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The article offers a critique of the proportional epistemology shaping political and economic theory about the public value of knowledge and opens up the potential for alternative descriptions afforded by ethnography. It does so by exploring one particular exemplification of the new public value of knowledge found in political calls for making Science and Society converge. Such convergence takes at least three forms: the public value of research; the economic public goodness of commercial science; and the public accountability of science as a trustworthy enterprise. I pursue this interest through an ethnography of the production of research among historians of science and philologists at Spain's National Research Council (CSIC). My concern here is to describe the epistemological economy of research at CSIC and provide an account of the terms of engagement through which researchers make sense of and relate to the social conditions of their own work.

KEYWORDS: public knowledge; proportionality; political anthropology; science and society

Walter Lippmann's famous critique of 'the public' as a proxy for democratic accountability (Lippmann 1993[1925]) makes for an uncanny reading in these times of overabundant knowledge. Lippmann's uneasiness with the public derived from his view that democratic robustness was ultimately an 'unattainable ideal', especially if constructed around the 'ideal of the sovereign and omnipotent citizen' (Lippmann 1993[1925], p. 11) For Lippmann, the idea of democracy could not be made to stand on the notion of an absolute and transparent command of all possible knowledge on the part of the citizenry. Therein lies the 'mystical fallacy of democracy', because an average citizen 'cannot know all about everything all the time, and while he is watching one thing a thousand others undergo great changes' (Lippmann 1993[1925], pp. 28, 15). In fact, Lippmann insinuated that it remains doubtful that knowledge, politics and the social are indeed correlates or commensurate with each other, for 'the problems of the modern world appear and change faster than any set of teachers can grasp them'. Thus, any attempt at making knowledge into a politically relevant object is 'bound always to be in arrears' (Lippmann 1993[1925], p. 17). The political hopefulness that informs the notion of public knowledge is thus understood as the malaise lying behind all theories of democracy.

Over 80 years have passed since Lippmann published his text yet the terms of his analysis – knowledge, the public and the critical analysis of society – continue to underpin our political imagination. Whatever our politics looks like, it is always an expression of some sort of proportional equivalence or balance between knowledge and the public. Thus, in a recent intervention on the new knowledge politics of the Internet age, Mike

Featherstone and Couze Venn have argued for a reconceptualization of public life as an autopoetic, vitalistic and self-generative politics:

> If a public involves self-organizing open communication amongst strangers, then there may be many emergent forms of public life. Rather than the model of the global public sphere, it can be argued that it would be more appropriate to think of 'global public life', with the displacement of the term 'sphere' by the term 'life' suggesting the difficulty of separating politics and aesthetics, and cognition and affect. The accent on life, furthermore, points to the potential for information to be conceived as *alive*, as an autopoetic system, or as a complex multiplicity which does not necessarily behave and act as a docile tool but rather is worldling, inventive and generative. (Featherstone & Venn 2006, p. 11)

In a similar vein, Clay Shirky, writing for the Internet colloquium space *The Edge* about the 'cognitive surplus' that is inherent to the web makes the following observation a propos the creation of a Wiki Map for crime in Brazil:

> If there's an assault, if there's a burglary, if there's a mugging . . . you can go and put a push-pin on a Google Map, and you can characterize the assault, and you start to see a map of where these crimes are occurring. Now, this already exists as tacit information. Anybody who knows a town has some sense of, 'Don't go there. That street corner is dangerous. Don't go in this neighborhood. Be careful there after dark.' *But it's something society knows without society really knowing it, which is to say there's no public source where you can take advantage of it.* (Shirky 2008, emphasis added)

Notwithstanding Lippmann's caution, for these authors the construction of political critique still requires the commensuration of the social with the economies of knowledge. If we want to keep apace our understanding of the new political economy of knowledge, they suggest, we need to enhance and multiply the public purchase of our political imaginations. More knowledge, more publics, better politics.

This article explores the proportional epistemology that lies behind the political imagination of the new knowledge economy (see also Corsín Jiménez 2008b). It asks what is at stake in making knowledge, the social and its forms of economy, including critique, into correlates of each other. It describes some recent attempts in political and economic theory and policy-making that aim to stop the political economy of knowledge from 'being in arrears' with respect to the social. In doing so, the article fleshes out the terms of the anthropological imagination that has enabled the experience of critique to be consistently conceptualized in terms of just such a *différance*. A central aim of the article, then, is to open an inquiry into the ways in which knowledge and its forms of economy are turned into socially productive forms today.

The article is in two parts. In the first I explore the political and anthropological epistemology that makes economy, knowledge and the social liable to theoretical commensuration in the first place. The second part of the article provides an example of the contemporary status of the public as a mediator of society/economy relations. In particular, I look at the political investment that has gone into making European scientific research publicly and economically valuable. I pursue this interest through an ethnography of the production of research among historians of science and philologists at Spain's National Research Council (CSIC), where the humanities and social sciences have recently undergone an important disciplinary and organizational transformation in the

name of public value. I examine how the public value of scientific knowledge is indigenously conceived by those who practise it and whether proportionality plays any role in shaping the imagination of how knowledge works. The ethnography shows the extent to which researchers' understanding of the production of scholarship differs from managerial understandings of the production of knowledge. This difference is premised on an anthropological epistemology where proportionality and horizontal relationality play no significant role. In their stead, the ethnography casts its own sets of categories, which focus on the historicization, epistemic reflexivity and reversible movements of knowledge.

A Political Epistemology of Knowledge

There is a wonderful passage in Deleuze's lectures on Leibniz's philosophy where he describes the playfulness of concepts. 'Concepts', he writes, 'are always the subject of movements, the movements of thought' (Deleuze 2006[1980/1986/1987], p. 109). To illustrate the variety of playful movements that concepts are capable of, Deleuze contrasts Leibniz's and Kant's theories of knowledge. Epistemologically, this boils down to the difference between analytical and synthetic models of knowledge. For Leibniz, concepts are intensive movements: every concept contains the world within, we just need to probe deep inside it and learn how to disentangle it. For Kant, the nature of knowledge resides outside concepts. The trick here lies in learning to say something relevant about a concept: to look outside the concept for the things that may belong to, or are of interest to the concept. For Kant, as Deleuze puts it, 'to know something is always to spill over the concept' (Deleuze 2006[1980/1986/1987], p. 115).

The idiom of a 'spill over' has recently gained political currency due in part to the redefinition of economic public goods as 'externalities' (Cornes & Sandler 1996). Much like Kantian concepts do, public goods, economists argue, have a tendency to flow over their market circumscriptions, delivering their 'goodness' beyond their original catchment area. An example is 'knowledge', whose qualities make it the travelling commodity par excellence. As Dominic Foray puts it, knowledge is a 'fluid and portable good' which 'as soon as it is revealed . . . slips out of one's grasp' (Foray 2006, p. 91). The slipperiness of public goods impregnates them with a sociology of horizontality: they radiate and create relations wherever they are deployed. Foray illustrates this radiation eloquently with the example of a musician who provides a positive externality to her neighbour who loves music (Foray 2006, p. 92). Brett Frischmann and Mark Lemley describe this relational effect when defining spillovers as 'uncompensated benefits that one person's activity provides to another' (Frischmann & Lemley 2006, p. 2). In this sense, although originally coined to give an idiomatic expression to an economic phenomenon, the notion of spillover may well fare as a sociological theory in its own right (see Callon 1998), because one may imagine sociality as forever spilling over each and every one of its social moments: my ideas or actions spill over your ideas or actions which in turn spill over someone else's ideas or actions, and so on. If we were to push the metaphor to its analytical limits, one could even think of society recurrently spilling over itself as a total whole.

The political implications of such a 'spillover sociology' are worth noting. For a start, one may speak of society *as a whole* as a public good, for sociality itself depends on this positive recursive entanglement of people and things. An apposite label for such a sociological image of society would be 'Utopia, minus', because our starting point would

be one of wholesale public goodness, from where we piecemeal subtract, one by one, relations and externalities when and if they are not positive, or when and if they must not be divulged. If the walls that separate a musician from her neighbour are too thick, sociality stops short of spilling over itself one more time: we find ourselves, then, one relational effect away from our utopian whole. Not surprisingly, it this subtractive imagination that leads economists to speak of 'internalizing' externalities when the relational effects are unknown or unwelcome. The classic example of an externality 'internalized' by the market is that of intellectual property rights. The total public good is here contracted down into a private figure; the utopian whole scaled down and replicated in a partial form.

As both an insider and outsider to its own political imagination, the notion of the public belongs to a tradition of political arithmetic whose sociology has gone through some remarkable transformations in the past fifty years. This is the political epistemology of parts-and-wholes. It was of course Mannheim's larger point in *Ideology and Utopia* that the incommensurable exchange of parts for wholes and wholes for parts characterizes the political culture of democracy in modernity (Mannheim 1952). Charles Turner has neatly re-described Mannheim's argument in terms of this play of excesses of parts and wholes over each other:

> Although the idea of democracy is consistent with the domination of the analytical outlook, in which all parties and groups would be forced to see the political and social world as a structure of elements rather than as an indivisible whole, it is precisely as an indivisible whole . . . that both ideological and utopian thinking are wont to view either the society they wish to preserve or the one they wish to see come into being . . . For each, a vision of the whole . . . obscures insight into current reality which can only be grasped analytically. (Turner 2003, p. 36)

What is remarkable about Mannheim's imagination of society as a spillover of parts and wholes over each other is that it constitutes the founding moment of the sociology of knowledge as an intellectual project. Although much criticized because it failed to take account of its own moment of political articulation – of its own consciousness as a sociology of knowledge, as Adorno put it (1974) – Mannheim's re-versioning of society into a refraction of parts and wholes sets the stage for thinking about society in terms of the political organization of knowledge. Moreover, for Mannheim, the political field wherein knowledge gains currency as an object of sociological consciousness belongs of necessity to the tradition of political utopianism. The sociology of knowledge thus takes its democratic cue in Mannheim from its location in a programmatic version of political hope.

There is a long tradition in Western philosophy and narrative that senses the sociological import of knowledge as a vehicle of political hope. Russell Jacoby cites Aristophanes' play *The Birds* to this effect. He recalls Aristophanes' description of the arrival of a mathematician to Cloud-cuckoo-land, the utopian community that figures centrally in the play. The mathematician arrives with the hope of settling there, declaring loudly that his joyful plan is to 'subdivide the air into square acres'. But his programme appeals to no one, and he is advised to leave and 'subdivide somewhere else' (Jacoby 2005, p. 40). Thus, for the inhabitants of Cloud-cuckoo-land, the subtractions and subdivisions that (mathematical) knowledge is capable of carry no political purchase.

Not so for Mannheim, and certainly not so for some of his most noted generational peers, such as Friedrich Hayek or Karl Popper (Hull 2006, p. 149), who had no doubts about

the place and rank that knowledge ought to occupy in the political organization of society. Popper, for instance, made it clear that there was no point in trying to apprehend society as a total whole, let alone intervene on the basis of such holistic intuitions, which amounted to nothing more than 'utopian engineering' and could only lead to totalitarianism. Knowledge of society resided in the details, in the parts, not the wholes (Popper 1960, pp. 66–67, 78–79). For Hayek, knowledge was also a matter of details, distributed across society as circumstantial evidence, embedded in times and places, so frail and elusive that it is often, especially among those scientifically minded, taken for non-knowledge, 'apparently because in their scheme of things all such knowledge is supposed to be ''given''' (Hayek 1945, p. 522).

Although Hayek does not employ the language of utopia to describe the arithmetic – the alignment of parts and wholes – of his ideal society, it is nonetheless true that this is still imagined in terms of political distribution: a distribution that, in Hayek's vocabulary, is self-evincing, that comes into its own so long as the distributive mechanism is the price system. This is a political organization that it is *ideal* because it is *efficient*: capable of providing signals of its movements and displacements, agile enough to capture its own advances. Hayek develops, then, a model of economic sociology that works, as he puts it, 'as a kind of machinery for registering change, or a system of telecommunications which enables individual producers to watch merely the movement of a few pointers . . . in order to adjust their activities to changes of which they may never know more than is reflected in the price movement' (Hayek 1945, p. 527).

The elegance of Hayek's solution to the problem of the consciousness of the 'sociology of knowledge' – the problem of having mathematicians around who think society will be better off if we subtract, subdivide or re-divide it into different parts and wholes – is that it turns mathematics itself into the tool of consciousness. Society *is* its subtractions and sub-divisions, and knowledge *is* the fractions and remainders that are thrown up by every mathematical operation, by every re-division.

Although to my knowledge Deleuze never commented on Hayek's solution to the problem of the sociology of knowledge, I think it is fair to say he would have in all likelihood applauded its formal brilliance. For Hayek develops a concept of society that is at once Leibnizian and Kantian, intensive and extensive, analytical in its efficiency and synthetic in its expansiveness. Hayek's society moves both inwards, holding firmly to the knowledge that it possesses, and outwards, signalling to those places which have the knowledge it needs to incorporate. The brilliance of Hayek lies in having developed a sociological imagination that aligns Society, Knowledge and Politics in one unified, homological body, a body that responds automatically to its own insufficiencies, that communicates internally its external needs: a body that has internalized its externalities.

Hayek's outline of a sociological intelligence that is self-reflexive, that can signal and communicate about its own deficiencies and displacements, has become common place in contemporary social theory. Nigel Thrift, for example, has argued that the rise of knowledge as a sociologically reflexive datum points to capitalism's present systemic capacity to reorganize around the 'whole of the intellect' (Thrift 2006, p. 296).

The problem of the 'intelligence' of knowledge is, of course, a problem in anthropological economy: a problem in the proportional imagination that we use to populate our social theory. An example of what I mean by proportional imagination is provided by Simon Schaffer's (1994) account of the connection between the appearance of 'intelligence' as a problem in economic theory and the philosophy of machinofacture

that took over the technological imagination of labour in the early 1800s. For the advocates and philosophers of manufacturing, 'the surplus value extracted from the machines was the product of the intelligence of capital made real in the force of steam-driven engines'. For socialists, on the other hand, the factory system 'used, and assumed, the image of the human body as ''living machinery''' (Schaffer 1994, p. 223). Thus, whilst the former populated their sociological theories with, and created homological alignments between, the concepts of intelligence and surplus value, the latter imprinted their sociological imagination with notions of process, labour and human value. Machinofacture thus levered and re-proportioned 'intelligence' as a source of value against human labour.

Much like it did in the nineteenth century, the proportional imagination works today by miniaturizing or aggrandizing its terms of description. Knowledge is one such term currently in favour – one which, in Hayekian fashion, grows by homological expansion: by turning into knowledge everything that lies outside its compass; internalizing externalities. Knowledge captures knowledge which captures knowledge. Paul David calls such modality of self-aggrandizement the 'infinite expansibility' of knowledge (David 1993, p. 28). In this guise, my interest here is in the observed tension in liberal societies between knowledge and the economy, and their respective nesting and interfolding through relations of proportional aggrandizement and/or miniaturization. To better understand some of the terms through which our social economy of knowledge marks its own process of magnification today, I look now at how historians and philologists working at Spain's National Research Council conceive of their own knowledge-making practices and how they view their own work against the larger political economy of global science.

More Knowledge, More Publics, Better Politics

CSIC is Spain's national elite research institution, comparable to France's CNRS or Germany's Max Planck network of research institutes. The Council employs some 11,000 people across Spain, although the headquarters and major cluster of activities are in Madrid. The Centre of Humanities, where I was based between June 2006 and June 2007, employs some 200 people, between faculty and staff. It is made up of three institutes, Philology, History and Spanish Language, each accommodating a variety of academic departments. In the two departments where I was located I spent most of my time with two research groups. In Philology, I worked with the 'Jewish Medieval Culture' group whose research focused on the historical heritage of Sefarad Jews and the cultural exchanges between Jews, Muslims and Christians. This concerned the study of Jewish Biblical and medieval language and literature, as well as work on medieval Jewish science and philosophy, in particular astronomy and medicine. Philologists' methods of work relied for the most part on the codicology, palaeography and study of ancient manuscripts.

At the History of Science department, on the other hand, I spent most of my time in the company of the 'Science, Politics and Empire in the Sixteenth–Nineteenth Centuries' research group whose work focused on the study of the 'forms and practices of the production and circulation of knowledge in the context of globalization' and the 'history of the Iberian empires in the context of globalization and the New Atlantic history' (Plantilla para el resumen del Plan Estratégico de los grupos de investigación del CCHS. Instituto de Historia: Ciencia, Política e Imperio, s. XVI–XIX, p. 1). I came especially close to some of these researchers, because of our common interest in the history, organization and practice of science and knowledge.

In what follows I examine the historical and political discourses, material conditions, and narratives and styles of reasoning by which historians of science and philologists at Spain's CSIC negotiated and made clear to themselves the public value of their own work (see also Corsín Jiménez 2008a). I use the word 'public' here advertently and with prudence, for two reasons. On the one hand, because the Council, as we shall see shortly, was at the time caught up in the global debate over the accountability and usefulness of science that Helga Nowotny and her colleagues have described so pertinently (Nowotny et al. 2005). Secondly, because prompted by this larger debate, the Council's researchers started to look inwards into the epistemological economies that, in their eyes, made their own work robust.

As regards the first of these matters, I will not say much. There is now an established literature describing the political forces that have ushered science into new kinds of democratic relationships with civil society and industry (Fuller 2000; Irwin & Michael 2003; Jasanoff 2007; Nowotny et al. 2001). As Nowotny has observed, having left the shadow of the state, science's capacity to control its own political location in the knowledge economy depends on its ability to define the 'public interest' of its enterprise vis-à-vis the market and civil society (Nowotny 2005, p. 17). The major forces pressing for a redefinition of the scientific endeavour are the rise of private investment and the introduction of shareholder values in research (p. 11). The stakes are high, for the utopian appeal of knowledge as an economic good circumscribes our political imagination. As she puts it, 'overlapping and conflicting interests and claims, rights and contractual relationships, operating under market and non-market conditions, under the shadow of monopolies or in the utopian mood of free access to other users, constitute and populate the public domain' (p. 23).

Nowotny's own answer to this avalanche of challenges and pressures is to look inwards into science's own strategies of accountability. Science has always been a public venture. Historically, the demands on trustworthiness placed by the witness- and peer-review models, and the gift-exchange economy of scientific knowledge, show that science has always carried its publics within (p. 5). The ethnography that follows, then, attempts to discern the contours of this 'internal' public that philologists and historians at Spain's CSIC set in motion when imagining the institutional spaces for their own academic work.

My arrival in CSIC coincided with an important moment of organizational change in the institution. The landmark event of this transformation was the establishment of a new Centre for Human and Social Sciences (hereafter CCHS). The Centre, which opened in November 2007, brought together in one site most of CSIC's social sciences and humanities (SS&H) departments in Madrid. The Council invested c.25 million euros in the building, which has an area of 60,000 m^2 and hosts what people refer to as Europe's largest library in the SS&H (Plan Estratégico CCHS 2007, p. 3). Some 650 people, between faculty, staff and postdoctoral researchers, moved to the new building.

CCHS is the flagship of a series of changes intended to transform the very way SS&H scientists engage in the production of research. For a start, following relocation academic departments were closed down and reorganized anew into 'Research Groups'. In this scenario, some Institutes took the opportunity to change names. The Institute of Philology, for instance, voted a change of name to the Institute of Mediterranean and Oriental Studies, whilst the Centre on Comparative Politics re-branded itself as the Institute of Public Policies, Resources and Goods. The change of names points to a larger strategic agenda of the Council to develop 'interdisciplinary' and 'transversal' research activities that aim to 'produce and transfer research outputs that will contribute towards the creation of

a knowledge society.' The idea is to establish the CCHS as an emerging institutional 'node' capable of 'coordinating research efforts that might otherwise remain dispersed.' Such demands for 'cooperation' in the world of international research are spoken of as evidence of the need for a new culture of 'transparency' and for an 'objective' and auditable culture of decision-making processes (Plan Estratégico CCHS 2007, pp. 3, 5).

The language of 'audit', 'transparency', 'interdisciplinarity' and 'transversal' research activities resonates with recent political rhetoric in European science policy circles (Jasanoff 2007). As the strategic plan for the new Centre puts it, 'Research Groups are to become the system's essential actors, so that every discipline, programme or research line will become visible through the groups' activities' (Plan Estratégico CCHS 2007, p. 6).

Although working in groups was something researchers had long been doing in practice, the explicit call by management to re-organize the administrative and corporate structure of the institution around newly-founded Research Groups caused much bewilderment among researchers. Academics were unclear about how many people and what 'kinds' of people made up a group. How far should one go in defining a group's remit: a historical period?, a collection of manuscripts?, a regional area? What kinds of scholars could be included in a group: doctoral students? postdoctoral researchers? foreign collaborators? And what about the collaboration with foreign groups: could one belong to many groups, some intramural, some international? Could one take up an invitation to join an international research group without compromising one's own work 'at home'?

In January 2007, at a faculty meeting of the Institute of History, these and other concerns were publicly aired with management. Widespread dissatisfaction was expressed at the way in which management had arrived at the definition of a 'research group'. Researchers complained that their views had not been taken into consideration. There was also much discussion about whether foreign scholars could or could not form part of a group. Rumours circulated that management was considering requesting 'exclusivity' from foreign scholars, a possibility which many thought 'absurd' and indicative of management's 'delirious grandiloquence'. Management made it clear at the meeting that the question of exclusivity was important because some departments were using the research output of foreign associates to inflate their own production figures. The director of CCHS described the issue as one of accountability:

> We need to be clear about who produces what. If you do research outside and it does not get counted here, that is a problem. Conversely, if an external researcher produces for us but we cannot count it as our own, that is a problem too. The point is that we need to know when such crossovers happen. We will not accept double accounting. And the only way to prevent it is by establishing some terms of exclusivity. Then, if people want to cross over, it will have to be on the basis of some pre-established terms of collaboration, between their institution and the Council. That is our ultimate goal: to define and route the circulation of knowledge at an institutional level.

The discussion prompted some interesting insights. For example, for management the production of knowledge centred on questions of location and volume: a research group thus provided an institutional circumscription for both. For researchers, on the other hand, the organization of research was a matter of epistemological economy: which objects (manuscripts, scholars, images, funded students) could be brought together in productive fashion. The distinction rehearsed a well-noted difference between management and

research approaches to knowledge, where the former is taken as a given output and the latter is envisaged as an open-ended process (Strathern 2006).

The discussion at the faculty meeting further prompted management to draft a document on the 'conceptual and practical issues involved in the setting up of groups', which was circulated by email a few days after the meeting. The document explained that research groups were 'voluntary associations of researchers' who in agreeing to work together were making an 'implicit contract' between themselves. This contract would further stand as a 'professional contract' binding researchers to the commitments and obligations therein assumed. The document also specified that although foreign scholars could indeed sign up to a group, they should do so on the understanding that their dedication would have to be exclusive to the group, and that their own academic production would undergo the Council's audit and evaluation systems. In this sense, the document stressed the distinction between a 'group' and a 'network' in order to emphasize that not all 'external relations' should be counted in the definition of a group (Aclaraciones conceptuales y practices sobre los Grupos de Investigación, p. 1).

Many researchers remained unconvinced, however. Both at the meeting and in conversations and exchanges throughout the year, they expressed their dissatisfaction with a strategic approach which they thought contradictory on a number of points. At the meeting, one historian observed that there was a contradiction between how groups were being told to get organized and the kind of work they were being encouraged to do:

> We are being told, on the one hand, to form large groups, groups with sufficient 'critical mass' to aim for big projects, to compete at an international level. On the other hand, we are being told that we need to establish ourselves as 'leaders', as scientific entrepreneurs. But leaders lead groups, and you can't have many groups if we are all leading. This is inconsistent. You can have one or the other, but not both.

Another researcher observed that there was a blatant contradiction in the call for researchers to join international networks of excellence and yet prohibiting foreign scholars from joining the Council's groups unless they committed to the terms of exclusivity. He gave his concern an ironic twist:

> So you want us to get involved with the best 'out there' but you will not allow them to collaborate with us, unless it is under such prohibitive terms? Well, I doubt very much it's going to work, to be honest. I just cannot imagine a Cambridge professor giving up his networks of excellence in order to work with *us* . . . In fact, would the Council do the same for us? Would they give Cambridge exclusivity on our work with them? No they wouldn't.

In calling for a reorganization of research in terms of 'research groups', management's conviction was that it was necessary to move beyond a culture of 'individualism' in SS&H research. When first encouraged to re-organize themselves into groups, in May 2005, 184 researchers got together in 78 groups (at an average of 2.4 members per group), whilst 69 researchers did not even make the effort to affiliate themselves with a group (Plan Estratégico del Área de HH y CCSS 2006, p. 94). The reference to groups' obtaining a certain 'critical mass', then, pointed in this direction: the larger the group, management held, the easier it would be for groups to develop a new culture of cooperation and collaboration, and the greater the chances for 'internationalizing' its activities and projects.

Whilst management recognized the significance, and in certain respects uniqueness of autonomous scholarship for SS&H research (Plan Estratégico del Área de HH y CCSS 2006, p. 96), there was also a sense in which they felt it was important to break away from the culture of individualism that characterized the institution. An underlying assumption was the idea that larger groups would not only stimulate productivity but would further deepen the relations between scholars and the general public. Larger groups would be better equipped to tackle some of the more complex sociological issues of the day; apparently, they would also tend naturally towards interdisciplinarity, which the Strategic Plan always spoke of as a positive outcome (see Strathern 2004). Again, the implicit assumption here was that 'more people, more knowledge, more publics, better politics'.

In the context of this programmatic transformation, the concept of 'individualism' carried profound historical baggage. When invoked in official discourse, it was associated with solitary scholarship, which was associated in turn with an indifference towards, even scorn of institutional audits; a refusal to do collaborative work, on the grounds that it was an inferior type of scholarship; and, in some cases, an aristocratic and self-indulgent conception of the state, 'who [those who indulge in solitary scholarship] think is there to serve them, rather than them being in its service', as a senior scientist put it to me.

However, the way in which researchers entangled themselves in the bureaucratic apparatus of the state has a historical dimension that is worth commenting on. Towards the end of my fieldwork, the director of the new CCHS provided an overview of the contours of this institutional imaginary when talking about some of the difficulties and frustrations he had encountered in trying to persuade researchers of the need to embrace a new culture of work:

> I am certainly not against individual research. It's part of who we are and what we do. I mean, I do it myself; I have only just published a monograph that took me six years to research and write, on my own. It's a model of research that we were brought up to admire: those singular and extraordinary scholars who mobilized an entire field of knowledge on their own. But the model is in crisis, one way or another. Trouble is that it is sometimes being substituted for the 'culture of the trivial', for a 'culture of the spectacle' model, where serious research becomes a preamble of official politics. All this is true. But there are some among us who think we need to look for that equilibrium point between serious and rigorous scholarship and emerging social needs. This is certainly not an easy task. I would say more: it is incredibly difficult, and one we are not doing very well. But we need to do it nonetheless. In fact, that is what we get paid to do. We are not being paid to replicate the model of the 1950s and 1960s, which we all sure agree was wonderful – although, in fact, we surely know too that, of all places, this institution cannot pride itself of having a particularly glorious past in this respect. Don't you think? I mean, there were some notable exceptions. But on the whole we all know where we come from and what this institution was like in the 1950s, 60s and 70s. And if you don't know what is what like you can take a look at some of the books that are being published these days.

The senior manager is referring here to a recent surge of academic interest in the history of CSIC in the 1950s–1970s, a time marked by Franco's dictatorship. The first time I heard a reference to this historical period was during an interview with a senior social scientist who described the Centre of Humanities – the centre that hosts both the Institute of Philology and the Institute of History where I did my fieldwork – as a 'National-Catholic bureaucracy'.

I was taken aback by the term, although it would soon reveal itself as a key organizing trope in the imagination of the politics of knowledge inside CSIC. For this researcher, the 'National-Catholic' idiom aimed to capture an atmosphere of individualistic and conspiratorial politics. He described the research culture of the Centre as *rancio* and *casposo*, two words that have no direct translation in English but whose semantic field encompasses terms such as conservative, backwards, Francoist (reminiscent of the Franco years), bureaucratic or stagnant. He said of philologists that they were 'archivists' and 'essayists', obsessed with books and their own historical genealogies. They spent too much time aligning themselves with particular scholarly traditions, 'summoning their ghosts', as he put it.

The ghosts of the Francoist years haunted the politics of contemporary research inside CSIC. At the time of doing fieldwork, the Council was celebrating the 100th anniversary of the Junta de Ampliación de Estudios, JAE (Higher Studies Board), CSIC's institutional predecessor. The Board was established in 1907 with the objective of helping the country leave behind its cultural and scientific isolationism and link up with Europe's best. It funded new laboratories, research centres, and scholarships to study abroad. A senior scientist and former manager once told me that the Board could have been an emblem of Spain's 'Golden Scientific Age' had it not been brutally brought to a halt by the civil war. In 1939, when the war broke, Franco's government closed down the Board and used its permanent structures to found the National Research Council. During my time in CSIC people invoked the JAE and the cultural prosperity of the Spanish Republic, prior to the civil war, to position and align themselves with contemporary political decisions and strategies. The same senior scientist who spoke of a 'Golden Age' said that some people in managerial decision-making posts today were 'desperately trying to connect with the spirit of the JAE'. It is this historical spirit that the director of the new CCHS referred to when saying that 'we all know where we come from and what this institution was like in the 1950s, 60s and 70s.'

Some of the historians of science with whom I was working were sceptical of this revivalist spirit. They protested about the historiography and iconography used to praise the alleged Golden Age of institutional science in early twentieth century Spain. A common judgment of theirs was 'This is "heroic" science, a science of great men'. An example of such heroic historicizations took place in May 2007 at the Residencia de Estudiantes (a famous Hall of Residence founded by the JAE in 1910 and one of Spain's most prestigious cultural foundations today). The event showcased the screening of an original documentary film of 1929–1930 titled 'What is Spain?' that included footage of famous scientists working in their labs. The screening was followed by a round table discussion led by three canonical figures of Spanish historiography, science and film studies. The day after the screening, Marta, a postdoctoral historian of science who attended the screening, told us about the happening.

> I found it ridiculous. They presented this vision of Spanish Big Science before the war. But not even the images measured up to the ideal. You could only see individual men working in miniature labs. The story about these great men, pushing against all odds, is not credible. It falsifies the material conditions of research at the times, which were of extreme precariousness.

The uneasiness historians of science felt about the present historicization of the Council's past was reflected too in their appraisal of management's strategic agenda for a new

sociological projection of the SS&H, as represented in the plans for the CCHS. They read all documents pertaining to the move to the new building with great attention and a critical eye. They saw in the JAE rhetoric a political and historicist discourse used to legitimate certain changes in science policy, in particular the call for making humanities' research more 'socially relevant' through the use of managerially-inspired scientometrics. In this light, the calls for research groups and interdisciplinarity, for accountability and big science, were interpreted not simply as gross neoliberal politics but were located instead in a complex institutional landscape inflected by historical, social and organizational legacies. An intriguing example of historians' subtle epistemic reflexivity in re-interpreting the calls for making their science 'go public' is provided by the following ethnographic vignette.

Epistemic Reflexivity

In the fall of 2006, two young women postdoctoral students in the History of Science volunteered to organize a weekly 'Thursday's coffee' event to bring together colleagues from all departments in the Institute of History to meet informally to share ideas, anecdotes or recent work experiences. The postdoctoral students, who were both young and female, felt that relationships inside the Institute were a little departmentalized, if not reified and mannerist, with most people knowing next to nothing about the whereabouts and research interests of those working down the corridor.

A couple of these Thursday meetings were convened to share experiences of working in foreign academic environments. On one occasion, Luis, an assistant professor who had recently joined the department of Modern History from a university in Madrid, was asked to talk about his experience as a visiting research fellow at the Institute of Historical Research in the University of London. His account centred on the dynamics of the Institute's seminar culture. He identified some reputed international historians who had given seminars at the Institute whilst he was there, and remarked with candour that people's comments and questions in the seminar were 'incredibly picky, often very nasty too, no matter who was giving the paper.'

After the presentation, a senior researcher asked Luis whether he had been asked about the kind of work done at the National Research Council. Luis replied that no one had ever asked him about the work being carried out at his home institution back in Madrid. A brief silence followed. This was broken by a young researcher protesting rather loudly about the arrogant views Anglo-American historians had of Spanish scholarship. 'How many seminars at British universities on Spanish history or Latin American issues are carried out in Spanish? How many? I mean, some of the people who work on these subjects can't even read Spanish!!'

This protest gave rise to an intriguing debate on the nature of historical work, including the relationship between national identities and reflexive historicity in the making of productive scholarship. To the young historian's declamation about the arrogance of Anglo-American academics, a professor replied that 'There is nothing new here. We all know that English is the lingua franca of international academia.' Another researcher took the argument in a new direction by observing that 'there is a new turn in the 'Englishness' of Spanish historiography. Have you heard of Stradling's review of Pío Moa's work?' The researcher was referring to a review essay by a British historian of the Spanish Civil War, Rob Stradling (2007), where he delivered a positive appraisal of the

contributions to Spanish historiography of a much contested figure in Spanish academic circles, the Spanish historian Pío Moa. Moa first hit the news some years ago when he published a new history of the Spanish Civil War and Franco's rise to power. Moa's work is on the whole appreciative of Franco's intervention and has been dubbed in some circles as promoting intellectual fascism. Moreover, Moa does not hold an appointment in an academic institution, a point his opponents single out to proclaim his lack of credibility. Aware of these disputes and divergences, Stradling titled his review piece 'Moaist revolution'.

Few had read or heard of Stradling's piece, so the researcher provided her own summary, saying that Stradling applauded Moa's work as one of the few Spanish historians ever to have attempted a history of the civil war. A number of the people in the room nodded their agreement. Two researchers noted that, indeed, the historiography of the civil war was still a 'predominantly British affair', with 'most of the classic works on the topic having been written by British academics.' Someone else remarked that although there was indeed abundant Spanish historiography on the civil war, most 'synthetic works are by British scholars, like Paul Preston or Helen Graham.' The observation prompted a professor to remark that 'Spanish academics are very bad at writing synthetic works. We love to go on and on over the same topic, but have no skill at synthesizing information and providing a bird's eye view of events.' Another professor picked up the argument, drawing an unusual analogy with CSIC's history:

> Now that we are celebrating the JAE's 100th anniversary [he pointed to some posters commemorating the occasion that hang from the room's walls] we might as well recall what Ramón y Cajal said about science being an international enterprise, so that to be doing vanguard science one needs to be addressing an international public. And yet I think about my own time at university and the opportunities we were given to learn foreign languages. How many languages were we taught then? Only in Spanish.

Luis, who had remained silent throughout most of the discussion, re-entered the conversation at this point. He drew a comparison between Spanish and British skills at doing syntheses. The latter, he said, were at an advantage, because they had 'completed their own historiography long ago. They have written on and documented every conceivable aspect of their own history. From parish or village histories to national politics there is nothing left for them to do. No wonder they decide to jump over and do other countries' histories.' 'For this reason,' he added 'we should think of ourselves as being very lucky, because there are plenty of topics and themes we can still write about.' 'Yes,' remarked a professor in a slightly frustrated tone, 'but that is also why the English have such a capacity for abstraction: because they are one step ahead of us all the time. They can spend so much more time speculating, creating complex connections between topics, figuring out new ways of being creative, because all the basic stuff is there, done.'

The conversation at the café shows the complex epistemological economies and reflexivities through which historians working at Spain's CSIC construct the institutional imagination of what it means for them to engage in productive scholarship. The production of Spanish historiography appears, in this context, embedded in a rich and dense economy of international collaborations, transversal and cross-country interpretations of national academic traditions, seminar cultures, language barriers or formative careers. Whatever the production of historical scholarship entails, one thing is clear: There is no straightforward sense in which historical scholarship relates to society, or in which

the work of historians can be said to have immediate public value. This is of course not to say that history has no value as an intellectual or social practice. It is just that this value is not generated through a positive relation to an 'external' or 'outside' agency. The 'sociology of knowledge' of history cannot, in this context, be apprehended through the sociological vocabulary or imagination of 'externalities'. Rather, it is inextricably embedded in its own process of historicization, a process that is at once institutional and epistemic, where the value of research as an intellectual practice depends on a series of institutional moments: whether it is an individual, solitary venture, or a group-based, socially responsive one; whether it is economically and sociologically transferable (to society and other disciplines) or is sensitive to and conscious of its own historical genealogies; whether these genealogies are themselves historicized or politicized narratives, and if they are both (which they often are), whether they are national or international, left or right wing, Republican or Francoist; whether science is seen as a necessary part of the state apparatus or a self-indulgent aristocratic occupation.

Each and everyone of these variations and nuances is folded over and over in the language that researchers employ to speak about the journals they publish in, the language they write in, the epistemological and theoretical canons they decide to write themselves into, etc. These tensions, between having to move one's research 'inside' history and make it dwell in one's epistemological economy, or having to connect it 'outside' to the world, re-producing its own valency for an external public, echoes the analytical/synthetic epistemological models that Deleuze ascribes to Leibniz and Kant, respectively. And it does so by setting both in motion at the same time. The practice of research as a 'public' enterprise requires thus moving in and out of the analytical and synthetic, folding the historical and the contemporary in one singular reversible moment.

Conclusion

My argument in this article can be summarized thus: I have sought to illuminate the sociological imagination that informs the appearance and shape of the concept of the public (in its various guises) in contemporary calls for making science and society converge; for making knowledge into a valuable social good. Following Helga Nowotny *et al.* (2001) we might say that all attempts at bringing science and society closer rely on some version of the 'public' as a point of reference and reconciliation. First, *public value* is that which science policy makers summon in their calls for steering and prescribing the direction of future research agendas. Second, *public goodness* is what commercializing science attempts: to the extent that a public good is an externality, the commercialization of science – the internalization of externalities – helps make the true market value of science explicit. Last, the turn towards an auditing and accountability of science is of course a move towards its *publicity*: opening up science's internal trustworthiness by making it transparent, by 'going public'. In all three cases, then, the notion of publicness figures centrally as reference and reconciliatory point. Building on Marilyn Strathern's argument about the difficulties of conceptualizing society as something robust enough to figure as a reference point for science (Strathern 2005), the political philosophy of our age has found in the idea of the public its articulating and reconciliatory mechanism.

There is, however, a second point to be made about this self-serving political philosophy, and this concerns its anthropological epistemology. My main concern has been to show that the new political philosophy of the public takes its cue from the

sociology of economic knowledge. This is a proportional and horizontal sociology where the critical terms of description are the idea that relationships radiate outwards: that things spill over into something external or beyond them; and the idea that this externalization can be re-incorporated, or re-internalized, by scaling the whole back down into a part. The shape that this part:whole, internal:external sociology takes is homological and proportional. It makes its objects take form as proportions of one another. It links back, in this sense, with the classical tradition in political utopianism, where things can be added up or subtracted in order to reach Utopia. But, with Aristophanes' Cloud-cuckoo-land settlers, we may prefer a sociological imagination where people go and subtract and sub-divide somewhere else. CSIC's researchers would agree. For them, the public value of scholarship and knowledge does not lie 'outside' nor, simply, 'inside'. They bring their own historical practice to bear on their sociological imagination, moving in and out of each, taking none for granted. They do Kant and they do Leibniz, and some of them do Deleuze too.

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