘while subscribing to...a language of moral understanding and intercourse.’ The engagement of agents in civil association is an act of intelligent and free choice. *Civitas* is entirely different from the ‘intermittent

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13 Michael Oakeshott (1975) p. 112
transactional association\textsuperscript{14} of bargainers who seek individual material satisfactions from each other and from the more durable relationships of ‘enterprise association’ where ‘agents may be related in the joint pursuit of some imagined and wished for common satisfaction...some substantive condition of things to be jointly procured.’\textsuperscript{15} Enterprise association involves the management of actions that are means to the common purpose.

\textit{Civitas} is also distinguished from relationship in terms of biology, kinship, or membership. Thus, civil association is severed from factors of ascription. The rules of \textit{civitas} do not specify particular performances; they are not recipes for concrete actions. They are ‘moral considerations’ that specify certain conditions to be subscribed to in choosing performances. The rules and considerations are indifferent to the outcome of performances. The system of law associated with \textit{civitas}, called \textit{lex}, is not malleable to the substantive wishes and purposes of interested enterprisers. At most, the actions of \textit{cives} may be such as to be included within the range of meaning ‘tolerated’ by \textit{lex}. In this regard, \textit{cives} are associated not as enterprisers with divergent interests and purposes, but as shaping their actions in terms of ‘a practice or language of civil intercourse which they have not designed or chosen but within the jurisdiction of which they recognize themselves to fall and which, in subscribing to it, they continuously explore and reconstitute.’\textsuperscript{16}

Ruling a \textit{civitas} is different from enterprise management. As civil existence is a condition, it is different from the choice to be transiently associated with others in transactions and bargains, or more durably in enterprise association. The exercise of \textit{majestas} must be left behind when the ruler wishes to enter ‘politics,’ or deliberations on \textit{lex}. ‘Politics’ is not the deliberation of enterprise policy for managerial purposes. The ‘subjects’ are not personal retainers; indeed they cannot be regarded as role performers in any sense. Public is distinguished from private and both are to be understood solely as relationships, not as references to physical place or actual person or actual engagement and performance. Thus ‘public’ refers to

\textsuperscript{14} Michael Oakeshott (1975) p. 112
\textsuperscript{15} Michael Oakeshott (1975) p.114
\textsuperscript{16} Michael Oakeshott (1975) p. 183
performances and ownership that are in subscription to the condition of civility. ‘Private’ includes all the proprietary desires, decisions, actions, property and so on. However, it is important to note that public and private are never mutually exclusive. Nor do these two domains completely annex each other. There is a deep and lasting tension between them. This tension is co-productive with the maintenance of porous, shifting, and constantly worked-on boundaries between them. The tension between them defines them in collusion and opposition to each other.

Having summarized Oakeshott’s views on civil association, transient bargaining, and enterprise association, I would like to discuss how this approach may be adapted to understand the ‘Janus-faced’ nature of the laboratory, as enterprise association that can only continue as such if its work is publicly found to be in subscription with the requirements of scientific civility. The moral economy of credibility and civility encourages non-locality. The lex, which supports scientific civitas, also supports those material and cognitive arrangements that generate a level of transversality in science.

According to Oakeshott, scientific inquiry is an instance of a practice, ‘a set of conditions which qualify performances.’\textsuperscript{17} Practice is a language of self-disclosure and self-enactment, just as are the languages of the law and scientific inquiry. As a language we may think of it as something that does not ‘fix’ all possible utterances; the langue does not choke parole. It serves as a fund of ‘transversal’ considerations in making particular, parochial utterances.\textsuperscript{18} Learning the language and using it go together. In playing the game, we are constantly acting within and animating ‘historical configurations of customary practices, group memberships, cultural patterns of interpretation, forms of socialization, competences, attitudes, and so forth.’\textsuperscript{19} The conditions of a practice may be ‘somewhat indefinite uses or customs.

\textsuperscript{17} Michael Oakeshott (1975) p.120
\textsuperscript{18} The difference between the transversal representations and the local understandings and usages can be considered as akin to the distinction between ‘Darstellungen’ and ‘Vorstellungen.’ Darstellen could be used to describe the public, ‘theatrical’ demonstration of phenomena and vorstellen for the local representations, the tacit dimension in scientific research. See Stephen Toulmin (1972) p.195-197
general maxims of conduct, or slightly more determinate rules and regulations. The considerations of a practice are conditions understood by agents in acting and choosing performances. Subscriptions to the conditions of a practice are intelligent activity. If scientific inquiry is an instance of a practice characterized by a language of moral intercourse, it should be acceptable to recognize that the laboratory, where science is actually done and which is undeniably enterprise association, also works with this language of moral intercourse. Conversely, the interests of the enterprise and the exercise of power within it cannot be separated from the speaking of that language.

When looking at science in its lex and enterprise avatars it would be advisable to recognize the identity of the interests in each of these spheres. The characterization of science as non-enterprise practice, as moral intercourse divorced from narrow interest, calculation and power, is a normative depiction for which RK Merton has been repeatedly excoriated. However, it does not help to go the other way and insist that public ethos and the institutions of credibility are subservient to production and exchange. From the work of Max Weber, we have a convincing riposte to economic determinist perspectives on capitalism. His analysis of selected aspects of beginnings of rational capitalism demonstrates that the ethical codes of 'worldly Protestant asceticism' were of immense significance in the making and re-investment of profit, for fulfilling one's vocation, and for maximizing one's success in this pursuit, without a view to hedonistic enjoyment.  

Oakeshott says that the language of a practice is not only an art, tacit and learned through use, but also the terms of a relationship. In this regard, the credibility which is required by lex and which is a condition of existence in research production brings about a relationship between the enterprisers, a relationship that is tied to their enterprise pursuits, but also goes beyond it, as a 'common recognition of uses and considerations that are to be intelligently subscribed to in self-chosen performances. The consideration of credibility is not as of a substantive means or an end. Credibility is a condition of

22 Michael Oakeshott (1975) p121
existence, and mediates in both the lex and the enterprise association of science. It is present as a means and an end, as something that conditions other means and ends, and may be 'indexed' through them, but is not always identifiable in itself as a concrete articulated means or an end. One does not do or perform credibility itself, but one considers it, and this consideration forms a means in the work to be published; the credibility reposed in the work in lex leads to its publication; publication is a means for the winning of credibility; the credibility embodied in the published work is used to win credibility and therefore also funding for more research, and so on. As discussed above, in the interpersonal relationships of the laboratory, this institutionally accorded credibility also translates into the institutional charisma of authors, and supports the disposition to obey them on trust, that is, without interrogating the basis of obedience. In this sense, there is a difference, however tenuous, between credibility, which must undergo and survive scrutiny, and trust, which derives from credibility but evades scrutiny. Trust is multi-layered. It is linked to particularistic attachments of friendship and loyalty, to issues of similarity in behaviour, culture, and values. It is also tied to exchanges with colleagues and collaborators, and premised on a degree of stability and dependability in mutual professional expectations. These aspects of trust are not readily separable. 

Trust implies the sharing of taken-for-granted beliefs. This sharing is not open to scrutiny and is not captured by legal transactions. It is supported by normative infrastructures and it creates commonalities of socialization. Although it facilitates instrumental actions, it cannot be reduced to the performance of these actions.

Cives are related in terms of a practice. Let me advance the argument that the enterprise associates of the laboratory are to be considered also as speaking the language and engaging in the practice of cives. Their practice, grounded in considerations of credibility, is characterized both by 'a procedure composed of rules and uses instrumental... for procuring a certain sort of substantive satisfaction or in the pursuit of a common purpose' and 'a set of conditions to be subscribed to in any or all of an agent's actions or utterances, which has no extrinsic purpose and is not related to procuring any substantive satisfaction or to

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23 See R. Putnam (2000) Bowling Alone: The Collapse and Renewal of American Community. Simon and Schuster, New York. Putnam has proposed the distinction between 'thick' particularistic trust based on highly personal relations and 'thin' trust or social trust, based on daily contacts and professional networks. Thin trust has less 'moral density' than thick trust. It is based less on face-to-face contact than on a notion of significant professional others. But it is best to remember that these differences between the two types of trust are very fragile.
the pursuit or achievement of any substantive purpose. Credibility, as a set of considerations for scientific practice, grounds the procedures that win rewards, and it is also a reward and a means to other rewards. The enterprise practices and the civil practices are linked together. The importance of credibility as a condition is that it is not an extrinsic substantive satisfaction in itself, although its incarnations are found visible in publications. Credibility is symbolic capital, or 'prestige' or 'authority', public legitimacy for symbolic goods. In science as in other cultural production, economic capital cannot win legitimacy directly and on its own; it must be first converted into symbolic goods. But credibility is linked to recognition, which is a 'substantive satisfaction', and to funding, which is a definite 'extrinsic purpose', which is needed for the whole process to start again.

The observances in the laboratory are underpinned by considerations of credibility, which are central to the generation of knowledge that can pass the adjudicative procedures of lex. These observances are not purely local. For instance, their implication in the production of research is complemented by their implication in the examination of research before it leaves the laboratory for the most underrated departmental seminar or as a submission to the most high impact factor specialist journal. These observances are the local, observable, and negotiable complements of the same observances of adjudication in the lex. The adjudicative observances in the laboratory and beyond it decide 'what has been said or done on a particular occasion and where the responsibility for it lies and to determine its relation to the conditions prescribed in lex.' The importance attached to credibility and equity inform laboratory and lex observances. They are also very similar in that they are generally not explicit specifications for concrete performances. Consider the clauses and phrasing of the brief 'Honour Code' of the Institute.

[Institute] operates on a principal [sic] of trust. Most research materials are kept in accessible locations. Research at [Institute] depends critically on colleagues being able to

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25 Michael Oakeshott (1975) p 131
trust each other’s data. Hence any form of cheating or falsification of data is totally unacceptable and will be dealt with harshly.\(^{26}\)

This Code resembles Oakeshott’s rule of civil association in that it prescribes a norm of conduct, ‘an abstract consideration proper to be subscribed to in choosing performances’ but it cannot itself be ‘obeyed or performed.’ It specifies no performances; in speaking for trust between the laboratory associates and in hinting at harsh punishments for transgressors, it never says just how cheating can be avoided. The laconic phrasing indicates that it is meant for adepts, for those who can lay claim to a ‘practical mastery’ of the performances that would be in subscription to the ethic or not. A tacit understanding and actions guided by experienced intuition, of the sort Polanyi associates with scientific practice, are presupposed by the Code. It is not directed to particular respondents; it exempts no one and is not debatable. How may we understand the significance of this impersonal statement of fiduciary prescriptions issued by an example of enterprise association? We can do so only if we grant that these enterprise relationships are guided by civility, of which impersonal trust, or rather credibility, forms an important component.

Oakeshott says that although civil relationship is fiduciary, it is not characterized by the personal relationships of friends. According to Sudipta Kaviraj, such impersonal trust is the skill or ‘social aptitude’ needed to effectively run bureaucratic institutions. ‘Despite its impermanence, its mutability, and its non-universality, the life of the civil society also requires skills in the development of trust — but of a kind very different from the family: to use a later term, the trust among strangers.’\(^{27}\) The work of Talcott Parsons also

\(^{26}\) From the Handbook of Guidelines of the Institute of field study. Compare with the definitions of ‘misconduct’ in these statements: (a) Department of Health and Human Services: “Misconduct” or “Misconduct in Science” means fabrication, falsification, plagiarism or other practices that seriously deviate from those that are commonly accepted within the scientific community for proposing, conducting or reporting research. It does not include honest error or honest differences in interpretations or judgements of data. (Federal Register 54:32446-32451, August 8, 1989) (b) National Science Foundation: ‘Misconduct means fabrication, falsification, plagiarism, or other serious deviation from accepted practices in preparing, carrying out, or reporting results from activities funded by NSF, or retaliation of any kind against a person who reported or provided information about suspected or alleged misconduct and who has not acted in bad faith.’ (Federal Register 56:22286-22290, May 14, 1991) These definitions of plagiarism and falsification, and the vaguely worded strictures against such wrongdoing are part of the discursive array that safeguards the relationship of name, researcher, and intellectual property. They are integral to the institutionalization, protection, and sustained animation of eponymous associations in science. These definitions and strictures demarcate ‘licit’ and ‘illicit’ domains in the performance and transversalisation of research.

tells us that that value commitments are associated with a fiduciary subsystem of society. Value commitments interpenetrate with power (political system) and influence (societal system). A reading of Weber highlights the association of impersonal trust with ascetic Protestantism.

‘Pessimistic’ and ‘disillusioned’ Puritanism was characterised by a ‘most extreme form of ... exclusive trust in God...It comes out for instance in the strikingly frequent repetition, especially in the English Puritan literature, of warnings against any trust in the aid of friendship of men...only God should be your confidant.’

Trust is linked to the Being with the greatest powers and the least interest in men’s matters of selfish calculation. Trust in this Being was the ‘power of religious asceticism’, which produced ‘sober conscientious and unusually industrious workmen, who clung to their work as to a life purpose willed by God.’ And one sees also the link between impersonal trust and accountability: ‘Man is only a trustee of the goods which have come to him through God’s grace. He must...give an account of every penny entrusted to him....’

According to Oakeshott, the rules of enterprise association are merely promotive of its purposes and do not constitute it. Thus the enterprise can only be defined in terms of its common purpose: any charters it has are to be judged by their capacity to promote or to hinder the common purpose of the association. Although the association may insist that the rules of enterprise charters be recognized, although offenders have to resign membership, the observance of the charters is not itself the purpose of the enterprise. I would argue instead that while the observance of a charter such as the Honour Code is not the stated objective of any of the laboratories at the Institute, the values stated in the Code strongly condition the stated objectives, such as the investigation of disregulated cellular signalling in cervical cancer, or else the neurobiology of learning and memory, or the genetic analysis of chemosensory perception and so on.

26 Max Weber (1958) p.177
30 Max Weber (1958) p.170
Further, these objectives would not be met if the unstated objective, subscription to *honestum* and *civitas*, were not also publicly acknowledged to have been met.

**ENTERPRISE INTERESTS AND THE LEGITIMATION OF RESEARCH**

The emphasis on credibility and trust, both within the Institute, each laboratory in it, and in the scientific *lex*, is both constitutive and promotive in the establishment and viability of the Institute and each laboratory within it. The significance of credibility is that it is a regulative, promotive, and constitutive means to actively and purposively gaining more credibility and generating more support for those who possess it. The rules related to credibility as means and end cannot be repealed. They are not negotiable rules, being ‘organic’ to the workings of science. This is not to say that tactical means are not employed for gaining credibility. This will be illustrated in Chapter 2. However, the fact that tactics are used to gain credibility does not in any sense detract from its significance as a condition of existence in science. While the enterprise associates may regard the particular observances related to the appeal for or allocation of credibility as advantages or hindrances, they cannot and do not contest the massive and invisible presence of the valuation itself.

Oakeshott says at one point.

‘The norms and prescriptions specifying the conditions of morally good conduct

*are not devices related to the pursuit of imagined and wished for satisfactions or for the achievement of a common purpose…they are themselves the terms of a moral relationship and have no extrinsic substantive end.*’\(^{32}\)

But at another he says:

‘An agent may choose to undertake an obligation. In a transaction aimed at an imagined or wished for satisfaction he may make a promise and thus put himself within the jurisdiction and under the authority of the rule that promises should be honoured. But

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\(^{32}\) Michael Oakeshott (1975) p158 italics mine
although he does this in a chosen transaction and as a chosen means for achieving a chosen purpose, although he can fulfil his obligation only in a chosen action which subscribes to the conditions of respublica, he does not choose the terms of the obligation; he ... employs them as a device for achieving his purpose.'

The second statement, which partially contradicts the first, highlights the possibility that moral subscription within enterprise activity can be a means to an end, which in the context of science means that credibility is 'pivotal', both means and end in research production and legitimation. Norms and prescriptions are thus a source of orientation for the enterprisers who must choose actions and projects that will be examined for their probity and morality, for their credible claim on knowledge-creation. Such a system of seeking and granting credibility can only be perpetuated by the interest and actions of the morally oriented enterprisers. If these considerations are eroded by no longer being deemed important, or through non-observance, then Oakeshott is not correct in saying that the norms and prescriptions are delinked from the pursuit of individual or enterprise gains. There is a very real two-way relationship here. Although Oakeshott dismisses this enterprise recognition of the terms of the civil obligation as a 'mere' device, it points to the real and powerful link between the chosen transactions and the promises of civility to be honoured. The meeting of obligations is not only for its own sake, but also because this is the only chance for those who meet them to meet them again through the continued performance of those chosen actions. The legitimation of the performances in terms of civility allows their continuation and that of the meeting of obligations. It also ‘entrusts’ and legitimizes the performers themselves.

The difference between the private observances of trust and public observances of credibility is that the adjudicative procedures of the lex are for the transversal settlement of the issue of credibility considered as impersonal trustworthiness. Further, the adjudicative observances of lex have a 'high degree of immunity from...disturbance.' Thus, one observes the anonymity of journal reviewers, the implacability and non-negotiability of their judgements. Although they have an ‘obligation to reach a

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11 Michael Oakeshott (1975) p.158 Italics mine
13 Michael Oakeshott (1975) p. 131
conclusion’ and may even supply reasons for this conclusion, they cannot be challenged. Thus, even when the reviewers reject a paper, their decision is not open to further interrogation, to pleas, commands, requests and so on. The reviewers’ intentions are not open to question, as ‘the arbitrary exercise of the subjective will of the judge.’ Enterprise interests cannot be imputed to the decision and be publicly stated. Just as in the exercise of the law, adjudicative conclusions must be understood to be resolutions of a specific ‘contingent uncertainty or dispute’ about the meaning of lex in relation to a contingent situation. This conclusion is not connected to the will, desire, or arbitrary action of the adjudicator. As Isabelle Stengers says:

A fiction that has the ambition of ‘being part of’ science aims for more than acceptance. Its vocation is to transform the collective, to modify the history of the collective. The wager of a fiction with a scientific vocation is always to propose, even if in a programmatic way, a new mode for the intervention of a phenomenon in discussions between humans. It is to constitute a phenomenon as a witness, authorizing and supporting the thesis of the one who speaks in its name. In this case, a scientific fiction has the vocation of inscribing itself in a history and of transforming this history, of having the testimony of the phenomenon accepted in such a manner that it becomes a point of reference in this history, and the starting point for new fictions.

Oakeshott says that a judicial conclusion is connected with lex in the sense of illustrating, not exemplifying it. The resolution of uncertainties must depend on an interpretation of the law that is within the ambit of lex. This implies that the juridical discourse and the ideology of civility are deeply conservative. One may draw a conclusion from this about the inherent conservatism of science and its processes of adjudication of new research. The discourse and the ideology expand around and engulf the shifts of thought; paradigm shifts do not threaten their stability. These are shifts within the ‘iron cage’ of the

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34 Michael Oakeshott (1975) p.133
35 Michael Oakeshott (1975) p.133
ideology and have never yet threatened it. Michael Polanyi's work argues that changes in the purposes and procedures of science should be the concern of scientists alone. One finds in this argument a strong resonance with Oakeshott's understanding that any changes in the ability of lex to tolerate new meanings can only be on the basis of that which is already prescribed in lex.

THE POWER AND AUTHORITY OF THE PRINCIPAL INVESTIGATOR

The Principal Investigator (PI), the head of the research laboratory is a key figure of authority. Each laboratory is eponymously identified with its PI. This relationship is so close that the career of the laboratory and the career of the PI are not separable. Given the linkage and the public determination of fates of the PI and the other laboratory associates, one cannot regard the PI, even at his most imperious, as exercising 'princely' prerogatives. The problematic of relations of work and authority in the laboratory cannot be reduced or subordinated to a 'problematic of the prince and his relationship to the principality of which he is the lord and master.' The PI cannot be seen as akin to a traditional sovereign, to whom obedience is given in a political dispensation wherein conformity to the sovereign's edicts are treated as sufficient in themselves. Obedience to the PI and to any other actors he may authorize is a means to an objective. It is not sufficient unto itself. It is a means in the production of knowledge, which must be found to subscribe to the requirements of lex. Only through this finding does obedience become a productive element in laboratory life.

The PI's supervisory control of 'his' laboratory has formal and personalized aspects, which sustain each other. One cannot see a relationship of annexation or unidirectional determination between these aspects of control. On one hand, it would be appropriate to regard the substantive relationship between the PI and his subordinates as 'pastorate' in the sense that Foucault uses it. As a pastorate relationship of power, the PI's supervisory control is over a group and a context of production, which he 'gathers together, guides, and leads.' The conjunction of the careers of the PI and the laboratory implies the continuity.

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58 Michel Foucault (2000) 'Governmentality' p204
59 Michel Foucault (2000) 'Omnes et Singulatim' p301
strengthening, or else weakening of both. The PI is included as an author in all research productions, with more laboratory papers at any time than any of the subordinates, each of whom may have senior authorship of one paper while the PI has at least four or five. He assumes both responsibility and authority for the destiny of the laboratory. He can be called upon to render an account of all the actions, acceptable and otherwise, of the laboratory’s associates. The activities of the associates are also imputable to the PI. The complexity and density of the moral ties between the PI and the subordinate associates are related to the intertwining of their careers and their individual actions. This personalized association is closely linked with the formal relationship of the PI and the laboratory. As a consequence, the relationship of formal authority and obedience is linked to a relationship based on personal control and submission. The PI’s ‘will is done, not [only] because it is consistent with the law, and not just as far as it is consistent with it, but, principally, because it is his will.’° Civility and pastorship are conjoined in the exercise of the PI’s authority.

On the other hand, the substantive relationship can only exist consequent to formal bureaucratic designation of the PI as such. The PI does not officially own anything or anyone in the laboratory. But he is deemed fit to govern, able to introduce and sustain oikonomia in the laboratory. He is entrusted to generate credible knowledge, which would in turn sustain the laboratory and its personnel. His governance of things and people is related to his ability to sustain a viable knowledge-production unit through ‘the right manner of disposing things.’ He must supervise the production of knowledge that will pass the rigours of examination in lex. This is made possible by wielding his knowledge of the material, cognitive, and interactional ensemble of the laboratory. This knowledge is also the tactical instrument for managing the laboratory. But this knowledge is tactical, of a highly contingent nature, relatively unconstricted by regulations. There is a remarkable paucity of regulations to guide actions and decisions in the laboratory. This paucity and the tactical and discretionary nature of the PI’s supervision are interconnected. The connection is engendered by the PI’s control of the laboratory. The PI can expel dissidents, and his control

° Michel Foucault (2000) ‘Omnes Et Singulatim’ P309; parentheses mine
can be extended outside the laboratory to exercise more punitive effects on the career prospects of the expelled researcher. Thus, control is generated by the substantive exercise of pastorate power within the formal authorization. The pastorate ensures a close ‘individualizing’ control over the researchers in the laboratory; it complements the impersonal, ‘totalizing’ bureaucratic structures of accountability, institutional and organizational orders of control. It is the intimately applied ‘technology of power’ in an impersonal authority structure. Authority and power are conjoined to produce a complex form of control in the laboratory.

Richard Whitley and Michael Polanyi independently observe that low regulation, ad hoc-ism and impromptu performances are necessary in a laboratory, given the sheer unpredictability of research performance. While I do not contest the significance of situational exigency, I would prefer to focus on the reciprocity between accumulations of credibility, of recognition of research, and the devaluation and attenuation of the official regulation of role performances. Senior researchers, with a preponderance of recognition of their work, of symbolic capital, draw on the community’s vocabularies and imaginaries of genius and achievement in order to exercise their authority. This is inevitably a tacit process of commanding and receiving obedience and personalized trust based on credibility. Seniors exercise authority based on an institutionalized recognition of authorial charisma. For the junior researcher, this recognition becomes a diffused and taken-for-granted recognition of the ‘rightness’ of the senior’s command position. Credibility, in the sense of impersonal trust, undergoes a cyclic mutation, to institutional charisma and thus into deeply personalized relationships of domination, obedience, and trust. There are a priori assumptions of reliability and know-how at the local, operational level, achieved and sustained through the tacit enactment of unproblematic quotidian routines. These relationships are characterized by unarticulated indexical and shared understandings of what it is to have ‘know-how’ and who likely possesses this ‘know-how.’ At the lex level, there is the civil virtue of credibility, sustained through contestations over research legitimacy, and also sustained in the research institute and the laboratory, which

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43 Michael Polanyi (1974)
‘incubate’ civic attitudes in their organizational styles. Networks link the parochial and the transversal levels. Thus, the exercise of ‘pastorate potestas’, personalized ‘lordship’, public credibility and private trust, are inter-generative. Lordship is closely related to the occupancy of a role of formal ‘rulership.’ It is not simply a case of lordly performances usurping the formal role of rulership.

In Michael Oakeshott’s work, for the most part there is a firm assertion that any possible conjunction of civil and enterprise association, of public and private, or of ruling and lordship would be a corruption of civility. Nevertheless, he does reluctantly concede,

‘This ‘invasion’ of public by ‘private’, of ruling by Lordship may indeed be recognized (somewhat equivocally) as a contingent situation which may emerge in the history of a civil association and not as a direct denial of the civil condition, when the common concern that the prescriptions of the lex shall be acknowledged is circumstantially transformed into a substantive purpose.’

We find in this observation a way to understand the laboratory as an imperatively coordinated enterprise association whose existence is predicated on and supports an ideology of credibility and civility. The laboratory exists for and because of the legitimation of its work in contexts that are also informed by and predicated on an ideology of civility. In the laboratory context, exercises of ‘lordship’ emerge in a particular situation, a conjunction of two circumstances. First, the continuance of formal rulership depends on legitimation of research work for its subscription to procedural civility. Second, trust, tacitness, and under-specified ad hoc exercise of authority supplant explicit, specific, formal regulations in the performance of research work.

The PI thus occupies an important and complex locus between the field of the laboratory and the larger field of research. He modulates the performance of work within the laboratory. The individual PI exerts a primarily but not exclusively local control over the connections between the laboratory and the lex fora, between research and its existence as credible knowledge. Through the exercise of such modulatory

\[\text{\underline{\text{\footnotesize{\textsuperscript{\textcopyright}}}}Michael Oakeshott (1975) p. 146}\]
control, the PI can influence the estimation of his professional strengths. The institutional estimation and support of the laboratory depends on the transversal estimation of the PI.

The role of the PI shares the ‘Janusian’ characteristic of the laboratory, in that he is both the ‘Principal Investigator’ and the ‘Boss’. ‘Principal Investigator’ is a role-designation that draws on the notion of ‘primus inter pares.’ Oakeshott says that considerations of equity are considerations of lex. The public nomination of the PI as ‘first among equals’ is possible, as the laboratory is an enterprise rooted in a practice and ideology of civility, speaking a language of moral intercourse. On the other hand, and closely related to the ‘PI’ function, the PI exercises a more personalized power as the ‘Boss’. This is the epithet attached to the PI in both the Delhi laboratories, and in every laboratory at the Institute. In conversations, I was asked about aspects of working with my ‘Boss.’ The ‘Boss’ and the ‘PI’ functions are closely related, ‘private’ and ‘public’ functions that meet in substantive engagements.

Oakeshott says that civil relationships are based not only on a consideration of ascertainable norms but also on the assurance that there will be a subscription to moral conditions in official and daily intercourse. The requirement of assurance ‘postulates procedures and offices which are not to do with adjudication but with the engagement of ruling.’ Thus the insistence on assurance and credibility and the exercise of authority are drawn into a mutually supportive relationship. Ruling involves injunctive utterances directed to and directing identifiable persons for the sake of ensuring that they act in specific desired ways. The execution of the law in order to ensure assurance among cives is related to lex adjudication. However ‘ruling’ is ‘annexed’ to lex, and it is dominated by ‘the office of adjudicating disputes about the meaning of lex.’

As discussed above, ‘ruling’ and ‘lordship’ are closely allied in the laboratory. Let us add another element to this alliance. Those who rule and exercise ‘lordship’ in the laboratory are also regarded as qualified to be adjudicators and their role as adjudicators is connected to their function as rulers. Without having acquired a certain level of recognition and credibility, a scientist cannot be a ruler or a lord in ‘his’

Michael Oakeshott (1975) p.141
laboratory. Without the occupancy of the ‘PI’ function, he is also not regarded as ready or fit to exercise an adjudicative function as a ‘peer.’ The separation of functions, authority, and power is tenuous at best. The recognition of civil subscription in the actions at the enterprise level place the PIs in the position of possibly governing adjudicative procedures. The PI is the ‘boss’ and also the Principal Investigator, the ‘lord’ or ‘ruler’, and also because of this conjunction he can be a ‘judge’.

Within the laboratory, the PI exercises a range of closely allied functions. With the authority of his rule, the power of his lordship, and the exercise of his prestige as a perceptive and experienced judge he ensures that the conditions for lex are met with adequate subscription. He does this through concrete, local performances of adjudication and command. He has the resources and the ability to exact compliance even against the will of those subjected in this manner. The ‘Boss’s’ potestas, his right to make assertoric, imperative statements, is left behind in the laboratory when the work is published. The published work created in part through his ‘private’ interventions is couched in probabilistic ‘reasons to believe’ to which the PI can append his name. The work is made transversal through the assertoric interventions and utterances that are made by the ‘Boss’ who acts from a ‘position to know’.

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46 Ludwig Wittgenstein (1980) On Certainty Basil Blackwell, Oxford. Wittgenstein problematizes trust and certainty in this critique of the work of the realist philosopher G.E. Moore. He says: ‘When Moore says he knows such and such, he is really enumerating a lot of empirical propositions which we affirm without special testing: propositions, that is, which have a peculiar logical role in the system of our empirical propositions...they all have a similar role in the system of our empirical judgements...We don’t for example arrive at any of them as a result of investigation... We do not learn the practice of making empirical judgments by learning rules: we are taught judgments and their connexion with other judgments. A totality of judgments is made plausible to us.’ Further, there is always a language-game, which demands certainty: in the laboratory, or elsewhere, when the scientist is defending or interrogating what happened in the laboratory, as witness, judge or jury. ‘For how do I know that someone is in doubt? How do I know that he uses the words “I doubt it” as I do?’ In the juridical examination of evidence and witness, the statements are probabilistic and impersonal. They provide ‘reasons to believe’. The ordinary language statement, “I know” implies subjective certainty. Such subjective certainty is absent from the formulation of a scientific hypothesis. For hypothetical statements to be given provisional trust, the question is asked: what is your evidence? If the evidence is satisfactory, it passes into assumption that the researcher is in a ‘position to know.’ Nevertheless, there are no questions about the grounds on which positions are assumed. The language-game demands certainty, and it can only be played, not interrogated beyond a point. Wittgenstein says certain kinds of doubt, such as about the possibility of existence, can only work in a language-game, provided we are first able to ask: what would such a doubt be like? This we cannot do always: ‘So this is it: I must recognize certain authorities in order to make judgements at all?... If you tried to doubt everything you would not get as far as doubting anything. The game of doubting itself presupposes certainty... “We are quite sure of it” does not mean just that every single person is certain of it, but that we belong to a community which is bound together by science and education... That is to say, the questions that we raise and our doubts depend on the fact that some propositions are exempt from doubt, are as it were like hinges on which those turn. That is to say, it belongs to the logic of our scientific investigations that certain things are in deed not doubted. But it isn’t that the situation is like this: We just can’t investigate everything and for that reason we are forced to rest content with assumption. If I want the door to turn, the hinges must stay put.’
a triple function, when he controls laboratory performances. By exercising such control this he protects the substantive purposes and interests of the laboratory as an enterprise and thereby also his own interests. He is also performing substantive actions that mirror, anticipate, and are in subscription to the requirements of the lex adjudicators. He is not a corporate head in the simple and pure sense. As the head of an enterprise association he combines the functions of rulership, judgeship and lordship with a view to gaining credibility, maintaining the rule of lex inside the laboratory, and at the same time protecting the substantive purposes and interests of the laboratory as a viable corporate entity in bureaucratic competitive capitalistic science.

CREDIBILITY AS A CONDITION OF LABORATORY LIFE

Having argued that the production of credibility and the private exercise of power and prestige are complementary, I would like to discuss how the ideology of civility is implicated in the efforts of the PI and his subordinates to place their work in lex. The engagement of scientific associates in the domain of lex with a view to having their work incorporated in its universe of meaning is suitably described as akin to Oakeshott's idea of 'politics.'

[Politics is] a deliberative and argumentative engagement directed to reaching conclusions sustained by reasons designed to persuade others of their cogency... A man deliberating a change in respublica may wish to consult others and one who has deliberated may wish to enlist the support of others for his conclusion. It is only the most desperate innovator for whom utterance anyhow is innovative or the most careless who are indifferent to their audience. Thus he will address himself persuasively to those whose agreement or concurrence is material to the success of his enterprise, soliciting their favour, enlisting them as partners, mobilizing their energies, and rehearsing before them the arguments which he thinks to be convincing... But he speaks always over their heads;
his utterances are addressed in the last resort to those who in virtue of the public office they occupy have authority to do what he proposes should be done...47

The excerpt above can easily describe the constant other-directedness of the scientific associates. As mentioned elsewhere, all actions in the laboratory are done so that the work can stand up to scrutiny and be deemed as credible. This consideration implies that the lex is actually never away from the laboratory, just as the PI’s presence or absence from the laboratory is unfailingly marked by the subordinate associates and conversed about, before taking so much as a coffee break. The engagements with lex are always at hand. Conversely, only through certain subtle shifts — in time, place, and self-presentation — can one actually say that the world of the laboratory is briefly left behind when the associates overtly engage with the lex. As will be discussed in detail later, the engagement with the lex is persuasive rather than injunctive, with statements that appeal and suggest rather than command. Oakeshott: ‘Rulers may participate in politics...they must put by their majestas and thus notionally vacate their offices, in order to participate.’48 In politics those who participate must speak ‘informatively, persuasively or defensively.’49 The PI ceases to be a ‘Boss’ in lex; he even ceases to be a PI insofar as his naming as an author in the research paper is concerned. He is instead a ‘corresponding author.’ (I shall shortly return to this significance of authorship naming.)

More significantly, Oakeshott’s description of the orientation to an audience suggests that such orientation is not composed of uniform considerations of all real or potentially significant others. It is conditioned by a specific hierarchisation of possible audiences. Thus, the publicly displayed work is not displayed in the same way in all fora. The prestige, legitimacy and legitimizing effect of the fora increases from the institutional seminar to poster presentations, national and international workshops and conferences, and finally the refereed journal, especially those with the highest impact factor ratings. The proportion of the data presented at each of these fora increases, as also the care taken with its presentation.

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47 Michael Oakeshott (1975) pp165-166
48 Michael Oakeshott (1975) p167
49 Michael Oakeshott (1975) p167
so as to maximize their perlocutionary effect vis-à-vis the audience. The data is moved up the scale of publicity. In this movement, the further it has moved, the greater the scrutiny it has withstood, and the greater is the acceptance or credibility attached to it. The movement of the data is also the movement of the researcher and inevitably the PI, always a senior 'first' author in every work of the laboratory, towards legitimacy and credibility through a successful negotiation of inquisitorial examination.

In order to understand the process of legitimation of research, I draw on Pierre Bourdieu’s work on the significance of ‘misrecognition’ in symbolic production. Bourdieu applies his ideas to the fields of literary and artistic production. One may think that this does not validate the application of his concepts in understanding symbolic production in science. I would however argue that the successful transversalisation of scientific research is related to the incorporation of literary and visual devices in it. These are the subtle tactical devices that are incorporated in the symbolic good to maximize its chances of gaining credibility and being legitimized. These incorporations are subtle and hidden, and their use is legitimized in two ways. They are seen as ‘aids’ to clearer understanding, and this ensures that they go unremarked for the most part. Despite the overriding concern about artefacts, these artefactual incorporations are not seen as such. When the research is legitimized, the devices pass into even greater invisibility, even as the productions they support are most visible, and can be most confidently exposed to scrutiny.

Drawing from Bourdieu’s discussions of the field of cultural production, outside the laboratory, there is a ‘constant, collective repression’ of economic interest and of the power and economic components of the research practices. Even though there is no significant personal monetary profit from research for the

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50 I follow Jürgen Habermas' understanding of J.L. Austin's categories of locutionary, illocutionary and perlocutionary acts. Locutions are expressions of states of affairs in the world, as expressed in the content of prepositional statements. Illocutionary acts are established in the form of statements, promises, commands, etc. which are generally expressed with a performative verb in the first-person, present tense. Perlocutionary acts are inherently persuasive, as they are meant to produce an effect on the speaker. In my understanding, the narratives of research claim to present veracious accounts of states of affairs in the world. The statements that deliver these locutions conceal the actor and the interventions that make them possible, with the removal of first-person pronouns and the use of the past-tense. The rhetoric seeks to achieve the appearance of objective distance between the scientist and the effect, between the revealing actor and the revealed, ostensibly non-engineered effect. This concealment of illocution is a perlocutionary, persuasive device. The persuasion works insofar as the devices that enable it are depersonalized, immunised from interrogation, and concealed from view. The participants in the encounters in which research is examined can sincerely claim that acts of interpretation and persuasion are alien to their 'truth-seeking' activity. Thus truth is supposedly sought and never achieved as a settlement or deployed as a strategy through illocutionary and perlocutionary acts. See Jürgen Habermas (1981) p.289.
scientist, there is a form of economism and economic rationality in the moral economy of research. The cycles of credit, credibility and symbolic profit in science may appear different from the profit-making activities of the non-scientist. However the movements, fluctuations, and repeated investments and audits of symbolic and financial capital, present a striking similarity to investment and profit in endeavours regarded as exclusively economic. The civility in the legitimation of research is homologous to the economism of enterprise association, although one may appear to be severed from the other.

Bourdieu says that 'symbolic capital is to be understood as economic or political capital that is disavowed, misrecognized and thereby recognized, hence legitimate, a credit, which under certain conditions and always in the long run, guarantees ‘economic’ profits. 51 The conversion of economic capital into non-economic forms is accompanied by ‘misrecognition’ of its economic and political aspects. This ‘misrecognition’ is crucial for the legitimation of ‘symbolic capital’ and for its use in raising the economic capital for its continued production and reproduction. Bourdieu provides two related insights on misrecognition in this discussion of symbolic capital. First, symbolic capital is transversally acknowledged ‘credibility’, ‘prestige’ or ‘authority.’ It denotes intersubjective, public legitimacy for symbolic goods. Economic capital cannot win legitimacy directly and on its own; it must be first converted into symbolic goods. Economic capital is rendered invisible in the process of exporting symbolic goods into \textit{lex}. This is through the deployment of ‘cultural techniques that camouflage economic reproduction with fictions of … truth…or communication…’. 52 Conversely, symbolic goods can be translated back into scarce and necessary economic capital that helped generate them. This is possible if they are sufficiently ‘consecrated’ so as to gain a healthy amount of prestige through the gain of credibility and therefore symbolic capital. Consecration involves a ‘misrecognition’ of these products, their producers and their production as disinterested and non-economic. The greater the consecration, the greater the symbolic profits, and the greater the chances of receiving increased economic capital to keep the cycle moving. The diminution or growth of money-credit/credibility/prestige may in fact be better conceived as a spiral rather than a circular

\footnote{Pierre Bourdieu (1993) p.75}
\footnote{Michel de Certeau (1988) p.29; ellipses mine. I am not entirely comfortable with de Certeau’s and Isabelle Stengers’ use of the term ‘fiction’, and would prefer to replace it with rhetoric.}
movement. The second insight is that the prolonged period of conversion of economic capital into goods for the restricted symbolic market increases the probability of estimation of symbolic goods as non-economic and disinterested. In scientific research, this time lag may be on an average about 3-4 years in the case of a PhD, and 1-2 years in the case of a postdoc.

My stance is that science, as a province of life, needs its own sense-making ideological apparatus. ‘Misrecognition’ is constitutive of research and its legitimization. Credibility and authority in science are powerful mediations in the production and legitimization of knowledge. As mediations, they should be considered as ‘conditions of existence’, without a theoretical status of their own in science, nevertheless possessing the strength of ‘conditional necessity.’ They are pervasive conditions; it would not help to think of them as foundations or central factors or initial causes. Their ‘practico-social’ function is crucial for cultural productions with an overtly ‘theoretical function.’ The laboratory simultaneously performs ‘base’ and ‘superstructure’ functions — producing knowledge and transmitting and reinforcing the ideology of civility and credibility. This ideology is ‘organic’ to science in this sense. As Althusser says, ‘Human societies secrete ideology as the very element and atmosphere indispensable for their historical respiration and life.’

We may recall Gramsci’s use of the term ‘historically organic ideologies.’ Amplifying the ‘positive conception’ of ideology, he says that these ideologies are ‘necessary to a given structure’, unlike ‘ideologies that are arbitrary, rationalistic, or “willed.”’ ‘Organic ideologies’ have ‘a validity which is “psychological”; they organize human masses, and create the terrain on which men move...’ He says of the interaction between ideological and material content, that ‘the material forces would be inconceivable historically without form and the ideologies would be individual fancies without the material forces.’ One finds this echoed in Foucault: The regime of truth is ‘not merely ideological or superstructural; it was a condition of the formation and development of capitalism.”

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The suffix 'mis-' appended to cognition may be singularly fallacious and infelicitous, if it is used with a foundational certainty about its separation from 'real' 'true' cognition. In this sense, I am critical of its use to connote a sort of deception, a hiding or distortion of the real, in the production of culture or knowledge. In this thesis, where I have retained the term 'misrecognition', the connotation is not that of a falsehood, but of an effective recognition or estimation. That which is shown, laid open to lex estimation, has effectivity of its own, and this is complemented by that which exists in the 'private.' In that sense, Oakeshott's formalistic distinction between the 'private' and the 'public' may also be critically appraised. Althusser says that the ideological apparatuses of family, education, communications, culture and the state repressive apparatuses, police, courts, and prisons are mutually supporting functions and aspects of the state. The apparatuses of the state are repressive, administrative, and ideological. The civil-juridical ideology of scientific research and its legitimation manifests itself in differing but closely related ways in the world of the laboratory and in the world of lex. Considerations of engagement with lex spectacularly condition and limit the choices of the researchers in the laboratory. As Oakeshott says, the private and the public spheres cannot be defined in exclusion. His formulations are lacking insofar as they favor formalistic distinctions over an account of the connections between the public and the private. The role of ideology in reproducing the power relations, tacit exercises of power and discretion over the workforce, and the means of production of the laboratory, cannot be seen in isolation from the juridical means to judge symbolic products in lex. The lex scrutiny of symbolic products invests them with the symbolic capital that enables their production in contexts that have an unscrutinised sub judice character.

AUTHORSHIP AND AUTHORITY

The objects produced by scientific research are turned into recognitions of a level of originality, novelty, in addressing a 'state of the question.' Recognition is won through the movement of symbolic goods through a 'network of professional and textual exchanges.' Although symbolic goods are inseparable from 'intellectual and social commerce', the process of seeking, according and winning

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56 Michel de Certeau (1988) p 43
credibility and recognition demands a ‘recasting’ of commerce, a structured forgetting of interest, collectivity, and conflict in generating the objects of discourse. Authorship and authority rely on such abstraction and purification. The author’s rights are established through the elision of the real relations of production in the export and transversal existence of the research product. Conversely, the flow of symbolic goods and recognitions also depend on the identifiability of each work with an individual ‘first’ author, whose identity is linked to the paper, achieving a seeming paradox, of his ‘individual and disinterested’ proprietorial association with the paper. It is not a real paradox: the paper would not be published without the rhetorical disavowal of interest, and it could not enter the symbolic market without the authorial insignia-cum-imprimatur, the mark that simultaneously denotes consent to publish, ensures accountability of the author (as honest and intentionless non-author), and comes to be linked to trust and authority.

Given the multiple authorship of the overwhelming majority of research papers in the sciences, the sequence of authors’ names acquires some conflict potential in the context of authorial ‘ownership’. In conversation at the Delhi laboratories, it was reported to me that a PI at a nearby laboratory always named himself first, senior, and corresponding author, regardless of the fact that a graduate student has actually done the work for the paper. The severe disapproval of the PI’s actions indicates that the subordinates regard such actions as abuse of power and breach of trust. However, such an exercise of relations of force and the fierce interest invested in the individual ‘ownership’ and protection of intellectual property remains concealed when a scientometrist naively enumerates authorial sequences, publication-lists, citation counts, and other such innocuous indices.

At CCL, as in most experimental research, most of the published papers are multi-authored.\footnote{A.J. Meadows (1974) \textit{Communication in Science}. Butterworth, London. The author says that sometimes the number of authors becomes so huge that the institution itself is named as the author. This happens in high-energy physics, a big science, with huge teams of researchers.} However, the sociologist should not see the attribution of authorship to more than one individual as the scientists’ full acknowledgement of the collective ‘ownership’ of research. Scientists themselves hold that
the paper 'belongs' to the 'senior author', numbered as '1' and with the name printed first in the sequence of the list of authors. For scientists, the sequence of authors thus takes on significance as an indicator of 'intellectual ownership', and thus deserves some discussion here.

As an illustration let us see the significance of this sequence: Kaushik\(^1\), Matthew\(^2\), Renu\(^1\), Aruna\(^4\), Evelyn\(^2\), Sridhar\(^1\). The convention at the Institute is, first, PIs and their graduate students are always co-authors. This ensures that at any time, the PI will have more recent publications in his resume than any single subordinate researcher in the laboratory. At CCL, the senior author was always the graduate student 'whose' research was being published. The PI, as the co-author, was last in the sequence of authors. The co-authorship was indicated by the superscript '1' after both their names. The first and last authors are, in that order, seen as the ones to have done the most work on the paper: the first author usually the graduate student or postdoc, and the last author usually the PI. In cases of seemingly equal senior-author contribution, the PI makes the decision about the order of naming. (Personal communication with Sridhar, 2004). Sridhar indicated that this decision is often a difficult one. One may see this choice as attributing more authorial importance to one over the other author, a choice that may result in dissent and unhappiness of the author who is relegated to a subordinate number.

The corresponding author, marked by an asterisk along with the number, is usually the senior author, the one who has done most of the hands-on research and is held as the most capable witness for the work. The senior-cum-corresponding author in the sequence is Kaushik\(^1\). When the senior author is unavailable for comment, having perhaps left for another institute, the last named author becomes the corresponding author. So the sequence would then change to read Kaushik\(^1\), Matthew\(^2\), Renu\(^1\), Aruna\(^4\), Evelyn\(^2\), Sridhar\(^1\). Thus, the PI is regarded as having the greatest authority to defend the work in the absence of the senior author, the principal researcher.

Finally, the other contributors may have provided ideas, reagents, laboratory facilities etc and are named. The numbers appended to their names indicates a rough assessment of their involvement or assistance to the research. Among the middle-named authors also, the numbering also reveals the subtle
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4. Visiting Professor, Maison des Sciences de L'Homme, Paris June 2004
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6. Member, Board of Studies, Centre for Linguistics, English Language and Culture Studies, JNU 2002-2004
7. Member, Board of Studies, Centre for French and Francophone Studies JNU, 2004
8. Representative, Standing Committee of Women's Studies Programme, JNU 1995, 2010
9. Member, Board of Studies of Spanish, Portuguese and Latin American Studies, JNU 2009
10. Consultant, Centre for Advocacy and Research, New Delhi
11. Board Member, Abhiwakasvinta Society, Delhi 1997-2005
12. Consultant India Foundation for the Arts, Bangalore 1999
13. Consultant Japan Foundation, Delhi 1994
14. Consultant British Council, Delhi
15. Consultant to Max Hospital, Delhi on Medical Ethics
17. to the Oxford University Press, Delhi 1994 to 1995 and from 2003 onwards
18. Vidyajyothi Institute of Religious Studies, (Society of Jesus) Delhi, board member 1996-99
19. Board of Committee for Bioethics in India, 2009
20. Concluded the Essex-JNU negotiations on behalf of CSSS, JNU with MCLI signed by Vice-Chancellor
21. Consultant to IGNOU at the introduction of a new course, 2009
22. Consultant Editor, Contributions to Indian Society
23. Organised CSSS/JNU weekly Thursday seminar for seven years, from 1997 to 2005

Research Guidance:
- Ph.D.s: 6 Awarded 5 in Progress
- M. Phil: 15 Awarded 4 in Progress

Major Themes of Dissertations currently under supervision:
1. North East Studies on Religion, Land and Law
2. On Theatre, and Dance as Therapy in Calcutta
3. On Masculinity in North Malabar
4. On Third Gender Studies: Hijras in India
5. Studies in Urbanism and Community with reference to Paris
6. Ritual, Performance and Culture among Communities in Rajasthan
7. Vashnavism in Assam
play of force and authority. In the sequence indicated, Aruna was the authorship of Aruna, a CCL PhD who had left the laboratory in 2001. Even though she had no actual, hands-on involvement in any recent research, her authorship is a mark of the foundational value accorded to her work. In a sense, one may say that the authorship both marks and authorises the uninterrupted presence of her work in the laboratory, in 2004, three years after her departure to MIT. In a conversation, I asked Sridhar, ‘What got you working on Notch in ICC?’ The response was ‘Well, first Spyros wrote a paper on Notch in cervical cancer, and then Vikas [former CCL Ph.D. Parentheses mine] and Aruna did an experiment and that set off everything.’

Among the others in the sequence, the author Matthew was a student who developed a cell-line essential for the research work. Evelyn was the authorship of Matthew’s PI; some of the research was done in ‘her’ Cambridge laboratory. The second-last position is indicative of the importance of this author. The naming of these scientists as authors is a form of thanking them for the gift of a crucial cell-line or the use of laboratory facilities. It is also something more. It works as a way of ‘swearing them in’ as witnesses for the credibility of the research in the paper. That they are authors, rather than brief mentions in the acknowledgments, is a mark of gratitude but also an attestation to the significance and value of the evidence they contribute to the paper.

These middle-named authors will include the paper in their list of publications but are always careful to state, in conversation, that the paper is ‘not theirs.’ Smita, a postdoc at the Institute, was named as a middle-numbered author in a paper whose senior author was the PhD Kaushik. I congratulated her, saying ‘Good to see you have a paper.’ She responded, ‘Well, thanks, but it’s not my paper. It’s Kaushik’s paper.’ When I asked Smita why she felt the paper was not hers, she said, ‘It’s not like I did the research. It was all Kaushik’s work.’ The remarks are representative of the individualistic and highly proprietorial attitudes to authorship. These attitudes are prevalent in almost all of experimental research. The rendering of authorship

\[ P. \text{Zagouras, S. Stifani, C.M. Blaumueller, M.L. Carcangiu and S. Artavanis-Tsakonas (1995) ‘Alterations in Notch Signaling in Neoplastic Lesions of the Human Cervix,’ Proceedings of the National Academy of Sciences, USA. Volume 92, pp6414-6418. Sridhar has done his postdoctoral research at Harvard where Tsakonas and his colleagues have worked on the role of Notch in cervical tumorigenesis. This work is an exemplar for the lines of research pursued at CCL. } \]
is connected to a 'structured forgetting' of the collective, of relations and networks. This sort of forgetting is
a preferential turning away of the gaze. The avoidance of observation of the networks of production
safeguards the notion of authorial rights. 'The illusion of authorship... camouflages the conditions of the
production of discourse and its object. For this negated genealogy is substituted a drama combining the
simulacrum of an object with the simulacrum of an author.' 59

The most important point raised by Bourdieu in his discussion of cultural production is
that 'misrecognition' of interest is necessary for the 'making of a name', the name both as a repository of
consecrations and a consecrating sign in its own right. In research, both scientific and social, the name of
the author is laden with consecration and consecrating force. The name is a trademark, a seal that attests to
a claim staked on a half-acre of intellectual property, to paraphrase Sartre.60 Names also acquire importance
as means to accountability. Similarly in Bourdieu,

'Words - the names of schools or groups, proper names - are so important only
because they make things. These distinctive signs produce existence in a world in which
the only way to be is to be different, to "make one's name", either personally or as a group.

The names of the schools or groups which have proliferated in recent painting... create
resemblances and differences by naming them; they are produced in the struggle for
recognition by artists themselves or their accredited critics and function as emblems which
distinguish galleries, groups and artists and therefore the products they make or sell.' 61

Bourdieu may just as well be describing the ethos of eponymy in scientific production, through
which the name of the PI is consecrated enough to become the quasi-identity for the junior researcher in the
'PI's laboratory'. These juniors are usually the graduate students or the postdocs, who are the labour force
doing the bulk of research in almost all laboratories at universities or research institutes. Thus my
conversations with the student-researchers at Delhi and the Institute usually included this query-response segment:

Q: Which laboratory are you in?

A: I am in [PI's name]'s lab.

This was also a segment that I overheard in the conversations of junior scientists introducing themselves to each other. This customary exchange is ubiquitous in research communities. It is thoroughly integrated into communicative practice between scientists, especially the junior scientists, working in one or another 'PI's laboratory'. So also is the 'disposition' which structures and is structured by such a practice. Custom and routine are self-perpetuating because they merit no comment or skepticism from the community that abides by them. They have perpetuity because they are relentlessly and unreflectively deproblematized. They should for that very reason attract the sociologist’s attention as something to be problematized.

The PI’s name is emblematic, a 'metonymic miniaturization,' and its use in self-identification reinforces that effect, even while pushing it farther into the sheer tacitness of the speaker’s habitus.

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63 See Pierre Bourdieu (1990) *The Logic of Practice*, Polity Press, Cambridge. According to Bourdieu, habitus is a system of generative schemes, a structured and structuring set of dispositions based on accumulated experiences. The past is always present in practice. Bourdieu insists on an understanding of the ways in which structure and agency mediate each other in the social sphere. He says that practice is what mediates between them. Neither pure novelty nor absolute determinism finds a place in his conceptualisation of practice. He focuses on the ways in which social actors engage in an ‘intentionless invention of regulated improvisation.’ Bourdieu engages in a critique of subjectivism and objectivism for neglecting everyday forms of experience and failing to explain socio-political facts. Subjectivism fails to consider the objectifying act with which an observer constitutes his/her object of study. Hence it does not account for the epistemological break between observing subject and observed object. Bourdieu attacks the structuralists’ excessive insistence on rules and models. Their merely heuristic value is replaced by their reification as empirical reality. The reification is accompanied by a denial of human agency. He also criticizes the existentialist notion of free consciousness for neglecting to consider the very real socio-political and economic checks on expression and action. Bourdieu introduces instead the concept of habitus. Bourdieu’s aim is to study the specific logic of this non-theoretical operative mode. He locates the principle of practice in the relationship between the external constraints and the dispositions that are produced by economic and social processes. These processes are more or less reducible to the external constraints, as defined at a particular moment. The minimal differences among individuals who share the same conditions of existence (social and economic) are basically determined by ‘the singularity of their social trajectories’. This ‘indeterminate, yet overdetermined’ logic is meant to account for both the cohesiveness and the internal differences of a social group. Bourdieu also examines symbolic economies that do not operate in terms of labour and money. He underscores the importance of a group’s symbolic relations for the reproduction of both the socio-political order and the productive apparatus (for instance, the spirit of the gift). A society represents itself in its symbolic organizations and definitions of distinction or value.
miniaturization is related to the tight ordering and the close bounding of the laboratory around one individual’s reputational gains, making the laboratory look like a ’supra-individual.’ An emblem makes the most practical sense to those people who have a practical mastery of its meaning, who share a tacit and untheorised understanding of its real or potential use in their language games. The use of an emblem such as the PI’s name provides to the interlocutor a measure of the speaker’s distinctness, allegiance, research interests, capital possessed (or not), prestige of laboratory membership, prestige of specialty and research area. The PI’s name, his signature appended to every document needed by the junior researcher, then acquires a ‘quasi-magical potency...the power, bestowed on certain individuals, to mobilize...symbolic energy....’ To be associated with a name, through association with an authority-bearer, or to be published and bear a name, to be sayable and said are of such importance to scientists that one feels justified in saying that they are interpellated with the ideology of charismatic authorship.

Althusser says:

'I say: the category of the subject is constitutive of all ideology, but at the same time and immediately I add that the category of the subject is only constitutive of all ideology, insofar as all ideology has the function (which defines it) of constituting concrete individuals as subjects...We observe that the structure of all ideology, interpellating individuals as subjects in the name of a Unique and Absolute Subject is speculary, i.e. has a mirror structure and doubly speculary: this mirror duplication is constitutive of ideology and ensures its functioning.'

In the subject-constitutive mirroring function of the ideology of authorship, recognition, and credibility, the subject who is recognized and constituted also reposes trust and recognition in the ideology

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64 Richard Whitley (1984) provides an elegant analysis of the sciences as systems of ‘reputational control.’ According to him, research is ‘oriented to collective goals and purposes through the pursuit of public scientific reputations among a group of colleague-competitors.’
65 Rom Harré (1981)
66 Pierre Bourdieu (1993) p. 82; ellipses mine
67 Louis Althusser (1971) p.163
68 Louis Althusser (1971) p.168
and other subjects similarly constituted. The subject who has been constituted through the ideology of
civility likewise believes and re-constitutes it through his actions and beliefs. The notion of a
'transcendental' unitary 'author' rests on a binary formed between private and public components of
identity, and on their differential expression in the processes of transversalisation and parochialisation.

Further, the desire for name, by association and by publication, is desire for recognition and
membership, for being ruled by the group. It is a polyvalent desire: with orientation to local recognition as
oneself and also as a transversally known literary name. It is orientation to others who are locally known
and trusted and transversally recognized as credible. To gain a name, subjecthood, for oneself, is tied to
subjection to someone with a name. Such subjection does not derive from physical violence; it is, at the
very least, manifestly 'trustful'. Obedience and trust are associated with an economic, sparing, and efficient
application of power. External applications of power, which may generate animus against the power-
wielder, are significantly, though never absolutely, replaced by self-governance and the docility of trust.
The two meanings of the term 'subject' should then be considered as homologous and not homonymous.
As Foucault observes:

'There are two meanings of the word “subject”: subject to someone else by
control and dependence, and tied to his own identity by a conscience or self-knowledge.
Both meanings suggest a form of power that subjugates and makes subject to.' 69

The significance of the name was highlighted in another exchange observed at CCL. Sridhar had
ordered certain changes to be done on Kaushik's paper to be sent for publication. Kaushik seemed reluctant
to carry out the changes. The problem was compounded by the fact that he had re-positioned figures
(illustrations), and thereby also changed their numbers and accompanying legends, when time was already
running short for re-submission. The shifting around of figures provoked the PI to considerable fright and
tension; time to the submission deadline was short and now there was a lot of additional work to be done to

69 Michel Foucault (2000) ‘The Subject And Power’ p.331
restore the original position and number of the figures. Also, the journal gatekeepers would not look kindly on the shifting-around, even in the unlikely scenario of a back-and-forth debate.

Sridhar: ‘I tell you to make the changes and you look at me like I am an **** [expletive deleted Parentheses mine]! What the **** do you think you are playing at here? Do you think you know more about this than I do? If you do, you are a complete ****[expletive deleted] fool! Tell me, who got Prateek’s paper [CCL postdoc], Asha’s paper [a former PhD, not in the laboratory anymore], and Hema’s paper [a PhD in another laboratory Parentheses mine], pushed through? What the ****[expletive deleted] else do you expect me to do? If you think you know so much about this, send it anywhere you like, just don’t put my name on that paper.’ [PARENTHESES AND EMPHASES MINE]

This last statement is a particularly telling threat for the capital-less junior scientist because it means the absolute withdrawal of institutional support; the PI is always the signatory to all the documents that link the junior scientist to the scientific establishment, to the home-institute, to other institutes, to funding agencies, to journals, and so on. The name of the PI is a sign of prestige, institutional affiliation, and symbolic capital. The research done by the junior scientists is crucial for the continued consecration of that name.

DOMINATION AND FREEDOM OF ACTION IN THE LABORATORY

Relations of force, orders to comply maintain the hierarchical relations of that research work and are maintained by them, within a field that institutionalizes, sanctifies and immortalizes eponymy. However, one cannot go so far as to speak of ‘total’ subordinate agents commanded by ‘total’ superordinate actors. Freedom and domination share a relationship. Domination requires the retention of freedom even where it seems to nullify it. Simmel observes quite correctly that the desire for domination can only exist insofar as resistance and the potential for freedom have not vanished. As soon as certainty replaces the ‘probability’ that an actor can make another act according to his will and command, the sociative form of
domination loses the resistant surface against which it can be exercised. As Foucault also observes, a power relationship 'can only be articulated on the basis of two elements that are indispensable if it is to be a power relationship: that “the other” (the one over whom power is exercised) is recognized and maintained to the very end as a subject who acts; and that faced with a relationship of power, a whole field of responses, reactions, results, and possible inventions may open up.'

For the work contract to have any meaning, there has to be a limit to the disparity of strength between the employer and the employee, a limit that permits the employee to consent rather than surrender to every behest of the employer. The threat of punishment, the weight of domination, can be effective only insofar as the subordinate retains the desire and capacity to evade the punishment and to resist it. Pure domination is untenable and cannot be found in relations no matter how coercive these may seem. This indicates that the freedom of choice of engagement in enterprise association can be found alongside coercion and domination, which are limited by considerations that absolutism is self-defeating. The idea of such freedom is grounded in the ideology of civility, which heavily conditions the actual working relationships of enterprise association, no matter that the two may be separated in their ‘ideal characters’. Recall Oakeshott: ‘cives ... are free agents...and civil association is... an understood relationship of intelligent agents.' The idea of free contract is found also in the enterprise associations that are so heavily conditioned by it. However, such a notion of untrammeled freedom to contractually engage in association or to disengage from it is mythical. It is doubtful if anyone can ever absolutely freely choose to absolutely freely engage in or disengage from civil association or for that matter from all enterprise association.

Self-chosen ‘freedom’ from enterprise association carries penalties, although these differ from the penalties attached to an equally futile attempt to choose freedom from civil association. There is always the scope for resistance: the subordinate retains some freedom. But the thing is, the subordinate, in his

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70 G. Simmel (1971) ‘Domination’ p97
71 Michel Foucault (2000) ‘The Subject And Power’ p340
73 Michael Oakeshott (1975) p. 113
relationship of ‘free contract’ with the superordinate, cannot exercise this freedom, because that would mean exile from the association. In science, the breach of relationship with the first PI would make it well nigh impossible for the subordinate researcher to join another laboratory, considering the weight of the PI’s recommendation or condemnation, and the importance attached to credibility of associates, and even more to trust, loyalty, and membership in eponymous associations. The breach of such an association is then squarely regarded as the subordinate’s insubordination and his inability to subscribe adequately to the conditions of scientific civitas.

At CCL, I observed that the time of submitting a paper for a journal was particularly fraught with tension. In conversations with the CCL and other laboratory members, I was informed that this is quite usual. Just as usual, as observed at CCL and the Delhi laboratories are the laboratory parties with food, drink and levity, which are customary upon acceptance of the paper for publication. One may therefore inquire why each submission is so tense and holds such potential for conflict and exercise of monocratic potestas by the PI. Further, one may ask whether each celebration means something more than an easing of tensions after the submission. It may be argued that the submission of papers is a sort of liminal event in the life of the laboratory. Liminal events are boundary-events, marked by an elevated sense of the potential for increased stability or chaos depending on the success or failure in negotiating such events. In the Handbook of Guidelines for Graduate Students at the Institute of field study, this following paragraph is notable for its legalistic precision, quite unusual given the scarcity of explicit rules and procedures at the Institute.

The work done by the student should constitute material for at least one publication in a refereed journal. The manuscript should be communicated to a refereed journal before submitting the synopsis; ideally it should appear in print before the thesis is

74 See Arnold van Gennep (1960) Rites Of Passage. Routledge and Kegan Paul. London. The author provides a rich analysis of the management of transition between social statuses. The social conspicuity of this transition involves, first, the preliminary dramatized setting apart of the individual from the community and, second, his reintegration into it. The point of transition, the threshold between one state and another, is replete with ambiguity and danger. The rite of passage is the ritual exercise of controlling the risk in the manoeuvre.
submitted. The minimum requirement for submission of thesis is that at least one
manuscript be accepted for publication in a refereed journal.\textsuperscript{75}

For the junior researcher, to have research published as a paper in a high-impact factor journal
(such as \textit{Journal of Virology} or \textit{Cell} for a laboratory involved in cell biology) is to pass a rite of academic
passage, when work is deemed fit to pass into greatest publicity, when it is declared most sayable, official,
and visible. It denotes the successful completion of an ‘operation which “converts” competence into
authority.’\textsuperscript{76} It means that the subordinate has met the norms enough to gain a fuller membership of the
group, with added responsibility and autonomy. It also shifts him into a more visible, hence advantageous
and capital-endowed, position on the highly competitive academic field.Visibility and recognition mean
credibility and distinction, the accumulation of symbolic capital. In the scientific field, the power of a
player to impose a vision and to get it legitimised is directly proportional to his symbolic capital.\textsuperscript{77}

The submission event occurs at and marks the boundary between the laboratory and the \textit{lex},
between the site of production and the market, between a deficit of credibility and a conferring of

\textsuperscript{75} \textit{Guidelines For Graduate Students}, of the institute where field study was done, used with permission.
\textsuperscript{76} Michel de Certeau (1988) p7
\textsuperscript{77} Pierre Bourdieu suggests a dialectical relationship between social structures and their representations. This dialectic
can be understood only through a consideration of the \textit{social topology} of human existence and relations. Social
structures express themselves as relations of power within a field, and within that imagined field spatial distance
indicates social distance. Social relations cannot be mapped through observation of interactions — social distance can
be masked through interactional strategies such as condescension. The mapping of social relations draws on a
consideration of the relative distributions of resources between loci on the field. ‘Resources’ refers to capital:
economic, cultural, social, and symbolic capital. Relative power is expressed as position on the field. It is determined
by the quantity and structure of available capital. Bourdieu attempts to address the question of how these relations are
established and how they are perceived and organized into groups and classes. Bourdieu suggests that construction of
the social world is structurally constrained, and second, the cognitive processes according to which we organize the
social world represent an internalisation of the very structures they seek to comprehend. This raises a problem that is
not satisfactorily answered: if habitus functions to make a particular worldview appear self-evident, how can
individuals begin to self-consciously manipulate perceptual mechanisms? Bourdieu says that the objective presentation
of the social world is as a symbolic system that is organised according to the logic of differential distance. The
consumption of champagne rather than beer and visiting an art gallery rather than a ballpark mark difference with
distancing in the social space. The visions of the social world are the reproductions of the relations of power and
difference. The visions contribute to the acceptance and permanence of these relations. Struggles over the power of
preserving or transforming the social world are struggles over the power to produce or activate categories of perception
of the social world. This makes the imposition of a legitimate ‘way of seeing’ a prime stake in social struggles. (See
Pierre Bourdieu (1990) ‘Social Space and Symbolic Power’ \textit{In Other Words: Essays Towards a Reflexive Sociology,}
Stanford University Press, Stanford, pp123-139) Bourdieu suggests that such struggles must somehow break away
from the determinative ability of existing structures. Symbolic power, the power to engage in this struggle, ultimately
depends on the possession of symbolic capital. Further, the vision that wins the field must present itself as a revelation
of ‘that which is already there’ than a construction pure and simple. The success of science relies on the success of
such a presentation of its activities.
credibility in a line of research, between the anticipation and the potential granting or withholding of prestige, credibility, and capital. These are the risky frontiers of ‘the scriptural economy.’ The boundary events therefore generate the tensions associated with boundary work, the successful negotiation of movement across boundaries while maintaining the integrity of the boundaries and the terrains that lie on either side of them. The point of submission is a point of transition. It is a point in which ‘an industrial inversion is made.... The things that go in are the indexes of a certain “passivity” of the subject with respect to a tradition; those that come out, the [concealed] marks of his power of fabricating objects.’ Relationships of force are deeply implicated in the process by which a laboratory maintains its parochial distinctness from its environment. One should not think of this distinctness as the result of the hardening of boundaries, because processes of parochialisation and centrifugal transversalisation exist only in relation to and difference from each other. The local practice is always oriented to the ‘Great Tradition’, alert and receptive, but not wholly submissive to it. Various ‘strategic’ manoeuvres are employed to maintain local distinctness and boundary integrity. These are directed by the PI, and via delegation and entrustment, by the other laboratory associates. Strategies are deployed in maintaining the local coherence and proprieties of the location.

The temporal point at which the symbolic good must be sent across the laboratory-lex frontier becomes crucial. This is because the event within which it is identified is seen as something that defines the continued stability of the physical, spatial, and temporal contexts that lie before and after it; and vice versa. The event creates a heightened consciousness of reputational fragility, and of the accompanying dangers to professional longevity. The participants approach the event and negotiate it in a state of heightened urgency, during which levity, discussion and argumentation are supplanted by commands and instructions. In a state of impending crisis, the subordinate tends to be less a communicating free subject and more a subordinate dominated subject.

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78 Michel de Certeau (1988) p.131
79 Michel de Certeau (1988) p 135 parentheses and ellipses mine
The temporal regulation of the submission of papers, the rules of *lex* at the first gate of entry into its domain, is such that it prohibits any real researcher-reviewer negotiation. This 'systemic' prohibition means also that no laboratory associate, however powerful, can evade the 'time-point' of submission, a sign of the rigidity of the temporal structures that govern the economy. This rigidity is supported in the laboratory, by the pure monocratic actions of the PI, the actor made most nervous by his eponymous association and investment in 'his' laboratory. His monocratic actions are observed in the laboratory at the time-point where laboratory and *lex* confront each other. These are in response to and, finally also in support of, the non-negotiability of the submission deadline and the demands of the gatekeepers of the economy. In CCL, personal-affective and discretionary elements in the PI's exercise of authority were most obvious at submission time. Affective violence was clearly observable in verbal commands from the PI. I observed two such submission events, both involving submissions of papers by Kaushik. One of these events has been detailed above. This was a time when the PI demanded unquestioning obedience of Kaushik. The timely submission of the paper was not marked by any group festivity. However, a jubilant party in the Postdoc room, with wine and snacks and much laughter and joking, marked the acceptance of the paper. This was clearly a cathartic event, marking the end of the transition. It served to repair group bonds that might have been affected by the adversarial events of the time of submission.

The events described prior to submission of Kaushik’s paper indicate that at such times, the PI's actions are an authoritarian response to the tacitly known risks and rewards of the economy of credibility. PIs are seasoned actors who tend to have an untheorised knowledge of the value of publishing in specific journals. They are constantly aware of the fluctuations of prestige attached to or withdrawn from specific areas and sub-areas of research. The interests of the PI are guided by this awareness and in turn guide the shifts of the laboratory's agenda into 'hot' new fields. These trajectories and itineraries are planned with an interest in maximizing the symbolic and financial capital of the laboratory and the PI, its 'quasi-owner.' The success in embarking on new research depends on the previous stocks of capital from earlier successes and from the requirements of the funding agencies. These agencies may or may not fund the research
depending on their assessment of its potential social utility. The PI's interests, ambitions and assessment of chances of success derive from a complex and nervous recognition: objects, practices and areas of research, funding, skill-sets, and credibility are both fragile resources and uncertain rewards.

PIs are aware that their future as powerful actors, endowed with discretion and capital, means the continued ability to bargain for funding for the physical resources needed for a viable laboratory. This depends on being published in the right, 'high-impact factor' journal at the right time, so as to gain priority, prestige, and visibility from the timing and forum of publication. This loss could have repercussions in the matter of getting funding for future research, and also for attracting novices to the labour force of the laboratory. A crucial aspect of control by the elites is the reproduction of the labour force of science, through the selection, training and indoctrination of novices. There is a correlation between the prestige of the laboratory head, and that of his disciplinary area, and the number of people who apply to be his students or for other membership in his research group.

At the Institute, Sridhar's area of research in molecular oncology was much sought after by doctoral novices, research fellows looking for a laboratory assignment, and short-term summer interns and trainees. In conversation with PhD program interviewees, 12 out of 20 interviewees informed me that they wished to do cancer research. Sridhar on the other hand stated in conversation, 'I would prefer to have one, maximum two PhDs in the laboratory. No more.' From the perspective of a thriving PI, the supply of aspirants is far in excess of his demand for them. Any event or action by a subordinate that could jeopardize the timely and effective marketing of the symbolic good bearing his name could also endanger the PI's status and future as a consecrated and consecrating actor. Such actions could diminish the PI's 'name'. The paramount actor in the laboratory therefore makes the subordinate aware of his status as a dominated agent.

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