

SCIENCE AND RECOGNITION*

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1 Different ways of producing goods

One may think of human societies as consisting, amongst other things, of various kinds of institutions through which equally varying kinds of human goods are produced. The question then arises of how these institutions manage to do this - by means of what devices and mechanisms, relying on what sorts of motivations and social relationships - and with what degrees of success. There is no reason to assume that the answers to these questions of 'institutional design' will be the same in every case: it might well be that different kinds of institutions are more or less successful for different kinds of goods.

In modern societies, perhaps the most familiar such institution is the economic market; almost as familiar is the classical, Smithian explanation for its considerable (though not unqualified) success. The market, it is said, ensures that producers, motivated exclusively by the pursuit of their own interests, nonetheless act in ways that are maximally beneficial to consumers: it is an institution which generates goods 'for others' precisely by the producers of these goods aiming only at their own well-being. The system, as it is sometimes put, produces just the outcomes which an altruist would wish to bring about, but precisely through the absence of altruism on the part of its agents.¹

There is thus a striking disjuncture between what may be termed the 'individual purposes' and the 'collective goal' (or at least the systemic effects) of this arrangement, so that it is *as if* an 'invisible hand' guided its operation: the goal of the institution, namely the material well-being of all, is achieved despite its achievement not figuring in the purposes of its individual agents. But it is *only* 'as if': the actual means by which this magical transformation of private into public interests takes place are themselves quite visible and mundane. The devices of competition and the price-system operate in such a way that self-interested producers can succeed only to the extent that they provide consumers with what they want, at a profitable price: failure to deliver the goods to others leads to failure to secure goods for themselves.

Yet even in modern societies, where the market is arguably the dominant institutional device for generating human goods, we are also familiar with other, quite different institutions which are nonetheless also highly effective in delivering 'their' respective goods. Amongst these is the institution of modern *science*.² It would

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¹ See O'Neill 1992. But there are justifications for the market other than this one: on the different implications of 'classical' and 'liberal' justifications, see 'Justifying the Market and its Limitation', in *Cultural Goods and the Limits of the Market*, London: Palgrave, 149-171.

² More specifically, the distinctive institutional form of organised scientific enquiry, often referred to as 'academic science', which emerged in Europe during the nineteenth century and came to dominate the

be hard to deny that this has proved remarkably successful in producing the goods of scientific knowledge, just as the market has for consumer goods. It would be equally hard to deny that the institutional organisation of modern science differs radically from that of the market.

The specific character of this organizational form has been the subject of considerable analysis and debate amongst sociologists of science, who have, in effect, been concerned with the question of what explains the success of this institution - of how it has managed to 'deliver its goods'.³ Later on I shall consider two such analyses which I have found especially illuminating: by Warren Hagstrom, in *The Scientific Community* and by Jerry Ravetz in *Scientific Knowledge and its Social Problems*.. But first I shall provide a broader context for that discussion by making some general remarks about possible alternatives to the market as a goods-producing institution.

It might initially seem that any alternative to the market must rely on motivations and relationships quite contrary to those upon which *it* depends, according to its classical rationale: more specifically, that self-interest must be replaced by altruism, and competition by cooperation. That is, assuming that one is considering institutions whose goal is to produce goods which enhance the well-being of those for whom they become available, any alternative to the market might seem to require an institutionalised form of social cooperation between individuals whose motivating purpose is precisely to achieve that goal. The disjuncture between individual purposes and institutional goals displayed by the market is then eliminated: in effect, the two become identical, and thus there is no need for a special, 'transformative' device to ensure their harmony.

Since the over-riding purpose of such individuals is to achieve their institution's goal(s), they must be prepared to act in whatever ways are necessary to do so - even, or indeed especially, where this requires them to do things they would otherwise be disinclined to do, or to restrain themselves from doing what they would otherwise be inclined to. They must, that is, be prepared to engage in personally 'costly' activities wherever there is a conflict between their own interests and what the realisation of the institutional goal requires of them. Thus, borrowing a term from Amartya Sen, one may call this alternative to the market the *commitment* model of institutions.⁴ Here, the (external) 'discipline of the market' is replaced by the (internal) discipline of commitment: whilst the market relies on the self-interest of producers and the transformative device of competition, this alternative model relies on the motivational commitment of producers to generate those goods which it is the institution's goal to produce.

conduct of 'pure' scientific research in the university system until at least the mid-twentieth century: see Ravetz 1971, chs 2 and 8.

³ A distinction is normally drawn between 'the sociology of science' and 'the sociology of scientific knowledge'. The former is mainly concerned with studying the institutional organisation of science, an activity whose epistemological credentials are broadly accepted; the latter attempts to explain the specific content of scientific 'knowledge-claims', in ways that are often seen to problematise those credentials. For a review of both (though more sympathetic to the former) see Zuckerman 1988; for debates about their respective legitimacy and implications, see the journal *Science Studies*, especially from the mid-1970s to mid-80s; for an influential articulation and defence of the sociology of scientific knowledge, see Bloor 19??.

⁴ See Sen 1977. So on this analysis, altruism is one particular form which commitment may take, namely where the relevant institutional goal is the well-being of others. What all cases of commitment have in

For which kinds of goods might this commitment model be seen as potentially superior? Here an assumption made by the classical justification of the market is of some significance: that consumers are able to judge the quality or value of the goods made available to them by producers. For if the market is to succeed in its goal of enhancing the well-being of consumers, it is not enough that their preferences, 'expressed' by their willingness-to-pay, exercise control over the decisions made by producers. It must also be true that consumers are in a position to judge the relevant qualities of the goods concerned. To the extent that this condition is not met, the market cannot be expected to succeed as a goods-generating device. It is on these grounds that *professional* institutions are often justified.⁵ Their members are said both to possess the expertise which their potential beneficiaries do not, and to be *committed* to providing them with goods of the requisite quality and character, acting under the constraint of relevant ethical norms.

Such claims in support of commitment-based, professional institutions are often met by considerable scepticism. The radical sceptics deny that the capacity for commitment forms part of our motivational repertoire, and view the self-attribution of commitment as merely a disguise for the self-interested motives by which we are naturally governed.⁶ Given these facts of human nature, it is better to subject self-interest to the discipline of the market than to allow what will always *be* self-interest - despite professional protestations to the contrary - to remain *undisciplined*. I find this radical form of scepticism implausible. But there is a less radical version which is a good deal less so. According to this, our capacity for sustained and self-constraining commitment, whilst real enough, is too limited - too vulnerable to the power of other, competing motives - to provide by itself a reliable basis for the effective operation of goods-producing institutions.

Does this 'failure' of the commitment model mean there is no effective alternative to the market? Not necessarily. For perhaps it was wrong to assume, in the line of thought leading to this model, that any alternative to the market must rely on motivations quite contrary to those which are harnessed by the market. More specifically, perhaps what is at issue here is not so much whether any motivationally realistic institution must rely on 'self-interest' rather than altruism or commitment, but whether the particular kind or form of self-interest relied upon by the market is the only one available. If this were not so, if there were others also, these might play some part in reducing the extent to which the commitment model requires unsustainable degrees of self-restraint on the part of those involved.

One such different kind of (self-)interest is the satisfaction often derived from the performance of those activities through which - 'as it happens' - human goods are produced. There is clearly nothing altruistic about the pursuit of these sources of 'intrinsic satisfaction': the activities concerned are not being conducted with the purpose of realising the institution's goals, and hence of producing the benefits their

common is the willingness of individuals to sacrifice or limit their own well-being for something they regard as more important than this.

⁵ But no such limitation to the professional model's desirable range of application is accepted by some of its advocates: see especially Tawney 1921 for a powerful argument in favour of this displacing the market altogether.

achievement confers on others. But neither are they being performed as a means of obtaining the kinds of 'extrinsic' rewards which the market is typically assumed to rely upon. Perhaps, then, there is some way of designing institutions which makes effective use of such intrinsic satisfactions as a way of ensuring that their goals are achieved? But it is difficult to see just how this could be done. For there seems no good reason to expect that those activities which enhance the intrinsic satisfactions experienced by those concerned will coincide with those required for the realisation of the institution's goals.⁷

Or at least, there is no such reason unless one limits the potentially wide range of intrinsic satisfactions to a particular sub-class of these. I have in mind here the kinds of satisfaction and enjoyment that are derived from what Alasdair MacIntyre has called the 'internal goods' of a *practice*. I have provided an account of MacIntyre's conception of practices elsewhere in this volume [see 'Consumer Sovereignty and the Integrity of Practices', in R. Keat and N. Abercrombie eds, *Enterprise Culture*, London: Routledge 1991, 216-30, reprinted in *Cultural Goods and the Limits of the Market*, London: Palgrave 2000, 19-32], so I shall not repeat this here. But the crucial point, in this context, is that internal goods are defined by reference to the *standards* of the practice concerned, standards which serve to identify what count as proper performances of the practice's activities, and hence what contributes to its overall goals. So not only do the internal goods of a practice provide significant sources of enjoyment for its participants, but the intrinsic satisfactions thereby experienced can be gained only from activities that contribute to, or are at least consistent with, the realisation of the practice's goals.

The significance of this can be brought out in another way. Any participant who tries to get away with conduct which fails to meet the practice's standards, or is in various ways deceitful, dishonest and the like, will be engaging in an essentially *self-defeating* project. For even if their fraudulent or shoddy work goes undetected, they will nonetheless have denied themselves the satisfaction that comes from the practice's internal goods: in failing to conform to its rules, they will fail to achieve such enjoyment. 'Cheats may win the game, but they deny themselves the satisfaction of playing it well'; it is therefore not in their interests to do so.⁸

So it may seem that one has here a viable alternative both to the commitment model and to the market - one which does not suffer from the former's unrealistically demanding motivational requirements, but which might be more effective than the latter in those problematic cases where one cannot rely on the ability of consumers to make the relevant judgments. Indeed, there is an important sense in which this 'practice' model, like the market, operates as if there were an 'invisible hand': it possesses an ingenious device through which the self-interested purposes of individuals can be made consistent with the achievement of institutional goals. Aiming only at their own enjoyment (of internal goods), participants

⁶ For what is, at least implicitly, a sociological version of such scepticism, see the analysis of professions in terms of power in Johnson 1972.

⁷ See Lane 1991, both for discussion of intrinsic rewards and for criticism of the market on the grounds that it generally fails to provide these for workers; I discuss this criticism in 'Consumer-Friendly Production or Producer-Friendly Consumption', in *Cultural Goods and the Limits of the Market*, London: Palgrave, 133-148.

⁸ See Crowley 1987, ch. VII, for an analogous but far more elaborate argument to this effect, and more generally for an illuminating account of the moral significance of practices.

find themselves performing just what it is that will realise those goals. Producers in the market can acquire profits only by providing what consumers want and will pay them for; participants in a practice can enjoy its internal goods only by acting consistently with its standards. In neither case is the realisation of the institution's goals the purpose of individuals' actions; nor, consequently, is there any (undue) reliance on their commitment to these.

Yet, attractive as this line of argument may seem, it is not altogether convincing. One can see this by noting that there is no guarantee that participants will not act in self-defeating ways: people do cheat, even if they thereby 'cheat themselves'. This may indeed be bad for them, but our concern here is with the effectiveness of the institution, its ability to deliver its goods. So unless we also have reason to believe they will not actually do what is bad for them, this is no help. Further, although self-defeating actions deny to participants their enjoyment of internal goods, these actions may nonetheless be - or seem to them - 'best overall'. For the standard-consistent action may still be too costly, when their *other* interests are taken into account. So although it is a great merit of the practice model to reveal the narrowness of market-related conceptions of human interests, it may nonetheless suffer from a parallel narrowness of its own, and lack any corresponding device to deal with this.⁹

2 Hagstrom and Ravetz on recognition in science

Having indicated both the apparent attractions of certain alternatives to the market and some of the difficulties faced by these, I turn now to consider the institution of modern science. As I suggested earlier, this seems to be a clear example of a successful goods-producing institution which has operated outwith the market. Perhaps, then, there is something to be learned from the means by which this success has been achieved, and which would resolve some of the problems for non-market institutions identified so far?

Drawing on the analyses provided by Hagstrom and Ravetz, I shall argue that this is indeed so. As will shortly be seen, both give particular emphasis to the part played by *recognition* in the institutional organisation of science.¹⁰ But I shall go on to argue, in section 3, that there is nonetheless something unsatisfactory about the way in which they conceive of the motivational basis and role of recognition. I will conclude, in section 4, by suggesting an alternative way of doing this.¹¹

⁹ Indeed MacIntyre himself insists that practices cannot be sustained without the support of *external* goods, namely money, power and status (MacIntyre 1981, p. 181). This is not just because they need material resources, but also because they need to find ways of ensuring that their members act in accordance with the practices' standards.

¹⁰ See also the discussion of this in 'Colonisation by the Market: Walzer on Recognition', *Journal of Political Philosophy*, 5, 1997, pp. 93-107, reprinted in *Cultural Goods and the Limits of the Market*, London: Palgrave 2000, pp 70-85: what follows can be seen as an attempt to elaborate and substantiate the claims made there about the damaging effects upon science (and other similarly institutionalised practices) of the 'colonisation' of recognition resulting from the illicit transfer of 'market meanings'.

¹¹ There is, of course, a great deal more to Hagstrom's and Ravetz's analyses than I shall consider here. In particular, anyone wishing to find a fully elaborated example of what MacIntyre would regard as an institutionally organised *practice* could do no better than turn to Ravetz's *Scientific Knowledge and its Social Problems*, 'despite' its having been written much earlier than *After Virtue*. Nor, conversely, are Hagstrom and Ravetz the only sociologists of science who have seen recognition as an important element in scientific institutions: see also, for example, Jerry Gaston's *The Reward System in British and American Science* (Gaston

The starting-point for Hagstrom's analysis is his dissatisfaction with previous attempts by sociologists of science to account for its success in terms of the norms and values shared by members of the scientific community. According to such accounts - the most influential of which had been provided by Robert Merton - the activities of scientists are governed by the following norms:

Communism - scientific knowledge is to be regarded as common property, belonging to all members of the scientific community (and others), and not as the exclusive property of those responsible for its discovery; *Universalism* - scientific claims must be assessed by impersonal criteria, without reference to the personal, class, national or other such characteristics of those who make them; *Disinterestedness* - scientists are to pursue knowledge 'for its own sake', rather than being motivated by concern for their careers, reputations, material rewards etc; *Originality* - priority must be given to the generation of new theories, results, methods etc, and not to the reproduction of existing beliefs, traditions etc; and *Scepticism* - scientists are to be critical both of their own and others' work, taking nothing for granted nor as beyond possible doubt.¹²

Hagstrom doubts the explanatory adequacy of such accounts. He thinks it unlikely that the training received by scientists could effectively 'socialise' them into accepting such norms as genuine constraints on their conduct, and he believes more generally that social norms are likely to be ineffective if they are not accompanied by 'positive reinforcement' for the behaviour they endorse. Putting this in the terms I used earlier, one might say that Hagstrom doubts whether 'commitment' to the norms of science provides a plausible explanation for the behaviour of scientists, and hence whether it could provide an adequate motivational basis for scientific institutions.

To the extent, then, that the conduct of scientists is broadly consistent with these norms - and Hagstrom does not wish to deny that it is - there must be something else which accounts for this. He thinks this must be something which scientists themselves find rewarding or satisfying. An obvious candidate here might seem to be the intrinsic satisfaction which they can derive from their intellectual work. But although Hagstrom accepts that such satisfaction is often forthcoming, and has a degree of motivational power, he believes that it cannot account for various features of their conduct which are crucial to the successful development of scientific knowledge.

In particular, he argues that this kind of motivation would not lead scientists to direct their efforts specifically towards *new* or *unsolved* problems, since the satisfaction that comes from solving problems is largely independent of the 'newness' either of the problem or of its solution. But such selectivity on the part of scientists is clearly important for scientific progress (as indicated in the Mertonian norm of 'originality'). Further, the intrinsic satisfaction generated by solving intellectual problems would not ensure

1978). For informative reviews of research and debates on the organisation of modern scientific communities, see Mulkay 1977 and Zuckerman 1988.

¹² My presentation of these (acronymically CUDOS) norms derives from Ziman 1984 (who notes also the re-naming of 'Communism' as 'Communality' during the Cold War). The norm of 'Originality' was added later by Merton to his original list of four 'institutional imperatives' (see Merton 1942 and 1957). See

that scientists actually *published* (or submitted for publication) the results of their work. But Hagstrom notes that for most scientists, the satisfying part of the process stops short of the point at which they prepare their results for publication, which is regarded instead as a painful chore. Yet without such public communication their work would be of no benefit to other members of the community, and hence to the development of scientific knowledge.

What is needed then, says Hagstrom, is some system of *rewards* which ensures that scientists are sufficiently motivated to do what it is that the development of scientific knowledge requires. He thinks it unlikely that the kinds of material rewards bestowed by the market would be effective here, and notes that in any case, there is ample evidence that they are actually far from crucial, since scientists who have achieved material security typically continue to publish, despite no longer 'needing' to do so. Instead, he argues, the reward that really counts is that of *recognition*.¹³

Hagstrom argues not only that there is considerable evidence that this is something highly valued by scientists, but also that this form of reward is ideally suited to the successful functioning of the scientific community. For recognition is conferred only on those who provide the community with what Hagstrom terms *information*, meaning by this the kinds of 'results' - proposed solutions to theoretical problems, reports of experimental studies and so on - which are of value to the development of scientific knowledge. Further, he suggests, one can best understand the process involved here by drawing on the ideas of certain anthropologists: in particular, on Marcel Mauss's account of systems of *gift-exchange* (Mauss 1954). What scientists 'give' is information, and what they receive in return is recognition.

I shall say more about Hagstrom's view of gift-exchange later on. But before doing so I turn to Ravetz's analysis of scientific institutions. As will be seen, his view of the significance of recognition is similar to Hagstrom's, although it is couched in rather different terms.

Ravetz argues that a central problem for the social organisation of science is to find some way of 'harmonising' its institutional goal, the advancement of scientific knowledge, with the motivations of individual scientists. Thus:

"The starting point of this present analysis is the distinction between the collective goals of that [ie scientific] work, and the private purposes of each of the agents involved in it. For the work to be successful, there must be a harmony, or at least an accommodation, between these two sorts of 'ends' or final causes. It is naive in the extreme to assume that they can, or should be, identical, even in cases where the work demands dedication and self-sacrifice from the agents. For the establishment of such harmony, there must be certain social mechanisms in constant operation...". (Ravetz 1971, p. 243).

Mulkey 1977 for a sceptical view of whether these norms are actually shared by scientists, and Gaston 1978 for a critical response to such scepticism.

¹³ See also Gaston's argument that the financial rewards gained by scientists cannot be sufficient to explain their scientific activities, since their work is frequently arduous and frustrating and they could easily earn more elsewhere (Gaston 1978, p. 14).

So although Ravetz acknowledges the need for dedication and self-sacrifice - and hence for 'commitment' - on the part of scientists, he clearly doubts their adequacy as a means of harmonisation.¹⁴ What is also required are social mechanisms or devices which enable the pursuit by individuals of purposes other than the advancement of science to nonetheless contribute to this goal. One such device, to which he devotes considerable attention, is that associated with 'The Published Research Report', the system by which the results of scientific enquiry are made available, through their publication in journals, to members of the scientific community.

The great beauty of this device - when it is working effectively - is that it simultaneously performs two essential functions. On the one hand, it contributes to realising the collective goals of science. It ensures that only work that is at least adequate is made public, since it is a condition of publication that this work has been confirmed as such by the journal's editors and/or referees, who are assumed to be competent judges in terms of the discipline's standards. It thus meets the need for what Ravetz terms 'quality control' in science. The published results can then be used by other scientists in their own research and ensuing publications: the results can be relied upon for their validity, accuracy and the like, and their citation both supports the credibility of this later work and enables its readers to check the sources it relies upon.

On the other hand, this system also provides individual scientists with a variety of rewards which may be expected to satisfy their motivational aspirations. In particular, the very fact of publication immediately confers the benefit of *recognition* upon its author, since the competence and quality of the author's work is thereby confirmed, and hence also their sense of worth as a scientist. Additional and similarly valuable forms of recognition may also follow this: the citation of their work in the publications of other scientists; invitations to present papers at conferences; the conferral of various prizes and honours - and indeed the material rewards stemming from tenure or promotion.

In his analysis of these additional forms of recognition, Ravetz gives particular attention to the role of citation. He argues that this can best be understood by conceiving of the published research report - along with other outcomes of an individual scientist's work - as a peculiar kind of intellectual *property*. Its peculiarity consists in the fact that it "comes into existence only by being made available for use by others"; the author provides materials "for others who are doing just the same as himself, in a common activity whose ultimate purposes, in terms of the benefit to the lay society that supports it, are remote and diffuse" (Ravetz 1971, pp. 245-6). Yet despite this availability to others, and the absence of any commercial charge for its use, authors retain certain property rights over the intellectual objects they have created. In particular, no-one is to make use of them without proper acknowledgement of their having done so, of the

¹⁴ That Ravetz nonetheless also thinks that the social mechanisms alone are not sufficient is indicated by the fact that the passage just quoted immediately continues: "... but for these [social mechanisms] to perform their functions, it is necessary in turn for those who are involved in their operation (both as agents and as subjects) to have attitudes appropriate to their roles in the system" (1971, p. 243); as he puts it at one point, quoting from the wisdom of an Atlantic City bus conductor: "wherever there's a system, there's a racket to beat it" (1971, p. 295). In a similar vein he notes that: "... doing good scientific work is strenuous and demanding, and the quality of the work done in any field is dependent, to a great extent, on the integrity and commitment of the community of scientists involved." (1971, p. 58). See also Note 37 below.

authorial source of what they now rely upon. There is thus an elaborate etiquette governing the use of citations in scientific work.¹⁵

Ravetz's analysis of the published research report, utilising this concept of intellectual property, differs somewhat from Hagstrom's corresponding analysis of 'information', couched in terms of gift-exchange. But there are otherwise such strong similarities between the two, especially in the role attributed to recognition, that one can see them together as identifying a crucial feature of scientific institutions which both distinguishes them from the market and enables them to avoid some of the difficulties identified in my earlier discussion of certain alternatives to this.

As with the market, a disjuncture between individual purposes and collective goals is assumed to obtain, and it is accepted that individuals' commitment to these goals cannot be relied on to ensure their achievement. Instead, some harmonising device must be found, which makes conduct consistent with those goals sufficiently attractive to those concerned. Yet unlike the market, the rewards provided for scientists do not consist, at least directly, in the satisfaction of their material interests, but in various non-material forms of recognition. Further, the means by which it is determined whether such recognition is to be accorded do not, as in the market, derive from the judgments expressed by consumers through their willingness-to-pay, but from the judgments made by other scientists about the quality and value of the work that has been done - a difference which, it might be argued, is quite appropriate in light of the specific character of the 'goods' produced by scientific work.

Of the non-market models considered earlier, the analysis of science provided by Hagstrom and Ravetz is closest to MacIntyre's account of institutionalised practices. But the role they attribute to recognition adds something of considerable importance to that account, since it provides the participants in practices with an additional and powerful reason for acting in ways that are consistent with its standards and hence with the realisation of its goals. By doing so they not only experience the intrinsic satisfactions associated with internal goods, but are also rewarded by various forms of public acknowledgment of their contribution and confirmation of their worth.¹⁶

I shall argue later that there are, nonetheless, certain problems in the way that Hagstrom and Ravetz conceive of the motivational basis for recognition, and hence for the function it can perform in

¹⁵ Ravetz refers to such recognition through citation as a kind of 'payment' made for the use of the individual scientist's property (Ravetz 1971, p. 247). Cf. Hagstrom, who notes that, in Mauss's account of gift exchange, the donor is seen as continuing to have a certain legitimate interest in the gift and what is done with it (Hagstrom 1965, p. 20).

¹⁶ It would seem that recognition is, in MacIntyre's terms, neither a (purely) internal nor a (purely) external good. It is perhaps closest to one of the latter, namely prestige (or status). But whereas it is a defining feature of external goods that - unlike internal goods - their character is independent of the specific nature of any practice and its standards, so that they can be obtained by potentially any means, the same is not true of recognition, since its receipt is conditional on acting in ways which meet the relevant standards. The 'good' of recognition might thus be viewed as *mediating* the relationship between internal and external goods, in such a way that the dangers which MacIntyre sees as stemming from the reliance of (institutional) practices on external goods can be obviated: see Notes 9 above and 37 below.

institutional practices such as science. But before doing so I shall consider briefly a further aspect of Ravetz's analysis which I have so ignored.

Ravetz is concerned not only to identify the means by which science has achieved its considerable success, but also to warn against what he sees as the threats posed to this by the changes to its institutional organisation which have been taking place since the end of the second world war. These changes involve the displacement of what he calls *academic* science by *industrialised* science: the production of scientific knowledge increasingly takes on the character of industrial production in a market economy, with large-scale, hierarchically structured and formally managed organisations replacing the relatively small-scale, informally organised groups of independent scientists typical of academic science.

This transition, says Ravetz, has been brought about largely by the increasingly capital-intensive nature of scientific research, and "... is as radical as that which occurred in the productive economy when independent artisan producers were displaced by capital-intensive factory production employing hired labour."¹⁷ Its main effect has been to undermine the previous and highly effective system of quality-control of 'academic' science, including the crucial part played in this by recognition. In the absence of any similarly effective system to replace this, standards both of adequacy and of value become adulterated.¹⁸ Increasing amounts of work are generated which are either 'shoddy', in failing to meet proper criteria of adequacy for the solutions to scientific problems, or 'insignificant', in that the problems addressed are unworthy of serious scientific attention.¹⁹

The basic reason for this decline is that the efforts and skills of scientists are increasingly directed towards securing the material resources for their work, especially in the form of research contracts, whilst at the same time the award of these is insufficiently based on the criteria appropriate for the evaluation of scientific research, such as those previously applied in the validation process for publication in academic

¹⁷ Ravetz 1971, p. 44. A largely similar claim is made by Hagstrom: "The traditional forms of co-ordination in science are analogous to medieval forms of economic organization. Free collaboration is similar to the partnership, and the professor-student relationship is similar to the master-apprentice relation... Just as the modern corporation has supplanted free partnership and apprenticeship in industry, so a more complex form of organization may be supplanting free collaboration and the professor-student association in science. Both changes involve the development of a more complex form of organization, the separation of the worker from the tools of production, and greater centralization of authority." (Hagstrom 1965, p. 140). However, this analogy between academic science and 'medieval forms of organization' should not be taken too far: Ravetz, for example, also emphasises the differences between modern (academic) science and pre-Renaissance science, with respect to the latter's reliance on appeals to authority, tradition and so on (Ravetz 1971, p. 247).

¹⁸ But Ravetz does not deny the possibility of some such alternative system; rather, he emphasises the historical specificity of the ways in which scientific enquiry has been conducted, and denies that there is any uniquely best way: for example, he notes the organisation of science during the 'academic' period, which began in the nineteenth century, differs markedly from that of the preceding period, beginning with the scientific revolution - which was itself no mean performer. But he denies that whatever form of organisation 'happens to emerge' will be successful, or that any form that *is* successful is guaranteed to survive.

¹⁹ For Ravetz's distinction between criteria of adequacy and of value, see especially ch. 5. Unlike many defenders of academic science, Ravetz does not insist that criteria of value must be exclusively 'internal' to science, thereby ensuring its autonomy from society. But he objects to the specific character of the 'external' criteria of value imposed on science in its 'industrialised' form. For criticism of Ravetz's conception of industrialised science, see Ziman 1984.

journals. The published research report is increasingly displaced as the primary form of scientific communication, since there are other, and more effective ways by which the attention and favourable opinion of those with influence over the allocation of resources may be secured. Thus informally circulated preliminary reports, abstracts of papers yet to be written, publication of conference papers in loosely edited collections, newsletters and the like, become increasingly common, all of them evading the previous system of quality control.²⁰

These changes, Ravetz claims, are mirrored by a shift both in the conception of a successful scientific career - "from being a series of successful research projects made possible by a parallel series of adequate contracts, to being a series of successful research contracts made possible by a parallel series of adequate projects" - and in the nature and location of a scientist's intellectual property:

"... under the old system it was fundamentally the published research report that □ constituted his property; on the basis of the informal evaluations of it by his colleagues, he expected appropriate rewards in his career, and the personal satisfaction produced by public recognition of his work. In the present situation, the research contract is not merely a prerequisite for the future possession of the property embodied in a published paper; it also brings immediate benefits in itself, in the way of prestige and possible material conveniences... Hence the location of a successful scientist's property tends to shift from his published results to his existing research contracts, and the personal contacts that will secure their continuation."²¹

Correspondingly, there is also a change in the bases upon which recognition is conferred, and in its relationship to other forms of reward. In academic science the primary basis of recognition is the evaluation of scientists' work through the peer-judgment process of publication, with citations, prestige and other rewards such as tenure and promotion being contingent upon the outcome of this process. But in industrialised science, these relationships are radically altered: prestige and status come to be based on the material indices of success, especially the ability to secure research contracts, rather than the independent evaluation of the intrinsic merits of scientific work. Serious damage is thereby done to the system of quality control employed in academic science, and to the means by which it managed to harmonise the purposes of individual scientists with the achievement of science's collective goals.²²

²⁰ In a similar vein - though describing this as a feature of 'disorganised' rather than 'industrialised' science - Hagstrom claims that leading scientists spend more of their time on administrative and political than on scientific work; articles are planned before their findings have actually been arrived at; the same results are reported in several different places; trivial findings are given exaggerated importance; scientists come to measure prestige more by the amount of grants that they obtain than by the significance of their results, and so on: see Hagstrom 1965, pp. 140-54. Ravetz claims also that more sub-standard work gets published, in low-grade journals, since it has only to meet the interests of the publisher (commercial returns from library sales), the author (another item on the list of publications for the grant application) and the editor (prestige).

²¹ Ravetz 1971, pp. 45-6; cf. pp. 258-9. Ravetz goes on to say that when this happens, the person concerned is better described as a 'scientific entrepreneur' than as a 'scientist'. But this term is perhaps more suitably applied to a different form of scientific organisation which, according to some commentators, is itself displacing 'industrialised' science towards the end of the twentieth century, especially in areas such as biotechnology - namely 'entrepreneurial' science: see for example Etkowicz 1983 and Remington 1988.

²² However, Ravetz is not entirely negative about industrialised science. Thus he notes the merits of its less individualistic conception and conduct of scientific enquiry, the associated reduction in the frequency and

3 The exchange of information for recognition

Whether or not one accepts Ravetz's gloomy view of science in the latter part of the twentieth century, one can at least see how damaging it would be if the changes he describes were in fact to occur: in particular, if recognition were still to be valued by scientists but could be achieved without producing work which meets the appropriate standards. One can also see how this might well be the outcome of a situation in which the material resources for scientific activity are obtained in the way that Ravetz attributes to industrialised science. But this is not the only potential threat to the effective operation of this device for harmonising individual purposes and collective goals. A similar threat is posed, I shall now argue, if recognition is viewed by scientists as something directly to be pursued, with the work they produce being seen as a means of achieving this.

To examine what is at issue here I shall return to Hagstrom's account of the scientific 'exchange' of information for recognition. Hagstrom insists that the kind of exchange which scientists engage in is significantly different from the kind which takes place in *market* transactions, and hence more generally that the organisation of science differs radically from that of commercial activities. In doing so he draws on Mauss's anthropological account of *gift-exchanges* - noting, for example, that in these, unlike market exchanges, there is no contractual relationship between the parties involved - nor, likewise, any role for that 'generalised medium of exchange', namely money.²³

There is, however, another feature of market exchanges which also distinguishes them from (at least some kinds of) gift-exchange, but which Hagstrom does not consider. This is the fact that in market transactions, each party is assumed to be providing what they 'offer' purely in order to obtain what it is that they will receive 'in return'. In the case of information-recognition exchange, this market model would imply that a scientist who provides information does so *as the means of obtaining* such recognition. By contrast, a non-market model of (gift-) exchange would imply something rather different: that information is 'given' *with the understanding that* recognition will be forthcoming. In the latter case, although failure to provide recognition on the part of the person receiving information will give rise to disappointment and resentment, obtaining such recognition is not the direct aim or purpose of the 'gift'; in the former case it is.

Hagstrom, I suggest, fails to notice this difference and its potential significance. Instead, he implicitly attributes to those involved in gift-exchange the kind of motivation which, I have just suggested, belongs not to this but to market-exchange. In this respect at least, the two forms of exchange are wrongly assimilated. As a result of this, I shall argue, a serious problem arises in his account of how the exchange of information for recognition can support the effective conduct of scientific enquiry.²⁴

bitterness of 'priority disputes' between individual scientists, and its implicit acknowledgment of the complexity and inter-dependence of the processes leading to 'the production of knowledge': see Ravetz 1971, pp, 258-9.

²³ See also Barnes 1985 on the isolation of information-recognition exchange from generalised monetary exchange, so that, for example, recognition cannot be exchanged for money.

²⁴ It also makes Hagstrom's position more vulnerable than it need be to the economic 'reinterpretation' proposed by Latour and Woolgar (1984, ch. 5) - though there is probably little one can do, intellectually, to

This problematic assimilation is revealed in Hagstrom's response to what he regards as a significant *prima facie* objection to his account of science: that many of the scientists interviewed in the study upon which his book is based explicitly and repeatedly *denied* that they were motivated by the pursuit of recognition. Hagstrom adopts a two-fold strategy to deal with this difficulty. First, drawing on Mauss's work, he says that it is one of the norms of gift-exchange that donors should publicly and vehemently deny their interest in receiving anything in return for their gift. So one should not necessarily take such denials at face-value: it may simply be that it is seen as unacceptable to admit to an interest which is actually present.²⁵ Second, he presents what he sees as positive evidence that, despite such public denials, the pursuit of recognition does indeed motivate scientists to 'give information': namely, that they often *do* 'admit' to becoming upset and resentful when others make use of their work without proper recognition of this through citations, or even claim to have 'discovered' what is contained in it themselves.

But although this evidence indeed suggests that scientists have *some* kind of 'interest' in receiving recognition, it does not show that their purpose in providing information is to achieve this - that they are, in any straightforward sense, 'pursuing recognition'. One may perfectly well feel resentful in the absence of some appropriate 'return' for what one does, without its being the case that this is done in order to receive that 'return'. This is true, for example, in relationships such as friendship, where norms of *reciprocity* typically apply: one may (eventually) cease helping one's friends if they fail to reciprocate, but this does not imply that when one does help them, this is done with the aim of being helped by them. The 'giving' in such cases is, one might say, *conditional* - as distinct from the 'pure' gift, which is entirely *unconditional*. But it is not only unconditional gifts which are motivationally distinct from economic exchanges; so too are conditional ones.

So the evidence which Hagstrom cites to support his hypothesis that scientists, despite their public denials, are in fact motivated by the pursuit of recognition, is equally consistent with an alternative hypothesis: that they are engaged in what I have just called 'conditional' giving. That is, they are giving information *on the understanding that* recognition will be provided: this is why they are resentful when such recognition is not forthcoming, not because the achievement of recognition was their reason for giving information. Further, this latter hypothesis is itself straightforwardly consistent with - even supported by - their public denials that they are engaged in the pursuit of recognition. There is no need to 'explain these away' in the manner suggested by Hagstrom, since they do not constitute even a *prima facie* objection to *this* hypothesis, as distinct from the one with which, I have suggested, it is conflated by Hagstrom.

counter this kind of theoretical cynicism. See Hands 1994 for discussion of economic models in the sociology of scientific knowledge.

²⁵ In a much quoted passage, Hagstrom puts the point as follows: "In general, whenever strong commitments to values are expected, the rational calculation of punishments and rewards is regarded as an improper basis for making decisions." (Hagstrom 1965, p. 21). He illustrates this dictum by considering people's conformity to social norms such as the law. But what emerges from his discussion of this is actually a quite different point: that such norms will not in practice be effective if people's behaviour is motivated only by the fear of sanctions or the pursuit of rewards, as distinct from its not being socially acceptable for them to admit that this *is* what motivates their conformity.

But what is important here is not so much whether Hagstrom misinterprets the evidence he cites, but whether, if scientists *were* to regard providing information as a means of obtaining recognition, this would be inimical to the effective functioning of the information-recognition exchange device. I shall now argue that this might well be so: that the direct pursuit of recognition, by contrast with its receipt being seen as a condition for (continued) information-giving, is likely to be *dysfunctional* for science. I shall provide some evidence to support this from Hagstrom's own account of what he (rightly) regards as the undesirable phenomenon of scientific *fashion*.²⁶

Hagstrom's discussion of this occurs in the context of noting how the development of science leads to changes in the problems which are seen as important and hence worthy of research. As these changes occur, the 'prestige' of different areas of scientific work, and hence the possibilities for achieving recognition, likewise change; thus scientists, motivated by the pursuit of recognition, shift their research activities into the currently prestigious area(s). But there is a great danger here: that considerations of 'mere fashion' begin to operate, so that an area of work is seen as significant despite the fact that objectively speaking it is not. There is then likely to be a misdirection of scientific energies towards comparatively less significant problem-areas, and a corresponding failure to develop research in less fashionable yet more significant fields. As Hagstrom puts the problem:

"Those who follow fashions deviate from a norm of science, for they lack originality (by the very definition of fashion). They claim recognition from others not because of the intrinsic importance of their work but because of its extrinsic characteristics - the kind of people who do it, the novel techniques or instruments they use, and the financial rewards they receive. Deviation in this form may lead to more serious forms of deviation such as falsifying data or plagiarism, because the deviant has come to be less concerned with solving 'important' problems than with obtaining immediate recognition from others."(Hagstrom 1965, p. 178).

As Hagstrom notes here, following fashion means deviating from one of the norms of science, 'originality' - and also, one might add, from that of 'scepticism' - and is therefore something which no scientist could admit to publicly. But the problem is not what scientists can or cannot admit to, but what their presumed motivations will lead them actually to do.²⁷ The uncomfortable 'fact', for Hagstrom, is that it is precisely because they are motivated by "obtaining immediate recognition from others" - a motivation which, according to his account, ensures that scientists act in conformity with the norms of science - that they engage in activities which fail to conform to those norms, and are therefore at odds with the achievement of science's collective goals.

²⁶ A similar conclusion might be argued on the basis of Hagstrom's account of the *competitive* pursuit of recognition, which gives rise to problems of secrecy and the like (Hagstrom 1965, ch. II). For an argument to this effect see O'Neill 1998, ch. 11; see also Nelkin 1984, for discussion of the related issue of patents and intellectual property rights. But Mulkey (1977) doubts whether competition between scientists generates problems of secrecy to a significant extent.

²⁷ Having pointed to several such dysfunctional consequences of following fashion, and more generally of directly aiming to achieve recognition, Hagstrom unfortunately conducts his own discussion of the problem in terms of 'what can be publicly admitted'.

But why should the direct pursuit of recognition have such consequences? After all, it might be argued, to desire recognition for what one has done is presumably to wish its value to be acknowledged by others, and hence one will only find such recognition satisfying if one regards it as well-founded - as the outcome of an 'objective' judgment by others. But if this is so, it would be self-defeating to attempt to obtain 'recognition' if all that is thereby obtained is 'the favourable opinion of others', despite one's having no grounds for believing this opinion to be well-founded, or even good grounds for believing it is not. Yet this is presumably just what one would be doing by 'following fashion'.

But, as I argued in the opening section, in response to a similar claim about internal goods, the fact that a certain course of action may be self-defeating is no guarantee that people will not choose to engage in it; nor, if they do so, that they will eventually discover their error, since by the time this error becomes noticeable, they may well have lost sight of what they were initially hoping to achieve.²⁸ In the case of 'pursuing recognition', I would suggest the following: the more that people focus their efforts on achieving recognition - rather than on doing something for which they would nonetheless wish to be recognised, and hence feel resentful if they are not - the more concerned they will become with whether they can gain the favourable opinions of others, and the less so with whether such opinions are justified - even though they may have 'started' by desiring only objective recognition.²⁹ They thereby lose sight of their original aim, and find themselves satisfied instead by realising a different one.

From the standpoint of science, then, it would be preferable if Hagstrom's gift-exchange model took the form of what I have called 'conditional gift-exchange', so that information is provided *on the understanding that* recognition will be accorded, and not *as the means of obtaining* this. In other words, the assimilation of gift-exchange to market-exchange should be avoided. But for this to be possible, there must be something other than the desire for recognition which could motivate the provision of information. If recognition is to function only as a condition for, but not as the purpose of, such provision, there is something missing from the account of recognition which Hagstrom and Ravetz have presented.

4 The desire to make goods

²⁸ It might be objected that I wrongly assume here that unjustified recognition can quite easily be achieved: surely there are 'quality-control' procedures which make this very difficult? But those who make the judgments required by such procedures may be just as liable to the pursuit of subjective recognition as those they are supposedly 'controlling'. Cf Ravetz's comments on the need for good morale and leadership to maintain such systems, since without these: "... what good work is done will fairly soon be driven out by the bad. There will inevitably be some leaders who will strive only for instant prestige; the referees associated with them will take the hint and authenticate any property which will enhance or share in that prestige; and ordinary scientists will be under pressure to achieve their private purposes most cheaply by identifying the current fashions and producing passable results in their image... If corruption spreads to the highest level...then whole areas of science can become gigantic confidence-games, producing pseudo-property at a feverish pace, and resembling a stock exchange in a bull market rather than a collective endeavour on behalf of the highest human goals". (Ravetz 1971, p. 311).

²⁹ In this context one might note the following passage from Mitchell Wilson's novel, *Live with Lightning*, in which the fictional physicist Erik Gorin declares: "I *am* ambitious... It's not that I want to be famous or rich. What I want, what I burn for, what I'd give damn near anything in my life for, is to be good enough to deserve to be famous, as a scientist" (Wilson 1949, p. 103; quoted by Gaston 1978, pp. 13-14). My suggestion would be that this is a potentially dangerous motivational starting-point, prone to corruption.

I have argued that although recognition can perform an important function in harmonising individual purposes and collective goals in an institutional practice such as modern science, its ability to do so will be undermined when its achievement becomes the direct aim of the individuals concerned - when they engage, that is, in 'the pursuit of recognition'. What this suggests is the need to provide a different account of the 'place' which recognition might have in our motivational repertoire - one which is consistent with its beneficial function but which removes the potentially damaging effects of its direct pursuit. It is this that I shall now try to do.³⁰

Both Hagstrom and Ravetz assume that recognition is a source of human satisfaction, and its absence of dissatisfaction. But one need not regard this simply as a 'brute fact' of human psychology, or as a particular expression of the more basic desire 'to be well thought-of by others'. Instead, I suggest, one can think of recognition as something whose value and purpose derive from a quite different desire, namely to make, or to be involved with others in the making of, things that are *good*.

The following are examples of the 'objects' of this desire: cooking a tasty meal; finding a solution to a practical or intellectual problem; creating (and looking after) a beautiful garden; building a seaworthy boat; making a welcoming home; constructing a better organisational system, and so on. As these examples indicate, what is meant by 'making' or 'creating' does not imply originality or 'creative genius': for the most part, the creation of goods is a quite mundane and well-charted process, and none the worse for being so. As they also indicate, there is a great variety of different kinds of goods in the making of which one can be involved - in particular, there is no restriction to those of a 'material' character.³¹ This variety is in principle indefinitely large, though for the members of any given society only a specific range of goods-making activities will be conceptually and practically available.

People's desire to create (or contribute with others to the creation of) goods is reflected in the enjoyment and satisfaction they experience in doing and having done so. This is to be distinguished from their enjoying the development and exercise of the various skills and capacities they deploy in the making of these goods - though this is something which they may also find satisfying. It is also to be distinguished from their use and enjoyment of the goods themselves, when created. Further, the desire to create goods is distinct from the desire to contribute to the well-being of others who may use and enjoy them, though it

On the differences between 'objective' (or public) and 'subjective' (or private) recognition, see 'Colonisation by the Market' [Note 10 above].

³⁰ The account I shall provide is not the only possible one, and it may seem at times unduly austere, or somewhat utopian. But its psychological plausibility is, I believe, at least as great as certain others which have dominated recent discussions of recognition, and it has the advantage of showing how the flourishing of individuals can be consistent with that of institutions. Implicitly, much of it can be read as a criticism of Francis Fukuyama's view of recognition which, from the perspective developed here, is largely (with the exception of its linkage with the demand to rectify injustices) a depiction of recognition in its pathological form. See Fukuyama 1992 and, for a critical response consistent with the position I take here, O'Neill 1998, ch. 8.

³¹ It is arguably a defect of Marx's inspiring account of human labour (Marx 19??) that it is too strongly focussed on the production of 'material' goods; also that it tends to exclude, which I would wish to include, the sustaining or maintaining of goods which already exist. See Kamenka 1972 (especially ch. 11) for a particularly illuminating interpretation of Marx's account, strongly influenced by John Anderson's ethical theory (Anderson 1962).

may in fact motivate the production of goods which have this effect. The desire to make goods is thus not to be seen as an altruistic one. True, its realisation may often be 'costly' to the person concerned, requiring conduct which frustrates the satisfactions available to them from other sources. But their willingness to forego these does not derive from benevolence.

Having tried to delineate the nature of this desire, I shall now suggest how the need and demand for recognition may be generated. One can begin by noting that the desire to make goods is dependent for its fulfilment on the ability to determine whether what one has made is in fact good. Correspondingly, the satisfaction that comes from making what one believes to *be* a good - and it is only the *belief*, not the *fact*, that can generate the satisfaction - is dependent on the degree of confidence one has that this is indeed the case. But since one desires to make what is actually good, and not merely whatever one happens to believe is so, it will matter to one whether this confidence is well-founded. So the 'discovery' that one's previous belief in the good-ness of what one has made is ill-founded will remove or diminish any further such satisfaction. Whilst this in itself is a 'loss', it is to be preferred to satisfactions based on detected or detectable illusions.

But how is such justifiable or well-founded confidence to be achieved? Only, it would seem, by applying to what one has made the standards or criteria which, so far as one can see, are appropriate for the kind of good one is attempting to make. That there will generally be such criteria, upon which these judgments can be based, is a reasonable expectation, since the repertoire of available kinds of goods to be made is itself largely determined by the correspondingly available repertoire of social *practices*, a constitutive element of which is their respective standards for making such judgments.³² But although the existence of such criteria in principle enables one to make the relevant judgements oneself - after all, the Wittgensteinian argument against private languages does not mean one is incapable of correcting one's own grammar - one may often lack either the ability to do so, or the confidence that one has this ability or has exercised it correctly. So in most cases one will be reliant on the judgments made by others whom one has reason to believe are competent judges.

It follows that, in order to realise one's desire to make goods, and to derive the satisfaction that comes from (what one regards as) a well-founded belief that one has done so, one will typically require others to *recognise* that what one has made is good.³³ This may be termed 'recognition as *confirmation*'; the need for such recognition, given the desire to make goods, derives from essentially *epistemic* considerations.

³² Although the reference here is to MacIntyre's concept of practices, I would also wish to include what Joseph Raz calls 'social forms' (Raz 1986, chs 12 and 13). It should be noted that a practice's standards are not unchanging or unchallengeable. Nor does their application to particular cases possess the determinacy of an algorithmic procedure. See also 'Justifying the Market and its Limitation', in *Cultural Goods and the Limits of the Market*, London: Palgrave, 149-171, on the need to reflect on the value of a practice's goods from a standpoint beyond that provided by its own standards.

³³ Cf. 'Colonisation by the Market' [Note 10 above], on the need for public recognition as the basis for self-respect, as distinct from self-esteem; also Elster 1989 for a similar argument in the case of 'self-realisation in work' - though I am less optimistic than Elster about the possibility of *consumer* judgments providing such recognition.

But this is not the only form of recognition which may be required, or at least desired, by the makers of goods. They may also desire that *the fact of their having made* these goods be appropriately 'recognised' by others. What is being requested or demanded here is not confirmation but *acknowledgment*: what is at issue is not whether the thing they have made is indeed good, but whether *their having made* it is properly recognised by others. The manner in which such acknowledgment is to be made varies considerably, and may in some cases be accompanied by the expectation of some specific form of reward. But the underlying sentiment remains the same:

'Here is a good thing I have made; you should acknowledge that I have done so. So you should not pass it off as if it were made by you, nor use it to your own advantage without in some way sharing this with me, nor behave towards me as if this thing had come into existence by some quite other means. If you act in any of these ways, I shall feel unjustly treated and justifiably resentful - because my worth, as expressed through my making of this good, would not have been acknowledged, and because the efforts I have made in doing this, and the other potential satisfactions I have thereby foregone, would have been disregarded'.

On this account, then, one can distinguish two different forms of recognition. Both are related to the desire to make goods, but although they may in some cases be conferred through the same 'act of recognition', they differ both in the function they perform and in what exactly they confer. The first is a response to the *need for confirmation*, to meet the *epistemic* requirement for 'knowing' that one has succeeded in making something good. The second is a response to the *demand for acknowledgment*, which derives - broadly speaking - from one's sense of what *justice* requires. The absence of confirmation will, unless one is the super-confident, undermine one's confidence that what one has produced is good, and hence one's satisfaction in having done so. The absence of acknowledgment, unless one is saintly, will reduce one's willingness to continue producing such goods, or at least making them available to others.

Recognition as acknowledgment is thus closely related to what I earlier called, in my discussion of Hagstrom, the 'conditional' gift; conversely, the *unconditional* gift is unaccompanied by any demand for acknowledgment, since nothing at all is demanded of the recipient of such goods. It is by no means unheard-of for gifts to be given unconditionally, nor for goods to be made and offered with no expectation of acknowledgment. For there are saints in all societies, and everyone is capable of occasionally saintly behaviour. But, like the closely related phenomenon of commitment, it is neither commonplace in a sustained form nor, therefore, a reasonable expectation on which to base any institutional arrangement for the production of goods.³⁴

³⁴ Hagstrom quotes the following passage from Max Weber's 'Science as a Vocation': "If the young scholar asks for my advice with regard to habilitation, the responsibility of encouraging him can hardly be borne... one must ask...: Do you in all conscience believe that you can stand seeing mediocrity after mediocrity, year after year, climb beyond you, without becoming embittered and without coming to grief? Naturally, one always receives the answer: "Of course, I live only for my calling." Yet, I have found that only a few men could endure this situation without coming to grief." (Hagstrom 1965, p. 22, quoting Weber 1946, pp. 132-4. I take it that Hagstrom's view, with which I would concur, is that if recognition really were distributed as 'unjustly' as Weber implies, one could have little confidence in science's ability to achieve its goals - unless everything could be achieved through the efforts of the saintly few alone.

Or at least it is not so for the members of modern societies, whose strong sense of individual identity makes it troublesome for them to produce goods without acknowledgement of their having done so, and of all that that entailed for them. The situation would no doubt be different in pre-modern societies - if standard accounts of these can be believed - or in any social group whose members lacked this sense of 'separate identity', possessing instead so strong a form of shared identity that they could not intelligibly demand acknowledgement for 'their own' contributions. But this is not the form of self-identity typical of, for example, the members of a modern scientific 'community'.³⁵ Hence recognition as acknowledgement is likewise closely related to that 'peculiar form of property' which Ravetz attributes to academic science, and which is itself a distinctively modern phenomenon in these respects. As Ravetz emphasises, individual scientists are seen as continuing to possess certain rights over what they have made 'freely available to others': "the community of academic science is not to be seen as 'a primitive-communist lay priesthood'."³⁶

The view of recognition I have presented here is, I believe, fully consistent with its operating in the ways required by Ravetz's and Hagstrom's accounts of the institutional organisation of modern ('academic') science. But by showing how the need for confirmation and the demand for acknowledgment arise from the more fundamental desire to make things that are good, the significance of recognition can be explained without having to assume that it is this which scientists are primarily aiming to obtain. Thus the dangers which I have identified in the direct pursuit of recognition are avoided.

Further, while the satisfaction generated by making goods has much of the motivating potential rightly attributed to 'intrinsic' satisfactions, it avoids the difficulty noted earlier that the enjoyment of these may often be associated with activities which fail to contribute to realising the collective goals of science. At the same time, reliance on this desire to make goods is by no means as risky as relying simply on commitment as the means of harmonising individual purposes with these goals. And the account I have suggested strengthens the effectiveness of recognition as a harmonising device by pointing to the epistemic need on the part of individuals who desire to 'create scientific goods' for the confirmation provided by institutional systems of quality control.

I would not, however, wish to claim that goods-producing institutions such as science can rely on this motivation alone, combined with devices of the kind depicted by Hagstrom and Ravetz. Thus my repeated scepticism about institutions which rely exclusively on commitment should not be taken to imply that they can do without this altogether; the same applies to the acceptance of various ethical constraints and the practice of certain virtues by their members.³⁷ Nonetheless, I would suggest that these further

³⁵ See also the discussion of pre-modern community and identity in section 5 of 'Markets, Firms and Practices', in *Cultural Goods and the Limits of the Market*, London: Palgrave, 111-132.

³⁶ Ravetz 1971, p. 41; cf. Note 15 above. Thus Ravetz implicitly modifies Merton's scientific norm of *communism*: interpreted straightforwardly, this would imply the absence of any such rights, and hence a conception of the published research report as what I have called an unconditional gift, at least in modern societies. (Likewise, both Ravetz and Hagstrom may be seen as modifying the Mertonian norm of 'Disinterest', since they assume the pursuit of an 'interest' in recognition - but regard this as consistent with, rather than damaging to, the successful realisation of science's institutional goals).

³⁷ Ravetz himself puts considerable emphasis on the need for ethical commitments, especially on the part of 'leading figures' in the scientific community: see Ravetz 1971, ch. 11, and also Note 14 above. Similarly MacIntyre insists that the acquisition and exercise of certain *virtues* are essential if practices are to resist the

requirements can themselves most effectively be met when the ethical commitments involved are themselves 'grounded' in certain attitudes and sentiments which, whilst not necessarily generated by the desire to make goods, may be seen as developing quite naturally from this.

What I have in mind here is that the desire to make goods, and the satisfaction that comes from doing so, may come to be associated with attitudes of care and affection both for the goods themselves and for the institutions and practices which make their production possible. We may then find ourselves feeling angry if they are abused or damaged, relieved when they are rescued or restored, delighted by their progress or development, and so on. Such sentiments indicate a kind of concern which is focussed neither on ourselves, and the contribution we have made to these goods and practices, nor on others, and the benefits they may derive from them.

To the extent that we experience such feelings, we will be disinclined to act in ways which run counter to the ethical norms required by goods-making institutions. Of course, we will still find ourselves tempted to do so. But we may hope that the attractions of such conduct will often enough be over-ridden, not just by the fear of sanctions, nor by ethical conviction alone, but by our sense of attachment to the goods themselves and to the institutions through which they are created and sustained.³⁸

potentially damaging effects of the institutions which they nonetheless depend upon: "... institutions and practices characteristically form a single causal order in which the ideals and creativity of the practice are always vulnerable to the acquisitiveness of the institution, in which the cooperative care for the common goods of the practice is always vulnerable to the competitiveness of the institution. In this context the essential function of the virtues is clear. Without them, without justice, courage and truthfulness, practices could not resist the corrupting power of institutions". (MacIntyre 1981, p. 181).

³⁸ The point here is not just that 'ethics work best when supported by sentiments', but that such sentiments may quite properly be directed towards objects and institutions, not just towards people.

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