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ARTICLE

Governing (by) Expertise. The Politics of Social Scientific Knowledge Production

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Abstract

This special issue examines the Janus-faced position of the social sciences in modern societies. The social sciences explain and critique social life while simultaneously serving as professions of government embedded in governing, policy-making, and institutional reform. Hence, academic autonomy is never absolute but always relative, negotiated within fields and structured by power. Ultimately, the social sciences are embedded in the very processes they are meant to study. Recognising this immersion calls for collective, socio-historical self-awareness and continually re-negotiating the autonomy of the social sciences in the context of societal heteronomies being integrated into their practice, making reflexivity the distinctive vocation of a relational sociology grounded in methodological pluralism.

Keywords

relational sociology, expertise, sociology of science, field theory, governance

INTRODUCTION

The social sciences occupy a peculiar place in modern societies. They are academic disciplines devoted to understanding or explaining, and often critiquing, social life. At the same time, they are also professions of government: they take part in policy-making and are generally embedded in processes of governing. From the emergence of statistics as a tool of state-building to contemporary debates on evidence-based policy, the social sciences have always been Janus-faced: disenchanting the world on the one hand, and projecting visions of future societies on the other (Weber 1948).

Sociology is particularly emblematic of this double bind. Its project of reflexivity makes it not only an observer of society, but also a discipline bound to reflect on the social conditions of its own knowledge production. Karl Mannheim (1960) insisted that sociology must grapple with the relationality of knowledge, analysing how social location and political forces shape intellectual production. Pierre Bourdieu (2004) later radicalised this claim, arguing that sociology is a "reflexive science" per se, and as such must consider its own conditions of existence – or accept its fate as an academically elaborated reproduction of the dominating symbolic order. This double reflexivity of a critical epistemology – reflecting both on society and on itself – is what distinguishes sociology

among the social sciences but also makes its limits visible: its autonomy is always relative, shaped by the very forces it analyses.

Bourdieu's field theory provides a powerful lens here (Steinmetz 2023a). While the scientific field is structured by its own rules of distinction and accumulation of scientific capital, it is always traversed by the field of power, where economic, political, and symbolic capitals interact (Bourdieu 1996). In contrast to academia's tendency to put its independence on a pedestal (the regulative idea of academic freedom, which Bourdieu describes as the field's illusio), autonomy is never absolute but constantly negotiated; it is produced through struggles, institutions, and symbolic boundaries. To negate that and to take academic freedom for granted constitutes an "illusionary fallacy" (Schmitz et al., 2017: 58). From a privileged position of dominance in the academic field, this fallacy reinforces the tendency to mask the heteronomous societal forces that structure academic knowledge production, leading to a universalisation of particular interests; at the fringes of academia, dominated positions are prone to reiterating it as an entry ticket to the field. Elsewhere (Gengnagel et al., 2016), we have described this as the "two faces of autonomy": on one of the faces, science claims independence from political and economic demands; on the other, it relies on external resources, legitimacy, and classifications that embed it in wider power relations which then allow it to make use of the very legitimacy derived from its claim to autonomy. As a reflexive social science, this relationship must be made explicit and addressed as far as the conditions of production allow.

This is not to say that making the social embeddedness of science explicit would counteract the forces at play – however, it would enable their being held accountable, open up a space for critique and a reflection on the qualities of sociology as an instrument of symbolic power. From a Foucauldian perspective, this double bind appears as governmentality. The social sciences provide categories, measurements, and discourses that make modern societies governable (Foucault 2007). The exert and legitimate expertise and experts of governing that excel in other fields because they can draw on the symbolic power stemming from their academic positions, and likewise profit from importing empirical experiences made in other field contexts into the academic field (Rose 1993; Eyal 2015; Straßheim 2020; Schmidt-Wellenburg 2025). At the same time, the social sciences are disciplined by the very dispositives they help create: the rise of indicators, rankings, and evaluation regimes illustrates how knowledge is simultaneously a tool of reflexivity and of rule, in- and outside of academia. Methods themselves embody power (Schmitz and Hamann 2022): quantitative surveys and qualitative devices do not merely reveal reality but construct and stabilise social orders.

Thus, sociology is neither fully autonomous nor purely heteronomous: Sociology in its relative autonomy is tasked with reflexively analysing the forces that shape it. The call for this special issue started from this premise, leading to an array of fascinating papers that investigate how the social sciences, and sociology in particular, are simultaneously shaped by societal demands and professional logics, and how they reflexively engage with this condition.

AUTONOMY, HETERONOMY, AND THE HISTORICAL EMBEDDING OF SOCIAL SCIENCES

The autonomy of the social sciences has always been historical and relative (Gengnagel 2021). In the nineteenth and twentieth centuries, disciplines like sociology, political science, and economics were deeply intertwined with projects of nation-building, providing categories of social classification – such as "society", "class", and "nation" – that not only analysed but also constructed the national order (Gorski 2013). Statistical practices, censuses, and national surveys rendered societies visible

as national entities (Lepenies 2016). In turn, states provided institutional homes, resources, and legitimacy to the emerging social sciences (Steinmetz 2013, 2023b; Chaubet 2014).

This mutual dependence highlights the impossibility of pure autonomy and points out the necessity of historicising and situating claims of autonomy and access to privileged knowledge production. As Max Weber noted, the social sciences have always been closely linked with the fate of political communities, even as they claimed to operate according to the ethos of Wertfreiheit (Weber 2012). The "professions of government" (Abbott 1988) – law, economics, sociology – derive their authority precisely from this intertwinement of knowledge and rule. While this has been the case for a long time, the arenas and respective configuration of autonomy and heteronomy have changed with the transnationalisation of expertise. Since the late twentieth century, processes of globalisation, Americanisation, and European integration have reoriented the production and mobilisation of knowledge (Büttner and Delius 2015; Heilbron et al., 2018; Schögler and König 2017; Vanderstraeten and Eykens 2018), turning social scientists into "operators of global governance" (Kauppi 2014). Funding schemes, evaluation mechanisms, and new public management institutions have reorganised the academic field, leading to the construction and contestation of autonomy beyond the nation-state (Baier and Gengnagel 2018; Münch 2014; Gengnagel 2021).

Paradigmatically, the European Research Council exemplifies this paradox. On the one hand, it presents itself as the guardian of academic autonomy, funding projects solely on the criterion of scientific excellence. On the other hand, it functions as a symbolic device within EU governance, producing "champions" of European science who embody competitiveness, marketability, and transnational prestige (Gengnagel 2026). EU research governance exerts a "gravitational pull" (Gengnagel et al., 2022), orienting disciplines toward market-like evaluations and impact-driven discourses (Kropp 2021). Autonomy, here, is reframed as self-determined alignment with European competitiveness. While this critique is well within Mannheim's Seinsgebundenheit des Wissens, the social underpinning of the transnational construction of expertise is pushed further by analysing how professions structure European political integration (Büttner et al. 2015; Schmidt-Wellenburg 2017, 2025). In a paradigmatically exemplary fashion, economic expertise has been pivotal in shaping European institutions, staffing bureaucracies, and legitimising austerity politics (Mudge and Vauchez 2018). Professions function as "transmission belts" between academia and governance, providing both the symbolic and the technical resources for integration. This demonstrates that heteronomy is not created by external pressure alone but mediated by the professions and disciplines themselves. At the same time, as Schmidt-Wellenburg and Schmitz (2022, 2023) argue, the social sciences are deeply embedded in the societies they study. Their research questions, methods, and orientations are shaped by the crises and transformations of their time – from neoliberal restructuring to the climate crisis. Reflexivity requires researchers to ask not only "what societies are we studying?" but also "in which societies are we ourselves conducting research?" The social sciences cannot detach themselves from societal demands; their autonomy is put into practice by analysing how those demands shape them.

REFLEXIVITY AND THE POLITICS OF METHODS

One crucial arena where autonomy and heteronomy intersect is methodology. Research methods are not neutral instruments but institutionalised practices that embody power effects. Quantification, surveys, and experimental designs do not merely measure reality but constitute it in particular ways and render societies governable (Porter 1995; Desrosières 2015).

The rise of evidence-based policy illustrates this dynamic. Randomised controlled trials (RCTs) are now widely presented as the "gold standard" of causal inference and policy evaluation (Jatteau 2018). Yet as Malte Neuwinger (this issue) shows, their credibility did not originate in scientific fields. For much of the twentieth century, RCTs had an ambiguous status within the social sciences. Their political legitimacy derived from bureaucratic practices and business management before acquiring scientific legitimacy in the 1990s, while their canonisation as scientific evidence only occurred retrospectively. Here, heteronomy preceded autonomy: political demands stabilised a method that was later re-imported into academia.

As Neuwinger's study shows, expertise is not a one-way road from academia to politics and other social fields: practical knowledge and experience in governing is, in many instances, the origin of projects that seek to gain societal approval and recognition by pushing for academisation. To understand this process of co-construction of legitimate expertise, scholarly reservations and appropriations have to be traced through the process of the academisation of a methodological paradigm.

Similar dynamics are evident in the proliferation of rankings and indicators (Hamann and Schmidt-Wellenburg 2020; Brankovic et al., 2023). As Strietzel and Tobias (this issue) demonstrate, NGOs producing global rankings such as the Climate Change Performance Index and the Financial Secrecy Index do not simply apply objective measures. Instead, they too, engage in practices of credibility-building: aligning with established facts, imposing non-standardised judgements, crafting narratives, and building alliances. While obviously agenda-driven in its application, the credibility of rankings mobilised by these civic society actors is the outcome of "trials of strength" (Latour 1987), not an inherent property of numbers. As Strietzel and Tobias argue, rankings show how heteronomous demands for advocacy and policy influence are woven together with claims of scientific objectivity.

Governing by expertise, in this vein, means establishing and stabilising instruments of governance which are based on a close connection between academic, political and other social practices. Their interwovenness needs to be explained by referring to different field logics, making any investigation an incursion into multiple fields. Such complex explanations, however, only become part of the trial of strength insofar as the disclosure of that interconnection is itself part of the instrumentalisation. Against this backdrop, a concept of fields as a heuristic instrument of sociological understanding is especially helpful – not as an ontological judgement from a privileged position, observing from a seemingly godlike view from nowhere, but rather a curious observer's position trying to make sense of how different social forces make pragmatic use of academic legitimacy.

Thus, methods are key sites where autonomy and heteronomy are negotiated. They are simultaneously instruments of reflexivity and governmentality. To analyse them sociologically means to acknowledge the double life of the social sciences: both as science and as government.

TRANSNATIONALISATION, EUROPEANISATION AND (RE)NATIONALISATION

Currently, the societal dynamics we observe are marked by contradictory processes of varying scope. On the one hand, transnationalisation continues apace. European integration, global funding schemes, and international rankings structure the conditions of knowledge production. Economic expertise has become central to European political integration, reinforcing neoliberal governance and shifting the balance of symbolic power toward economics (Schmidt-Wellenburg 2017). The ERC

and Horizon programs reconfigure the social sciences by rewarding competitiveness and excellence (Gengnagel et al., 2022; Gengnagel 2026).

On the other hand, crises of globalisation and Europeanisation have fuelled (re)nationalisation. Populist movements, challenges to the EU, and geopolitical tensions have revived the nation-state as a framework of belonging and governance (Recchi 2019; Rosamund 2019). The COVID-19 pandemic further underscored the persistence of national logics in crisis management. For the social sciences, this raises pressing questions: does renationalisation also imply a renationalisation of expertise? Or do national frameworks remain perforated by transnational competition, producing hybrid logics of autonomy and heteronomy?

Aykiz Dogan (this issue) addresses precisely this question by analysing in a historical perspective how expertise is mobilised transnationally to construct and govern the Turkish nation-state. As her inter-war case study highlights, expertise does not travel automatically; it requires intermediaries and symbolic resources. Migrating experts seek and co-construct contexts in which their knowledge can (re-)gain and maintain relevance for governmental projects. Therefore, the authority of their expertise depends on how it is embedded in transnational fields and linked to national arenas, which demonstrates that expertise has never been a stable possession but a relational capacity, contingent on the interplay of autonomy and heteronomy.

As Dogan's detailed account illustrates, expertise and its effect in a national setting depend not only on the current social struggles of the respective context governed, but also on broader – here: transnational – power structures in which experts are located. To a significant extent, especially in times of fundamental change in the field of power, the influence of and demand for experts is created in translocal settings well beyond the societal context in which they then take effect. At the same time, the paper, while focusing on a historic case, provides a rich texture against which current-day debates about Turkey and its relationships within Europe could be productively investigated.

Aurélien Boucher and Xiaoguang Fan (this issue) examine the localisation debate in Chinese sociology. Drawing on a survey, they show that despite political ambitions for the Chinese government to "sinicise" the social sciences and to "tell beautiful stories about China", most sociologists adopt pragmatic positions combining Chinese and Western theories. Rather than aligning neatly with either state ideology or international academic capital, Chinese sociologists navigate a space of relative autonomy, balancing heteronomous pressures with disciplinary logics, and thus silently co-governing and limiting the politicisation of their craft. A rather weak claim to autonomy and can be better understood as structured by a form of bi-nomos: a basic commitment to sinisation seems to be necessary throughout. In consequence, an engagement for scientific autonomy can only be pursued against this backdrop and appears as a relative openness towards Western approaches, often hand-in-hand with a transnationalised biography. On the other hand, an outright rejection of Western sociology is mostly employed by agents who are locally rooted and considered dominated in the symbolic hierarchy of the field – which the authors see as a subversive strategy in the Chinese context.

As Boucher and Fan's detailed analysis shows, autonomy of a specific academic field or discipline must be explained in its own socio-historical context in order to prevent false conclusions based on hasty analogies. The task is to understand autonomy in relation to various forms of heteronomy based on the historically developed differentiation between fields and its expression in a specific field of power, that is, in its turn, embedded in specific transnational relations. The approach counteracts unilinear explanations linked to plain dominance of state-bureaucratic or economic fields and invites

historical and national as well as transnational comparisons that encourage epistemological curiosity as well as caution (Schmidt-Wellenburg and Gengnagel 2025). These analytical conclusions invite further questions, spurring a debate on specificity and adequate generalisation, begging the question: Do cases observed really succumb gradually to manifestations of the same field principles at play in all academic fields and if so, to what degree?

Walter Bartl and Sebastian Heer (this issue) focus on how instruments of democratic participation such as co-creation workshops funded by the EU help to create regional expertise as a decisive element of EU multilevel governance. Drawing on materials from an EU-funded research project on energy transition processes of which both researchers were part, they investigate how regional expertise is assembled by scientists and regional stakeholders in practices of co-production. Here, legitimacy for EU multilevel governance rests on specific regional expertise of transformation created with the active help of social scientists in a double move: they provide theoretical arguments for the worthiness of regional stakeholder knowledge and participation and, at the same time, certify the practical implementation of the instrument. The regional specificity of expertise contributes most to easing practical processes of governance on the lower levels of such multi-level governance structures. If experience-based knowledge is translated to higher levels of governance (e.g., EU institutions, consultations, and conferences), this does not mean that it is actually devalued, but it almost necessarily changes its form: it is now no longer actively used in projects for action but merely referenced to legitimate and bolster positions in transnational struggles over the definition of sensible EU policies with a potential for implementation.

Depending on the context in which expertise as a certain form of symbolic capital is used, it may serve different and even counter-intuitive purposes. This heightens the need to further develop our analytical concepts for researching expertise and experts in times of mode-2 knowledge production (Nowotny 2003) to focus on the integrated production of expertise and experts between various fields as well as the diverse scopes of practices used to create and legitimise governmental effects. This allows us to understand how practical knowledge from regional actors is used for governing while at the same time reflecting on the role of midwife often played by the social sciences in policy projects of “co-creation”.

All three contributions exemplify how processes of nationalisation, transnationalisation, regionalisation, and renationalisation are closely intertwined and mutually dependent. This becomes particularly clear when, as is done in these articles, actor-centred perspectives are used that focus on the knowledge policies behind the attributions of global, national, regional, international, transnational, European, etc., and on their anchoring in different field contexts (Steinmetz 2016; Go and Krause 2016; Buchholz 2016; Krause 2018, 2020). The focus is then on the power and knowledge relations producing certain perspectives and enforcing them as valid scopes of sociation (Schmidt-Wellenburg 2023). Here, levels of government that are taken for granted while scientific analyses attest to their reality – and the unconditional belief in both – appear as stakes in the struggle for the reach of different government projects and the associated notions of statehood (Bourdieu 2014).

TOWARD A REFLEXIVE SOCIOLOGY OF EXPERTISE

The contributions to this issue provide a multifaceted view of how sociology and related disciplines are implicated in governance while claiming autonomy. They illuminate the reflexive task of sociology: analysing how it is shaped by heteronomous forces even as it asserts its relative autonomy. On that note: just as academic autonomy is always relative, the intellectual endeavour of practicing a critical epistemology can only be a collective one (Bourdieu 1998) – for which the pages of this

esteemed journal provide a well-suited platform (see e.g., Heilbron et al., 2017; Benz et al., 2024). We are thus grateful for the possibility of bringing them together in this special issue.

The focal point of this special issue is that sociology and the social sciences are never fully autonomous. Their autonomy is always relative, constantly negotiated against heteronomous demands. This is not a weakness per se, but certainly a defining feature. As reflexive disciplines, the social sciences are tasked with analysing precisely how societal demands enter into their practices, methods and institutions (Kauppi 2020).

This requires methodological pluralism. Bourdieu's (1996, 2004) field theory highlights the autonomy/heteronomy tension and the structuring of academic capital. Foucault's (2007) analytics of governmentality cautions us on how knowledge practices shape governance. Latour's (1987) emphasis on practices and "trials of strength" directs attention to credibility and networks. Drawing on these approaches, among others, enables us to envision a reflexive sociology of expertise that can analyse how autonomy is constituted through or practised in conditions of heteronomy and at times used to oppose them.

The contributions to the special issue show that forms of expertise differ according to the societal contexts in which they are located. Expertise varies not only according to the governmental project for which it is used and the heteronomy or autonomy of the academic field in which experts are primarily rooted, but also with regard to the relationship between these and other fields, i.e., the prevailing structure of the field of power and the extent of its claim to validity (Bourdieu 2020; Lebaron 2000, 2018).

In times of heightened conflict between different ideas about the legitimate structure of societies, their extension and relation to other societies and forms of societalisation – as evidenced by the contested notions of globalisation, Europeanisation and (re)nationalisation – ideas about expertise come under fire. However, the quickly diagnosed crisis of expertise (Eyal 2019) or even its death (Nichols 2017) falls short: From a relational and reflexive perspective, it is more accurate to speak of a shift in the concept of expertise in the course of a reconfiguration of social power relations (Stampinski 2023), which is seen as a crisis in those areas of the field that hold vested interests in traditional concepts of intellectuals and expertise, and – to the contrary – as an opportunity for those with vested interests to open up and re-configure expertise.

It is by no means a new insight, but one that requires constant re-iteration against the steady pressure of an uncritical epistemology falling for the lure of scholastic fallacies: sociology and other social sciences are not external to society but deeply immersed in the current relations of knowledge production and policymaking. Therefore, we join the chorus of calls on sociology to understand itself as a collective subject and, as such, to perpetually work on its socio-historical self-awareness by reflexively and relationally bringing to mind the social conditions of its own knowledge production. The contributions to this issue can be seen as different responses to this call and the necessity to continue to negotiate the autonomy of the social sciences in the context of the integration of heteronomy into sociological practice, making reflexivity the distinctive vocation of sociology.

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ARTICLE

When Did Social Experimentation Turn Into Social Science? The History of Randomised Controlled Trials and the Role of Science in Evidence-Based Policymaking

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Abstract

Supporters and critics of contemporary evidence-based policymaking (EBPM) widely view randomised controlled trials (RCTs) as an example of social science turned into an instrument of policymaking. Yet, historically, it is more adequate to say that policymaking turned RCTs into social science. Questioning standard views on the history of RCTs, this article argues that for most of the twentieth century, the scientific status of RCTs remained ambiguous—yet this did not stop them from gaining political credibility in social and medical applications. Until recently, experimental approaches were used not because they were regarded as the hallmark of science but because regulators and businesses felt that testing policies and drugs systematically was a sensible thing to do. Debates about the scientific status of RCTs became significant only in the 1980s when some leading social scientists opposed RCTs, viewing them as *un*scientific trial-and-error tinkering. RCTs eventually emerged as a generic instrument of evaluation in the 1990s, when they became regarded as politically credible *and* scientific. Judging from the history of its "gold standard", present-day EBPM's focus on social science instead of politics and business essentially reverses the direction in which it expects evidence to come to policy.

Keywords

randomised controlled trial, social experiment, problem of demarcation, boundary work, history of social science, evidence-based policymaking

INTRODUCTION

Social science is a crucial resource for improving policies and political programs—at least, that is what social scientists and policymakers keep saying. Supposedly, contemporary evidence-based policymaking (EBPM) mines social science for insights and then uses this evidence to develop better policies: more effective ways to help the poor, more sustainable ways to organise the economy, and more sensible ways to tackle acute crises. Whether optimistic or sceptical about achieving these aims,

everyone considers social science the source of potential solutions (Baron 2018; Chupein and Glennerster 2018; Hadorn et al., 2022).

This article examines the history of randomised controlled trials (RCTs)—a kind of social experiment often regarded as the "gold standard" of EBPM—to suggest that the conventional vision sketched above is more recent and less self-explanatory than may first appear. While drawing on the standard repertoire of critical scholarship—asking questions like, What exactly is meant by "science"? What counts as "evidence"? Who is to say what is a "better" policy? (Eyal 2019; Parkhurst 2017; Turner 2013)—the article's main concern is not to question the applicability of RCTs to policy evaluation or to criticise the aims of EBPM. More modestly, it takes on a question asked far less often (Oliver et al., 2014; Stoker and Evans 2016): Why is it necessarily social science that is supposed to make policymaking evidence-based? As it turns out, the answer to this question is far from obvious, especially if by "social science" we mean RCTs.

Over the past twenty years, RCTs have become known as a tool that may "revolutionize social policy" just as they "revolutionized medicine in the twentieth century" (Duflo and Kremer 2005: 117). They are celebrated as a "credibility revolution" in social science (Angrist and Pischke 2010), some of their supporters have recently won a Nobel Prize in economics, and research centres engaged in running and disseminating RCTs define their mission as "ensuring that policy is informed by scientific evidence" (J-PAL 2024). Yet, speaking about RCTs in this way has not always been obvious. As is generally the case historically, experimentation preceded science, and its primary purpose was to guide action rather than improve scientific knowledge (Hansson 2015).

It is widely acknowledged that RCTs in social contexts have been run since the early twentieth century (Baron 2018; Jamison 2019). But what role did social science play? This article argues that, compared to today, researchers and policymakers drew the boundaries between "science" and "non-science" quite differently—and what these boundaries meant varied historically as well. For instance, during the 1960s and 1970s, the success of RCTs was mainly due to bureaucrats and commercial research firms. But unlike today, whether these actors were legitimised through social science was not a standard concern. Later, by the 1980s, leading social scientists, including future Nobel Prize laureate James Heckman, opposed the widespread application of RCTs precisely because they were regarded as *un*scientific trial-and-error tinkering. In their view, creating evidence for policymaking should be left to lesser, non-scientist minds. The idea that social scientists might improve policymaking is not new (Wittrock et al., 1991). Nevertheless, the history of RCTs puts things in perspective, suggesting that the contemporary view of EBPM signifies a shift not only in how we define "rigorous evidence" but also in what we expect from "social science".

What is more, we will see that the dominance of *medical* trials emerged only in the early 1970s, again largely through efforts taken by businesses and government agencies, and became widely regarded as unambiguously scientific only later. Yet when political credibility and scientificity finally came together during the 1990s, "the RCT" was created as the *all-purpose instrument of evaluation* celebrated by contemporary EBPM. For medical and social RCTs, tinkering came first, more serious practical applications followed—and only then, late in the twentieth century, did they come to be widely regarded as "scientific". In today's image of EBPM, the direction of how evidence comes to policy has been essentially reversed, with science assuming a historically unlikely leading position.

The rest of the article proceeds as follows. The next section discusses how the history of RCTs is best understood conceptually. It argues that analysing the "boundary work" among scientists and practitioners helps question whether RCTs were always regarded as part of social science. But at the

same time, putting boundary work centre stage forgets to ask whether RCTs' scientific status always mattered when the technique was applied in twentieth-century policymaking. A perspective modelled on Pierre Bourdieu's sociology of science improves on analyses of boundary drawing, by also considering how categories like "science" and "politics" have changed their meanings over time. The following two sections discuss how the history of RCTs has been written so far and show empirically how taking the shifting boundaries of social science seriously improves our understanding of EBPM. The concluding section considers some objections to the article's main argument, stressing that debates about whether RCTs are part of social science are not just an academic question—the outcome of these debates is one of the main reasons RCTs are seen as the gold standard of EBPM.

RCTS, SCIENCE, AND POLITICAL CREDIBILITY

For the last half-century, one of the workhorses of the sociology of science has been the concept of *boundary work*. Sociologists generally present it as a practical solution to the "problem of demarcation" in the philosophy of science: instead of abstractly debating what activities qualify as "scientific"—something philosophers keep worrying about (Hirvonen and Karisto 2022)—boundary work describes the practical strategies scientists use to position themselves on the side of "science" and others on the side of "non-science" (Gieryn 1983). Crucially, sociologists insist that these practical strategies also have an important practical purpose. Especially in political contexts, whether one's activities are regarded as scientific is an essential source of *credibility* and *legitimacy*. This means scientists and experts will often attempt to preserve their authority and integrity by stabilising, redrawing, or otherwise manipulating the boundaries between science and non-science (Jasanoff 1987).

Historians of RCTs have almost entirely ignored the importance of boundary work. As will be discussed later, they instead draw on a model that remains largely implicit. Reflecting a particular researcher's main interests and expertise, the rise and fall in popularity of RCTs over the twentieth century is usually interpreted either as an outcome of "scientific developments" or "political manoeuvring". In doing so, historical writings consistently associate RCTs with "science"—without checking whether this was how people thought about them at the time. This perspective still guides present discussions. Even contemporary critics usually accept the scientific status of RCTs and focus on other problems, such as the ethical and political implications of experimenting with "human subjects" on a mass scale (Bédécarrats et al., 2020; Fejerskov 2022; de Souza Leão and Eyal 2019).[1]

At first sight, the concept of boundary work seems to enlighten the discussion. Indeed, we will see that zooming in on the instability of the science–politics boundary immediately reveals that unambiguously associating RCTs with science is a very recent novelty. At the same time, it is important to note that most historical writings retain a subtlety that the concept of boundary work tends to conceal. While boundary work makes no assumption about how boundaries are drawn empirically or how they should be drawn epistemically, it makes a strong assumption that the

[1] To be sure, critics have made some scattered angry remarks, according to which the current popularity of RCT is "more a matter of faith than science" (Ravallion 2020: 60), or that "if experimentation were the hallmark of science, there would be Nobel Prizes for alchemy and not for the physics of astronomy" (Pritchett, in Picciotto 2020: 259). But these are not focused analyses of the boundary between science and non-science. Instead, critics question whether RCTs should be regarded as "really scientific", much like the debates in the 1980s we discuss later.

existence of boundaries is universally *significant* and that their significance is *stable* over time. As one researcher expresses the concept's premise:

> It is universally accepted [among all parties involved in discussions of policy-relevant science] that 'science' should not be influenced by politics and that judgements as to what constitutes 'good' science should be left to scientists. All agree, as well, that scientists should not be involved in making 'policy' (Jasanoff 1987: 199).

But while today it seems quite intuitive that credibility and legitimacy flow from boundaries between science and policy—that drawing and maintaining boundaries guards EBPM against turning into "policy-based evidence-making"—the history of RCTs puts a question mark on the transhistorical stability and universal significance of this principle. At various points during the twentieth century, RCTs seemed *credible* enough to base policy decisions on them regardless of their scientific status. The question of whether RCTs were supported by scientists seems to have arisen only later, namely in the 1980s, when academic economists began emphasising the boundary toward non-science explicitly.

Thus, because political credibility, scientific status, and the significance of science–policy boundaries did not move in sync, focusing on boundary work solves one problem and introduces another. It reveals that RCTs became unambiguously associated with science only recently, making it quite anachronistic to speak of historical applications of RCTs as instances of "science influencing policy". Yet the concept is itself anachronistic when assuming that maintaining the boundary between science and policy is equally important in all circumstances.

One way to overcome this conundrum can be gleaned from the work of Pierre Bourdieu (1991, 2004). Similar to the idea of boundary work, Bourdieu insists that social science, politics, or business are never entirely separable. The autonomy of these fields always remains relative, meaning that the question of whether RCTs are scientific may become a significant stake relevant to social actors' credibility. But in addition, Bourdieu argues that everyday categories—again, think of "science", "politics" and "business"—themselves have a history. Accordingly, at different historical moments, such categories are associated with different assumptions and expectations: the boundary between science and politics *may* turn into a stake relevant to RCT proponents' credibility, but this need not happen in all circumstances.

Therefore, it becomes necessary to consider assumptions about "proper" or "normal" boundaries in their historical context, performing an "epistemological break", as Bourdieu liked to say. This makes it possible to newly "construct the object" of analysis—in this case, RCTs—and understand its meaning and significance at a particular point in time (Bourdieu et al., 1991: part 1; Swartz 2013: 21–22).

The following sections will show how Bourdieu's conception (or at least his weariness of others' conceptions) avoids some of the oversights in previous analyses of the history of RCTs, taking seriously the phenomenon of boundary work without necessarily associating claims to political credibility with scientificity, or vice versa. In this process, "the RCT" indeed turns into a new object: one that moves freely between science, politics, and business—but whose credibility emerges from all of them at different times. To demonstrate the value of these arguments, we may begin by considering how the history of RCTs has been written without them.

THE STANDARD PERSPECTIVE ON THE HISTORY OF RCTS

Randomised controlled trials are experimental procedures to compare different options—drugs, policies, technologies, or anything else—about whose causal effects one is not entirely sure. In its simplest form, an RCT randomly assigns people to either a "treatment" group that receives the drug, policy, or whatever it may be, while the other does not. According to the logic of present-day EBPM, if the treatment group shows more promising effects than the control group, the treatment probably "works" in the sense that it likely caused the effect being observed.

The way this procedure has entered policy evaluation is not straightforward. In the United States, interest in RCTs conducted in the social realm—until recently usually called "social experiments" (Hausman and Wise 1985; Riecken and Boruch 1978)—followed a "boom and bust" cycle (Rossi and Wright 1984: 332). At several points, people became excited about experimental methods; a few years later, they lost interest; and yet a few years later, they became excited again. Some scholars have identified three "waves" of RCTs (Jamison 2019), others two (Oakley 2000), and yet others only one (de Souza Leão and Eyal 2019), but the exact number does not matter here.[2] The point is that every serious scholar agrees that the history of RCTs is marked by numerous ups and downs, at least in the United States. (Given that the history of RCTs outside the US remains to be written, it is not the focus of the present discussion).

In reviewing the waves, a *standard perspective* on the history of RCTs emerges. Contemporary historians and RCT experts paint a detailed picture of the ups and downs of experimental studies in social contexts over time. But in framing these ups and downs as a process of "scientific development" or "political manoeuvring", they collectively miss that, for most of the twentieth century, RCTs were not associated with social science in the sense they are today. We will see later that this has significant consequences for how we think about the history of EBPM.

Waves of social experimentation over the twentieth century

Various proto-versions of RCTs have appeared at several points in post-Enlightenment history. Historians of medicine get excited about the eighteenth-century Scottish surgeon James Lind, whose experiments showed that sailors at sea could be prevented from scurvy if they ate enough citrus fruit (Jamison 2019). Similarly, historians of philosophy proudly point out that one of the first people with a good enough grasp of randomisation to use it systematically was the nineteenth-century Pragmatist Charles Sanders Peirce (Hacking 1988). However, these proto-RCTs remained isolated attempts and received only limited attention. Eighteenth and nineteenth-century intellectuals were strikingly united in their lack of interest in taking the experimental procedures common in the natural sciences and applying them to investigations in the social realm (Brown 1997).

The first distinctive wave of social experiments emerged in the 1920s and 1930s. One famous example is the attempt to improve worker productivity at the Hawthorne factory of the Western Electric Company near Chicago, using variations in lighting. Because workers were aware of the ongoing experiments and accordingly adapted their behaviour, we now speak of the "Hawthorne effect" that can complicate the interpretation of experimental studies. Randomised social experimentation continued during World War II (deLeon 1991; Guglielmo 2008). For instance, the US Army's Morale Division—a "psychological warfare" branch inside the military—randomly

[2] One researcher has even identified *four* "waves of evaluation diffusion" (Vedung 2010), but his account extends beyond RCTs in particular.

assigned American soldiers to the film series *Why We Fight*, hoping to learn about its "educational effects". The filmmaking and experimenting were attempts to counter the Germans' successful propaganda efforts, most prominently the movie *Triumph of the Will* (Dehue 2001: 293–294). According to one scholar, American research on the efficacy of propaganda techniques may well have been "the decisive spur to the use of randomized trials to evaluate policy programs" after the war (Oakley 2000: 322).

Another, much larger, wave of RCTs began to roll in 1964, the United States' "War on Poverty". One of the most famous experiments was conducted between 1968 and 1972, a "negative income tax" (NIT) trial that would integrate the social security system into the regular tax system. Because the NIT model posited that people under a certain income threshold would "pay negative taxes"—as in, the government paying *them*—people would effectively be guaranteed an income independent from wage labour (Steensland 2006). While the plan died under later administrations, the NIT trial and its three successors "helped to usher in a brief but heady period of large-scale social experimentation in domestic federal agencies" of the United States government (O'Connor 2001: 190).

Further experiments in policy areas like education, housing, health insurance, and labour market reintegration followed (Hausman and Wise 1985). It was a "golden age of evaluation" (Rossi and Wright 1984: 331). Led by firms including MDRC (then called the Manpower Demonstration Research Corporation), the Institute for Research on Poverty (IRP), Abt Associates, the Urban Institute, and a greatly expanded RAND Corporation, a veritable research industry emerged (Berman 2022: chapter 5).

With the Reagan administration's mission to get rid of the welfare state, the political climate changed during the 1980s. Still, social experiments remained in demand. Indeed, one scholar argues that "quantitative and microeconomic reasoning, with its promise of certainty and rigour and powerful normative foundation, first became really attractive when the content of policies became more controversial" (Jann 1991: 124). The Social Security Act of 1962 allowed states to apply for waivers. If states pledged to design "innovative" (i.e., in this case, conservative) policy reforms, standard welfare law would not apply. However, to get the waivers approved, the federal government required states to test their proposed reforms in experimental studies (Harvey et al., 2000). One labour economist notes that this arrangement made RCTs "almost mandatory" during the late 1980s and early 1990s (Moffitt 2004: 520). This de facto obligation ended in 1996 when Bill Clinton announced his plan to "end welfare as we know it". However, as we know today, experimental methods only took a few years to regain traction.

"Science" and "politics" as drivers of RCTs?

Why were RCTs popular at one point in time and less popular at another? Why were they so popular in the United States rather than elsewhere? Following the standard perspective, historians of social science and experts on RCTs suggest that it must have something to do with political priorities and the relative relevance of social science in different countries and at different times—how developed the social sciences were and how much attention politicians paid to the insights of social scientists (Backhouse and Fontaine 2010; Levitt and List 2009; O'Connor 2001). But while the idea that some combination of "science" and "politics" is responsible for the varying popularity of RCTs over time seems natural, the intuitiveness of these categories tends to insulate them from scrutiny. As we will see now, almost all historical narratives dubiously depict RCTs as a technique that was "scientific" all along, even though their status was ambiguous at best.

The United States was the first country to develop the kind of professionalised social science we know today. Early on, American academics established the conviction that "first and foremost social science was *science*, not philosophy, or social reform, or history" (Bulmer 1991: 164, emphasis in original). They explicitly imagined themselves "as detached, nonjudgmental observers rather than as helping neighbours or political allies" (O'Connor 2001: 47). With some exceptions, the rest of Europe lagged behind these efforts (Backhouse and Fontaine 2010; Wittrock et al., 1991). To historians of social science, it follows that developments in social science explain the fondness of Anglo-American countries for experimental methods. Having the most policy-focused social sciences and the most social science-oriented political tradition gave them a head-start.

RCT experts have built on this general characterisation, filling the gaps with empirical detail. RCT enthusiasts, in particular, generally name *scientific* factors for the first wave of experimental trials, such as breakthroughs in statistics. Along with his earlier publications, Ronald Fisher's 1935 book *The Design of Experiments* is credited with being "a main catalyst for the actual use of randomization in controlled experiments" in various contexts (Levitt and List 2009: 3). Similarly, RCT experts commonly trace the second wave to a famous 1966 textbook by Donald T. Campbell and Julian Stanley, *Experimental and Quasi-Experimental Designs for Research* (Rossi and Wright 1984: 334). Invariably, RCT experts explain the increasing application of experimental methods in policy research through methodological advances in social science. Some, like Campbell and Stanley themselves, even attributed the *end* of each wave to the same factor. In their view, scientific advances simply had not been significant enough (Campbell and Stanley 1967; Rossi 1987).

More historically-minded scholars—who are usually more critical of using experimental methods in policy evaluation—do not dismiss science as a significant force of social change, nor do they doubt the scientificity of RCTs. Still, they stress the importance of *politics*. They argue that social problems increasingly came to be seen as technical issues with technical solutions—which rendered scientific breakthroughs applicable to politics in the first place. During the War on Poverty, social scientists could count on "hitherto unheard-of amounts of financial support and ready access to policy makers" (deLeon 1991: 91). For instance, the first NIT experiment, conducted in New Jersey, was based on an initial idea by graduate student Heather Ross. Worth at least $US5 million at the time (over $US30 million in today's dollars), it was "perhaps one of the most expensive doctoral theses in economics" (Levitt and List 2009: 5).

What is more, the NIT experiments were enabled by a striking political U-turn. While early efforts of the War on Poverty had been primarily focused on community programs aimed at organising poor people politically, a media scandal involving several Chicago street gangs led to the installation of former RAND Corporation staff in the relevant government agencies. By October 1965, these new personnel had substituted community organising for a more conservative policy of income maintenance and job creation (O'Connor 2001: 171–178).

During the 1980s, the innovation waivers discussed previously kept RCTs alive, but other political factors stopped them from spreading further. Because experimental interventions were less generously funded, they became smaller and more selective. As the welfare state came under fire, doubts about the contributions of social science also became prominent. In the words of a group of distinguished historians, the Thatcher and Reagan governments defunded the social sciences because they saw them, and "in particular the non-economic approaches, as biased towards the welfare state and towards the cause of the political opponent" (Wittrock et al., 1991: 53). Subsequent research tended to focus on checking existing programs' cost-effectiveness, hoping to guard the state against high expenditures.

It is obvious that scientific developments and political manoeuvring are indispensable for understanding the history of RCTs. The historical facts are not in question. At the same time, historians of social science themselves agree that the conceptual distinction that (however innocently) structures their narratives is hard to maintain. For instance, they occasionally note that because social scientists made increasing "efforts to tackle social problems" after World War II, the "traditional domains of the social sciences were redefined" in post-war societies (Backhouse and Fontaine 2010: 184). Another scholar writes that "the greater methodological rigour of American, as opposed to British, research" might be due to the "position of social scientists in the two societies and their self-conception" (Bulmer 1991: 159).

Still, historians and RCT experts generally treat experimental techniques as unambiguously "scientific" and explain their popularity over time through the clearly distinct variables "science" and "politics." The following section emphasises the fact that these variables are far more permeable than often assumed.

RCTS BEYOND SCIENCE AND POLITICS

The power of theory is greatest when its existence remains implicit. The implicit theory inherent in the standard perspective on the history of RCTs sounds simple enough and broadly "fits the facts" it purports to explain. What other than scientific and political factors would be responsible for RCTs' ups and downs over time?

This section describes how a view modelled on Bourdieu's sociology of science changes the picture. As indicated previously, this perspective stresses that the boundaries between science and politics are dynamic rather than static. It also recognises that social scientists rarely operate entirely autonomously from "heteronomous" considerations outside the area demarcated as science, meaning that those least favoured by the epistemic norms of the scientific orthodoxy will be most inclined to appeal to "non-scientific" considerations (Bourdieu 2004: 85–87). Finally, beyond exclusively analysing the permeability of the boundaries between science and politics, Bourdieu insists on putting the categories of "science" and "politics" themselves in historical context. Whether an activity is viewed as scientific *can* become a relevant stake related to political credibility—but whether and when it does cannot be assumed in advance (Bourdieu et al., 1991: part 1; Swartz 2013).

These considerations suggest that the standard perspective's assumptions are too neat in some cases and actively misleading in others. As we will see, one example is that the "golden age" of RCTs during the 1960s and 1970s was due to the rising political influence of economists, as some scholars have correctly observed (de Souza Leão and Eyal 2019: 392–394), but not necessarily due to "social scientists". At the time, RCTs became attractive because economists successfully established counterfactual thinking as a new criterion of political credibility. Yet unlike in today's EBPM, whether RCTs (or their supporters) were regarded as scientific was not a primary concern.

A second example is that, in the 1980s, influential social scientists considered RCTs as "too political" to be part of "science" proper—making them turn against both RCTs and against intervening politically. When governments and research firms began to endorse RCTs, social scientists turned the very meaning of "social science" into a subject of boundary work. Finally, a third example of the limitation of the standard perspective is that *clinical* trials were not considered scientific for much of the twentieth century either. Only when the evidence-based medicine movement finally merged clinical trials' political credibility with scientific ambition, a generic instrument of evaluation called "the RCT" was born.

Credible RCTs without social science, social science without RCTs

The contemporary success of RCTs relies on the fact that a large group of social actors—social scientists, governments, non-governmental organisations, private research firms, and philanthropic foundations—supports them. They all agree on RCTs' credibility in scientific *and* applied contexts (Neuwinger 2024). Two debates in the area of labour market research, where RCTs have had the greatest success over the longest timeframe, illustrate that this view is relatively novel historically. The first debate, from the 1970s, shows how a new *criterion of credibility*—the "criterion of net impact"—was established in the US government, sowing the seeds for large-scale RCTs in social policy. The second, from the 1980s, features two groups of economists, one advocating a detached and the other a more applied type of analysis, arguing about *whether RCTs were proper social science*. Together, these debates show how the political credibility and scientific status of RCTs emerged—but largely independently of each other.

Do labour market policies, such as programs teaching unemployed people labour market-relevant skills, improve their earnings? Before the 1970s, answering this question was easy. Program administrators simply checked whether a person's earnings were higher after participating in their program than in previous employment. But if you think like an economist, this is wrong. And because the number of economists employed in government had continuously increased since World War II, they would not let administrators get away with common sense (Berman 2022). After all, economists said, people's earnings were likely to rise simply because of favourable macroeconomic conditions or mere luck. The assumed causal relation between the training program and higher earnings was spurious. A more adequate criterion of program success, economists argued, was based on "the counterfactual": whether a person's earnings would also have been higher if they had *not* been part of the program. This is what they called the *criterion of "net impact"*. According to economists, counterfactual reasoning was a better guide to assessing impact than the factual experience of program administrators (Breslau 1997a: 882–884).

Why would government officials want to evaluate their programs against hypothetical situations dreamed up by economists? The short answer is that they were essentially forced to do so. Fierce discussions ensued. Eventually, however, the Assistant Secretary for Policy Evaluation and Research (ASPER), the Office of Management and Budget (OMB), and the Department of Labor (DOL) used their combined executive power to mandate the use of the net impact criterion (Berman 2022: 108–110; Breslau 1997a: 889–891). By the late 1970s, the terms of debate had changed in favour of the economists. It was not that everyone had changed their minds, but that economists had used their increasing influence in government circles to establish their criteria of good argument inside its leading institutions. The government had become guided by an "economic style of reasoning" (Berman 2022: 5–6). It was now thinking counterfactually. Today, we may describe this new standard of reasoning as more scientific than the old one. But notably, it was not established by maintaining a solid boundary between "science" and "policy" (or by blurring it) but rather by ignoring its relevance.

This first debate does not yet have much to do with RCTs, only with establishing the criterion that made them *politically credible*. This is where the second debate begins. After economists had established the net impact criterion in government thinking, the state hired numerous research firms to run RCTs. Yet, at some point, this led to the question of whether the approach favoured by the government was actually *scientific*. It was only then that boundary work between science and politics became a significant principle for the historical trajectory of RCTs.

By the 1980s, the problem was that people calling themselves "social scientists"—those employed at universities—would not touch RCTs. To them, interdisciplinary fields like program evaluation or the "policy sciences" remained largely suspect (Brunner 1982). Professional evaluators and academic social scientists were "living in worlds with different cultures and senses of what was at stake" (Gueron and Rolston 2013: 271). The situation resembled what the small group of people who *did* support RCT *and* were employed by universities termed a "Catch-22":

> …applied social research [primarily meaning RCTs] is relatively less prestigious inside the university; therefore, university researchers are not attracted to it, particularly as a full-time career line. And yet the reason why applied research is not highly regarded in the first place is that not much of it goes on in the university! (Rossi et al., 1978: 181).

While evaluation companies conducted sophisticated experiments, their efforts were not considered a part of legitimate social science. Why? One important reason was that a group around James Heckman, an econometrician who would win the Nobel Prize in 2000, held that sophisticated statistical modelling made RCTs largely unnecessary (Heckman 1979, 2020). Heckman's followers were opposed by a more diverse group of economists employed by research firms and academics frequently contracting with the government, who argued that proper experimental tests proved the assumptions of econometric models unrealistic (Breslau 1998: 87–88, see Fraker and Maynard 1987; LaLonde 1986). Overall, econometricians attempted to protect the discipline of social science from trial-and-error tinkering, while social experimenters attacked the academic orthodoxy from the scientific margins of applied research. To keep social science "pure", most university-based researchers could not approve of RCTs and their "contract shop epistemology" (Breslau 1997b).

The disdain social experimenters and social scientists held for each other is amusing enough to quote at length. Even when being interviewed in the 1990s, one of them still fumed:

> [People doing RCTs at commercial research firms] have absolutely no respect for data. They have absolutely no respect for knowledge. They're beltway bandits, pure and simple, just a bullshit operation. They have absolute contempt for anything. What they're good at is sort of greasing the palms of Labour Department bureaucrats with low IQs. And that's basically what they have got in the pocket right now (quoted in Breslau 1997b: 377–378).

To the people facing this kind of critique, things probably would not have appeared quite as amusing. Judith Gueron, long-time president of MDRC, the research firm mentioned previously, recalls that at the time, she was "fortunately […] not aware of the depth of vitriol or of the personal disdain some academics felt toward what they called 'contract shops'" (Gueron and Rolston 2013: 270).

In any case, while the debate partly mapped on the two parties' concerns for money and prestige, it was also an implicit fight about the *primary concerns of social science*. Academic economists held the immediate problems of government administration at arm's length, while social experimenters insisted on helping governments solve concrete problems. Because social experimenters' RCTs remained "directly action-guiding" rather than "epistemic" (Hansson 2015)—trying to find out whether things work in practice rather than how they work in theory—social scientists refused to regard them as science and remained sceptical of them.

Overall, the two debates show that RCTs are a far less stable object than assumed in contemporary EBPM or the standard historical perspective. Their credibility and scientific status had to be established through active struggles among various actors. And while boundary work is part of these struggles, the story is further complicated by the fact that the political credibility and scientific status

of RCTs changed independently of each other. However, one additional piece of a better understanding of the history of RCTs is how their credibility and scientific status finally came together. As I discuss now, this merging involved developments in medicine.

Medical trials, medical science, and the emergence of "the RCT" as a generic instrument of evaluation

Much rhetoric on the "gold standard" status of RCTs in present-day EBPM depends on the intuitiveness of an analogy: that running RCTs in social contexts is roughly equivalent to running RCTs in medical contexts. Following Nobelists Duflo and Kremer's (2005: 117) exclamation that experimental studies may "revolutionize social policy" just as they "revolutionized medicine in the twentieth century", medicine is commonly used as a scientific success story other fields should seek to emulate (see Leigh 2018: chapter 2; Manzi 2012: chapter 7).

Yet such an argument from analogy would not have been plausible for much of the twentieth century. As we will see, this is because medicine was itself a latecomer to RCTs, and it was hardly "science" that convinced the majority of medical professionals. As with social policy RCTs, the scientific status and political credibility of medical RCTs emerged largely independently. Experimental approaches were popularised and mandated on a large scale by commercial firms and the state during the 1960s and 1970s. Only later did medical experimentation become recognised as unambiguously scientific and turned into a politically credible blueprint for RCTs in the social realm. This finally turned "the RCT" into a *generic instrument of evaluation*, endorsed in scientific and political contexts and applicable to a large variety of problems.

Clinical trials are "newer than the automobile or the airplane, and about the same age as the color television or the electronic computer" (Manzi 2012: 78). While this fact is seldom made explicit, it is hardly controversial. Similar to the econometricians of the 1970s discussed previously, before the 1950s, doctors widely deemed randomised experiments unnecessary and unethical. Because medical professionals were confident about their knowledge of how to treat illnesses, randomly subjecting people to treatments "known" not to work seemed outrageous (Bothwell et al., 2020; Marks 2000: chapter 1). Yet, little by little, simplified versions of statistical insights (which had also initiated the first wave of social experiments in the 1920s and 1930s) gained ground among the medical establishment. While battles over whether medicine could or should be a science kept raging, by the 1960s, statisticians had convinced significant numbers inside the medical profession that experimental methods were worth considering, at least in principle (Marks 2000: chapter 5).

Yet the primary motivation for the widespread adoption of RCTs did not emerge through science, as understood then or today. Instead, it was due to capitalism and the determination of states to protect their citizens from the adverse effects of an all-too-free market. As the pharmaceutical industry expanded, regulatory agencies had to deal with large numbers of often unproven and sometimes dangerous drugs. Perhaps the most significant push came as a reaction to the thalidomide crisis of the late 1950s and early 1960s. First marketed by the West German company Grünenthal as *Contergan*, it soon turned out that the drug many women took against morning sickness during pregnancy led to massive increases in stillbirths and babies being born without arms or legs (Bothwell et al., 2020).

These events prompted the US Food and Drug Administration (FDA) to write some surprisingly stringent regulations. By 1970 (but not earlier), all pharmaceutical products had to undergo a sophisticated process involving randomised and double-blinded experimental studies. If this did not

lead to official approval, selling them was illegal. The new standard of demanding RCTs was soon adopted by all regulatory agencies of the industrialised world (Bothwell et al., 2020; Carpenter 2014: 238–245). By now, a global industry of contract research organisations (CRO) has taken over data collection and analysis (Petryna 2009). What we think of today as the "scientific standards" of medical trials could just as well be described as "industry standards" created and imposed by state power.[3]

So then, why do we regard these (very sensible) standards as so unambiguously scientific that they can work as a blueprint for social RCTs in contemporary EBPM? Given the early resistance among doctors and the involvement of regulatory agencies, one might guess quite the opposite. Indeed, the most significant case of boundary work to render medicine genuinely scientific (and insist on this status) succeeded only in the early 1990s with the emergence of "evidence-based medicine", or EBM (Daly 2005; Evidence-Based Medicine Working Group 1992; Sacket et al., 1996). Until the 1980s, medicine had been an "impure science" whose standards and scientific aspirations were constantly challenged (Epstein 1996). Yet, piggybacking on the new experimental orthodoxy in pharmaceutical research, EBM advocates like Cochrane began to clarify the evidence base of clinical practice and promoted systematic reviews, meta-analyses, and experimental evidence as the most scientific approach to medicine (Daly 2005).

One interesting example of how advocates worked the boundaries comes from the 1992 article that first popularised EBM in academic circles. Explicitly drawing on Thomas Kuhn's theory of science, the article referred to its project as "a new paradigm for medical practice" (Evidence-Based Medicine Working Group 1992: 2420). The dominance of clinical trials and meta-analyses in the assessment of new drugs, the article claimed, was what Kuhn had meant when he talked about paradigms as "ways of looking at the world that define both the problems that can legitimately be addressed and the range of admissible evidence that may bear on their solution" (ibid.).

EBM thus explicitly directed attention away from physiological mechanisms and common sense and toward advanced statistics and experimental methods. Much like Kuhn suggested for the development of science, it popularised these convictions in guidelines, textbooks, courses and journals. EBM also constantly referred to a selection of core exemplars, such as experimental tests of tuberculosis and polio in the 1940s and 1950s (Solomon 2011: 454–457). According to Kuhn, "the member of a mature scientific community is, like the typical character of Orwell's 1984, the victim of a history rewritten by the powers that be" (Kuhn 1970: 167). As the powers of EBM established their new paradigm of proper medical science, they rewrote history in their own sense.

Overall, the history of medical and social RCTs is almost analogous. As with social trials, clinical trials first became popular and politically credible in practical applications, outside the realm then understood as science. Like social scientists did at the time, doctors long rejected experimental methods, and it required tremendous regulatory pressure to make the pharmaceutical industry change course. Like social RCTs, clinical trials have been turned into the epitome of science only recently, through the successful boundary work of the EBM movement. Indeed, this movement was so successful that, in effect, it established RCTs as a *generic instrument of evaluation*—one that was credible politically *and* scientifically, and that could, in principle, be used in the medical *and* social realm.

[3] Policy researchers are used to concepts like "regulatory science" to describe the hybrid of scientific and regulatory knowledge common in regulatory agencies, but present RCT debates are not so subtle.

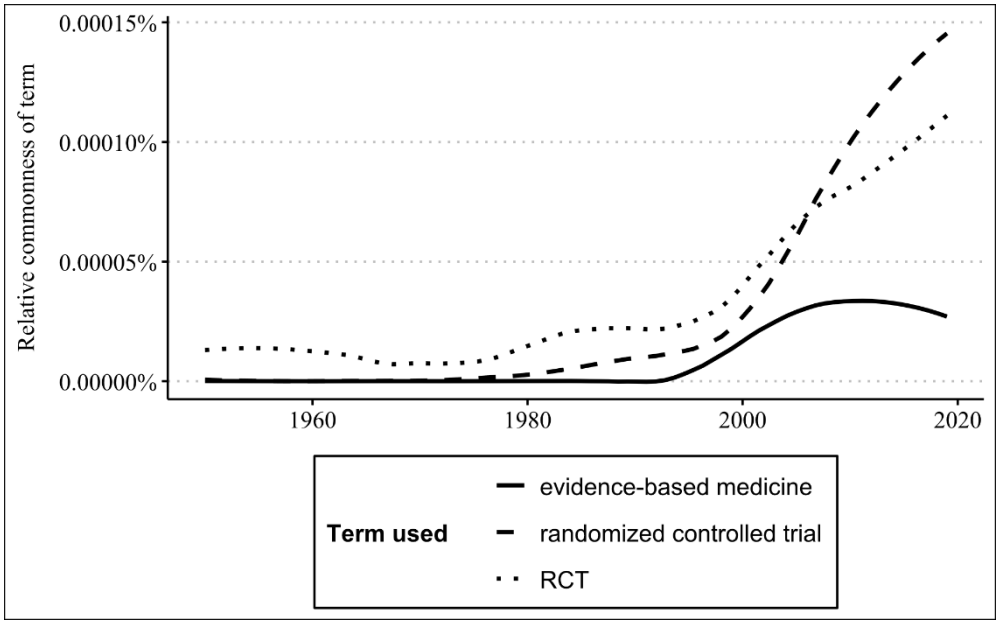

Figure 1 Mentions of "randomised controlled trials" and "evidence-based medicine" in general and scholarly literature, 1950 – 2019. Data: Google Books Ngram Viewer, 2019 corpus.

In a sense, one may even say that the rise of EBM created "the RCT" as such. As shown in Figure 1, the 1980s and 1990s were the time when the term "randomised controlled trial" first became common usage, making the hitherto familiar "social experiment" sound as suspicious as it sounds today. After medicine provided a plausible analogy for political credibility and unambiguous scientificity, RCT supporters could begin building the coalitions between players from science, business and politics that make today's EBPM movement so strong (Neuwinger 2024). In one sense, running a *social experiment* in the 1960s or an *RCT* in the 2020s is precisely the same activity. But in another sense, the history of experimental trials is genuinely discontinuous, beginning anew when "the RCT" emerged as an all-purpose evaluation tool of unambiguous scientific standing.

CONCLUSION

The present wave of RCTs was preceded by many previous ones. Historically, the contemporary "credibility revolution" in social science and EBPM builds on at least a hundred years of randomised social experimentation. But while in today's EBPM discussions, political credibility goes together with claims of social scientific rigor, these two components of the credibility revolution remained separate for much of the twentieth century.

Observing the history of RCTs from a contemporary perspective thus tends to obscure the fact that the credibility of experimental studies was established by governments and commercial research firms, whose actions we associate with social science only in retrospect. It also obscures the fact that some leading social scientists used to oppose RCTs during the 1980s—and that they did so not because they were "anti-science" but because they were "pro-science". Finally, the contemporary perspective obscures the fact that clinical trials (and medicine more generally) emerged as a generally accepted role model for EBPM only by the early 1990s, when scientific and political credibility came together with support from the evidence-based medicine movement. The notion that policy should be guided by social science is flattering to social scientists. But the history of the present "gold standard" of EBPM suggests that this notion is far less obvious than it may first seem—the outcome of subtle shifts in the meaning and purpose of social science.

Before concluding, it is worth considering two objections to these arguments. First, historically-minded people may point out that the reconstruction attempted here is no news to anyone. After all, isn't it common knowledge that progress and innovation have very often emerged from practical, economic, or regulatory endeavours rather than "independent science"? Isn't this what the history of statistics teaches us (Porter 1986), and couldn't the whole history of experimentation—not only of RCTs—be told according to the catchphrase that "tinkering and technology preceded science"? (Hansson 2015). These historical remarks are quite apt, and they indicate, as historically-minded people are fond of putting it, "what RCTs are a case of". However, the response to these questions is that most historical writings have, in fact, *not* treated RCTs as a case of "technology before science" or of boundary shifts in what we expect from "social science". If the argument is not particularly original, it is all the more notable that RCTs are usually discussed as an obvious example of "science influencing policy".

This leads directly to the second objection. More scientifically-minded people may ask why it matters at all whether RCTs are regarded as "scientific" or "technological" or anything else. After all, these are only words, and what matters is not words but whether RCTs can actually improve policymaking. Hasn't the "problem of demarcation" between science and non-science been shown to be quite unimportant long ago, a mere "*machines de guerre* in a polemical battle" about nothing in particular? (Laudan 1983: 119, emphasis in original). Again, this objection is understandable. Yet it only emphasises the practical importance of boundary work. Dismissing matters of demarcation as sophistry—insisting that merely verbal debates should be ignored—underestimates the way sophistries about "science" and "non-science" might be precisely the kind of *socially significant sophistry* that keeps contemporary EBPM debates going. While their track record of improving policymaking is mixed at best, RCTs are nevertheless seen as the "gold standard" for achieving improvements in the future. Maintaining this hope largely depends on present-day RCT supporters' success in presenting their favourite tool as unambiguously scientific.

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ARTICLE

Making Rankings Credible: How Ranking Creators Navigate Trials Of Strength

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Abstract

In this article, we take a closer look at the production of social knowledge beyond conventional academic sites in two NGOs that produce rankings of nation states. Whereas extant literature on rankings has mainly focused on the critique of methodologies or the inherent power of numbers, we suggest viewing them as webs of practices aimed at generating credible knowledge about the world. Using Latour's concept of trials of strength and applying it to two in-depth case studies, we investigate these practices empirically to unpack how these organisations attempt to create epistemic credibility.

Keywords

Rankings, trials of strength, knowledge production, credibility practices

INTRODUCTION

Rankings and similar evaluation practices have proliferated in many different areas of social life (see Beaumont and Towns 2021; Ringel et al., 2021a). Global governance is no exception: global performance indicators, including rankings of nations, have become increasingly common since the 1990s (Kelley and Simmons 2019). For example, the Global Benchmarking Database by the University of Warwick listed a total of 334 global indicators in 2021.[1] Many areas of global policy making are marked by quantitative indicators, and their relevance as tools of governance that provide orientation in an increasingly complex global world has been recognised by social scientists (Cooley 2015).

The proliferation of global performance indicators in recent decades has raised the question of how to make sense of them from a social scientific perspective. A lot of writing has focused on the task of debunking, problematising, and calling into question the credibility of global policy rankings, stressing either the questionable methodological foundations, the choice of what is measured, the veiled power structures behind these indicators, or other features deemed problematic (e.g., Beaumont and Towns 2021; Bhuta et al., 2018; Merry 2016). What has been less present is the

1 https://warwick.ac.uk/fac/soc/pais/research/csgr/benchmarking/database (03.05.2024)

question of what the creators of these rankings do on a practical level to make them credible for policy and media reporting.

In this paper, we therefore take a different approach. Drawing on two case studies, we investigate how ranking creators work to create epistemic credibility – that is, how they support their claims of representing the world and try to make them count as credible knowledge. Instead of treating them as problematic or non-scientific *by definition*, we propose an empirical perspective treating rankings as practices of social knowledge production. Drawing on the definition of social knowledge by Camic et al., (2011), which includes non-academic agents like think tanks or policy institutes, we investigate questions on the specific practices in which the people behind these devices engage to make their rankings count as credible knowledge.

To address this question, we borrow a concept from Bruno Latour's sociology of science: *trials of strength* (1987, 1990). This concept implies a practice-based understanding of knowledge production. Objectivity is not attributed or denied by a sociological observer, but rather treated as a contingent outcome of negotiations and struggles among participants in particular settings. Analogous to a scientific experiment, the rankings we study can be seen as claims to knowledge: the people behind them invest considerable resources into forming durable networks of practices. This involves developing arguments, defining criteria, collecting data, observing and investigating their respective political environments, anchoring their judgments in defensible principles, or telling a coherent story about their findings and why they matter. By transferring this concept from scientific practices to the making of rankings, we align with Camic and colleagues in using the conceptual tools developed in Science and Technology Studies to make sense of broader forms of "social knowledge practices" (Camic et al., 2011:2). Rather than focusing on whether rankings produce objective facts, we will demonstrate that this quality is the outcome of continuous practical achievement that is "relative to trials of strength in specific settings" (Latour 1987:78). The ranking creators have developed different practices aligned with Latour's conceptualisation of trials of strength. At stake in these trials is the credibility of the rankings: do they represent credible – that is, in this instance: objective – knowledge or not?

To address this question, we proceed in several steps. First, we review how existing literature on rankings and indicators has approached the question of their epistemic credibility. We argue that fixating on particular aspects of rankings, such as their (imperfect) methodologies or their use of numbers, has led many researchers to overlook the complex practical work carried out by the creators to ensure their credibility and acceptance as knowledge in political arenas. Second, we briefly introduce the cases we studied – two rankings published by relatively small civil society organisations addressing climate change and financial transparency respectively – as well as the methodological approaches we used to study them. Third, we analyse how claims to epistemic credibility are articulated in the two cases, building on the concept of trials of strength. We look at how the ranking creators work to pass trials of strength in different regards: (1) by explicitly *situating* them in their respective policy environments, (2) by telling a specific story about the world that underpins the *operationalisation* of their indicators, (3) by consciously *managing the non-standardised* elements of their production and (4) by *recruiting other actors* to solidify credibility. We close by reflecting on the implications and further connections of this approach and how it can be used to better understand the production of rankings and other forms of social knowledge.

THE EPISTEMIC STATUS OF RANKINGS AND INDICATORS IN THE LITERATURE

Social scientists have investigated different aspects of rankings and indicators. Authors have pointed out the "reactivity" and side effects of law school rankings (Espeland and Sauder 2007), the embedded power relations and how they are shaped by particularistic worldviews (Rottenburg and Merry 2015), or how rankings have fostered competitive relationships among actors (Brankovic et al., 2018). The focus of this literature encompasses several key areas, including the history, the effects, the underlying assumptions behind the production, and the epistemic nature of rankings.

For this paper, we focus on contributions that address the question of epistemic credibility. We identify three strands of literature: (1) we consider arguments from what we call a "methodology-focused perspective" on rankings, which is made up mostly of scholars from international relations and political science (e.g., Cooley 2015; Kelley and Simmons 2019; Beaumont and Towns 2021); (2), we discuss a line of thinking we call the "power of numbers perspective", which comes mostly from anthropology and sociology (e.g., Rottenburg and Merry 2015; Merry 2016; Bhuta et al., 2018); and (3) we turn to contributions we label "rankings as social practice", which aligns with our perspective but mostly focuses on university rankings (e.g., Mehrpouya and Samiolo 2016; Brankovic et al., 2022; Hamann and Schmidt-Wellenburg 2020).

METHODOLOGY-FOCUSED PERSPECTIVE

The methodology-focused perspective mostly focuses on the epistemic status and the scientific quality of indicators with the goal of improving this quality (Cooley 2015); offering caution against their potentially harmful effects (Kelley and Simmons 2019); or sometimes simply describing them as "bad science" (Broome et al.; 2018:516). A common idea is that rankings are insufficient when measured against scientific criteria (Cooley and Snyder 2015; Beaumont and Towns 2021).

While studies in the methodology-focused perspective have rightfully pointed out the shortcomings of many indicators, we find that it tends to be too narrow to grasp them as a social phenomenon with its own internal complexity and empirical variation. While rankings do indeed claim epistemic credibility, focusing on critiquing and improving the methodology of rankings overlooks the social dynamics and processes. Our approach differs from this perspective in that we are interested in the specific practices and strategies in which ranking-producing organisations engage in order to make it possible for their products to count as credible knowledge.

POWER OF NUMBERS PERSPECTIVE

The second perspective we identify is the power of numbers perspective. It focuses on the use of numbers and quantification as the primary factor in making rankings count as credible knowledge. According to these scholars, numbers possess a certain inherent quality of persuasion and objectivisation (Broome and Quirk 2015). There is a widespread notion that indicators and rankings are articulations of certain (problematic) macro trends, such as neoliberalism (Davies 2015), audit culture (Shore and Wright 2015a, 2015b), or new public management (Desrosières 2015), though the latter two are often seen as sub-trends of the former. Connected with this point is a recurring emphasis on the power structures ingrained in numeric evaluation – while numbers tend to appear as neutral representations of the world, they are all but neutral, as ideology shapes what gets counted (Merry 2016).

While these studies have important merits in sensitising us to the embedded political agendas of rankings, we argue the social processes that lead to making rankings credible are not reducible to the

effects of numbers alone. Instead, practices of quantification are part of a web of practices involved in creating robust claims (Latour 1987). One example is the practical work invested by some ranking organisations to explain their reasoning behind the numbers in the methodology papers, or press releases. As we show in the empirical section of this paper, numbers often do not speak for themselves, but have to be embedded in narratives about social problems and their solutions to become relevant in political arenas.

Likewise, treating rankings as mere manifestations of audit culture or neoliberalism tends to overlook empirical variations in the networks of practices (see Wilbers and Brankovic 2023). To make this clearer, consider the differences between the PISA rankings published by the Organisation for Economic Cooperation and Development (OECD) on one hand, and the Financial Secrecy Index discussed at more length in this paper, which was initially published by a small group of activists and designed as a critique of economic inequality and the prevailing discourse on tax evasion (Hansen and Vestergaard 2018). Whereas one may plausibly argue that the former represents a certain form of neoliberal thinking,[2] it would hardly be convincing to classify the latter as such. It is part of a political project that centres on inequality as a social problem and advocates for a robust tax state as the solution. Treating both indicators only on the basis of their utilisation of numbers, while disregarding the entire web of institutions and practices around them, risks losing the potential of empirically investigating the specific ways in which practices are employed to produce and maintain rankings in specific cases.

SOCIAL PRACTICES PERSPECTIVE

Finally, the rankings as social practices perspective is the closest to our own: rankings are neither seen exclusively as forms of knowledge that need to be improved by social scientists, nor as statistical technologies of power, but rather as bundles of practices that constitute an interesting object of sociological inquiry. In particular, this literature emphasises the need for studies on the level of organisational practices (Brankovic et al., 2022). Some studies focus on practices of legitimisation such as "discursive work" and the ability of ranking-producing organisations to respond convincingly to criticism (Hamann and Ringel 2023) or their ability to link global with national policy arenas (Hamann and Schmidt-Wellenburg 2020). With a few exceptions though, the bulk of these contributions focuses exclusively on rankings and indicators in higher education. While we broadly align with the conceptual approach of these studies in focusing on organisation-level practices, we apply this perspective to global policy rankings, which have, so far, rarely been studied in this way (though see Mehrpouya and Samiolo 2016 for a noteworthy exception).

THE CASE STUDIES: CLIMATE CHANGE PERFORMANCE INDEX AND FINANCIAL SECRECY INDEX

The findings presented here are a result of two case studies that we have conducted on the Climate Change Performance Index and the Financial Secrecy Index. Both cases have in common their production by relatively small NGOs and can be characterised as "boundary organisations" (Medvetz 2012), meaning they wield a particular form of influence that depends on a mixture of ties to different societal domains, such as politics, media, or academia. The rankings can be seen as "public interventions" (Eyal and Buchholz 2010) that leverage expert knowledge - among other things - to influence public discourse and ultimately shape policy decisions in their respective areas.

2 Though this would still not be a sufficient explanation for its success, in our view.

The Climate Change Performance Index (CCPI) is a ranking that has been published yearly since 2005 by Germanwatch e.V. and is currently published in cooperation with the NewClimate Institute and Climate Action Network International. The aim of the publication is to be "an independent monitoring tool for tracking countries' climate protection performance"[3]. To this end, the 63 countries and the EU responsible for more than 90 percent of global greenhouse gas emissions (GHG-emissions) are assessed on the basis of a total of 14 indicators.

The Financial Secrecy Index (FSI) has been published by the Tax Justice Network biannually since 2009. According to its website, the FSI ranks "jurisdictions most complicit in helping individuals to hide their finances from the rule of law".[4] To do this, it combines a qualitative component, namely an assessment of countries' legal frameworks regarding financial transparency, with a statistical component called the "Global Scale Weight", which is a figure representing the number of financial services offered by a particular country to non-residents per year.

A commonality between the cases is the institutional position and the size of the organisations behind these rankings. Both Germanwatch e.V. and Tax Justice Network are non-profit organisations that rely on external, mostly project-based funding, for which they have to apply on an ongoing basis. To secure funding, they have to demonstrate the impact of their work and frame it as being oriented towards a specific goal (Krause 2014). Impact, in this sense, refers to the engagement by relevant stakeholders, such as other civil society actors, government officials, and the media. Both rankings are produced by teams comprised of less than 15 people.[5] The rankings have been continuously published for more than ten years and therefore allow insights into how ranking creators work to make sustained publication possible (see Ringel and Werron 2021).

Unlike official intergovernmental organisations, the funding of these non-profits is not guaranteed, and they do not command the resources, staff, or reputation of organisations like the International Monetary Fund (IMF) or the World Bank. Scholars have observed that even the IMF, a large intergovernmental organisation, relies on its reputation as an expert institution to influence member countries (Barnett and Finnemore 2004). This is even more relevant for small non-governmental organisations, which rely on their credibility as knowledge producers, while simultaneously lacking the resources, prominence, and direct channels to governments typically possessed by intergovernmental organisations.

METHODS

Our empirical approach is characterised by the triangulation of different methods (Flick 2011) and data sources used to gain a comprehensive understanding of the practices of ranking production (Schatzki 2012). In line with the dictum "follow the actors" (Latour 2007), we draw on different data types to make sense of how ranking creators assemble their knowledge claims across different sites – including, for example, internal discussions about press releases as well as public-facing communication on the ranking's website. To generate insights into the daily routines and activities of the participants, each of the authors conducted ethnographic fieldwork over several months at one of the organisations described above, producing observation protocols of everyday work (Breidenstein et al., 2013). This approach was complemented by semi-structured, expert interviews

3 https://ccpi.org/ccpi-philosophy-team/ (03.05.2024)

4 https://fsi.taxjustice.net/ (03.05.24)

5 The number varies each year depending on available resources and the specific time of the production process. Both ranking teams use temporary workers like interns to support the core team.

to gain access to participants' perspectives on their work (Kaiser 2014), and practice-oriented document analysis (Asdal and Reinertsen 2022) to integrate the many written documents produced by our research objects.

Aligning with our methodological approach, the argument we develop in this article is based on a mixture of different types of data. The participant observations, interviews and documents have informed our thinking about ranking production. For the analysis presented in this paper, we mainly draw on publicly available documents, complementing these with other data types where needed. In short, the presented analysis would not have been possible without the contextual knowledge acquired through field work.

The ethnographic data was collected in two phases in each of the cases allowing for an initial phase of open observation and a second phase of selective and focused observation. For the FSI, virtual fieldwork lasted eight months in total. During these phases, Can David Tobias was actively involved in the ranking production work on a part-time basis,[6] conducting participant observation and generating fieldnotes and observation protocols. In total, 20 interviews were completed, 14 with members of Tax Justice Network and six with external informants. In the case of the CCPI, the participant observation was conducted by Elisabeth Strietzel over two phases, each lasting three to four months. The first phase entailed full-time participation as a member of the ranking team, while the second phase involved multiple observation periods across the ranking cycle, with no active involvement in the ranking production. For the CCPI, 27 interviews with 18 different informants were conducted. In both cases the organisations and participants were aware of the conducted research.

Our interpretation is based on an in-depth examination of the material, followed by multiple cycles of interpretation and discussion between the two authors. To facilitate systematic insight across the data, we employed the qualitative analysis software MAXQDA to code the data, including field notes, transcribed interviews, and collected documents. Crucially, the comparison and discussion of similarities and differences across the two cases enabled us to identify common themes and interesting variations. The empirical categories that we present in the analytic part of this paper are the result of an ongoing comparative discussion between our cases (Kelle and Kluge 2010). The goal of our research design is to conduct two explorative case studies (Savin-Baden and Major 2012:154f.) in which to explore the research gap regarding the sociological understanding of the production of (nation state) rankings. While this in-depth view allows us to gain insights into the inner working of ranking production, this study design does not allow insight into the effect of the two rankings, an outside perspective on the ranking, or insights into commonality of the found practices across different rankings.

THE CLIMATE CHANGE PERFORMANCE INDEX AND THE FINANCIAL SECRECY INDEX IN TRIALS OF STRENGTH

In the following section, we turn to the empirical analysis of how the epistemic credibility is constructed in the cases we studied. We use Latour's concept of trials of strength (1987; 1990), applying it to global policy rankings. This lens helps us to view epistemic credibility (or objectivity) not as a fixed fact but as the result of ongoing efforts and negotiations by actors. According to Latour,

6 The production of the FSI takes place entirely on a virtual basis, meaning all members work from their home office and interact through digital devices like Zoom, Slack or Sharepoint. Part time means that the ethnographer was involved in the organization for 3 days per week on average.

each (scientific) claim to credible knowledge undergoes three different trials of strength that are anticipated by actors, in which the robustness of the claim is put to the test: one on the level of narratives, one on the level of evidence, and finally one in front of an audience where all three are put to test. The gist of this triangular conception is explained in this way by Latour: "An experiment is, however, none of these trials alone. It is the *movement* of the three *taken together when it succeeds or separated when it fails*" (Latour 1990:61, emphasis in original). Actors therefore need to anticipate all three trials to assemble a robust claim.

In our understanding, trials of strength do not only refer to *public* trials in which a claim is tested and other actors attempt to dismantle it, but also to the practices of anticipating dissent devised by the author of a claim. As experienced members of their political arenas, the organisations we studied are well aware of the public scrutiny they could face (and have faced in the past). Therefore, their strategies of preparing the rankings to withstand this scrutiny are as much part of their trials of credibility as the public challenges they face.[7] The former is the focus of this paper.

With this understanding of how epistemic credibility can be constructed by actors, we look at how the ranking creators we studied prepare the ranking to resist trials of strength in four different regards (see table 1): gaining credibility by *connecting* to higher facts in their respective policy environments; *balancing* mechanical objectivity and trained judgment; devising a specific *reasoning* in their design of indicators that rests on a narrative about how the world works; and *recruiting* allies to solidify the credibility of the rankings.

Table 1: overview of empirical practices

Trials of strength (Latour 1990)	**Cross-case practices**	**CCPI**	**FSI**
Evidence	Connecting claims to an established fact	Keeping nation states accountable to the Paris Agreement	Introducing fairer, more universal criteria than OECD's tax haven blacklists
	Managing non-standardized judgment	Externalizing judgment	Proactive transparency of non-standardized judgment
Narrative	Reasoning behind operationalization	Climate mitigation as a responsibility of nation states	Scale of financial business defines responsibility of nation states
Audience	Recruiting allies for publication	Mobilizing partners from civil society	Mobilizing partners from civil society

To set the stage for the analysis, we first give one example of criticism faced by the two rankings in the past, showing the contested nature of their epistemic credibility. Following this, we show how trials of strength play out in our two cases.

7 Latour also analyzes practices of anticipation with regards to his concept of trials of strength, and not public trials exclusively (see 1987; 1990)

WHY TRIALS OF STRENGTH MATTER IN RANKING PRODUCTION

To understand this drive for objectivity, it is essential to comprehend the scrutiny to which the rankings are subjected. It is only through the ability to effectively anticipate criticism that ranking teams are able to attain a status of expertise (see Ringel et al., 2021b). By passing trials of strength the rankings can be regarded as credible. The following example demonstrates how one of the two cases is subjected to criticism and how this question of credibility is relevant to ranking creators.

In 2019, the Australian Prime Minster challenged the credibility of the CCPI. This came after the country received the worst climate policy rating of ranked countries a few weeks earlier.[8] When asked about the CCPI report in a press conference on, inter alia, the climate policy of the country, the following exchange took place:

> JOURNALIST: Prime Minister, just on climate change, you said there's a global effort, but Australia ranks last in the world on climate policy in a new global index. Isn't that an indictment of your Government's response?
>
> PRIME MINISTER: No, I completely reject that report. We don't accept that.
>
> JOURNALIST: You don't accept the report?
>
> PRIME MINISTER: No.
>
> JOURNALIST: Why not?
>
> PRIME MINISTER: Because I don't think it's credible.[9]

This criticism can be seen as part of trials of strength about the credibility of the ranking. The journalist uses the CCPI to question the Australian Prime Minister's claims of adequately addressing climate change. The ranking results continued to be used by other actors to call for political action from the Australian government which indicated that the critique was not successful. Just a few months later, Greenpeace included the ranking results in a press release about protests at Australian embassies and consulates to claim that the Australian government is not taking sufficient actions on climate change.[10]

This example is not an isolated event in the history of both cases, nor in that of rankings more generally. As Ringel and colleagues (2021b) have shown, this dynamic also applies to rankings more broadly, as many ranking-producing organisations devise strategies to react to and resist public contestations. (Policy) rankings can therefore be seen as a form of social knowledge characterised by its predisposition to attract criticism as well as by conscious efforts to anticipate this and defend itself. The example illustrates why the question of credibility is important for ranking creators. If the specific ranking is not seen as credible, it cannot be used as a tool for advocacy work. This demonstrates the need for epistemic authority of rankings to intervene in political discourse.

8 https://www.theguardian.com/australia-news/2019/dec/13/author-of-report-ranking-australia-worst-on-climate-policy-hits-back-at-pms-claim-its-not-credible (03.05.2024)

9 https://web.archive.org/web/20210115174305/https://www.pm.gov.au/media/press-conference-melbourne-vic (03.05.2024)

10 https://www.greenpeace.org/international/press-release/28338/greenpeace-holds-global-protest-telling-australian-government-act-on-climate-phase-out-coal/ (03.05.2024)

CONNECTING TO A MORE ESTABLISHED FACT

In *Science in Action* (1987: 74-79), Latour describes how a claim is challenged by a "dissenter" on individual parts of an experiment. As the dissenter scrutinises one element of the argumentative chain after the other, the claim is defended by *linking* the claim to a principle, idea, or fact, which has already been *established*, so that the dissenter is forced each time to challenge the accepted principle. Latour argues that a claim to epistemic credibility gets validated (or objectified) when its link to more widely accepted claims is maintained:

> Every time the visitor [i.e., the dissenter] followed a lead he reached a point where he had either to quit or start a new controversy about a still older and more generally accepted fact… This means that when the doubter tries out the connections, all these other facts, sciences and black boxes come to the Professor's rescue (Latour 1987:77–78).

Looking at the Financial Secrecy Index, we can find an example of how this plays out in the case of a ranking. Proponents of the Financial Secrecy Index often argue that it constitutes a 'non-political' ranking, relying on 'objectively verifiable criteria'.[11] Yet, these statements do not stand for themselves, but can be understood by an informed observer as performative speech acts that "do" something in social reality that goes beyond their propositional content (Austin 1971): they implicitly distinguish the FSI against a specific other, the tax haven blacklists.[12] These blacklists were first introduced by the OECD in the late 1990s and represented the first attempt by a major international organisation to directly name specific countries for their alleged complicity in transnational tax evasion. By singling out smaller jurisdictions beyond the financial centres of the world – and especially by not including well-known tax havens like Switzerland, Luxemburg or the US state of Delaware – these blacklists caused major political turmoil and a strong backlash from a variety of actors, including tax experts from civil society (Mayne and Kimmis 2000; Christensen 2007). A key point in the critique of the blacklists was their neglect of more powerful countries: even though OECD member states like Luxemburg or Switzerland were known tax havens, they did not appear on the blacklist. By contrast, less powerful countries like Liberia, Panama or Samoa were included. Critics took this as proof that the lists did not apply the same criteria to everyone, and instead were politically tainted (Dean and Warris 2020). This explains the emphasis on being a 'non-political' ranking on the part of the Financial Secrecy Index.

The following quote by Alex Cobham, chief executive of Tax Justice Network, in which he criticises the EU's ongoing blacklisting effort in 2017, exemplifies the way the Financial Secrecy Index is explicitly framed as the better – that is, more impartial – alternative to the blacklists:

> There's a long and largely ignominious tradition of tax haven blacklists, mainly at the OECD and IMF. They've tended to be subjective efforts, naming economically smaller jurisdictions with less political power, and steering well clear of major financial centres – regardless of their behaviour. The Tax Justice Network established the Financial Secrecy Index in 2009, largely as

[11] These wordings were used frequently by participants in conversations and interviews with the ethnographer as well as in public communication, for example this blog: https://taxjustice.net/2017/11/27/blacklisting-the-eu-paradise-lost/ (03.05.2024))

[12] The concept of a blacklist establishes a discrete, two-tier hierarchy between 'good' and 'bad': in the case of the tax haven blacklists, a number of countries are listed as non-compliant, while those not appearing on the list are implicitly marked as compliant. Those not on the list are thereby 'whitelisted', i.e., not considered to be doing anything wrong (hence the backlash against the non-listing of countries like Switzerland or Luxemburg). Thus, in contrast to a ranking, what matters is not *where* a given country is on the list, but whether it *is* on the list at all.

> an attempt to put transparent and objectively verifiable criteria in place, to allow a fair comparison of jurisdictions – a level playing field, if you like.[13]

In more abstract terms, the critique of the blacklists revolved around the question of fairness and universalism. While claiming to represent a global-universal view on the issue of tax evasion, in the eyes of many observers they were clearly tainted in the interest of powerful OECD member countries. Hence the critique of a "political" list that is not based on fair and universal criteria, but a mere articulation of pre-existing power constellations: if you are powerful enough, you can do what you want and still not land on the list. Instead, what was needed according to the critics, was a "level playing field" that treated all countries in an equal way and applied the same criteria, regardless of their political influence.

It is precisely this argument that represents one of the leading legitimisation strategies of the Financial Secrecy Index. By placing both smaller countries as well as the biggest economies of the world on a "secrecy spectrum" (Cobham et al., 2015:283) while considering the scale of their financial business, the creators of the Financial Secrecy Index translated the critique of the blacklists into an argument for the credibility of their ranking. In this way, the creators of the Financial Secrecy Index connect it to a more established fact: that of fairness and equal treatment. The language of a "non-political list" and "objectively verifiable criteria" represents this connection.

In contrast to the FSI, the CCPI is embedded in a very different way in its political environment. The methodological construction of the CCPIs indicators is aligned with the 2015 Paris Agreement, which has been the basis of the latest methodology used since 2017. The Paris Agreement marks a departure from the Kyoto Protocols approach of climate action focusing on regulation to "a framework for making voluntary pledges that can be compared and reviewed internationally, in the hope that global ambition can be increased through a process of 'naming and shaming'" (Falkner 2016:1107). Linking the methodology of the ranking to this UN institution and to the consensus reached by nation-states during negotiations, the CCPI translates this consensus into the definition of targets to assess the performance of countries in climate mitigation. This illustrates how the CCPI was reshaped to mobilise international and domestic pressure to push for the pledges that lay at the heart of the Paris Agreement.

One aspect in which the connection between the CCPI and the Paris Agreement can be traced throughout the ranking's publication is the phrasing "well below 2°C". This is a phrasing based on the Paris Agreement designating the goal set during the climate negotiations to limit global average warming to this target (Leemans and Vellinga 2017). Since the Paris Agreement, this phrasing can be found in the texts published alongside the ranking. Referring to this phrasing as a mark of successful climate action, the CCPI team incorporates the ranking into the broader framework of global climate efforts.

Compared to the FSI, which creates a connection to an established concept in its policy field through critique, the CCPI creates a connection by aligning the ranking criteria with the established Paris Agreement. This means that for this aspect, in order to call the criteria of the CCPI into question, critics would have to either show how they do not accurately represent the goals of the Paris Agreement – i.e., sever the links of the CCPI's claim with the more established fact of the agreement – or they would have to question the Paris Agreement itself, and subsequently find themselves in an

[13] https://taxjustice.net/2017/11/27/blacklisting-the-eu-paradise-lost/ (03.05.2024)

entirely different discursive arena. In Latour's terms, the Paris Agreement acts as an ally to the CCPI strengthening the rankings in a trial of strength.

By comparison, the FSI does not tie itself as directly to established institutions in its policy field, but rather draws its main rationale from criticising the tax haven blacklists. At the same time, however, we can see how the index also connects itself with a "more established fact": by incorporating the *level playing field*-argument into its methodology, the FSI invokes the universalistic discourse of global justice and fairness. Again, critics would have to either show how the FSI does not accurately represent this principle, or they would have to challenge the principle of a level playing field itself – that is, in this case, of equal rules applying to all countries.

REASONING BEHIND OPERATIONALISATION

Another aspect of the credibility of rankings is the reasoning behind the selected indicators. It refers to why the indicators are chosen, what is measured, and how this relates to the evaluated issue. As we have discussed above, scholars in the powers of numbers-perspective have rightfully pointed out that there are always contingent decisions involved in choosing what is measured and how it is weighted. In the following section, we look at how this reasoning is used in strengthening the claim to credibility. This reasoning can then also be subjected to tests of strength, as critics can call into question the connections it makes. If the connections do not hold, the credibility of the ranking is dismantled.

One way of gaining credibility through the reasoning behind the operationalisation, and selection of indicators, can be seen in how the rankings aim to record improvements over time. This aspect of the social practice of rankings has been pointed out by Ringel and Werron (2021). As serial comparisons – repeated publishing with updated data – rankings create comparison over time. The competition is therefore not only between the ranked entities, but also between current and past performance in the ranking.

In the case of the CCPI, the rankings methodology is aimed at capturing the process behind lowering GHG-emissions. The CCPI consists of four different categories of indicators: (1) GHG-emissions, (2) energy use, (3) renewable energy, and (4) climate policy. Each of these categories is measured by a set of indicators designed to assess current status and trends over time. The category of GHG-emissions is weighted the most with 40 percent, reflecting the notion that this is the category that ultimately needs to improve to mitigate climate change. But rather than only measuring GHG-emissions, the ranking team includes three other categories (weighted 20 percent each). This choice of indicators and their weight in the evaluation contain the way in which the ranking team envisions climate mitigation. It makes clear that the climate crisis should be addressed by nation states through climate policy aimed at lowering energy use, expanding renewable energy, and ultimately lowering GHG-emissions.[14] This is not implicitly included in the construction of the ranking but expressed in documents accompanying the rankings publication, as found in this description:

> Whether these policies are effectively implemented, can be read – with a time lag of a few years – in the country's improving scores in the categories "Renewable Energy" and "Energy Use" and

[14] Technological improvements such as carbon capture, or individual consumption limitation like carbon footprints are therefore not seen as the main way to climate change mitigation. The CCPI ranking creators clearly emphasis the responsibility of nation states to act rather than to individualize the solution as is done in the concept of the carbon footprint.

> lastly in positive developments in the category "GHG Emissions"... This weighting scheme allows the CCPI to adequately capture recent changes in climate policy and newly achieved improvements on the way to reduce GHG emissions. As GHG emissions reductions are what needs to be achieved for preventing dangerous climate change, this category weighs highest in the index (40percent) (CCPI Background and Methodology, 2024:4).

In presenting these steps towards climate mitigation, the team proposes a model of measuring and promoting the process of climate action by national actors. They argue that this way of measuring accounts for the time and processes it takes to lower GHG emissions on a national level. This narrative of change needs to pass tests of strength to support the status of the ranking as an objective measurement of climate action.

Dissenters could sever this connection between the four categories and/or between the indicators used to represent these categories in questioning this narrative. If successful, the ranking would be marked as unreliable, but if this narrative survives trials of strength the ranking is one step closer to being seen as credible. For example, we could imagine a dissenter who contests the assumption that national climate policy is needed to promote the lowering of GHG emissions. Rather than focusing on nation states, one could point to needed technological advancements of carbon capture or the use of nuclear energy.[15] If this was successful at severing the link between climate policy and the other categories, the ranking itself would fail the trial of strength. To be successful in severing the connection the dissenter would therefore either need to present a stronger network or be able to dismember the connections the narrative proposes.

The FSI, on the other hand, promotes an image of tax evasion that is not only incongruent with other representations found in its policy area, but in part directly challenges them (Seabrooke and Wigan 2015). Similar to the critique of the tax haven blacklists described above, it also sharply contrasts with the Corruption Perception Index (CPI). The main point made by the proponents of the FSI regarding the difference between the two indices is that the CPI represents corruption as an individualised problem ultimately caused by national elites – overwhelmingly in countries of the global south.[16] The FSI is intentionally designed to incorporate this critique, as it aims to frame the problem of tax evasion not as one of individual countries, but rather as a shared global problem, to which countries contribute in varying degrees. According to this reasoning, the rank of a country does not reflect the transparency of financial legislation alone, but also its responsibility towards other countries. This is measured in the number of financial services provided to non-residents in a given period. On the level of indicator design, this ambition to include a measure of the responsibility of countries is reflected by the so-called global scale weights, yielding a very different picture of the problem in which countries like the US, Germany or Japan rank among the worst spots – contrasting with the CPI, which ranked Syria, Venezuela and Somalia in the three lowest spots in its 2023 edition.[17]

[15] While this section focuses on an *imagined* dissenter, there are many instances where the creators of the CCPI are confronted with real dissenters proposing alternative paths for climate change mitigation in general or directly challenging the choice of indicators. The creators of the ranking pay close attention to the different lines of argument and evaluate whether they need to change the choice of indicators. It is not necessary to understand one specific dissent in order to understand the motivation of the ranking creators to clarify their reasoning. Rather, using Latour's concept, we can understand that the creators of the ranking anticipate various (imagined) dissenters in the conceptualization and communication of the ranking, and therefore develop practices to make their claim defensible.

[16] https://www.taxjustice.net/cms/upload/pdf/0701_Mirror_Mirror_corruption.pdf (03.05.2024)

[17] https://www.transparency.org/en/cpi/2023 (03.05.2024)

The reasoning behind this choice has been called into question several times. In particular, the connection between a large number of financial services exports and a higher responsibility towards other countries to maintain transparent laws has been attacked. One example of this is a report by Cayman Finance, an organisation representing the financial sector of the Cayman Islands, published in 2020 after the jurisdiction had been ranked in the worst spot of the FSI.[18] Among other criticisms, the report argued that the number of financial services exports is in fact a measure of success, in that it shows how well a country has attracted foreign capital. Therefore, it argued, the FSI unfairly penalises countries with successful financial sectors (Mansour 2020). In an official statement published on its website, TJN responded to this challenge by argumentatively reinforcing the link between global scale weight and responsibility towards the global problem:

> Secrecy jurisdictions often twist this analysis [that includes the global scale weights, author's note] on its head, claiming they are being treated unfairly for 'successfully' attracting money from non-residents… These claims miss the central purpose of the Financial Secrecy Index's ranking of countries by their supply of financial secrecy as opposed to the secrecy of their financial laws: All countries have a responsibility to safeguard against financial secrecy. The more financial activity a country seeks to pull in from other countries' residents, the greater the responsibility that country has to make sure its financial sector is not abused by people to evade or avoid tax in other countries or to launder money they've obtained elsewhere through illegal means. (Mansour 2020)

The fact that TJN staff invest the effort to publicly react to these challenges and take up the point of the global scale weights underlines that the narrative is not self-evident and needs to be explained and defended against scrutiny – or in other words, to stand in trials of strength. The intervention by Cayman Finance can be seen as an attempt to sever the narrative connection between "contribution to the global problem tax evasion" and "size of financial sector", while TJN's reaction represents a reinforcement of that connection, as it is an integral part of the FSIs underlying narrative.

Taking both cases together, the credibility of rankings does not only rest on the relationships the ranking teams established with higher concepts within the policy field alone, but also on the way it succeeds in arguing for a reasoning behind the operationalisation. In the case of the CCPI, this can be found in the way the ranking imagines a way for nation states to transition to lower GHG emissions. The FSI, on the other hand, suggests a specific understanding of the problem of tax evasion as genuinely global, to which countries contribute in different measures and correspondingly carry more or less responsibility to make their financial systems transparent. Both of these reasonings are neither self-evident not completely consensual in their respective policy areas. They need to be defended and underpinned by convincing narratives that can be articulated each time the operationalisation is scrutinised.

MANAGING NON-STANDARDISED JUDGMENT

Another way in which the CCPI and FSI strive to create epistemic credibility is that they both make the problem of non-standardisable elements of the ranking process transparent. Many authors argue that measuring nation state performance according to standardised criteria is by definition problematic, as the world does not lend itself smoothly to standardisation (Timmermans and Epstein 2010; Merry 2016). In line with this, the ranking creators we studied devise practices to deal with

[18].https://caymanfinance.ky/2021/09/22/flawed-cayman-finance-challenges-credibility-of-tax-justice-networks-biannual-financial-secrecy-index/ (03.05.2024)

the problem that their standardised criteria and methodologies – which are, as we have shown above, a crucial prerequisite for their credibility claim – do not easily apply to all individual nation states in exactly the same way. Sometimes information is harder to find, ambiguous, or sometimes the particular situation might be different.

Borrowing from Daston and Galison (2007), we can reformulate this problem as the tension between mechanical objectivity on the one hand and trained judgment on the other. In the case of the FSI, this can be found in the internal evaluation practices of the ranking team. The CCPI, in contrast, externalises judgments to third parties allowing for so-called country experts to highlight issues specific to the respective nation state. In other words, they have both developed distinct strategies to manage the unavoidable need for non-standardised judgments.

In the case of the FSI, the tension between mechanical objectivity and trained judgment is addressed by conscious effort to make explicit where the limits of the former are reached and where the latter is needed. The need of referring to trained judgment in some instances is acknowledged in the following passage of the methodology document:

> In cases of conflicting information, we resorted to reasoned judgement – while recognising the necessary subjectivity of the approach. Where this was the case, therefore, we aim to provide full transparency about criteria and interpretation. As a result, in addition to references to all underlying sources, the database reports also include a large amount of supporting information and notes relating to data analysis (FSI Methodology 2022: 18).

For each datapoint that ultimately makes up the ranking, the FSI staff publishes a note aimed at explaining the reasoning behind that particular judgment. The majority of these notes refer to the standardised criteria that were applied to reach the specific score.

However, there are instances that do not allow a mechanised application of criteria and instead require extensive interpretive work to match a given legal situation with the standardised FSI criteria. One such example is the assessment of the implementation of a so-called beneficial ownership register for US companies in the FSI edition of 2022. The US had just passed a law introducing such a register – yet the FSI team ultimately decided it was insufficient as it had too many exemptions.[19] The judgment, which led to a poor score in the ranking, is accompanied by a multi-paragraph text on the index website providing a point-by-point explanation of the reasoning. As this example illustrates, the situations requiring such interpretive work are explicitly marked as such by the authors of the FSI. In other words, the limits of mechanised objectivity are acknowledged and there is a conscious effort of *managing* these situations in a proactive way. Thus, the situation in which non-standardised judgment becomes necessary is rationalised on the terms of the author – forestalling possible critique of subjectivity and aiming to strengthen the claim.

In the case of the CCPI, non-standardised judgments can be seen in the texts that are published alongside the ranking. These texts use what we call externalised judgment to explain the ranking results. This externalised judgment is collected in an annual survey sent out by the ranking creators to local partners. The local policy experts, most of whom work in climate change NGOs, answer questions on various aspects of their country's national and international climate change policies, such as fossil fuel phase-out, implementation of climate change laws, or land use policies. This survey is the basis for the policy indicators and informs the text explaining the ranking results of the

[19] https://fsi.taxjustice.net/country-detail/#country=US&period=22 (03.05.24)

evaluated countries. In the so-called country text, written for each of the ranked nation states, the input of the country experts is intertwined with that year's ranking results, and past performances.

Each text is written by the ranking team, reviewed by country experts, proofread by an editor, and finally published with the ranking on the launch day. The texts connect the quantitative indicators with the specific political context and demands of local NGOs. The evaluation used for this is based on the individual judgment of the participating country experts. This is emphasised in the country texts as the individual assessments of policy, and calls for concrete policies are attributed to country experts. Country experts are therefore assigned the capability to make a trained judgment about the country's climate policy while the ranking creators do not provide this kind of judgment. As one example, aspects of Denmark's general climate policy are highlighted as evaluations of the country experts: "All the experts agree that implementing a tax on agricultural production would be a crucial step towards lowering the country's high emissions in this sector".[20] Demands by the country experts for further action are then framed as a prerequisite for continued good performance in the ranking.

In giving external voices a platform to demand specific policy improvements, the ranking team make a clear distinction between their evaluation based on mechanical objectivity and the trained judgment of country experts. These trained judgments are therefore externalised and a differentiation is made between the expertise required for these judgments and the expertise of the ranking team. As such, the claim of credibility is strengthened as the ranking team extends the connection to other actors to support trained judgments and therefore makes clear that they are not experts in climate policy for all ranked nation states.

SOLIDIFYING CREDIBILITY BY RECRUITING ALLIES

Another element contributing to the credibility of the rankings is their endorsement by external actors. Around each edition's release, the ranking creators actively seek allies to buy into the claim and to multiply it within their respective context, especially national publics. Not only does it expand the public reach, but we argue it is vital for them to be reproduced by actors beyond the original creators to strengthen credibility. As Latour writes: "An idea or a practice cannot move from A to B solely by the force that A gives it, B must seize it and *move* it" (Latour 1988:15f.). Each positive reference made to the ranking by a local civil society actor, a journalist writing for a national newspaper, or a politician engaging in a discussion; solidifies the credibility of the ranking, and as such, actors make their endorsement visible to wider audiences. In this sense, we interpret the efforts of the ranking creators to find and mobilise allies to adopt the ranking in their own communication as a preparation for trials of strength with regards to audiences (Latour 1990:62).

In our cases, the presentation does not take place on a single stage, but on multiple stages, ranging from global political fora like the annual Climate Conference to local civil society actors using the ranking in their advocacy work. For each of these stages, the ranking creators try to find allies – mostly local civil society organisations, but also journalists – to act as intermediaries and amplify the ranking for each particular setting. On the part of the ranking creators, the main task in this regard is to recruit allies willing to adopt the ranking for their own projects and thereby put their weight behind its credibility (Callon 1984).

As Ringel (2021) has shown, the releases of rankings in today's world are often "painstakingly orchestrated performances" (ibid.: 56). Ranking creators invest considerable resources into planning

[20] See https://web.archive.org/web/20240409190443/https://ccpi.org/country/dnk/ (03.05.2024)

launch events, crafting visualisations, or finding storylines to align the rankings with current public debates. This is also true for the cases we studied. In the following paragraphs, we offer a brief overview of the practices devised by these organisations aimed at increasing engagement by external actors.

The CCPI team uses the yearly climate negotiation as a platform to hold a press conference at the COP venue. The ranking creator encourages local partner organisations to write and publish their own position about the ranking and its results. To make this easier for partner organisations, they offer a pre-release package to those who participate in the policy evaluation to give them time to prepare their own reaction to ranking results. The pre-release package includes information on the results of the ranking as well as graphics illustrating the results. It is not compulsory for partners to use these materials, but they are encouraged to use them as they see fit. This decentralised media strategy offers locally working partner organisations the benefit of preparing their own (social) media content for the publication of the ranking.

The team behind the FSI follows a similar strategy of publication, the main difference being the lack of a similar event to the climate conference for global tax governance. Prior to the official launch date of the Index, the in-house team of the Tax Justice Network uses various strategies of getting external actors on board. When preparing a global press release for the ranking, the team discusses which issues might be relevant to the transnational community of tax policy actors. At the same time, they reach out to regional and national partner organisations and journalists to share the release date and ranking results with them ahead of time. Additionally, the press team of TJN, similar to the CCPI, prepares so-called "partner kits" which summarise the ranking results for a given country. They also co-host region-specific launch events with some of these partners, presenting the central findings of the ranking to audiences mobilised by the local partners. The importance of getting these intermediaries to engage with the ranking is visible throughout the publication process.

Taken together, these publication practices underline the importance of engaging with external actors for ranking creators. In both cases, the creators invest significant resources to encourage intermediaries like national civil society partners or journalists to disseminate the ranking in their own name. This not only expands the range of the ranking and helps to make it relevant to more stakeholders but also reinforces its credibility. The concept of trials of strength thus helps us to understand the practice of recruiting other NGOs to the ranking, not only as a means of enhancing its visibility, but also as a strategy for expanding the network of defenders against criticism. Each positive engagement solidifies the credibility of the ranking, therefore getting external organisations to sign on to it is vital to strengthen its credibility.

CONCLUSION

In this paper, we have set out to study the question how ranking creators work to create epistemic credibility – that is, how they support their claims of representing the world and make them count as credible knowledge. Instead of assessing the credibility of the rankings ourselves, we have focused on the practices and strategies engaged by the actors to build credibility and convince their audiences. To this end, we have presented findings from two ethnographic case studies of rankings produced by globally-oriented civil society organisations: the Climate Change Performance Index (CCPI) and the Financial Secrecy Index (FSI).

We have shown how the people behind the CCPI and the FSI strive to create epistemic credibility in four ways. (1) By embedding claims in their respective political environments, ranking teams create

a connection to established facts which makes it harder to argue with them. (2) The process of managing non-standardised judgments illustrates how ranking producers account for aspects of the evaluation work that cannot be standardised while still protecting their claims against critique. (3) The reasoning of operationalisation is underpinned by specific narratives about the respective policy area, as they provide the connection between concepts, data, and imagined effect of the ranking. (4) The recruitment of other actors to support the publication of the rankings reinforces the claims of the ranking teams.

As we have stated, this is an explorative study that does not claim to be exhaustive. There are many more ways in which ranking creators construct credibility, as well as other actors and sites that have a part in trials of strength, and could be investigated in future research. Our main goal in this article has been to demonstrate the fruitfulness of applying practice-based and empirically open conceptions of knowledge, like Latour's notion of trials of strength, to rankings. By treating rankings as projects of knowledge production that can be studied using the conceptual tools developed in the sociology of science (Camic et al., 2011), we hope to contribute to a practice-based, empirically driven study of rankings. As Latour points out, the attributes of subjective and objective "are relative to trials of strength in specific settings" (Latour 1987:78). In line with this, we avoid subsuming the rankings under any pre-conceived label such as "quantification" or "neoliberalism", but instead focus on the concrete practices and strategies the ranking creators devise to pass these trials, including – but not limited to – the use of numbers. Moreover, we have suggested one should take the social position of the authors of such claims into account: in the cases we studied, their marginal institutional position as well as the comparatively small number of resources they command, require them to come up with creative strategies to pass trials of strength and create political traction.

Regarding the question of generalisation, we see this as a tentative step towards a practice-based understanding of rankings and their claims to credibility on the basis of two case studies. More systematic and comparative studies of rankings in different fields – like, for instance, higher education or the arts – are needed to gain a more general understanding of the various credibility strategies pursued by ranking creators. This article provides first insights into practices that ranking creators utilise to strengthen the credibility of rankings in trials of strength. Rankings are often characterised by scholars as "technologies of governance" (Merry 2011) or tools of "governing by numbers" (Shore and Wright 2015b). While we agree that researchers as well as journalists and other users of indicators should be alert to the political agendas implicated in them, we argue this does not apply exclusively to rankings, but to any form of social knowledge production which aims to influence public policy and media reporting. At the same time, we suggest to not always make this insight the starting point of empirical analysis of rankings. Instead, they can be studied through the lens of a practice-focused sociology of knowledge that carves out the strategies of strengthening and legitimising claims of credible knowledge. As we have shown, the studied ranking creators invest resources to establish their credibility and expertise, which is an essential building block in their attempt to influence governance.

By unpacking the credibility practices in our empirical cases of ranking creators, we contribute to a deeper understanding of these practices, and this understanding can be extended to other cases and areas of social knowledge production. How do actors who wish to make other actors or audiences buy into their claims equip those claims with credibility, so that they do not simply appear as articulations of opinion or ideology, but as knowledge about the world? In particular, the approach we have taken on rankings could also be applied to all kinds of policy reports and other outputs where various types of actors, like NGOs, interest groups, think tanks, or government agencies try to

intervene in political processes by providing expertise that goes beyond mere opinion. We would therefore argue that our approach of applying the concept of trials of strength to knowledge claims beyond academia is a promising avenue for future research on credibility practices in different forms of social knowledge production.

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ARTICLE

Mobilising Transnational Expertise in National Construction: Exiled Academics and the Transformation of Interwar Turkey

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Abstract

This article examines transnational expertise in state formation, focusing on early Republican Turkey's strategic mobilisation of foreign specialists, particularly European academics fleeing Nazism. These exiled scientists were invited with high aspirations for international expertise, unburdened by foreign influence, and provided with arrangements to act as modernising agents. This study analyses their contributions to Turkey's development, the challenges of their integration, and the political choices that empowered or marginalised scientists. It reveals how efforts to avoid reliance on a single foreign model inadvertently fostered new dependencies. The article highlights the unequal dynamics of North-South scientific circulations and challenges assumptions about the novelty and neutrality of science-driven governance.

Keywords

Expertise; transnational experts; nation-building; interwar Turkey; exiled academics

INTRODUCTION

This article examines the role of transnational expertise in state formation and reform, focusing on the early years of the Republic of Turkey. It explores how international experts, particularly those willing to migrate and settle, were strategically employed across multiple sectors to contribute to Turkey's nation-building and development during the interwar period.

Emerging from a war of independence against Allied occupation and the Sevres Treaty following the Ottoman Empire's defeat in WWI, Turkey secured international recognition with the Treaty of Lausanne in July 1923 and proclaimed the Republic in October. After decades of war and financial dependence, the new state invested its limited resources into a wide-scale reform and reconstruction programme, importing international expertise and inviting transnational professionals. While Turkey's imperial legacies and geopolitical position were specific, its case provides a valuable lens for understanding how transnational expertise shaped 20th-century state-building in contexts marked by asymmetrical global hierarchies.

This study draws on the historical sociology of globalisation, particularly Dezalay's (2004; Dezalay and Garth 2011; Dezalay and Nay 2015) analysis on the global markets of expertise and the institutions and elites that (re)produce hegemonic relations. Like the late Ottoman Empire, Turkey continued to import expertise, creating openings for Western interference through soft power strategies aligned with political and economic interests. The Kemalist leadership sought expertise detached from imperialist agendas and found it in scientists and professionals forced into exile under Nazism. Through the mediation of transnational experts and activists, the Turkish government strategically employed these exiled academics to reshape not only the country's educational infrastructure but the broader state-building and modernisation process.

Exiled academics were invited with high expectations and found in Turkey a rare context where they were empowered as modernising experts, supported institutionally, and entrusted with prominent roles in public service and academia. This contrasted with the downward mobility experienced by many exiles in other contexts,[1] as highlighted in studies on recent waves of academic exile from Turkey to Europe and North America, where exiles were assigned non-agent roles as victims (Sertdemir Özdemir 2021; Vatansever 2020). The reverse migration of high skilled exiles to 1930s Turkey highlights the asymmetries of global circulations and the contextuality of subalternity and agency.

Despite their favourable reception, integrating transnational expertise in nation-building involved controversies and conflicts. The "émigré experts" – as they were called in Turkey – faced difficulties, some common to the exile experience (Tuori 2022) and others specific to the Turkish context. Their professional recognition within Turkey contrasted with diminished visibility in the international scientific community, due to Turkey's peripheral status. They also encountered cultural differences, language barriers, material and infrastructural limitations, the negative consequences of geopolitical tensions, and local resistance. We explore these challenges and the tensions between exiles, local elites, and Nazi experts. We show that despite challenges, these transnational elites received support from Turkish authorities and were able to contribute to Turkey's economic and social development extensively, informing and implementing public policy, and shaping national institutions. They contributed to state-building in diverse ways, from linguistic policies to arts and humanities, shaping a new national identity in dialogue with global modernity.

The study also challenges simplistic notions of knowledge transfer. As Edwards (2020) argues, mainstream diffusion approaches often overlook the agency of peripheral actors and simplify the multidimensional effects of imperialism to claim and explain the global diffusion of Western norms, templates and knowledge either by imitation (as in world polity theory) or economic/political pressure (coercive theory). Drawing on a sociology of expertise (Eyal 2013), this article emphasises the agency of Turkish actors who provided the exiles with material, institutional and individual arrangements within a broad network extending from academia to other public institutions, allowing them to operate as practitioners of legitimate expertise and agents of change.

The study draws on diverse historical sources, including state archives, press articles, expert reports, memoirs and biographical documentation. This multilevel archival approach seeks to capture the institutional mechanisms, personal experiences and national-international entanglements that structured transnational expertise in early Republican Turkey (Dogan 2024). The article first

1 Tuori (2022: 176) draws a parallel between scholars under Nazism and recent academic refugees, both subject to institutionalised downward mobility (cf. Vatansever 2020).

introduces the historical context of employing transnational scientists and professionals in public policy during the transition from empire to nation-state, then focuses on the role of scientific migration in Turkey's transformation.

TRANSNATIONAL EXPERTISE FROM EMPIRE TO NATION-STATE

To navigate a world divided by colonial hierarchies distinguishing "civilised" (Western) nations from the "uncivilised" (or "Oriental")[2] other, Turkey's ruling elites have, since the Ottoman period, developed strategies to enhance the state's global competitiveness. These include sending students abroad, establishing European-style schools, bringing in transnational experts, and other means of translating dominant expertise and models (Dogan 2021). Despite a relatively extensive Turkish academic literature on foreign experts' involvement in public policy,[3] the transformative role of transnational expertise in (re)constructing a sovereign nation-state remains underexplored (Dogan 2022).

State archives and expert reports published by ministries document the activities of *mütehassıs*, *uzman* and *eksper*[4] (terms for "expert" or "specialist") as well as foreign *müşavir* (advisor) and *profesör* (professor, academic). In these historical documents, "expert" denotes not just a title but an institutional position substantiated by a contract, often reserved for Western professionals. These bureaucratic procedures, though not uniformly documented, offer glimpses into the role of transnational elites in translating Western knowledge and models into the Turkish context.

Turkey's Ottoman heritage challenges Eurocentric narratives portraying expert involvement in decision-making and governance as a recent Western phenomenon that diffused to the Global South only through postwar development programmes. The Ottoman governance model, which evolved over six centuries, featured a regulated and structured scholarly body (*ulema)*, similar to their counterparts in the Islamic world and the "scholar-officials" in Chinese history.[5] With responsibilities ranging from education to the judiciary, these scholars were represented in the state governing divans, and influenced decision-making by advising state leaders, proposing reform projects, examining policy implementation, reporting on social conditions, defining problems and proposing solutions. Alongside the *ulema,* Ottoman rulers also used the expertise of local professionals and foreign advisors – often political exiles – in decision-making and policy formulation (Dogan 2021). The Ottoman state, favourable to migration, promoted knowledge and technology transfer by employing migrant professionals as early as the 15th century (Karpat 2002; Shaw 1993: 1-3).

This institutional framework underpinned the increasing recruitment of Western professionals in the Ottoman public service during the 19th century, a period of intensive state reforms. As the polycentric world shifted to a core-periphery order (Buzan and Lawson 2015), the growing

2 See Edward Said's (1979) analysis of Orientalism as a discourse that emerged within European colonial imperialism, simultaneously constructing the "West" and its "Oriental" other through a derogatory essentialisation of the latter.

3 These experts are often analysed by sector, and occasionally by nationality (e.g., Yıldırım 2012).

4 Uzman, mütehassıs, and the less commonly used eksper are interchangeable terms, with roots in Turkish, Arabic, and French respectively (Nişanyan 2002-2024).

5 While comparable to Chinese "scholar-officials" (Li 2018), Ottoman ulema were directly rooted in the Islamic scholarly tradition (Gilliot et al., 2012). Derived from the Arabic plural of âlim (scholar) and ilm (knowledge, science), the ulema comprised the ilmiye, a key institutional pillar of the Ottoman state organisation (Ipsirli 2004).

involvement of Western experts in Ottoman state affairs aligned with global trends that some scholars describe as a new form of imperialism (Buchanan 1986; Mitchell 1988).

The influx of Western elites into Ottoman administrations arose from a dual dynamic: Ottoman rulers sought expertise to acquire techniques and technologies from rival powers threatening their sovereignty and territorial integrity, while transnational elites simultaneously leveraged these opportunities to enhance their international and symbolic capital as experts, promoting hegemonic models. These circulations were backed by Western powers investing in exporting expertise as a soft power strategy alongside aggressive interference policies (Dogan 2021, 2022; see also Dezalay 2004). Germany, for instance, seconded officials to Ottoman administrations to promote its geopolitical and economic interests. This trend, which Ortaylı (1981) have termed "neocolonial", persisted until the empire's fall.

In the Republican era, concerns over foreign interference continued as Turkey sought to maintain sovereignty while embracing Western knowledge and models. To mitigate such risks, the government diversified its sources of expertise, enlisting specialists from small, neutral, or non-hegemonic states that did not threaten Turkish sovereignty.[6] This also meant diversifying policy models. Turkey did not rely solely on experts from Europe and the United States, but also looked to the Soviet Union, which represented an alternative economic and sociopolitical model. After supporting Turkish independence during the independence war and peace negotiations, the USSR provided financial and technical support to Turkey's state-led development and industrialisation, even though the government's anti-communist stance limited a full-fledged alliance (Dogan 2022; Gökay 2012; Tekeli and Ilkin 2009).

The rise of Nazism and Fascism in 1930s Europe offered Turkey a unique opportunity to import international expertise detached from imperialist interests. Nazi persecutions against the non-Nazi intelligentsia created a surplus supply of highly qualified professionals and scientists, which Turkey partially absorbed by inviting specialists across sectors.[7] The following sections explore how the Turkish government strategically employed these exiles to reshape both the country's scientific infrastructure and its broader development policy.

University Reform and the Strategic Role of Education

Among the many sectors transformed by transnational expertise, education stood out as a central pillar of Turkey's reconstruction. This sector warrants attention not only for its extensive integration of transnational expertise but also for its role as a driving force behind national construction serving both ideological and practical purposes: producing and disseminating legitimate knowledge, redefining state-society relations distinct from the Ottoman multicultural legacy, cultivating national identity and a skilled workforce.

In this context, the role of transnational scholars in Ottoman and Republican education has garnered considerable scholarly attention, with recent studies, like Sarmis (2019) on the "transnational construction of the Turkish education system", highlighting continuities between the two eras. Existing scholarship often interprets this transnationalism through the lens of "Westernisation", reflecting asymmetries in circulations and exchanges. Since the late Ottoman period, Turkey has

6 See, for example, why the statistical reform was entrusted to a Belgian expert (Dogan 2023a, 2023b).

7 While other categories of exiles fleeing Nazism came to Turkey during the 1930s (Neumark 1980: 23-7; Shaw 1993), this study only focuses on officially recruited experts and scientific personnel.

been a unidirectional importer of European expertise, which shaped its educational field through competing models, expertise, and languages.

French influence dominated the cultural field from the 18th century onward (Ergün 1990), with French becoming the privileged language of higher education by the 19th century (Ergin 1977). French institutions like the (still active) Lycée de Galatasaray (a joint Ottoman-French project established in 1868) trained elites in French culture and served as educational models (Georgeon 1994; Gürtunca 2017). French dominance was challenged with increasing German influence, as seen in schools like the Alman Lisesi (1868-), which became bastions of German culture (Kılıç 2005; Ortaylı 1981; Widmann 2000). By the end of the century, American colleges such as Roberts College, alongside British, Italian, Russian, and Greek schools, also proliferated. As part of missionary activities, supported by capitulations granting extra-territorial rights and privileges to Western powers, foreign and minority schools reinforced "Westernisation" but also ethnic segregation of the empire (Göçek 1993; Kılıç 2005; Sezer 1999).

Concurrently, Ottoman reformers invested in education as a lever for social change and survival in a threatening geopolitical environment. They financed overseas studies, and invited foreign scholars to establish European-style schools, military academies, and *grandes écoles*.[8] These policies cultivated "state nobility"[9] – a generation of intellectuals, officers, and professionals trained in Western knowledge – who later built the Republic.

The Republican government similarly instrumentalised education for social change and, drawing on European nation-state models,[10] cultivated a skilled workforce and fostered an homogeneous national culture, in line with earlier "turkification" efforts (Ülker 2005), which often included aggressive policies toward minorities. The 1924 Constitution mandated compulsory, free, primary education. The Ministry of Education secularised and centralised all educational institutions, revised curricula, and closed religious schools (*mektep* and *medrese*).[11] Most foreign schools did not survive this centralisation and unification.[12] Following the 1927 census, which revealed an 11 percent literacy rate, the Kemalist reforms defined Turkish as the national language and replaced the Arabic script with Latin in 1928 (Dogan 2022, 2023a).

Education reform drew upon diverse international models, including those of Bulgaria, Czechoslovakia, Denmark, Hungary, Italy, Japan, and Sweden (Ergün 1990), alongside cooperation with the Soviet Union (Aslan 2020). Western experts were invited to assess the education system and advise on policy. Most expert reports were published by the Ministry in 1939, including those by American sociologists John Dewey (1939) and Beryl Parker (1939); the Hines-Kemmerer committee;

8 The French model of grandes écoles was created during 18th century as higher education institutions operating outside the traditional university structure to train elites as future state officials, selecting their students through a highly competitive process.

9 Bourdieu's (2002) concept of the "state nobility" draws an analogy between ancien régime nobility and graduates of the grandes écoles serving as senior civil servants in contemporary France. Similarly, Ottoman policies, modelled on the French education system, established grandes écoles to (re)produce state elites with shared knowledge and values. See Berkes (1998: 59-65, 173-95) and Ergin (1977) on the creation of military academies for engineering and medicine, as well as grandes écoles for civil servants during the 19th century.

10 Turkey's education policies constitute an example for the constructionist theories of nationalism, with scholars like Gellner (1990) and Hobsbawm (1992).

11 After the abolition of the Sultanate (November 1, 1922) and the proclamation of the Republic (October 29, 1923), the Law on the Unification of Education (March 3, 1924) centralised all educational institutions under the Education Ministry.

12 See Ergin (1977: 2078-2091) on measures regarding foreign schools which survived the transition to nation-state.

Weimar German officer Alfred Kühne (1939); Belgian expert Omer Buyse (1939); and Swiss expert Albert Malche. These studies constituted "series-B" in a collection guiding education policy, second only to "series-A", containing guidelines from President Atatürk and Prime Minister Ismet Inönü. The collection also included "series-C" (Ministry of Education expert reports), "series-D" (reports on foreign education systems), and "series-E" (feedback and recommendations from other ministries).[13]

Malche's report on university reform holds particular significance. Malche (1876-1956), a public administrator and external lecturer at the University of Geneva,[14] was invited in 1932 to assess and advise on higher education, especially for reforming Darülfünun, Turkey's oldest university.[15] From January to May, he conducted site observations, multi-actor surveys, and interviews with political leaders, faculty, and students, submitting his report in June, as agreed with the Turkish ambassador in Bern, who had recruited him (Malche 1939).

Although Malche (1939: 1, 3) claimed to conduct his study "objectively and without prejudice" and, as a foreigner, to have no preconceptions about Turkish higher education, his benchmarks were European and US universities, which by then had already become global benchmarks. He noted that Darülfünun operated "under harsher conditions than its counterparts in Europe" and was incapable of training future professors, who were instead educated abroad (Malche 1939: 4, 14-5, 18).

Malche's observations echoed issues highlighted in other experts' reports, such as material deficiencies, outdated teaching methods, low salaries and poor student living conditions. However, he attributed Darülfünun's "underperformance" not only to material and technical shortcomings but also incorrect policy choices.

Darülfünun was an experimental university founded in the late 19th century and reformed in the early 20th century, inspired by European models (İhsanoğlu 1993). Of all the European academics teaching at the university, the German scholars were the most numerous, numbering at least twenty during WWI. Despite their influence on new institutes and curricula, they were unable to replace the dominant French influence entirely or to determine higher education policy (Dölen 2008: 30-33; Widmann 2000: 61-5). Before their departure in 1918, they recommended reorganising the university in line with the German model, with all powers centralised in the Education Ministry, which would govern the university through an intermediary officer, leaving only symbolic authority to the university president and directors. This recommendation was never implemented (Dölen 2008: 36-38). On the contrary, the regulations enacted between 1919 and 1924 secured the

13 This collection of reports was published upon the initiative of Hasan Ali Yücel (1897-1961), the Minister of Education between 1938 and 1946.

14 A graduate in literature, Malche was secretary general of the Department of Public Instruction (DIP) in the canton of Geneva before being appointed as director of primary education. In 1927, he was elected to the Geneva Council of State with responsibility for the DIP, and in 1931 to the Council of States (the upper chamber of the Swiss Federal Assembly), where he remained until 1951. See his biography at the University of Geneva, where he occasionally lectured (UNIGE 2011).

15 Despite not holding university status, Istanbul had military and civil academies, while Ankara housed a Higher Agricultural School (founded in 1930, it became a university-equivalent Institute in 1933), a Law School (founded in 1925) and other higher education institutions, later incorporated into Ankara University upon its official founding in 1946. The Republic's inherited Ottoman academies functioned as faculties and were gradually annexed by universities. The Ottoman medreses, focused on Islamic theology, were all closed by the 1924 law.

university's financial, administrative and academic autonomy,[16] only to be abandoned by Malche's advice (until 1946[17]).

Criticising Darülfünun's autonomy for turning the university into an insular, introverted institution disconnected from society, Malche (1939) urged governmental oversight. The Ministry of Education was to impose an academic programme to integrate the university within the reform agenda and vice versa, and take over recruitment and faculty decisions to mitigate favouritism and cronyism (Malche 1939: 5, 9-10). The rector, as "a faithful translator and representative of the Ministry", should be assisted by a competent senior civil servant, capable of dialoguing with both foreign and local academics (Malche 1939: 8, 23-4). Criticising lectures as outdated, he recommended restructuring curricula and revising teaching methods and assessments to emphasise practical aspects and applications of courses. Noting that students were not "polyglots" and did not master foreign languages, he advised introducing French and German language courses earlier, during secondary school (Malche 1939: 11, 24-30). He further recommended addressing the lack of scientific literature in Turkish, by investing in translations and original works, and reorganising libraries. His recommendations also encompassed defining recruitment and promotion criteria and restructuring the university's administrative organisation according to Western models (Malche 1939: 30-55).

Malche (1939: 21) concluded, as if he were analysing factory productivity, that "Darülfünun is a large establishment with low output"; and therefore it was necessary to "simplify the machine, condense its operation and provide the best means to the machine operators to avoid any energy loss". He recommended reviewing and reducing the number of faculty members who were overly numerous and lectured only as a secondary occupation due to low academic salaries. Finally, he posited that the university's role was "to disseminate a scientific spirit" urging that it become "an instrument of civilisation", critical to "the intellectual, moral and even social future of Turkey" (Malche 1939: 58).

The Turkish government adopted Malche's recommendations but went even further with reform. In 1933, it replaced Darülfünun with a new institution, appointing Malche to participate more directly in the higher education reform through a new contract.[18] As an expert adviser to the Ministry of Education, vested with extensive authority, Malche served as the principal mediator in the recruitment of exiled academics from Nazi Germany into Turkish civil service and academia, supporting the establishment of the new University of Istanbul and other higher education institutions in the new capital, Ankara.

EMPLOYING EXILED ACADEMICS AS NON-HEGEMONIC EXPERTISE

The Nazi regime's 1933 "Law for the Restoration of the Professional Civil Service" forced many Jewish and politically dissident scholars out of their positions. Despite the efforts by victims and humanitarian organisations, these persecutions were not recognised as an international problem, unlike the refugee waves they triggered, as highlighted by the resignation letter of the American diplomat James G. McDonald, who served as the League of Nations' "High Commissioner for

16 Following the Allied occupation towards the end of WWI, Darülfünun nearly ceased operations. Occupied by British forces, some faculty were arrested and exiled, while most students joined the armed liberation struggle. After the conflict, the 1922 regulation defined Darülfünun as an autonomous public institution, and ended the separation of male and female students (Dölen 2008: 28-9; 41-2).

17 One of the émigré experts, Hirsch (2012) played an important role in the postwar policy re-attributing autonomy to the university.

18 This new contract was signed in 1933 for an additional year. BCA 30-18-1-2_34-16-7 (13.03.1933)

Refugees from Germany". Submitted in late 1935 as an alarming appeal to the international community, his letter warned of "the pauperisation or exile of hundreds of thousands", deprived of "the means of livelihood":

> Government officials, doctors, lawyers, educators, renowned artists and celebrated scientists… The astronomer, the mathematician, the engineer, the chemist, the physicist, the musician, the painter, have not been spared (McDonald 1935: v, 10-11).

Turkey absorbed a segment of this displaced elite. Between 1933 and WWII, dozens of exiled academics – mainly from Germany and Austria – were integrated into Turkish institutions, particularly Istanbul University and new faculties in Ankara. These immigrants brought cutting-edge research, expertise, and know-how, leaving an indelible mark not only on Turkish education reform but also the broader reconstruction of the nascent state. Their contributions are well documented in a rich literature, drawing on Widmann's (2000) doctoral thesis on Turkey's university reform, the detailed research of Reisman (2006) (himself exiled in Soviet Russia), as well as autobiographies and memoirs by exiles like Fritz Neumark (1982), Ernst Hirsch (2012 [1997]), and Philipp Schwartz (2003).

Schwartz (1894-1977), a Hungarian neuropathologist expelled from the University of Frankfurt, had organised in Switzerland an emergency association "Notgemeinschaft" for German scholars in exile.[19] Through Albert Malche's mediation, Schwartz was invited to Ankara, where, in July 1933, he met the Foreign Minister, the Minister of Education and ministry officials. Schwartz and Malche responded to their specific demands, proposing candidates for Turkey's academic vacancies. The resulting five-year contract for thirty-three scholars served as a template for later hires. Schwartz and Malche continued to connect German-speaking academics with opportunities in Turkey.[20] Schwartz returned to Turkey in August 1933, accompanied by the surgeon Prof. Rudolph Nissen, to help organise the new Istanbul University (Schwartz 2003; Widmann 2000: 94-5).

Reisman (2006: 9) estimates that contracts eventually covered 300 academics and 50 technicians, with over a thousand exiles including assistants and accompanying families. He attributes this influx to restrictive immigration policies and anti-Semitism in university and hospital recruitments in Western countries.[21] Most exiles were German and Austrian Jews. A smaller portion comprised anti-Nazi opponents from diverse backgrounds, including liberals, socialists, communists, Catholics or protestants, as well as several academics from Italy and occupied countries like Czechoslovakia and Poland. A few had prior professional experience in Turkey as experts or advisors on the eve of or during WWI.[22]

Neumark (1982) notes that the Turkish government sought to recruit scientists based on Western criteria – primarily academic qualifications – while relying mostly on emigrants' recommendations. Professional and scholarly networks hence played a decisive role in recruitments. Though most hires were men, women also found work in Turkey, albeit mostly among the "little people", technicians,

19 Notgemeinschaft deutscher Wissenschaftler im Ausland (Emergency Association for Exiled German Scholars).

20 Malche, authorised by the Turkish government to negotiate contracts abroad (particularly in Switzerland), facilitated recruitments. The Turkish Ministry of Education predetermined the general contract framework with Malche, Schwartz, and later Nissen. See Neumark's (1982: 10) observations.

21 Reisman (2006: xxiii) testifies this risk himself and through his acquaintances, noting, for example, his shock on reading Albert Einstein's letter in 1936, stating that "he was told explicitly that they did not want to hire Jews at Princeton".

22 For examples, see Neumark (1982: 9).

assistants, nurses and other staff accompanying the "big names" (Erichsen 2005). In a departure from Ottoman tradition, the Republican government had begun to recruit female experts, whose numbers increased with the exiles, although they remained highly disproportionate to male experts in the Turkish civil service. Aligned with the feminist gains in Turkey, such as equal rights and full suffrage (1924-1934), the hiring of women experts in a variety of fields (humanities, education, architecture, medicine, chemistry, etc.) made Turkey's proportion of women academics competitive with that of Western institutions at the time.[23]

Among women experts who acquired international recognition was Hilda Geiringer, a pioneering mathematician in genetics, who joined Istanbul University in 1934 as a professor. She was one member of a distinguished group of mathematicians, which included Richard von Mises and Hans Reichenbach from the "Berlin Circle" and the "Vienna Circle" (Eden and Irzik 2012). This group emigrated from Turkey to the USA in the late 1930s, but Geiringer struggled to secure a satisfactory position due to sexism in university recruitments (Erichsen 2005: 341-2).

Gender specific challenges limiting women exiles' access to employment, with reported discrimination from other host countries (Tuori 2022) were also observed in Turkey, where institutions displayed a similar "patriarchal structure" (Erichsen 2005). The case of Erna Eckstein, who unlike her husband did not receive a work permit in Turkey despite her expertise as a paediatrician, exemplifies gender inequalities in this exile context. Maksudyan (2024a) examines Erna Eckstein's informal contributions to Turkey's development, particularly regarding children's health and welfare through volunteer work and contributions to her husband Albert Eckstein's activities, while raising their three children.

Like Erna Eckstein, women who accompanied their husbands into exile in Turkey played a central role in forging a community of German-speaking, mostly Jewish, emigrants in Istanbul and Ankara. This community sustained their solidarity through regular social and sporting activities, clubs, and excursions, creating a supportive network that likely eased their integration into Turkish society despite the significant challenges.[24]

CHALLENGES OF INTEGRATING FOREIGN EXPERTISE INTO TURKISH PUBLIC SERVICE

The integration of immigrant experts into Turkish public service faced multifaceted challenges stemming from material constraints, cultural differences, and geopolitical tensions. These issues were compounded by the gap between reform ambitions and the realities of implementing such wide-scale transformations.

The reform began with high-profile consultations. Nobel laureate physicists James Franck and Max Born, along with mathematician Richard Courant – former heads of Göttingen University's institutes – joined Philipp Schwartz in assessing the new Istanbul University.[25] All three were affected by the

23 According to Erichsen (2005: 339), the proportion of women academics and professionals hired by the Turkish government mirrored the situation in Germany. Of the estimated 1000 exiles who emigrated to Turkey, including academics, specialists and support staff and their families, approximately 300 were women, primarily working in chemistry, biology and various medical fields (Erichsen 2005: 339-40).

24 See also Hirsch's (2012) and Neumark's (1982) accounts of the exile community in Istanbul.

25 For more background information, see Georgiadou (2004) and Eden & Irzik (2012).

Nazi Civil Service Law. Born, actively engaged in the Notgemeinschaft, connected Schwartz with Courant and Franck in Zurich.

Upon receiving the offer relayed by Schwartz to lead Istanbul University's new mathematics institute, Courant sought advice from Constantin Carathéodory, a Greek mathematician whose father had served as an Ottoman diplomat. In his July 1933 letter, Courant inquired about the salary (500 Turkish lire/month), living conditions, and the feasibility of the reform project, expressing his preference to conduct negotiations "in accordance with the German authorities and [...] Germany's interests".[26] His visit to Istanbul a few months later confirmed his decision to decline the offer.

At the end of their visit, Courant and Franck reported their observations in October 1933 to the Rockefeller Foundation, which followed the process closely. Their report emphasised the Turkish government's aspiration "to create a promising scientific centre in Istanbul, which should contribute to the development of higher education in Turkey". However, it also highlighted the "miserable" condition "of the so-called university" deeming it inadequate to emulate European standards in the near future. The report noted significant hurdles, including language barriers, insufficient institutes, equipment, apparatus, and library resources, which would demand considerable organisational efforts from the professors involved. It observed that students and assistants were "not yet sufficiently trained for a true cultivation of the sciences". Courant and Franck added their "impression that out of a certain ignorance the Turks underestimate these problems and are of the opinion that merely adding a line-up of recognised scholars, would be sufficient to start up a real university".[27]

During this period, Schwartz (2003) also contacted the Rockefeller Foundation's European office seeking financial assistance for the integration of exiles in Turkey. However, the Foundation's annual reports do not indicate any funding other than a limited contribution to the Ankara Central Institute of Hygiene, which had been granted previously in 1929 for four years.[28] The exiles' transnational scholarly network helped reinforce ties with this influential American foundation, which was engaged in extending and consolidating US hegemony globally (see Parmar 2014). Its philanthropic activities, especially in health and education, grew over the coming years, becoming a major stronghold of US intellectual and political influence in Turkey (Erken 2020).

While Turkey later received Western military and economic aid during and after WWII, external funding during the early Republican period was minimal, with the exception of Soviet development aid for Turkey's first industrial plan in the early 1930s (Dogan 2022; Ilkin and Tekeli 2009). Allocating resources to scientific institutions required substantial sacrifice in the context of financial difficulties exacerbated by the Great Depression and sovereign debt obligations.

The material deficiencies of Turkish institutions and infrastructural problems were a recurring issue highlighted in expert reports and exiled academics' memoirs. These scholars faced the daunting task of building facilities, libraries, and laboratories from scratch, training local staff and developing

26 According to Georgiadou (2004: 283-4), Courant either refused to recognise the dramatic political change, believing that the Nazis would not remain in power for long, or perhaps was only trying to avoid the tax imposed on those fleeing Germany (Reichsfluchtsteuer), by attempting to acquire official authorisation. See a copy of Courant's letter to Carathéodory in Georgiadou (2004: 282-3).

27 This report (October 25, 1933) is quoted and analysed by Reisman (2007a: 268-70).

28 The Foundation's annual reports from 1929 to 1935, along with Erken (2020: 20-3) and Erdem and Rose (2000), detail the Rockefeller Foundation's interest and activity in Turkey, which almost exclusively focused on public health before 1940.

teaching materials.[29] Their achievements ranged from simple arrangements for importing basic equipment to establishing Turkey's first observatories, zoos, and botanical gardens. Yet the lack of resources, including qualified personnel, remained a challenge.

Liselotte Dieckmann (1964: 124), who taught foreign languages at Istanbul University (1935-1937), described "tragicomic stories" about the lack of infrastructure and qualified support staff. Similarly, Erich Auerbach, Chair of Western Languages and Literatures (1936-1947), noted in a personal letter:

> We have certainly achieved a lot at the university, but not nearly as much as would have been possible; the uncertain and often amateurish policies of the administration make work very difficult, although it must be admitted that it is not easy for them; I have learned here how difficult it is to Europeanise a non-European country in a short time.[30]

This Eurocentric framing aligned with their assigned role: identifying and bridging the gaps between Turkey and Europe. The challenges had a distinct anthropological dimension as Auerbach wrote to his close friend Walter Benjamin:

> The enormous amount of difficulties, trouble, obstructionism and mismanagement on the part of the local authorities and from the local circumstances, which is driving some colleagues to despair, is not unpleasant for me, because it is far more interesting as an object of observation than the possible goal of my work.[31]

Erika Bruck, a young doctor who later became a paediatrics professor in New York, likewise found her Turkish experience deepened her understanding of public health and medical practice despite resource constraints:

> I learned […] how to achieve goals […] without using tools that are considered indispensable in Central Europe. Through contact with Turkish students, patients and – during trips to Anatolia […] – ordinary Turks in small towns and villages, I learned a lot about the influence of school education, public hygiene, the environment, etc. on the health of individuals and the population as a whole (cited by Erichsen 2005: 346).

Contracts (typically 3-5 years) required scholars to make substantial investments in their roles, including developing curricula and textbooks, preparing expert reports upon request, and learning Turkish. Until proficiency, they taught with translators, often Turkish assistants assigned to work alongside and learn from them. This clause was a new policy agreed upon with Notgemeinschaft representatives and Malche, whose 1932 report (1939) highlighted that, apart from a minority of

29 The paediatrician Erika Bruck exemplifies these efforts: "we built a pretty good laboratory from nothing. We made urease for determining urea in blood ourselves, if I remember correctly, from soybeans. Among other things, there were no trained laboratory technicians in Turkey other than the Germans, no distilled water, which we had to produce ourselves for years, no apparatus or glassware" (cited by Erichsen 2005: 345).

30 Letter of 22.6.[19]46 to Dr. Martin Hellweg. We Refugees. Erich Auerbach: "Everything is provisional". https://en.we-refugees-archive.org/archive/erich-auerbach-everything-is-provisional/

31 Erich Auerbach (1892-1957) is considered the founder of the discipline of comparative literature. See a copy of the letter in online archives of We Refugees. Erich Auerbach to Walter Benjamin: Istanbul – Paris 1937. https://en.we-refugees-archive.org/archive/erich-auerbach-to-walter-benjamin-istanbul-paris-1937/

students trained in European languages in elite secondary schools, the overwhelming majority of students did not understand European languages.[32]

As in other host countries (Tuori 2022), the language barrier posed the greatest difficulty for integration, and was a motivating factor for returning home after the war, as reported by immigrant scholars (Hirsch 2012; Neumark 1982). Some, like Hirsch (2012: 216-7), were convinced of the necessity to learn and teach in Turkish without translators. Hirsch noted that Turkish translators (scholars, assistants and students) struggled with technical and discipline-specific terminology, a challenge compounded by the evolving terminology. Initiated just months before the negotiations with Schwartz and Malche in 1933, the linguistic reform had destabilised Turkish grammar and vocabulary. Hirsch (2012: 218-9) complained that Ottoman terms learned by foreign academics between 1933 and 1936 were soon replaced – a result of both linguistic reform and broader state transformation. For instance, social scientists and jurists like Hirsch (2012: 218) studied Islamic legal terminology, only to find it obsolete after the legal reforms and secularisation.

While contributing to Turkey's national construction, their ambiguous status as foreigners in local institutions presented challenges for Turkish authorities. The government grappled with balancing foreign and national staff, avoiding potential interference, and managing the financial burden of employing esteemed scientists amid severely constrained budgetary resources. These difficulties are partly reflected in Prime Minister Ismet Pasha's reply to Albert Einstein's appeal in September 1933. Einstein, writing "in his capacity as Honorary President of the Union des Sociétés OSE", a Paris-based humanitarian organisation, requested Ismet Pasha "to allow forty professors and doctors from Germany to continue their scientific and medical work in Turkey", offering to work without pay for a year.[33] The plea, delivered by Samy Gunzberg, a Jewish dentist in Istanbul, included a Turkish translation and an explanatory note, highlighting the credentials of these professors from elite German universities and young doctors trained by leading experts. Gunzberg emphasised that while he had recommended they submit an official application to the Turkish embassies, the request came from "this community", to forward the letter of "the famous Professor Alber Aynişitayin" (Einstein) from the "Jewish Health Society known as OSE".[34]

Handwritten notes in Turkish on Einstein's letter indicates that the Ministry of Education deemed the proposal "incompatible with regulations", particularly regarding unpaid work, and "the prevailing conditions". Ismet Pasha's response explained that his government had already contracted over forty professors and doctors with similar qualities and "political situation", working "in accordance with existing laws and regulations". He declined Einstein's request, citing concerns about maintaining internal balances: "We are currently working to ensure the smooth running of the delicate mechanism constituted by an organism containing members of very different origins, cultures and languages".[35]

Despite this refusal, Turkey continued recruiting exiled scientists and professionals in public service. This policy, in its early stages during the correspondence with Einstein, expanded considerably in the following years. The government's preference for structured collaborations (e.g., with

32 Archive files indicate that neither foreign recruitments during the Ottoman era (including German professors hired at Darülfünun during WWI) nor those of the early Republican period before the massive recruitment of exiles from 1933 onward imposed such an obligation.

33 The original letter is in English. BCA 30-10-0-0_116-810-3 (14.11.1933).

34 The original note is in Turkish. Loc. cit.

35 Original letter is in French. Loc. cit.

Notgemeinschaft) over ad hoc humanitarian appeals was not only because the former responded to their expectations and requirements. It is also possible that the government refrained from engaging with a transnational organisation affiliated with a local ethno-religious minority community, viewing any form of international humanitarianism as potentially imperialist (Liebisch-Gümüş 2020) due to earlier experiences of foreign interference through local Christian communities in the Ottoman era (Rodogno 2012).

With its history of state violence against minorities and turkification policies adapted to the ethno-national state model prevalent in the global community (Liebisch-Gümüş 2020), 1930s Turkey was far from an inclusive society. Exiles fleeing Nazism faced a precarious situation, worsened by rising global xenophobia and antisemitism, especially for German-speaking immigrants as international tensions escalated toward WWII (Reisman 2006: 259-291; Tomenendal et al., 2010). While Turkey struggled to maintain neutrality amid geopolitical threats, its "small state diplomacy" (Weisband 1973) – involving compromises with Nazi Germany – affected exile's living conditions. Germany actively pursued propaganda activities in Turkey, especially in cultural and educational fields (Glasneck 1966), and monitored émigrés,[36] even revoking their citizenship. Contrary to its expectations, Turkey did not expel denationalised individuals, even though statelessness created administrative hurdles. In fact, there are no known cases of involuntary return from Turkey to Nazi territories (Erichsen 2005: 343). When Turkey joined the Allies late in WWII, German citizens faced internment in Anatolian cities for months, though most émigré scholars were exempted.[37]

For many exiles, Turkey was a temporary refuge. Its peripheral position in global fields limited scholars' international visibility and recognition, symbolic resources that were essential for establishing scholarly identities. For example, Hilda Geiringer's innovative work went unrecognised in the 1930s, according to Reisman (2006: 216), mainly because she conducted her research and published in Turkey. Reisman notes that such symbolic barriers drove top scientists to leave for US institutions when possible.

In conclusion, Turkey's integration of transnational expertise was marked by a mix of achievements and challenges. The contributions of these elites to science and development were substantial. However, the difficulties they encountered stemming from language barriers, resource constraints, geopolitical tensions, symbolic dynamics and local resistance – as we explore in the next section – reflected broader tensions between official ambitions and practical realities.

DISCONTENT AMONG TURKISH SCHOLARS AND ANTI-REFORMIST TENDENCIES

The position of exiles as a sizable minority supported by government policies was both strong and fragile. Managing diversity and multiculturalism – characteristic features of the previous Ottoman state – posed challenges for the nationalist regime as potential sources of inner conflict and instability. Balancing foreign and national staff in public institutions became a major concern as migrant scholars grew in number and faced resistance from locals.

36 The German embassy collected information on émigrés in Turkey, even directly contacting them with a questionnaire that inquired about their non-Aryan origins or relatives. A copy of this questionnaire is archived online at We Refugees. "Questionnaire from the German Consulate General in Istanbul to Fritz Neumark, May 1938" https://en.we-refugees-archive.org/archive/questionnaire-from-the-german-consulate-general-in-istanbul-to-fritz-neumark-may-1938/#.

37 For an analysis on the internment experience of a scientist in exile, indologist Walter Ruben, see Maksudyan (2024b).

The situation at Istanbul University was emblematic. From 1933 to 1934, foreign academics and technicians, mostly German immigrants, comprised over a quarter of the staff.[38] During the following nine years, German-speaking émigrés filled 80 percent of faculty chairs (Erichsen 2005: 340). Despite their contributions to the development of the new university and faculties, they reportedly encountered resistance, and in some cases, open hostility from Turkish academics.[39]

This resistance stemmed partly from fears of replacement. In fact, this is exactly what happened when the Law No. 2252 (31 May 1933) abolished Darülfünun and established Istanbul University overnight on the same campus, dismissing nearly two-thirds of the staff (Dölen 2010; İhsanoğlu 1993).

While dismissed scholars protested to the authorities and in the press, state authorities publicly defended the reform. Press articles like "We will Occidentalize also in science" ("İlimde de garplılaşacağız" 1933) quoted Education Minister Reşit Galip, framing the new university as a rupture with Darülfünun's alleged indifference to the ongoing "revolutionary" policies transforming Turkey. The reform committee also briefed the press on the criteria for faculty selection, based on academic background, experience, age, and potential for future contributions ("Universite kadrosu" 1933; "Üniversite ıslahat heyetine hücumlar" 1933.) The committee, led by Malche, consisted of expert panels for each faculty that decided who would remain or leave, favouring younger scholars educated in Western universities with active, internationally recognised publications.[40] Over 100 foreign professors were contracted within just two months, to staff the university for the 1933–1934 academic year.[41] Both Ankara and Istanbul institutions continued recruiting foreign academics in subsequent years.

For Turkish academics who retained their positions, the changes were dramatic. German-speaking scholars dominated not only in numbers, but also in positions of power. Many were granted the exceptional status of "Ordinarius", as a highest possible rank. Occupying institute, faculty and department chairs, émigrés exerted extensive authority over administrative decisions, including hiring, promotions and awarding degrees.

These power asymmetries were also reflected in remuneration disparities. Reisman (2006: 522, 2007a: 261-3) notes that an Ordinarius earned 600 Turkish liras per month – 100 liras more than the Prime Minister's salary. Neumark (1982: 11) emphasises that Turkish colleagues typically earned

38 According to Widmann's (2000: 107-9, 118) data, Istanbul University already had 42 (23 percent) foreigners alongside 138 Turkish academics in 1933. Including assistants and technicians, the total staff in 1933-1934 was approximately 323, with 85 (26 percent) being foreign. There were more exiled academics in medicine and science, and fewer in law and literature. Besides Germans, British, French, and other European (Hungarian, Swiss, etc.) professors also taught at Turkish higher education institutions (Dölen 2010).

39 Hirsch (2012: 212-4) and Neumark (1982: 9), among others, emphasise this resistance at first hand. See also Reisman (2007b) who reveals testimonies in exiles' correspondences.

40 Malche administered a questionnaire to gather information on Darülfünun scholars' qualifications, publications and foreign languages. Following interviews, he selected three scholars for each faculty expert committee and collaborated with them to determine the academic staff ("Universite kadrosu" 1933). For more information, see Dölen (2010).

41 The expert committee member highlighted in his press declaration that this was a very short time period to enrol foreign scholars, sharing an anecdote by Malche who apparently struggled for many years just to invite one scholar from Suede when he was working for the Swiss Education Ministry. He explained also that the conditions required for these recruitments were rather difficult as besides scientific excellence they sought for the capacity of "scientific adaptation" especially the obligation of learning Turkish just in three years ("Universite kadrosu" 1933).

less than half, or even a quarter, of émigré salaries.[42] Archives reveal even starker gaps: foreign academics could receive around 1000 liras, approximately 12 times more than a Turkish professor.[43] These disparities mirrored Ottoman-era pay gaps between European experts and locals.[44]

Such inequalities were glaring, especially given Turkey's financial difficulties and substantial debt, which initially raised doubts among invited experts regarding the government's capacity to honour its promises (Neumark 1982: 11-3). The privileges granted to foreign staff – including free housing, tax exemptions, and personal assistants – further fuelled discontent among local scholars. This resentment – or "jealousy", as émigrés described it (Hirsch 2012: 213; Neumark 1982: 12) – likely contributed to stigma and discrimination. Hirsch (2012: 212-4) notes the émigrés' own role in this dynamic: many displayed cultural superiority, constantly comparing local culture and conditions unfavourably to their homeland while maintaining parallel lifestyles and resisting adaptation. Their "individualist" approach and "arrogant", even "contemptuous" attitudes hindered both integration into Turkish society and solidarity within the immigrant community, which might otherwise have led to collective action or resistance against their Turkish colleagues. Instead, most viewed themselves as a "special" Western elite (Hirsch 2012: 213). This perception of superiority was reinforced by the recognition they received from the Turkish authorities as agents of European-oriented modernisation.

Turkish scholars expressed their discontent in various ways, including anti-reformist tendencies (Neumark 1982; Hirsch 2012). The hostile environment drove some émigrés to leave, like Herbert and Liselotte Dieckmann, who departed for the USA in 1938 after teaching languages at Istanbul University for three years. Liselotte explained this with bitterness:

> Atatürk had indeed consulted his government when he invited the German academics, but not his Turkish professors. The foreigners were simply placed before them one day and they reacted accordingly [...] Before the war, there were probably never any arrests or punishments among the foreigners – only the Turkish professors were ruthlessly dismissed or disciplined in other ways. But there was an atmosphere of mutual mistrust that made any cooperation almost impossible [...] And so, one by one, we left a country that had so much beauty and interest to offer, but that could not absorb us. Most of us went to America before the war, where, adapting after the Turkish experience was child's play (Dieckmann 1964: 125-6).

The ideological underpinnings of this resistance were complex. Not all intellectuals were in favour of all the far-reaching reforms that were transforming Turkey at an unprecedented pace, but even those who supported this social change were not necessarily convinced of the central role assigned to

42 Neumark notes that their untaxed wages varied between 500 and 600 lire and had a fairly high purchasing power.

43 According to the 1933 budget of Istanbul University, Turkish professors earned around 80-90 liras and assistant professors 50-60 liras. BCA 30-10-0-0_141-12-1 (16.11.1933). The salaries of "ecnebi" (foreign) Ordinarius are left blank in this table. However, according to other archival documents and the press ("Universite kadrosu" 1933), the salaries offered to foreign academics ranged from 500 liras for regulars to 800 and 1000 liras for distinguished professors such as Nobel laureates.

44 See Dölen (2008: 33-4) on the contracts of German scholars who worked at Darülfünun between 1915 and 1918. Their monthly salary, which was, in most cases, either 750 or 1000 liras, was multiple times higher than the salary of local professors.

foreign experts in the process. As Auerbach summarised in a letter, they were distrusted as foreigners.[45]

At the same time, however, Widmann (2000: 109-13) highlights the considerable support for the reforms and migrant experts among Turkish academics, students, and the general public, particularly due to German doctors' visible impact in university hospitals. The government communicated regularly with them, taking their advice on measures to facilitate their work, whether through new legislation or even the replacement of the university rector and administrators. The new rector met regularly with professors and informed the Prime Minister of the positive feedback from foreign academics on the university reform, quoting the German physicist Friedrich Dessauer's remark that it had "achieved in one year what Western universities could do in ten years".[46]

Neumark (1982: 11-2) notes that the government treated foreign experts' living and working conditions as a matter of "national prestige". Despite the numerous challenges of exile, these transnational actors integrated as a protected minority elite, endowed with financial, social and political privileges. This support fostered what Reisman (2006: 258-9) describes as an "esprit de corps" commitment to Turkey's modernisation.

The cooperation of governmental and other local actors, including colleagues, students and patients, hence proved as crucial as institutional arrangements in empowering them as modernising experts. Their extensive autonomy and authority over hiring, promotion, curricula, research practices and processes of expertise allowed them to act as agents of change, restructuring science, education and research, and shaping state reforms and public policies in their respective fields.

Although the exiles became the absolute majority, not all foreign experts in Turkish civil service were exiles. In addition to the French, Swiss and other nationalities, there were also Germans on secondment or temporary assignment from the Nazi government in Turkish public administrations and educational institutions. We address the challenges posed by Nazi cultural diplomacy in the following section.

CULTURAL DIPLOMACY IN A CONTEXT OF RISING INTERNATIONAL TENSIONS

Germany's influence on the late Ottoman Empire, solidified through the deployment of German officials in its military and civil administration (Ortaylı 1981), had lasting effects, even after defeat in WWI and Turkey's subsequent move under Mustafa Kemal towards greater independence from foreign powers. This persistence was underpinned by pro-German factions in the Republic's bureaucracy and army, including state elites trained in German institutions, as well as Germany's continued investment in maintaining influence. Nazi Germany particularly focused its propaganda efforts on cultural and educational fields, as evidenced by a 1935 report from the Nazi Ministry of Education, praising German experts in Turkey for advancing Germany's intellectual and economic influence (Glasneck 1966).

45 Letter of May 27, 1938, to Johannes Oeschger, We Refugees. "Erich Auerbach on the situation for exiles in Turkey". https://en.we-refugees-archive.org/archive/erich-auerbach-on-the-situation-for-exiles-in-turkey/

46 See Cemil Bilsel's report of 1935, a year after he took office as the new rector of Istanbul University. BCA 30-10-0-0_142-13- 6 (01.06.1935).

The most revealing insight into German cultural imperialism comes from Herbert Scurla's comprehensive report to the "Reich" Ministry of Science, Education, and Culture.[47] Completed after his mission to Istanbul and Ankara in May 1939, following his first investigation in 1937, this document clearly demonstrates how cultural diplomacy served Nazi ambitions and interests. In continuity with earlier German foreign policy, the Nazi government actively worked to secure expert and academic positions in Turkish public institutions for seconded German civil servants.

A key German stronghold was the Ankara Higher Institute of Agriculture (YZE), established in 1930 on a German-inspired scientistic model,[48] with the assistance of a German expert committee invited by the Turkish government in 1928. Its original faculty included four German professors and one Luxembourgian, all working under the rectorate of the German economist and agronomist Prof. Falke. Between 1933 and 1938, approximately twenty-five additional German scholars – including at least four exiles – joined to develop and lead new departments.[49] During this period, the Institute operated predominantly in German, with Turkish assistants (many of whom had been trained in Germany) providing translations. German academics also held key positions in other Ankara institutions, particularly the Faculty of Languages, History and Geography.

However, this German dominance began to decline in 1938 when the Nazi government recalled Falke, followed by other German faculty departures during the war years. By 1942, the YZE had no German professors except for a few exiles, and leadership roles passed to a new generation of Turkish scholars, who had trained under the German faculty (Widmann 2000: 66-9).

Scurla's 1939 report reveals his frustration at the decline of Nazi influence since his first visit in 1937, exposing the tensions underlying this reversal. While German diplomacy viewed the Ankara Institute as a stronghold of extra-territorial influence managed by German academics on secondment, Scurla's report (2009 [1939]) documented growing resistance from Turkish authorities. As an agent of Nazi cultural imperialism, Scurla particularly resented measures reducing the dominance of German language at the Institute.[50] He emphasised the strategies of the Turkish Ministry of Agriculture to take control of the Institute, to reduce and limit the role of Germans and to balance it by bringing in "Turkish elements who had been kept away from it, thanks to the energetic efforts of Professor Gleisberg when he was rector" (Scurla 2009 [1939]: 42). Under the pretext of financial difficulties, the Ministry refused to hire new German academics or renew existing contracts. It rejected the "diplomatic channel for appointments", and restricted faculty communications and relations with the German embassy. Furthermore, despite German efforts, "the school remain[ed] outside the scope of decisions on agricultural policy", being unable to influence policy makers (Scurla 2009 [1939]: 43-6).

47 Halm and Şen (2009) published a "revised and corrected" version of this report translated into French in their book, which includes a second part prepared by Hoss (2009) on the biographies of the exiles cited by Scurla as well as other historical documents (letters, reports, photographs, etc.). The following citations are my own translation from French.

48 According to the official institutional history, Ankara Higher Agriculture College, established in 1930, "adopted the German education system" and was transformed into Higher Agriculture Institute (Yüksek Ziraat Enstitüsü) in October 1933, with faculties of Agriculture, Forestry, Veterinary, Natural Sciences and Arts. It aimed "to modernise the Turkish agriculture, scientifically define its problems and find solutions, educate Higher Agricultural Engineers to serve Turkish agricultural sector, and for research and education purposes in the field of agriculture". Ankara University Faculty of Agriculture. "History". http://www.agri.ankara.edu.tr/en/about-us/

49 See the lists provided by Widmann (2000: 67-9).

50 Turkish authorities had recently abolished the requirement to write the final dissertation in German (Scurla 2009 [1939]: 49).

This resistance had emerged as a "revenge of the context" (Olivier de Sardan 2021). Scurla (2009 [1939]: 43-4) attributed this backlash to the design of the Institute itself, which was "copied from the German model", and "neither its internal structure nor the objectives it had set itself corresponded to the concrete needs of the country". Reporting that parliamentary critics had noted the school's failure to improve Turkish agriculture, Scurla ironically criticised the lack of expertise of officials recruited on the recommendation of German authorities. In particular, he criticised Falke, who was serving as an advisor to the Ministry of Agriculture, for his ignorance of Anatolian agricultural conditions, geological and demographic factors, and climate, his mechanical transplantation ("too much inspiration") of German models in organising the university and poor hiring decisions ("unfortunate choices") for German faculty.

Scurla (2009 [1939]: 44) also blamed the "damaging influence" and "activity of emigrants appointed as experts to the Ministry of Agriculture and the Ministry of Economy" for amplifying local resistance. His report disparaged these émigrés, and advocated their replacement by Nazi state scientists to serve the political and economic interests of the "Third Reich".

Part of Scurla's report was devoted to his proposals for filling academic vacancies in Ankara. Of particular importance among these strategic appointments was the technology chair, given Germany's interest "in the exchange of high added-value products value with Turkey". Scurla (2009 [1939]: 54) noted with concern that American and Belgian specialists dominated textile research in Turkey, arguing that, "sending a German fibre specialist would also be important for our economic policy", mirroring "similar efforts in Yugoslavia and Romania". While the Minister of Agriculture had nominated Dr Baade, an émigré expert of the ministry, his "appointment was successfully rejected by the university". Instead, Scurla recommended Dr Wilbrandt, who, unknown to the Nazi ministries, had been an expert in the Turkish Ministry of Economy since 1934, and "according to information from the Secret State Police, there [was] nothing against him, either politically or criminally". Although Wilbrandt remained "rather indifferent to the German cause in Turkey" and "isolated [...] from the German colony", Scurla (2009 [1939]: 50) considered him ideal for the position due to his familiarity with "the Turkish situation".

Biographical research reveals Hans Wilbrandt as a political exile who fled to Turkey in 1934 (Hoss 2009: 207-8). He was active in refugee aid organisations, chairing the Istanbul branch of the Nazi Victims Aid Committee, established by the American IRRC trade unions. Wilbrandt refused the German government's order to join the army in 1944. He was briefly interned with his family after Turkey joined the Allies towards the end of WWII and, once released, resumed his commercial activities in Istanbul before returning to Germany in 1952 to work on development aid at the Kiel Institute.

German surveillance by the embassy, the secret service, commissioned officers like Scurla and seconded experts, was extensive but imperfect. Scurla perhaps also misjudged Christiansen-Weniger, an agricultural expert working for Turkey from 1923. Scurla, portrayed him as ""the most influential German expert at the Ministry" who had "proved himself in the field of cultural policy", having "consistently shown a loyal attitude to the Reich"; and whose "children attend the embassy school and are members of the Hitler Youth". His biography by Hoss (2009: 133-4) claims, however, that his children were associated with this Nazi group against his will, and that "all the members of 'German Colony B', as the exiles in Ankara jokingly called themselves, knew that he was hostile to the Third Reich". Christiansen-Weniger likely operated as a double agent until 1940 when he was appointed as director of an agricultural research institute in occupied Poland. He later returned to the Ankara embassy in 1952 as an agricultural advisor until retirement.

The divide between the "German colony" of the embassy ("Colony A") and the exiles ("Colony B") was less absolute than Scurla assumed, with many maintaining cross-group relationships.[51] In his "Critical Remarks on the Scurla Report" (2009), Neumark notes, despite embassy support, Scurla failed to persuade the Turkish authorities to replace the exiles with "loyal" Germans, underestimating Turkish diplomacy and commitment to independence.

Further research could determine whether Scurla's report reflects broader patterns and strategies of other states exporting expertise. Our analysis reveals how expertise is instrumentalised for indirect influence supported by cultural diplomacy. It also shows that the Turkish authorities were aware of the risks of employing foreign experts and developed effective counter-strategies to mitigate them.

ÉMIGRÉ EXPERTS' CONTRIBUTIONS TO STATE BUILDING

Neumark (1982: 4-7) observes that while a few countries hosted more exiles numerically, their relative impact in Turkey was more significant due to their proportion among the literate population. His own trajectory exemplifies this multilevel influence. Dismissed from his position in Germany in 1933 under anti-Semitic laws, Neumark relocated to Istanbul with his family and taught economics at the Faculty of Law. He led the establishment of the Faculty of Economics and created a transnational scientific platform through the faculty journal he directed. Neumark learned Turkish and contributed extensively to economic sciences in Turkey with his numerous publications. His transnational networks facilitated scientific exchanges, inviting European scholars to lecture at Istanbul University while reciprocally lecturing at institutions like the Sorbonne and Collège de France. In parallel, he contributed to public policy as an advisor to the Ministry of Finance, leading Turkey's tax reforms. After returning to Germany in 1950, Neumark continued to serve in academic and advisory capacities and headed the European Fiscal Committee on tax harmonisation. He leveraged his expertise on Turkey for the benefit of the German state, accompanying political leaders on state visits and advising successive governments until his death (Andic and Andic 1981; Hoss 2009: 174-6; Neumark 1982).

Neumark's (2009: 108) reflections on "Atatürk, founder of a modern and progressive Western-style republic" illustrate his assimilation of Kemalist discourse and interpretation of modernisation as Westernisation. Like Neumark, many émigrés embraced Turkish values, while simultaneously promoting cultural references rooted in their German-Western background, and contributed to social change with a Eurocentric viewpoint. Turkish leaders anticipated this dynamic, desiring the consequent hybridisation based on values they considered universal.

However, some exiles viewed this process critically, often through Orientalist lenses. For instance, in his letter to Walter Benjamin, Auerbach described Turkey's secular, modernising approach as:

> …a fanatical anti-traditional nationalism: rejection of all existing Mohammedan cultural traditions, connection to a fantasised original Turkishness, technical modernisation in the European sense to defeat the hated and admired Europe with its own weapons: hence the preference for European-trained emigrants as teachers, from whom one can learn without having to fear foreign propaganda. The result: nationalism in the superlative with the simultaneous destruction of the historical national character.[52]

51 See for example Eckstein's family friendships in Maksudyan (2024a).

52 See a copy of the letter in online archives of We Refugees. Erich Auerbach to Walter Benjamin: Istanbul – Paris 1937. https://en.we-refugees-archive.org/archive/erich-auerbach-to-walter-benjamin-istanbul-paris-1937/

This critique parallels Kemalist rupture narratives emphasising a sharp break from Ottoman history and traditions, while oversimplifying Turkey's complex transformation. Continuities with Ottoman knowledge transaction policies and the global context of the travelling models during the transition from empire to nation-state (Dogan 2021, 2022) invite a reconsideration of Turkish nation-building and modernisation as a negotiated synthesis, blending diverse governance traditions and epistemologies. Transnational experts formed just one contingent among diverse actors shaping state development (Dogan 2024). Even the German-speaking émigrés, who constituted the majority of Turkey's transnational experts, encompassed a great diversity of intellectual traditions, scientific and disciplinary approaches and political orientations – from social-democrats like Fritz Baade to communists like Margarete Schütte-Lihotzky, from socialists like Ernst Reuter to ordoliberals like Wilhelm Röpke and Alexander Rüstow.

Despite limited resources, émigré experts made distinctive contributions to Turkey's transformation, leaving an intellectual and institutional legacy that endured for generations. They spearheaded university reform, constructed a new international scientific library with their works and translations, and trained future generations across multiple sectors – from public administrators to teachers, scientists and private professionals. They made a significant contribution to language reform and Turkification, playing a key role in establishing a new lexical and semantic repertoire. They worked in terminology commissions, produced lexicons and dictionaries, and translated scientific and literary works (Berk-Albachten 2010). Spanning multiple fields such as law, economics, medicine, chemistry, anthropology, and other professional and scientific disciplines, these efforts created spaces of equivalence with European scientific and professional worlds. As advisors to public authorities, they set policy agendas, diagnosed social problems, and devised solutions in their fields of specialisation.

Among the most influential groups were economists, sociologists and jurists, many of whom were social reformers in their home countries. These social scientists played key roles in shaping economic and social policies and regulations in Turkey. Gerard Kessler, for example, taught labour economics at Istanbul University, introduced a sociology and social policy programme and with his students, established the first Turkish labour union. Ernst Reuter, socialist politician, public administrator and academic, contributed significantly to urban planning and collaborated with Turkish ministries to build public transport systems in Turkish cities. Planning Ankara's construction in the Anatolian steppes, Reuter collaborated with the famous Austrian architect Margarete Schütte-Lihotzky who had previously planned and built entire cities in the USSR's Siberian region. According to Reisman (2006: 45), Schütte-Lihotzky not only embodied the figure of a competent female professional idealised by the Kemalist modernisation project, but also brought innovative designs for working-class housing especially adapted for working women. Architects contributed to new Turkey's construction in very literal terms. Among the professors of architecture of Istanbul faculties, for example, Clemens Holzmeister and Bruno Taut designed educational institutions and iconic public buildings including the parliament and its ministries.[53]

One of the most numerous groups among the exiles was the health professionals, who modernised public health in Turkey. Albert Eckstein, a renowned paediatrician, exemplifies this contribution. Expelled from the Dusseldorf Medical Academy for being Jewish, Eckstein found refuge in Ankara in 1935, accepting the Turkish government's offer to head the paediatric clinic of Numune hospital.

53 Holzmeister's verbal autobiography includes his positive impressions of his experience in Turkey. SALT research archives. "Verbal autobiography of Clemens Holzmeister" https://archives.saltresearch.org/handle/123456789/5260

Alongside his wife Erna, also a paediatrician, Eckstein conducted extensive field trips to Anatolian villages, studying children's health conditions and advising the ministry.[54] Their efforts helped spotlight issues like infant mortality and led to concrete measures to combat diseases (Akar et al., 2007; Maksudyan 2024a).

In addition to these sectors, exiled scholars made significant contributions across various fields, including astronomy, pharmacology, chemistry, biology, mathematics, and physics. Archaeologists, philologists, historians, museologists, librarians, archivists and others contributed through their collaborations with the public authorities and the Turkish Historical Society (Türk Tarih Kurumu) to imagine the Turkish nation (for examples, see Resiman 2006: 61-86). Artists founded conservatories, art academies and symphony orchestras, and introduced opera to Turkey (Resiman 2006: 87-104). Through public policy, research, education, technology and the arts, these transnational experts contributed to shaping Turkey's national identity and development trajectory in dialogue with global modernity.

CONCLUSION

This analysis highlights the impact of North-South scientific circulations on Turkey's development, showing how scientific expertise was woven into governing practices – indeed operationalising the very nationalisation of state-society – and characterised by transnationalism. By historicising the internationalisation of science and its role in governance, it contributes to scholarship challenging the assumed novelty of these phenomena and exposing the unequal structures underpinning knowledge circulations (Benz et al., 2024).

Turkey's selective appropriation of Western models produced a hybrid system that was simultaneously outward-looking (seeking global validation) and inward-driven (anchored in Kemalist objectives). Its reliance on transnational expertise for nation-building and modernisation was an extreme case of international influence in institution-building, enabled by the unique national and international context of the 1930s. While importing foreign expertise was a long Ottoman tradition, the early Republic marked a rupture. The Kemalist government strategically engaged transnational scientists and professionals – especially those fleeing Nazism – to advance nation-building and reforms aligned with international norms, while navigating geopolitical tensions, financial constraints, and local resistance. This experience highlights both the possibilities and limitations of agency in a developing Global South state where infrastructure and resource deficiencies constrained reform implementation.

This experience also underscores how institutional and political choices can empower or marginalise scientists, whether domestic or displaced. From a sociology of expertise perspective, we see how Turkish authorities invested in expertise networks and institutional arrangements that empowered exiles as modernising experts while side-lining parts of the local intelligentsia. Far from being subaltern refugees, these scholars were received as renowned international elites and drivers of reforms. Endowed with symbolic and material resources unavailable to locals, they reshaped science and governance in Turkey, advising ministries and co-authoring legislation, public policy and reform projects. Their activities exemplify intense academia-state entanglement in a transnational setting structured by global-to-local inequalities.

54 See the photographs Eckstein took on these excursions on: Cambridge Digital Library. "Saving Turkey's Children" https://www.cam.ac.uk/stories/eckstein

Yet, despite their privileged status in Turkey, these scholars experienced downward mobility in global scientific fields, with limited international recognition due to Turkey's peripheral position. They leveraged these challenges through ties to transnational scholarly networks extending from academia to influential NGOs such as the Rockefeller Foundation, enabling eventual reintegration into prestigious institutions. Their trajectories illustrate the multidirectional yet unequal exchanges structuring both nationalisation and internationalisation of science and the potential for translation and adaptation.

A socio-biographical lens further exposes persistent inequalities within international markets of expertise – particularly regarding gender and geography. Although expert profiles remained predominantly middle-aged White males from the Global North – especially Germany – with significant cultural, social, and political capital (Dogan 2021), experts recruited during the early Republican era showed greater diversity in intellectual and ideological orientations and trajectories, including a small but growing number of women. While various categories of transnational actors contributed to Kemalist Turkey's wide-scale national construction and reform agenda, "émigré experts" were both numerically and qualitatively the most influential. Their impact extended far beyond the university reform, shaping social, legal, and economic policies. The massive integration of these German-speaking scholars into Turkey's public institutions, alongside other Western and even Nazi-affiliated experts, highlights expertise as a site of collaboration but also competition. Despite resource shortfalls, language barriers, cultural frictions, and local resistance, they played a decisive role in Turkey's development, training new generations, reshaping institutions, and fostering cross-cultural exchange. While economists, jurists, and other social scientists actively participated in policymaking, other academic disciplines, including medicine, architecture, and the arts, also contributed to governmental projects, transforming Turkey from a Eurocentric perspective, and fostering state development and nationalisation as hybridisation.

Even during the interwar years, contemporary benchmarks were set by US and European institutions, establishing an assumption of "global comparability" to which Turkish institutions had to adapt as part of one global system. This analysis challenges the idea that expertise, even in its "scientific" form, can be non-hegemonic or neutral: global power relations invariably shape which systems become the norm. Turkey's case shows how efforts to avoid dependency can reproduce new dependencies.

As an unintended consequence of Turkey's reliance on transnational experts who settled in Turkey, the dominance of German-speaking experts inadvertently reinforced Germany's long-term intellectual and cultural influence. The prevalence of German as a foreign language in Turkish education and the broader professional sphere reinforced path dependencies, increasing Turkey's demand for German language teachers and laying the groundwork for post-war academic and cultural ties with Germany including initiatives like the Goethe Institutes (established in 1956).[55] This strong German influence is rivalled only by the United States, which has gradually occupied the hegemonic position, exporting expertise globally (Dezalay and Garth 2011) and locally in Turkey, expanding its sphere of economic and political influence since the beginning of the Cold War (Erken 2020; Widmann 2000). This legacy invites comparative research on how states today negotiate transnational expertise in a world shaped by power asymmetries.

55 Glasneck (1966) observes that German authorities maintained ties with university graduates who returned to Turkey after studies in Germany and became managers in schools, technical institutes, railways, industry and mining, agricultural institutions and public administrations.

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ARTICLE

The Space of Chinese Sociologists and Their Opinions on the Localisation/Sinicisation of Chinese Social Sciences

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Abstract

Since the mid-1920s, Chinese sociologists have explored different paths toward the localisation/sinicisation of social sciences. While sociologists and social experts are now more broadly invited by the Chinese government to develop "social sciences with Chinese characteristics" and an "autonomous system of knowledge" to "tell beautiful stories about China", vigorous academic debates are ongoing. Based on the International Chinese Sociologists Value survey, our article explores the possible structural homology between the position of Chinese sociologists and their position taking on the localisation debate by using geometrical data analysis. It demonstrates and explains its non-existence and showcases how Chinese sociologists capitalise on different sources of knowledge and silently limit the politicisation of their craft.

Keywords

China, Sociologists, Localisation, Structural Homology, Eigensinn

INTRODUCTION

Since the introduction of sociology to China, Chinese sociologists have perceived the discipline as a means of modernising their nation, with the aim of making it as powerful and wealthy as Western powers. More precisely, as pointed out by Aurore Merle in 2007, early 20th-century Chinese social scientists and intellectuals wanted to "transform Chinese society in order to 'save the country' and build a modern nation in the face of the threat from Western powers" (Merle 2007: 31). Scientists in general and social scientists in particular were expected to play a crucial role in this process by bringing their applied expertise to political leaders. Prominent intellectuals, such as Chen Duxiu, one of the founding fathers of the Chinese Communist Party (CCP), spoke of two gentlemen named "Mr. Democracy" and "Mr. Science" (Chen 1919), who were expected to cure China of the darkness in its politics, morality, academic scholarship, and thought (Fan 2022; Gu 2001).

Simultaneously, the pursuit of scientific modernisation in the nation through the acquisition of scientific knowledge and development has consistently raised concerns about preserving Chinese identities and culture. Chen Duxiu stated the following: "If we support Mr. Democracy, we must oppose Confucianism, rites, chastity, old morality, and old politics. If we support Mr. Science, we must oppose old arts and old religion" (Schmalzer 2022). However, most Chinese experts and scientists in

the early 20th century sought to harmonise Western modernity with Chinese traditions in governing society and advancing science. Notably, Liang Qichao, a prominent education reformer and intellectual and one of the early advocates of the development of sociology, which was referred to as the study of groups (*qunxue*), championed this perspective (Chen 2018: 10).

Within this specific political context, Chinese sociologists during the 1930s contemplated how to adopt advanced methods, theories, and knowledge from the West while adapting these theories to "Chinese circumstances" and preserving the Chinese intellectual heritage (Zheng and Wang 2009: 27). For instance, Sun Benwen, one of the most eminent sociologists of the 1920s, articulated the following (Li and Cao 2013: 3):

> Using methods imported from the United States and Europe and starting with the theoretical intricacies of European and American scientists to develop a distinctly Chinese ideology and societal system, then following this with a practical analysis based on the realities of the entire country and amalgamating it all to establish a sinicised sociology, is a task of utmost urgency.

The notion of developing a sociology with local (*bentu*)[1] or sinicised (*zhongguohua*) characteristics re-surfaced shortly after 1978 when the CCP lifted the ban on sociology[2] and entrusted Fei Xiaotong with re-establishing the discipline (Li 2015). As was the case in Taiwan and Hong Jong, in Mainland China, the localisation/sinicisation debate experienced various periods of blooming and fading, while the attempts to localise/sinicise the social sciences were continuously renewed. For instance, in the late 1980s, some authors advocated for a localised social sciences to conform to Marxism and Mao Zedong's thought, while others began to pay attention to the localisation/sinicisation attempts made in the US and Taiwan, which criticised modernity and positivism (Chen 2022).

At the beginning of the 21st century, the number of sociologists exploring this path became so significant that the definition of the localisation/sinicisation of social sciences remains highly ambiguous (Xie 2021; Xie and Roulleau Berger 2017). This ambiguity ranges from Zheng Hangsheng's attempt to amalgamate Chinese and Western theories while accentuating *Chinese characteristics*

1 In this article, we translate the term bentuhua as localisation. While we argue that sociologists have proposed different definitions of localisation, their common idea is to adapt or develop methods and concepts that better depict Chinese society and its transformation. The term localisation was already used (including by the same scholars) in the 1920s together with the term sinicisation (zhongguohua). Therefore, they are relatively equivalent. Nevertheless, sinicisation has a more nationalistic connotation. It relies on the assumption that the long-lasting civilisation formed by Chinese society is too unique to be captured through the concepts of other civilisations. Compared with the concept of localisation, it relies on a stronger assumption: the homogeneity of Chinese culture and civilisation over centuries and across territories.

2 The ban on sociology during the period 1952–1978 was quite unique compared to other so-called socialist countries. As explained by Lu (2021: 175–187), several important sociologists from the Republican period were consulted over the land reform movement (1950) during the first year of the regime after receiving ideological training. In short, sociologists were asked about how to classify the peasants into landlords, rich peasants, and poor peasants and how to redistribute the land to the poor peasants. Sociologists then contributed to the ethnic minority classification work in which they were asked to conform to Stalinist principles and Marxist ideology. Considering the Soviet grand brother model, Chinese leaders reformed higher education in 1952. They favoured engineering and applied sciences, closing several law departments and banning sociology. Sociology was initially considered a bourgeois science in both countries. However, the discipline was revived under strict political control in the Soviet Union in the context of de-Stalinisation, starting from 1956 (Weinberg 2024). In China (Lu 2021: 175–187), Fei Xiatong, Chen Da, Wu Jingchao, Wu Wencao, Lei Haojing, Sun Wenben, and Yuan Fang carefully began to revive the discipline in 1956. However, their attempt did not survive the anti-rightist campaign. This political campaign followed the Hundred Flowers Campaign during which citizens were invited to advise the government and the Party. In every working unit, Party secretaries had to find a pre-established proportion of rightists (i.e., enemies of the regime). Western-educated intellectuals particularly suffered from this political campaign lasting from 1957 to approximately 1959. For instance, Fei Xiaotong was purged and considered a major rightist.

(Zheng and Wang 2009) to Zhai Xuewei's and Jing Tiankui's essentialist and culturalist efforts to reconstruct Chinese sociological theories based on ancient Chinese philosophers' concepts and values (Zhai 2001; Jing 2022). Jing Tiankui's project can be seen as the most radical because it deviated from localisation (which implicitly conveys the idea that something is imported) to create a native sociology entirely without foreign influence. At times, the endeavour to localise/sinicise sociology seems to resemble an academic carnival (Bayard 2010).

While Chinese sociologists held varying opinions regarding the necessity for and approach to the localisation/sinicisation of social sciences, the then CCP general secretary, Xi Jinping, tasked them with further exploring this path (Froissart 2018). Experts in the social sciences are not only expected to develop concepts and methods for a deeper understanding of the sociohistorical transformations in society but are also encouraged to create a discursive system capable of challenging Western ideological dominance (Yan and Cao 2017: 267).

In this context, recent years have witnessed particularly contentious debates in Chinese sociological journals (Fan 2024), with some advocating for the localisation/sinicisation of the social sciences (Zhou 2020; He 2020) and at times aligning with the views of the top leaders (Li 2021; Hong 2022). Meanwhile, sociologists such as Xie Yu (2018) have characterised the localisation of the social sciences as a *pseudo debate*.

In response to this fervour for localisation/sinicisation, other sociologists, inspired by Pierre Bourdieu (1988), have begun to investigate a potential structural analogy between positions in the field of sociology and positions taken in this debate. According to Li Junpeng et al., (2022), those supporting the localisation/sinicisation of the social sciences are purportedly marked by "positive academic and political" capital, while "different combinations of academic and political capital result in different positions on indigenisation". In other words, according to their perspective, individuals fall into categories such as "radical indigenists", "moderate indigenists", "moderate universalists", or "radical universalists", depending on the extent and structure of their academic and political capital.

The present article revisits the question of the potential structural homology between sociologists' positions in the field and their stances on the localisation/sinicisation of social sciences through an analysis of a representative survey of 167 Chinese sociologists. This survey enables us to collect opinions from Chinese sociologists on this debate and construct their positions in the academic space through a multiple correspondence analysis (MCA), utilising various distinguishing characteristics, including the academic positions of sociologists, their recent publications, areas of specialisation, research grants, and other indicators related to *academic capital*.

Ultimately, this examination of the academic space of Chinese sociologists concludes that there is an absence of structural homology, even in an approximate sense. Indeed, the majority of Chinese sociologists consider the development of indigenous theories to be compatible with the utilisation of sociological theories originating in Western countries. They largely adopt a pragmatic perspective that echoes Deng Xiaoping's assertion: "It doesn't matter if the cat is black or white, so long as it catches the mice" (Deng 1962 [1989]: 305). We elaborate on how this viewpoint may be influenced by the fact that Chinese sociologists, whether or not partially trained abroad, extensively read and utilise both Western and Chinese sociological texts, thus building their legitimacy through publications that draw from various theoretical traditions.

Beyond this consensus, those who regard the localisation/sinicisation of the social sciences as a pseudo debate occupy slightly different positions in the academic space. More specifically, we argue

that those who perceive the localisation/sinicisation as a pseudo debate possess a large amount of international academic capital, while those who wish to combat Western hegemony through the localisation/sinicisation of social sciences appear to be dominated on both sides (i.e., by both those who possess national academic capital and those who possess international academic capital).

In conclusion, our paper explains why there is limited enthusiasm among Chinese sociologists for embracing the localisation/sinicisation project as defined by the Chinese leadership. Using Alf Lüdkte's concept of *Eigensinn* (Lindenberg and Lüdtke 2020; Rowell 2015), we elucidate why so few Chinese sociologists view the localisation/sinicisation of social sciences as crucial and necessary for challenging Western hegemony.

THE PERCEPTION OF CHINESE SOCIOLOGY'S ORIGINS AND IDEAL DEVELOPMENT: A QUESTION OF ACADEMIC TRAJECTORY

Since 1979, Chinese sociologists have displayed a heightened reflexivity regarding the history and development of their discipline. Concerning the history of the discipline, scholars diverge in identifying the origins of Chinese sociology, assigning different weights to various schools of thought. For instance, Zheng Hangsheng and Li Yinsheng's book *History of Chinese Sociology* (Zheng and Li 2000), originally published by the Higher Education Press and now re-published by the Marxist Editing House for Textbooks (*Ma gongcheng jiaocai*); dedicated an entire chapter of its first volume on "Chinese Sociology before 1949" to the development of the Marxist sociology school.

In contrast, Fei Xiaotong, a highly influential scholar who was responsible for re-establishing sociology after 1979 and who presented himself as being influenced by Bronislaw Malinovsky in his early career, did not mention the existence of such a school of thought in his 1994 article published in *Sociological Studies* (*Shehuixue Yanjiu*) (Fei 1994). According to Fei, the Yanjing school of thought and Sun Benwen played more prominent roles in the development of the discipline by writing textbooks in Chinese starting from 1931, a time when all the available sociological books were written in English. As Fei stated, when the sinicisation of social sciences began in the 1930s, Marxism's influence in sociological departments "was pretty limited because of the political situation of that time" (Fei 1994: 6). In other words, unlike Yan Ming's well-substantiated research, Fei Xiaotong underlined the influence of Western sociologists, including British-located and American scholars who came to China (e.g., Robert E. Park). In the view of Fei and Yan, the birth of Chinese sociology appeared to be, at least at the beginning, the result of a quite common process of internationalisation (Sapiro 2023), which preceded its institutionalisation as an academic discipline and subsequent indigenisation. This claim contrasts sharply with Zheng and Li's (2000) *History*, which strongly emphasised localisation and Marxism, as if the Western influence was just a detail of the history of the discipline from which Chinese sociologists were immediately emancipated.

Jing Tiankui (Jing 2022) somehow went even further in silencing the importance of foreign influences on the birth of sociology. While most specialists have identified Yan Fu and Liang Qichao, who were strongly inspired by Spencer's social Darwinism and pioneers of Chinese sociology in China (Chen 2018), Jing found the origin of sociology in ancient Chinese philosophers and scholars such as Xunzi (Jing 2022: 6). Paraphrasing Bourdieu, this contrast in the way of presenting the history of the discipline suggests a symbolic struggle to determine the legitimate origins of sociology and, by extension, to justify one's theoretical opinion in the academic field (Bourdieu 2014).

Indeed, sociologists have identified various origins of the discipline and celebrated these very differently depending on how they have made their own intellectual careers and positions in the academic

space. More specifically, Fei Xiaotong, a sociologist trained by Bronislaw Malinowski, a prominent anthropologist from the London School of Economics and Political Science, emphasised the strong international character of the first age of Chinese sociology. Zheng, a professor at the People's University of China who trained as a philosopher before the Cultural Revolution when Marxism became the principal philosophical *doxa*, emphasised the influence of Marxist scholars. Finally, Jing, who enrolled at university in 1967 during the Cultural Revolution (just one year after becoming a Party member) and completed his training after the Cultural Revolution, advocated for a supposedly long tradition of Chinese sociological studies, while building his own reputation for developing indigenous Chinese sociological concepts inspired by Chinese philosophy.

These quite different discourses on the origins of Chinese sociology invite us to take some precautions regarding possible symbolic struggles in the academic space and to adopt a more empirical stance in analysing the debate on the localisation of social sciences in China. Indeed, Jing Tiankui's case suggests that *academic trajectory*, which can be defined as an individual's succession of training places, theories explored, fieldwork, and accumulated distinctions and positions, should all be taken into consideration in determining a scholar's opinion on the localisation/sinicisation of social sciences.

BEYOND LI JUNPENG AND COLLEAGUES' SHORTCOMINGS

Li Junpeng et al., (2022) were the first to investigate the relationship between a Chinese sociologists' academic trajectory, political stance, and position taking on the localisation/sinicisation debate. Their study was based on a panel of 32 prominent sociologists, almost all of whom had a professorial position or equivalent (e.g., researcher) and had expressed their opinions in academic papers published in the last two decades. They divided this group of sociologists into four different groups, namely "radical indigenists", "moderate indigenists", "moderate universalists", and "radical universalists", based on the opinions expressed by these sociologists. More precisely, the first criterion of division "stands out: whether a sociologist agrees that Western theories can be directly applied to Chinese society" (Li et al., 2022: 58). This criterion helped them distinguish between indigenists who "believe that Western theories cannot be directly applied to China and that all of those theories must be indigenised" and universalists who "see no point in making a distinction between Western and indigenous theories and have no problem with applying mainstream theories originating in the West".

To refine their distinctions between scholars, Li et al., (2022) used a second criterion based on how sociologists view the possibility of comparing China with other societies. According to Li et al., (2022: 58), moderate indigenists differ from radical indigenists because they are "open to the comparison between Chinese and Western societies". In a similar way, they distinguish moderate universalists by their propensity to "analyse the uniqueness of Chinese society through rigorous research and to compare it with Western societies".

At this point, their methodology is quite transparent but immediately problematic. Xie Yu, who performed many comparisons between China, East Asian Countries, and the US in terms of marriage and fertility (Raymo et al., 2015), is classified as a radical universalist. Li Peilin (2021) and Li Youmei (2023), who both followed Xi Jinping's call to establish a new *discursive system* challenging the West, are classified together with Zheng Hangsheng, who stood for a more moderate and less politically cleaved opinion, advocating the development of local theories without completely neglecting

so-called Western theories. Finally, Xie Lizhong, who defended the usefulness of non-Western sociologies, is classified as a moderate universalist, although he could also be viewed as a moderate indigenist (Xie 2021).

In addition to the unrepresentativeness of their sample and the classification issues, the paper by Li et al., (2022) contains methodological shortcomings. While the authors suggest a Bourdieusian structural homology, stating that "different positions in a field are associated with different ideas" (Li et al., 2022: 62), they never use the *objectivisation tools* Bourdieu repeatedly employed to assess the existence of such homologies (Lebaron 2009). More specifically, they do not adapt the model applied, for example, to the study of German sociologists (Schmidt-Wellenburg and Schmitz 2022), in which data are collected systematically *before* constructing an academic space through a correspondence analysis or related method and *before* investigating how opinions on localisation are located in this space.

This shortcoming is quite surprising given the abundant literature explaining how Bourdieu viewed social spaces and fields as *relational* and as intimately bounded with the systematic use of correspondence analysis and related methods, such as MCA, also named geometric data analysis in the literature (Lebaron 2009; Rouanet, Ackermann, and Le Roux 2000). Bourdieu himself stated as follows in the preface of the German edition of the *Craft of Sociology*:

> I use Correspondence Analysis very much, because I think that it is essentially a relational procedure whose philosophy fully expresses what in my view constitutes social reality. It is a procedure that 'thinks' in relations, as I try to do with the concept of field (Bourdieu, Chamboredon, and Passeron 1991: xiii).

As Le Roux and Rouanet (2010: viii) explained, MCA enables a relational form of reasoning in so far as its object is:

> …to display geometrically the rows and columns of the data table—where rows represent individuals and columns the categories of the variables—in a low-dimensional space, so that proximity in the space indicates similarity of categories and of individuals.

Moreover, it enables the re-construction of groups of ideal-type characteristics in a population (for the space of the categories of the variables) and groups of individuals sharing relatively homogenous characteristics (for the space of individuals), before inquiring how these ideal-types characteristics correlate with other properties that are not used to construct the space. Such methods have already been used to explore the power structures and symbolic divisions in the field of American sociology (Warczok and Beyer 2021) and to explore the affinities between social science disciplines, position taking toward non-academic institutions, and interests and research subjects among Danish social scientists (Kropp 2013). In the case of Chinese sociologists, it would thus be necessary to construct the space of Chinese sociologists before inquiring how their positions align with their opinions on the localisation debate and before eventually making conclusions regarding the existence of a structural homology, that is, an alignment between positions and opinions.

Given these shortcomings, the present paper aims at using appropriate data, methodology, and concepts to reinvestigate the question of a possible structural analogy between positions taken in the sinicisation debate and positions in the academic space. Regarding the data and methodology, as explained in more detail in the next section, we present a survey of a representative sample of the

population of Chinese sociologists working in Project 211 and Project 985 universities,[3] which correspond to the elite universities identified by the Chinese Ministry of Education (Qingnian, Duanhong, and Hong 2011). These universities are particularly important given that they receive more economic support from the central authorities, and they were generally among the first universities to establish a sociology department. For instance, the People's University of China belongs to the 39 Chinese universities that have had the mission of becoming "first-rate universities of international advanced level" since 1998.[4] Its department of sociology was established in autumn 1987, and in 1989, there were only 10 departments of sociology in Chinese universities (Wang 1989). Regarding Project 211, this was initiated in 1995 to prepare Chinese universities to overcome the challenges of the 21st century. The selection was less strict since 116 universities and departments had been awarded the label by 2008, including less renowned universities, such as the Tibet university. In addition, many 211 universities, including Shehezi University, do not have a sociology department and are not recorded in international rankings, even in the Shanghai Academic Ranking of World Universities, which includes more Chinese universities from Mainland China.

In addition, our sample includes sociologists working in the national or local branch of the Chinese Academy of Social Sciences (CASS), which played a key role in the re-establishment of the discipline in 1979 and is more committed than universities to producing policy reports, such as the Analysis and Projections for the Chinese Society (Boucher 2013).

CONCEPTUAL FRAMEWORK

In terms of our conceptualisation, we draw our inspiration from the seminal work of Pierre Bourdieu (1988) on the academic field, while refining his concepts because of the specificities of our *object of research*. For instance, we do not use the term *field* in this research because the sociohistorical configuration analysed by Bourdieu is too different from the one we investigate. As Bourdieu (1993: 72) explained, a field exists in the sense of "specific stakes and interests, which are irreducible to the stakes and interests specific to other fields (you can't make a philosopher compete for the prizes that interest a geographer)". Moreover, a field shapes the structure of the relations between social agents who at least agree with the idea that the game is worth being played. Consequently, a field exists insofar as it enjoys a *relative autonomy* from other fields. In addition, a field may be considered as a space that is not polarised with respect to a single end (or *nomos*, to reuse Bourdieu's word), as is the case for the social space.

Given these definitions, we consider that Chinese sociology does not form a field; rather it forms a *bi-nomos space* (i.e., a space in which social agents are equipped with two sets of principle of vision and division that determine what is sayable and thinkable). Indeed, since the discipline was re-established in 1979, the CCP has used its monopoly of symbolic violence and rational–legal means to subordinate sociologists. For instance, during the national meeting for the planning of philosophy and social sciences organised in March 1979 (*Chinese Sociology Statistical Yearbook: 1979–1989* [1989]: 9), attendees were asked to "carry out the work of sociological research" according to the "ideological principles of Marxism–Leninism and Mao Zedong's thought" and to "contribute to the

3 These two projects relate to specific announcements by the Ministry of Higher Education. Both announcements detail the objectives of these universities, provide them with additional resources, and contain a list of the universities enrolled in the two projects. Therefore, "211 universities" correspond to the universities benefiting from Project 211, while "985 universities" refer to the universities benefiting from Project 985. Some universities are both 985 and 211 universities.

4 Nine universities benefited from Project 985 in 1998, and the list of 985 universities was enlarged in 2004.

Chinese socialist modernisation". Put differently, Chinese sociologists had to adhere to the ideological principles of the regime and to serve the regime's ends to be tolerated and supported as scientists producing knowledge about society.

While the importance and political control of scientific production and producers have varied from one political period to another, this rational–legal subordination of scientific activities to political power has not since disappeared. According to Article 3 of the statute of the Chinese Sociological Association, the association's vocation still adheres to the "guidance of Marxism–Leninism, Mao Zedong Thought, Deng Xiaoping Theory, the 'Three Represents,'[5] the Scientific Outlook on Development,[6] and Xi Jinping Thought on Socialism with Chinese Characteristics for a New Era". According to the same article, the association's mission is to establish the "Four Consciousnesses", strengthen the "Four Self-Confidences", and achieve the "Two Safeguards", while ultimately helping "to build a prosperous, strong, democratic, civilised, harmonious, and beautiful modern socialist society".

Therefore, Chinese sociologists have a double mission. They must not only produce knowledge but also adhere and contribute to the ideological and societal goals determined by political leaders, without having the legal and practical independence to define their own profession. The space of sociologists is so intertwined with the space of power that political outputs, such as reports considered by political authorities to be deserving of study by the cadres (*pishi* in Chinese), are considered in academic evaluations as equivalent to publications in high-ranked peer-reviewed journals. In Bourdieusian terms, this means that political capital (Bourdieu 1993: 126) can be converted into academic capital and vice versa, as *contributions* to science can, in certain cases, lead to political responsibilities or honours.[7]

In addition to these conceptual considerations, our study differs from Bourdieu's work by offering a new conceptualisation of the states and sources of academic capital. In his earliest research on the *scientific field*, Bourdieu extensively used the term *scientific capital* (Bourdieu 1976). He pointed out that scientific capital depends on the possession of "rare scholarly titles", although he was not as precise as in his studies of the different forms of cultural capital, in which he came to objectivise, measure, and conceptualise the different *states* of scientific capital. Moreover, since 1976, when Bourdieu's article was published, the ways in which social scientists are evaluated, classified, and distinguished have changed tremendously, especially in China (Tian, Su, and Ru 2016; Yaisawarng

5 This doctrine is the legacy of Jiang Zemin, general secretary of the CCP from 1989 to 2002. It stipulates that the CCP leads the advancement of the productive force (i.e., leads the workers), coordinates China's advancement in science, technology, and culture (i.e., leads the intellectuals), and represents the fundamental interests of the Chinese people (i.e., leads politically for the interest of the majority).

6 This doctrine is the legacy of Hu Jintao, general secretary of the CCP from 2002 to 2012. It emphasises the importance of sustainable development, rebalancing coastal and inland province development, and promoting the material and spiritual development of the people.

7 For instance, Hong Dayong, a professor at the People's University of China, was designated as head of the Propaganda Bureau in 2024. Li Peilin, the former head of the sociology department at the Chinese Academy of Social Sciences, was nominated as an alternate member of the Central Committee of the Communist Party of China in 2012. As an alternate member of the Central Committee, he belonged to the most influential political advisors in China, debating the future implementation of policy and laws before they were decided by the Politburo and then ratified by the National People's Congress. This function led him to accompany the members of the Central Committee on several occasions, including diplomatic visits.

and Ng 2014; Beyer and Schmitz 2023; Tian and Lu 2017; Han and Li 2018; Xu, Rose, and Oancea 2021).

While Bourdieu (1991: 7) distinguished between various forms of scientific capital, such as the statutory authority one scientist inherits from their affiliation with a more or less renowned institution, we conceptualised the different forms of academic capital according to the peculiarities of 21st-century Chinese sociologists who are embedded in a structure of highly hierarchised and hierarchising institutions, which provides the possibility of securing reputation and status through national or transnational *achievements* (mainly grants and publications in peer-reviewed journals).

The academic capital of Chinese sociologists depends on the following aspects:

1. The statutory state: This refers to honourific titles, responsibilities, and positions in institutions that signal long-lasting expertise in the domain. A high level of statutory academic capital means that scholars belong to a group of elite sociologists who control the careers, publications, and grants of newcomers. Typically, this form of capital includes aspects such as academic titles (professor, associate professor, assistant professor, etc.); whether people work for a 211 or 985 institution or another kind of institution whose prestige is unequal; whether people are on the boards of prestigious academic journals; whether they are experts who review research proposals for the national grant foundation. Sociologists who have a large amount of academic capital in its statutory form have generally previously accumulated other forms of academic capital and somehow control the opportunities to distribute academic capital (e.g., through the power of nomination and selection).

2. The objectivised state: This refers to achievements quantified by departments and universities to assess the productivity of their faculty members. From interviews with Chinese sociologists, we came to understand the critical importance of this form of capital for obtaining tenure and promotion. It principally relates to the accumulation of national research grants, publications in peer-reviewed journals, and to a lesser extent the publication of books and book chapters.

3. The pedagogical state: This consists of the pedagogical duties and achievements required of sociologists. For instance, sociologists more equipped in pedagogical academic capital are asked to teach and supervise postgraduate students and to possess the certification required to supervise PhD students, and they generally enjoy a lower teaching load. This form of capital is sometimes converted into objectivised academic capital, as some researchers reported publishing research based on empirical material collected by their students. This capital is regarded as less important for tenure and promotion, as pedagogical skills (often mismeasured by student evaluations of teaching performance [Langbein 2008]) are sometimes not considered or only marginally considered (i.e., the academic only needs to avoid being among the 25% of lowest-rated teachers to be considered qualified for tenure).

4. The socialised state: This relates to the people and institutions through which sociologists can obtain human, economic, and collaborative support, enabling them to capitalise more efficiently from other forms of academic capital. This state of capital is the hardest to measure, but it can be approximated through sociologists' affiliations with various professional associations and interest groups in academia.

Given our pre-existing knowledge of the evaluation system in sociology departments and the debates between international and national scientific products and producers, it was necessary to distinguish the sources of academic capital. As previously explained by Zhang Letian (2008), Chinese sociologists trained in China and abroad do not have the same intellectual resources to make a career in the field, do not necessarily research the same topics, and can be more or less advantaged in their career advancement depending on how their departments value international and national publications in peer-reviewed journals.

In addition, the distinction between national and international sources of academic capital has been identified as structuring the positions and position taking in other national academic fields, reflecting the increasingly globalised nature of national academic fields (Heilbron 2014). For instance, Schmidt-Wellenburg (2024: 86) noted that French economists working in France and known for heterodox positions had a lower volume of capital and symbolic capital compared to French economists who had graduated or worked abroad. In another study of German sociologists, Schmidt-Wellenburg and Schmitz (2022: 115) demonstrated that the conflict between sociologists using quantitative methods (inspired by natural sciences) and sociologists using qualitative methods (inspired by the human sciences tradition; *Geisteswissenschaften*) was intimately bound to international academic capital for the former (publication in English) and national academic capital for the latter.

Therefore, for the Chinese sociologists, it was particularly important to capture whether they were publishing in journals included in the Social Sciences Citation Index (SSCI) and/or the Chinese Social Sciences Citation Index (CSSCI). For the same reason, we asked them if they had experience of working abroad and if they had obtained their PhD degree in or outside Mainland China.

With these conceptual and methodological considerations in mind, the reader can better understand the data and methods presented in the next section.

DATA AND METHODS

Data from the International Chinese Sociology Value Survey

The first step of this investigation required the design of an ad hoc and representative survey: the International Chinese Sociology Value Survey. This strategy was preferred to a full sample because we observed that several faculty members working in 211 universities did not have webpages through which we could find their email addresses; thus, they were possibly less likely to participate in our survey because of their subordinate status.

The first section of the survey entitled Academic Career contained 17 questions, principally requesting the sociologists' positions, working units, areas of expertise, pedagogical duties, research grant achievements, board memberships, and involvement as experts for the national research grant foundation. It thus principally captured the statutory state of academic capital. The second section contained 20 questions measuring the objectivised state of academic capital, principally the number and types of publications accomplished in the three years preceding the study.[8] The third section was shorter, containing only eight questions relating to affiliations with national and international professional associations. It captured academic capital in its socialised state. The fourth part, which was most relevant to the current paper, contained six questions exploring the intellectual references of

8 The importance of the objectivised state of academic capital for tenure and promotion, together with the necessity of identifying the transnational or national nature of such capital, invited us to generate more questions regarding this aspect.

the sociologists (sociologists with whom they were familiar and sociologists who inspired their research), and the sociologists' opinions toward the localisation/sinicisation of social sciences. The possible answers to the question "What is your opinion on the localisation of social sciences?" were as follows: "The localisation of sociology is an urgent affair; it will help improve the level of Chinese sociology and challenge Western academic hegemony"; "The localisation of sociology can eventually provide some help but will not prevent the use of Western theories to explain Chinese society"; "The localisation of sociology is a pseudo proposition because most of the concepts and methods in social sciences are common"; "I do not have a clear opinion"; and "Other; please be precise".

After the design of the survey, we elaborated a strategy to obtain a representative sample of Chinese sociologists working in Mainland China. The population surveyed in Mainland China consisted of two subpopulations: 1) full-time sociologists working in a college of social sciences or an independently established sociology department at a Project 985 or Project 211 university (we named this population the "university sample"); and 2) full-time researchers working at the China Academy of Social Sciences (CASS) or one of its local branches (we named this subpopulation the "CASS sample").

For the sociologists in the university sample, a research assistant first created a list of 938 sociologists from 41 universities. This list was relatively exhaustive since there were 43 universities with independent schools of social sciences or sociology departments (22 for Project 985 universities and 21 for Project 211 universities) at the time of the survey.[9] The information contained in this list included names, genders, research areas, and emails. From this initial list, we used random sampling to select 375 sociologists working in 20 different universities, selecting 25% of the overall population. By doing so, we made sure that sociologists working in less prestigious universities would be equally invited to complete the survey. We then invited these sociologists to complete the online survey in December 2021, and we sent a reminder in February 2022.

For the CASS sample, the sampling process was partially different from that of the university sample. This was because the website construction of provincial-level social science academies is lagging far behind that of universities, and they are less likely to provide faculty members' contact details and profiles. After careful consideration, we selected 12 sociological research institutes from 31 provincial social science academies based on geographic location: southeast, northwest, and central. The overall sample size comprised 187 people (including 82 people from the national CASS located in Beijing). For the CASS sample, we applied the same sampling principles and procedures as for the university sample, selecting 25% of the overall population (83 people).

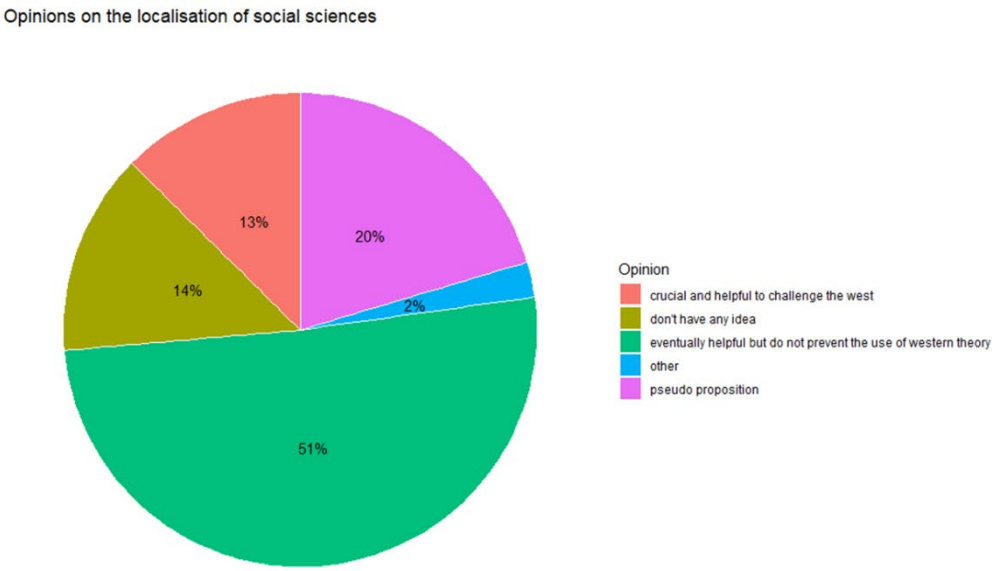
Finally, the effective response rate was 36.8% (n = 138) for the university sample and 34.94% (n = 29) for the CASS sample. Compared to similar surveys conducted overseas, the survey response rate was fairly standard (Shih and Fan 2008). We finally obtained a sample of 167 respondents, corresponding to approximately 15% of the entire population of sociologists, albeit highly representative of institutional divisions and hierarchy.

Overview of the opinions on the localisation of social sciences

9 We could not open the websites of the sociology departments at Suzhou University and Xinjiang University. Therefore, these two universities were excluded from our sample.

Thanks to this survey, we obtained an initial idea of how the Chinese sociologists rely on so-called Chinese and Western theories, as well as their opinions on the localisation/sinicisation debates.

Figure 1: Distribution of opinions on the localisation of social sciences Source: ICVS data



As can be seen from the pie chart, more than half the sociologists support the idea that localisation could eventually be useful but does not prevent the use of Western theories and methods. In other words, a relative consensus exists on this debate, while polarised opinions (i.e., "pseudo proposition" and "crucial and helpful in challenging Western academic hegemony") are relatively rare. More precisely, those having a polarised opinion represent only one third of the respondents, and the sociologists not having any idea (or perhaps not daring to communicate their opinions) outnumber the sociologists declaring the localisation of social sciences as crucial and as enabling Chinese sociologists to challenge Western academic hegemony.

Table 1: Number of Chinese sociologists with whom the sociologists declare familiarity (max. 5).

			Number of Chinese sociologists with whom the sociologists declare familiarity			
			0	1	2 or more	Total
Place where the sociologists obtained their highest diploma	Mainland China	Frequency	24	31	60	115
		Row %	20.9%	27.0%	52.2%	100.0%
	Other locations	Frequency	17	25	10	52
		Row %	32.7%	48.1%	19.2%	100.0%

Cramer's V = 0.31, chi-square = 16.073, p-value = 0.006.

These results are not surprising given that the majority of Chinese sociologists who had obtained their highest diploma abroad mention being familiar with at least one of the five Chinese authors

proposed (Fei Xiaotong, Zheng Hangsheng, Lin Nan, Yuan Fang, and Lu Xueyi), while Chinese sociologists who had obtained their highest diploma in Mainland China are also very familiar with Western sociologists.

Table 2: Number of foreign sociologists with whom the sociologists declare familiarity (max. 16)

			Number of foreign sociologists with whom the sociologists declare familiarity						
			0	1	2	3	4	5 or more	Total
Place where the sociologists obtained their highest diploma	Mainland China	frequency	3	13	15	21	11	52	115
		row %	2.6%	11.3%	13.0%	18.3%	9.6%	45.2%	100.0%
	Other locations	frequency	0	7	7	11	6	21	52
		row %	0.0%	13.5%	13.5%	21.2%	11.5%	40.4%	100.0%

Cramer's V = 0.061, chi-square = 12.12, p-value = 0.85

In Table 1, the results for Cramer's V suggest that sociologists who graduated abroad are less familiar with Chinese sociologists, while less than one-third are entirely unfamiliar with the proposed Chinese sociologists. Regarding the familiarity with Western sociologists reported in Table 2, there is no statistical difference between sociologists depending on where they obtained their highest diploma, as the majority are familiar with four or more foreign authors in the list.

To conclude this brief overview, both foreign sociologists and Chinese sociologists belong to the mental universe of Chinese sociologists, and those only familiar with Chinese sociologists are rather rare (1.7% of the entire population). Given these circumstances, it is not surprising that Chinese sociologists who partly draw on foreign authors in their research generally advocate for the addition of their sociological theories and methods.

Construction of the space of sociologists

While these preliminary results tend to suggest that the opinions are not necessarily very polarised, MCA is still necessary to construct the space of sociologists before inquiring into a possible structural analogy between positions in the academic space and opinions on localisation. To do this, a set of 28 active variables was used. The list of variables and their modalities can be found in the appendix.

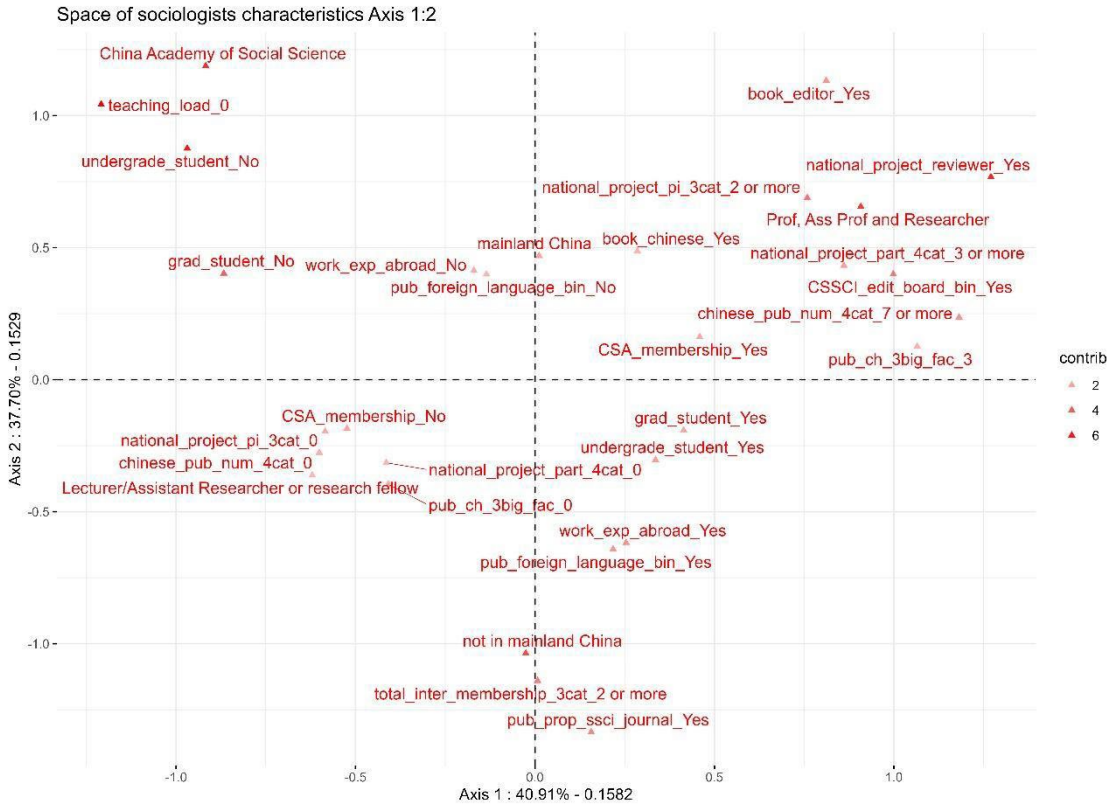
Interestingly, the space of sociologists is more differentiated in terms of the volume and structure of academic capital. The three first axes contribute to explaining 87.70% of the variance between individuals (Benzecri's modified rate). The first dimension contributes to 41.42% of the variance, the second dimension contributes to 37.70%, while the third dimension contributes more marginally to 8.58%.

To this extent, the survey adequately captures major differences between the scholars and between their different characteristics. A representation of the space of modalities is helpful in further understanding the principles of differentiation of the space.

Table 3: Axes and measurement of the explained inertia

Axis	Eigenvalue	Rate	Cumulative rate	Modified rate	Cumulated modified rate
1	0.1582	9.73	9.73	41.42%	41.42%
2	0.1529	9.40	19.14	37.71%	79.13%
3	0.0947	5.83	24.97	8.58%	87.70%
4	0.0736	4.52	29.49	3.10%	90.81%

Figure 2: Space of the sociologists' characteristics—Axis 1–2 (30 most contributing modalities)

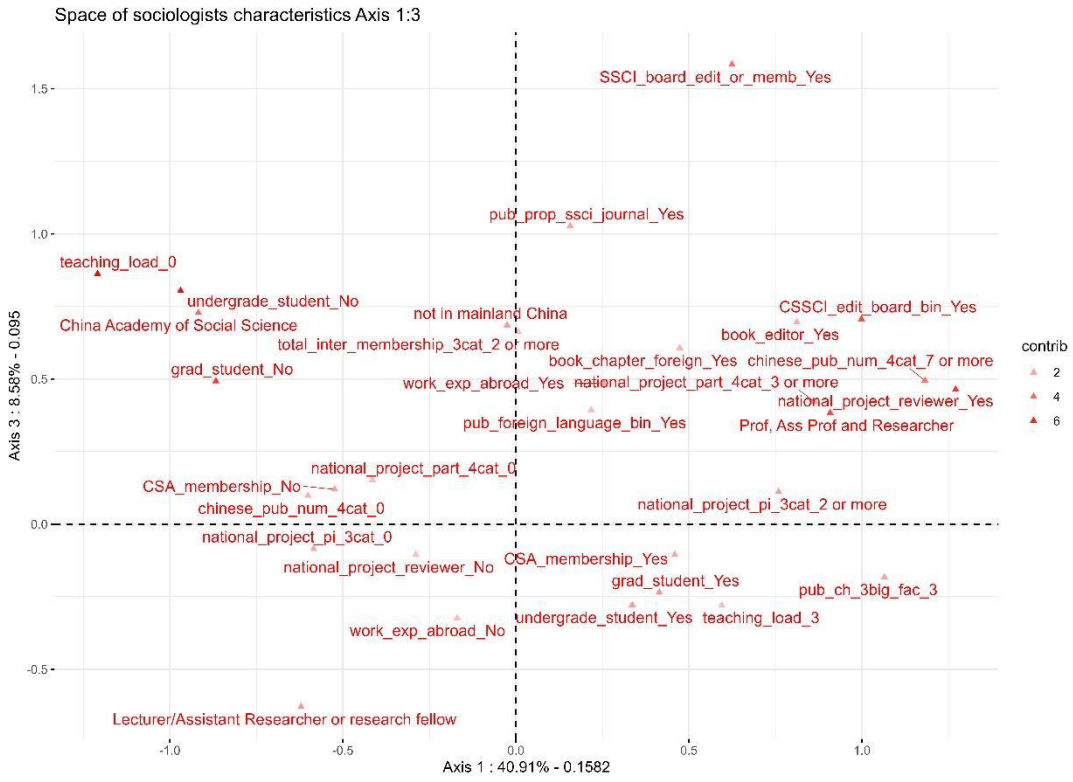


Considering the variables and modalities contributing to the inertia on Axis 1 (also reported in detail in the appendix[10]), it seems that this axis separates the gatekeepers (on the right) who control the means of production of national academic capital from the most junior scholars (on the left). Indeed, the individuals on the right possess more prominent academic capital in its statutory state ("professor, associate professor, or researcher", "national project reviewer", "Chinese SSCI board member", and "national project expert") and benefit from this status in term of teaching (lower teaching load and postgraduate student teaching and supervision). Moreover, the sociologists on the right side of

10 In the appendix, we provide the position of each modality on the three first axis of inertia together with their contribution. Other measures, such as cos2 and eta2, p-value of eta2, and v-test, were considered in our analysis to qualify each axis.

Axis 1 generally have more national academic capital, as they are more likely to have published in all three of the most-prestigious Chinese sociology journals published in the mainland and are more likely to be members of the Chinese Sociological Association. They are also more likely to have published 10 or more CSSI articles in the past three years. To sum up, this axis divides young scholars from older prominent scholars who forge their legitimacy by publishing in Chinese journals while gaining control of national academic capital.

Figure 3: Space of the sociologists' characteristics—Axis 1–3 (30 most contributing modalities)



Axis 2 differentiates those who possess international academic capital from those who are more oriented toward the production of official reports. Indeed, among the variables that contribute to the upper part of Axis 2, we find "having a working experience abroad", "published in proposed SSCI journal", "published in SSCI journals in the past three years", and "PhD obtained: not in Mainland China". The lower part of Axis 2 is characterised by "blue book chapter",[11] "Chinese Academy of Social Sciences", "no teaching load", and "book editor", which correspond to the profile of CASS researchers who have no teaching duties but are asked to publish public (i.e., chapters in "blue books") and confidential reports (*neibu baogao*) on ongoing reforms. In terms of contribution to inertia, this aspect is almost as distinctive as academic capital in its statutory state (mainly retrieved on Axis 1).

The volume of capital is illustrated by Axis 3. Sociologists located on the upper part of Axis 3 are better equipped in terms of not only international academic capital but also national academic capital. For instance, the variable "SSCI editorial board member or editor" particularly contributes to

11 Blue books are a series of books edited by the CASS. They can be considered as the public version of the reports on ongoing policies and topics that are highly regarded by the political leadership.

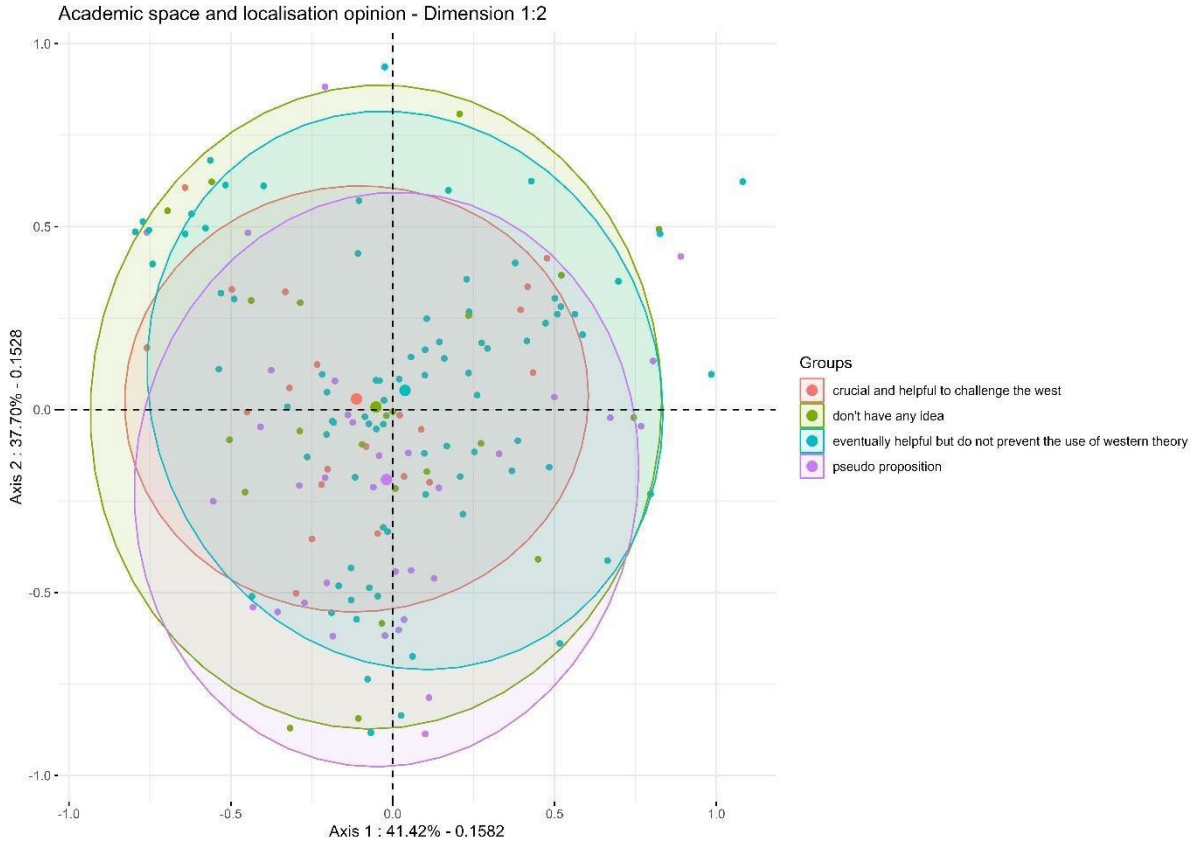
the inertia on this axis. Meanwhile, other aspects, such as "national project expert", "CSSI editorial board member", and "participated in only one national project", also contribute heavily to Axis 3, albeit to a lesser extent. Therefore, Axis 3 may separate those who have a double strategy of accumulation of academic capital (international and national) from those who are leaning more toward one of the two sources.

Since the properties of the academic space have been made clear, we can investigate in the next section how sociologists' opinions on the localisation of social sciences are distributed in this space. Moreover, we shed light on the absence of structural homology, while explaining why some of the polarising opinions, such as "localisation is a pseudo proposition" and "crucial and helpful in challenging Western academic hegemony", are still tendentially associated with specific characteristics and positions in the space.

DEBUNKING THE PRE-SUPPOSED STRUCTURAL HOMOLOGY

To gain an idea of the distribution of the different opinions in the space of sociologists, we first mapped the different groups of respondents and underlined the occupied areas by drawing confidence ellipses for each group.

Figure 4: Projections of opinions on the localisation on the space of sociologists—Axis 1–2



The ellipses contain 85% of the sociologists, who agree on one of the proposed answers with the exception of the modality "Other; please be precise" (only four respondents). As visualised, the area covered by the ellipses on the first axis (left to right) seems similar. However, the coordinates of the

barycentre of each group on Axis 1 are close to the origin. This suggests that the different opinions do not depend on Axis 1, which separates the prominent older scholars (professors or researchers) who are well equipped in national capital from the young scholars who have no major achievements (publications in the most prestigious Chinese sociological journals, national grants, etc.) and no responsibilities (e.g., being a board member of a CSSCI journal).

To further confirm the absence of an association between academic capital in its statutory form and attitudes toward the localisation of social sciences, we conducted the typicality test recommended by Le Roux; Biénaise and Durand (2019) and the Wilcoxon rank test, which fitted the nonparametric nature of the distribution of individuals along Axis 1. Given the value of the aka eta-squared of the "localisation opinion" variable on Dimension 1 (3.8%), the results of the typicality test,[12] and the results of the Wilcoxon Rank test (all being nonsignificant for all opinions), a remarkable absence of difference on this axis is noted. In other words, the positions of gatekeeper and marginal contributors are not correlated with an affinity for a particular opinion.

On the contrary, Axis 2, which reflects the opposition between international academic capital and CASS researchers who have no teaching duties and produce reports for public institutions, is associated to a limited extent with opinions on localisation. To return to the ellipses of confidence shown in the previous graph, those assimilating the localisation/sinicisation of social sciences to a "pseudo debate" are more likely to be in the lower part of the graph, especially when compared to those replying that localisation/sinicisation is "eventually helpful but does not prevent the use of Western theories" and those responding that localisation/sinicisation is "crucial and helpful in challenging Western academic hegemony". From a statistical point of view, the opinions taken together are poorly correlated with Dimension 2, as the aka eta-squared is only 5.8%. Nonetheless, the p-value of the typicality test of the modality "pseudo proposition" (0.0034) and the p-value of the Wilcoxon rank test, which compared the individuals responding "pseudo proposition" with those responding "eventually helpful but does not prevent the use of Western theories" and those responding "crucial and helpful in challenging Western academic hegemony" (0.009 and 0.002), confirm that those who are better equipped in academic capital originating from international sources are more likely to consider the localisation/sinicisation of social sciences as a pseudo debate.

For instance, half the sociologists considering localisation/sinicisation as a pseudo debate had published an SSCI-indexed article over the past three years, while this proportion was only 36.4% for the other attitudes. Additionally, 14.7% of sociologists considering localisation/sinicisation as a pseudo debate are board members or editors of at least one SSCI journal. This proportion shrinks to 5.9% for those who consider localisation as potentially helpful but not preventing them from using Western theories and methods and to 0% for those who declare localisation/sinicisation as crucial and helpful in challenging Western hegemony.

Finally, it appears that international association memberships are strongly correlated with opinions on the localisation debate. Indeed, 59% of sociologists considering localisation/sinicisation as a pseudo debate have at least one international or foreign national association membership compared to only 40.6% of those expressing one of the other attitudes. More interestingly, none of the scholars considering localisation/sinicisation as crucial and helpful in challenging Western hegemony are members of the International Sociological Association, and only one is a member of the American

12 More precisely, on Axis 1, the results are as follows: p = 0.17 for the "crucial and helpful in challenging Western academic hegemony", p = 0.50 for "don't have any idea", p = 0.21 for "eventually helpful but does not prevent the use of Western theory", and p = 0.75 for "pseudo proposition".

Sociological Association. In short, everything happens as if the accumulation of international academic capital is functioning to strongly defend the universality of the methods and concepts invented by Western sociologists. Conversely, the sociologists dissociating themselves the most from the international space of knowledge production are more prone to perceive localisation/sinicisation as an academic and political tool to challenge the West.

The relationship between international academic capital (Axis 2) and other attitudes is slightly more challenging to interpret. Among those who declare "not having any idea", we find a group of five individuals who graduated abroad, have at least one international membership, and have published in an SCCI-indexed journal during the past three years. It remains possible that these relatively young individuals, who are equipped in international academic capital, are unfamiliar with the ongoing debates or answered "not having any idea" to avoid mentioning a potentially divisive opinion given the political nature of the debate.

Another subgroup of sociologists who declare that they have no opinion is composed of six individuals located in the bottom-right part of the graph, which means that they are not gatekeepers and are poor in international academic capital. These marginalised scholars may continue their academic journeys without paying much attention to the ongoing debates, having not truly considered this matter. In short, the heterogeneity of the "having no idea" position possibly arises from the fact that these sociologists are choosing the same pre-coded answer, while not necessarily having the exact same reason to select it.

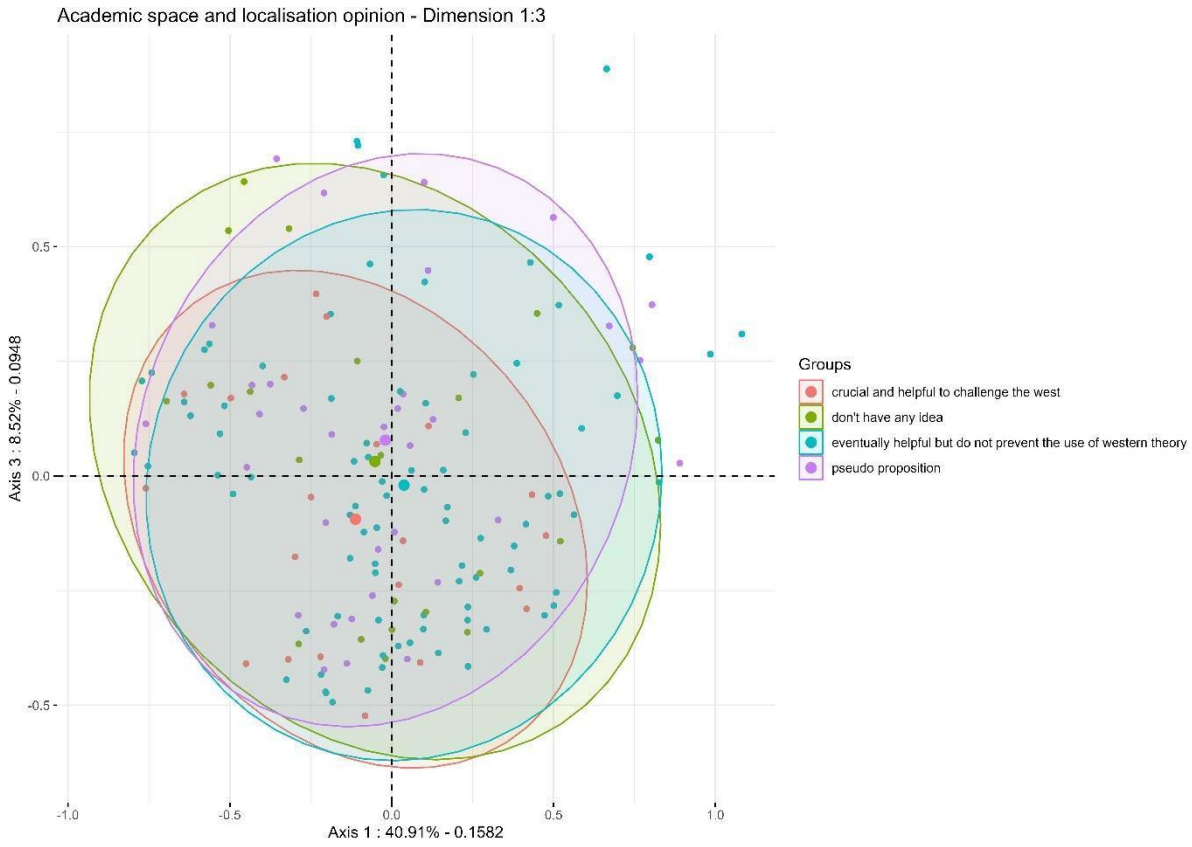
The extremely heterogeneous position (characterised by the large square sum of the distance between the individuals composing that group) of sociologists declaring that localisation/sinicisation is "eventually helpful but does not prevent the use of Western theories and methods" can be related to their academic trajectories. As explained earlier, those sociologists who trained in China are nevertheless familiar with European and American sociology, while most sociologists who obtained their PhD outside Mainland China became familiar with authors such as Fei Xiaotong during their bachelor's degree. Therefore, Chinese and Western authors and theories are employed in combination or alternatively by most of the sociologists whatever their position in the space. They all have an objective interest in continuing to draw on this multi-polar world of intellectual references.

Chinese sociologists are also the depositaries of an intellectual tradition beginning with Liang Qichao, which aims to combine "Western modernity" with "Chinese specificities". This encourages the sociologists to play the role of knowledge brokers between the different societies that they have to know to be regarded as true intellectuals. Put differently, the *doxa* among the sociologists is no different from the doxa among Chinese intellectuals: the desirable state of science and society arises from a synthesis of Chinese traditions and Western modernity.

At this point, our investigation clearly demonstrates that the position of gatekeepers versus newcomers is not associated with a particular opinion, while the origin of academic capital (national vs. international) only correlates with the attitudes categorised as "pseudo debate" and "crucial and helpful in challenging Western academic hegemony". Investigation of the position of individuals on Axis 3, which is more related to the global volume of academic capital, further establishes the absence of

structural homology, while helping to better understand how the polarising opinions relate to positions in the space.

Figure 5:Projection of opinions on the localisation debate on the space of sociologists—Axis 1–3.



In Figure 5, the barycentre of the ellipses corresponding to “pseudo proposition” is positioned marginally higher than the barycentre of the ellipses corresponding to “eventually helpful and does not prevent the use of Western theories” and “crucial and helpful in challenging Western academic hegemony”. Since the upper part of Axis 3 corresponds to the volume of academic capital (with the modalities “being an editor of an SSCI journal”, “being a board member of an SSCI journal”, and “being an expert for the national social science grant foundation” greatly contributing to the explained inertia), we may temporarily assume that those having a larger volume of academic capital (by accumulating both national and international academic capital) are more likely to perceive localisation/sinicisation as a pseudo debate. However, neither the Wilcoxon rank test (p-value = 0.051) nor the typicality test (p-value = 0.074) validate this hypothesis with great certainty.

Moreover, these results must be considered cautiously because of the modalities that contribute greatly to Axis 3. Indeed, modalities such as “being a member of an SSCI journal editorial board” and to a lesser extent “contribution to a book chapter written in a foreign language” are related to international academic capital. In other words, on Axis 3, the position of sociologists considering localisation as a pseudo debate is partly due to their accumulated international academic capital.

Because other important modalities relating to national academic capital are in play (principal investigator of three or more projects and participating in four or more projects led by other scholars), further inspection of the most crucial aspects of national academic capital is necessary. Therefore,

we conducted additional bi-variate analyses to examine the most crucial aspects of national academic capital. In this regard, the most relevant variable is "in how many of the three main Chinese sociology journals did the sociologist ever publish?" These journals—*Shehuixue yanjiu* (Sociological Research), *Shehui* (Society), and *Shehui Xue Pinglun* (Sociological Debates)—are more valued in Chinese sociologists' evaluations for continuous appointments and promotions. For instance, in most of the departments using the tenure system, a publication in one of these three journals in the last five to six years is deemed necessary to obtain tenure (this can be replaced by a publication in an SSCI journal in Q1). Put differently, more than any other aspect, publication in these journals indicates the possession of national academic capital and can be exchanged for academic capital in its statutory form.

Interestingly, only 42.9% of scholars who find localisation to be "helpful in challenging Western academic hegemony" have ever published in one of these three journals. This proportion reaches 47.1% for those who consider localisation as a pseudo debate and who are more likely to publish in SSCI journals. Meanwhile, this proportion is 75.9% for those who respond "eventually helpful but does not prevent the use of Western theory". In other words, the sociologists who respond "helpful in challenging Western academic hegemony" lack the most important resource to secure their careers, especially if we compare them to those who advocate for a fusion of so-called Western and Chinese knowledge.

Table 4: Number of national grants as principal investigator obtained during the career vs. opinion on the localisation of social sciences.

			Number of national grants as principal investigator obtained during the career			
			0	1	2 or more	Total
Opinion on localisation	Crucial and helpful in challenging Western academic hegemony	frequency	10	9	2	21
		row %	47.6%	42.9%	9.5%	100.0%
	Pseudo proposition	frequency	15	15	4	85
		row %	44.1%	44.1%	11.8%	100.0%
	Eventually helpful but does not prevent the use of Western theory	frequency	24	36	25	
		row %	28.2%	42.4%	29.4%	
	Do not have any idea	frequency	7	10	6	23
		row %	30.4%	43.5%	26.1%	100.0%
	Other	frequency	1	1	2	4
		row %	25.0%	25.0%	50.0%	100.0%

Cramer's V = 0.171, chi-square = 9.784

To complete the analysis of national academic capital in its objectivised state, we performed another analysis of the number of national grants obtained over the sociologists' entire careers. Once again, we considered this aspect because national grants are deemed necessary for tenure and promotion in the most prestigious sociology departments, while a provincial-level research grant can be sufficient in less prestigious ones.

While the overall correlation is not exceptionally strong here (Cramer's V being below 0.2 and the chi-square not being relevant given the low frequencies in certain cells), there are still significant local deviations (measured thanks to the local PEM).[13] For instance, those declaring that localisation is "eventually helpful but does not prevent the use of Western theory" and those "not having any idea" are over-represented in the category "two or more national grants". Once again, the sociologists willing to consider localisation as a means to challenge Western hegemony appear in a particularly weak position in terms of national academic capital. Nearly half have never secured a national grant, as is the case for those considering localisation as a pseudo debate.

To sum up, this demonstrates that those who want to develop localisation to challenge the West are generally dominated on both sides. They are dominated by those who support the addition of national and Western methods and who publish in top Chinese journals and are more likely to obtain two or more national research grants (and less likely to have never obtained such a grant). They are also outperformed by those who disregard localisation and have accumulated more international academic capital. To this extent, the willingness to support localisation to challenge the West can be viewed as a heterodox opinion, arising from a dominated position in the space and led by a practical interest in changing the rules of the game of academic recognition.

DISCUSSION AND CONCLUSION

In their research, Li et al., (2022) concluded that the localisation of social sciences correlates with a high amount of political and academic capital. However, their results may well be biased due to the sociologists selected in their study and the absence of adequate empirical inquiry.

Our results based on a representative sample of the population of Chinese sociologists invite a completely different conclusion. While the present article does not address the question of political capital, we demonstrate that sociologists willing to support the localisation of social sciences to challenge the West generally lack international academic capital compared to those combating localisation. Moreover, they have accumulated less national academic capital in its objectivised state compared to those advocating for the development of localisation alongside the use of Western theories. To use Bourdieu's term, it seems that supporting localisation to challenge Western hegemony is more of a subversive strategy that is employed by dominated sociologists.

Nonetheless, it can be acknowledged that this strategy may have been adopted by some influential sociologists, such as Li Peilin and Li Youmei, who have in common being trained in France (which is not very profitable, as highly valued SSCI publications suppose the mastery of English) and being highly committed to their administrative and political duties (to the point that some sociologists compared them to eunuchs during informal discussions). For these two scholars, their position (which is both political and academic) might encourage them to be more responsive to the demands of the political leadership.

Despite the elective affinity between sociologists who are *dominated on both sides* and the willingness to challenge Western hegemony, it should be recalled that there is no one-to-one correspondence between position in the space and attitude toward localisation. Indeed, Chinese sociologists generally rely on both theories and consider the development of indigenous theories to be compatible

13 An analysis in terms of the odds ratio, complemented by the Fisher test, would have led to the same interpretation. PEM stands for Percentage of Maximum Deviation.

with the utilisation of Western sociological theories and methods. Put differently, there is no structural homology between the volume and structure of academic capital: on the one hand, there is a consensus; on the other hand, there are certain polarised attitudes that are more correlated with positions in the academic space. In Bourdieu's words, such a phenomenon might be named a limited *field effect*. However, it cannot be named as such in the present configuration. In China, sociologists co-survive in a bi-nomos space rather than in a field. Indeed, they are not embedded in a field because a field supposes a high degree of autonomy (Bourdieu 1993; Duval 2022: 76) and polarisation toward a single goal: the production of scientific knowledge. This perspective is not absent from China, but it is combined with the goal of producing knowledge that helps the leaders govern society. To this extent, in Weber's footsteps, it is safer to describe the distribution of the polarising opinions on the localisation debate as resulting from an *elective affinity*.

The impossibility of organising Chinese sociology as a field may partly explain why most Chinese sociologists tend to favour a compromise in the localisation debate, while others have no opinion (or do not dare to formulate it). In such a configuration, sociologists might develop the tendency to seek compromise or, to use a boxing metaphor, to prefer the stick jab to the power jab. This consensual strategy probably explains why a prominent sociologist such as Li Lulu, professor at the People's University of China, has declared that the localisation problem no longer exists since this question has become purely academic and sociologists are solely concerned with the production of knowledge (Zhou 2021: 323).

More importantly, most Chinese sociologists, especially the most prominent, would lose a lot of their academic legitimacy if they solely relied on theories, methods, and concepts invented by Chinese sociologists. As we demonstrated in our preliminary analysis, Chinese sociologists rely heavily on theories invented in other countries. Even those trained in China are familiar with classical sociological thinkers such as Weber, Durkheim, and Marx, who form the inevitable trident of authors taught in classical sociological theory courses. Chinese sociologists entirely trained in China are also extensively familiar with Bourdieu and other major Western contemporary sociologists. Conversely, Chinese sociologists who obtained their PhD abroad do not throw away Fei Xiaotong's and other Chinese sociologists' books after crossing the Chinese border. While they advocate less for familiarity with Chinese authors, these authors are still part of their intellectual world and background, especially after these returnees are further socialised through colleagues who completed their training entirely in China.

What seems even more interesting to us is the unwillingness of Chinese sociologists to adopt the views of political leaders enjoining them to transform the social sciences into a discursive weapon to challenge the West and defend the *Chinese model*.[14] Only 12.6% of sociologists adopt the idea that localisation is a crucial tool for challenging Western hegemony, although this idea has been established as a national goal. This undoubtedly suggests a strong willingness to see the space as more

14 The China model, sometimes named the China experience or China's development model in Xi Jinping's thought, is presented by the political leadership as a model of rapid economic development that considers the interests of the people. According to the propaganda, this model leads to "common prosperity", "harmonises the relationship between nature and humans while proceeding to modernisation", and relies on the harmonisation and unification of the Chinese people. More importantly, this model is achieved thanks to the leadership of the CCP and following the principles of socialism with Chinese characteristics. In short, it is presented as a model of development forged thanks to experimentation led by the CCP and avoiding the problems inherent in neoliberal societies, such as poverty, division, and the destruction of the environment.

governed by academic considerations and as involving a silent rebellion against the politicisation of social sciences.

This phenomenon has been named *Eigensinn* by Alf Ludtke (Lindenberg and Lüdtke 2020). More precisely, Lüdkte's concept conveys the idea that politically dominated social agents, who can hardly express discontent or organise collectively in a public manner, still adopt self-affirmation strategies to put at a distance political injunctions or hierarchical demands. This concept partly derives from (but cannot be reduced to) its literal meaning in the German language: "stubbornness" and "obstinacy". In the case of Chinese sociologists, *Eigensinn* takes the form of reclaiming the definition of the craft of sociologists, as well as being a discrete refusal to play the role of an ideological standard bearer.

This phenomenon is, of course, not specific to Chinese sociologists or, more broadly, to Chinese experts. Indeed, academic colleagues and parents' organisations in Florida, USA, are currently implementing work-around strategies to circumvent Ron DeSantis's Stop Woke Act, which deters them from explaining the history of racial inequality in the US and how sexual orientation is socially constructed and stigmatised.[15] While their resistance to De Santis's ideology and law is still sometimes organised through nongovernmental associations, the dissolution of some of some of these organisations may lead to less publicly assumed positions and a certain form of "obedient autonomy" (Evasdottir 2004), which has been practiced for years by Chinese experts.

Therefore, the silent but obstinate reluctance of Chinese experts to contribute to the creation of an ideologically grounded discursive system in, and thanks to science, can be regarded as a more common form of response to the authoritarian empowerment of experts, which offers, unfortunately for social scientists, many occasions for documentation (Blois 2021; Hager 1949; Mobius 2021; Zaslavsky 1977; Yan and Cao 2017; Hao and Guo 2016).

Despite the large existing literature on this matter, the present research not only provides information about the situation of Chinese sociologists but also offers some refined conceptual tools to construct and investigate the relation between positions and opinions in the academic space. More precisely, it demonstrates the necessity of distinguishing different forms of academic capital and conceptualising them according to the peculiarities of the sociohistorical configuration that is under scrutiny. Moreover, as the Chinese case demonstrates, there is not always structural homology between positions and opinions; instead, there is sometimes an elective affinity between certain forms or origins of capital and opinions. Such crucial nuances could not become apparent and intelligible if we were not able to enhance Bourdieu's "intellectual machine" (Bourdieu 2023: 113), as he himself wished, by proposing a refined definition of the different forms of academic capital that is suited to our sociohistorical configuration. To continue to improve such an "intellectual machine" and to understand why structural homologies are not always observed, it would also be necessary to investigate more microscopic aspects of the careers of social agents. In our case, Chinese sociologists possibly partly inherit their opinions on localisation from their PhD supervisors, who can play the role of an "intellectual master" and guardian angel.[16] Moreover, the network of interpersonal relations,

15 See the following website: These small acts of defiance are helping Floridians fight Ron DeSantis' oppressive policies (fastcompany.com).

16 Among the sociologists we interviewed, one mentioned a PhD supervisor supporting his former student to buy an apartment. Another consulted his former PhD supervisor about life partner choices. Many others mentioned still working with their former PhD supervisors or people from their PhD supervisors' groups (shimen in Chinese) more than one decade after graduation. These anecdotes invite us to consider how the strong and long-lasting intellectual ties between supervisors

which is potentially a network for the exchange of ideas and favours, may invite sociologists to reconsider their opinions about the localisation of social sciences. Finally, to paraphrase Passeron (2009: 23–35), an academic journey is also made up of the contingent dynamics of events and bifurcations that make social agents' opinions not totally reducible to a structural homology between their positions and opinions.

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and students extend beyond the professional sphere and may also play a role in sociologists' opinions on the localisation of the social sciences.

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APPENDIX

Table A1: Active Variables MCA first three dimensions (contr. ≥ 2,1 % in italic)

Variables	**n and (%)**	**Dim 1**	**Dim 2**	**Dim 3**	**contr. dim 1**	**contr. dim 2**	**contr. dim 3**
Statutory state of the academic capital							
Type of academic institution employed (institution_3cat)							
211 university (not 985)	39 (23.4%)	0,04	-0,25	-0,47	0,01	0,41	*2,27*
985 project	99 (59.3%)	0,25	-0,25	-0,03	0,99	1	0,02
China Academy of Social Sciences (local or national)	29 (17.4%)	-0,92	1,19	0,73	*3,85*	*6,69*	*4,05*
Job position (Job_position)							
Assistant Researcher/Assistant professor	85 (50.9%)	-0,28	-0,24	0,02	1,07	0,78	0,01
Prof, Ass Prof and Researcher	49 (29.3%)	0,91	0,66	0,38	*6,37*	*3,44*	1,9
Lecturer/Assistant Researcher or research fellow	33 (19.8%)	-0,62	-0,36	-0,63	2,01	0,71	*3,44*
Reviewer National Social Sciences Foundation (national_project_reviewer)							
Yes	31 (18.6%)	-0,29	-0,17	-0,11	*2,01*	0,71	*3,44*
No	136 (81.4%)	1,27	0,77	0,46	1,8	0,68	0,4
CSSCI editorial board member (CSSCI_edit_board_bin)							
Yes	32 (19.2%)	1	0,4	0,71	*5,03*	0,83	*4,21*
No	135 (80.8%)	-0,24	-0,09	-0,17	1,19	0,2	1
SSCI editorial board member or editor (SSCI_edit_or_board)							
Yes	13 (7.8%)	0,62	-0,24	1,58	0,8	0,12	*8,59*
No	154 (92.2%)	-0,05	0,02	-0,13	0,07	0,01	0,72
Working experience abroad (work_exp_abroad)							
Yes	67 (40.1%)	0,25	-0,62	0,48	0,68	*4,17*	*4,14*
No	100 (59.9%)	-0,17	0,41	-0,32	0,46	*2,8*	2,77
Experience as Postdoc (Post_doc)							
Yes	44 (26.3%)	0	-0,12	0,13	0	0,1	0,19
No	123 (73.7%)	0	0,04	-0,05	0	0,04	0,07
Place of highest diploma (high_diploma_place_bin)							
In mainland China	115 (68.9%)	0,01	0,47	-0,31	0	*4,12*	*2,91*
Not In Mainland China	52 (31.1%)	-0,03	-1,04	0,69	0,01	*9,12*	*6,43*
Objectivised state of the academic capital							
Participation in national project of other PI (national_project_part_4cat)							

0	66 (39.5%)	-0,41	-0,31	0,15	1,79	1,07	0,4
1	44 (26.3)	-0,02	0,03	-0,48	0	0,01	*2,67*
2	29 (17.4%)	0,15	0,26	-0,03	0,1	0,31	0,01
3 or more	28 (16.8%)	0,86	0,43	0,42	*3,27*	0,85	1,32
Number of national project led (national_project_pi_3cat)							
0	57 (34.1%)	-0,58	-0,2	-0,09	*3,07*	0,36	0,11
1	71 (42.5%)	0,05	-0,22	0,01	0,03	0,57	0
2 or more	39 (23.4%)	0,76	0,69	0,11	*3,54*	*3,02*	0,13
Published books in Chinese (book_chinese)							
Yes	56 (33.5%)	0,29	0,49	0,03	0,72	*2,17*	0,01
No	111 (66.5%)	-0,14	-0,25	-0,02	0,36	1,1	0,01
Published chapter in blue book collection (blue_book_chapter)							
Yes	31 (18.6%)	-0,22	0,67	0,23	0,25	*2,24*	0,43
No	136 (81.4%)	0,05	-0,15	-0,05	0,06	0,51	0,1
Book editor (chinese book)							
Yes	15 (9.0%)	0,81	1,13	0,7	1,56	*3,14*	1,92
No	152 (91.0%)	-0,08	-0,11	-0,07	0,15	0,31	0,19
Published in listed SSCI journals (pub_prop_ssci_journal)							
Yes	19 (11.4%)	0,16	-1,33	1,03	0,07	*5,53*	*5,28*
No	148 (88.6%)	-0,02	0,17	-0,13	0,01	0,71	0,68
Published in foreign language (pub_foreign_language_bin)							
Yes	64 (38.3%)	0,16	-1,33	1,03	0,48	*4,31*	*2,6*
No	103 (61.7%)	-0,14	0,4	-0,24	0,3	*2,68*	1,62
Published a book chapter in foreign language (book_chapter_foreign)							
Yes	27 (16.2%)	0,22	-0,64	0,39	0,96	0,95	*2,62*
No	140 (83.6%)	-0,14	0,4	-0,24	0,18	0,18	0,5
Number of CSSCI articles (chinese_pub_num_4cat)							
0	45 (26.9%)	-0,6	-0,28	0,1	*2,56*	0,56	0,11
1	22 (13.2%)	-0,13	-0,21	-0,17	0,06	0,16	0,16
2	31 (18.6%)	-0,22	-0,02	-0,39	0,25	0	1,21
3	26 (15.6%)	-0,1	0,3	0,16	0,04	0,37	0,17
4 to 6	22 (13.2%)	0,67	0,24	-0,15	1,57	0,2	0,13
7 or more	21 (12.6%)	1,18	0,24	0,49	*4,63*	0,19	1,35

Publications in 3 dominant journals Chinese sociology (pub_ch_3bigfac)							
0 (didn't publish in any of them)	71 (42.5%)	-0,41	-0,4	0	1,87	1,82	0
1 (published in one of them)	46 (27.5%)	-0,06	0,27	0,18	0,03	0,54	0,39
2 (published in two of them)	33 (19.8%)	0,42	0,41	-0,16	0,92	0,92	0,23
3 (published in the three journals)	17 (10.2%)	1,07	0,13	-0,18	*3,04*	0,04	0,15
Pedagogical state of the academic capital							
Annual Teaching load (teaching_load)							
No course	32 (19.2%)	-1,21	1,04	0,86	*7,38*	*5,68*	*6,26*
1 course	26 (15.6%)	-0,23	-0,33	0,14	0,23	0,48	0,13
2 courses	53 (31.7%)	0,34	-0,08	-0,31	0,96	0,06	1,32
3 courses	32 (19.2%)	0,6	-0,11	-0,28	1,79	0,06	0,66
4 or more	24 (14.4%)	0,32	-0,7	-0,24	0,4	1,94	0,37
Teaching to undergraduate students (undergraduate_students)							
Yes	124 (74.3%)	0,34	-0,3	-0,28	*2,21*	1,87	*2,54*
No	43 (25.7%)	-0,97	0,88	0,8	*6,37*	*5,39*	*7,32*
Teaching to postgraduate students (grad_students)							
Yes	113 (67.7%)	0,41	-0,19	-0,24	*3,06*	0,68	1,65
No	54 (32.3%)	-0,87	0,4	0,49	*6,4*	1,43	*3,45*
Socialised state of the academic capital							
Member Chinese Sociological Association (CSA_membership)							
Yes	89 (53.3%)	0,46	0,16	-0,1	*2,96*	0,38	0,26
No	78 (46.7%)	-0,52	-0,18	0,12	*3,38*	0,43	0,29
Number memberships other national or international associations (total_inter_membership_4cat)							
0	93 (55.7%)	-0,12	0,38	-0,18	0,21	*2,24*	0,79
1	45 (26.9%)	0,24	-0,06	-0,06	0,42	0,03	0,04
2 or more	29 (17.4%)	0,01	-1,14	0,66	0	*6,16*	*3,35*
Collaboration with NGO for research (collaboration_ngo)							
Yes	25 (15.0%)	0,15	-0,19	0,53	0,09	0,15	1,85
No	142 (85.0%)	-0,03	0,03	-0,09	0,02	0,03	0,33

Table A2: Passive variables

Other variables - not active	**N and (%)**
Employed research methods (research method)	
Qualitative	79 (47.3%)
Quantitative	49 (29.3%)
Mixed methods	39 (23.4%)
Opinion on the improvement of chinese sociology (chinese_soc_improv)	
fundamental research credit	6 (3.6%)
Localisation of social sciences	23 (13.8%)
professional standards	22 (13.1%)
researcher autonomy	75 (44.9%)
secure career	39 (23.4%)
Other	2 (1.2%)
Opinion on the localisation (localisation)	
crucial and helpful to challenge the west	21 (12.6%)
eventually helpful but do not prevent the use of western theory	85 (50.9%)
pseudo proposition	34 (20.4%)
Don't have any idea	23 (13.8%)
Other	4 (2.4%)

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ARTICLE

Assembling Regional Expertise: Co-creation Workshops in EU-funded Research on the German Coal Phase-out

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Abstract

Regional expertise is a crucial aspect of effective and legitimate policy formulation within the multilevel governance system of the European Union (EU). We argue that EU-funded research contributes to the multilevel network governance in the EU. However, until now, this aspect of expertise formation has undergone little analysis. Drawing on materials from an EU-funded research project on energy transition processes as well as our participation in this project as contracted researchers, we investigate how regional expertise is assembled in co-creation workshops of scientists and regional stakeholders. We first reconstruct the implicit programme theory of transformative research which emphasises the value of practical knowledge of (regional) stakeholders in complementing scientific knowledge for governance purposes. Using our analysis of the interactions between workshop participants, we point out the benefits and challenges of assembling regional expertise from heterogeneous actors. Finally, we compare this hybrid mode of knowledge co-creation with (hypothetical) more segregated forms of expertise.

Keywords

Mode 2, academic knowledge, practical knowledge, socially robust knowledge, multilevel governance

INTRODUCTION

Some decades ago, scientists were divided in their assessment of whether society was able to adapt to the environmental risks that emerged as externalities of human activity on earth (Beck 1992; Luhmann 1989). The different logics of the functional subsystems of society, such as science and politics, were regarded as being major obstacles (Luhmann 1989). It is the task of expertise to bridge these logics by assembling reliable knowledge for collective decision-making. While politics has relied on academic expertise for many years, this relationship has recently become closer, but also more contentious, transforming both the production of knowledge as well as the governance

structures. For several decades, policy advice and expertise have been provided by think tanks operating at the interface between science and policy (Gellner 1995; Weaver 1989). The European Union (EU) has become especially known for relying on expertise when formulating its policies (Radaelli 1999). Yet, this proliferation of expertise seems to have also contributed to the current crisis in which policy knowledge is needed but is also mistrusted (Bechmann 2003; Eyal and Medvetz 2023). In the EU, this paradox has been addressed by structural changes to knowledge production as well as new forms of multilevel governance in networks (Börzel and Heard-Lauréote 2009). However, within these networks, regions have had extremely diverse and often (only) informal influence (Bauer and Börzel 2010). This paper focuses on the hybridisation of knowledge production for governance purposes in an EU research project, which contributes, at the same time, to the creation of policy networks among regional stakeholders.

One structural change in scientific knowledge production results from the framing of funding programmes by formulating so-called "grand challenges" (Isakova et al., 2024; Kaldewey 2018), such as climate change, and the uncertainties these represent for policymaking (Ruser 2018). The aim is to align research questions with the knowledge demands of policy formulation from the outset. The EU's Horizon 2020 funding programme, for example, encourages grant proposals oriented toward policy goals (Gengnagel et al., 2022; Kim and Yoo 2019), such as climate mitigation research (Boezeman and Coninck 2018).

Furthermore, in response to the perceived gap between science and policy, the production of knowledge has also been transformed from within science. The concept of a Mode 2 society, for example, assumes that the production of scientific knowledge becomes more integrated with society to produce "socially robust knowledge" (Nowotny 2003; Nowotny et al., 2001), i.e., knowledge that is scientifically reliable as well as socially accepted by potential users. Helga Nowotny (2003) suggested three possible strategies for creating such knowledge: widening the circle of those who are considered legitimate contributors to knowledge production; experimenting with methods that account for the uncertainties implied in knowledge production; and repeatedly testing and modifying knowledge. Yet, "socially robust knowledge" has been criticised as being a term that is more programmatic than analytical (Weingart 2008). This paper, in contrast, turns to the empirical example of a research project which aimed to produce such knowledge, and explores some benefits and difficulties that arise in pursuing this objective. We analyse a project from the EU Horizon 2020 programme in which, following a transformative science approach, co-creation workshops made up of scientists and regional stakeholders were designed to produce "socially robust" policy recommendations based on the scientific and practical knowledge of the region.

The co-creation workshops from the ENTRANCES (ENergy TRANsitions from Coal and carbon: Effects on Societies) project are exemplary in highlighting the hybridisation of knowledge production at the interface between science and policy because they embody both a theoretical and methodological approach to research. Some of the recently defined transdisciplinary scientific approaches are regarded as playing a particular role in contributing to the responses to societal challenges (Renn 2021) and hence aim to restore academic as well as political agency. Among those, transformative research (Schneidewind et al., 2016; Strohschneider 2014; Trevors et al., 2012) is an approach which strives to create "socially robust" knowledge that can be translated into policy formulation, implementation and, consequently, social change. This catalytic understanding of research deviates from the assumption that objectified scientific knowledge is necessarily superior to less universal forms of knowledge. Regarding regions as places with unique properties, for instance formal academic knowledge, is incomplete because it lacks the practical knowledge base of

regional actors (Groves 2017). Accordingly, deliberative methods of knowledge co-creation that integrate researchers and issue stakeholders are valuable due to their potential to create transformative knowledge based on principles of reciprocal interaction (Renn 2021). However, the rather messy empirical reality of the interaction between such heterogeneous actors has rarely been examined (e.g., Fritz et al., 2019).

We build our theoretical perspective on concepts of knowledge and expertise (Collins and Evans 2002; Epstein 2023; Scott 1998). Using these concepts heuristically, we analyse programmatic literature on transformative research in order to reconstruct the "programme theory" (Bickman 1987) of the scientific approach. Programme theory describes the assumptions that are explicitly or implicitly made in any line of organised action. The aspiration of transformative research to produce knowledge for catalysing social change neatly fits the notion of expertise, which emphasises the assemblage of knowledge for governance purposes. While some theoretical approaches would lead us to assume that certified experts and scientific knowledge are likely to dominate a setting in which researchers and practitioners interact in a co-creative task (Collins and Evans 2002; Scott 1998: 311), this would contradict the declared objective of transformative research. Others argue for a more nuanced view. The notion of lay expertise, for instance, stresses the hybrid nature of expertise marshalled by groups with members who are not formally considered experts. Lay expertise assembles heterogeneous knowledge bases, including formal and experiential knowledge, to make expertise effective in informing collective action (Epstein 2023). We conceptualise regional expertise as a specific form of lay expertise that includes formal and practical knowledge about a region, including singular regional features and particular people and networks.

EU-funded research projects offer good opportunities to study how researchers and practitioners interact to produce policy expertise. The ENTRANCES project was one of the Research and Innovation Actions (RIA) funded under the Horizon 2020 call "Building a low-carbon, climate resilient future: secure, clean and efficient energy". RIAs are intended to establish new knowledge and explore "a new or improved technology, product, process, service, or solution" (REA n.d.). The ENTRANCES project combined several theoretical frameworks but had a strong focus on transformative research. The project applied a mixed-methods case-study approach to compare thirteen European coal mining and/or carbon intensive industrial regions. The methods applied in ENTRANCES ranged from the secondary analysis of administrative data, to content analysis of media, standardised surveys, semi-structured regional stakeholder interviews, focus groups and co-creation workshops made up of regional stakeholders whose objective was to formulate policy recommendations. This paper focuses particularly on the co-creation workshops with regional stakeholders, whose lay expertise was considered crucial for formulating and implementing policy in the regional transition process following the phase-out of mining activities. The co-creation workshops embody most clearly the transformative aspiration of the project because they explicitly aimed to formulate policy recommendations. Focussing on these workshops, *we investigate the ways in which academics and regional stakeholders interact in co-creation workshops when they are expected to formulate policy recommendations for German lignite mining regions. How can the knowledge of regional stakeholders be utilised in policy formulation?* Our objective is to establish an empirical base for reflecting upon the potential benefits and problems of co-creation workshops as a research format that aims to create "socially robust knowledge" for policy recommendations.

Our empirical analysis is based on the mixed-methods case studies of the lignite mining regions Central Germany (*Mitteldeutschland*), Lusatia (*Lausitz*) and Rhineland (*Rheinland*) which were

developed within the framework of the ENTRANCES project. We participated in this project as contracted researchers and were involved in coordinating regional surveys, conducting semi-structured stakeholder interviews, and moderating focus groups and co-creation workshops. We also participated (virtually) in the project's final hybrid conference, where results were presented and discussed with representatives of the European Commission in Brussels. We performed a secondary analysis of the project materials and reflect on our participation as researchers in the project, especially our moderation of the co-creation workshops.

We begin by describing the institutional background of the research project, then outline the paper's theoretical context and methodological approach. The section headed 'The co-creation of research expertise' presents the results of our empirical analysis, and these are discussed in the subsequent section.

THE MULTILEVEL GOVERNANCE OF REGIONAL DECARBONISATION

Political and institutional work has been performed at various social levels to reduce global CO_2-emissions, as well as to mitigate negative economic and societal effects of clean energy transitions. Political aims of climate change mitigation have been formally defined at the international level, most importantly with the 2° C and 1.5° C thresholds agreed on in the Paris Agreement of 2015 (Folkers 2022:94). The document was ratified by both the EU and Germany. One important element of climate change mitigation policies is the phase-out of coal and lignite mining (David and Gross 2019).

Research on past coal mine closures identified several socio-economic and societal challenges, such as rising unemployment, the establishment of new regional identity narratives, and the creation of a sense of justice (Diluiso et al., 2021; Measham et al., 2024; Strambo et al., 2019).[1] To compensate for possible negative effects of climate change mitigation policies, the EU put in place the Just Transition Mechanism, which will run from 2021 to 2027 and is expected to mobilise 55 billion euros for the regions most affected by the transition to a climate-neutral economy (European Commission n.d.). In Germany, the Commission on Growth, Structural Change and Employment ('Coal Commission') was created in June 2018 and presented its policy recommendations in January 2019. The political process in the run-up to the formation of the Coal Commission was embedded in increasingly heated public debates. These were most contentious around the time that the commission was formed and peaked in 2019 after the commission had presented its results (Bartl et al., 2022b: 106; Markard et al., 2022: 130). The energy transition narrative became deradicalised and hegemonial in national public discourse (Buschmann and Oels 2019). However, comparative studies suggest that regional discussions in mining regions have remained more contentious than the national public debate (MDR 2022; Radtke and David 2024).

In August 2020, the German Bundestag passed three acts as part of a legislative package, which we will refer to as the Coal Phase-out Act (*Kohleausstiegsgesetz*).[2] The act aims to phase-out lignite coal production in Germany by 2038, compensate power plant companies and affected workers (over the age of 58), and initiate significant investments in mining regions (Brachert et al., 2023: 17). In this context, several scenarios were published for an earlier phase-out of lignite, some of which were

1 The following three paragraphs build on a section in Baron and Bartl (2024).

2 The legislative package consisted of the Act to Reduce and End Coal-Fired Power Generation (Gesetz zur Reduzierung und zur Beendigung der Kohleverstromung), the Coal Region Investment Act (Investitionsgesetz Kohleregionen) and the Structural Reinforcement Act for Mining Regions (Strukturstärkungsgesetz Kohleregionen).

commissioned by Germany's federal government (e.g., Luderer et al., 2021). In 2022, the state government of North Rhine-Westphalia, where the Rhineland mining area is located, and the federal government, agreed to accelerate the regional coal phase-out and complete it by as early as 2030. In contrast, the state governments of Brandenburg, Saxony and Saxony-Anhalt, responsible for the mining areas of Central Germany and Lusatia, rejected bringing forward the phase-out date to 2030, which corresponds to the opinion of the regional majority (MDR 2022, 2023).

As part of the legislative package, the Structural Reinforcement Act for Mining Regions (*Strukturstärkungsgesetz Kohleregionen*) allocated up to 14 billion euros to the lignite mining regions until 2038 for investments aimed at facilitating structural change. Furthermore, the federal government has also agreed to support the coal mining regions by providing another 26 billion euros until 2038 through its own measures (Bartl et al., 2022b: 50).

The legislative procedure was accompanied by structural development programmes created by the affected federal states themselves. These were preceded by bottom-up strategic visions (*Leitbilder*) for transforming the lignite mining regions (Heer et al., 2021) affected by the Coal Phase-Out Act (BGBl 2020: 1803–1808). Lignite mining regions, which historically have no formal boundaries, were created as spaces for policy intervention and were demarcated along county lines through the Coal Phase-Out Act (Ribbeck-Lampel et al., 2023: 35). The lignite mining regions of Central Germany and Lusatia straddle more than one federal state; therefore, new governance structures had to be set up to manage project approvals, the allocation of federal subsidies, and implementation (Heer et al., 2024: 7.). These governance structures differ considerably between regions (Barrett 2022: 46–49; Bartl et al., 2022a: 52–55; Bartl et al., 2022b: 49–52). Even though public participation in the regional governance of structural transformation processes is highly important for its acceptance (Heer 2020), opportunities have been rather limited (cf. Herberg et al., 2020; Radtke and David 2024).

Decarbonisation policies have been accompanied by research programmes at the national and EU level that strive to create expertise for policy purposes. Oriented toward climate change mitigation policy, the EU's Horizon 2020 programme has funded research that strives to generate and improve knowledge production at the science-policy interface through, e.g., "collaboration, stakeholder participation and iterative communication" (Boezeman and Coninck 2018: 1). The ENTRANCES project is an example of the coordination between policy and research at the EU level. The German phase-out policy is evaluated separately at the national level (Brachert et al., 2023).

FORMS OF EXPERTISE IN CO-CREATIVE RESEARCH

Transformative research was an important theoretical approach that informed the ENTRANCES project. While expertise is not a central concept in transformative research, we use the notion of expertise to reconstruct the co-creative methodological "programme theory" (Bickman 1987) of this approach and to analyse actual practices in co-creation workshops. The notion of expertise is especially useful for highlighting how co-creation workshops instrumentally create spaces of interaction for researchers and stakeholders, and for analysing how participants in these settings interact. In the ENTRANCES project, they were designed to form regional expertise from heterogeneous actors.

The hybridisation of expertise

Expertise is a form of assembling knowledge at the interface between science and policy. Paradoxically, governing public issues increasingly depends on expert knowledge; however, this knowledge is called into question more frequently and publicly (Büttner and Laux 2021; Eyal and Medvetz 2023). Two debates have emerged in response to this paradox: the question of participation in the formulation of expertise, and the relationship between certified academic knowledge and various forms of "alternative" knowledge (Epstein 2023). Public issues that require specialised knowledge might come under the scrutiny of societal actors who question the legitimacy of the expertise contributing to policy recommendations. One response to this critique has been to widen the circle of expertise taken into consideration when developing policy recommendations. Widening participation acknowledges that expertise is never purely technical but also normative because it (implicitly) has political consequences. A second line of critique towards formal academic knowledge questioned its technical effectivity. Several case studies showed that scientific expertise was often not enough to effectively respond to the problems at hand because it lacked a good deal of experiential knowledge about the subject (e.g., Wynne 1989: 37). Similar arguments have been put forward about the specific properties of places that are relevant for place-based policies (Groves 2017).

Both debates prompted attempts to delineate expertise on the basis of its epistemological foundation. Collins and Evans (2002), for example, distinguish between no expertise, interactional expertise and contributory expertise. At the same time, they acknowledge that relevant expertise is not necessarily grounded in formal academic knowledge but might be based on non-certified forms, variously referred to as experience, implicit knowledge and local knowledge. Concerning spatial planning, they clearly see a role for local expertise, but stress that it might nevertheless be partially or politically biased toward the location (Collins and Evans 2002: 267). Their understanding of local knowledge corresponds to the notion of place as a space with a unique identity that cannot be fully captured by formal measures (Knoblauch and Löw 2020: 274). Practical local knowledge encompasses singular features of a place including credible networks of issue stakeholders (cf. Levelt and Metze 2013). To rebalance this potential bias of local knowledge, Collins and Evans recommend combining local expertise with the knowledge of professional experts. Hence, in their view, local expertise is complementary to but also subordinate to academic knowledge, a view that is widely shared (cf. Epstein 2023: 82). James Scott, for instance, when distinguishing between academic and practical knowledge, is sceptical about combining them on equal terms: "The relation between scientific knowledge and practical knowledge is, as we shall see, part of a political struggle for institutional hegemony by experts and their institutions" (Scott 1998: 311).

Epstein's concept of lay expertise speaks to the same debate as the ideal types developed by Collins and Evans. Unlike their ideal types, lay expertise, an ostensible oxymoron, emphasises the hybrid character of knowledge situated between 'pure' experts and 'pure' lay people. Furthermore, in response to criticism that the epistemological foundation of such expertise had remained unclear, Epstein (2023: 84) adopts a definition of expertise that shifts the focus from knowing to doing: "Experts mediate between the production of knowledge and its application; they define and interpret situations; and they set priorities for action" (Grundmann 2017: 27). Furthermore, following Eyal (2013), he characterises expertise as a collective phenomenon that draws on diverse elements (Epstein 2023: 85). This emergent combination of heterogeneous elements becomes evident, for example, in stakeholder groups that lack formal credentials but draw on their subjective experiences as well as on scientific evidence to press for policy change (Rabeharisoa et al., 2014). But this combination also seems to be at play when lay expertise becomes a strategic complement to scientific knowledge as well as a legitimacy booster in transformative research.

Transformative research and the co-creation of catalytic knowledge

Transformative research aims to produce knowledge that is politically useful for changing social and economic conditions in the direction of sustainable futures, becoming itself a driver of change (Meisch 2020; WBGU 2011: 22). This requires knowledge of leverage points (Abson et al. 2017), transformative capacities (Wolfram 2016) as well as transformative interaction modes among the relevant stakeholders. Within this line of research, it is clear that neither a strong goal orientation nor well-thought-out transformation paths nor well-developed normative justifications necessarily lead to the desired outcome of sustainability transitions, as these processes are emergent and offer no guarantee of positive results (Geels and Schot 2007).

A methodological consequence of this contingency is that the recourse to formalised methods of knowledge production alone is considered insufficient (Fazey et al., 2020; Hölscher et al., 2021). Overly abstract disciplinary perspectives are often problematic in that the derived policy recommendations can be politically difficult to implement, for example, when strictly disciplinary solutions lack a regionally adapted embeddedness or when they have failed to generate the necessary local acceptance of a political action. Therefore, transformative research strives to gain insights into how societies can translate knowledge into action (Keeler et al., 2017).

Methodologically, transformative research seeks to integrate formal, academic expertise with the expertise of practitioners in co-creative research settings in order to produce knowledge that is likely to catalyse change (Pärli et al., 2022; Renn 2021; Schuck-Zöller et al., 2017). The creation of solution-oriented transformative knowledge that translates into implementable policies is assumed to be facilitated by minimising possible hierarchies between academic knowledge and the expertise of regional stakeholders. Such transdisciplinary research settings have rarely been analysed empirically (Fritz et al., 2019). Therefore, it is useful to investigate the extent to which such a level playing field can be achieved.

Transformative research as a form of valorising lay expertise

We draw these two perspectives together by using the notion of lay expertise as a sensitising concept for the declarative and procedural knowledge of practitioners. We consider expertise to be a form of knowledge that is distinct in its aim to inform collective decision-making and implementation (Epstein 2023; Grundmann 2017). Transformative research is a paradigmatic case for this notion of expertise because it aims to achieve the co-creation of knowledge that catalyses social change by methodologically integrating certified academic knowledge with the knowledge of practitioners. In the governance of mining activities, local knowledge of the place is crucial because of the unique identity of places. Furthermore, for policy purposes, networks among regional stakeholders can be considered pivotal. We assessed how researchers and practitioners, confronted with the task of providing recommendations for regional policy, interact in an actual workshop setting.

ASSEMBLING REGIONAL EXPERTISE: THE ENTRANCES PROJECT

Our empirical findings are based on our participation as contracted researchers in the ENTRANCES project and in particular on the comparative analysis of three co-creation workshops, each one organised with stakeholders from the still active German lignite mining regions (Central Germany, Lusatia, Rhineland). The project employed a complex mixed-methods approach including secondary analysis of administrative data, content analysis of media publications, standardised regional surveys, semi-structured stakeholder interviews, focus groups, and co-creation workshops. The basic

objective of the co-creation workshops was to develop policy recommendations for an effective regional sustainability transition. Below, we present the workshop design, the characteristics of the participants, and the analytical approach we used in this paper. Our analysis is based on a secondary reading of the workshop transcripts and an ethnographic reflection of the interactive performance of expertise between researchers and other actors involved.

Workshop design

Since the project consortium consisted of twelve research institutions, considerable effort was made to standardise methods across case studies. At the same time, interdisciplinary differences, professional judgement and regional specificities limited the effects of standardisation. Nevertheless, project meetings often provided a general direction of how to conduct the research. For instance, a phrase that was used by anglophone colleagues for characterising the ethical orientation of our case studies was that we should conduct "non-extractive research", i.e., research that benefits the researched as much as the researchers. While such an orientation could have been supported by explicit references to academic literature (e.g., Gaudry 2011), coordination was based to a large extent on an implicit understanding of oral communication. Such an implicit understanding was possible because the concepts of transformative research and co-creation overlap rather significantly with concepts that have been around for decades:

> A co-creation approach, when used for example in research, has similarities with co-production, transdisciplinary research, transdisciplinary knowledge co-production, action research, participatory research, and stakeholder engagement. Beyond research, it has commonalities with social innovation and collaborative governance (Prager and Nicholas 2024: 51).

Hence, the design of the co-creation workshops was guided by a mixture of explicit criteria and implicit professional judgement. Two criteria shaped the workshop design: the use of scenario techniques by researchers and the involvement of regional stakeholders from different categories.

Table 1: Workshop structure by region

Region	Duration (h:mm)	Moderators	Differences in content
Central Germany	2:41	3	Presentation of survey results and discussion Three quantified scenarios
Lusatia	3:42	1	Two qualitative scenarios
Rhineland	3:43	3	Presentation of survey results and discussion Three quantified scenarios

The workshops included the initial description of alternative scenarios for the future of the region. Scenarios do not claim to be correct or comprehensive and are best understood as approximations of a possible corridor of development, within the limits of which an actual development might be assumed (Kosow and Gaßner 2008). However, future scenarios are more than mere speculation. The scenarios we used were grounded in earlier empirical analyses of the ENTRANCES project. They took the challenges and coping strategies of the mining regions identified in the project and extrapolated these approaches into the future. Scenarios provide added value for finding and designing viable future development paths by questioning conventional assumptions and considering options that may be 'unthinkable' today. Thus, future scenarios ideally enable regional actors to gain a better understanding of the dynamics and interactions shaping socio-ecological and technical systems. By using this modelling, they are able to foster strategic decision-making impulses

for stakeholders within and beyond the region (Wilkinson et al., 2013). The categories used to invite regional stakeholders (politics, economy, civil society, environmental NGOs) were roughly in line with the Quintuple Helix innovation model (Carayannis and Campbell 2010).

Scenarios in two of the three workshops were based on socio-economic projections of statistical data (Table 1). While these formal projections were visualised in the workshops for Central Germany and Rhineland, in the Lusatian co-creation workshop, the quantitative socio-economic simulations were qualitatively reworked into narrative stories to make the scenarios easier for the participants to discuss. Furthermore, one of the authors of this paper interpreted the request for non-extractive research as an invitation to use the co-creation workshops to inform regional stakeholders about the initial results of the project and to give them the chance to comment on them. Therefore, in the workshops for Central Germany and Rhineland, results from a survey analysis comparing economic concerns in the mining regions were presented. Such first-hand results were thought to be of interest to regional stakeholders and an immediate benefit for investing their time. Due to these differences in design, the workshops varied in duration between under three hours to nearly four hours.

The workshops in Central Germany and Rhineland were moderated by a three-person team consisting of two economists and one sociologist, all members of regional research institutions. The workshop in Lusatia was moderated by a political scientist affiliated with a regional research institution. In each case, involving regionally embedded researchers was meant to establish trust among regional stakeholders and to portray the ENTRANCES project as being motivated by a genuine interest in regional development and not merely an extractive research exercise.

Participants, data collection and analysis

Participants in the co-creation workshops came from a pool of 50 to 60 stakeholders per region that were divided into three categories (politics and public administration, the economy, civil society and environmental NGOs) through desktop research at the beginning of the ENTRANCES project. During the project, these stakeholders were sequentially contacted for individual interviews, focus groups and the co-creation workshops. In Central Germany and Rhineland, the entire pool of stakeholders was invited by personalised emails to participate in the co-creation workshops. The pattern of participation was the result of self-selection, with little overlap to former participation in interviews and focus groups. In Lusatia, regional stakeholders that were interviewed or took part in focus groups earlier in the project were individually invited to participate in the co-creation workshop. To achieve a broad representation of the three stakeholder categories, members of missing categories were specifically contacted. In Lusatia, there was a greater overlap in stakeholders who had participated in previous project activities.

Table 2: Participants by region and stakeholder category

	Politics and public administration	Economy	Civil society and environmental NGOs	Total
Central Germany	1	4	2	7
Lusatia	3	2	1	6
Rhineland	1	2	6	9
Total	5	8	9	22

The workshops took place online between 24 April and 31 May 2023. In the end, a total of 22 regional stakeholders participated, seven in Central Germany, six in Lusatia and nine in Rhineland (Table 2). Five participants were involved in politics and public administration, eight were economic stakeholders, and nine came from civil organisations and environmental NGOs.

Most of the participants from Central Germany were economic stakeholders, three representing a large labour union and one representing the regional chamber of crafts. One civil society representative identified themselves as a researcher in regenerative energy supplies and an activist for environmental issues through "community organising" (Meier et al., 2022). The second civil society representative was a professional historian, knowledgeable about the economic and cultural heritage of the region. The political stakeholder was an active member of a local city council from a centre-left party.

The participants in the Lusatia workshop were representatives from state politics and public administration across various levels of government. The civil society representative had once been a long-term mayor of a village now known for its vibrant local community. Economic actors represented the regional chamber of industry and commerce and the regional branch of a large energy supplier.

Most participants in the Rhineland workshop were active in civil society, four were actively involved in environmental issues at different levels of government, one worked in a regional education network, and one represented a regional women's council. The economic stakeholders represented the regional business association of the chemical industry and the agricultural business chamber.

With prior consent of the participants, the workshops were recorded via a web-based meeting platform and transcribed afterwards.

Secondary qualitative content analysis and ethnographic reflection

Our aim in this paper is to reconstruct the interactive performance of expertise between researchers and regional stakeholders. While secondary analyses of qualitative data have yet to reveal their full potential (Heaton 2008), we did a secondary content analysis of the workshop transcripts (Kuckartz 2014). First, we analysed the transcripts sequentially, focusing later on communicative cues of declarative and procedural knowledge about the region. Since we were moderators of the workshops and participated in other parts of the ENTRANCES project, we also reflect upon our observations and experiences as participants in the field. During our participation in the project, we had no intention of being participant-observers (cf. Gold 1958), which helped us to perform our roles. Only afterwards did we reflect upon our observations and experiences and work on objectifying them through theoretical reflection (Scheffer 2002).

THE CO-CREATION OF REGIONAL EXPERTISE

In this section, we begin by presenting the range of role expectations that participants had in the workshop. Then we discuss the typical interaction patterns of the scientists and regional stakeholders that formed the hybrid character of the regional expertise. While scientists were expected to meet the regional stakeholders on an equal footing, the latter were expected to provide formal and practical knowledge about the region that was relevant for policy formulation at various levels of government. Finally, we discuss two issues surrounding selectivity that might undermine the formation of regional expertise.

Role orientations of individual participants

If we understand expertise to mean the marshalling of knowledge for action, it is important to note that the stakeholders in the workshops not only had professionally different knowledge bases but also differential access to policy formulation. The latter is especially evident in the expectations of actors regarding their role in the workshop.

The researchers participating in the workshop (including the authors of this paper) introduced themselves as representatives of the ENTRANCES project and hence advocates of the general objective to produce policy recommendations. In their roles as moderators, presenters of scenarios, and technical assistants, they were expected to put their views largely to one side and elicit the relevant knowledge from stakeholders.

Participants who introduced themselves at the beginning of the workshop as being able to contribute expertise in policy formulation were stakeholders from the political and economic sectors. Political actors in Lusatia, for example, expected the workshop to provide them with "food for thought" and "suggestions to concretise visions". Since policy formulation is part of the professional remit of politicians and administrators, the co-creation workshop offered them the opportunity to explore unconventional solutions and budget restrictions. The city council member in Central Germany regarded the workshop as a learning opportunity, especially about how a city could contribute to making the clean energy transition more equitable. The representative of the energy supplier in Lusatia had been well-established in the region for 25 years and also participated at the national level in the so-called Coal Commission. He was hoping to find ways to accelerate the process of the clean energy transition. Similarly, the representative of the business association in Lusatia hoped to "improve" the structural change process or "correct" maldevelopments that occurred in the course of this process.

A second functional understanding of expertise expressed by the participants was knowledge brokering through networking. Representatives of the labour union in Central Germany considered that their role in policy formulation was to ensure that workers participated in the change process by networking within and across the lignite mining regions. Similar networking approaches were used by the member of the chamber of crafts in Central Germany, the educational agency manager in Rhineland, and the members of the two environmental NGOs in Rhineland. For these stakeholders, participating in the co-creation workshop was a crucial element of their networking activities.

A third approach to expertise relates to the articulation of criticism. The representative of a county-level business development agency in Lusatia took a slightly critical view: While self-identifying as a political decision maker, he recognised the need to take "citizens more on board". Similarly, a former Lusatian mayor expressed the need for more attention to be paid to peripheral areas and for small-scale developmental projects to be considered in structural change funding schemes. The representative of a citizens' group from a village close to an open pit mine in Rhineland took an even more critical view towards the governance of the transformation process because he believed stakeholders from civil society were at a severe disadvantage in terms of political participation; the same views were held by the representative of a civil organisation in Central Germany.

The director of a historical collection in Central Germany was an interesting case. Although he did not feel he was "worthy" of participating in the workshop, he had nevertheless accepted the invitation because of his academic profession. While he explicitly denied having technical knowledge about

structural change, throughout the workshop, he contributed significant knowledge about the economic history of lignite mining. Due to his reflective professional role, he was able to introduce more visionary suggestions than other participants. A similarly visionary approach was taken by civil society stakeholders in Rhineland, who called for discursive spaces for debating values and developing utopias.

Even though participants were invited particularly because of their assumed practical expertise as members of stakeholder organisations, some doubted that they had any expertise at all about structural change. These stakeholders had been encouraged by others to participate, e.g., colleagues, their superiors, or the research team. The spokesperson of a women's association in Rhineland, for example, questioned her own expertise on the topic. Although she had been surprised to receive an invitation from the research team, she decided to participate because women were also part of the population affected by the lignite phase-out and she wanted to disseminate the knowledge she acquired in the workshop afterwards. A public administration officer at the district level in Rhineland belittled her own potential expertise by saying that her work was concerned with the "details of open pit mines rather than the broader picture". Later, she provided clear information on the recultivation options for open pit mines – but only after being directly addressed by a moderator who deliberately asked very basic questions that occurred to him as a lay person on that specific topic, e.g., What would happen if we did not do anything with the open pit mines? Would the mines not be automatically reclaimed by nature? She ruled this out as a viable option for legal and for safety reasons. The representative of the agricultural business chamber in Rhineland said she was new to the job and therefore had almost no expertise on the issue. These participants' low self-assessment meant that they only made marginal contributions to the discussion and some dropped out early.

The hybrid knowledge base of regional expertise

To investigate the assembly of regional expertise that was sought after in the ENTRANCES project, we compare the interaction of scientists and regional stakeholders in several instances where disputes over knowledge emerged or could be expected to emerge. Previous studies argued that experiential knowledge was often dominated by abstract forms of academic knowledge (Groves 2017). For this step of the analysis, we assumed that both academic and experiential knowledge could be relevant for assembling regional expertise, however, we wanted to examine how their relationship is situated by looking at how different forms were used by participants. Theoretically, experiential and formal knowledge could complement one another, but they could also conflict with one another as well.

Scientific knowledge challenged by regional stakeholders

We begin by describing how regional stakeholders interacted with scientific knowledge that was obtained through a standardised survey in an early attempt to evaluate policy. In the workshops for Central Germany and Rhineland, the moderator presented survey results relating to the development of personal economic concerns in lignite mining regions in Germany (2016-2021). These results were still under review by an academic journal and were based on the German Socio-Economic Panel Study which defined mining regions according to the statistical construct of labour market areas. Findings showed that there were no statistically significant differences between lignite mining regions and the rest of Germany. There was basically a downward trend in the likelihood of there being strong economic concerns. To a certain extent, this can be interpreted as a positive evaluation of the regional structural policy that accompanied the coal phase-out. Stakeholders actively engaged with the results, utilising their experience in interpreting numbers and their

knowledge about the regions. In Central Germany, for instance, they mentioned contradictory evidence from other surveys, especially for Central Germany and Lusatia (MDR 2022). Above all, they questioned the operationalisation of mining regions on the basis of economic arguments about labour market areas being too broad for the purpose of policy evaluation. Instead, they insisted that the legally codified definition of mining regions at the county level ought to be used. Regional stakeholders argued that applying the legal definition would assure comparability with other studies. As a consequence, the project team changed their operationalisation of mining regions to facilitate the commensurability of results. While the adjustment in the operationalisation of mining regions did not significantly change the results, it should be noted that the potential use of the results for policy formation was decisive for this workshop controversy and not theoretical arguments about territorially interlocking labour markets.

This part of our analysis demonstrates that it is possible for co-creation workshops to temporarily establish a level playing field among heterogeneous participants that allows for reciprocal exchange. This does not mean, however, that institutionalised hierarchies between academic and non-academic forms of knowledge no longer persist — as will be shown below.

Singular and commensurable regional knowledge

Since the ENTRANCES project aimed to generate policy-relevant knowledge about specific features of regions, we would like to describe how regional characteristics were marked communicatively during the workshops. Using an ideal typical distinction: were regions described as singular places or as commensurable territories?

When participants referred to the regions by name, but also by using indexical expressions, such as “here”, “in our region” etc., both forms of reference demarcate regions as places with a unique identity and singular features. However, in order for subjective knowledge about a region to become policy-relevant, it needs to be connected to more general forms of knowledge, otherwise, it will be difficult to justify claims in public debates. The workshops provided several examples of how stakeholders translated specific collective experiences of regions into more commensurable terms that have already been established as scientific facts. For example, stakeholders in Central Germany and Lusatia discussed demographic decline and skilled labour shortages to a much larger extent than in Rhineland. This coincides with current scientific knowledge on the regional pattern of demographic change but also with the negative experiences of the people in the region, especially in Eastern Germany. Since public money is largely allocated to municipalities based on population figures, demographic decline means fewer monetary resources which is often accompanied by a decline in infrastructure. The effects of demographic decline have been felt most strongly in Lusatia because of the overall sparser population density there. Infrastructure is a spatially unique feature; hence, when projects are discontinued or never implemented, this is often perceived as a collective devaluation or an existential threat to the place itself. Stakeholders in Lusatia, for example, expressed their disappointment that railway projects they had hoped for have not been realised because “they [the federal government] think that there is not enough of us”. Administrative regulations use viability thresholds, translating continuous variables (population numbers) into allocations in-kind (infrastructure categories for particular places). These criteria play out negatively for Lusatia because of demographic decline but also because the region borders the Czech Republic, which makes it a peripheral territory in a national sense despite being quite centrally located in Europe.

Hence, this brief example shows how the specific collective experience of a region can be translated into more commensurable terms, especially when academic knowledge backs it. However, academic

knowledge can also be used to challenge experiential knowledge. In another instance, the representative of the chamber of crafts in Central Germany argued that the value of water turbines in small streams to make energy production more efficient was being underestimated. While he had first-hand knowledge from practical cases on how to implement such turbines, his efficiency claim was challenged by a female environmental activist citing established expert knowledge which states that the potential for water-turbine-based energy production is basically exhausted in Germany. Her reference to this quietly ended this debate.

Conceptual debates initiated by regional stakeholders

Often, academic discourse that focusses on a critique of terms used in public or academic debate is considered too detached from practical relevance (Abend 2023). The debates of lay experts in these workshops question such a view by speaking to the performativity of academic debates in (mundane) politics.

The title of the ENTRANCES project includes the notion of "energy transition" as a way of defining its subject. There was, however, no strict language policy among project members during the actual project activities. Project members used terms such as "energy transition", "structural change", "sustainability transformation", "climate mitigation policy", etc., depending on their disciplinary or theoretical background and the situation at hand. The Coal Commission and the resulting federal legislation had a clear focus on economic terms, such as regional "investment" and "structural reinforcement" policies. The media, an important source of common knowledge, have not applied a consistent language policy when reporting on the coal phase-out. Hence, in the absence of clear terminological expectations, it is rather surprising that debates about the political implications of concepts emerged in two of the workshops. These debates were initiated by regional stakeholders, not researchers.

Overall, regional stakeholders used "energy transition" and "structural change" roughly twice as much as "transformation". A civil society stakeholder in Central Germany explicitly asked which term to use in the workshop because of the normative and systemic implications of concepts. Civil society stakeholders in the Rhineland workshop discussed the conceptual implications of different terms more extensively and supported each other in criticising the narrow focus of North Rhine-Westphalia's regional policy on economic structural change when actually a much broader societal sustainability transformation was needed in their view. One civil society stakeholder used a local example to emphasise the practical implications of differentiating between concepts:

> And then you end up with what I call a technocratic understanding of how structural change can work. That's what I call the logic of economic development. You can see that here. That's how it works here. Occasionally, decisions are made. You find them a bit questionable. And then there are those who write letters to the editor. 'Why is the indoor riding arena at Soers Sports Park being subsidised?' And so on. What does that have to do with structural change? But the other thing would be to move away from or beyond this economic development logic to understand the whole thing as a transformation […] We are undergoing a major transformation, whether we like it or not. The transformation actually means, yes, doing business and living more sustainably. It also means taking biodiversity into account. That is also part of the process if we want to transform the mining area here in Rhineland (own transcript).

The historian in Central Germany argued for the strategic use of the term transformation because it allows a link to be made between the current aim of a sustainability transformation and the historical

experience in the region of the peaceful revolution in the former German Democratic Republic. He felt that it was crucial to create a connection between these two processes in order to reinforce the regional identity of the Central German region. In the Lusatian workshop, such conceptual issues were not discussed in depth. The most frequent term used in that workshop was structural change. Since the categorical composition of the regional stakeholders in the workshops differed significantly, it is difficult to ascertain whether this divergence is attributable to regional specificities or to the stakeholder categories.

Overall, the stances taken by regional stakeholders indicate a greater sensitivity for the implications that concepts can have on policy than that which was found among the participating researchers. The similarities between workshop and academic debates could be interpreted as a form of emergent scientisation of society (Beck and Bonß 1989; Weingart 1983).

Problems of selectivity

Scientific knowledge usually claims to be universal, a claim that is based on methodological standards on the one hand, as well as on the specification of a scope of validity. Two forms of selectivity turned out to be especially problematic in relation to the production of regional expertise for multilevel governance in the co-creation workshops that we analysed.

Selective participation

When interpreting the recommendations produced in regional co-creation workshops, it would be ideal to be able to control the categorical composition of the stakeholders. In the workshops we analysed, participation was highly selective. There are many reasons for this, including the fact that the workshops fostered existing regional policy networks. Another reason is that regional stakeholders face time restraints and some also lack self-confidence. Time restraints were mentioned, especially by civil society stakeholders, but probably apply to many other stakeholders that were invited but did not participate. A lack of self-confidence became indirectly clear in the reluctance of some participants to actively contribute to the workshop debates. Assuming an equal composition of stakeholders, specific workshop topics could arguably be interpreted as indications of regional specificities. In lieu of controlling stakeholder participation, information from workshops can be cross-checked with current academic knowledge of the region for external validation. Otherwise, it is difficult to draw conclusions about regional singularities.

The problem is exemplified by the topic of qualified workforce shortages. Such shortages were alluded to in all regions, with problems being portrayed as being most severe in Lusatia, followed by Central Germany and Rhineland. This rating corresponds to the scientific information available (Jansen and Schirner 2020). However, problems related to vocational education were only discussed in great detail in Central Germany. Economic stakeholders there consisted of three labour unionists and a delegate from the chamber of crafts. Their expertise was based on universal knowledge as well as copious experience of the competition between vocational and academic education programmes, on the one hand, and school dropouts on the other. Their normative and analytical emphasis of this problem is in contrast with stakeholders in Lusatia and Rhineland. Another example of the subject-related influence of stakeholder positions and thus evidence for the importance of workshop recruitment is the evaluation of participatory governance instruments in designing regional policies of structural change. Although participatory governance instruments were considered important in all regions, existing procedures were most vigorously criticised by civil society stakeholders in Rhineland. Since no systematic comparisons of participatory governance instruments across the

three lignite mining regions exist, it is difficult to assess where improvements would be needed the most. What can be seen, however, is the degree of elaboration on the arguments put forward. Stakeholders in Rhineland, for example, justified their criticism by providing more than just negative experiences. They also suggested that improvements could potentially be made through "citizen councils" (Dienel 2002; Lietzmann et al., 2021). The discussion of participatory governance was fundamentally different in Lusatia, where stakeholders criticised citizen participation as often being ineffective and inefficient. Instead of participatory governance, a broader distribution of the benefits of clean energy projects at the local level was considered a superior form of "economic participation".

Hence, if co-creation workshops are expected to gain information on regional specificities, it would be crucial to minimise selection bias in participation. While a random selection of participants seems to offer a possible solution to this problem, it does not rule out selectivity completely and might miss the point of recruiting regional stakeholders, who are relevant for policy formulation and implementation, instead of individual citizens.

Selective aggregation and communication

As in every research endeavour, contributions to co-creation workshops do not speak for themselves – they have to be interpreted, evaluated and aggregated to be able to inform policy. This argument assumes that policy recommendations are written up by researchers in a report or are orally presented to policymakers. Reports for comparing a relatively large number of regional case studies (in ENTRANCES there were 13) can be highly standardised and, as a consequence, much of the regionally specific knowledge is levelled out in the writing process. Alternatively, leaving more space for professional judgement runs the risk of producing regional reports that are difficult to compare and aggregate into meaningful messages. While policy recommendations from the co-creation workshops in ENTRANCES have been documented in public reports, we do not aim to systematically compare them here. Instead, we would like to describe how we experienced the oral presentation of policy recommendations to representatives of the European Commission in Brussels, which was another format of co-creation that we attended virtually.

At a certain stage of the workshop, a summary of the policy recommendations was presented. It was a rather long list of diverse issues with no explicit aggregation criteria and without a single "take-home message". These recommendations were later discussed in a round table attended by several principal investigators of the project. During this discussion, the conclusion of one economist stood out to us. He asserted that the conclusions to be drawn were obvious: As a society, we have to restrict our energy consumption. The best way to achieve this was to raise energy prices, mainly through carbon emission trading. This would mean less growth in the future compared to what could be expected were there no restrictions on energy consumption. But the latter would mean not achieving the internationally agreed on global warming targets, which he did not consider to be an option.

Hence, there it was, a clear message for policymaking, ready to be translated into governance structures. It was difficult to see how these comparatively clear-cut conclusions could have been drawn from the diverse list of policy recommendations presented earlier. Instead, they corresponded mainly to economic theorising about climate mitigation policy (MacKenzie 2009). These policy recommendations could have been generated (and indeed have been implemented to a certain extent) without the co-creation workshops in different European regions. Were these workshops then in vain?

We (the authors of this paper) discussed this question and came up with the following answer: Today, policy – including the coal phase-out policy – is created by actors at different levels of governance. While the formally derived policy recommendation of economists could have been obtained without co-creation workshops, the policymakers relevant for such a policy operate at rather high levels of governance, such as the EU or the national level. At the same time, the design and implementation of structural change policies accompanying the coal phase-out are, to a large extent, decided at rather low levels of governance, such as the regional or local level. At these governance levels, the problem of aggregation is either absent or less severe. Furthermore, at the regional and local level, co-creation workshops not only contribute to policymaking through the development of declarative knowledge but also provide procedural knowledge by virtue of being carried out. Co-creation workshops constitute not only instances of co-operative debate but also potential networking among participants that could be activated at a later point.

DISCUSSION AND CONCLUSIONS

In this paper, we set out to analyse the objectives and practice of co-creating regional expertise. We applied the notion of expertise to the objective of transformative research to produce "socially robust" knowledge that is assumed to catalyse social change. In transformative research, co-creation of knowledge between researchers and practitioners is considered a way to produce such knowledge. In terms of concrete workshops that aim to generate policy recommendations, this places certain expectations on participants. We analysed three co-creation workshops in German lignite mining regions to assess how these expectations are met empirically. In this section, we summarise our results on the assembling of regional expertise in co-creation workshops and then reflect upon the role of such hybrid forms of knowledge production for science and politics.

We summarise our results by highlighting three points: *First*, the role orientations of regional stakeholders in co-creation workshops depend on their access to political power. They ranged from being open to new ideas among policymakers, to a rather fundamental critique of existing policies among civil society actors, to a rejection of the notion of being an expert at all. Ideally, such different viewpoints can stimulate each other to create an emergent form of collective regional expertise that is more than the sum of its parts. Indeed, interaction formats that enable trust-based relationships between scientists, decision makers and technical administrative staff are most likely to facilitate policy learning (Rittelmeyer et al., 2024). For facilitators of workshops with heterogenous target groups, it is crucial to be attentive to the self-marginalising voices among participants and to encourage them to make themselves heard. The participants who saw themselves as being rather ill-equipped to contribute to collective regional expertise were either a) working in a subordinate position, b) academics scholars from the humanities or c) female. *Second*, during the interaction in workshops, the participants did not establish a clear hierarchy between academic and experiential forms of knowledge so that the collective knowledge base was a rather hybrid one. This became evident in two ways: Facilitators explicitly encouraged participants to share their views, and regional stakeholders both challenged the academic knowledge presented to them and drew on objectified knowledge themselves in addition to their practical knowledge. *Third,* the regional specificity of expertise is most valuable at lower levels of governance. If experience-based knowledge is translated to higher levels of governance, this does not mean that it is actually devalued, but it almost necessarily changes its form (Demszky and Nassehi 2012, cf.). It is also at local and regional governance levels that the networking effect for participants of co-creation workshops is most relevant for further exchange in the future (cf. Levelt and Metze 2013). Such networking effects can be especially valuable for the practicability of policies, i.e., enhancing their efficiency. While the EU

uses network governance to increase the efficiency and legitimacy of its policies (Börzel and Heard-Lauréote 2009), we were able to show how EU research policy fosters this approach.

However, reflecting upon this workshop format, we also identified two main challenges: *First*, participation in co-creation workshops is highly selective. This selectivity in the composition of workshop participants is also reflected in how they contribute during the workshop. Because of this selectivity, the aim to widen the knowledge of expertise can therefore be questioned not only from a scientific but also from a democratic standpoint. Stakeholders involved in the co-creation of (regional) expertise are still self-interested actors (Seibicke 2024). Whose voices are likely to be assembled into regional expertise? One scientific response to the problem of selectivity is professional research ethics: Researchers conducting co-creation workshops bear the responsibility of ensuring a balanced interpretation of results. This responsibility can be pre-structured by developing professional guidelines for developing policy recommendations (Rudnicki and Wojnicka 2024). Furthermore, from a democratic perspective, it can be argued that policy recommendations resulting from co-creation workshops are typically fed into policymaking processes that usually enjoy democratic legitimacy and policymakers are free in their decision to use them or not. While co-creation workshops, such as the ones presented here, generate recommendations for policymakers, it is the task of politics to create political liability from them.

A *second* potential limitation that we identified was the aggregation of regional expertise to higher governance levels. While there is no obvious or unchallenged mechanism available in processes of knowledge co-creation to aggregate diverse forms of knowledge, democratic political institutions usually provide formal and informal rules on how to aggregate a diversity of viewpoints.

Against this backdrop, we point out three possible alternative approaches to complement or stand in place of co-creation workshops for the purpose of assembling regional expertise: Citizen councils with randomly selected participants (Lietzmann et al., 2021), surveys with regionally representative samples, and more socio-demographically representative political institutions. While the first two alternatives have variously been implemented, the problem of more socio-demographically representative political institutions has remained unresolved. Engagement in political parties and candidates running for parliamentary office have increasingly come from an academic and urban background (Bovens and Wille 2017; Elsässer et al., 2017; Haffert 2024), which means that practical knowledge of various fields in a functionally differentiated society is systematically crowded out of policymaking. Therefore, this seems to be the most relevant challenge for the future of democratic governance.

What do we learn from this form of network governance in EU-funded research for the relationship between science, politics and society? *First*, our research shows that EU-funded research is an important force in the hybridisation of science and politics as proposed, for instance, in the Mode 2 literature. This is even more so because third-party funding has become an indispensable way of financing research. Indeed, third-party funding increases the likelihood of knowledge exchange between science and policy (Thune et al., 2023). Even though it can be argued that EU funding criteria that favour hybrid formats of assembling policy expertise have contributed to methodological innovation, these formats and their implications will continue to require scholarly reflection (Barbosa Mendes et al., 2024). *Second*, while our analysis suggests that (regional) expertise from such hybrid production formats does not permeate higher levels of the EU governance system to the same degree as lower levels, this is not necessarily a downside because demand for expertise is likely to correspond to the responsibilities of the respective governance levels. Other research suggests that there are also national varieties of evidence use in (energy) policy (McDowall 2024). At the same

time, the likelihood of policymakers participating in knowledge exchange increases in proportion to their own experience in academia (Luhmann 1977; Thune et al., 2023). Hence, taking a further step back from the hybridisation of formats for producing expertise (e.g., through funding criteria), it is the secular academisation process that produces a hybridisation of expertise in society that is often overlooked (Stock et al., 2024).

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Serendipities
Journal for the Sociology and History of the Social Sciences

BOOK REVIEW

Blois, J.P. (2018) Medio siglo de sociología en la Argentina. Ciencia, profesión y política (1957-2007)

Lautaro Lazarte

Blois, J.P. (2018) Medio siglo de sociología en la Argentina. Ciencia, profesión y política (1957-2007), EUDEBA, Buenos Aires,

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Juan Pedro Blois's book, Medio siglo de sociología en la Argentina. Ciencia, profesión y política (1957-2007) (Half a century of Sociology in Argentina. Science, profession and politics, 1957-2007), shows the consolidation of the history of sociology as a productive research field in that country. Relying on existing studies of the history of social sciences, using a varied set of materials (university syllabus; student publications; institutional documentation on schools, research centres and professional organisations; interviews; etc.), and assembling previous contributions - from the same author as well as other researchers - the author undertakes the complex task of composing a mosaic of the vicissitudes of the discipline over its past fifty years. This catalogue of sources allows readers to understand the eventful local history of the discipline, characterised, at least until the definitive return of democracy (1983), by a series of violent disruptions that included the loss of institutional archives and invaluable voices and testimonies.

Unlike previous works, more limited in their temporal extension, this book offers a reliable account of the various periods and conjunctures of sociology in Argentina since the creation of the first modern academic university department in the late 1950s. The work is structured on a periodisation that balances local political-social milestones with the history of the discipline itself. Organising the institutional trajectory in this way allows the writer to show how across different periods, more weight was either given to the autonomy of the discipline, or to its dependence on local politics or international forces, even though both were present. This produces a periodisation composed of six heterogeneous stages:

a) 1957-1963 (antecedents and creation of the sociology department);
b) 1963-1966 (generalisation of internal dissent and failure of the original project);
c) 1966-1973 (political radicalisation and renewal of theoretical orientations);
d) 1973-1983 (generalised politicisation and withdrawal during the years of political repression);
e) 1983-1990 (the long institutional reconstruction in the post-dictatorship);
f) 1990-2017 (sociology in the neoliberal university and the reorganisation and expansion of the labour market).

The extended time period covered in the book makes it possible to link Argentine sociology not only with changes in its socio-political and cultural context but also with dynamics and events beyond its borders. As such it is a chapter about the history of Social Sciences in Argentina, but at the same time a story about the broader context of sociology - national, regional and global. This adds value to the analysis by ensuring that it is not restricted to the local context, but rather, the work explores sociology in Argentina - in terms of theoretical influences and research problems, establishment of networks of actors, institutions, intellectual influences and resources and the development of the professional practice of sociology - in comparison with sociology in the United States, Europe and other Latin American countries. This comparison serves as a methodological resource that seeks to overcome the views already explored on the singularity of the Argentine case. Thus it is possible to draw a broad portrait that takes readers from the foundation of the department of Sociology at the University of Buenos Aires (UBA) in 1957, to the growing diversification of the employment situation of the sociology graduates at the beginning of this century.

Although this was not the only university institution that taught sociology in the country, the author suggests it was the one with both the largest enrolment of students and teaching staff and the presence of the most prestigious sociologists visiting its classrooms and engaging in prominent intellectual, local debates and disputes about methodologies and goals. Likewise, Blois points out three main topics that influenced the trajectory of the Sociology department at the UBA during the entire period of analysis: Firstly, the book sets out discussions about the status of the sociology, especially on its disciplinary autonomy and national character. Secondly, the relation between sociology and political agenda. And thirdly, the challenges of professionalisation of sociology.

The establishment of the sociology degree in 1957 within the Faculty of Philosophy and Humanities at the UBA occurred within circumstances that reflect both national and international issues. Among the former, student organisations supported both the academic consolidation of the discipline and the university modernisation since 1955. For the latter, it is possible to link with the expansion of the structural functionalist “orthodox consensus”, the concern for socio-economic development and the search to implement social changes based on planning. The name to be highlighted is that of Gino Germani, an Italian exiled scholar who is generally noted as the pioneer in the introduction of modern sociology in the country. In his vision, this space would serve as an incentive to promote a broad process of modernisation of Argentine society in line with the prevailing developmental ideology at the time; as well as a place for the training of future professionals who will implement this transformation in the state administration and in other institutions of civil society.

Germani and his group of collaborators managed, between 1957 and 1963, to articulate a successful strategy for the formation of the department and the dissemination of a ‘sociología científica’ [scientific sociology], concerned with empirical research and the resolution of social problems. This initiative was built on the support from the student movement and funds received from a series of international organisations and philanthropic foundations, dynamics shared by other sociology departments and institutes in the region. The success of its institutional project is exemplified with the holding of the ‘Primeras Jornadas Argentina y Latinoamericana de Sociología’ [the first Argentine and Latin American Conference on Sociology] in September 1961, in which the Department of Sociology acted as host. Germani took advantage of the funds available for the organisation of the event, invited international and regional exponents of the discipline and exhibited the activities of the department and the institutional networks in which it was inserted. In this way, the event became a showcase of regional and international peers and advanced a scientifically oriented sociological discipline. However, this abrupt conversion from previous

sociological production demonstrated a particularity of the Argentine experience, for elsewhere, in other countries of the region, there was a more peaceful coexistence with previous forms of social thought. The event reinforced the idea, prevalent both in the stories of the protagonists themselves and in various later histories of local sociology, that the creation of this department is synonymous with the introduction of sociology as a scientific discipline in the country. Blois suggests this confrontation resulted in the exclusion of almost all the animators of the discipline who had had some relevance before 1955 linked to the 'sociología de cátedra' [Chair Sociology], as well as a series of figures related to social essayism. These displaced intellectuals, who to a greater or lesser extent contended with the project articulated by Germani, took refuge both in extra-university settings and in existing sociology chairs and institutes in universities in the interior of the country or in departments that were opened in new private management universities.

As of 1963, the consensus that enabled the opening of the institution began to disarm quickly. Attacks on Germani's leadership came from multiple directions. Some young scholars came back from abroad holding a PhD and openly expressed their dissatisfaction with the prevailing structural-functionalist and quantitative orientation and advocated for the introduction of new perspectives and methodologies, such as Marxism, French structuralism, ethnomethodology and phenomenology. Various sectors of the student movement, politically radicalised by the Cuban Revolution and the decolonisation processes, began to loudly demand a better connection between teaching and social demands. The heterogeneous criticisms ranged from questioning imperialist links and financing from the US to demands for improvement in the teaching standards and a diversification of perspectives in the curriculum. The intensity of conflict was one of the reasons leading to the resignation of Germani from the leadership of the department. This action produced a leadership crisis and deprived the department of new external subsidies.

In parallel, there were milestones which strengthened the local sociological field. On one hand, sociology departments opened in private universities linked to the Catholic Church, such as the Universidad Católica Argentina (UCA 1959) and the Universidad del Salvador (USAL 1962). On the other hand, these new private research centres enabled sociological research to be carried out outside the university and government. On the other hand, some new private research centres were created in Buenos Aires. They clearly divided in two groups. One, institutions devoted to traditional scientific social research, such as the Instituto de Desarrollo Económico y Social (IDES) or the Instituto Torcuato Di Tella (ITDT). Two, organisations also focused on research but more oriented by ideological orientation and political issues. Marxist-oriented Centro de Investigación en Ciencias Sociales (CICSO) could be includes in that category.

All these institutions had to set up various strategies to guarantee income that would allow their operation, from the signing of research agreements with foreign organisations and universities to offering advanced courses for new sociologists. Such organisations, in times of student discontent or political turbulence, have become places of refuge where professors excluded from national universities are able to continue their teaching and research activities. Likewise, some questions about the professional role of local sociologists emerged. It was so clear that sociology labour market could not find a constant demand to absorb totally first cohort of graduates.

The coup d'état of the Argentine Revolution (1966) closes this period and opens a new conjuncture marked by the rearrangement of the local sociological field. The UBA sociology department underwent dramatic academic staff changes. Most professors resigned or were forced into exile. These new vacancies were soon filled. At first, the scholars who arrived were sympathetic to the military regime, but they did not stay for long due to the intense student opposition. Many of the

dismissed and resigned professors from the UBA moved their activities to the private universities or private research centres. In a contrary movement, actors who had been excluded from the department by Germani - particularly those linked to networks of militant Catholics - re-entered the system and together with a new batch of young graduates and teaching assistants, were promoted to higher positions. Due to the lack of more qualified or experienced individuals, this last group comprised the teaching staff of the department. This context, together with political radicalisation and an increasingly active student mobilisation, enabled the emergence of new orientations within the department, with two experiences that set the tone for the new teaching context, in which different professors grouped around the 'Cátedras Marxistas' [Marxists Chairs] and the 'Cátedras Nacionales' [National Chairs]. Besides these groups of sociology teachers and students expressed diverse ideological positions, they sought to link sociology with a revolutionary and anti-authoritarian political praxis. In addition, this context allowed the introduction of new pedagogical practices, including collective evaluation, the diversification of theoretical references, the recovery of social essayism and the emphasis on the links between sociological practice, social work, political commitment and militancy in the poorest neighbourhoods. In other words, it became just as valid to read Marx, Durkheim and Weber, as well as Lenin, Gramsci, Althusser, Mao-Tsé Tung, "Che" Guevara and Juan Domingo Perón. The heightened atmosphere brought into question the degree of autonomy the discipline should have in a time of local, regional and global political and cultural turmoil and its role in achieving social transformation.

The return to the country of General Juan Domingo Perón and his subsequent ascent to the presidency in 1973, accelerated the dissolution of the autonomy of the field within the logic of the political dispute in general and the Peronist movement in particular. This fact allowed many of these professors and activists - especially those linked to the 'Cátedras Nacionales'- to exercise management in the functioning of various chairs, institutes and the department itself. However, these actions caused the breakdown of the militant networks formed in the previous period. That same year, the first reform of the study plan was also carried out, which brought the realisation of professional practices in various departments of the state apparatus, and gave them legitimacy. In the midst of an increasingly violent political climate, the department was separated from the orbit of the Faculty of Philosophy and Letters in 1974, placed under direct control of the UBA Rector Office, and finally closed in mid-1975.

The 1976 military coup reproduced the previous pattern of dismissals and layoffs at the UBA, but reopened the local sociology department at the end of that year, with scholars linked to the official political line were allocated to head the office, even some of those with military ranks. In that context, the department was rebuilt in the midst of a material and symbolic isolation, in great contrast to the appreciation it had had at its founding moment. From 1977, a fresh sociological curriculum was applied. This was based on technical criteria but its contents were heterogeneous, which made it very difficult to offer a lucid, coherent graduate profile. For their part, the private research centres, largely financed by international organisations, resumed their complementary training role, becoming a refuge for those expelled from the department. In other national universities with sociology departments, this hostile climate forced the general closure of the institutions; private universities, apart from USAL, closed their sociology undergraduate courses and reoriented their teaching towards the postgraduate level. The purge of the teaching staff and the negative evaluation of the discipline forced a new movement, both spatial and professional, of its practitioners. Exile, both internal and external, was a reality that many had to face; and with the management of research projects financed by foreign foundations, there were new professional work dynamics and a general

reorientation of departments into new fields, such as human resource management, marketing, advertising.

The return of democracy in 1983 implied a "re-foundation" of the department, thanks to the alignment of the UBA management with the policy carried out by the administration of President Raúl Alfonsín. Here a series of processes were combined where, on the one hand, there was an experience of plural dialogue and criticism of the past militant regime by many of its protagonists in exile in Mexico, Venezuela and Europe. On the other, the incorporation of new theoretical references linked to the recovery of Weberian and Gramscian thought is observed, which converge with the rise of an international agenda concerned with democratic transitions and their strengthening. All these factors set the tone for proposing a new institutional framework based on a broad inclusion of various actors and it is from here that the regular operation of the department was consolidated. However, this process occurred slowly and chaotically, amid a lack of material resources that wouldn't allow for professorial appointments or sufficient resources for the departments. In 1988 this re-founding was strengthened in institutional terms. At the UBA, a new curriculum plan was approved and the department became part of the new Faculty of Social Sciences. Also, a rule was enacted by the national congress aimed at regulating the professional practices of sociologists in the country.

Although economic hardships were exacerbated by neoliberal reforms throughout the 1990s, there was at this time also a process for the creation and reopening of undergraduate and graduate degrees at various national and private universities in both the metropolitan area of Buenos Aires and the provinces. The appearance of these new academic spaces decreased the centrality of the UBA department on the local sociological map.

In its final section, the book focuses on the complexity of graduate employment and career development of sociologists since 1990. The appearance of new fields and labour opportunities led sociology departments to raise novel questions about professionalisation. However, syllabus changes or new postgraduate programs seemed to be insufficient to fill the gap between sociology training and labour market. Here, the emergence of sociological demand differed from classic academic jobs. It was clear that new job offers for sociologist opened in areas such secondary schools teaching, consulting, management in Non-Governmental Organisations and design and evaluation of public policies at the national, provincial and municipal levels. The wealth of this last section lies in the use of testimonies that expose the variety of places for the professional development of sociology and the skills required for a sociologist to find a job.

To close this review, some critical points could be raised. First, the sociological perspective of the author should be recognised. He explained the fate of the UBA sociology department regarding shifts in other factors (e.g., state institutions; private research centres; other sociology departments; the editorial market). Due to the thoroughness and depth of its analysis, this work can serve as a good model for future works investigating the dynamics favouring the institutionalisation or deinstitutionalisation of academic spaces for training sociologists and the consolidation or weakening of the local sociological space. Second, the networking analysis was achieved very well throughout the work. Thus, it covered sufficiently the formation of links among international and regional centres, the assembly of a diverse range of networks that facilitate academic mobility and the provision of funding and prestige. However, the book also analysed the impact of negative elements such as institutional and political instability; the financial uncertainty and the lack of voices in the public debate able to defend and proclaim the importance and usefulness of sociology. Third, this volume clearly showed the complex bonding links between the institutionalisation of sociology

in universities and the diversification of employment for local sociologists, which required the creation of professional institutions and regulating laws on professional practice.

Additionally, without diminishing the merit of the author, some words could be said about the time period chosen in the book. Following a large literature, the volume slightly mentioned some institutional milestones prior to the creation of UBA department of Sociology in 1957. This information blackout made invisible traditions, authors and topics that contributed in many ways to the consolidation of the discipline in the country. The inclusion of that past history could both enrich the narration and offer a more comprehensive evaluation of facts and processes. Another element that is missing as well. Besides this has also been quite neglected by local historians of the discipline, Blois do not show a more detailed narration of teaching experience at both privately managed universities and higher education institutions located in the interior of the country. The inclusion of such elements would enable a better explanation of their role in the production and distribution of sociological knowledge in Argentina. The use of comparison with regional and international experiences appears intermittently throughout the work, but it yet fails to take on a systematic dimension. This means that, to a certain extent, the book remains anchored in the account of the local organisation of the discipline, referring to a vision essayed by the local historical tradition, centred primarily on itself and using the singularities and the evolution of Argentina. As a last point, with the emphasis placed in the final chapter on the impact of the neoliberal reforms, the mutations this brought to the local university in the teaching and research work, and the multiplication and diversification of employment for sociologists, a greater link with other regional cases is missing. In this sense, it would have been of some value to compare what happened in Argentina and at the UBA with other countries and universities in the region, given that most of the region suffered similar political cycles of neoliberal and progressive governments through the 1990s and the first decades of the 21st century.

Delineating these linkages and opting for a long-term perspective then makes it possible to jointly articulate the synthesis of the institutional dynamics experienced by the sociologists at the UBA. It is valuable the mixture of novel sources and the revision of documents already studied. Thus, the book could summarise a varied set of investigations on partial aspects of a broader process. Being able to synthesise all these issues in one work, the volume marks the maturation point of research on the history of local sociology, but this is also an achievement that has been replicated in other countries in the region. Thus, it seems possible to tie together, taking into account the items detailed above, a more interconnected history of the discipline, which allows us to delve into the similarities and differences that bring the trajectory of sociology in Argentina closer or further from what occurred in other countries, whether in Latin America or elsewhere. Indeed, this type of work could serve as a basis for proposing a new comparative analysis of the classic cases where similar development pathways (Argentina, Brazil and Mexico), as well as investigations where there has been recent progress in reconstructing local disciplinary histories (such as Chile, Colombia, Paraguay, Peru or Uruguay). In short, this book expresses, on the one hand, the arrival point for the long work agenda ahead; but on the other hand, a starting point for future discussions about the need for more comparative work, particularly with regard to the history of Argentine sociology in relation to other regional and international cases.

Serendipities
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BOOK REVIEW

Bourdieu on Sociology mirror's effect

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Pierre Bourdieu, Retour sur la réflexivité, Paris, Éditions de l'EHESS, 2022.

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FROM EPISTEMOLOGICAL VIGILANCE TO SCIENTIFIC REFLEXIVITY

More than twenty years after his death, Pierre Bourdieu, the most cited sociologist (Korom 2019), continues to be admired and criticised in his homeland. In a stream of recent French publications devoted to Bourdieu, one in particular caught my attention and seems to be interesting to a English-speaker audience. *Return to Reflexivity* aims to come back to a central problem of Bourdieu's work: the practice of scientific reflexivity. In the introduction, the editors, Johan Heilbron and Jérôme Bourdieu, recount the elaboration of this concept as a way of doing social science. Bourdieu's scientific project is guided by reflexivity. Long before using the word, the idea emerged in his first fieldwork during the war in Algeria (Perez 2023). Then, the concept appeared in the transatlantic context of the scientific exchange with Loïc Wacquant (Fringuant 2025). Finally, the idea culminated in his last course at College de France, *Science of Science and Reflexivity* (Bourdieu 2004). Throughout his life, the sociologist sought to establish a new conception of scientific reflexivity based on sociological knowledge. In doing so, he promoted the need for a squared sociology (or sociology of sociology) in order to pursue the progress of scientific reason. By its mirroring effect, this project of sociology applied to social scientists invites us to intellectual humility and reveals the ethical dimension of Bourdieu's oeuvre.

FROM PHILOSOPHICAL EPISTEMOLOGY TO SOCIOLOGY OF SOCIOLOGY

The book is a compilation of four texts, among which the first three were originally speeches. The first, from 1967, written in the same period as *The Craft of Sociology* (Bourdieu et al., 1991) is titled 'Epistemology and Sociology of Sociology'. This text is Bourdieu's proposal for a debate organised at Sorbonne University on using model analysis in human sciences. He came to this event with a performative intention, or more precisely 'with a lot of ulterior motives' (p. 34), in the same vein as his later *Leçon sur la leçon* (Bourdieu 1982). His intention was to provoke the audience to think critically about the topic of the debate. He then transforms the question of models from a theoretical problem to one that is sociological. This is the subversive and critical aspect of his thesis: refusing to solve theoretical problems with philosophical discourses on science and replacing them with the question of the sociologist's social insertion into his work. He writes that the problem of the theory of knowledge applied to sociology 'is not so much a question of answering the question of its scientificity, but, more concretely, of helping itself to move in the direction of its scientificity' (p.

38).[1] When locating this discourse in the French intellectual field of the time, one can see the connection with Bachelard's idea of a psychoanalysis of the scientific mind, aimed at returning scientific reason to science itself (Bachelard 2002). Moreover, for Bourdieu, as for Bachelard, 'every discourse on method is a contextual discourse' (p. 42). Here his opponents are both the methodologists without data and the sociologists without epistemology.

Bourdieu then attempts to outline a research program in the sociology of scientific practice, more specifically in the sociology of sociology. This project will show how the epistemic unconscious, or our specific ways of thinking, are linked to their social conditions of possibility. He writes: 'Sociology encloses within itself the power to reflect on itself and in particular to reflect on its own scientificity' (p. 34). It is this mirror effect of sociology applied to the scientific activity that becomes his leitmotif in the practice of social science. Rather than formulate theoretical answers to the problem of the use of statistical models in sociology, Bourdieu prefers to question the mathematical skills of those researchers who pose such a theoretical problem. Taking the reflexive analysis of sociological production a step further, he states: 'It would also be easy to show that a certain type of social organisation of intellectual work generates a certain type of epistemology' (p. 38).

Similarly, Bourdieu applies this sociological lens to one of the classical problems in the theory of scientific knowledge: determinism. He writes:

> Sociology could go further, until analysing for example the affinity which can exist between an epistemological position and a social position: these points of view on the problem of the determinism in the human sciences are not undoubtedly distributed randomly but according to the social insertion of the researchers, according to their social origin, etc. (p. 39).

The final aim is to establish a collective scientific program which would serve the progress of social science itself: 'the sociology of sociology or, more precisely, the sociology of the social conditions of the production of sociological sciences is one of the fundamental conditions to the progress of sociological knowledge' (p.38).

SCIENTIFIC REFLEXIVITY AGAINST NARCISSISTIC ONES

The second text, published solely in Germany in 1993 and entitled 'Scientific Reflexivity and Narcissistic Reflexivity', is dedicated to his student and friend Loïc Wacquant. In his position statement, Bourdieu defends his own conception of reflexivity in relation to those of his competitors in the international academic field. His argument can be summarised as follows: first, to reject the outsized reflexivity of neo-Marxists such as Georg Lukács or Karl Mannheim. For Bourdieu they both culminated in what he called a 'paralogism of the short-circuit' (p. 52), by ignoring the autonomy of the field of cultural production (Bourdieu 1993). In his view, this type of analysis tends to reduce intellectual production to the social characteristics of its producers. But Bourdieu also seeks to distance himself from another form of intellectual reflexivity, the textual narcissism of 'postmodern' anthropology and sociology. Rather than the mirror effect of social science, these theorists often prefer the emphatic depiction of their unique reflection. Bourdieu's conception of reflexivity is completely different:

> The form of reflexivity that seems to me to be the most scientifically fruitful is quite paradoxical, in that it is profoundly anti-narcissistic. This is undoubtedly one of the reasons why it is so little

[1] All translations are the review author's.

> practiced and why its products arouse so much resistance. The properties that this sociology of sociology discovers, all opposed to an intimate and complacent return on the private person of the sociologist, have nothing singular nor extraordinary; they are common, for a part, to whole categories of researchers (therefore banal and not very "exciting" for the naive curiosity). This sociology questions the charismatic representation that intellectuals often have of themselves, and also their propensity to think of themselves as free of all social determinations. It makes us discover the social in the heart of the individual, the impersonal hidden under the intimate (p. 51).

Here the French sociologist comes close to Norbert Elias's criticism of *homo clausus*, this individualistic conception of humans being derived from Western philosophy. Bourdieu's scientific project to establish sociology applied to the sociological field proposes to reject the last great narcissistic wound inflicted to humanity (Bourdieu 1992: 132; Kilminster 2007: 154-155), born with the sociological revolution (Kilminster 1998; Joly 2017). The idea is simple: we are never alone; people are always interconnected. Contrary to the modern myth of the scientist as a solitary genius, the sociology of scientific activity reinserts the products and the producers within their socio-historical conditions of possibility into a field, a specific social space with its own rules. For Bourdieu, this realistic view of science could, at best, counteract intellectual arrogance, all the while enabling the development of a collective scientific practice in which everyone knows their strengths as well as weaknesses according to their education and technical abilities. In this perspective, the sociologist once notes: 'My dream would be a scientific field in which - and this is particularly necessary in the social sciences - everyone is a socio-analyst of others, but not in a polemical logic' (Bourdieu 2015: 357).

Bourdieu goes on to distinguish three levels in the reflexive analysis of scholarly production. The first is to emphasise generic social properties (age, gender, language, class, education) of the agent. Then, the reinsertion of his production and his social and intellectual positions within the struggles that structure the state of the field (p. 53) at a given moment. Finally, to methodically erase all the biases inherent to the scholastic position. This bias consists in the idea that in order to study the social world, we must necessarily adopt an abstract and distanced glance. But this view ignores the practical and concrete relationship that those we study have with the social world (p. 54). Here appears the difference between doing something, and saying something about what someone is doing. Sociological analysis must necessarily consider these two issues. Without a theory of practice, the sociologist runs the risk of not being able to go beyond the description of the social action derived from common sense. On the contrary, the scholastic fallacy consists in putting 'a scholastic head into non-scholastic bodies' (p. 79). The best example of this scholastic bias in social science is rational choice theory, which assumes that all agents act rationally.

THE SOCIOLOGY OF SOCIAL SCIENCES AS A RESEARCH PROGRAM

The third text, 'Project for a Social History of Social Sciences', is the introduction to the seminar Bourdieu held at the School for the Advanced Studies in the Social Sciences in 1997. Bourdieu appears here as a team leader, or a 'rugby coach' (p. 69). In this text, it is the pedagogy of sociology that becomes reflexive. In a kind of double discourse saying both what he would like to do, and why it is necessary, but also why there are so many obstacles standing in his way, Bourdieu tries to make the practical necessity of reflexivity clear to students. Close to Goffman's analysis of the Lecture (Goffman 1981:166-196), the French sociologist outlines reflections on what a seminar is from the point of view of research communication. He also points out the social constraints on any form of collective organisation. Pedagogically, his point is that 'the teaching of research is done by

researching' (p. 64). The teacher's ambition then, is not to train disciples, as well-disciplined Bourdieusian's *lectores*, but rather to discipline them to indiscipline, and thus to form *auctores*. The latter are in short: 'people capable of appropriating methods, theories, readings, in order to make something of them' (p. 64).

But how to teach research? How to teach this specific practice? These questions are still relevant today. Bourdieu is certainly not unaware of the intricacy of after mentioned questions, and even goes so far as to speak of religious 'conversion' (p. 65). He does not stop showcasing examples to reiterate his point. Thus, the young researcher is compared to a 'top athlete' (p.18). He/She/They must not cease to train tirelessly, to organise themselves methodically, and to criticise themselves, in order to perfect the art of doing research. All this work takes place in a dialogical relationship, which is sometimes tense, but always regulated by one's peers and competitors (agents of the same field). This intervention also takes on an ethical dimension. The professor cautions students against the risk of a cynical use (p. 90) of knowledge when it comes to the history of social sciences or the sociology of intellectuals. This cynical use is made most evident when observing polemical debates within the intellectual fields. Sometimes biographical details or economic interests are revealed in order to discredit certain works rather than develop a sociological analysis of the scientific field. Bourdieu invites his students to move away from this form of 'perversion' (p. 89), this illegal use of social science where the purpose is to nail an opponent or legitimise an ally rather than analyse the social phenomenon under study.

TOWARD A SCIENTIFIC REAL POLITIK?

The last text was first published in 1995 and served as an introduction to two issues of *Actes de la recherche en sciences sociales*.[2] These issues were devoted to 'The social history of social sciences'. Unlike the other speeches, this text, conceived as a piece of writing, could be difficult for anyone who has not mastered Bourdieusian rhetoric. This text is a plea for the history of social sciences. In Bourdieu's view, this project contributes to the progress of reason through reflexivity it gives us about our scientific practices.

> Social science has the privilege of being able to take its own functioning as an object and being able to bring to consciousness the constraints that weigh on scientific practice: it can thus make use of consciousness, and of the knowledge that is has of its functions and functioning, to try to remove certain obstacles to the progress of consciousness and knowledge. Far from ruining its own foundations by condemning to relativism, as it has often been said, such a reflexive science can on the contrary provide the principles of a scientific Realpolitik aiming at assuring the progress of the scientific reason (p. 98).

Promoting the unification of a global scientific field, this fourth text takes on performative, programmatic (Camic 2011: 276) and utopian dimensions. In Bourdieu's view, the awareness of the social constraints on scientific life leads to the hope of being able to overcome them collectively. The sociologist reiterated the need for a comparative history of disciplines and national traditions in the social sciences, a scholarly program that later flourished among his students (Fassin and Steinmetz 2023). Bourdieu then calls for the organisation of 'socially instituted forms of communication that favour the production of the universal' (p. 124). This text also contains one of the most beautiful formulations of the connection between the scientific field and individual reflexivity: 'A point of view that perceives itself as such, that is, as a view taken from a point, a position in a field, is able to

[2] The journal of Bourdieu and his collaborators.

overcome its particularity; in particular, by entering into a confrontation of differences in vision based on an awareness of the social determinants of these differences' (p. 122-123).

BOURDIEU BEYOND BACHELARD

At the end of the reading, Bourdieu seems faithful to the Bachelardian epistemology in which he was trained by Georges Canguilhem, and which strongly inspired the *Craft of Sociology*. The main legacy of this epistemology is to propose a reflection on scientific practice against all idealistic epistemologies. This kind of epistemology became later, in Bourdieu's vocabulary, a 'science of science'. It is a regional theory of knowledge built out of the scientific activity itself rather than a unified theory of knowledge based on a set of abstract propositions founded on the idea of an immutable reason and an isolated and transcendent knowing subject. But Bourdieu, as a philosopher converted to sociology at a time when the social sciences expanded in France (Heilbron 2015), went beyond Bachelard. Bachelard was close to a sociology of science when he talked about the 'social relations of science' (Bachelard 1972: 54) and the intersubjectivity of knowledge within the 'scientific city' (Bachelard 1951: 16). But Bourdieu went further and offered a coherent sociological model with his conception of the scientific field (Bourdieu 1975). Likewise, in analysing our epistemic unconscious, Bourdieu transformed 'the psychoanalysis of the objective knowledge' (Bachelard 2002) into the sociology of the sociology.

Among scholars who are intimately familiar with Bourdieu's work, these four short texts will not radically change their views. But others may discover a new dimension of it, related to a central problem of social science: the fact that sociologists are both subject and object of their scientific inquiry. According to Bourdieu, this social situation must be controlled by an ethical practice of scientific reflexivity, which means always applying the tools of sociology to sociologists themselves.

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BOOK REVIEW

Geertz, Clifford (2022) Sūq: Geertz on the Market, edited and introduced by Lawrence Rosen

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ISBN: 9781912808984. E-book freely downloadable at: https://haubooks.org/suq-geertz-on-the-market/

Originally issued as part of the 1979 collective volume, *Meaning and Order in Moroccan Society. Three Essays in Cultural Analysis* (Geertz *et al.* 1979), Clifford Geertz's long *tour de force* on the *bazaar* economy has been published in 2022 as a standalone book by HAU Books and the University of Chicago Press with a solid introduction by the Princeton anthropologist, and Geertz's longtime collaborator and friend, Lawrence Rosen. Preceded by a number of works on Indonesian *pasars* (see Geertz 1963), the piece builds on Geertz's ethnographic work in Sefrou, Morocco, to challenge the (general) understanding of *bazaars* as irrational phenomena or transitional stages towards more advanced (i.e., Western-like) market systems. Combining his trademark interpretive anthropology and information theory, Geertz argues that *bazaars* are in fact coherent and meaningful economic institutions whose defining element is the fact that "information is generally poor, scarce, maldistributed, inefficiently communicated, and intensely valued" [3]. The practices and institutions typical of the *bazaars* might be explained as emerging from the search for reliable information to which all traders are incessantly committed. Following this premise, the essay is a rich piece of work: after a short theoretical introduction, Geertz traces the historical evolution of the *sūq* of Sefrou from a stop on a long caravan route to a complex, articulated economic system. The *sūq*, as he observed it between 1964 and 1976, included permanent shops, a weekly market, and Western-style stores all rooted in a host of common economic and religious institutions [4 ff.].

In a clear but understated criticism of Ernst Gellner's "segmentary" model (Gellner 1969), Geertz explores the "mosaic pattern" of Moroccan society [20] and the economic structures, roles, and norms of the market before getting to the meat of his contribution [52 ff.]: a detailed analysis of the meaning of, and the connections between, ten keywords operating as the "model of" the bazaar for its regular dwellers and the "model for" they action within it [82 ff.]. These include descriptive terms such as *zḥām* (crowd), *klām* (words), and *ḵbar* (news); evaluative principles as *ṣdiq* (candor), *macrūf* (consensus), and *ṣḥīḥ* (wholeness); and ways to frame a judgment like *macqūl* (reason), *ḥaqq*

(reality), *bāṭel* (deception), and *kdūb* (vanity). This semiotic analysis reveals the emic vision of the market as a space crowded with rivals, words, and news, where information must be evaluated according to social and theological standards and reasoning. It also explains why day to day exchanges are mainly addressed via two practices—clientelisation and bargaining [101 ff., 104 ff.]—which Western tourists and dwellers have seen as "exotic" (and maybe even "amusing") but that are perfectly rational, for they serve to reduce the complexity of a system lacking stable and unambiguous informational processes. In conclusion, Clifford Geertz sees (and makes us see) the *sūq* as a sophisticated and functional economic institution, adapted to its specific cultural and communicative conditions [79 ff., 95 ff.], challenging simplistic or evolutionary views of non-Western market systems. His analysis demonstrates the intricate interplay between social dynamics, economic structures, and cultural meanings in shaping the *bazaar*'s unique character and functionality [117 ff.], and has been regularly taken into account by economic anthropologists or economists working on this kind of institutions.

To the reader who now holds this new edition of *Sūq* in her hands, a number of features stand out immediately. First of all, following Geertz's 1960s habits, this is a book where the ethnographer and the people he met are nowhere to be found. Although the author can effortlessly weave historical description, meaningful categorisations, multi-level analyses, and witty turns of phrase, he is curiously stingy with details, stories, and dynamism. This might be far from current ethnographic styles and sensibilities: as said by Corinne Cauvin Verner (2014: 13), "missing are the smells of the market, the atmosphere, the movement of cattle and *suwwāqs* [i.e. *sūq*-dwellers] by truck, moped or donkey; missing is the misery of rural life in the face of urban opulence, and all the negotiations that make the market a place for capitalising on dysfunctions. It's hard to see what's legitimate and what's illegitimate. Neither disorder nor explosion of neutralities are reported." It seems like Geertz is taking to the extreme his often-quoted aphorism that anthropologists do not study villages but *in* villages—but here the "village" of Sefrou becomes a web of terms, words, concepts, labels, categories, and structures, with no humans in sight.

To the 2025 reader, the lack of flesh and blood Sefrouis sounds like a symptom of a wider methodological flaw in Geertz's practice of interpretation and thus in the very practice of interpretive social science—a flaw that, to be sure, has been variously addressed in the nearly fifty years that separate us from the first edition of the essay (Ortner 1997; Alexander et al., 2011). As I read this and other Geertzian texts, the real issue lays in the fact that Geertz lacked a theory of epistemic attribution, so that he ended up shifting from a phenomenological perspective to a more "objectivist" stance (see Crapanzano 1981), oscillating between "experience-near" and "experience-distant" concepts—the declination of Heinz Kohut's emic/etic distinction he put forward in "The Native's Point of View" (Geertz 1974: 28). Here the lack of real people doing real things within real contexts was paradoxical but not incidental: instead of reflecting on the existence of multiple points of view and their delicate relationships, Geertz emphasised the "objective" nature of his subject as an ordered system, stressing the connectedness and the *mise en forme* of the whole. Sure, he added, "the constraints are partial, the logic approximate, the motivations incomplete, the comprehensibility limited—and the system thus, as all social systems, far from fully coherent." The *sūq* remained nonetheless "an ordered whole with properties of its own" that might be observed, analysed, understood, and written down in a properly scientific report [95]. If the *sūq* is a system, the observer might well abstract it from real life and extract words, concepts, and schemes from their contexts and their peculiar (and particular) operational rules as to present an ordered semiotic system that is then translated into a (ideal typical) mindset and/or a bundle of (ideal typical) emotions and sentiments. The universal, or at least some level of it, ends up prevailing on the particular—an

anathema not only to an author who, like Clifford Geertz, wanted to defend the specificity of cultures, individuals, practices, and institutions, but also, I suspect, to the majority of contemporary ethnographers and social scientists.

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BOOK REVIEW

Pascal Génot and Olivier Thomas (2023) Bourdieu: Une enquête algérienne, Paris: Steinkis

Matthias Fringant

Pascal Génot and Olivier Thomas (2023) Bourdieu: Une enquête algérienne, Paris: Steinkis.
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Graphic novels are not among the usually reviewed works in scientific journals. At best, they seem to be means of vulgarising existing knowledge and will rather provoke suspicion on the part of professional academics. Graphic novels specifically dealing with the history and sociology of the human and social sciences are not new (see for instance Goodwin and Burr 2012). Some, by focusing on specific thinkers (for instance Maier and Simon 2012, 2013), can be understood as a stage of the latter's canonisation. Pascal Génot and Olivier Thomas's *Bourdieu: Une enquête algérienne* is somewhat different. In an original way, it contributes not only to a series of recent publications on Bourdieu's early sociological work in colonial Algeria (Pérez 2023 [2022], Yacine 2022), but also to the history of sociology in colonial contexts (Steinmetz 2023).

The French graphic novel relates Pierre Bourdieu's (1930-2002) experience of Algeria as a soldier and a social scientist during the war of independence (1954-1962). Written by screenwriter Pascal Génot and drawn by cartoonist Olivier Thomas, the book rests upon a substantial documentary inquiry conducted with the help of Saadi Chikhi. Perhaps in connection with the current interest for the history of (de-)colonisation, the novel has been a commercial success with not only four reprints and more than 10,000 copies sold, but also a large and enthusiastic media coverage.[1] In addition, at least two positive reviews have been published in established academic journals.[2] As successful as this reception seems, it has nevertheless been limited to the French-speaking world. To my knowledge, there has so far been no English review dedicated to the novel.[3] In what follows, I shall first describe the book – its aims, its approach and its main contents following its original plan – before reflecting on how it can contribute to two larger issues: the specific contribution of graphic novels to scientific knowledge and its diffusion; and current debates on the relationship between (history of) sociology and colonialism.

[1] In France, the book has been positively mentioned by at least three media focusing on cartoons (*ActuaBD, BubbleBD* and *BDZoom*), one regional newspaper (*Sud Ouest*) and four nation-wide media (*Libération, Slate, FranceTVInfo, TV5Monde* and *Fnac*).

[2] First, by sociologist Joachim Benet Rivière in *Images du travail, travail des images* (Benet Rivière 2024), and shortly afterwards by linguist Smaïl Djaoud in *Lectures* (Djaoud 2024).

[3] An exception is an unpublished text by sociologist Johan Heilbron entitled "How to decolonise social science? Pierre Bourdieu, Abdelmalek Sayad and 'colonial sociology'".

As Génot himself explains at the beginning of the book in a sort of *mise en abyme*, the story started when he was invited as a screenwriting teacher to the 2011 edition of an international comics festival in Algiers. By then, he had already written two fictional comics and was thinking about starting a documentary one. This idea grew as he stumbled upon a mysterious photograph of Algerian writer Mouloud Feraoun (1913-1962) and Pierre Bourdieu. Shot around 1958 in Algiers, the picture featured in a book dedicated to Feraoun. Génot was intrigued as he was acquainted only with Bourdieu's later and more widely known works. He started reading a few studies on the relationship between the sociologist and Algeria (for instance Bourdieu 2008 and Martin-Criado 2008). This turned out to be the first stage of a proper inquiry. During an eight year period, Génot not only systematised his readings on the topic but also made three trips to Algeria (in 2014 and 2015), allowing him to take photographs and conduct more than twenty interviews with witnesses and researchers, some of whom shed new light on this matter. Génot also accessed the French national archive dedicated to French colonial presence overseas located in Aix-en-Provence.

In treating his abundant material, Génot had a particular example in mind: Maltese American cartoonist Joe Sacco's *Footnotes in Gaza: A Graphic Novel* (Sacco 2009). In this pioneer documentary comics, Sacco articulated original research work in Gaza with the narrative of the slaughter of Palestinians in Gaza in 1956. Génot tried to build upon this strategy by choosing an auto-fictional posture, in which nothing is purely fictional but most of the events are re-imagined. In six mainly chronological chapters, the novel thus alternates between the story of the inquiry conducted by the auto-fictional character named Pascal and Pierre Bourdieu's story in Algeria.

The first chapter recounts both the history of Pascal's inquiry and Bourdieu's biography until the latter's arrival in Algeria in 1956. The reader learns several well-known facts about Bourdieu's life. Pierre Félix Bourdieu was born in the rural village of Denguin in South-West France in 1930. He was the only child of Noëmie, coming from a family of landowners, and Albert, coming from a peasant's family. The latter dedicated his life to his job as a postman in Lasseube, another small village nearby Denguin where the Bourdieus moved shortly after Pierre's birth. Pierre was a talented, albeit agitated, pupil. From 1941 to 1947 he became a student in a boarding school at Pau – one of the major cities in the area. Thanks to his good grades, he was admitted in 1948 to *Louis-le-Grand* in Paris, by then one of the most prestigious schools of the country preparing students for the entry exams for the *Grandes Écoles*. In 1951 he was admitted to *École Normale Supérieure de la rue d'Ulm*, once again an institution at the top of the French educational system. In 1954, just after he obtained the *agrégation de philosophie*, one of the most difficult national academic competitions, he became a philosophy teacher in a high school in Moulins, a small city in the middle of France. He also started a PhD in philosophy under the supervision of philosopher of science Georges Canguilhem.[4] As promising as this career in philosophy seemed to be, it was cut short by war. In late 1955, Bourdieu had to stop teaching as he was called to arms after the beginning of Algerian war of independence. These biographical facts as such are not new. What is new is the way in which they are narrated. Indeed, the graphical presentation enables the reader to relate to young Bourdieu in a livelier way than academic books may be able to do.

Having set the biographical context, Chapter two briefly describes Bourdieu's first moments in Algeria. After a military preparation in Germany and France and a hard boat trip, Bourdieu landed in Algeria in the spring of 1956. From there, he was sent to an air base in Orléansville, between Algiers

[4] Up to this day, the best works on the formation of the young Bourdieu are from Collard (2021, 2022 and 2024).

and Oran. As Chapter three explains, Bourdieu's stay in Orléansville was very short. He was immediately sent to Algiers to the *Gouvernement général*, a central institution of French colonial power. There he wrote texts for the information service in the context of the *action psychologique* – an expression used by the French army to describe war propaganda – until the end of his military enrolment in late 1957.

Being uncomfortable working for the French army in Algiers, Bourdieu started thinking about a sociological project on the transformations of Algeria under the effects of colonialism, an idea that would lead to *Sociologie de l'Algérie* (Bourdieu 1958). Thanks to the help of historians like André Nouschi (1922-2017), Bourdieu aimed to understand in a more accurate way (than for instance Raymond Aron and Germaine Tillion) what was actually occurring in Algeria (see Aron 1957 and Tillion 1957). Thanks to an insightful interview, Génot can establish that Fadhila Sahraoui, an ex-activist for the liberation of Algeria, and Bourdieu met in Paris in spring of 1958. Sahraoui explains that she met a very informed albeit tormented Bourdieu who asked her how he could help, before he went back to Algiers, not as a soldier but as a philosophy teacher. As noted by Smaïl Djaoud (2024), this important information is nowhere to be found in previous studies on Bourdieu's work in colonial Algeria.

Chapter four recounts that when he came back to Algiers in late 1958, Bourdieu had published *Sociologie de l'Algérie*, a short synthesis relying extensively on the existing literature and published in the prestigious "Que sais-je?" book series for vulgarising knowledge at the Presses universitaires de France. As a philosophy teacher at the University of Algiers, Bourdieu met students and soon-to-be collaborators like Fanny Colona, Alain Accardo and, most importantly, Abdelmalek Sayad (1933-1998). Thanks to interviews, Génot uncovers the story of the picture that triggered his own inquiry. Shortly after its publication, Bourdieu gave a copy of *Sociologie de l'Algérie* to Mouloud Feraoun and then visited him on multiple occasions to ask him questions about Kabyle customs and discuss the book. The picture was then taken by one of Feraoun's daughters.

Chapter five centres on Bourdieu's first anthropological and sociological inquiries that started in mid-1959. In the context of the Constantine Plan, a social and economic program introduced in 1958 by President Charles de Gaulle to lessen the gap between citizens of French Algeria and Metropolitan France, French colonial administration needed information on the Algerian population. Bourdieu received grants to begin some ethnographic inquiries to complete the statistical parts conducted by French statisticians. In the meantime, after the publication of *Sociologie de l'Algérie,* he was contacted by Raymond Aron who had been impressed by the book. The latter asked Bourdieu to help him run a new social science research centre in Paris – what should become the *Centre de sociologie européenne* (Duval, Heilbron and Issenhuth 2022). Between late 1959 and 1961, Bourdieu thus made round-trips between France and Algeria. As he was only partly in Algeria, Abdelmalek Sayad supervised the inquiries about the effects on colonialism on the life conditions of Algerian workers. Out of this work would later emerge *Travail et travailleurs en Algérie* (Bourdieu, Darbel, Rivet and Sebel 1963 [2021]). Chapter six then discusses Bourdieu and Sayad's inquiries in Kabylia that started in mid-1960. The two sociologists tried to focus specifically on the long-term effects of displacements on the Algerian population. They published their results in *Le Déracinement* (Bourdieu and Sayad 1964).

Finally, Génot also develops the legacy of the inquiries conducted by Bourdieu, Sayad and their collaborators. Bourdieu came back to Paris in the middle of the year 1960 to become Raymond Aron's assistant at the Sorbonne. While Sayad continued fieldwork on his own until 1963, Bourdieu used the research techniques learned in Algeria to initiate a new body of inquiries in France, all while

reflecting on a possible theoretical generalisation. Via further transnational mediations, this attempt led to what would later be called "reflexive sociology" – a sociology wary of its own conditions of production and the use of social scientific knowledge.

A second legacy of Bourdieu and Sayad's works is, according to Génot, how they can be used to think about postcolonial Algeria. While they were doing fieldwork, Bourdieu and Sayad engaged in a dialogue with revolutionary thinkers, with whom they disagreed. For instance, Fanon's thesis on peasants and workers as revolutionary forces (Fanon 1961) was wrong in their eyes. In some of their works, Bourdieu and Sayad emphasised the fact that a revolutionary consciousness requires a kind of stability nowhere to be found among peasants and workers in Algeria. Moreover, they feared that power could be seized by the actors of the revolution. History proved them right, and this might be important to reflect on the historical roots of contemporary movements of protest in Algeria (such as Hirak in 2019, as Génot suggests at the end of the novel), bridging the gap between past and present.

If not immune to criticism,[5] *Bourdieu: Une enquête algérienne* can be the starting point for a set of two reflections for scholars in the history and sociology of the social sciences. The first has to do with the specificity of graphic novels in relation to scientific knowledge. It is often said that graphic novels are an alternative kind of writing that could have a broader impact than conventional books. In this perspective, Génot's graphic novel might be interpreted as an interesting case of the diffusion of the history of the social sciences to the wider public. Interestingly, Génot himself (2024), however, contests this idea. According to him, as far as impact is concerned, graphic novels are likely to be less efficient than other media such as videos.[6] If graphic novels as a way of broadcasting science on a large scale seems endangered by other modes of expression, Génot sees their specificity in how they can experiment with ways of narrating reality. In the case of *Bourdieu: Une enquête algérienne*, the articulation of two interwoven narratives – inspired by Joe Sacco's solution – seems to be a fruitful way to introduce to an important yet not well-known part of an œuvre that can be intimidating by its sheer scope. By linking Bourdieu's inquiries on Algeria to his own investigation, Génot found an interesting way of both questioning the timeliness of a relatively esoteric work and bringing it to a larger audience that might be interested in the topic of colonialism.

The second point is related to the history of the social sciences. As we have seen, the book both rests upon and represents an original contribution to the recent literature on Bourdieu and Algeria. But it can also be used to think about a larger debate on (the history of) sociology and colonialism. For instance, in a quite influential book dealing with this topic, Australian sociologist Raewyn Connell (2007) calls for more equal patterns of knowledge circulation after a long history of either ignorance or data mining of the global South by Western social scientists. To make her point, Connell centres on three of these scientists, James Coleman, Anthony Giddens and Pierre Bourdieu. Her analysis draws upon the reading of one of each author's book, in the case of Bourdieu *The Logic of Practice* (1990 [1980]). In Connell's eyes, Bourdieu's book is an exemplary attempt of a so-called universal social theory both built on data mining from Algeria but also ignorant of Algerian thinkers and their problematics. In 2007, when she wrote *Southern Theory*, English translations of Bourdieu and

[5] In his review, Smaïl Djaoud notes a couple of factual inaccuracies and regrets that Génot did not make a more systematic use of the French national archive.

[6] This seems to be true if one compares the 10,000 copies of the book sold to the number of views of one of the main French sociological Youtubers, Gregoire Simpson. Simpson uses his videos to introduce the works of various sociologists, for instance Pierre Bourdieu's *Masculine Domination* or Raewyn Connell's *Masculinities*. The first video reaches 42,000 views, the second one more than 150,000.

Sayad's empirical works were unavailable. Some of them such as *Travail and Travailleurs en Algérie* still are, but *Le Déracinement* has recently been translated to English (Bourdieu and Sayad 2020 [1964]). Not only these new translations but also a current broader interest for the role of the social sciences in colonial contexts might help foster a better global understanding of Bourdieu's work in and on colonial Algeria. Contrary to Connell, Julian Go has suggested in an article and a book that Bourdieu's early work develops a fully-fledged theory of colonialism and thus anticipates later developments found in postcolonial theory (Go 2013 and 2016). More recently, through a broad empirical study focusing on what he calls the French subfield of colonial sociology, George Steinmetz has drawn on Bourdieu's tools to offer new insights on the large categories and oppositions shaping the current debates about social sciences and (post)colonialism (Steinmetz 2023). Through the focus on Bourdieu's theoretical synthesis in *The Logic of Practice*, his early work has too often been reduced to an abstract social theory (for the various studies coming together in Bourdieu's theory of practice, see Heilbron 2011). After work done by others, Génot's graphic novel can help to understand Bourdieu's work in Algeria rather as an original endeavour that might still be relevant for studying the complex conditions of production and use of the social sciences in both colonial and postcolonial contexts.

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