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Knowledge and Advocacy in Trade Union Research

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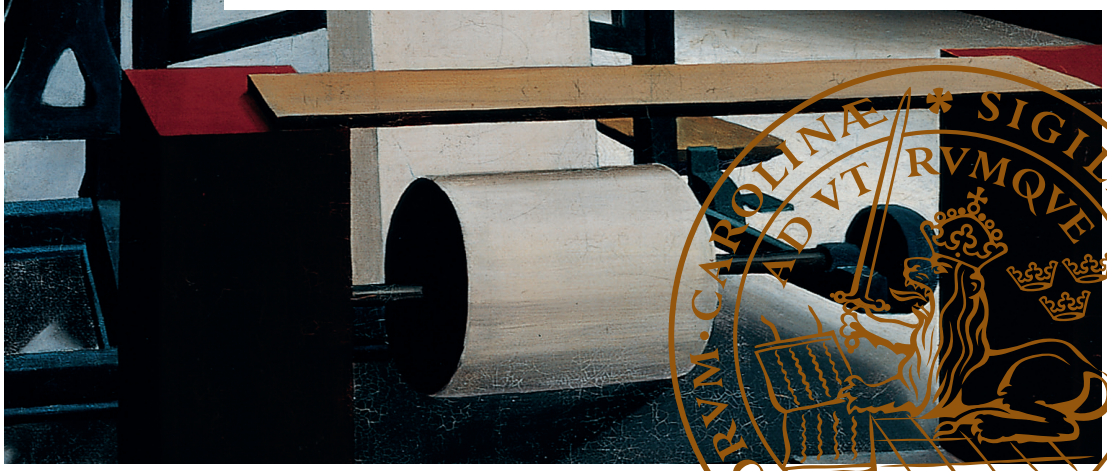

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Knowledge and Advocacy in Trade Union Research

Staffan Edling

LUND
UNIVERSITY

DOCTORAL DISSERTATION

Doctoral dissertation for the degree of Doctor of Philosophy (PhD) at the Faculty of Social Sciences at Lund University to be publicly defended on the 6th of December at 10.15 in Edens hörsal, Allhelgona kyrkogata 14, Lund

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Abstract: Political organisations routinely, and increasingly, employ researchers to make knowledge claims and suggest political action. Swedish blue-collar trade union organisation LO has done so since the 1930s, employing economists and other social scientists drawing in part on methods of analysis and presentation similar to academic social science, and in part on interaction with spokespersons for trade union members to produce texts and other output which they treat as both knowledge-production and advocacy. Starting from assumptions and perspectives from the sociology of science and science and technology studies, I analyse the activities of political researchers in the organisation of LO, drawing on interviews, participant observation and printed documents. Special attention is paid to the interaction between political and epistemic phenomena in their research, and the situational boundaries drawn between the two, from three different angles: in the production of different kinds of linguistic statements, in LO researchers' claims to speak for union members, and in how LO research relates to academia, politics, and other sites as social worlds external to themselves. Through these different analytical angles, I argue that political research at LO involves the interaction between the political representation of union members on the one hand, and the epistemic representation of reality on the other, and occasionally the dissolution of the boundary between the two. As the distinction between speaking for union members and speaking for reality is contextually bound, and at times dissolved, questions of causality between politics and knowledge can only be answered situationally, challenging common assumptions of political research as either value-free or reducible to political strategy.

Key words: trade unions, LO, policy research, political epistemology, democracy, expertise

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1. Introduction

This study is a case study of political research done at the Swedish trade union confederation LO. Political research is a combination of words that seem to go together rather uncomfortably. They evoke and mobilise opposite parts of a number of dichotomies at the heart of Western societies: values and facts, representation of political subjects and of reality (Soneryd & Sundqvist, 2023), society and nature (Latour, 1993b). Nonetheless, political organisations of different sorts – parties, think tanks, and interest organisations – commonly employ researchers and experts to both produce factual accounts of society, to act as advocates and to develop political ideas. Such experts often draw on the methods and literary techniques of social science when writing reports, memos and other documents, producing versions of the world their organisation acts in. They produce representations of reality while also representing the political interests of their employer, or those their employer speak for. They are neither apolitical experts speaking truth to power, nor purely political creatures seeking only to maximise power, truth be damned.

Despite the widespread existence of such partisan experts in Western democracies, social scientists have struggled to make sense of them as being both producers of knowledge and political actors, to the extent that we acknowledge their existence at all. In this thesis, I aim to provide an analytical account of the work of such political researchers, drawing on interviews and participant observation with Swedish trade union researchers, and analysis of published reports and other documents. In this account, I am trying to understand how the production of epistemic claims by these researchers interacts with different kinds of politics within the organisation, or, in other words, the political epistemology of the organisation.

Political epistemology of LO

LO is the confederation of blue-collar trade unions in Sweden, organising all major blue-collar unions on the national level. As the Swedish labour market

is characterised by a high degree of unionisation and collective bargaining between organised labour and employers, and as LO is closely tied to the country's largest political party – the Social Democratic Party – they are an important actor on the Swedish labour market and in Swedish politics. LO has employed researchers since the 1930s for research, advocacy and the development of policy; at first economists, and later other social scientists as well.

The work of these researchers entails a number of interactions between the production of knowledge, the political views of the organisation and the collective interests of union members. They are tasked with doing research which is of relevance to the members of the confederated trade unions, and which furthers their collective interests. In doing this, they produce reports and other texts which contain both epistemic accounts of society and policy statements which come to be taken as the shared beliefs of LO as a corporate body, and the expressed will of working-class organised labour. The knowledge produced is used both within the organisation, guiding their political positions and strategies, and communicated to others both within and outside of the trade union movement, in order to influence politics, public discourse and collective bargaining. In order to do their job, LO researchers need to know how to find relevant and trustworthy statistical data and theoretical explanations from academic and non-academic sources, how to find out what the members' collective interests are, and what politics and policies the organisation supports. How these different things interact with each other in the work practices of LO researchers is the object of study of this thesis.

The broader topic of this thesis, and what I take LO research to be a case of, is political epistemology; I am interested in how understandings of the world are made in political organisations, and in how actors in these organisations produce and work with knowledge. The kind of knowledge I am interested in is a form of specialist or expert knowledge, more specifically social scientific technical knowledge: epistemic claims produced by researchers who work with methods of analysis and writing similar to those found in academic social science, drawing on their university training. There are other ways of knowing the world in LO, as in other political organisations. The president of the organisation, for example, spends a significant part of their working life – about two days per week in the estimate of one person working close to the LO leadership I talked to – visiting workplaces and regional offices, talking to union representatives about the ongoing struggles and issues at their workplaces. While this makes the president remarkably, maybe uniquely, informed about certain aspects of the Swedish labour market, this knowledge is not printed in propositional form supported by methodological discussions

and references to other published documents. The kind of knowledge production I am interested in is close in kind to academic social science, and because of that this thesis primarily draws on, and is in communication with, the sociology of science and science and technology studies.

There are two main arguments for the value of research on the knowledge production of political researchers. The first one is that the number of political experts and researchers has increased rapidly in recent decades, not least in Sweden (Garsten et al., 2015), making them an important part of the political machinery in contemporary so-called knowledge societies. In addition, as political researchers have access to means of dissemination of their epistemic claims through political organisations and public debate which most academic social scientists do not, they can be assumed to be comparatively important for how people understand society in general. While significant attention in the social sciences has been paid to think tanks (e.g. Åberg et al., 2021; Medvetz, 2012; Sörbom, 2018; Stone, 1996) and to policy experts generally (e.g. Garsten et al., 2015; Svallfors, 2020), the tendency has been to study how such organisations and experts form networks and influence policy rather than how such organisations and their experts produce knowledge. Understanding their epistemic work is important for understanding an important part of contemporary politics.

The second reason for studying political research is that it actualises issues of the relationships between collective interests, democratic representation and epistemic claims which have been central to the sociology of science and science studies since the 1970s (e.g. Barnes, 2015; Jasanoff, 1990; Woolgar, 1981b). Empirical research at a site where collective interests and political representation explicitly and visibly interact with epistemic claims is potentially generative for finding new perspectives on questions of how politics and knowledge interact generally.

In the study behind this thesis, I have interviewed LO researchers, elected representatives, and others who work in or in close connection to the research units at LO between the years of 2019 and 2024, asking them about different aspects of their work. I also draw on participant observation of the LO researchers, as I have followed them along on a tour of Sweden they made as part of the efforts to publicise a larger report series. A third kind of empirical material used is printed reports and other documents published by the organisation, which is one of the key products of LO research.

I have taken an exploratory approach in this study: I have tried to understand the epistemic work of researchers at the trade union confederation without thinking that I knew what was going on beforehand. I have, however, been guided by some specific research interests in how I have approached this

empirical phenomenon. I have been especially interested in how research activities in the organisation interact with the making of politics. Politics is a word with many different meanings, and this focus has included how the research relates to the formulation of policy, to the production of collective interests of union members, and to the world of party politics in parliament and in public debate. I have looked for what goes into making knowledge claims and suggestions for political action that are seen as successful within the organisation, and for what researchers do to make them succeed. Consequently, three specific questions have guided the writing of this thesis:

1. How do the respective processes of producing epistemic claims and policy interact?
2. How does LO research relate to the collective interests of trade union members?
3. Does LO research bridge politics, academia and other sites?

These questions all revolve around the relationship between knowledge and politics, and correspond to chapter 5–7 of this thesis, which are structured around providing answers to them.

Politics and research

At the beginning of this chapter, I claimed that social scientists struggle to make sense of political research. What I mean by this is perhaps best illustrated with two different reactions I have met to this project as I have presented it during the course of the study. The first was at a sociology of science seminar where I presented a draft for a chapter of this thesis that I ended up scrapping. I had begun by talking about the project broadly, stating that I was interested in how knowledge is produced by political organisations. One of the first comments I got after I had held my presentation was that the people I am studying do not work with the production of knowledge at all, but with the production of arguments. My response at the time was that academic research, too, can be characterised as the production of arguments. This was a poor response, hardly getting to the crucial assumption behind the comment. This assumption was, or so I infer, that the conclusions are already decided when political researchers set out to write something that is to be published; the work is only in finding arguments that support them. The conclusions, presumably, follow from the political values or interests of the organisation, producing a picture of unidirectional causality from values to facts. While political

researchers of the kind I am studying may claim to do research, they are, in actuality, putting the cart before the horse.

The other reaction I have in mind is from when I had just started the project, or before I even had, as I presented the idea for it at a sociology conference. This reaction was a set of questions asked by a critical sociologist: Why study the trade unions? Why not study the national employer organisation instead? Why not look at their work to make the Laffer curve a fact in Swedish politics, for example? If the accusation implicit in the first comment was one of naivety, this one was worse, at least as I interpreted it at the time: wanting to study the trade unions rather than the employers is politically suspect. The key assumption behind this idea is that any sociological study of the production of knowledge is necessarily a devaluation of that knowledge from true fact to mere social construction. The implication is that any research into how the claims of an actor are produced is the same as criticism of those claims and the actor making them, which raises the question why one would want to study what is seen as the relatively powerless and relatively left-wing trade unions rather than the powerful, right-wing employers. Better, then, to study how the Laffer curve was invented by economist Arthur Laffer at a restaurant dinner with Dick Cheney and Donald Rumsfeld, jotted down on a napkin, and somehow transported to Swedish politics where it was turned into an incontrovertible constraint on tax policy!

Both of these reactions were, and continue to be, puzzling to me. The assumptions behind them, at least as I interpret them, are inconsistent with understandings of truth, of research and of sociology that I had assumed were shared by critical sociologists and sociologists of science alike. The view that political researchers are producers of arguments instead of knowledge, while rather cynical, is perhaps not entirely without plausibility. Examples abound of politicians finding new arguments for their preferred political action, and the relative causal disconnect between political solutions and the problems that necessitate them is well-established (e.g. Kingdon, 2014). The problem with this view is that it is often very difficult to tell the difference between open-ended inquiry on the one hand and looking for arguments to support a position on the other, and that it seems likely that both activities, if they can be told apart, would be part of research of any kind, as sticking to the beliefs you hold may be rational, even for staunch falsificationists (Lakatos, 1970). A similar view of scientific research is captured in the famous Max Planck paraphrase that science progresses one funeral at the time; individual scientists are difficult to convince and stick to their preferred conclusions. Thomas Kuhn's view of normal science as work within a paradigm leads to similar conclusions, with the addition that certain set beliefs are a necessary part of scientific research

(Kuhn, 1970). In addition, the assumption that political researchers produce arguments rather than knowledge ignores an obvious reason why political organisations may want to employ social scientists as researchers in the first place: like the rest of us, political organisations do not have unproblematic access to the world they act in, and need ways of understanding it.

The assumption behind the other reaction, that studying research delegitimises it, follows from an epistemological understanding of successful empirical accounts as passive reflections of reality. If this is the case, research done properly is closed for sociological inquiry as it merely transports the state of affairs out there to a written account. If politics, or social phenomena more broadly, influence this process, they can only distort or bias it (Bloor, 1991), and showing that the research is done by people – that its results are constructed – is the same as exposing it as untrue. As the kind of research this study concerns is done in a setting that is explicitly political, it would be especially vulnerable to such arguments. The sociologist should choose carefully then which target this critical edge is directed at.

What was puzzling with these specific assumptions was that they seem to rely on a different form of research as an implicit comparator to the work of political researchers: a pure form of research that is open-ended and untainted by politics and other social phenomena. I would not expect the persons who reacted to my project to actually believe in the existence of this kind of research. The claim that scientific research – which I take this comparator to be – is unavoidably social, perhaps even political, seems fairly uncontroversial to sociologists in general, and saying that scientific facts are constructed (socially or otherwise) would be met with a resounding "so what!" in all sociology of science contexts I have been to. It should follow from this that some of the categorical boundaries that are otherwise drawn between academic scholarship and other ways for society to generate knowledge about itself, such as the research of think tanks, political parties, or, as is the focus of this thesis, trade unions, become less categorical, if not dissolved entirely. This conclusion, however, seems difficult to draw.

An explanation for the difficulty in arriving at this conclusion – beyond the professional interest of academic scholars to portray what they do as special (cf. Gieryn, 1983; Mulkay, 1976) – can be found in the dichotomies I alluded to at the beginning of this chapter. These dichotomies are well illustrated by the idiom of speaking truth to power. The idiom conjures an image of bravery: one actor with access to the truth, a researcher perhaps, speaking this truth to another actor with power, perhaps a politician, at the risk of displeasing the powerful. These two roles follow from a common division of labour in Western democracies, between experts, set to speak for the state of reality, and

politicians, set to speak for collective political interests (Soneryd & Sundqvist, 2023). The two roles are taken to follow different and incommensurable logics of representation: representation of reality by experts and representation of political subjects by politicians. Expert knowledge especially is assumed to be unconcerned with the wants and demands of specific persons or groups, as described, for example, in Robert Merton's account of science as guided by norms of impersonality and universality (Merton, 1974). Defying this division of labour by combining the two roles into one – the political researcher – is difficult.

As fundamental as this dichotomy may be to Western understandings of both knowledge and politics, it has hardly gone without challenge. One prominent critique is found in Michel Foucault's concept of power-knowledge: the identity of the exercise of power and the production of knowledge in an ever-rising spiral of domination (Foucault, 2020). Another set of critiques can be found in the field of science and technology studies, where the interrelations between science, politics and the rest of society have been repeatedly shown in empirical detail (e.g. Jasanoff, 2004b; Latour, 1993a; Shapin & Schaffer, 2011). At least to those social scientists who do not see themselves as being in the business of finding value-free, transcendentally true objective facts, conceding the right to produce knowledge to experts and researchers employed by political organisations should be unproblematic. If the difficulties in making sense of the political researcher as engaged in both politics and research persist, it is in spite of these well-known challenges to the dichotomy between power and knowledge.

One solution to the seeming contradiction of political research is found in the reactions above: what political researchers do is not real research, but politics. Another solution would be to imagine a linear process of policy production, where the researcher is apolitical but produces epistemic claims that serve as inputs into the production of policy. Such a solution maintains a strong division between facts and values, where the purview of the expert is the production of facts, and values enter into the picture at a later stage, as the politician takes these facts into consideration when deciding on a course of political action. This is the view of the relationship between knowledge and politics of the movement for evidence-based policy, and I mention it here because it is implied by the other solution: if research and politics need to be kept separate, policy-relevant social science needs to come before from the formulation of policy temporally in order to affect it without being affected by it. Few would assume this to be the case, however, when political organisations employ their own researchers; the interpretation of their work as policy-based evidence seems to come easier, at least in the reactions I have encountered to

my research. What this view of the apolitical researcher informing the politician has in common with the first view is a unidirectional causality between research and politics; either from politics to research, delegitimising the research, or from research to politics, preserving the apolitical purity of research, but raising issues of the undemocratic influence of experts on policy (Latour, 2004a; Soneryd & Sundqvist, 2023).

Neither of these solutions seems amenable to an empirical account of political researchers such as the ones at LO. Political researchers of their kind, tasked with representing both reality and political interests, seemingly collapse the distinction between knowledge-making and politics as distinct domains between which arrows of causality, in either or both directions, can be drawn. My aim in this study has been to investigate the kind of political research done by LO researchers without assuming a simple causal relationship between politics and epistemic claims of either kind; neither dismissing political research as mere politics, or assuming that it has to be apolitical if it is research. Avoiding both of these assumptions, I have aimed to study the practices of political research in an exploratory way, grounded in empirical description of the work practices of political researchers rather than in a priori beliefs about knowledge and politics, guided by a broader interest in how knowledge and politics hang together.

The structure of this thesis

The chapter immediately following this, chapter 2, is an account of the role of LO and their research in Swedish politics. The purpose of the chapter is to provide a contextualisation of the empirical case which should be largely familiar to those interested in the Swedish labour movement, and which I think is needed to make sense of the analysis of the thesis. I focus on their relationship to the Social Democratic Party, their historical and contemporary influence through Swedish corporatism, and their research historically and today.

In chapter 3, I develop the approach of political epistemology I take in this thesis by looking at how politics has been understood in sociology of science and science studies, and, in a sort of mirror image, how knowledge in politics has been understood from different perspectives in political science and political sociology. These literatures serve as both an inspiration to and a point of contrast to the theoretical and methodological approach I have taken in this study, and while there are few examples of empirical studies of political

research in these literatures, they could all plausibly and meaningfully be extended into the kind of political epistemology I am interested in.

Chapter 4 is a detailed account of the methods of research and analysis employed in this study: how I went from the initial idea behind the project to an empirical case study, how I gained access to the empirical site, the methods of data collection involved in the research and their ethical implications, and how I have analysed the empirical data produced.

In chapter 5, corresponding to research question 1, I examine the production of epistemic claims on the one hand and policy statements on the other in LO research, and how the two relate to each other. The chapter is motivated by an emic distinction made in LO research between policy, which is produced by researchers but needs the approval of persons or bodies with a democratic mandate to speak for the union members, and epistemic claims, which are the purview of the researchers themselves. In the chapter, I look at how the making of these two classes of linguistic statements succeed or fail, how failure is avoided, and, crucially, how the two categories interact.

In chapter 6, I provide an answer to research question 2 by presenting an argument in three steps on the relationship between collective interests of union members and the epistemic claims of LO research. In the researchers' own understanding, their work is epistemically valuable because it draws on a working-class perspective otherwise absent in research in political and government organisations. This standpoint-epistemological perspective relies on an association between collective interests and knowledge which has been a recurring theme in the sociologies of knowledge and of science, and in feminist science studies over the last century, and which, when phrased as a set of abstract propositions, leads to theoretical difficulties in defining both the relevant collective and their interests. In LO research practice, however, I find that such difficulties are avoided through the use of specific methods for constructing collective interests drawing on democratic technologies of representation and on the experiential knowledge of union member spokespersons within the organisation.

Chapter 7 concerns what goes into and what goes out of LO research to other sites. The question behind this chapter – research question 3 – is motivated by the commonly used metaphor of political research acting as a bridge between the world of academia and the world of policy, and in the chapter, I look at how LO researchers enact academia and politics along with government agencies and the trade union movement as relevant social worlds which both provide input into their research and act as destinations for their output. Such social worlds are in LO research treated as coherent sites which can both inform research and be the recipients of epistemic claims and political

influence and advocacy. While the bridge metaphor assumes a stable distinction between knowledge and politics, their enactment of these different sites shows only situational boundaries between the two, and epistemic and political phenomena frequently mix.

Chapter 8 is the concluding chapter of the thesis, in which I discuss two broader themes that cut across the three preceding analytical chapters: the unstable separation of knowledge and politics, and the role of the political expert as bearer of local solutions to tensions between expertise and democracy. Following that, I end the thesis by presenting an argument for continued empirical research on the knowledge-production of political actors, and suggest a starting point for such research.

2. LO and their research

Guided by a broader interest in political epistemology, the case of this study is a specific organisation, or more specifically, the parts of that organisation which do research. This organisation is the Swedish trade union confederation LO. In this chapter, I will present what I imagine the reader should know about this organisation and their research before they read the rest of the thesis, assuming a reader who is unfamiliar with the Swedish trade union context. I have three motivations for doing this. First, I want whatever conclusions can be drawn from this study to be useful for understanding other empirical sites, and for this to be possible, what is highly particular to this organisation needs to be understood. This is not to say that you can subtract the particulars to reach the universal, but that the particulars of both sites should be taken into consideration when drawing on one thing for understanding another. The second reason for describing the particulars is that many of them quite simply need to be known in order to make sense of the study. A point of comparison here is ethnographies of laboratories and hospitals; studies that I have found inspirational when writing this thesis. Such places, like any other, have their particulars. They are also, however, plugged into the universalising networks of scientific research and medicine. A PhD in Belgium is taken to be more or less the same as one in Japan, and so is atherosclerosis and the gravitational constant of Earth. Through the standardising effects of such networks, the ethnographer may let certain particulars fade into the background, assuming the reader to understand what is going on without them.[1] For the present study, this is not the case. LO is a thoroughly Swedish organisation, tied up in the networks of the Swedish labour market and Swedish national politics rather than those of scientific research. While some aspects of the organisation will presumably be recognisable to anyone familiar with trade unions, political organisations, or interest groups, many will not. My third motivation for wanting to present the particulars is more personal, though presumably shared with many others who have conducted similar research: I find myself wanting

[1] A counter-example to this can be found in Harry Collins' study of evidentiary cultures at an American and an Italian research group for detecting gravitational waves (Collins, 1998).

the reader to understand the particular organisation I am studying as I do. Extracting what is needed for my analysis without giving a fleshed-out account of the particulars feels like doing those who have generously cooperated in this research a disservice.

In the chapter, I will first present some of the basic characteristics of LO as an organisation, with the purpose of giving the reader what I take to be the shared understandings of LO researchers themselves informing their work. Following that, I will focus on their research, which is often attributed quite spectacular historical effects on Swedish policy-making. Finally, I will talk about some of the issues that have arisen when using the English language to describe a site where another language is spoken, and how a couple of the key terms of this thesis are translated.

LO

First, some basic information. LO, or the Swedish Trade Union Confederation, is the national confederation of all major blue-collar trade unions in Sweden. It is one of three such national confederations of trade unions, along with TCO and Saco, both organising white-collar workers. LO is a trade union organisation, but not itself a trade union; it is rather a meta-organisation (Ahrne & Brunsson, 2005) consisting of trade unions. Consequently, the LO members are not blue-collar workers – a mistake commonly made by journalists in print – but 14 different trade unions, the largest two of which are the Swedish Municipal Workers' Union, and IF Metall, the industrial and metal-workers' union. In total, these unions have 1.4 million members, 1.2 million of which are currently in employment (Medlingsinstitutet, 2023), which means that they organise around 20% of the total Swedish labour force of 5.6 million (SCB, 2023). Since the highest unionisation levels of the mid-90s, when around 85% of all Swedish employees were members of a union, the total percentage of organised workers has decreased steadily, especially for blue-collar workers, with the exception of a small increase during the pandemic. The degree of union membership is still, however, very high in Sweden, second only to Iceland in international comparison at 69% nationally in 2022 (Kjellberg, 2023a), though lower for the blue-collar groups organised by LO unions at 59% (Kjellberg, 2023b).

The organisation

The building of the LO head office is an impressive six-story building with two prominent towers, built in 1899 and overlooking the park at Norra Bantorget. It is located in central Stockholm, five minutes' walk from the Central Train Station. If you are a reader of Swedish newspapers, you are likely to be familiar with at least the front of the building as pictures of it figure regularly in reporting. It is known as *LO-borgen*, the LO Castle; a term metonymically used to refer to the central organisation of LO.

Figure 1: LO-borgen from Norra Bantorget

This is where the offices of the national organisation of LO are located. As this is where the researchers of the organisation work, it is the physical location that I have been to the most for the empirical work of this study. Apart from

the national organisation, LO has 10 regional offices, the LO Districts, coordinating their work on the municipal and regional levels, and smaller organisations in the municipalities. The head office is considerably larger than any of the districts, with over one hundred employees; more than five times as many as the largest district. Around twenty of these employees, the number has varied slightly over the course of this study, are researchers. The employees of the LO head office work in eight different departments, located on different floors, or half-floors, of the building. These departments have managers, and these ultimately answer to the leadership and the decision-making bodies of LO. The researchers are, at the time of writing, mostly located in one such department: the Department for Politics.

There are different levels of decision-making bodies and elected leaders of LO. As is common in civil society organisations, these are separated temporally by how often they meet, with the bodies with the most authority meeting more rarely. The day-to-day work of the organisation is headed by the four-person LO leadership: the president, the first vice president, the second vice president, and the negotiations secretary. The leadership answers to the Executive Council – LO's board of directors – which consists of the leadership and the presidents of the LO unions and which meets every other week. Above the Executive Council is the General Council, consisting of 100 representatives meeting twice per year. Finally, as the supreme decision-making body, is the Congress, which is made up of 250 delegates chosen by the trade unions, and meets every fourth year. The leadership, Executive Council and General Council are all elected at the Congress, though the presidents of the 14 LO unions are always the nominees for the Executive Council.

LO in Swedish politics

In Swedish national politics, LO is an important actor. One reason for this is that the Swedish labour market – like those of the other Nordic countries – is characterised by a comparatively low degree of legal regulation, with many issues which are decided by law in other countries instead being the object of negotiation between unions and employer organisations, collectively referred to as the social partners. The clearest example of this may be the legal minimum wage, which there is none in Sweden or in the other Nordic countries. Issues that would otherwise have been decided in parliament, such as the minimum wage, are handled in bargaining between the social partners, leading to a central role in the functioning of the labour market for both union and employer organisations. The degree to which such bargaining is done centrally by LO – as opposed to by the individual unions – is low today

compared to the highly centralised period between the 1950s and the early 1980s, but the negotiations of individual unions are still coordinated by LO (Elvander, 1988).

Two other circumstances are relevant for understanding the role of LO in Swedish politics. The first one is the organisation's close ties to the Swedish Social Democratic Party, which has been in power (on their own or in coalitions) for 76 of the 104 years between Sweden's first election with near-universal suffrage for adults in 1921 and 2024.[2] The second one is found in that what is sometimes called corporatism, the institutionalised influence of organisations taken to represent social interests, has been a central characteristic of both government and politics in Sweden throughout the 20th century, as in many other European countries. The main organisations associated with corporatist influence in Sweden are labour organisations, especially LO, and employer organisations. While corporatist influence decreased markedly in the Swedish political system in the 1990s, there are still institutionalised modes of representation in government and politics that LO has access to as social partner.

LO, the Social Democrats, and the labour movement

The relationship between LO and the Social Democratic Party is intimate and complex. Two short quotes from my fieldwork may help illustrate it. The first one: "The Social Democrats and LO, we are not two friends walking arm in arm. We are two arms on the same body. We are the labour movement, together." The quote is from the inaugural speech of LO president Susanna Gideonsson, at the LO Congress – a public event covered by news reporting, and broadcast live on the LO website – of 2021. The corporal metaphor for the relationship is not unusual, though the two are more frequently called the two legs of the movement (historically with a third leg added in the Swedish cooperative movement) than its arms. This proclamation of bodily unity was not made in a vacuum. The unspoken background of the speech was what was reported in the LO publication *Arbetet* as an unusual degree of tension between the two organisations at the time of her election (Frisk, 2021). In light of this troubled relationship, unity needed to be asserted.

Another quote, from another speech in my fieldnotes shows a different view of the relationship between the two organisations. "Very many years ago, we

[2] Suffrage was gradually expanded throughout the 20th century, and the contemporary right to vote in parliamentary elections for all Swedish citizens over the age of 18 who are or have at some point been registered as living in Sweden was instituted in 1989.

started a party." This quote is from the president of one of the LO unions, at a meeting for local and regional LO representatives, markedly less public than the Congress, with no journalists invited.[3] It indicates something like LO ownership of the Social Democratic Party. Technically, the organisation of LO was founded at the 1898 Social Democratic party congress, so a Social Democratic politician claiming that they once started a confederation of trade unions would have been equally plausible. The we in question is perhaps better taken to be the trade union movement in general, in which case it is true that unions started the Social Democratic Party as both their political branch and their central organisation in 1889, with the party for its first nine years having the coordinating role that LO has today. If the point of the LO president's description of the two organisations as arms on the same body was to communicate unity in the face of perceived conflict to a wider public, this union president attempted, as I understood him, to communicate to an audience of union representatives that the Social Democrats are their party, and that union representatives should encourage local and regional cooperation between their unions and the party. What I interpreted as the background for communicating this at the time was an issue which was later raised by one of the participants at the meeting: that large groups of LO union members do not say that they vote for the Social Democrats in surveys, with the nationalist Sweden Democrats at times polling above the Social Democrats among large groups of LO union members.

What I want to say with these quotes and the situations in which they were spoken is that while there is probably no singular description of the relationship between LO and the Social Democrats, they are something more than two independent organisations, at least from perspectives within LO; there is something in between a strong relationship and a partial identity between the two. Two examples of what this relationship has looked like in practice may help concretise it. The starkest one is found in the collective affiliation of union members to the Social Democratic Party. Between 1900 and 1991, becoming a member of most of the LO unions automatically made you a member of the party, which meant that you were registered as a member of the party organisation, without necessarily being expected to vote for the party (Allern et al., 2007, p. 614). Since such collectively affiliated members made up the majority of party members, the membership lists of the LO unions and the Social Democratic Party before 1991 would have looked fairly similar put to

[3] Strictly speaking, no journalists were, to my knowledge, invited to the physical LO Congress in 2021 because of the ongoing pandemic. Webcasting the event on the LO website filled a similar function.

side to side. It also meant that the number of members dropped sharply when collective affiliation was banned by law: with around 70%, according to a contemporary news report (TT, 1991). As the party lost much of its funding with this sharp drop in membership, LO has since 1991 funded the party directly to a comparable degree.

While collective affiliation is a historical example, the other example is ongoing. The president of LO has a regular seat on the party's Executive Committee: the group of seven people, otherwise consisting of the party leader and leading Social Democratic politicians, responsible for day-to-day decision-making in the party. This is unlike important organisations affiliated with the party, such as *SSU*, the Social Democratic Youth League (which, incidentally, is also the youth organisation of LO), and Social Democratic Women, the presidents of which have the right to attend, to speak at, and to make suggestions to the Executive Committee, but who are not regular members with the right to vote. This means that the LO president has some influence on the direction of the party, and also that they have direct access to discussions held at the highest level of the party organisation.

Beyond these concrete examples of how the structure of the two organisations are and have been integrated, cooperation between the two organisations is general and diffuse. Local and regional union offices coordinate with local and regional party offices through LO, in what is within the labour movement termed union-political cooperation. The last leader of the Social Democratic party, prime minister Stefan Löfven, got to his leading political position as first a member of the Executive Committee and then party leader not because of his experience within the party, which was limited, but through being the president of the large LO union IF Metall. And, as I will discuss further in chapter 7, when LO research is directed towards the world of parliamentary politics, it is generally the Social Democrats who are the target. This is all to say that while the LO and the Social Democrats are two different organisations with an occasionally tense relationship, the ties between them can hardly be overstated.

Despite the close association between the two organisations, you do not necessarily have to be active in, or be a voter of, the Social Democrats to work as an LO researcher. Job ads for positions at the organisation require applicants to "share the values of the labour movement". The labour movement, however, is a term the definition of which differs from context to context. While the Social Democrats and LO are sometimes called the two legs of the movement, other organisations see themselves and are variously seen as belonging to it, including the Tenant's Association, the Cooperative Union, the Left Party, and the radical syndicalist union SAC. Distinctions are sometimes explicitly made

between narrower or wider senses of the phrase, but the term in the context of job ads is unspecified. This openness to interpretation gives potential applicants to positions at LO some freedom to decide for themselves what it means to share the values of the labour movement.

Corporatism

A second reason for LO's influence on Swedish politics, beyond its ties to the frequently-in-power Social Democrats, is found in the history of what is sometimes referred to as the corporatism of the Swedish political system; the institutionalised influence of organisations taken to represent important social interests on governance and politics. The foundational idea behind corporatism is that specific collective social interests exist and are legitimately represented by specific organisations, and, at least by some of its Swedish proponents, that cooperation and negotiation between such interests in and through government agencies lead to outcomes that are beneficial for society as a whole (e.g. Rothstein, 2008). While corporatism has been described as a characteristic of most Western European political systems (Streeck, 2006), the Swedish one has in the 20th century been considered to be particularly corporatist in international comparison (Hermansson et al., 1997). The most relevant example of Swedish corporatism for this study, as well as the most commonly used example overall, is the role that trade unions and employer organisations have played in Swedish governance, though the term has also been used, in research and in public debate, to describe the organisation of the Swedish rental housing market (Jonung, 2024), and the influence of sports associations, religious organisations and other kinds of interest groups on politics broadly (Lundberg, 2015).

Corporatism is generally described as something of the past in Swedish politics. In the early 1900s, trade unions and employer organisations were given equal representation on the boards of the first employment agencies, marking the beginning of the institutionalised influence of organised labour market interests on governance in Sweden. Between the post-war period and the 1990s, at the peak of Swedish corporatism, LO was remarkably well-represented in Swedish governance; not just in labour market agencies like the employment agency, but also, for example, on the boards of directors of public universities and of the prison authority. By one count, LO had over 1100 representatives in 663 national government agencies, public inquiries and councils in 1984, which was significantly more than any political party had at the time (Rothstein, 2008). The decisive moment in the dismantlement of Swedish corporatism is supposed to have been in 1991, when the national

employer organisation *SAF* withdrew all participation from first the board of directors of the national employment agency *AMV*, and then all other government agencies except the court of law for labour market disputes, severely undermining the legitimacy of board representation of trade union organisations. As trade unions and other organised interest groups were consequently removed from the boards of government agencies in the 1990s, the decline, fall or even death of corporatism in the Swedish political system became a frequent topic of writing for political scientists throughout the decade (e.g. Hermansson et al., 1997; Lewin, 1992; Rothstein & Bergström, 1999).

Despite the withdrawal of interest group representatives from the boards of government agencies, however, other forms of institutionalised LO influence on and representation in politics and government have remained, and specifically researchers at LO are often involved in these kinds of representation. While the national employer organisation stopped participating in the executive work of government agencies in 1991, they did not withdraw their representatives from advisory boards (Rothstein, 2008), and consequently LO and other trade union organisations are still represented there, often by their researchers. Another avenue for political influence – though on law-making rather than executive governance – is the frequent role LO plays in the legislative referral system; the consultation of interest organisations which are taken to be stakeholders when new laws are drafted. Such requests for referral statements from organisations are addressed and at least in principle taken into consideration by the government before drafts for legislative proposals are sent to parliament, and LO's statements are generally written by their researchers.

Beginnings of the LO research department

The association with the Social Democratic Party and the corporatist influence on politics and governance were both in place when LO first employed their first researchers as part of the permanent staff in the 1940s, though corporatist influence developed and intensified in the post-war period. Once LO had its own research department, research became one of their mechanisms for political influence and their researchers were at times notable political actors, at least in standard accounts of modern political history in Sweden (e.g. Hadenius, 2008; Möller, 2023). While organisations conducting research had existed in the form of think tanks in the UK since the late 19th century and the US since the early 20th century (Medvetz, 2012; Stone, 1996), permanently employed researchers in partisan organisations was a more or less novel

phenomenon in Sweden when the first LO researchers were hired (Garsten et al., 2015 pp. 232–240).

This is not to say that research and politics were disconnected social domains before this. The development of modern Western European social and labour policy from the late 19th century and onwards was characterised by a strong belief in rational, knowledge-based political solutions to social problems, and a correlated displacement of political conflict to the production of competing knowledge claims (Rabinbach, 2017). Organisations like the Fabian Society in the UK and the *Verein für Sozialpolitik* in Germany played important roles in developing supposedly rational social policy in their respective countries, drawing on the authority of expertise and the making of epistemic claims (Rueschemeyer & van Rossem, 2017). Academic social science disciplines developed in response to the concerns of national politics and governance throughout the 20th century, and had profound effects on the development of the modern welfare state (Pettersson, 2003; Wikman, 2019; Wisselgren, 2000). When the first LO researchers were employed, research and politics were already in a relationship of co-production (Jasanoff, 2004a): politics was as much an epistemic business as social science research was political. What was seemingly novel about the first LO researchers in the Swedish context was not that they connected research with politics, but that they were a permanent research unit within a partisan organisation, employed as all-in-one researchers, advocates and developers of policy, in a role frequently found in political organisations today (Garsten et al., 2015).

The first researchers at LO were employed in 1943, though economists had been hired for more temporary research jobs since the 1930s (Hellberg, 1997). The decision to set aside funds specifically for research was made by the LO Congress, after having been argued for by August Lindberg, the president of LO at the time, and Swedish sociologist, economist and Social Democratic politician Gunnar Myrdal. In historian Lars Ekdahl's account of the start of the research department, the idea for it came about as a reaction to the founding of the employer-funded academic Research Institute of Industrial Economics in 1939 (founded as *Industriens utredningsinstitut*). This research institute, in turn, was founded because the Swedish right-wing parties and the closely tied employer organisations were of the opinion, rightly as it would turn out, that the Social Democrats would be in power for the foreseeable future, shutting them out from access to the well-developed political research capacities of the Swedish state (Ekdahl, 2001). The purpose the LO research department was supposed to have was to be a centre for knowledge production and policy development within the labour movement, or, as Lindberg described it, the "union movement's shock troops" in ideology battle (as quoted in Ekdahl,

2001, p. 116, my translation). The perception within the labour movement at the time was that the Social Democratic Party was too caught up in the day-to-day governance of the country to develop long-term political ideas, which the LO researchers were supposed to do (Ekdahl, 2001). For the first decades of its history, they were remarkably successful in this.

The first generation of LO economists were educated in the Stockholm school of economics (not to be confused with the academic institution with the same English name) and had all been students of Myrdal. The Stockholm school was, similarly to Keynesian economics, characterised by a focus on aggregate demand as the driving force behind economic growth, and LO economists today still describe a demand-side, Keynesian view of economics as their shared theoretical outlook.[4] Another characteristic of the Stockholm school was its members' high degree of involvement in national politics, with Myrdal serving as minister of trade between 1945 and 1947, preceded on the post by Liberal party leader and economics professor Bertil Ohlin, who was another prominent member of the school. One of the first three economists employed at LO, Richard Sterner, had been Myrdal's research assistant in the study behind his book An American Dilemma, and went on to work for Myrdal in his capacity as cabinet member in 1945 (Ekdahl, 2001; Hellberg, 1997), while the other two, Gösta Rehn and Rudolf Meidner, had been his students at university.

While Sterner only worked as LO economist for a couple of years, Rehn and Meidner stayed for longer; Rehn for a decade, and Meidner for two decades (and eventually returning to LO in the 1970s). Rehn and Meidner became fairly well-known in Sweden as the architects behind an ambitious economic reform programme in the post-war period eponymously known as the Rehn-Meidner model. This model was developed at LO and came to be adopted wholesale by the Social Democratic government, though not without resistance from minister of finance Per-Erik Sköld who initially dismissed the young LO economists as "playful kittens" (as quoted in Rehn, 1977, p. 214, my translation). One of the foundational ideas behind the model, quite alien to the strong belief in the efficiency of markets that has characterised mainstream political discourse during my lifetime, was that private enterprise would tend to keep outdated practices of production for as long as they could, and that they needed a push from both the trade unions and the government towards

[4] When Swedish economists are in a patriotic mood, they sometimes say that the Stockholm school predated John Maynard Keynes' work on aggregate demand, with the implication that scientific priority should have gone to someone like Myrdal. Whether or not economists outside of Sweden agree with this assessment is beyond the scope of the present study.

modernisation and technological development in order to organise production rationally and efficiently (Ekdahl, 2001). The trade unions' main role in this was to implement what was called a wage politics of solidarity, in which employees with the same role and the same level of experience would get the same wages, bargained at the national level, regardless of how productive the organisation employing them was. This was supposed to lead to something like an increased evolutionary pressure on the industries, as productive companies would be subsidised and unproductive companies penalised compared to employer-level bargaining where the individual company's productivity is taken into account when setting wages. Central wage bargaining on the national level also made it possible for the trade unions to coordinate and control nominal wage increases, supposedly stopping the accelerating inflation rate that low unemployment, or full employment, was otherwise assumed to lead to. The key role for the government in the model was to provide incentives for better working conditions and increased productivity, and to increase the mobility of the labour force to productive industries through employment agencies and by subsidising the relocation of individuals and families within the country. When I did undergraduate studies in economics at a Swedish university in 2011, the Rehn-Meidner model was matter-of-factly presented as the causal explanation for the success of the Swedish economy in terms of strong economic growth and low unemployment and inflation in the years between Second World War and the early 1970s. Although recent research has called the causal effects of the Rehn-Meidner model on the Swedish economy into question (Molinder, 2017), its efficacy – and consequently the central role of LO research – has been part of the shared understanding of Swedish post-war history both in the discipline of economics in Sweden and in Swedish politics, perhaps especially in the labour movement. Mirroring this, the playful kittens at LO seem to have perceived themselves as the epistemic and ideological centre of the labour movement at the time, as can be seen from how Meidner rather confidently described their role during these decades: "[T]he trade union movement takes care of the ideological initiative, and precedes the party through research, public policy programmes and expert opinion" (as quoted in Nyström, 2023, p. 20, my translation).[5]

[5] Meidner later came to have cause for a less confident understanding of their relationship to the Social Democratic Party. In the 1970s, he had become disillusioned with the wage politics of solidarity of the Rehn-Meidner model, finding that the subsidised wages for productive companies led to increased profits rather than reinvestment, with increasing income inequality as a consequence. For the 1976 LO Congress, he, together with Anna Hedborg and Gunnar Fond, presented a proposal for a reform in which privately owned companies would have to set aside 20% of their profits for buying ownership in the company for investment funds managed by the trade unions. This radical proposal would

The reason for focusing on the first decades of LO research here is that the influence of the early days of LO research at its time led to a continuing legacy from the days of Rehn and Meidner in LO research today, as in the Swedish labour movement more broadly. Some of the key terms of the Rehn-Meidner model, such as full employment and wage politics of solidarity, are still in use, though their meanings have shifted over the decades. The Rehn-Meidner model itself lives on, both in LO research and in Social Democratic rhetoric as the foundations of what in contemporary language is called the Swedish model. For LO research specifically, two characteristics of its research in the 1940s and 1950s are still present. First, LO research is done within the organisation, but the LO economists – unlike other LO researchers – have, as Rehn and Meidner did, a certain degree of autonomy in relation to the rest of LO. When the decision was first made to employ researchers, at least some prominent voices within the organisation wanted to create an independent economic research institute rather than a research unit, and the idea of organisationally divorcing the research department from LO recurred for at least the first fifteen years of its existence (Meidner, 1958), without being realised. The relative autonomy of the economists is a compromise position compared to full organisational independence. What this autonomy means in practice is that LO economists can choose their own research topics, that they do not need the approval of elected bodies or persons before publishing their results and that they can make policy suggestions that are to be interpreted as them speaking as individuals rather than with LO as an organisation as the authorial function of the text. This autonomy is in contrast with non-economist researchers at LO, which is about half of the researchers I study. Non-economist researchers, or analysts as I call them throughout this thesis, came to be employed gradually by the organisation in the decades following the Second World War, as the organisation found itself in need of qualified research staff to more directly support the leadership and others at the head office (Hellberg, 1997). Similarly to the economists, they write reports and acts as experts, but unlike the economists, they do need the approval of elected bodies or individuals before

eventually have led to indirect majority ownership of all Swedish companies by the trade unions, in a form of socialisation of the means of production. Changing the power dynamics of the labour market through such a change in ownership was the stated goal of the reform. This proposal, called the Employee Funds, met very strong opposition from employers and right-wing parties, but also from within the Social Democrats. While the party adopted the proposal, it had been significantly watered down when it was passed as law in 1983, taking the form of a small tax on company profits, the income from which was used for state-owned investment funds (Ekdahl, 2005).

publishing reports, they get tasked with doing research on specific areas, and they are not supposed to make policy suggestions in public on their own.

The second characteristic present since the start is found in its relationship to academic research. LO research is not academic research, in the sense that few of their employees have terminal degrees and their work only very rarely leads to publication through academic publishers or in peer-reviewed journals. This is not seen as a failure in recruitment or publishing, and engaging with academic research in such ways is not particularly highly valued in their research. In a contemporary framework, their research is better characterised as something like policy research, akin to the work done by think tanks. Applied to the early days of the LO research department, however, such a description is anachronistic: the think tank concept had not yet been imported to the Nordic countries, and when Meidner described their research department in English for an academic audience, he seemed to struggle to find the right words for it, describing it in rather vague negatives as not being "an institute in which economic research in the real sense of the word is carried out" (Meidner, 1958, p. 85), meaning in the sense of academic research. At the founding of the research department, its relatively weak ties to academic research was seen as a problem, with LO president August Lindberg expressing disappointment that none of the first economists had PhDs at the time of employment, as the unrealised hope at the time was that LO would found an academic research institute for producing economic research relevant to the trade unions (Ekdahl, 2001).[6] LO researchers today, however, do not see their relative lack of academic credentials and engagement as a problem, treating what they do as qualitatively different from academic scholarship. The remarkable success of the first generation of LO researchers may go some way towards explaining that they no longer evaluate their own research in relation to academic research. Christina Garsten, Bo Rothstein and Stefan Svallfors (2015) described the first LO economists as the first and paradigmatic example of policy professionals – partisan knowledge workers employed by parties and interest organisations – in the Swedish political context, with the success of post-war LO research serving as a model for political professionals in other organisations. There may be reason for scepticism towards the argument that Rehn, Meidner and Sterner were the first such professionals in Sweden, as the LO research department was at its founding understood as partly inspired by the employer-funded Research Institute for Industrial Economics, implying that they were at least not seen as a qualitatively new phenomenon at the time

[6] Both Rehn and Meidner eventually received doctoral degrees, and both moved between LO research and academic settings through their careers.

(Ekdahl, 2001). On the other hand, a significant difference between the LO research department and the institute that inspired it is that the former was organisationally independent with no explicit partisan aims. Regardless of their novelty, however, it seems plausible that accounts of the remarkable success of the first LO researchers has served as model for future epistemic-political work in LO and elsewhere, and has helped establish their own kind of research as a category on its own, as opposed to something to be compared to academic research.

LO research today

LO research is generally seen as having been very successful when it came to effecting policy change in their first few decades; so much so that their offices for a time was considered the place where Swedish economic policy was developed. When describing their research today, I would characterise their research offices as one actor among many in a larger milieu of organisations doing political research in Sweden. Like other actors in this milieu, they primarily produce what social scientists chauvinistically call grey literature, or, more generously, a form of Mode 2 knowledge (Nowotny et al., 2003).[7] Research activities similar to those of LO are performed at the other two Swedish trade union confederations TCO and SACO, at the larger trade unions, at employer organisations, at think tanks and in the offices of larger interest organisations. The LO head office employs around 20 researchers at any given time, which is more than twice as many as the other union confederations, which would otherwise seem like the closest comparators. They are also significantly larger than any of the left-wing think tanks in Sweden – all predominantly financed by LO and their unions – which have 4–5 people employed. They are, however, outfinanced and outstaffed by the national employer organisation and their associated think tanks.[8] While LO's ties to the Social Democratic party still give them a relatively influential position, at least compared to think tanks and interest groups who generally have less direct

[7] While LO research matches the criteria for Mode 2 knowledge production very well, it predates the supposed shift into Mode 2 by some decades (Nowotny et al., 2003). In line with Olle Edqvist's argument about the historical exceptionality of post-war Mode 1 research, this throws doubt on the novelty of Mode 2 (Edqvist, 2003).

[8] Numerical comparisons of the researchers of other organisations in this paragraph are inferred from job titles in the staff directories at the websites of the relevant organisations.

access to politicians, they are in terms of their political research one actor among many in Swedish politics.

Almost all of the 20 researchers employed today work at the same department at the LO head office, The Department for Politics, though a clear emic distinction is made within this department between economists on the one hand and analysts on the other, with about half of the researchers in each category. This difference is most clearly expressed in in the economists' autonomy, as discussed above. The term LO economist, an important emic term, is not necessarily indicative of a researcher's topic of specialisation; an analyst may be trained as an economist, but if their job title is analyst, they are not an LO economist. During the time I did the majority of the interviews and observations for this study, 2018 to 2021, economists and analysts worked at different departments on different floors of the building – the Department for Economic Policy and the Department for Welfare, Education and the Labour Market respectively – making the distinction between economists and analysts clearer on organisational charts and in the physical location of the researchers' offices. In 2022, as I was doing the some of the last few interviews of the study, a major restructuring collapsed these two departments into one. Apart from the researchers at the Department for Politics, formerly at two different departments, three people are employed with the title of analyst at the Department for Negotiations.

The day-to-day work of LO researchers is varied. When Meidner described the activities of the LO research department in 1958, he categorised what they do into three different sets of tasks: research, wage policy, and information and education (Meidner, 1958). One of these, wage policy work, has become markedly less relevant since Meidner's days, as collective bargaining has since then been decentralised to the individual unions, though still coordinated between the unions. The other two, however, are still a fair description of what they do some sixty years after Meidner's description, and I will here describe the activities involved in research and information and education in turn.

Research

The category of research includes the preparation and writing of different kinds of documents, but also preparing responses to proposals to the LO Congress and acting as experts on different boards, councils and committees. The main kinds of documents written by LO researchers are reports published by the organisation, which I will discuss at length in chapter 5 of this thesis, and responses in the referral part of the Swedish legislative process. Compared to reports, which are generally meant for public consumption, these latter

documents are formal and dry in prose, and unlike the reports, they are not adorned by pictures of LO union members at work. Besides the manifest function of providing the government with comments on proposed laws, referral requests are an occasion for LO to decide what their position as a corporate body is on various political issues. Already published referral responses are also an important policy source for LO researchers; if they want to find out what the organisation's position is on a specific topic, looking at older responses is one way, alongside looking at the protocols of decision-making bodies and published reports approved by a decision-making body.

Unlike reports and referral responses which are, at least nominally, primarily intended for readers outside of the organisation, preparing comments on proposals for the LO congress is a more internal affair. For the LO Congress, which takes place every four years, LO unions and their members write proposals for positions to take or activities to do by the organisation. Before such proposals are sent out to the Congress delegates, comments are added by the head office. The contents of these comments are varied, but they generally provide a brief summary of the background to the proposal, they may add what is taken to be relevant facts, such as statistics and whether or not similar proposals have been made before, and they almost always describe what the consequences of approving the proposal might be. While these comments are written with the LO Executive Council as the authorial subject, they are drafted by LO researchers and sent to the Council for approval. In the months leading up to an LO Congress, writing such comments is a significant part of the LO researchers' job.

Finally, LO researchers frequently act as expert representatives on boards, councils and committees in the labour movement, in government agencies, in referee groups for public inquiries, and, very occasionally, in collective bargaining. In these contexts, they both represent LO as an organisation, and act as experts on the topics that their own research at LO concerns.

Information and education

A central part of the work of LO researchers is to communicate what they do to people within the organisation, or in other organisations within the labour movement. My main reason for using Meidner's categorisation of the tasks of LO researchers is to highlight this aspect of their activities; this is where their job most clearly differs from, for example, experts employed by a think tank, or researchers at a research institute. The trade unions, and the labour movement more broadly, organise courses, seminars and public events for organisation members and representatives, and speaking at such events is part

of the job for LO researchers. In Meidner's account, he underlines that such meetings are not a unidirectional passing of information from the experts to the non-experts, but that union members often ask knowledgeable and critical questions that force the researchers to rethink their understanding of economic theory (Meidner, 1958, p. 165). Elsewhere, he referred to such contact with members and representatives as his "second university" (Ekdahl, 2001), which is a quote that at least one of the LO researchers I have talked to also used. While such accounts should probably in part be understood in relation to concerns about the professionalisation of trade unions and the undue influence of experts on democratic organisations generally (Hellberg, 1997; Mellquist & Sörbom, 2023), similar ideas about meetings with members have been frequent in my interviews with LO researchers, as I will discuss more in chapter 7.

Translations

Part of the motivation behind this chapter is to provide a picture of the particulars of LO research, by describing both their activities and their relationships to other actors in Swedish politics. One such set of particulars is in the words used, and as almost all empirical data drawn on in this thesis are in Swedish, I have translated key concepts and quotes when writing this thesis.

Translation is, as the Italian-French saying goes, treason. The phrase is clichéd in the science and technology studies literature, as a consequence of its use as an analytical point in actor-network theory, indicating that any transference, whether between sites, forms, or otherwise, is also a transformance. The original linguistic referent of the saying, however, has been abundantly clear in the writing of this thesis. While this is probably true for almost any social scientific work in a language other than the one used at the empirical site of research, the difficulties in finding the right words for things may have been accentuated by the interrelated national characters of the Swedish language, the Swedish trade unions, and the Swedish political system. A couple of terms especially need elaboration, as they are both central to the study and are not easily translated into English without friction. The first one is the term *arbetare*, which is the people organised by LO unions. The second one is *utredning*, which is the term for research done by LO researchers.

Arbetare

The most literal translations of the word *arbetare* is worker or labourer. In Swedish, it is contrasted with *tjänsteman*, the literal translation of which – service man, though the term is gender neutral in Swedish – gives little clue to what it actually means, which is something like office worker. The English phrases blue-collar and white-collar workers roughly map onto this Swedish dichotomy, and these are the terms I have used throughout the thesis. There are some important differences between the two sets of words however. First, arbetare have a strong connotation to the working class in Swedish, as the word worker has in some, but not all, Anglophone contexts. Second, the word is tied to how the Swedish trade unions are organised and to LO as an organisation. The distinction between blue-collar and white-collar in Sweden has a quite precise definition in many Swedish contexts, including parliamentary debates and the production of public statistics: a blue-collar worker is in these settings explicitly or indirectly defined as someone who is or potentially is organised by an LO union.[9] Consequently, phrases like the LO collective or LO members are commonly used as co-extensive with blue-collar workers or the working class, though erroneously in the case of LO members as these are technically the 14 confederated trade unions of LO. What workplaces are organised by which unions is negotiated and decided in what is called delimitation agreements between unions, in which it is decided what kinds of workplaces and employees should be organised by which unions. As there are inevitably groups of workers who are not easily categorised as blue-collar or white-collar, there are differences between Sweden and other countries, and between Swedish and other languages, in who is considered to belong to which category; shop clerks, for example, are arbetare in Sweden, but not in Denmark despite a similar labour movement history to Sweden, and professional musicians are organised by an LO union despite being quite far from the prototypical blue-collar worker. This definitional aspect of being a worker is interesting in relation to LO's claim to speak for the workers, as I will discuss in chapter 6.

The terms arbetare and tjänstemän can also be used to differentiate between different employees at the LO head office. Most people employed by trade unions, especially in the regional LO Districts, have the Swedish job title of *ombudsman*. An ombudsman is employed from the LO union members to work

[9] Small numbers of what is considered blue-collar workers are organised by, for example, the syndicalist union SAC and by the independent Dockworkers' Union, but these work in workplaces that are otherwise organised by an LO union, which is what defines their workers.

for the union, and is still considered an arbetare, despite holding an office job. The researchers, and other union employees, employed for specific professional competencies such as communication specialists or legal professionals, however, are tjänstemän.

Utredning

Throughout this thesis, I refer to the people I study as LO researchers, and to what they do as research, as translations of the Swedish words *utredare* and *utredning* respectively. While this is the word the organisation itself uses in its communication in English (LO, 2016), it may not seem like the obvious translation of the Swedish word *utredning* to a Swedish-speaking reader. The reason for this is that the most common Swedish translation of the word research, *forskning*, is quite narrow in meaning; similarly to its German cognate *Forschung*, it generally refers specifically to academic research at universities or research institutes. The word utredning, on the other hand, has another rather specific meaning, as the knowledge production of the government or of larger organisations, especially in the realm of politics.[10]

Besides the difference in context – academia on the one hand, and government and political organisations on the other – the distinction between forskning and utredning in Swedish sometimes connote differences in epistemic quality between the two activities, which is, I suspect, why the word research may seem like an uncomfortable translation of utredning. Utredning is often taken to be less theoretical, closer to fact-finding in relation to specific issues, compared to the academic research of forskning. The contrast between these two words has been used by LO researchers themselves in my fieldwork, at times when they have defined what they do in contrast to forskning. If such distinctions are taken to mean more than distinguishing what is published by academic publishers and universities from that which is not, however, they mobilise the well-known problems of demarcating science from non-science in the philosophy of science; in practice, the line between the two kinds of knowledge production is impossible to draw. Apart from the difficulties in establishing definitional criteria for what should count as scientific research, and the similar problems of distinguishing the development of theory from mere fact-finding, academic researchers are frequently employed by

[10] The word has several different meanings. One related to the one I discuss here is in the context of policework, where the closest English translation would be investigation. Another meaning, rather poetic in this context, is disentanglement, especially of balls of thread or yarn.

government and political organisations to contribute to utredning as researchers, and utredning sometimes involves original academic research. In principle, something like Thomas Gieryn's boundary work approach, where the making of distinctions between science and non-science – or in this case forskning and utredning – is treated as an empirical resource (Gieryn, 1983, 1999) would in principle make sense for understanding the difference. In the case of LO research, however, these boundaries seems robust in the sense that LO researchers generally understand what they do as utredning, only rarely drawing the boundaries in such a way that what they do is closer to academic research; in their research, a distinction between utredning and forskning is generally made in such a way that what they do is the former and not the latter.[11] This is not to say that there actually is a categorical difference between their research and, for example, my own, but only that they often make such a distinction themselves.

Although the word utredning refers to most research done by political organisations and government agencies, the prototypical form of utredning in this sense of the word is probably to most Swedish speakers the public inquiry reports written as a common first step in the Swedish legislative process, where the government tasks either the Government Offices or an external person or a parliamentary committee with writing a report about the topic, with suggestions for if and how the law should be drafted. Externally authored reports are published as part of the Swedish Government's Official Reports, or *Statens offentliga utredningar* in Swedish. The Swedish Government Offices are small compared to those of other Western countries, meaning that there is relatively little internal capacity for knowledge production within the offices of the cabinet. As a consequence, externally authored reports – the Swedish Government's Official Reports – are common (Zetterberg, 2011); so much so that the use of external committees as the norm is often considered a central characteristic of the Swedish political system (Heclo, 1974; Johansson, 1992), in contrast with political systems where the use of public inquiries external to the government is generally reserved for especially controversial issues. While the use of committees has declined in the last couple of decades, with quicker one-person inquiries taking their place (Dahlström et al., 2021; Riksrevisionen, 2004), public inquiries by committee still form an important part of what Sheila Jasanoff has called a civic epistemology (Jasanoff, 1996) particular to Sweden: it is part of the expectations publics have on knowledge-producing activities related to governance. While governments may have different reasons for

[11] A few exceptions to this are discussed in chapter 7, when I examine the relationship between LO research and academia.

commissioning such a report, including postponing or displacing the responsibility for the making of law on controversial topics, one specific part of the civic epistemology of committee reports is that they are supposed to facilitate epistemic and political consensus between the political parties, as the committee members are generally parliamentary representatives of different political parties tasked with agreeing on both the basic facts of the matter and on the recommendations of the committee (Johansson, 1992). Public inquiries – whether done by parliamentary committee or by an individual person – also employ a group of experts, and LO researchers often serve as such experts, which is one of the advisory functions that the organisation of LO still has despite the withdrawal of corporatist representation from the boards of government agencies.

Utredning in the sense of public inquiry is broadly considered to be a rigorous and trusted form of knowledge production in Swedish politics. The point here is that despite the analytical difficulties in demarcating science from non-science, a categorical difference is made in Swedish between two kinds of research: in academia on the one hand and by political and government organisations on the other. LO research is categorised as the latter of these two. That this category of research connotes a degree of legitimacy may help to explain why LO researchers do not value publishing in academic contexts or employing people with PhDs highly.

To add to the potential confusion of translating utredning into research, those LO researchers who are not LO economists have the job title of utredare – one who does utredning – at LO, though the LO economists too would be considered utredare outside of this emic distinction. Throughout this thesis, I have chosen to use the term researcher to denote both of these groups, and the word analyst – another plausible translation of utredare – to denote those researchers who are not LO economists.

LO and its research

My goal in this chapter has been to provide an account of some specificities of LO that I think should be known before reading the rest of the thesis, focusing on the organisation of LO in relation to the labour movement and the Swedish government, on the role of their research historically and today, and on key terms that are difficult to translate to English without friction. As this is a study of their contemporary research, I have tried to limit this account to what is relevant to their research today, including aspects of their historical beginnings

which are relatively well-known and which I take to form part of the researchers' own understanding of their work. There is reason to sceptical towards the notion of context. It may well be "simply a way of stopping the description when you are tired or too lazy to go on", as Bruno Latour once had an author surrogate claim (Latour, 2007a, p. 148). The argument goes that as soon as the context has an effect, it stops being context and starts being content; there is no backdrop to the actors that does not itself act. With this in mind, I have tried to have as a guide throughout this particular contextualisation of LO research what I take to be the shared understandings of insiders in the Swedish political system, including the LO researchers, or, differently phrased, the explicit and implicit understandings which inform their work.

Throughout my account of LO and their research in this chapter, an overarching narrative can be seen: that of an organisation with its past behind it. Union membership has declined since the mid-90s, corporatist influence on government and politics was greatly reduced in the same decade, and Rehn and Meidner's ability to, at least in retrospect, seemingly dictate government policy is a memory from the post-war era. This is not to say that the organisation necessarily is in decline, and certainly not that it is no longer important. The decreasing degree of LO union membership is highly recognised as a threat within the organisation; it came up as a frequent topic of conversation, and of presentation in speeches, in my fieldwork, and while I was doing my fieldwork, LO was running a large project for coordinating the recruitment of union members dramatically named "Organise or die", though few researchers were to my knowledge directly involved. The outcome of such efforts is yet to be determined. Regardless, they are still an organisation close to the centre of Swedish politics, because of their role as social partner on the Swedish labour market, because of their close proximity to the Social Democratic Party, and because of their institutionalised influence on government and politics.

What has struck me as one of the clearest characteristics of LO as an organisation when I have been to their head office and their regional offices, however, is nostalgia. The first thing you see as you enter the vestibule of the LO headquarters is a mural of the working man by an anvil, bare-chested with a pair of blacksmith tongs in hand, dating back to when the building was first bought by the trade unions in 1926, in a style that I, though I am no art historian, would characterise as socialist realism.[12] If you come to the building

[12] The working woman is depicted in the same room, but unfortunately in a much less visible corner of the room and, at least when I have been there, partly obscured by a large potted plant. Instead of tools, she is accompanied by her children. While a point could certainly be made about this – as has been done by Yvonne Hirdman (2008, p. 11) – nothing I have

with a meeting booked, as I have often done, you may well be shown to the meeting room Saltsjöbaden, named after the decisive 1938 agreement between LO and the Swedish Employers' Association, or the room Ådalen, named after a 1931 strike where five strikers were lethally shot by the military, which is another pivotal point in the history of the Swedish labour movement. On the way there, the person you are meeting may point out the heavy wooden desk of first LO president Fredrik Sterky, stood in the corner of a hallway for decorative purposes. Slightly less impressive adornments of a similar genre are found on the walls of the regional offices: photos from local May Day parades with prime minister Olof Palme, large trade union flags from the turn of the century, and printed versions of the declamatory trade union vow.[13] As this nostalgia has seemed to be present in the rest of the organisation more than in their research, it has not been a central point of analysis in this study. For the purposes of giving the reader a rich understanding of LO as an organisation, however, it is necessary.

seen in my fieldwork shows that this is indicative of pervasive issues of sexism at the LO research department today, though this has also not been the focus of this study.

[13] "We vow and assure to never under any circumstances work for a lower wage or under worse conditions than what we are now promising one another. We make this vow with the deep understanding that if we all keep this promise, the employer must meet our demands."

3. Political epistemology

The LO researchers in focus in this thesis are political researchers. This kind of actor is often claimed to be in-between different worlds; neither purely of the world of research nor purely of the world of politics, but in an interstitial space (e.g. Garsten et al., 2015; Medvetz, 2012). Such claims reinforce the idea of science and politics as separate and qualitatively different worlds, reifying the distinction between knowledge and power. Challenging this distinction has been at the heart of science studies since the late 1970s. Simultaneously, there has been an awareness that understanding knowledge production is important to understanding policymaking in research on policy processes since at least the 1960s. Despite the longstanding recognition that research, knowledge and politics are interrelated, research in both science studies and in epistemically oriented policy studies has generally reinforced the distinction between science and politics in the sense that one of them, science studies, has primarily looked at politics from a perspective rooted in the empirical study of science, while the other, policy research, has looked at science and research from a perspective rooted in the study of policy processes. As central to politics as knowledge has been recognised to be, little attention has been paid directly to the epistemic work done within the context of politics: the work of researchers and experts employed by political organisations. While few studies have centred the research of political organisations, both the field of science and technology studies, and knowledge-centred policy research could plausibly be extended to this empirical phenomenon.

In this chapter, I will explore how different literatures have engaged with knowledge in relation to politics. First, I will provide a brief and partial account of the early development of science and technology studies as a field, before looking closer at the field's political turn starting in the early 2000s. In this account, I will focus on two tendencies in how politics has been approached in science and technology studies that I believe have led research in the field away from the traditional sites of politics where political researchers like the ones in focus in this thesis are found. Following this argument, I will look at a few central perspectives on knowledge in politics from political science and political sociology: the work of American political scientist Hugh Heclo, who

was one of the earlier policy theorists to recognise the importance of the knowledge of political actors; the epistemic-communities framework of Peter Haas, in which research has primarily focused on when and how groups of scientists gain influence in policy-making; and the recent Swedish literature on policy professionals, a group which is argued to affect politics with knowledge as their main tool. These different literatures and theorists all treat politics as epistemic and knowledge-production as political, though without paying empirical attention to the production of knowledge in politics. Finally, I will, drawing on these different literatures, discuss how the present project of political epistemology connects to and differs from similar research, and some of the assumptions about knowledge, politics and political research I start from.

Science studies

This thesis largely draws on theories, insights and sensibilities from science studies; primarily from the sociology of science and from science and technology studies (STS).[14] STS is an interdisciplinary field of social scientific research on science and technology, the history of which is usually traced back to the sociology of scientific knowledge, SSK, in the 1970s (e.g. Sismondo, 2010). The central principle of SSK was a commitment to opening all aspects of scientific research to sociological inquiry (Bloor, 1991). This principle was formulated in contrast to two other ways of understanding science: first, Robert Merton's sociology of science, in which scientists were studied as social actors – most famously, but not exhaustively, as following a set of shared norms for scientific research – but in which the scientific contents of research was explicitly defined as closed for sociological inquiry (Merton, 1974), and second, epistemological understandings of science, in which scientific facts were seen as unproblematic reflections of reality leaving room for sociological explanations of science only when scientists are led astray by bias or other social factors. Inspired by Thomas Kuhn's work on scientific paradigms and revolutions (1970), proponents of SSK argued that scientific research is a social phenomenon like any other, all aspects of which can be understood sociologically, including the production of what is taken to be true scientific facts. David Bloor, one of the central figures of early SSK, formulated four

[14] I have, throughout this thesis, done my best to avoid the use of three-letter acronyms. In this chapter, however, avoiding some of the more well-established ones in the sociology of science and science studies turned out to be too cumbersome.

principles which have, to different degrees, remained central as SSK developed into the contemporary field of STS: causality, meaning that explanations should be causal; impartiality in relation successful and failed science; symmetry in the style of explanation for beliefs that are taken to be true or false; and reflexivity, in that the sociology of science itself should be open for study using its own explanations (Bloor, 1991).

One of the central themes in the early work in SSK was the study of scientific controversies: historical or contemporary situations where scientists or groups of scientists disagree on specific scientific issues. In the Edinburgh school of SSK, such controversy studies generally focused on historical controversies, drawing on the social interests of scientists to explain differences in preferred methods or theories between scientists (Barnes, 2015; Jacob, 1976; MacKenzie, 1978). In chapter 6 of this thesis, I discuss such interest explanations further, situating them in a longer sociology of knowledge history of associating collective social interests with knowledge, in relation to the connections between interests and research at LO. Outside of the Edinburgh school, and in the study of contemporary science, the laboratory ethnography became an important method for studying science in the mid-to-late 1970s, with a few studies gaining paradigmatic status in the field (e.g. Knorr Cetina, 1981; Latour & Woolgar, 1986). Early work in SSK, perhaps especially work on contemporary science, often made sociological arguments challenging assumptions in the philosophy of science, empirically demonstrating phenomena like the interpretability of evidence (Collins, 1981) or the potential trajectory of factual claims from subjective to objective, to implicit (Latour & Woolgar, 1986).

In the 1980s, the field developed in two major ways. The first development was that the Edinburgh school, which had served as a theoretical centre in the field, came to be challenged from both a position of ethnomethodology (Woolgar, 1981b) and from proponents of actor-network theory (Callon & Law, 1982; Latour, 1983) for giving sociological understandings of society an epistemic privilege that they were unwilling to extend to the natural sciences when seeking sociological explanations for the production of scientific facts. The latter of these two challengers, ANT, has since come to hold something of a hegemonic position in the field, and while the term ANT itself is no longer particularly commonly used, contemporary STS largely – but certainly not exclusively – builds on its sensibilities and assumptions, particularly its radical anti-essentialism and its refusal of a priori distinctions between human and non-human actors. The SSK tradition, however, has lived on in parallel to this development, as have other approaches in the sociology of science and in STS. The other major development in the field was that social constructivist

perspectives on technology were incorporated into science studies, leading to the unified field of STS (Pinch & Bijker, 1984).[15]

Science, technology and politics

Attention to the interrelatedness of science and politics has been foundational to the field of STS since the beginning of the field, with a recurring theme being to have "identified politics where politics was not supposed to be" (Moore, 2010, p. 793). Throughout the 1970s and 1980s, the argument that science is inherently political was forcefully repeated in the field, through different theoretical, analytical and methodological approaches. Studies in the Edinburgh School of SSK argued that epistemic beliefs held by scientists were informed by the political interests of the social groups scientists belonged to, including interests in demonstrating the political usefulness of science (Shapin, 1975). Also within the SSK tradition, Simon Schaffer and Steven Shapin argued in their seminal study on the development of the modern experiment that Robert Boyle and Thomas Hobbes' conflict over what science should be also was a conflict over how society should be governed (Shapin & Schaffer, 2011). Outside of SSK, the early works of Bruno Latour, Steve Woolgar and Michel Callon showed how scientists both depend on and fundamentally change politics (Latour, 1993a; Latour & Woolgar, 1986), and Latour, Callon and John Law tried to develop a broad analytical language for capturing both scientific and political representation (Callon, 1984). In the field of technology studies, Langdon Winner famously argued for the inherently political effects of technological artifacts (Winner, 1980), while feminist STS scholars showed how neither science nor technology was politically neutral (Cockburn, 1983; Harding, 1986). In the turn to ontology of the 2000s, Annemarie Mol discussed the political implications of how realities are enacted in healthcare practices

[15] The use of the acronyms SSK and STS is complicated and contested. SSK is sometimes taken to be the Edinburgh school and the Bath school centred around Harry Collins specifically, which is how I use the term here, though Latour and Woolgar identify their work in Laboratory Life as SSK (Latour & Woolgar, 1986), as does Malcolm Ashmore in his thesis drawing on the reflexive approach advocated by Woolgar (Ashmore, 1989). The Edinburgh and Bath schools are also called the Strong Programme, as opposed to the supposedly weaker Mertonian sociology of science. The more inclusive terms science studies or social studies of science have also been used instead of SSK, to mark the interdisciplinarity of the field, as opposed to identifying it as a field of sociology. In my use of the terms here, I have aimed for simplicity while aligning to what I perceive as the standard usage today, and I use science studies as a broader category of social scientific research on science, including sociology of science and STS, as well as history of science and other related fields.

(Mol, 1999a, 2012), and, relatedly, Steve Woolgar and Daniel Neyland explored relations of governance and accountability in the ontological constitution of mundane technological objects (Woolgar & Neyland, 2013).

One culmination of this series of arguments on the interrelations between science, technology and politics can be found in the idiom of co-production, most strongly associated with the work of Sheila Jasanoff (2004): "the proposition that the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it" (p. 2). The idiom of co-production is not just an argument about the interrelatedness of knowledge production and politics, but also a solution to problems of social versus technological or natural determinism; as society and politics on the one hand and science and technology on the other were considered to be inseparable from each other, the question of if science and technology are determined by social factors or if society and politics are driven by nature and techno-scientific development became moot. A co-productionist understanding of science and politics informs this thesis. While Jasanoff's argument that politics and knowledge production are entangled is focused primarily on scientific research, what I aim to do in this thesis is, in a sense, a mirror image: knowledge production drawing on technical expertise is an important part of politics, as political organisations need to understand the world they act in and such understandings in contemporary democracies are often produced through such research. My approach is thus largely in line with the idiom of co-production, though unlike the majority of this literature, my attention is not directed towards the political aspects of the sciences, but to a form of research native to the world of politics.

Democracy and science

STS research on politics has generally been informed by a normative position striving for democratic participation in science and technology. While most historiographies of STS trace the field back to SSK, there is another origin to the field in the critical voices raised against scientific and technological development in the 1970s, often by scientists and technologists themselves. This activist version of STS called for social responsibility and democracy in science and technology; a theme that has continued to be present in the field alongside the ambitious theoretical projects associated with studying science empirically (De Vries, 2007; Sismondo, 2010). Although calls for a democratisation of the sciences have varied in how they understand both science and democracy, the problem has generally been seen as being that scientific research and expertise play an increasingly important role in modern

democracies, and that there is a lack of democratic accountability of scientists and experts. Starting from this understanding of the problem, there was what has been called a turn to politics in STS in the early 2000s (Asdal et al., 2007), when authors from both the SSK tradition and in ANT proposed reconceptualisations of expertise and science in relation to politics (Callon et al., 2009; Collins & Evans, 2002; Latour, 2004a; Stengers, 2010), or otherwise turned the conceptual tools and assumptions of STS towards politics (Latour, 2003; Marres, 2005; Mol, 1999b).

In the SSK tradition, Harry Collins and Richard Evans argued for a third wave of science studies, in which the role of experts in relation to politics was the object of study. More specifically, they suggested a normative view of expertise in which the experiential expertise of non-scientific practitioners and others with relevant experience can be recognised, while still limiting the influence of the general public on decisions where expertise is needed (Collins & Evans, 2002). This proposal was roundly criticised by authors outside of SSK for relying on overly simplistic ideas of politics: for treating expert knowledge and policymaking as separate phases of political processes (Jasanoff, 2003), and for limiting their view to who is to be taken to be an expert on a given issue, ignoring questions of framing and meaning-making that lead up to definitions of what the issue is in the first place (Wynne, 2003).

Around the same time, Bruno Latour published his book Politics of Nature which, drawing on Isabelle Stengers' notion of cosmopolitics, suggested what he called a new constitution for science and democracy (Latour, 2004a; Stengers, 2010). In this new constitution, the concept of a singular nature as distinct from society was abolished, and with it the division between facts and values. Instead of producing truths from the realm of nature that politicians can make use of in their decision-making, the role of scientists was to act as unreliable but necessary spokespersons for new entities – brionic diseases, for example, or ecological crises – as they enter the shared collective of beings, while the role of politicians was to decide how these new entities should be dealt with (Latour, 2004a). Critiquing the book, Gerard De Vries (2007) argued, with some similarity to the criticism against Collins and Evans, that Latour's conceptualisation of politics, and conceptualisations of politics in STS more broadly, was one borrowed as-is from political science, in which the political is understood as an aggregation of the preferences of sovereign individuals. This unreflexive adoption of standard understandings of politics was put in contrast with how authors in STS in general, and Latour in particular, have refused to accept standard-philosophical accounts of science, preferring empirical accounts of the everyday work of scientists. De Vries proposed a different approach drawing on an Aristotelian view of politics as

the praxis directed towards the highest good. Similarly to Latour's suggested constitution, however, the key role of scientists in De Vries' approach was to introduce new objects to the realm of politics (De Vries, 2007).

What is shared between these approaches to expertise and democratic politics is the mode of critique against them: the charge has been that science studies has relied on underdeveloped notions of what democratic politics is, quite unlike how the field has challenged established understandings of the sciences.

Simplistic accounts of politics

The standard STS version of democratic politics has roughly been as follows. Scientists, and other experts, are assumed to engage with political processes with a legitimacy in making epistemic claims – either through making propositional claims as experts, as in Collins and Evans, or as spokespersons for new things that demand the attention of politicians and the public, as in Latour and De Vries. This legitimacy is taken to be, at least potentially, at odds with the democratic legitimacy of elected representatives, as these epistemic claims force, constrain or otherwise affect political action. Compared to the complex and empirically grounded understandings of the work of scientists, the political side of this picture has been underdeveloped, often substituting simple notions of increased public participation as a normative good for more nuanced understandings of political processes. This problem has been recognised recurringly in the field (Moore, 2010; Soneryd & Sundqvist, 2023).

One solution to this problem is to provide versions of political theory for STS (e.g. Brown, 2009; Latour, 2003; Marres, 2007). Another solution is to approach democratic politics empirically, similarly to how STS researchers have studied science and technology with a commitment to empirical research over theoretical accounts. This latter approach is what I am hoping to contribute to with this thesis. While there are a few notable examples of such research (e.g. Birkbak & Papazu, 2022), they are relatively rare, and the political turn in STS has primarily taken the form of studying the influence of science, technology and scientific expertise on political processes.

Studying political settings empirically with few a priori assumptions seems like a clear way forward for a field characterised by empirical research with a recognised problem of underdeveloped understandings of politics. With this thesis, I am proposing one specific avenue for such empirical research – focusing the epistemic work of political actors – which, to my knowledge, has not been clearly explored in STS. Considering how simple the move of approaching politics in the same way that the field once approached science is,

the scarcity of empirical STS studies of political sites, and the lack of attention to the knowledge-making of political organisations, may call for an explanation. One such explanation can be found in the manner in which politics has been approached in STS research. More specifically, two different characteristics of how politics has been approached have, or so I think, unnecessarily limited the empirical scope of research in the field: first, a general preference for studying qualitatively new actors, and second, an issue-centred political ontology in the post-Latourian strand of STS.

Two characteristics of the political turn

Despite a focus on everyday practices in science and technology, STS attention to politics has been marked by a general preference for the novel and spectacular over the established and mundane. A large part of the attention of STS scholars looking at politics has been on how new, generally non-human, actors enter the political stage through scientific discovery or technological innovation; actors such as acid rain (Asdal, 2008), nanotechnology (Krabbenborg, 2022) or first-trimester CUB screening (De Vries, 2007). This follows from how Latour describes the meaning of politics primarily used in STS: "Every new non-human entity brought into connection with humans modifies the collective and forces everyone to redefine all the various cosmograms" (Latour, 2007b, p. 5). For a field focused on the study of science and technology, it is perhaps unsurprising that attention has been directed towards what is produced in the world of techno-science. For those interested in arguing that non-human entities are central to what is otherwise described as social (Latour, 1993b), showing the importance of technological artifacts and the natural phenomena of science to politics serves a secondary function of furthering that argument. Taking the notion of a turn to politics in STS seriously, however, this focus is rather limited: as important as the production of qualitatively new entities are for politics, much of the activities that go into what we think of as politics – almost regardless of how we define it – are not meaningfully related to the production of new entities for the polity to react to. I call this a preference for the new rather than a preference for science and technology because I believe that it may have more general implications, being a preference for innovation and novelty in general. An example of this is that the relatively few empirical STS studies of democracy beyond its interactions with science and technology have tended to look at experiments and innovation in public participation (Lezaun et al., 2017) such as citizen panels (Voß et al., 2021; Voß & Amelung, 2016); phenomena which, while readily

understandable by scholars used to studying technological innovation, form a fairly marginal part of democratic politics generally.

If I am right in pointing out this tendency, the solution is simple: pay attention to other things. The other characteristic I have in mind may necessitate a deeper reconceptualisation of politics. STS approaches to democratic politics have since the political turn of the early 2000s – a turn mainly involving researchers in a post-Latourian, or post-ANT tradition – largely drawn on a political ontology in which individual issues are the fundamental unit of politics. This view can be traced back to pragmatist philosopher John Dewey's understanding of democracy, in which publics are constituted in relation to specific problems faced by groups of people, instead of simply assumed to exist. Dewey's association between publics and problems came out of his 1920s debate with Walter Lippmann, who had a pessimistic view of democracy in which a key problem was the non-existence of the public – or the democratic demos – as anything but a useful fiction for the powerful (Lippmann, 1993). In response to this view, Dewey's argument was that politics, government, and democracy all arose out of the need for solutions to shared problems, generally caused by the actions of others, constituting those affected as publics in their collective response to the issues (Dewey, 2016).

Dewey's linkage of problems and publics was picked up by Noortje Marres, who saw in the Lippmann–Dewey debate the outline of an STS approach to democracy where the formation of new political issues and their associated publics was the empirical target. Marres argued in her PhD thesis that prominent STS approaches to politics already centred issue formation empirically, but that a theoretical argument for why issues are central to democratic politics was missing. The constitution of democratic publics through issues in the Lippmann-Dewey debate was useful as such an argument, by providing an understanding of democracy in which specific issues rather than lofty ideals and constitutional frameworks were the stuff that democracy is made of (Marres, 2005, 2007).

Marres' programme was quickly and broadly picked up within the field, with her thesis supervisor Bruno Latour – already at the time interested in Dewey's pragmatism (Marres, 2023) – as one of its most vocal proponents. In response to De Vries' criticism of his Politics of Nature, Latour posited that "politics, even in the narrower way in which it has been conceived by political sciences, has always been in effect an issue-oriented activity" (Latour, 2007b, p. 103), before presenting a model of different meanings of the word politics corresponding to different stages a political issue can move between. The introduction of new entities to the polity was the one where an STS contribution to understanding politics was the clearest (Latour, 2007b), but the

political issue was in Latour's model central to all meanings of politics, which was assumed to be issue-based as a matter of political ontology. As politics was fundamentally and ontologically a matter of issues, it follows that inquiries into politics should start from the issue.

Latour's issue ontology of politics was further developed in An Inquiry into Modes of Existence (2013), in which one of the limited number of modes of speaking and acting in Western societies was to do so politically. The political mode, in that book, is defined in contrast to views of politics as a specific domain with specific contents; rather than understanding politics as being what goes on in relation to certain institutions or what is related to certain social problems, Latour understands political action as the formation of political collectives – or publics – around specific political issues. Such publics need to be reformed for every new issue, and any stability of political collectives across issues is the outcome of repetition and habit. In contrast to the starting point of this thesis – that doing politics involves the production of epistemic claims that can be meaningfully studied as such – epistemic reference is taken to be a threat to the political mode of speaking and acting: in Latour's account, political speech appears untruthful when compared to the specific criteria for veracity of research, leading to widespread cynicism towards politicians and politics more generally, not least among social scientists who, in Latour's account, tend to understand political actors as Machiavellian power-maximisers. Along with positioning issues at the centre of his ontological account of politics, Latour thusly purifies the political from the epistemic, which is taken to be incommensurable with the mode of politics.

The notion that politics is best understood by looking at specific political issues is by no means unique to STS; one prominent proponent of such a view is political scientist Hugh Heclo (1978), who I will return to in the next section of this chapter. It may be unusually easy for STS scholars to adopt such a view, however, for a few different reasons. First, the paradigmatic role of controversy studies in STS, across different theoretical frameworks in the field, may have preconfigured the adoption of an issue ontology. The controversy studies approach has an, at least surface-level, elective affinity with an issue-based political ontology in that you take specific questions on which there is disagreement as your analytical starting point, focusing on the groups involved. Second, treating politics as a matter of political issues follows somewhat naturally from the preference to look at the introduction of new actors; the new entities brought forth by science and technology generally enter the world as issues that other actors take a position in relation to. And third, an issue-oriented political ontology in which publics are created through political issues is highly congruent with the anti-essentialism of post-Latourian

perspectives, as the relevant political subjects – the publics – are created in interaction around the issues, meaning that the sociologist can follow the actors in deciding who the relevant groups are rather than assuming the stable existence of a body politic a priori. The issue ontology, however, along with the preference for studying qualitatively new things, seems to have limited the scope of the political turn in STS.

From ontology to analytical choice

My argument here is not that the preference for studying new political entities or an issue-based political ontology is wrong; if anything, the view of specific issues as at the centre of political processes seems even more apt today than in the early 2000s, if contemporary diagnoses of a current state of polycrisis in world politics are any indication. My argument here is rather that such an ontology directs our attention towards certain phenomena and away from others by deciding how politics as the object of study works a priori, with the consequence of producing a warped and limited view of what politics is. One central area which is likely to be obscured by defining politics as fundamentally issue-oriented and focusing on techno-scientific innovation is the work of traditional political actors, such as political parties, think tanks and, especially in the Nordic countries and of relevance for this thesis, trade unions. Such actors engage with multiple issues, and they enact entities such as ideologies or class interests out of the positions taken across multiple issues. In political systems with a tradition of corporatism especially, such as the Swedish one, they have also been crucial for the formation of publics without much reference to specific issues: the working-class in Sweden has been enacted and stabilised specifically through the activities the labour movement broadly and LO specifically, in a way that seems irreducible to specific issues and does not seem adequately captured with reference to the habitual repetition of individual instances of public formation (pace Latour, 2013).

The target of the turn to politics in STS has been, as Kristin Asdal put it, to "to conduct empirical studies of politics, understood as practice in contrast to politics as a formal, strictly institutionally localized activity" (Asdal et al., 2007, p. 44). While narrowly defining politics as strictly localised to certain institutions would undoubtedly obscure significant parts of political processes, ignoring the institutions to which a more traditional view localises politics seems even more problematic. Asdal has elsewhere, together with Bård Hobæk, argued along similar lines, pointing to how the issue focus of the political turn has led it away from central political settings such as parliamentary politics. Asdal and Hobæk found a solution to this by reading

some of Max Weber's work on politics in light of the issue focus in STS. In Weber, they found a view of parliamentary politicians as potential instigators in the formation of political issues out of individual cases from the bureaucracy, and as actors who modify both issues and their relation to publics, thusly finding a justification for studying traditional political institutions within the issue ontology of politics (Asdal & Hobæk, 2020). To me, this seems like a somewhat unnecessary justification for studying parliaments – or trade unions – when one is interested in politics.

Rather than finding ways in which an issue-oriented approach to politics can be broadened to include empirical sites like parliaments, parties, interest groups and trade unions, I want to propose a reconceptualisation of the role of issues in politics from being a matter of ontology to being one of several possible analytical choices. Deciding that politics ontologically are about political issues have, as Asdal and Hobæk argued, directed research attention to certain empirical sites and away from others. More than this, it directs the analysis to certain kinds of question, focusing our attention on how actors form alliances and mobilise others in relation to the specific issues we are focusing on, whether we are studying technological innovation or parliamentary politics. Letting go of the assumption that the fundamental unit of politics is the issue as a matter of ontology does not disqualify analytical attention to issues and public formation, and for plenty of political questions such an approach may be the most useful one. It does, however, open up the political turn in STS to sites and phenomena which are commonly understood as political but are difficult to approach within the issue ontology; treating an issue focus as an analytical choice rather than a natural extension of what politics fundamentally is opens the door for empirical inquiry of political settings with different things in focus. In addition, assuming less about the nature of politics a priori seems theoretically generative for reasons that are similar to why early science studies was generative; fewer a priori assumptions enable the challenging of widely-held beliefs and the generation of novel insights.

What I am suggesting with this thesis is a specific kind of broadening in the approach to politics: that of paying attention to the epistemic work of political organisations, or in other words, extending STS research on politics into political epistemology. Such an approach is one where insights about research and knowledge production from STS and from the sociology of science can be used with a focus on political actors and their activities, while being cognisant of the specificities of and differences between politics and science. It differs from both of the tendencies outlined above in that both issues and the new phenomena of science and technology enter the picture only when they become

relevant for the political actors, instead of taking them as the starting point for the analysis. It also differs in that it – similarly to Jasanoff's idiom of co-production (Jasanoff, 2004a) – treats knowledge as something intrinsic to politics, instead of studying the interactions between scientific knowledge production and policy-making, or treating the making of epistemic claims as anathema to the political mode of existence (Latour, 2013). In contrast to Latour's political ontology (Latour, 2003, 2013), the approach I suggest is closer to treating politics as a domain than as an activity, as the sociologist needs to have decided a priori that a specific site of study is a site of politics in order for the political epistemology to make sense. Compared to the issue ontology, however, it is also deflationary, in the sense that the nature of politics is left undefined beyond this initial decision that the object of study is a political one, and the relationship between the political and the epistemic is left open for other actors to decide; similarly to how one does need to solve the demarcation problem of science to claim that a laboratory ethnography is science studies, no rich conceptualisation of politics is needed to study a political setting.

My argument here is that by abandoning the issue ontology, STS scholarship on politics could be more easily extended to the study of how political actors produce the worlds in which they act, and consequently, that the knowledge production of political actors could be better understood drawing on insights from the study of science. The word extending, in this context, implies that this empirical object is located on the metaphorical edges of existing STS literature on politics. Another quite plausible way of approaching this political epistemology is from literatures situated at another border to this empirical phenomenon: literatures in political science and political sociology which recognise the role of knowledge in politics. Before discussing some of the assumptions of the political epistemology I am suggesting, I want to discuss how some key frameworks in these literatures have approached – although not fully engaged with – the knowledge production of political organisations.

Knowledge and policy-making

In mainstream political science, knowledge has been recognised as something with causal effects in policy-making, as opposed to something epiphenomenal to power struggles, since at least the 1960s (Radaelli, 1995). Some research approaches in the study of political processes have paid special attention to the role of knowledge in politics, making up a kind of mirror image of STS

scholars interested in the realm of politics in the sense that the interrelations between epistemic claims and politics are approached from the political side. I will here focus on three such approaches in relation to the political epistemology approach I am suggesting with this thesis: the work of Hugh Heclo, the epistemic-communities approach of Peter Haas, and the recent Swedish literature on policy professionals. This is in no way an exhaustive review of the vast literature relating knowledge to politics. As both of the key terms in this relationship are unusually wide in scope and have varying definitions, any number of scholars could be said to belong to this literature; perhaps most notably, though still outside of the scope of this review, researchers in Foucauldian traditions, and historical studies on the parallel developments of academic social science and Western welfare states in the 20th century (Larsson, 2001; Pettersson, 2003; Rueschemeyer & Skocpol, 2017). Unlike these traditions, the examples here are chosen because they in different ways start from similar assumptions to myself, and because they have focused specifically on the role of knowledge and knowledge production internal to politics.

Hugh Heclo, knowledge and issues

Heclo, an American political scientist, was one of the earlier scholars to treat knowledge as something independent from political power in the policy process. Heclo wrote his PhD thesis, later published as a monograph (1974), on the development of the modern welfare states in the United Kingdom and in Sweden, taking – similarly to the issue ontology in STS – individual policies as the unit of analysis.[16] Despite not explicitly focusing on the role of knowledge and knowledge production in policy processes, he understood political actors as trying to understand the world they act in as much as they are trying to change it. On the interests of political actors, for example, he argued that interests are "self-serving but they have not been self-defining in the policy process. Substantive pressures from any group have depended on the capacity of the organization to recognize its interests to be at stake and to define what that stake is" (Heclo, 1974, p. 271). This view differs from one in

[16] A curious, but incidental, connection between Heclo's thesis and my own is that the LO economists figured in his study as influential producers of Swedish public policy in the post-war period. As discussed in the previous chapter, the institution of the LO research units in the 1940s was informed by the perceived need for a centre for policy development within the Swedish labour movement, and the LO economists are generally seen as having been such a centre in the post-war period.

which the interests of actors are taken to be given a priori, and recognises the activities required to produce and to within a given organisation successfully argue for what their interests are – activities that I would situate in the realm of political epistemology, as they treat political interests as something epistemic. A parallel can be drawn to parts of the criticism levelled against the Strong Programme in STS from ethnomethodological and ANT perspectives, in which the problem identified with the Strong Programme was that they took interests to be able to affect actors and to be known the analyst, instead of treating them as emerging in interaction (Latour, 1983; Woolgar, 1981b); unlike interest explanations in SSK, Heclo treated interests as internal to the empirical situation in the sense that they needed to be understood by the actors in order to have an effect. More generally, and in line with the STS notion of co-production, Heclo concluded from his comparison between the UK and Sweden that governments "not only 'power' (or whatever the verb form of that approach might be); they also puzzle. Policy-making is a form of collective puzzlement on society's behalf; it entails both deciding and knowing" (Heclo, 1974, p. 305). This is not to say that Heclo discounted differences in influence and power in his analysis of policy, but that he saw epistemic practices as necessary activities for political actors, powerful or not, to be able to act in the first place.

Heclo's most influential contribution to the study of policy-making is found in the concept of issue networks (Heclo, 1978). Similarly to Asdal's call for the STS turn to politics to go beyond notion of politics as something strictly institutionalised to specific settings, the concept of issue networks seeks to capture all those engaged in a political issue, including activists, experts, business interests and concerned citizens. The basic analytical unit is, as the name implies, the singular political issue, and an issue network is characterised as "a shared-knowledge group having to do with some aspect (or, as defined by the network, some problem) of public policy" (p. 449). The issue network has no clear boundaries, and very little is assumed about the actors in the network apart from being involved with the issue and with others involved with the issue; Heclo does not assume, for example, that all participants in a network want to effect political change, finding instead that some just want to know what is going on in relation to the issue in question. An outcome of the network, however, is that a shared understanding of the political issue in question is produced by those who belong to it; as in his comparative study of the UK and Sweden, knowledge is centred in his approach, alongside power and influence. Unlike Latour's ontological claim that politics "has always been [...] an issue-oriented affair" (Latour, 2007b, p. 13), Heclo saw attention to issue networks rather than traditional political actors as necessitated by what

he perceived as a shift in American politics: the political parties were seen as being in decline, with the power associated with them dispersed among a multitude of actors (Heclo, 1978).

Heclo's approach to knowledge in politics has some affinity with the post-Latourian STS research discussed earlier, and consequently differs from the approach I am suggesting in similar ways; especially by taking the political issue as the fundamental unit of analysis. In general, his framework is characterised by a remarkable theoretical openness, seemingly understanding policy-making on its own terms instead of imposing a theoretical framework on it. Despite recognising that knowledge in different forms is central to understanding how policy is made, however, Heclo paid little attention to how knowledge is made; the issue network concept invites analysis of how connections are made between different actors, rather than of what goes into the understanding of singular actors. This is a further difference in my own approach to that of Heclo's: agreeing that the knowledge of political actors is central to how politics is made, I aim to look empirically at the practical details of these knowledge processes.

A difference between the Latourian issue ontology and Heclo's issue networks is that Heclo historicised the analytical usefulness of issue networks as dependent on a perceived change in American policy processes rather than as a consequence of a political ontology. This is in line with my own argument that an issue focus should be seen as analytical choice rather than ontological necessity. Drawing on the decline of corporatism in Sweden, as discussed in chapter 2, a similar argument could be made for the Swedish context, and something like it is implied by the common view in political science that corporatism has been replaced by lobbying as the main avenue of political influence in the Swedish political system (e.g. Lundberg, 2015); if a corporatist political system assumes and enacts relatively stable social groups to be represented through national organisations, a system with a stronger emphasis on lobbying is likely to lead to political mobility of actors between issues in such a way that taking the individual issue as the analytical starting point makes sense. Such a political change, however, is unlikely to have happened to such an extent that traditional political organisations are no longer relevant for understanding political processes.

Epistemic communities

Another strand of research with some similarities to Heclo's issue networks is that centred around the concept of epistemic community. The most influential operationalisation of the concept was written by Peter Haas for a special issue

of the journal International Organization (Haas, 1992), where he, drawing on science studies forerunners Ludwik Fleck (2008) and Thomas Kuhn (1970), defined it as "a network of professionals with recognized expertise and competence in a particular domain and an authoritative claim to policy relevant knowledge within that domain or issue area" (p. 3). Epistemic communities were further defined by having shared sets of normative beliefs, shared causal beliefs in relation to their expertise, shared criterions for validity in their field, and a shared set of policy beliefs. They were argued to become relevant, and potentially influential, when policy domains are technically complex in a way that produces uncertainty, which was taken to lead to a perceived need for outside expertise. In Haas' operationalisation, what he called the shared causal understandings of epistemic communities – the shared epistemic, as opposed to the normative, understanding within a community of the issues they study – were approached in a constructivist way. The shared beliefs of community members were taken to be the outcome of social processes, and in Haas' version the "epistemic communities approach focuses on this process through which consensus is reached within a given domain of expertise and through which the consensual knowledge is diffused to and carried forward by other actors" (p. 23). Despite this, subsequent research centred around the concept of epistemic community has generally not focused on the production of knowledge in epistemic communities, but rather on how such communities achieve political influence (Cross, 2013). The framework as formulated by Haas is in principle open to the study of any kind of group with shared epistemic and policy beliefs (Haas, 1992), including the category of political researchers that I am interested in. With the notable exception of think tank scholar Diane Stone (Stone, 1996, 2004), however, researchers in the epistemic community framework in general have specifically studied university scientists with expertise related to policy issues, such as environmental researchers and economists (Cross, 2013).

As with the work of Heclo, there is some affinity between the epistemic community framework and STS approaches to politics. This affinity can be seen partly in Haas drawing on scholars such as Fleck and Kuhn, who within STS are frequently presented as part of the pre-history to the field (Sismondo, 2010), and partly – following these theorists – in a social constructionist approach to knowledge which enables a sidestepping of questions of veracity in epistemic communities' beliefs, in favour of paying attention to the social processes of consensus formation. The kind of research done within the field has differed markedly from research in STS, however, especially in the nomothetical aspirations of epistemic communities framework, in which universal claims about under which circumstances knowledge-actors influence

politics are sought (Cross, 2013), in contrast with STS research which tends to be more open to complexity and to situational specificity. Similarly, while the epistemic-communities framework in Haas' programmatic formulation is somewhat consistent with the political epistemology I am suggesting, the majority of the literature on epistemic communities differs from my own approach both in this tendency to look for universal truths, and in the narrow focus on when scientists influence policy.

Policy professionals

A third literature that approaches knowledge in politics is the recent (predominantly) Swedish literature on policy professionals: people who work with knowledge on a partisan basis to do things that ultimately are for the purpose of affecting policy (Svallfors, 2020, pp. 1–4). This category includes analysts and economists like the LO researchers I study – LO researchers have been interviewed by researchers in this literature, and the early LO economists were, as discussed in the previous chapter, portrayed as the first policy professionals in Swedish politics (Garsten et al., 2015) – as well as political aides and other employees of political organisations. The starting point of this literature was the publication of the 2015 book *Makt utan mandat: De policyprofessionella i svensk politik*, which roughly translates to "Power without a mandate: Policy professionals in Swedish politics", by anthropologist Christina Garsten, sociologist Stefan Svallfors and political scientist Bo Rothstein. The book was the culmination of a study involving a large career database of and 100 interviews with people employed by political organisations, produced and conducted by research assistants Niels Selling and Björn Werner. *Makt utan mandat* has been quite impactful in the Swedish context: it was extensively reviewed on the culture and editorial pages of Swedish dailies when it was first published (e.g. Möller, 2015; Rydell, 2015; Swedin, 2015), and it has been frequently cited in the (relatively small) Swedish academic literature on think tanks and other epistemic policy actors since its publication (e.g. Åberg et al., 2020; Sörbom, 2018).

Drawing on a warning made by Robert Dahl that increasing complexity of modern societies might lead to a technocratic erosion of democracy, the authors argued that policy professionals should be understood as a coherent category of employees in modern societies. This categorisation was made not just on an analytical level; they argued – by looking at how policy professionals move between the similar jobs – that policy professionals form a community with certain shared characteristics, or, as they put it, a tribe. Within this tribe, the authors made an ideal-typical distinction between hacks, focusing on

political strategy, and wonks: "specialists at the contents of politics, [who] think about how institutions and systems should be shaped, how new or existing knowledge in a policy area can be framed so that it becomes a resource for their own party or employer" (Garsten et al., 2015, p. 258, my translation).[17] Knowledge is presented as the main tool of all policy professionals, and the knowledge they work with is divided into three categories: processual knowledge, which is knowledge about the political process in practice; surveying, which is the ability quickly find reliable facts; and – of special relevance in relation to my own approach – problem formulation, which is defined as "describing the state of society, often with the help of research-based indicators, in a way that benefits the values and interests they are meant to serve" (Garsten et al., 2015, p. 256, my translation). Based on these two distinctions, the LO researchers of this thesis could be described as wonk-ish policy professionals who primarily work with problem formulation.

The stated intent of the book *Makt utan mandat* was to launch a research programme on this category of political employees, and from the present vantage point nine years after its publication, the authors seem to have been moderately successful in this. Researchers involved in the original project have continued to study the influence of policy professionals (Selling, 2021; Svallfors, 2016b) and their motivations (Selling & Svallfors, 2019; Svallfors, 2016a), as well as comparing Swedish policy professionals with other national contexts and presenting the programme in English (Svallfors, 2020). Others, too, have picked up the policy professional framework, studying their roles and influence in civil society (Mellquist, 2022b, 2022a), how they relate to their organisations' members (Mellquist & Sörbom, 2023) and their influence in other national contexts (Kelstrup, 2020).

Despite describing policy professionals in general as working with knowledge, and describing the production of accounts of society as one of the main ways in which they work, the process of knowledge production is barely engaged with in the book, nor has it been in the research programme that it started. Starting from an implicit realist position, knowledge is treated as an unproblematic resource; either as something that actors do or do not have, as when policy professionals' knowledge of the political system is presented as what they sell to the organisations they work for (Garsten et al., 2015) or when a knowledge gap is identified between policy professionals and members of civil society organisations (Mellquist & Sörbom, 2023), or alternatively, as

[17] For the Swedish-speaking readers, I would be remiss if I did not provide the authors' Swedish translation of hacks and wonks: *fixare* and *klurare*.

something that policy professionals draw on and frame when producing accounts of society that are beneficial to their employers.

The focus in the policy professionals programme has, as the book title "Power without a mandate" implies, been on the influence and legitimacy of policy professionals, and the programme as a whole has been marked by a kind of cynicism towards the group, which is primarily treated as a potential or actual threat to the democratic functions of society as a whole or of the specific organisations that employ them. Cynicism towards policy professionals may be justified; as Stefan Svallfors has pointed out, employing people to work with politics is a way to convert economic capital to political influence, and a political system where policy professionals play a central role is one where the financial means to employ them is important (Svallfors, 2016b). Combined with an unreflexively realist understanding of knowledge, however, this cynicism has led to an unrealistically bleak view of the epistemic work of policy professionals. Policy professionals seem to, like fraudulent accountants, keep two sets of books: one public book, in which they present whatever accounts are of gain to their employers, and one private book, in which they keep true and accurate knowledge about the world. When policy professionals write a report that is published by the organisation, for example, this is characterised as problem formulation, which, while often drawing on "research-based indicators" (Garsten et al., 2015, p. 256), is ultimately determined by the interests of the employer. This leaves little room for the research behind the report being guided by curiosity, or a perceived need to learn more about the topic in question. More problematically, the private knowledge needed to write such reports – including knowledge about what the interests of the organisation are – is left unaccounted for, and assumed to be something that policy professionals simply possess. While it is certainly possible to maintain a sharp distinction between research leading to beliefs held within an organisation on the one hand and what is presented to the public on the other – ExxonMobil's secret climate change models from the 1970s and onwards come to mind (Supran et al., 2023) – it seems unlikely as a universal description of what researchers in political organisations do. As research in the literature on policy professionals has focused on questions concerning the internal dynamics of this group, how they gain influence, and how they relate to their organisation and others rather than processes of knowledge production of policy professionals, this view of what policy professionals do when they do research is more of an assumption in this literature than an empirically supported claim.

The policy-professional framework is relevant to this study because it raises important questions about the influence of people working with knowledge in

politics, and because it is partly focused on the same kind of employees of political organisations that I am interested in. The policy-professional research, however, starts from the assumption that all employees in political organisations, including researchers but also political aides, speech-writers, and other categories of political employees, are one cohesive group with shared characteristics, while I am interested specifically in the kind of policy professional who works with research. Central of these shared characteristics of policy professionals is that their work can be reduced to affecting politics, in the sense that all of their activities – including research – should be understood as political strategy, seemingly assumed to preclude the production of knowledge in political research within the framework. Because of this, the policy-professional literature primarily serves as a point of contrast to my own research.

Political epistemology

The picture I have tried to paint in reviewing both the STS literature on politics and political science and political sociology literatures on knowledge above is of different fields adjacent to the kind of phenomenon this thesis is focused on – the knowledge production of political actors – without directing any substantial analytical attention towards it. A tendency has been, as in most STS approaches and in the majority of the epistemic-communities approach, to study how the work of scientists specifically relates to politics, and thus externalising knowledge from politics. For those who have treated knowledge as something intrinsic to politics, as Heclo and the policy-professionals framework above have done to different degrees, little attention has been paid to how epistemic beliefs are produced in politics. I am not trying to argue that this is a gap in the literature exactly, as if sociologists were collectively solving a jigsaw puzzle with a piece missing, but rather that these different literatures could plausibly and gainfully be extended into how research is done by political actors – albeit from different starting points – and that the insights of these different literatures are to varying degrees useful for understanding this phenomenon.

The approach I am proposing with this thesis is one of studying the knowledge activities of political organisations, focusing on the research of LO. For the rest of this chapter, I will strive to clarify my own approach in this. First, I will describe how what I do in this thesis connects to and differs from other research on similar topics. By doing this, I hope to clarify, by comparison

and by negation, the focus and interests of this study. Following that, I will end this chapter by discussing some fundamental starting points of this study and how I understand the central concepts of knowledge, politics and democracy.

Similar terms and approaches

The words political epistemology have been put together before – Latour used the term in Politics of Nature (2004), for example, as did Holger Straßheim in his attempt to bridge STS with critical policy studies (Straßheim, 2015) – but I use them as a description of the direction of this thesis rather than as a way of associating it with an existing literature.

In the present study, I draw inspiration from the literatures above, and especially from STS approaches to science. In the rest of the chapter, I aim to clarify the main assumptions and starting points of the study, both in terms of what I bring with me from the works of others and in terms of how I understand the empirical object of political epistemology in this thesis. What I mean with the term is empirical inquiry into how epistemic claims and beliefs are produced, reworked and circulated in the realm of politics; or, differently phrased, the production of knowledge internal to world of politics. Drawing on STS approaches to science, studying the topic empirically means trying to avoid preconceived notions of how knowledge and policy-making relate to one another, staying close to how the political-epistemic actors themselves produce realities in practice. This differs from the recent analytic philosophy turn to political epistemology, in which questions of justice in relation to knowledge access and testimony (Medina, 2022) and the relationships between truth and politics in the abstract (Hannon & Ridder, 2021) are explored.

In Straßheim's use of the term political epistemology, he was primarily interested in the role of policy experts in political processes, understanding a policy expert as an actor with both political authority and epistemic authority (Straßheim, 2015). In focusing the expert, his work is similar to a tradition within STS studying experts and expertise. This tradition includes the earlier mentioned third wave of science studies heralded by Collins and Evans (2002), but also, among other examples, Brian Wynne's study of Cumbrian sheep farmers' interactions with government technical experts following the Chernobyl disaster (Wynne, 1989), Stephen Hilgartner's Goffmanian analysis of the performance of expertise in science advisory bodies (Hilgartner, 2000), and Michel Callon, Pierre Lascoumes and Yannick Barthe's essay on technical expertise in democratic societies (Callon et al., 2009). While the people I am interested in throughout this thesis are a form of expert, and while I understand what they produce as a form of expert knowledge, what I am interested is not

expertise, but research. The difference is that expertise is a term used in relation to the non-expert laity. I am less interested in how the technical knowledge of LO researchers relates to non-expert audiences, or at least in how they relate to non-expert audiences in the role of expert, and more interested in the internal workings of LO research processes; the things that go into making epistemic claims and related policy when LO researchers write reports and other documents.

Another use of the term political epistemology is found in a book by historian of science Pietro Daniel Omodeo which, like this thesis, used these words in its title (Omodeo, 2019). Unlike this thesis, however, Omodeo's concern was not the research of politics, but the politics of science studies; drawing on a Gramscian theory of hegemony, Omodeo strived to explore ideological visions of science of the sociology of science, STS, and related fields. Apart from the differences in empirical and theoretical orientation, his book differed from my own project in that it drew on an understanding of politics as theoretically defined and expansive in the sense that the politics, or ideology, of anything could be explored, in contrast to the present study where politics is left undefined a priori beyond the initial decision that the object of study is political.

Besides these earlier uses of the term political epistemology, two other concepts are sufficiently similar to merit clarification on how the present study differs from them: Sheila Jasanoff's concept of civic epistemologies, and Andreas Glaeser's of political epistemics. While the present study has clear similarities to both of these, it differs from them in substantial ways.

With civic epistemology, Jasanoff referred to the shared cultural expectations of publics on the knowledge production of the state in relation to political decisions (Jasanoff, 1996). As discussed in the previous chapter, civic epistemologies specific to the Swedish context are central to LO research, not least in connection with the word *utredning* used to describe political and government research as a distinct form of knowledge making. What I am primarily interested in, however, is first, knowledge production located not only within the state apparatus, though civic epistemologies could plausible be extended to encompass non-state actors, and second, the ways in which political actors understand the world, rather than the shared cultural expectations of evidence by publics. The concept of civic epistemology, however, is close to the political epistemology in focus in this study in that expectations on the epistemic aspects of governance and politics are important for understanding how political organisations produce knowledge.

Political epistemics, on the other hand, was the label Glaeser put on his study of the interaction between knowledge and politics in the GDR, especially

during the last years of the republic. Drawing on historical ethnography of Stasi agents and dissidents, Glaeser tried to understand and provide a theoretical framework for the epistemic worldview of these two groups, with a particular focus on the rigidity of the epistemic beliefs of Stasi officers in the face of changing political circumstances (Glaeser, 2011). Like Glaeser, I am motivated by a dissatisfaction with standard accounts of the relationship between knowledge and power, agreeing that they "are neither solely the autonomous Socratic antipodes allowing knowledge to speak truth to power, nor [...] simply Foucauldian bedfellows augmenting each other in upward winding spirals" (p. xii). Unlike Glaeser, I am not trying to provide a generalisable theoretical account of how individuals in political movements come to maintain or change beliefs, but rather a local account of how epistemic claims and policy is produced within a political organisation; I am less interested in the psychological understanding of individuals, and more interested in processes of constructing more or less durable epistemic claims in politics. Another difference is that while the work of Stasi agents Glaeser studied is a form of knowledge production – and understood as such in his work – it differs from the research I am interested in that it is not technical knowledge production drawing on social science methods.

While I have argued that STS research pays little attention to political organisations and other sites of traditional politics, there are exceptions to this tendency. One notable such exception is the edited volume Democratic Situations (Birkbak & Papazu, 2022), which called for STS attention to democratic processes as empirical phenomena. The volume included, of relevance here, ethnographic studies on a course on debate and critique organised by Danish newspaper *Politiken* understood as potential infrastructure for public debate (Birkbak, 2022) and of the work of municipal electoral administrators, showing the fluidity of the distinction between political and administrative issues (Vadgaard, 2022). Alongside these, there are several relatively recent studies of parliamentary work grounded in STS perspectives (Asdal & Hobæk, 2020; Brichzin, 2020; Laube et al., 2020). These examples of studies of the traditional sites of politics can be seen as attempts to understand political processes drawing on perspectives and assumptions from STS. This differs from the political epistemology of this thesis in that I am specifically interested in a form of research taking place in political settings; I draw on the sociology of science and STS as my object of study is a kind of technical knowledge production with similarities to scientific research, rather than using STS perspectives to understand politics more generally.

Finally, while the literature I draw on primarily comes from science studies, the questions asked in this project find resonance farther back in the history of sociology: similar topics of inquiry were raised in the theoretical writings of the early sociologist of knowledge Karl Mannheim (1960, 1993). Mannheim posited that the beliefs of actors were associated with the social group they belong to, and his main research interest was on the relationship between political worldviews of social groups and knowledge. Assuming that any understanding of society is made within a political worldview, Mannheim argued that a science of politics – either as academic social science or as political research – is only possible either internally to such a worldview with local criteria for validity, or in an intellectual milieu that is not collectively too closely bound to any one social group. Those in such a milieu – a free-floating intelligentsia which Mannheim took to exist at universities – could synthesise the different political worldviews of a given society, the number of which Mannheim assumed was limited to five ideal-typical positions in early 20th century Europe (Mannheim, 1960), and produce a situational but meaningful picture of the intersection of knowledge and politics in a given society. The synthesis in this latter possibility for a political science seems implausible for a couple of reasons: the view of academic researchers as less socially bound than others seems more like a normative defence of academia than fair description, and positing a limited number of political worldviews seems unreasonably arbitrary. Mannheim's other possibility, however, of political research which is valid within the political context in which it is made, seems like a good description of the kind of phenomenon I take as the object of study in the political epistemology I propose.[18]

[18] The relationship between Karl Mannheim's sociology of knowledge and science studies since the 1970s is complex. Like several other prominent German-speaking sociologists in the first half of the 20th century (e.g. Schütz, 1960; Simmel, 1977; Weber, 1964), Mannheim attempted to carve out a space for interpretative sociology by differentiating it from the predictions and quantified measurements of objective science. In Mannheim's version of this, the epistemic claims of mathematics and the natural sciences were not bound to particular social positions in the way that social knowledge was (Mannheim, 1960). Dismissing this distinction between objective natural science and socially situated social science was crucial for opening up the hard sciences to sociological inquiry in the Strong Programme (Bloor, 1991). At the same time, several of the authors in the Edinburgh school drew heavily on Mannheim's work in their sociology of science (Barnes, 2015; Bloor, 1973), using central parts of his sociology of knowledge – especially the association between collective interests and beliefs – as model for the sociology of scientific knowledge.

Inspirations and assumptions

This study departs from a position on meaning drawing on American pragmatism. As a philosophical influence of the related frameworks of the Chicago school of sociology, symbolic interactionism, ethnomethodology, and Luc Boltanski's sociology of critical capacity, pragmatism has a long and diverse tradition in sociology.[19] Across the varied forms of pragmatist sociology is a shared interest in how the social world is made up, focusing how actors construct the world in practical action and experience. While such an interest does not follow with necessity from the work of the early pragmatist philosophers Charles Sanders Peirce, William James and John Dewey, they are also central to this study. Beyond a broad pragmatist position, I take inspiration from, without committing to, one perspective with affinity with American pragmatism: the material semiotics that has grown out of actor-network theory.

Material semiotics, as the name implies, is a tradition drawing on the semiotic study of signs and meaning – especially on the idea that the meaning of a sign is in its relations to other signs – extended to material and other non-symbolic phenomena. What this means is that any quality of an entity, or of a human or non-human actor, is found in how it relates to other entities. The term is strongly associated with the actor-network theory perspective, and in this formulation, such relations between actors form a rhizomatic web of connections – an actor-network – in which all action takes place, constantly reshaping the network. An actor, in this framework, is anything that has an effect, or acts, on other actors, which is to say, anything that exists, whether human and non-human, as existence without affecting anything else is taken to be a self-contradiction (Latour, 1999a; Law, 2009). The equation of humans and non-humans under the word actor is probably what actor-network theory is most well-known (or infamous) for. This non-humanism is an extension of a more general anti-essentialism in material semiotics where distinctions between, for example, micro and macro (Callon & Latour, 1981), linguistic and non-linguistic (Latour, 1999b) and local and universal (Latour, 1993a, p. 227) phenomena are treated as outcomes of action rather than as fixed qualities.[20]

[19] While Garfinkel positioned his ethnomethodology in relationship to phenomenology, especially that of Alfred Schütz, and to the work of Émile Durkheim (2002), I follow Mustafa Emirbayer and Douglas Maynard (2011) in stressing the affinities between the early American pragmatists and ethnomethodology.

[20] In this, actor-network theory is in line with James's and Dewey's anti-dualism and their respective arguments that ideal and material objects are made of the same stuff – pure experience – but enable different kinds of action (Dewey, 1905; James, 1904).

As with any similar framework, the perspective cannot be exhaustively captured in a description like the one above, but is better understood as "a disparate family of [...] tools, sensibilities and methods of analysis" (Law, 2009, p. 141). I would not place this thesis within that disparate family. My aim here is not to profess my belief in a metaphysical or metatheoretical position, but to help the reader situate some of the claims I make by pointing out some central inspirations. What I take with me from material semiotics is a strong anti-essentialism and a focus on process and action, treating any quality of an actor, including its existence, as a local outcome of interaction rather than as something passively existing; assumptions which are shared with other non-foundationalist perspectives, such as ethnomethodology (Lynch, 1993). Knowing these assumptions is probably – and hopefully – not necessary to follow my arguments for the majority of the thesis, but for parts of it, confusion may be avoided by having these assumptions in mind. In chapter 6, for example, where I discuss LO researchers' ability to speak for the workers, I take this to be an ability that effort goes into producing and that exists locally, rather than making a universal claim about their ability to represent others.[21] Similarly, when I discuss the democratic organisational structure of LO, I do not aim to make an evaluative claim of how democratic the organisation is in comparison with some abstract and normative ideal of democracy, but I rather accept the version of democracy used at LO as a legitimacy-producing set of activities and actors in its own setting. Crucially, I take all outcomes of LO research to be local achievements that are built to travel; they exist at the site of LO research, but they are constructed in such a way that they are able to retain some of their form in other settings (cf. Latour, 1987, pp. 223–228). When LO researchers produce epistemic claims and policy statements, for example, I am interested in how they are successfully made within LO research, but the effort that goes into making them successful within the organisation also means that they have a good chance to hold outside of the organisation, albeit variably depending on where they go. Epistemic claims do not immediately lose their ability to represent reality, nor do policy statements their ability to represent the will of the members, as they leave the building.

Two differences between this thesis and most work drawing on material semiotics are worth pointing out here. The first is that authors in this tradition

[21] A useful point of contrast here is Guyatri Spivak's argument that intellectuals' claims to speak for marginalised social groups – the subaltern, in Gramscian parlance – necessarily betrays these groups, as they are always excluded from speaking for themselves (Spivak, 1988). Such an argument hinges on an idea of what it would mean to speak accurately or truthfully for others as a point of comparison, while I take the ability to speak for others as a claim that can fail or succeed in its setting, but which is never universally true.

have often been sceptical towards the production of generalised accounts and explanations (Latour, 1988). While I share this hesitance, I am somewhat willing to do both of these things, and I sometimes understand my empirical object in general terms as recurring practices in LO research. The second is that unlike what is at least the reputation of actor-network theory, this study is generally focused on human actors, or not-quite-human actors such as documents, decision-making bodies, and policies; relatively little of my analytical focus is on materiality. This latter aspect of this study is primarily an artefact of the empirical setting, my research interests and the methods employed in this study. The people I study are social scientists rather than natural scientists, meaning that non-humans do not enter into picture as what the LO researchers work with; they speak for Swedish workers rather than for nature. In principle, a plausible argument could be made about, for example, how the materiality of LO publications have connected differently to different actors as they have changed from printed booklets into .pdf documents available online, or how the building of the LO headquarters make up a central actor in the organisation, with changing qualities as it has been repurposed and rebuilt over the 125 years since its original construction as the opulent residence of a wood merchant (cf. Gieryn, 2002; Sloterdijk, 2008). I have not found such arguments to be relevant to my own interests in this study, and if I had, a different kind of ethnographic study of the LO offices would have been necessary.

Knowledge and politics, research and democracy

Politics, as Max Weber has noted, is an extremely broad concept, comprising "any kind of independent leadership in action" (Weber, 1981, p. 77). Knowledge, similarly, is a notoriously contestable concept, with any belief held by an actor being what is probably the most well-known sociological definition (Berger & Luckmann, 1991). Within STS, no considerable amount of effort has been spent defining the word knowledge beyond the definition above, and the word itself has often been avoided, presumably because of its associations with analytic philosophy definitions like justified true belief. The word politics, on the other hand, has been the target of some analysis, one example being Mark Brown's categorisation of conceptualisations of politics in STS as being either of politics as activity or politics as site (Brown, 2015). For certain kinds of questions, it may be necessary to have fairly clear analytical definitions of these words; this may be the case if, for example, one is interested in arguing that science – or STS itself – is political (Jasanoff, 1996), or that politics are inscribed or otherwise present in technological

artifacts (Latour, 1990; Winner, 1980). For the purposes of the political epistemology I propose here, leaving the concepts somewhat loosely defined is preferable, allowing the empirical material to guide me in what both politics and knowledge are, in line with the slogan of following the actor (Latour, 2007a).

That said, some clarification on what I mean by knowledge and politics is necessary. The loose understandings I have of both concepts are informed by a theory of meaning, and a related theory of truth, taken from American pragmatism. Language is here taken to be a tool used for action in the world, rather than something existing beyond the world, and the meaning of words are in the context of their use rather than existing in the abstract. The implication for truth is that correspondence theories of truth, in which a proposition is true if it corresponds to a reality out there, become questionable, or even nonsensical, as propositions are in the world as much as what they are taken to correspond to, and as their meaning are not fixed but dependent on the context of use. In the pragmatic theory of truth, specifically in the formulation of William James, a belief is true if it is in agreement with reality as we engage with it, rather than with a reality independent of the person holding the belief (James, 1907), and the reality and the belief are not taken to be categorically different on an ontological level (James, 1904).[22] From this position on language and truth, I take knowledge to be something like linguistic claims that an actor takes to be true and that they believe that there is good reason that others should accept as truth as well. An understanding of knowledge as necessarily taking the form of contextual epistemic claims invites analysis on how such claims fail or succeed in the contexts in which they are made, or, drawing on the concepts of ordinary-language philosopher John Austin, how they as speech acts are felicitous or infelicitous (Austin, 1962); such a perspective on epistemic claims – along with policy as another category of linguistic statements produced in LO research – forms part of my analysis in this thesis.

[22] The most common, but commonly misunderstood, phrasing of the pragmatic theory of truth is that a proposition is true if it is useful to believe. The misunderstanding is that that would make anything true as long as we find it useful to believe in it, meaning for example that a person would be right in disbelieving anthropogenic climate change if they do not find that belief personally useful. The misunderstanding arises, as I see it, out of interpreting the pragmatic theory of truth as a normative theory telling us what we ought to believe, like correspondence theories are, rather than a descriptive theory, attempting to capture what we mean when we say that something is true instead of giving us usable criteria for deciding what is true.

More specifically, the kind of knowledge I am primarily interested in in this thesis is epistemic claims that result from research. As discussed in chapter 2, the kind of research I am studying is not understood as science by the researchers themselves, nor will I call it that in this thesis. It is, however, a kind of formalised knowledge production drawing on its practitioners' academic training as social scientists, and on methods of both research and presentation commonly found in academic social science; in broad terms, I understand their research as specific uses of the same technologies of representation used in academic social science. Consequently, while I draw on theory from science studies, I discuss research more than science in this thesis, while striving to be mindful of the specificities of the kind of research done at LO. When I do discuss science, it is primarily as a point of contrast or in relation to LO research, and is either based on my own or on the LO researchers' experience and understanding of academic social science. In order to avoid issues of boundary-drawing related to science, I prefer to distinguish LO research from academic research, focusing on the setting of research rather than assumed differences in epistemic practices.

As for politics, I have already in this chapter drawn on shifting meanings of the word as I have engaged with authors using it differently. This has especially been the case in my review of STS perspectives of politics, as, for example, the political mode of existence in Latour's later works differed markedly from the ontological politics of Mol, which in turn differed from politics in the sense of political collective interests of groups of scientists in the Edinburgh School of SSK (Latour, 2013; Mol, 1999b; Shapin, 1975). What I take politics to be as a starting point of this thesis is practices and settings related to explicit decisions about governance and the state, such as political parties, parliamentary politics, and public debate related to the same; a comparatively narrow understanding of politics, where politics is not ubiquitous but found in specific sites and activities.[23] It is with this understanding of politics that I take LO to be a political organisation, and LO researchers to be political researchers, and thus initially decided on studying them as a case of political epistemology. It is not a useful definition of politics, nor do I intend for it to be, or think that one is needed to study politics. As mentioned earlier, a parallel can be drawn to empirical work in science studies: there is close to an absolute consensus in the field that there is no singular scientific method, that science

[23] This view of politics has informed my review of theories in this chapter, as I have focused on theories which are plausible for understanding politics in this sense, and have not discussed, for example, the concept of ontological politics in-depth, as it has not seemed generative for understanding politics in this sense.

cannot be meaningfully defined, and that the question of where the boundaries of science should be drawn is unanswerable, yet there is little problem in deciding that a specific laboratory is a suitable site for studying science. Similarly, it does not seem necessary to have clear a priori criteria for what politics is in order to study a political setting. Throughout the analysis, however, I draw on other meanings of politics, primarily in political as opposed to epistemic phenomena in LO research when such divisions are made by those I study.

When politics has been approached in science studies, it has often been in the form of democratic politics, generally starting from the assumed tension between the elitism of science and the egalitarianism of democracy (Guston, 1993). Democratic representation also plays an important part in the analysis of this thesis, as LO researchers frequently engage with actors who have a democratic mandate to speak for trade union members. In my analysis, I do not start from any normative theory of what democracy is or should be. Instead, I treat democracy in a similar way as I treat research: as a set of technologies which are drawn on in specific settings to produce legitimate claims of representation. In a seminal text in the actor-network theory tradition, Callon used the concept of translation to capture both scientists representing scallops in research and the elected representatives of members of a fishermen's union, in order to explore the interaction between different kinds of representation using the same analytical language (Callon, 1984). While agreeing that both kinds of representation are similar acts of speaking for others, I draw on Soneryd and Sundqvist (2023) in treating representation through research and democratic representation as distinct, more specifically by treating them as relying on separate sets of technologies for producing legitimate representation.

What I mean by this is that different sets of material and discursive techniques are used recurringly and competently by the actors I study, generally without making them explicit, to produce legitimate claims of representation through research and democracy respectively. Examples of activities and devices employed in research are the use of established or validated research methods, explicit intertextuality, a formal tone of writing, and explication of methods in connection to epistemic claims. Examples in democratic representation, specifically in Swedish civil society organisations such as LO, are a hierarchy of mandates between decision-making bodies, specific phrases uttered at public meetings, the taking of certified meeting minutes, elections and votes. These examples are not of universal criteria based on a singular method of either research or democratic representation, but along with similar techniques, they form legitimacy-producing sets of activities and

actors, that are easily recognisable as such in their setting.[24] I will return to democracy and research as technologies of representation in the final chapter of this thesis, along with a discussion of how they relate to other modes of representation in the kind of political research done at LO.

The term I use for describing experts working in political organisations is political researcher. The term may have the unfortunate effect of implying that there are apolitical researchers out there. To the question of the existence of this kind of researcher, I am here taking a position of agnosticism: what I mean by political in this context is only that the researchers in question are employed by an overtly political organisation, and that their work as researchers is understood as having both political and epistemic aims.

Political research

While the political epistemology I am proposing here may in principle be open to studying very different empirical settings, it lends itself to some specific theoretical and analytical assumptions over others. Studying the epistemic work of overtly political organisations assumes that epistemic work is in fact done in such organisations: that is, that political actors produce epistemic accounts of the world in a meaningful way, as opposed to assuming that the production of knowledge and the doing of politics are mutually exclusive to one another, or that political actors are fully rational in the sense that they already have access to a singular reality independent of the means of knowing it. One consequence of this assumption is that there is no a priori categorical distinction between, for example, research done in a political organisation and research done at a university in terms of whether or not they produce knowledge. There are, however, differences in the practices and constraints of producing epistemic claims in different settings, as well as differences in felicity conditions for epistemic claims (Austin, 1962; Callon, 2007). Such differences should be a topic of study for a political epistemology, rather than a matter of a priori assumption.

From this starting point, one could, as seems to be the case in the literature on policy professionals (Garsten et al., 2015), imagine that there is a sharp

[24] My aim with this simple and somewhat tentative conceptualisation is a deflationary understanding of both of science and democracy. In science studies, science has generally been treated in such a deflationary way, as a loose collection of practices, institutions and infrastructures to be studied empirically, rather than a desirable and universal set of methods for observation and deduction in need of theoretical elaboration. A similarly deflated view of democracy, however, has been rare, and more commonly, rich theories of democracy have been both used and called for (Brown, 2009; Soneryd & Sundqvist, 2023).

distinction between epistemic work done within an organisation to produce internal documents, and epistemic work done to produce documents meant to affect politics through publication. The assumption would then be that the former kind of research would be more like Kuhnian puzzle-solving and the latter kind more like the production of political arguments, or propaganda. While such a distinction is possible, I see little reason to assume that it holds universally, or that research meant to result in published documents are not to be understood as the production of knowledge. Either way, whether or not differences of this kind exist is an empirical question. It should also be noted that epistemic policy actors themselves often see policy researchers they disagree with as engaging in the production of mere political arguments, while seeing their own and similar work as genuine knowledge. Consequently, the designation of certain epistemic claims as mere argument and others as genuine knowledge is better treated as an empirical resource, similar to Thomas Gieryn's boundary work in the demarcation of science from non-science (Gieryn, 1983, 1999), rather than as a distinction to be made by the analyst.

Another central assumption of this study is that no definitive distinction can be made between normative statements and descriptive or positive statements. This assumption is shared with similar research approaches (e.g. Straßheim, 2015), and is based on the position that normative and descriptive beliefs are mutually constitutive of each other in the sense that one category involves the other; normative beliefs assume states of the world, and descriptive claims have normative implications. Consequently, when I throughout the thesis discuss epistemic claims in contrast to other kinds of statements made by LO researchers – most notably normative policy statements, suggesting political action – I do not take these to be devoid of normative content, and I strive to follow the actors' categorisation of statements as either epistemic or political. More generally, this position entails a view of politics as being about what is as well as what ought to be. An extreme version of this view has been suggested by Latour: “We perhaps never differ about opinions, but rather always about things—about what world we inhabit. And very probably, it never happens that adversaries come to agree on opinions: they begin, rather, to inhabit a different world” (Latour, 2004c, p. 455). Whether or not disagreement on opinions, or normative views, should always be understood as being about what world we inhabit, the view of epistemic questions as being fundamental to political disagreement is central to how I situate political researchers in the domain of politics in this thesis.

Finally, in studying political researchers, I find inspiration in early laboratory ethnographies, in which an exploratory and, in a loose sense of the word, inductive approach was taken in empirical studies of the work of

scientists. This attitude is well-illustrated by Latour and Woolgar's ethnographic study of the Salk Laboratory in Laboratory Life (1986), in which the attitude towards the object of study was described as anthropological, by which was meant that they approached natural scientists like the colonial anthropologists of old approached non-Western tribes: by trying to understand the internal workings of their practices and worldviews without accepting their beliefs as true. To Latour and Woolgar, this attitude was, or so I infer, motivated by commonly-held beliefs of the natural sciences as mirrors of an objective reality pre-existing its enactment as scientific facts in the laboratory and in print. My starting point, however, is not that the work of political researchers is generally taken to be objective, but the opposite: that it is often not recognised as knowledge at all, but seen exclusively as political strategy. Consequently, I spend little effort trying to show the constructed nature of their epistemic claims, without for that matter assuming or trying to show that what they produce are necessary truths.

4. Methods and analysis

The idea for this project began with a form of visual metaphor, informed by Isabelle Stenger's cosmopolitics (Stengers, 2005, 2010), originally as filtered through Bruno Latour's actor-network theory presentation of it (Latour, 2004b). As an alternative to standard accounts of political differences, which I had some experience of from my undergraduate studies in political science, I pictured political ideas, or political ideologies, as clusters in a network where the nodes are both beliefs normally categorised as facts, beliefs categorised as values, and various kinds of hybrids between the two. In common understandings of political differences, or so I imagined, we tend to overemphasise certain of these nodes – the ones we think of as values – as what political differences are actually about, ignoring that political discussion and disagreement is as much, or more, about what we take to be empirically true. The difference between a socialist and a liberal, to simplify, is as much in what they think the world currently is as in what they think the world ought to be. Generally, no clear distinction can be made between these two categories of beliefs as values imply facts and facts imply values. Consequently, there is little reason to assume that the factual beliefs are determined by the normative ones any more than the other way around; regardless of how we categorise beliefs, they should be treated similarly as nodes in a weave of beliefs.

I still see no clear objections to this simple political ontology, though one could perhaps disagree with the sweeping judgements of what I perceived as standard accounts of politics. Regardless, it is the idea that I started from. As I thought of the more factual beliefs of politics as having been largely ignored, or treated as being epiphenomenal to values, I wanted to conduct empirical research of a site where work was being done with both factual and normative claims, taking the circulation and production of factual claims as seriously as the normative ones. Inspired by Dorothy Smith's institutional ethnography (Smith, 2005) and by empirical research in science and technology studies, I wanted to go to a central place where things are put together and sent out to other sites. My first ideas for such sites were either a political decision-making body, such as a municipal council or the Swedish *Riksdag*, or an organisation that produces reports for political settings, such as a think tank. I do not

remember the moment in which I decided on the Swedish trade union movement as the empirical case to look into, but I was aware of the historical influence on Swedish post-war politics ascribed to LO economists Gösta Rehn and Rudolf Meidner. With this history in mind, I wanted to learn more about how the unions produced epistemic claims and political proposals today. Directing my attention to their research departments, their research quickly seemed like a generative site for the kind of inquiry I was interested in.

The kind of political-epistemic activities done at LO are not a unique phenomenon in the Swedish context and other sites would have been plausible for this study. Other trade union organisations – including the other two Swedish trade union confederations – employ their own researchers to produce epistemic claims and policy suggestions, as do employer organisations, think tanks and interest organisations. While my choice to study LO was originally motivated primarily by their historical prominence, a few different aspects of the organisation and its research made it an especially useful case for this specific project, at least in retrospect. Compared to the other union confederations and think tanks in Sweden, it is an organisation with a large number of researchers employed and with a long history of conducting research. This makes it a relatively important epistemic site in Swedish politics, and it also means that there are stable patterns of activities and interactions to focus on, compared to the smaller think tanks which are more likely to constantly reinvent their ways of working. While this was by no means necessary for conducting the kind of study this thesis is based on, it facilitated the use of semi-structured interviews and observations conducted over a few years; if this thesis presents a picture of their activities at a given point in time, the organisation is slow-moving enough for the long exposure time of my method to produce more than a blur. Unlike think tanks, an interesting feature of LO and other trade union organisations is that they are membership organisations with a democratic organisational structure in the sense that mandates to govern within the organisation comes from being elected by delegates for union members.[25] An implication of this is that arguments for political positions and for collective interests are made explicitly, making the association between interests and the research done within the organisation visible. Compared to the other union confederations, LO is an overtly partisan organisation with strong connections to Swedish

[25] As LO is a meta-organisation comprising 14 trade unions, the link between those who are elected and the individual humans who are members of trade unions is indirect. Delegates to the Congress, which elects the LO leadership, however, are taken to speak for trade union members.

parliamentary politics through its ties to the Social Democratic Party. This makes it easier to observe how their research ties into party politics, for example through defining parliamentary groups or other party representatives as targets for advocacy. These qualities of the organisation and its research, to be clear, were not immediately obvious to me at the beginning of this project but have emerged as I have got to know the organisation and, to a lesser degree, other similar organisations better; they are not the reason why I first approached LO asking if I could do my study in their research offices, but they may help the reader situate them among organisations doing similar work.

Getting to the field

When I was first planning the empirical research for this project, I envisioned doing something like a laboratory ethnography of LO research, treating their offices as comparable to a scientific laboratory but for political research. In a broad sense, there are similarities. The LO research department, like a laboratory and most other sites of research, is what has been called a centre of calculation: a place where things – statistics, policies, laws, ideas – are gathered from different places and turned into durable claims about specific aspects of reality, that are in turn sent out to multiple other places (Latour, 2007a). The same description could be used for any number of offices. What I brought with me from approaching the LO research departments as a laboratory, which remained throughout this study, was a certain research ethos that characterised early work in science and technology studies: an attempt to approach an empirical site without imagining that you understand what is going on beforehand, and an interest in what the practical activities of research can tell us about how we understand the world.

With this idea in mind, I contacted LO in the early spring of 2019 with the hope of getting ethnographic access to their offices for about a year or a year and a half. After an initial meeting with the research directors, I started doing interviews the same semester, in parallel with negotiating access to the site. After a couple of meetings where I presented my research plan and talked about potential risks and benefits of participation for the organisation, my request to do participant observation at the offices was turned down. In this process, I learned something about the organisation. While the stated reason for not giving me access to their offices at the meeting had to do with the potential disturbance to their work the study would cause, one of the persons at the meeting gave me another explanation as they were walking me out of the

building: the organisation is "under constant siege" by their ideological opponents, and they were concerned about what would come out of this study. I took this to at least potentially mean that they were uncertain about me and my intentions as researcher; possibly with the worry that I was motivated by a political desire to show that the facts they produce are in fact socially constructed. Despite these initial concerns, I was expressly allowed to interview LO researchers during workhours, and all of the researchers I have contacted have been very generous with their time.

As my interviews proceeded, a more limited form of participant observation than the one I had originally imagined became possible. For the first year of research, I paid special attention to one specific research project at LO; a large project done over four years, which was drawing to its close when I started. In connection with the publication of the final report of the project in the fall of 2019, the research department organised a tour of the LO Districts that I was able to join as participant observer. In addition to this fieldwork, I have conducted observations of the webcast 2020 Congress along with several other events that were broadcast in video format on the LO website during the Covid-19 pandemic, and engaged with the various documents produced by LO research in different ways. Apart from these different ways of interacting with LO research, which I will discuss in more detail in the following sections, I have engaged with their work in various minor but recurring ways throughout my research: I have gone to in-person public events at the LO head office, I have sought out what LO researchers write on social media and in newspapers, and I have regularly looked for what they publish on their website and on the LO researchers' shared blog, starting in the fall 2018 and continuing up until the end of the project in the spring of 2024. Such engagement with the field, however, while no doubt informing my research in ways I can hardly account for properly, has been haphazard and unsystematic.

Interviews

One of the most important sources of empirical data for this thesis has been 25 semi-structured interviews with different people related to LO research. These were conducted between the spring of 2019 and the spring of 2024, though only two of the interviews were done after 2022; around that time, I had mostly stopped feeling surprised and confused when doing interviews, and the two I did after that were more narrowly directed at specific topics I wanted to know more about. My strategy in recruiting interview participants has been to centre the interviews on the LO research offices, branching out to others in contact with their research when I encountered interactions they had with others that I

wanted to know more about. Consequently, 19 of the interviews have been with researchers and research directors at the LO head office. Of the others, three of them were with *ombudsmän* – non-researcher employees – at the head office, one a group interview with three regional ombudsmän at one of the LO Districts, one with an academic scholar who had recently written a report for LO, and one with former LO president Karl-Petter Thorwaldsson. As I talked to five of the researchers twice, one of the researchers three times, and one interview was a group interview with three participants, the total number of persons that I have interviewed is 21. The group interview differed from the individual interviews in that it enabled me to see how the persons who participated discussed the topics I asked about between themselves, but I did not treat it as significantly different in what kind of data it produced. 16 of the interviews were done in person at the workplace of the persons I have interviewed, one at a café, and the rest, due to the Covid-19 pandemic, through a videotelephony computer program. All of them were about one hour to ninety minutes in length. All but one of the in-person interviews were recorded for audio, and all the others for audio and video. All interviews were conducted in Swedish.

When deciding who I wanted talk to, I have spent some effort considering the relationship between my own empirical data and the phenomena I am primarily interested in throughout this study, which is the epistemic practices of LO research. As around 20 researchers were employed by LO at any given time during the study, I have interviewed a significant portion of them. In deciding who to interview, I was mindful of who I asked to talk to, striving for a number of interviews with people of different genders, experience, and research roles roughly proportional to the number of researchers of these categories employed. As the LO researchers have been generous in allowing me to interview them – none of the ones I have asked have declined an interview – sampling in this way has been unproblematic.

Partly because of the ethnographic approach that informed this project from its beginning, and partly to regulate my own nervousness, I have tried to think of my meetings as mundane but interesting conversations with knowledgeable people, rather than interviews. A few things, however, set them apart from most conversations I have with friends and colleagues. For each of them I prepared about four to six themes or questions. While I made sure to have time to talk about all of the topics that I wanted to know more about, I did not follow a strict interview guide and jumped back and forth between themes liberally. The prepared topics ranged from fairly specific questions, such as "How do you keep up to date with current research in your field?" to broad themes like "The interests of the LO union members", with a few prepared questions

associated with the theme. For all questions and themes, I asked follow-up questions. When participants talked about things that seemed interesting outside of the prepared themes, which happened regularly, I asked questions about those things, giving equal weight to these ad hoc topics as to the ones I had prepared. The conversations were, in short, semi-structured. I also made more conscious use of what one might call conversation techniques than I do in most conversations I have, though these were techniques I brought with me from my unfinished training as a clinical psychologist as much as from sociological methods classes: I tried to maintain a tolerance for silence, I frequently summarised what I was told in an attempt to make sure that I had not misunderstood and to encourage participants to tell me more, I started with easy-to-answer biographical questions, and I saved the questions I expected to be interpreted as critical – though these were few – to last. Rather unlike the prototypical therapist, however, I was not too hesitant to express my own thoughts and ideas about what they told me and what I thought was analytically interesting during the conversations. I tried to be strategic about such expressions of my own thoughts, and mindful of how what I said might influence what they told me, both during our conversations and later when I analysed them, but I generally found doing this to be very helpful, since most of the people I talked to did not hesitate to correct or contradict me. They are, after all, the experts on what they do. One final thing belying that these were everyday conversations is that I have been absolutely exhausted after each interview.

The majority of the 25 interviews can be divided up into rounds of interviewing, where I asked questions on the same themes over 2–6 different interviews done over a few weeks, though several of them have been one-off interviews. The reason for this has in part been practical; especially for the physical interviews, I wanted to do as many as I could during the same trip to Stockholm, leading to batches of interviews. When I moved to video interviews in 2020, however, I kept the same format. As analysis was done in parallel with data collection, analytical themes that emerged as I worked with the material became interview topics in new rounds of interviews. The interview guides for these different rounds have had varied formats and have been quite messy, and examples of preparatory notes from the interviews can be found in the appendix.

Inspired by Annemarie Mol's praxiographic interviews where she explored everyday practices through interviews as a supplement to observation (Mol, 2002), a large part of the questions I asked were focused on how the work of LO researchers is practically done, and I attempted to get as detailed accounts of their work as possible, for example asking my participants to describe the

process of writing a report from start to finish. During in-person interviews, I sometimes asked participants to talk me through printed reports and asked about the reasoning and efforts behind different parts of the text. Asking such questions had varied results; some kinds of practices were relatively easy to describe, while others tended to lead to more vague responses. While my own interest was primarily in how LO researchers do things rather than in how they understand things, the conversations sometimes drifted towards more reflexive topics, and these, too, have been of relevance in my analysis.

Almost all of the interviews led to the production of two different documents: one with a transcription of what was said during the interview, and one with fieldnotes from the interview. In these fieldnotes, all of which were written immediately after the interview, I briefly summarised the conversation, focusing on mood, my own reactions and other things that I expected to be difficult to infer from the audio recording. I also tried to capture what we talked about before and after I started and stopped recording, the physical surroundings for in-person interviews, and my immediate analysis of the contents of the interview. The exceptions to this were one preliminary interview, which I did not record and thus only has a more detailed fieldnote associated with it, and two of the video interviews done towards the end of the project, for which I did not find taking fieldnotes meaningful as they were recorded for video and mainly ended up confirming what I already thought.

Participant observation

Alongside the interviews, this study draws on a few different kinds of ethnographic observation. The main one was in following LO researchers, leaders and their staff on a tour of the LO Districts in the fall of 2019. This tour was organised in connection to the release of the final report of a large research project comprising 20 different reports, with the purpose of spreading information about the project's findings and initiating discussion on both its epistemic claims and its policy suggestions within LO and the LO unions. Of the ten tour stops planned in the tour, I participated at four, one of which spanned two days, with a total of about 40 hours of fieldwork. The tour was planned for two LO researchers, one of the LO leaders, and one union president, who each held part of a presentation at each tour stop, but the individuals participating varied from stop to stop. Apart from this group, representatives from the district, myself and, at the meeting where the LO president participated, a communications specialist were part of the tour group, forming along with the researchers a kind of entourage around the person from the LO leadership. This meant that for parts of the tour, I became one additional

person taking notes in the background which was, if nothing else, a very comfortable role to play. Becoming part of the same group as the researchers enabled me to easily ask questions and otherwise engage in conversation with them.

Figure 2: A picture of the author wearing a hard hat, as evidence of fieldwork

The public talks were a constant over the different places we went to, with multiple talks at each stop with a similar format but to different audiences; some for a general public, and some for union representatives or Social Democratic politicians. Like the audiences, the venues were varied: conference rooms at hotels, labour movement schools and conference centres, an LO District office, a Workers' Educational Association lecture hall and, for some reason, a conference room at an ice-hockey rink. Apart from the presentations, activities at the tour included workshops led by the researchers, meetings at LO District offices, workplace visits at a construction site and at a hospital, and variously festive – though generally not very – meals with union representatives. At the workplace visits, the person from the leadership would

talk to the people working there, meet local union representatives, and hold short speeches, and we would be shown around the workplace.

During the tour, I took scribbles and notes whenever I was able to, on my computer when I could do so unobtrusively, and otherwise in notebooks, on my phone and, on one poorly-planned occasion, on my arm with a marker. These notes were rewritten into more detailed fieldnotes as soon as I had the chance, which sometimes was in a lull between activities on the tour but more often after I got home. When taking notes, I have tried – in line with the explorative approach of the study – to heed Harold Becker's advice to write down as many details as possible in order to avoid only noticing that which fits standard accounts and my own pre-understanding of the empirical site (Becker, 1998). As my attention during the tour was focused on the researchers and others from the LO head office, the details I wrote down were primarily about what they did. Large parts of the tour can be described as a kind of shadowing (Czarniawska, 2008), as much of it was spent following the tour group from place to place, and the researchers occasionally directed my attention to specific things, often things that they themselves pay attention to.

Apart from the tour, I have made use of another form of observation in this study, though of a rather non-participatory kind. Since the beginning of the research process, I had planned to go to the 2020 LO Congress as a participant observer. While the main role of the researchers in relation to the Congress is usually in the preparatory work leading up to it, they had planned to hold an event of some kind at the Congress, most likely a workshop, presenting the same research project associated with the tour. The details of this event were never fully planned, as the Covid-19 pandemic made public in-person events impossible in 2020. The Congress was held online on the planned dates with a very small number of people present in person and only the most necessary points on the agenda, such as elections of the LO leadership for the coming four years. It was broadcast in video format on the LO website with the hope, optimistic in retrospect, of holding the rest of Congress at full scale the following year. As the pandemic went on, however, the rest of the 2020 Congress, held in the fall of 2021, had a larger but still limited number of participants: it was held in person for the Congress delegates, but other participants were not invited. I was not one of the relatively small number of participants invited to either of these events. I did, however, have access to the broadcast video of both parts of the Congress. I tried to treat these video recordings much as I did the observation of the tour: I watched the Congress events sitting in my home office, taking notes as I watched them. While the mediation through recorded video in principle enabled much more detailed analysis than my in-person participant observation did as I could rewatch it, I

ended up not taking much advantage of this, as the Congress turned out to be of mainly supplementary analytical interest for the study. While the contents of the Congress were meaningful in that I could see how elected representatives and delegates made use of research in speeches and proposals, the parts of the Congress that I had planned to focus on – where researchers communicate their work to delegates – did not take place, which made the Congress less relevant for this study than it would have been otherwise. My video-mediated access to the Congress also severely limited what I could observe, in quite obvious ways: I did not have access to informal conversations with or between delegates, I could only see what the camera operators and editors decided to show, and so on. The video recordings of the Congress from both 2020 and 2021 were in total around 17 hours long. Similarly to the Congress, I observed around four hours of other events broadcast on the LO website during the pandemic: one seminar, and three press conferences related to the publication of LO reports. These too were of supplementary interest to the study, for similar reasons.

All of the structured observations which are part of this study are of events which are more or less unusual in the work of LO researchers, as opposed to observing their day-to-day interactions at the workplace. The in-person observation of the tour offered an occasion to talk to the researchers in new settings, to see how they interacted with audiences and union representatives, and to see how the union presidents and LO leaders made use of research in their presentations. The lack of observations of meetings and less formal interactions at the LO offices, however, has made certain aspects of their work less visible in this study than they would have been otherwise; especially accounts of conflict between researchers over epistemic or other issues are relatively absent from this study. Consequently, I have been somewhat limited in what claims I can make about how disagreement is resolved, if anyone has the final word, and what arguments hold weight in informal internal discussions.

Published documents

A third kind of material used in this study is the various kinds of documents produced by LO researchers and published by LO. These documents include reports, public consultation responses, graphs and short statistical documents. I have been given physical copies of around 20 reports by the participants, but all such documents are also available on the subpage LO Facts of the LO website at the time of writing. My treatment of these documents has not been consistent. For a few of them, I did a very close reading, taking detailed notes

on both linguistic contents and their formatting and graphic design. For all reports published from September 2020 to August 2022 – 33 in total – I created a simple database, detailing report title, publishing date, number of pages, author, author role, sources of empirical data, kinds of other texts referenced, whether they were a source of official LO policy, and whether or not they were published as part of a series. For most reports, and for other kinds of documents, I have simply read them; the number of reports in this category is approximately 60. My main focus in this study has been the practices, understandings and activities of LO researchers, and I have been interested in the documents they produce only to the extent that they shed light on how they do research, rather than treating the report contents as the object of study. While approaching the texts in these three different ways – close reading of a limited number, categorisation of contents of a larger number, and simply reading a larger number still – was the result of indecisiveness rather than of a well-thought-out research strategy, I have found that the different approaches have complemented each other to produce both a good picture of what kinds of documents they produce, and an overview of what they have produced during the years of the study.

Ethical considerations

I have informed every person I have interviewed for this study of what the study is about, that participation is voluntary, that they can withdraw consent to participate at any point, that they can tell me if they want me not to use what they say or have said for quotes or in my analysis, and that while the organisation of LO will be identified, I will take care to deidentify individual participants. An exception to the last point is LO president Karl-Petter Thorwaldsson, who as a highly public figure readily agreed to be named and whom it felt slightly ridiculous to deidentify as the number of LO presidents during the six years of empirical research was two. A few participants have expressed surprise when I have told them their rights as research participants, and have based on these rights expected more sensitive questions than the ones I have actually asked. On two occasions, the person I talked to asked me not to quote them, and both times were on topics of little relevance to the analysis of this study.

In my participant observation, the degree of informed consent has varied between participants. Those I have been directly interested in – the researchers themselves and other representatives of the LO head office – have been well-

informed about my research project and have actively allowed me to participate; almost all of them have also been people that I have interviewed. At closed meetings requiring an invitation to participate, I have presented myself as a researcher briefly stating the purpose of my research and invited participants to ask me about it if they want to know more. At open, public events, researchers and others from the LO head office have known of and consented to my presence, and I have presented myself as a researcher when there have been presentation rounds and when talking to people, but otherwise simply participated as an audience member. I have taken care to avoid recording information that can be traced back to individual audience members or other non-LO participants.

My main ethical concern when writing the thesis has been that I would write something that would cause for me unforeseen repercussions for the individual research participants. In order to further deidentify participants, I have avoided pseudonyms when writing, instead referring to those I write about by their role or job title. This is somewhat unusual in ethnographic writing, and there are a few potential problems associated with it. First, it makes it more difficult for the reader to judge if I rely too much on quotes from one or a few individual interview participants, as most of the people I have interviewed have the role of researcher. I have tried to avoid this by referring to their more specific roles as analyst and economist when possible, but it ultimately requires some degree of trust on the reader's part that I have taken care to include multiple perspectives. Second, and relatedly, it could make it slightly more difficult to judge when something I draw on in my analysis is the perspective of an individual person or of multiple persons, as it risks turning the participants into an anonymous choir of voices. I have strived to manage this by clearly writing out when something is from the perspective of an individual researcher. Third, it may at times make the text less engaging for the reader. The benefit of avoiding pseudonyms in the thesis is that it makes decoding what is said by who significantly more difficult for those familiar with the LO research offices, which is something I wanted both for ethical reasons and to enable my participants to speak more freely during the interviews. For interview participants with unique positions, I have used slightly more inclusive roles, for example using research director to denote the three job titles chief economist, research department manager and research coordinator. The exception, again, is the LO president. When I refer to printed reports in the thesis, I have avoided doing so in such a way that the citation of the report can identify a participant in either interviews or participatory observations; from cited reports, one can tell that the authors are or have been LO researchers, but not that they have participated in this study.

Something I have considered extensively when doing this study is to what degree harm to the organisation of LO can be considered harm to the individual participants. The argument for thinking so is that at least some of the participants would be likely to experience it as such themselves as they generally care deeply about their work. The argument against, which I think is weightier, is that such a definition of harm would make many kinds of valuable sociological research impossible. In my own research, I have nonetheless decided that I do not want to write things that would be received as potentially causing harm to the organisation unless it is necessary for the analysis, as I neither want those who have kindly participated in the study to regret doing so, or future researchers studying LO to be met with undue scepticism. As the LO researchers themselves are better qualified to judge what this would be than I am, I asked three LO researchers to read and comment on a draft of my thesis as it was nearing its final state, and we had a meeting close in time to my final seminar, where an almost-finished draft of the thesis was presented. This meeting served the additional function of member checking, or making sure that I had not misunderstood or inadvertently misrepresented the participants; avoiding misrepresentation has obvious analytical value, but is also an ethical question. My idea was that they could help me understand how the thesis will be received at their own offices and by others closer to their world, but the LO researchers commenting on my draft did not have the right to veto any of my claims. The LO researchers who commented on my draft suggested no major revisions, and the outcome of our meeting was that I changed a few minor details, either because I had misunderstood something or because my phrasing was unclear or unintentionally misleading. Having LO researchers read through my draft was on my own initiative, and has in no way been a prerequisite for or facilitated conducting research at the organisation.

Coding and analysis

In the analysis, I have primarily drawn on my interview material and the in-person participant observation, while the documents and observations of online events have been more supplementary. I have transcribed the recorded interviews and coded the transcriptions multiple times with different strategies, with the help of a computer program for coding qualitative data. The interviews were first coded from the bottom up, in the sense that I started with a fine-grained coding for content, summarising topics in the data, and successively combining similar codes in order to find central themes in the data. Fieldnotes

were coded in a similar way, though on a less granular level. While this exercise turned out to have limited direct value in the actual analysis, it gave me a deep familiarity with the empirical data. Later rounds of coding of interview transcripts were of more direct usefulness. In these rounds, I coded the material top-down, in the sense that I predefined relevant themes and went through the data looking for those. Although I started these top-down rounds of coding with defined codes, I occasionally added new codes, or revised existing ones, when I encountered passages that were relevant to the broader themes I was looking for without fitting neatly into any of the codes I had chosen. Such rounds of top-down coding were done for each of the following three chapters – chapters 5 through 8 – and two additional times for false starts and ideas for analysis that I ended up scrapping.

The reason I used a coding strategy with repeated coding of the data is that the analyses for the different chapters have proceeded somewhat independently from one another. While each chapter is unavoidably informed by the analysis in the other chapters, I have during the process of analysis attempted to make different arguments in the different chapters, as opposed to building up to one overarching argument. This follows from the exploratory approach I have had since the beginnings of the project, though not with necessity; while one could imagine finding one analytical perspective central enough to merit being the focus of the whole thesis, I have not done so, instead trying to capture what I have taken to be the most central themes for understanding what goes on in LO research in my analysis. While the analytical approaches differ, the analyses of the following three chapters all concern the relationship between politics and knowledge in LO research, though drawing on different meanings of both politics and knowledge that have emerged in my analysis.

When writing this thesis, I have translated quotes from interviews, fieldwork and written materials from Swedish into English. All of the interview, observational and published data used in the analysis were originally in Swedish, and consequently all quotes from these sources are translated. In translating the data, my aim has been to keep both the semantic contents and the tone and level of formality of the original Swedish, for example trying to find idiomatic expressions with a similar meaning and connotations when possible. When translating quotes from spoken sources, I have done some light editing by removing repetitions, words out of context and false starts for sentences, taking care not to change the meaning of what was said. In a few quotes, details insignificant to the analysis have been changed to preserve the anonymity of respondents. When translating the names of government agencies, I have generally followed the recommendations in the Swedish

government's *Utrikes namnbok*, though occasionally deviating from it for clarity.

I have tried to balance a position of wilful credulity with one of critical distance when analysing my data. What I mean by credulity here partly follows from the pragmatist position outlined in the chapter previous to this; I have resisted comparing claims made in LO research to normative positions external to the empirical site – methodological standards of my own field of research, for example, or ideals of democratic representation from political theory – instead accepting the participants' perspectives on whether epistemic or representative claims fail or succeed, as valid in their own setting. This is not to say that such comparisons would have led to criticism, but only that I have avoided making them altogether. In a similar vein, I have strived to take what LO researchers say and do seriously in the sense of focusing my analysis on what is empirically observable as opposed to searching for deeper sociological phenomena to explain what I observe. At the same time, some degree of scepticism to the accounts of participants has occasionally been necessary. I have generally assumed that they want to present their organisation in a good light, which is something at least several of them have had significant experience of from interviews with journalists. My own interviews have also varied in how guarded or unguarded I have experienced the person I have talked with to be, but while I do not have full confidence in my own ability to intuit that difference, I have generally experienced my participants as speaking freely. Striking the right balance between these positions of credulity and scepticism has been something that I have spent a significant amount of effort considering when analysing my data, making sure to regularly interrogate how I understand the material.

5. Knowledge and policy in LO reports

LO researchers produce various kinds of documents: public consultation statements addressed to the government, executive summaries for debates and negotiations and responses to motions written to the LO Congress, to name a few. In such documents, LO researchers formulate both epistemic claims, factual statements about a world out there, and policy statements, which are taken to be the collective position of LO as a corporate body. In this chapter, I will discuss one such type of document – the LO report – and its process of production. Reports contain both epistemic claims and policy positions, and I will explore how these two categories of linguistic statements are made, drawing on John Austin's concept of felicity conditions for utterances (1962), and how they relate to one another.

Reports are generally made publicly available on the LO website, under the subpage titled "LO Facts", where they are given the label "Report" and found alongside public consultation statements and free-standing graphs and tables. They are also commonly printed as short books or booklets, spanning from a dozen to over a hundred pages, and given unique ISBN codes. They are uniform in size, shape, and to a large extent in graphic design, and they cover all kinds of topics deemed relevant to the interests of trade union members; economic forecasting, gender inequality on the Swedish labour market, the rise of the Swedish far right, and the effect of inequality in the classroom are a few examples of report topics from recent years. They are written in a formal, academic register, they often cite sources of information and theory, and interspersed throughout their texts are tables, graphs and images that serve as illustration and decoration.[26]

[26] Most reports are made public, and those are the ones I focus on in this chapter, but some follow a different trajectory: they are not meant for public consumption, but are instead intended to be kept internal to the organisation from the beginning of the research leading to their production. These primarily cover topics that are to be used in negotiations with LO's employer counterpart and are considered to be strategically sensitive, and they are generally written at the Collective Agreement Unit at the LO head office. These are not

Figure 3: Two LO reports: *Fackföreningsrörelsen och den fulla sysselsättningen* (Meidner & Rehn, 1952) and *Program för jämlikhet* (Lindgren Åsbrink et al., 2019)

The report does not play the same role in LO research as the published paper does in academia. Overestimating the similarity between these two types of documents was a mistake that I made early in the empirical work leading up to this thesis; starting out, I focused on the writing and publication of LO reports assuming that this was essential to being an LO researcher, and what LO researchers primarily do. This is not the case. Other modes of knowledge production and expertise are equally valued within the organisation, and there are LO researchers who rarely or never publish any reports at all, focusing on other tasks instead. Reports are, however, the main output in the sense that they are the kind of document produced by LO researchers for which the most effort and resources are spent in making them publicly available. They are also the

printed as small booklets, given an ISBN code or adorned with a cover page. This chapter is not about these unpublished documents also called reports, but about the public ones.

kind of output that is the easiest to point to when describing what it is that LO researchers do, or so I find.

I will begin by describing the process for writing a report at LO, starting with how the decision is made that one should be written on a specific topic. This description is written in rather general terms, trying to capture what the process leading up to the writing of an LO report generally looks like, drawing on multiple interviews and on analysis of reports. Following this, I will look at first the production of epistemic claims, and then the formulation of policy statements in reports. By epistemic claims, I mean the knowledge claims resulting from research on the LO researchers' topics of inquiry: descriptive and explanatory statements on the historical, current or future states of Swedish society. Policy statements, on the other hand, are a specific kind of normative statement, generally taken to represent the political opinions of the organisation as a corporate body and emically separated from other kinds of linguistic statements in LO research. For both categories of statements, and after that for the report as a whole, I will discuss how they succeed or fail, drawing on the concept of felicity conditions. Concluding the chapter, I will discuss the relationship between the production of knowledge and the production of policy in LO research.

A decision is made

Before a report is written, someone, or a collective of someones, makes a decision that one should be written. As a starting point in the lifecycle of a report, the decision that one should be written is somewhat arbitrary: all such decisions have a pre-history, and a few LO researchers have told me that they often know or have a feeling for what topics will be written about before the decision is made, based on what topics are being discussed in the LO head office or in the media. It is, however, as good a place as any to start when accounting for the process of producing a report.

The actor making the decision may be one of the LO researchers or a research director, or it may be someone who is not a human individual: an elected body in the organisation, such as the Executive Council, or the LO Congress. In some cases, the decision has been made years, or even decades, before its writing. This is the case for the three recurring reports – *Ekonomiska utsikter*, *Makteliten*, and *Jämställdhetsbarometern* – published annually or biannually, and the oldest of these series of reports, the economic forecasting series *Ekonomiska utsikter*, dates back to 1947, only a few years after LO

employed their first researchers. Sometimes, the decision to write a report is difficult to precisely locate to a point in time or to an actor, as is the case when the Congress decides that a larger theme should be researched – themes like equality or full employment, to take a couple of examples from the last decade – but a researcher decides on the specific report topics under that theme. Regardless, the decision that a report should be written is made.

Once the first necessary step is taken and it is decided that a report should be written, a different question needs to be answered: Who is to write the report? For many reports, the decision on who to write it is made at the same time as the decision that it should be written, and sometimes the person deciding that a report should be written is the one who will write it. An LO analyst, to take one example from my interviews, might be tired of being asked what LO's position on welfare policies is by others in the organisation, and annoyed that people wrongly assume what the organisation's policies are, which may lead them to want to write a report gathering policy statements scattered over a handful of different documents in one place. They may then attempt to convince their research director that they should be tasked with writing such a report. Alternatively, an LO economist may, to take a different example from my interviews, find something they are interested in when looking through datasets from government agencies or non-governmental bodies such as the OECD, and decide that it should be the topic of a report. If so, they may, after consulting their colleagues, decide to write a report about it.

If the decision instead comes from a research director or from an elected body, the process of matching a researcher with a project is relatively straightforward. All researchers have portfolios that include their areas of expertise, such as "welfare politics", "structural transformation" or "education and the labour market". If a report is to be written on, for example, future developments of the health insurance system, the three researchers who are specialised in welfare politics would be the ones considered to write the report. As being the author of a report is not tied to any particular prestige or personal gain, unlike the publication of articles in academia, this is a not a decision with particularly high stakes for anyone involved, and conflicts about who writes what are unlikely to arise.

A research director, or the researchers in charge of a research project comprising more than one report, may also decide that the topic of the report lies outside of the expertise of in-house staff. In this case, a person external to the organisation is employed to write it. This person is often, but not always, an academic social scientist whose research interests are sufficiently similar to the topic that the organisation is interested in. The external author may already

be in the personal network of an LO researcher, they may be found through social media or an internet search, or they may simply be an academic researcher that one of the LO researchers are familiar with. They can also be found through online networks that exist for the purpose of getting university students or academics in touch with political organisations, such as the Social Democratic networks *Unga utredare* and *Arbetarrörelsens forskarnätverk*, though, while both of these networks have been mentioned in my interviews in the context of finding external authors, the use of them seems relatively uncommon. Frequently, according to one project leader I talked to, a senior academic asked to write a report declines and passes the question on to a junior academic or PhD student, who – according to the project leader and corroborated by my personal experience – is likely to value their time less and be more easily flattered by such requests. Journalists and consultants may also be hired to write reports. Unlike researchers employed at LO, writers of externally produced reports are not required to share the values of the Swedish labour movement, though as one research director I interviewed put it, "if you don't think that LO is a legitimate organisation, or represents some form of relevant social interest, then you might choose to not cooperate with us". Being or having been active in the Social Democratic party as an academic or journalist may be something that makes you more likely to be asked to write a report, but is not a requirement.

As discussed in chapter 2, there is an important distinction made between LO economists and analysts within LO: the former, relative to the latter, have what is considered to be an autonomous position within LO. While LO economists are part of the organisation, they can collectively or individually act on their own in the role of LO economists without consulting the elected LO leadership. Analysts, on the other hand, cannot do this. This difference has a few important implications for the process of writing a report. One way that LO economists' autonomy plays into the writing of reports is that economists can freely choose their own research topics. If an LO economist finds something to be an interesting social development, they can make the decision themselves to write a report on it. Analysts, on the other hand, generally get assigned the reports they write. This does not mean that they cannot influence what they want to write, but unlike the economists they need to convince their boss – the research director of their department – to task them with writing the report.

Epistemic claims and policy statements

Once a decision has been made that a report should be written and on who the person responsible for writing it should be, the contents of the report need to be produced. For the rest of the chapter, I will divide such contents into two broad categories: epistemic claims and policy statements. Epistemic claims are more descriptive, as opposed to normative, statements on the topic of the report, such as statistical data and their interpretation, expositions of legal frameworks, explanations of causal mechanisms, economic models and predictions and theoretical accounts from academic literature. What I mean by policy statements here are statements which are more normative, which express policy positions or political beliefs, and which are taken to express the will and opinion of LO as a corporate body. These are not the only political suggestions in the writings of LO researchers; LO economists, for example, have a mandate to express political beliefs which are not taken to represent the organisation as a whole. The policy statements of LO differ from these in that the researcher is never the sole author of the statement, as it needs approval by an actor with a democratic mandate to speak for the LO unions to be legitimised.

The distinction between LO policy and other contents of reports is made in the process of producing statements, and is generally made clear in the structure and formatting of the printed reports. Epistemic claims, on the other hand, is an analytic category. What I mean by epistemic claims are claims that are presented as expert knowledge through citations to other published works or by drawing on social science methods of analysis and presentation. The category is composed of descriptions and explanations of the state of reality, primarily aspects of Swedish society, in history, today, or, through modelling and predictions, in possible futures. The category is looser than policy statements, in the sense that there is more ambiguity in what should be included in the category or not as the category is not emically defined. The distinction between epistemic claims on the one hand and policy statements on the other throughout this chapter and the rest of the thesis is, as noted in chapter 3, not a strict distinction between normative and descriptive statements: policy statements have epistemic contents, and epistemic claims have normative contents, either by implication or explicitly. Policy statements, however, are more explicitly normative, and epistemic claims are more explicitly descriptive.

Epistemic claims

The first thing to note about the epistemic claims of LO reports when analysing them is that they vary from report to report, and that reports themselves are diverse in style and genre depending on topic, researcher and purpose. While most reports combine multiple data sources and draw political conclusions based on them, some are policy programmes that state the organisation's political positions on a topic without explicitly referring to any other published texts, and hardly make epistemic claims at all. Some review the academic literature on a specific topic, citing only academic sources, others exclusively report and interpret public statistics, and others still do both of these things.

Following the conventions of academic writing, explicit intertextuality in the form of citations is generally used in LO reports, and looking at the kinds of literature and databases referred to in LO reports paints a map of what LO researchers consider to be credible or reputable sources of information.[27] Reports and statistics from government agencies and from intergovernmental organisations such as the OECD and the IPCC, together with reports from LO and other trade union organisations, are the most frequently cited sources of information; citations to these are found in most LO reports. Academic sources – peer reviewed papers, books written for an academic audience, and working papers and reports of research institutes – are used somewhat less frequently, and primarily in longer and more detailed reports; these most frequently come from the fields of economics, political science and sociology. Other sources, for example news articles, opinion pieces, journalistic books, annual reports of publicly traded companies, and blog posts or websites, are also used but not with the same frequency as governmental and intergovernmental, trade union, or academic sources. Government agencies, intergovernmental organisations and trade unions seem like the most important sources of information, followed by academic research.

In order to produce epistemic claims, individual LO researchers need to keep up with research and social developments in the fields that are part of their portfolios. This includes keeping up to date with academic research and reports published by other organisations, and regularly looking through the most recent statistics from government agencies and international organisations, such as the Employment Agency (*Arbetsförmedlingen*) and OECD, or from

[27] There are also, as always, implicit intertextual connections. Sufficiently well-established facts do not need to be stated, or are built into the tools of knowledge production (Latour & Woolgar, 1986) Explicit citations may paint a picture, but they hardly provide an exhaustive inventory of all things that have found their way into a document.

other sources.[28] Looking through these data can be what motivates a researcher to initiate the writing of a report, in which case the data is already available before the writing of the report; in these cases "it's more the data that leads to the report, than a report that leads to the data," as one LO economist I talked to put it. If not, publicly available data are found, or data are requested from Statistics Sweden (*SCB*) or other government agencies. Researchers know which government agencies produce statistics that are useful for their specific portfolios, and some datasets, perhaps most notably Statistics Sweden's *Arbetskraftsundersökningen*, the Labour Force Survey, are seen as relevant for many of the topics covered by LO reports and used very frequently in LO reports. In some cases, survey data is commissioned from polling organisations specifically for the purpose of writing a report, though this is rare.

The production of the epistemic contents of a report differs depending on if the report is a one-off report or if it is part of a series of reports. Most reports, about two thirds of those published in the last few years, are part of a series. These are either one of the recurring reports published once or twice per year, or they are part of a larger research project that spans a number of reports and generally lead to a policy programme outlining a comprehensive set of political suggestions in a policy area. Reports that are part of a larger research project are often externally authored and are quite diverse in style and topic; the large project *Jämlikhetsutredningen* comprising 20 reports, for example, included literature reviews of the academic research on a topic (Boguslaw, 2018; Brandén, 2018; Molinder, 2018; Szulkin, 2018), summaries of journalistic interviews (Karlsson, 2020), and an essay on inequality and the rise of the far right (Nilsson & Nyström, 2018); most of these were written by people who are not otherwise employed as LO researchers. This diversity is a marked difference from reports which are part of a recurring series – the yearly or biyearly reports on gender equality, the elite, and the state of the economy – which look more like one another within the same series. These usually draw on similar data sources and literatures from report to report within the same

[28] Maintaining expertise in their field is a crucial task for LO researchers. A study of comparable organisations, also drawing on interviews with experts at political organisations, found that knowledge accumulation practices of NGO experts were characterised by being rhizomatic and "messy and opaque" (Unander & Sørensen, 2020, p. 6). In this study, I have not found the knowledge acquisition of LO researchers to be particularly rhizomatic, at least not in comparison to my own somewhat unsystematic (or, I suppose, non-arboreal) ways of coming into contact with new ideas. In order to keep up with what happens in their fields, LO researchers go to conferences, read journals, talk with people in their personal networks, follow the publications of individual researchers or organisations, and encounter things on social media; most of these activities are likely to be familiar to academic researchers as ways of keeping up with the literature.

series, and often have the same author from year to year. As one researcher, describing working on a recurring report, told me: "it's decided that it should be written and more or less how it should be written, and the framework is pretty clear, so it's more about just doing the work. You know approximately what the end product will look like." When a report is part of a series of recurring reports, the question of what epistemic contents it is to be filled with has a partial answer before the report is written: earlier reports in the series serve as a template for what kinds of data is to be used, meaning that the question of what epistemic claims are to go in the report is less open.

To summarise, the main things that are added to reports for making epistemic claims are statistical data and epistemic claims from other trade union reports, from government agencies and international organisations, from academic research, and from journalistic sources and other kinds of documents. This is not to say that the things taken from these sources are simply reported without being reworked or transformed into qualitatively new epistemic claims. Subsets of the statistical samples or populations are created in order to look for differences between groups of perceived interest to LO members, epistemic claims from academic literature or other reports are chosen, brought together and synthesised into new claims, implications and interpretations of facts are stated, and hypothetical scenarios given certain economic or social developments are described. In short, things collected from these various sites are used to produce qualitatively new epistemic claims by LO researchers.

Success and failure of linguistic statements

In the preceding chapters, I discussed what goes into different categories of reports in terms of epistemic contents. Before moving on to the other category of things produced in reports – their policy contents – I will look closer at how epistemic claims of LO reports could potentially fail, and why they rarely do so. My starting point here is language philosopher John Austin's concept of felicity conditions. Austin identified a class of statements he called performatives – utterances which do something in the world, as opposed to factual references to a reality external to language[29] – and found that such

[29] Such factual references were the synthetic statements in logical positivism which, along with analytic statements, were one of two kinds of meaningful language in the tradition. Austin's argument was made in conversation with logical positivism, and its power came from treating language as a tool for communication rather than as a reflection of reality or

utterances had conditions for failure or success; they may or may not actually do what they are expected to do. Certain utterances are very clearly performative in that they do not obviously refer to a factual state beyond the utterance, for example warnings, requests, legal notices, or words uttered as part of rituals. Austin, however, noted that factual claims such as the ones I am concerned with here also have a performative quality, in that they are made in specific settings where they may succeed or fail as factual claims; they may for example fail by being received as non-sequiturs, by being overly or underly specific, or by being seen as implausible or false (Austin, 1962). In LO research, a judgement of such success or failure is made when a decision is made to print, revise or scrap a report draft, and one way for epistemic claims in a draft to be infelicitous is that they are never made public at all if the organisation does not print the report or the version of the report containing them. Failure of epistemic claims can also be evident after the publication of a report, if the report provokes a negative response within the organisation. These are ways the claims can fail more or less within the LO head office, and they both stem from the circumstance that an epistemic claim made by an LO researcher to some extent is a claim made by LO as an organisation, meaning that the organisation as a whole to a large degree shares responsibility for the claim. I will return to the ways in which claims can fail once they have left the research department later in the chapter.

What is notable is that epistemic claims relatively rarely seem to be infelicitous in these ways internal to the head office. In the following section, I will discuss the few occurrences of such failures I have encountered in my research, attempting to find an explanation for their scarcity.

Speaking truth to power

Given that LO researchers are knowledge producers not wholly unlike academic researchers, one might expect conflicts between researchers and elected representatives at LO leading to the failure of epistemic claims to be frequently occurring. This expectation may arise if we believe research to be a fundamentally unpredictable business, where the researcher looks for the unexpected and is guided by the facts regardless of political consequences; in short, if we imagine the relationship of knowledge and politics be one of

expression of logical argument, similarly to the view of language in pragmatism informing this thesis (Austin, 1962).

"speaking truth to power" (cf. Wildavsky, 1987). When it comes to epistemic matters, where the presentation of a factual claim is controversial in the organisation, such conflicts seem rare at LO. When controversy does arise over reports or other texts written by LO researchers, it generally is over statements that are treated as potential LO policy – the policy recommendations at the end of a report, for example – rather than epistemic claims; such statements need approval by an elected body in order to become the position of the organisation as a corporate body. That this can lead to controversy is unsurprising, as the purpose of this approval process is to discuss and potentially dismiss policy statements that are not acceptable to those speaking for LO members. The possibility of disagreement is built into the process of producing them.

Despite looking for controversies between the elected leadership of the organisation and the researchers throughout this study, only two specific conflicts over factual claims have come up in my interviews: the first one concerned an unexpected value for the average age of retirement for blue-collar workers found in an LO report, and the other the consequences of a hypothetical change in the Swedish inflation target. Of these two, only one was infelicitous in the sense that the epistemic claims were removed from the report in question. I will return to both of these examples in more detail in the following pages. While one possibility for this relative absence of controversy over epistemic claims is that it is an artifact of the method of this study – LO employees and representatives may want the researchers to appear less politically governed, or more united in purpose, than they really are – I think three aspects of the research processes at LO go some way towards explaining the relatively harmonious view of what the world is between researchers and the leadership and Executive Council: the relative predictability of their research, the shared sets of political and epistemic commitments and interests between researchers and representatives, and the ability of researchers to mobilise more epistemic resources to strengthen their claims than the political representatives can.

Looking for the expected

Compared with science, or rather with idealised versions of science, LO research has an element of predictability to their research processes. In accounts of scientific research, predictability is a vice, and originality is a virtue. In the sociology of science, a recurring motif is the competition between scientists for priority in the making of new discoveries. Robert Merton famously described priority and the attachment of your name to a finding as the main reward for scientists in their system of norms (Merton, 1974). In

Merton's phrasing, "it is through originality, in greater or smaller increments, that knowledge advances. When the institution of science works efficiently [...] recognition and esteem accrue [...] to those who have made genuinely original contributions to the common stock of knowledge" (Merton, 1957, p. 639). Although the latter part of this quote has been nuanced or challenged by later theorists highlighting the contingent and interpretative aspects of the attribution of discovery (Brannigan, 1981; Woolgar, 1976), the claim that at least perceived originality, and the associated priority of findings, is an important aim in scientific research has not been seriously challenged; it hardly needs stating that the production of what is seen as unexpected findings is a normative good in academic research. In contrast, coming up with something significantly new, and attaching your name to it, is not an important goal for LO researchers; finding unexpected, original and counterintuitive results that challenge earlier held convictions is not a target in their investigations, and they consequently do not look for the unexpected. One research director I interviewed explicitly compared what they do – implied as a kind of applied research – with academic social science:

> In basic research or whatever you'd call it, there's a very large element of risk taking, and you just have to take a chance in order for something to happen. And that's not the case for us. We know approximately where the starting and finish lines are.[30]

The difference to academic research described here is reflected in the respective reward systems of academic and of LO researchers. Compared to academic settings, in all their diversity, there is a marked difference in career incentives that expresses the non-privileging of the unexpected at LO. Unlike academic researchers, analysts and economists at LO do not benefit personally from being known to discover qualitatively new and counterintuitive claims: it does not advance their careers or bring them personal fame, nor is creativity or other ways of expressing the ability to find surprising results perceived as a trait necessary for a good economist or analyst to have. Consequently, their reports contain none of the more or less convoluted justifications for why they constitute a novel and innovative contribution to their fields that is frequently found in academic writing (see chapter 3 of this thesis for an example).

The comparatively low value of producing ground-breaking claims is well-illustrated by the first example of controversy I have encountered, retold to me

[30] What the research director here refers to as basic research is academic research overall, as opposed to the work of their own and similar organisations.

by former LO president Karl-Petter Thorwaldsson in an interview. The conflict concerned a change in the average age of retirement and was presented as a success story for LO research. Two LO reports on retirement conditions had found that the average age of retirement in LO professions had increased with five years over less than a decade. This, according to the LO president, "didn't fit the picture that the unions were talking about on the streets"; early retirement for blue-collar workers had been talked about as a consequence of harsh labour conditions compared to white-collar jobs, which, in the Swedish pensions system, leads to lower pensions. As this sharp increase in retirement age did not fit the narrative of inequality told in the unions' communication, the union presidents on the LO Executive Council were at first unwilling to approve the report for publication. The details of the disagreement were skimmed over in Thorwaldsson's retelling of the meeting, which instead highlighted the eventual decision to publish the report and the political success presented as a consequence of it: a three-part agreement between unions, employers and the state about a new pensions framework. Seen as a controversy over the epistemic claims, the researchers won. It was clear, however, that this victory did not come easy: the unions "didn't recognise the numbers" and consequently "hated that research", in the LO president's words. The issue was not the methodology behind the results, but rather that they did not fit the epistemic claims which were drawn on in the political arguments of the unions.

Looking at the reports in question, however, there is no trace of this controversy, and the new, higher numbers are not in any way presented as novel or unexpected. The average retirement age in the reports, 63.8 years, is not compared to the average believed to have been correct earlier, which was under 60 years; the older number is, in fact, not mentioned at all. The new number is instead compared to the age needed for pensions payments conforming to the norms for a reasonable living used when the current pensions system was designed, which is 67 years. Compared to this number, blue-collar workers still retire too early, leading to low pensions. While the average age of retirement was seen as novel and surprising in the Executive Council discussions, the reports frame it in a way very similar to how the lower numbers it replaced were framed, signalling continuity with earlier arguments instead of a break from the narrative of inequality (Andersson, 2015; Morin & Andersson, 2016). That the results were surprising was not treated as something worth calling attention to. That novel results are not highlighted even when they are found illustrates their low value in LO research, in comparison to in academic research. A low evaluation of novelty in research seems likely to lead to fewer results that the elected representatives of the

organisation find challenging than what would otherwise be the case, as it would be easier to predict the results of research before it begins.

This does not mean that LO researchers write what they are told to write by the political leadership of the organisation, or that they only produce what they think is politically expedient. Take the economic forecasts of the LO economists, for example. One economist I talked to stressed the importance of independence from political decisions in research, in relation to forecasting:

> Economist: When it comes to economic forecasting, this independence is important because, for example, there is a number for how we think wages will change [in the coming year]. We need that to make our calculations. And it's not like we go and ask the leadership of LO for permission for what we can believe there, but we write the number that we think is the most plausible assessment. Not what the best outcome for LO would be or what is the best [to communicate] publicly, but what we actually think. It wouldn't work if we took political considerations into account when we do economic forecasting.
> Staffan: Because it wouldn't be plausible?
> Economist: No, there'd be no point in doing economic forecasting if we did.

When making a prognosis for a national economy, expected wage increases in the coming year is a key figure, which, on the Swedish labour market, is primarily the outcome of collective bargaining agreements. This is a number that could plausibly be the mechanism of Mertonian self-fulfilling prophecy (Merton, 1948), or, in a more contemporary framework, a kind of performativity of economics (Callon, 2007; MacKenzie, 2007). If employers and unions expect wages to increase strongly, for example, they may also expect an associated increase in inflation, which would lead to further increased demands from unions wanting to preserve real wage increases; expectations of what the outcome of collective bargaining will be thus affect the outcome of collective bargaining. This is something that one could imagine would be used, either as an argument in negotiations about reality between economic forecasting organisations in Sweden, or as a way to strategically communicate the unions' intentions before collective bargaining. Yet this is not the case; the LO economists instead make an independent judgement of what the outcome of collective bargaining will be, without consulting those who are involved in the same negotiations. In order to be epistemically useful, political considerations need to be ignored.[31] Consequently, knowing "where the

[31] The forecasts include political conclusions and suggestions, but these are separate from the forecasting parts of the reports. A few of the economists I have talked to have pointed towards another, more latent, function of the reports: they are written collectively by the economists, and they serve as a yearly occasion to find agreement on central political

starting and finish lines are", as a research director put it, does not mean that the facts are determined politically before the inquiry; it simply means that they do not look actively for the unexpected.

If I have here painted a picture of a sharp contrast between innovative academic research looking for surprising, and sometimes uncomfortable, truths on the hand, and the political research of LO researchers looking for the expected on the other, this picture needs some nuance. Ground-breaking findings belong to the spectacular rather than the everyday in scientific research, and the production of largely unsurprising facts of LO can be compared to the Kuhnian puzzle solving often taken to characterise research (Kuhn, 1970; Law, 1994). As the quest for the unexpected may belong more to the ideology than to the actual practices of science (cf. Mulkay, 1976), this difference should not be overstated.

Shared assumptions

The second aspect of the LO research that leads to a relatively harmonious co-existence between the organisation's politically elected and the researchers is that the latter share important sets of ideas about how the world works, both with each other and with the elected representatives. This is, in part, because of the assumption and requirement for employment that LO researchers – like other LO employees – are ideologically aligned with the Swedish labour movement. When it comes to researchers, this does not just entail party sympathies or political opinions, but also certain sets of epistemic commitments. The most visible example of this is found in the LO economists: the recruitment of economists that sympathise with the labour movement is largely the same thing as the recruitment of economists that primarily believe in Keynesian models for explaining economic development. Political and epistemic commitments are intertwined in such a way that LO economists as a group have an epistemic preference for looking at aggregate demand as the driving force of economic growth, which is the central assumption of Keynesianism. For LO researchers, this perspective has tended to lead to conclusions favouring state intervention.[32] Consequently, they are also broadly

issues and increase social cohesion in the group; they become, as one economist put it, "a campfire to gather around". Not even economists want to appear sociologically naive, I suppose.

[32] In Keynesian theory, markets' ability to self-regulate is seen as limited – Keynes' theory was developed as a response to the failure of labour markets' self-regulation of wages following the Great Depression – and the GDP in total is posited as equivalent to aggregate demand in the economy. One straightforward way to increase the total societal demand is to

sceptical towards hegemonic neoclassical economic models that highlight the efficiency of unregulated markets.[33]

A point made in the introduction of this thesis is worth repeating here: this is not to say that the causality between political values and epistemic facts go from the former to the latter. If a distinction between facts and values can at all be made, the causality is more complicated than that, with the epistemic claims made in LO research also feeding into the production of policy and political ideas. What these shared epistemic and political commitments mean is rather that LO researchers are likely to come to conclusions that make sense to, and are acceptable to, elected representatives in the organisation.

While the epistemic results of LO research are generally acceptable to the Executive Council and the elected leadership, the other example of epistemic controversy I have encountered – the case of a hypothetical change in inflation target – provides an illustrative counterexample to this. In the case in question, an LO economist I interviewed had to remove a few paragraphs from a report, having to do with the effects of a hypothetical increased inflation target on the Swedish economy; the economist described some possible effects of doubling the inflation target from the two-percent target that the Swedish central bank has maintained since it was made politically independent in the 1990s, to a four-percent target. The idea for this change to a more expansionary monetary policy came from a 2015 public discussion by prominent American economists, and was understood as having effects comparable to the increased

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increase public spending; if the state borrows 5 billion kronor and spends it on infrastructure, the economy grows by that amount, plus by whatever further demand increases are caused second-hand (and third-hand, and fourth-hand etc.) by those the state pays to do the work. This is not to say that Social Democratic politics follow with necessity from Keynesian assumptions. Policies like tax cuts and quantitative easing, neither of which are preferred political actions of the Swedish Social Democrats, similarly stimulate the economy in a Keynesian framework. Keynesianism still broadly connotes Social Democratic politics, however, and the Swedish right-wing parties have in the last few decades generally tended to favour politics that assume markets to be self-regulating, as has the mainstream of economics theory up until fairly recently when Keynesianism – as I understand it – has seemed to have a bit of a comeback in the discipline.

[33] One way to describe this shared outlook, following Dieter Plehwe's study of economics and neoliberalism (2009), would be to see the LO economists as the centre of a Fleckian thought collective, which could presumably be mapped in the present by looking at connections to economists outside of the organisation, and traced backwards in time to the historical roots of the LO economists in Gunnar Myrdal's demand-side oriented Stockholm School of Economics. The kind of network analysis and historical genealogy that this would entail is beyond the scope of this study; for here it suffices to state that all LO economists are Keynesians and all LO economists are sceptical towards neoclassical economics.

public spending of expansionary fiscal policy. The speculated consequences of this possible change were varied, but one potentially harmful effect from the trade unions' perspective was that higher inflation would lead to the risk of decreased real wages. The reason for this is that nominal wage decreases are very rare, but nominal wage increases lower than the rate of inflation – as most Swedish employees experienced in 2023 – are less so.

Despite having other possible effects more beneficial from the LO perspective – primarily the economic growth that expansionary monetary and fiscal politics lead to in Keynesian economic theory – the risk of decreased real wages was unacceptable to some of the representatives in the Executive Council to the degree that even speculatively weighing this risk against the benefits of the suggestion was impossible. "[W]e talked about it, but it was just," the economist said of her meeting with the Executive Council, "it was just like walking straight into a wall, really." The report in question was also mentioned by the LO president Thorwaldsson when I talked to him, who described the process of finding agreement between the unions on the report as one of the greatest challenges he faced during his time as president. The final outcome of this controversy was that the paragraphs speculating on an increased inflation target were removed from the report, as part of the compromise the union representatives in the Executive Council agreed on.

Though this example is of when Keynesian epistemic commitments led to unacceptable propositions from the Executive Council perspective, my argument here is that the shared assumptions of LO researchers tend to generally prevent such disagreement. Unlike in this case, the Keynesian perspective of LO economists more often lends itself to justification of policies that are agreeable to the elected representatives, such as increased unemployment benefits which from a demand-side perspective are taken to have a stabilising effect on the economy, while also strengthening the bargaining position of unions. Had the LO economists as a group been committed to neoclassical economics, or to market-radical Austrian economics, instead of to a Keynesian perspective, they would likely have been at odds with the leadership and Executive Council more often than they are now, and their continued existence within the organisation would presumably be at peril.

Another way in which a shared outlook with a difficultly disentangled combination of political and epistemic positions decreases the risk of epistemic claims being infelicitous is related to the research interests of LO knowledge producers. An analyst I talked to made an interesting comparison with a similar job at another organisation that, unlike LO, is supposedly politically neutral:

> ...in the public debate you can get the impression that you're more controlled when you work at LO since we have clear shared values, we share the labour movement's values. That there's political control here. But as a researcher, my experience is the opposite, since [the former employer] is politically neutral, and then you don't want to go too far in either direction, you want to be balanced. So there, a much larger proportion of my analyses were put in the file drawer, since they weren't politically interesting based on this idea of balance. They were a bit too left-wing since I have a tendency to take an interest in social categorisation, socioeconomic differences and such, simply because of my background and my personal values. Here at LO, significantly fewer of my analyses are put in the file drawer.

This perceived increased freedom to pursue his research interests was understood as following from research interests that tend to have political implications more compatible with LO's values. If we take this analyst's experience to be general, LO employs researchers that can fully engage with whatever research interests they have because they share the values of the labour movement. Another way to phrase this is to say that the elected representatives of the organisation rarely see themselves as needing to control the output of the researchers, since their shared epistemic interests tend to lead them to make inquiries in topic areas where conclusions are generally acceptable to the organisation.

The call for researchers to speak truth to power is based on the assumption that truth and power are discrete entities belonging to separate spheres. If we instead look at what researchers and elected representatives at LO have in common, a different picture emerges. Rather than brave researchers speaking their truth to potentially oppressive political representatives, both groups then appear to belong to something like the same thought collective (Fleck, 2008) or epistemic community (Haas, 1992): they share central beliefs about relevant normative and epistemic beliefs, and they have some shared assumptions about both what kinds of knowledge are valid, and what political practices are effective. LO researchers do, in other words, speak truth to power, but this truth is situationally produced and based on assumptions shared between the finders of truth and the wielders of power. Or, in other words, they are "making sense together" (Hoppe, 1999). This is not to say that these are merely local truths in contrast to some imagined universal and singular scientific truth; if we adopt the perspectives of Fleck or Haas (2008; 1992) all facts are similarly dependent on their context.

To these two explanations of the relative lack of infelicitous epistemic claims – the non-directedness towards the unexpected and the shared sets of epistemic and political commitments – a third plausible reason can be added.

Conflicts concerning policy statements seem to be more frequent than those concerning epistemic claims, as I will show in the next section of this chapter. When it comes to epistemic matters, the researchers are the ones who can mobilise the most resources to legitimate their claims: they are employed primarily for their academic qualifications, and they are able to use the techniques of academic research to back up their claims. While the knowledge of union representatives on matters relevant to union politics should not be underestimated, experts would not need to be hired if there was not at least a perceived asymmetry in either knowledge or the ability to make legitimate knowledge claims between researchers and elected representatives. When it comes to policy matters, on the other hand, elected representatives are – as I will explore further in the chapter following this – taken to be able to speak for members of trade unions and their interests. If researchers are employed as the more legitimate spokespersons for empirical facts, and elected representatives are seen as the more legitimate spokespersons for union members, it makes sense that the latter would contradict the former more when it comes to policy matters than when it comes to epistemic claims.

The making of policy

The other category of contents of LO reports is policy: written statements that are taken to express the will and opinions of the LO as a corporate body. The distinction between text that is taken to be LO policy and other language is emic; policy statements are sanctioned political positions that people within and without the organisation are expected to refer to if they want to know what the organisation thinks. Policy statements are more often written by analysts than by economists, as the latter have an autonomous position which gives them a mandate to express personal political opinions. Policy statements are often easily distinguished from the rest of the text in a report, commonly either through being gathered in a separate section at the end, or, if they are interspersed throughout the text, through typographical separation, for example by being written in a box with a different-colour background from the rest of the text. They typically have a heading like "Our proposals", or, more frequently, "LO believes that", or "LO demands".

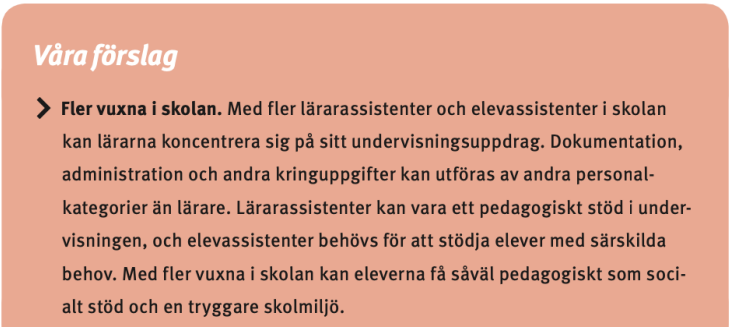

Figure 4: Policy statements in an LO report, under the heading "Our proposals", clearly distinguished from the rest of the report by their light salmon background (from Lindgren Åsbrink, 2018, p. 23)

Reports are not the only sources of policy in the organisation; policy can also be expressed in public consultation responses (*remissyttranden*), in the protocols from meetings of elected bodies, and, to some extent, in the public words of the elected leadership of LO. In a sense, meeting protocols are foundational to the other written sources of policy, in that a report or a public consultation response is only seen as a policy source if it has been approved by an elected body, which meeting protocols serve as evidence for. The principle behind this is one of democratic legitimacy: since the members of the Executive Council and the Congress are seen as legitimately speaking for the members of LO unions by virtue of being elected, their approval of a political opinion is the same thing as that opinion being expressed by the collective voice of LO union members. Not all reports contain policy; in fact, only about one third of reports published in the last few years do. Conversely, some reports have the primary purpose of establishing new policy and contain few epistemic claims. These latter are often, but not always, policy programmes that are produced in larger research projects spanning multiple reports, where the policy programme is presented as being based on the findings of earlier reports in the series.

Producing policy statements

As noted, policy statements are infelicitous within the organisation to a greater extent than epistemic claims, in the sense that they more often have to be rewritten or are scrapped in their entirety when they go up for approval to the

Executive Council. For a researcher writing a report with policy statements, there are two different conditions for the policy they suggest to become legitimised as LO policy. The first one is that the policy statements are highly consistent with already established LO policy as found in other policy sources, to the extent that they are not considered the taking of a new position. Alternatively, if the policy deviates from earlier policy or suggests policy on a new issue, it must be approved by, and thus written in a way that is acceptable to, an elected body; generally, this elected body is the Executive Council, though for important decisions, the Congress may be the body making the decision. Writing policy that is consistent with priorly established policy is relatively unproblematic for LO researchers. It can be done by looking at already published policy sources – reports, consultation responses and meeting protocols – and these documents are since 2021 gathered in an archival database called *Navet* – the Hub – though an experienced researcher is very likely to already be familiar with established LO policy on issues related to their specialisation.

Writing qualitatively new policy statements likely to be approved by an elected body is a bit trickier. Doing so requires the researcher to find out what the different member unions of LO see as their interests in the topic and what they would accept or object to in the Executive Council, as the Council consists of representatives of all LO unions. In order to find this out, researchers generally engage in what they call grounding work, which consists of making sure that at least the largest or most directly affected unions agree with the policy beforehand. Having friends and work acquaintances at the unions is useful for this; the simplest way to find out if your policy suggestions are acceptable is to contact representatives of the unions that might object. LO researchers are likely to have professional ties to the researchers that the larger LO unions employ, not least since many of the LO researchers have worked at one of the unions before coming to LO, so the person they contact is often an economist or analyst; someone with a role equivalent to their own at one of the member unions. This can also involve a degree of negotiation and discussion with the members of the Executive Council or otherwise with the leadership of the affected union, to convince them to accept the policy statement. If an issue is known to be contentious or politically important, representatives of the member unions can be directly involved in the research project, and having such a committee involved in the writing of the report is a way to increase the chances that the Executive Council can agree on its policy conclusions. This is not, however, a guarantee of success, since representatives can turn out to be unreliable; the union representatives may be wrong about what the leaderships of their respective unions think, in which case the results of such negotiations

between union representatives can fail to be approved by the board of directors despite being approved by the union representatives (cf. Callon, 1984).

If reaching agreement between different unions proves difficult, policy statements can be rewritten to more palatable for the Executive Council or the Congress. A strategy that many researchers use in such cases is to increase the level of abstraction or vagueness of the policy suggestion; if "LO is of the opinion that limited companies should be banned from owning publicly funded schools in Sweden" is difficult to accept, "LO is of the opinion that all actors on the Swedish school sector should be non-profit" might be easier. This rests on the assumptions that unions generally agree on the broad outlines of political decisions, and that vague policy statements are less effective and easier to ignore than specific ones and therefore more easily approved of by those who disagree with it.[34] One analyst expressed some ambivalence towards this strategy:

> And then those who have worked here for a long time may say, let's move upwards [in abstraction] and be less concrete. And I'm like, okay, that will get it through the Executive Council, but we'll be less effective as advocates. And that, well, that's the way it is in democratic organisations. If we succeed in getting everyone behind a suggestion, then that carries a lot of weight.

On the one hand, less concrete policy makes LO less effective in their advocacy. On the other, getting policy statements through the process of approval, and getting the unions to speak in one voice, is valuable too, as it puts a lot of weight behind the suggested policy.

Both strategies presented above, grounding work and increasing the level of abstractness of statements, are ways to try to make policy statements felicitous in the sense of getting them printed in reports. Despite these efforts, infelicitous statements are made with some regularity, and are generally perceived as a failure. Conversely, doing grounding work well so that the policy positions written in proposed reports are accepted by the Executive Council is taken to be one of the marks of a good LO analyst. While few of the LO researchers I have talked with have been able (or willing) to point to specific cases where their policy proposals have been rejected, a strong testament to the risk of rejection is that most analysts I have talked to about the production of policy have talked about both grounding work and increasing the level of abstraction

[34] The same strategy can also be used by the elected decision-making bodies. The example with school ownership comes from the 2020 Congress, where the metalworkers' union *IF Metall* opposed the proposal that LO should work towards illegalising limited companies in the school sector, arguing for a less specific ban on profit-driven organisations in schools.

as strategies for getting policy approved. They do not seem to have similar strategies for epistemic claims – though grounding work may be used as part of making sure that their research topics are relevant to the unions – and the existence of such strategies shows that the risk of failure for policy statements is prevalent.

Economists' autonomy

The most important aspect of the relative autonomy of LO economists compared with analysts is in relation to reports and other texts containing political opinions. As mentioned earlier, one part of this autonomy is that economists can make their own decisions on what to write reports about. The aspect of the autonomy that the researchers I have interviewed have stressed the most, however, is in relation to policy statements. In the context of policy, the economists' autonomy means that they can make political suggestions speaking as LO economists rather than as LO as an organisation; when they express a preferred political opinion, this is not treated as being LO policy within the organisation, but instead as the voice of an LO economist. Consequently, reports produced by LO economists do not need the formal approval of an elected body that the reports of analysts do, and they generally do not produce policy statements taken to reflect the will of LO as an organisation. Because of this, LO economists are generally less required to make sure that their policy is acceptable to the unions beforehand, and they are less used to having to compromise by rephrasing or removing statements or increasing the level of abstractness. This autonomy is not, however, unlimited; all economists I have talked to have, perhaps unsurprisingly, expressed that they see their job as producing knowledge that furthers the interests of LO unions' members. What the autonomy means in terms of success or failure of policy statements is that the felicity conditions are less formalised for political opinions expressed by LO economists; there is no formal process for approval that they must go through.

Rarely, however, LO economists are tasked with producing policy for the organisation as a whole. The intensity of emotion with which one LO economist talked about such an exception from their usual autonomy is a good illustration of the importance of their autonomy to LO economists:

> ...it was difficult for me in that report, because it had to pass through the Executive Council. And I wasn't quite there mentally, because I've only ever been an LO economist. So I felt it was really difficult and quite, well, disturbing to have to listen to what they thought about what I'd done and to adjust to that.

> And at one point there was a real conflict, where I had to remove material that I wanted to present. I didn't like that. I almost couldn't accept it, but I had to accept it of course, because, yeah.

The autonomy of LO economists is a double-edged sword: on the one hand, they can, except in rare cases as in the quote above, speak on policy matters relatively freely. On the other hand, when they do speak on policy matters, it is the voice of a small group of union economists speaking rather than the combined voices of 1.4 million LO union members. This is the case for the majority of reports authored by LO economists.

The difference between LO economists' expression of political positions and LO policy statements is important in the organisation. When I shadowed researchers and elected leaders, the LO president made this distinction in a speech he held to a small audience, saying, "Our proposal, or the LO economists' proposal, because we haven't decided this in the Executive Council..." The moment of confusion leading to the need to clarify this for the LO president is indicative of a general difficulty in telling the voice of the LO economists and the voice of LO as corporate body apart. While there is a broad awareness that the LO economists do not speak for the organisation as a whole within the organisation, this is not as clear to those outside of it, and media outlets occasionally – to the chagrin of LO researchers and representatives – conflate the two. To add to the confusion, the elected leadership of LO, most importantly the president, may at times use LO economist policy statements as talking points in interviews or other public appearances, without qualifiers like the one in the quote above. The extent to which leadership persons do this varies depending on the person, as one LO economist told me:

> Our former president was a bit partial to communicating our opinions, I mean the LO economists' opinions, and then as he said them, they became a bit more formal, even if he didn't have a board decision behind him and so on. It becomes a bit more formalised as an LO message. And then there's been other presidents who are more careful about how grounded things are first and so on, so it differs.

Since the LO president is elected to speak for LO union members, them picking up the opinions of economists publicly will to some degree elevate those policy statements to LO policy, though the legitimacy of such statements as policy is weaker than for statements which have been approved by an elected body. This method for LO economists' opinions to become LO policy statements shows that there is some flexibility and ambiguity both in how LO policy is made, and in what is considered policy or not; such opinions may be "a bit more

formalised", but they are not unambiguously LO policy, and if the unions end up disagreeing with them, they may cause problems for the LO president. While there are relatively established ways for policy statements to be produced and legitimised, the system is not rigid.

Felicitous reports

Described at a sufficient level of abstraction, the processes of producing epistemic claims and policy statements are similar. A form of intertextuality is central to both of them; both gather things from other places in order to be felicitous. In the case of epistemic claims, these other places are published reports, databases or other knowledge sources, and in the case of policy statements they are sources of policy that have been priorly established by LO, and the expressed interests of representatives of the member unions. For epistemic claims, such referencing is often overt, marked by citations, while intertextuality for policy statements is implicit in the texts but clearly seen in the work process of writing reports, as researchers refer back to earlier documents when drafting policy. While the two kinds of statements are similar in their production at a high level of abstraction, they thus draw on different kinds of sources without overlap, use different rules for engaging with these sources, and, in addition, face different criteria for their success or failure within the organisation. Consequently, the production of epistemic claims and policy statements seem to be different games following different rules, played in tandem when writing a report containing both.

While the discussion up until this point has treated epistemic claims and policy statements as felicitous or infelicitous separately, this distinction is harder to maintain once the report is put together and published: at that point, it becomes the report as a whole that succeeds or fails rather than individual claims in it. If the success or failure of specific claims is made clear and explicit when the decision to publish a report or not is made and is consequently easy to point to, the success or failure once a report is printed is less so. Printed reports succeed (or fail) in terms of being picked up either by other actors, by others within LO or in future LO research. Such success or failure is less decisive than whether or not the reports, or parts of the reports, are printed.

An expectation one might have of LO reports, or at least one that I had going into this study, was that they would be discursively tested through debate, similarly to how academic papers – at least supposedly, and occasionally in actuality – provoke responses from other academics, through which the relative

strength of arguments is imagined to be made clear.[35] When I first started studying LO research, I imagined that something comparable would take place in the form of reports and counter-reports exchanged between LO and their employer counterpart: the employer organisation Swedish Enterprise and its think tanks. In my imagination, the flaws and weaknesses of arguments made on either side would be pointed out by the other in public debate. References to these organisations in LO reports, however, are very rare; LO and Swedish Enterprise are not generally engaged in debate through the medium of reports, and LO researchers do not feel the need to respond to, or disprove, claims made by employer funded organisations, at least not directly. If an explanation for this lack of debate is needed, one such explanation came up repeatedly in the interviews I did for this study: LO researchers see the knowledge production of employer-funded actors – and especially the employer-funded think tank Timbro – as less legitimate than their own activities. This lack of legitimacy was sometimes justified with the perceived democratic virtue that LO represents a large amount of trade union members, as when a research director expressed annoyance at frequent media comparisons with Timbro:

> I find it a bit painful that the media sometimes seems to see Timbro and us as similar. My experience is that we work on behalf of a large group of people in Sweden, while Timbro, to me, they work on behalf of Sweden's billionaires.

As I will explore in the chapter following this, LO researchers' ability to speak for the Swedish working class is not, or at least not only, an end in itself; it is also central to how they justify their research epistemically through something like a standpoint epistemological perspective. That Timbro speaks for Sweden's billionaires is not just a question of how large the groups of people being represented are, however, but is also tied to perceived biases in their research. Another researcher expressed a negative view of Timbro based on such biases:

[35] The field of science and technology studies has produced several notable instances of such debates, which have served, if nothing else, to clarify the positions held by various authors and groups of authors. One such exchange is the debate on interests published in Social Studies of Science starting in 1981 (Barnes, 1981; Callon & Law, 1982; MacKenzie, 1981; Woolgar, 1981), which I will return to in chapter 6 of this thesis. Other examples include the epistemological-chicken debate in the 1992 edited volume Science as Practice and Culture (Callon & Latour, 1992; Collins & Yearley, 1992a, 1992b; Woolgar, 1992), and the debate on expertise referred to in chapter 3 of this thesis, starting in 2002 (Collins & Evans, 2002; Jasanoff, 2003; Wynne, 2003).

> I think, when I look at that research, parts of the research that Timbro publishes is very distorted by a specifically neoliberal ideology. I think they draw too strong conclusions from statistical correlations and say that, there's a correlation between A and B, and thus there must be some kind of [causality].

Such evaluations of their counterpart's research means that there is little point engaging in debate through research, and the felicity or infelicity of LO reports does not come from this kind of test. This is not to say that the epistemic claims made in reports are not used in debates. Researchers engage in debate through opinions articles in the Swedish dailies and other public settings, and some of them are in frequent debates with political opponents on social media, which one frustrated LO economist described as "a constant sending of facts back and forth, where no one ever wins". The arguments made by the researchers in debates on social media and elsewhere – sometimes with employer representatives – may come from reports even if the publication of reports itself is not in conversation with their counterpart, but regardless, such debates do not seem particularly important to the researchers as a measure of success or failure of reports.

Keeping reports alive

Instead, the success or failure of LO reports has more to do with the extent to which they are picked up and used by those who generally agree with the organisation, and within the labour movement, than with winning or losing debates. An important emic concept in this context is that of the shelf-warmer: a report which is published but never picked up and read, or which is read but never used in other ways within the movement. In my fieldwork, the concern that many reports become shelf-warmers was expressed by all different groups of people within LO I talked to: by the researchers themselves, by union and LO presidents, and by local and regional union representatives. Considerable effort is spent in order to avoid reports becoming shelf-warmers. One example of such efforts is that a public event of some sort is organised in connection with the publication of most reports. These events are generally press conferences or one or several seminars with guests external to the organisation – commonly Social Democratic politicians, representatives of employer organisations, or academic researchers – and are webcast on the LO website. The number of people watching these events live vary, but are normally between a few dozen to a couple of hundred people. The events, and the reports themselves, are publicised in social media posts through the organisations' own accounts and often by the researchers themselves and their colleagues. More

rarely, LO researchers have also organised tours of the LO districts in connection to the release of reports deemed to be especially important, such as the final reports of large research projects.

After I had followed LO researchers on one such tour of Sweden where LO researchers presented the final report of a large project, one of the researchers explained why they did the tour:

> The purpose was to talk about this report, try to keep it alive and get people working with it locally and regionally, because it's, if you write a report, if you want it to lead to some kind of change or something, that doesn't happen by you writing a report. There are so many reports written. It has to be maintained, and worked with.

This reason illustrates two important aspects of the epistemology of LO research. First, it is not enough to produce knowledge, active efforts have to be made in order for the knowledge to actually be used. A comparison can be made here with efforts to advance and highlight the non-academic impact of academic research: the increased focus on the application and effects outside of academia of research in European research policy since the early 2000s. The most notable example of this development is the UK Research Excellence Framework, or the REF, where the tying of impact evaluations to funding have led to active efforts of university departments to discursively position their research as impactful, adopting specific ways of speaking in relation to the REF (Wróblewska, 2021). Systematic reviews of the impact of disciplines or fields drawing on the methodology of the REF have been conducted by major research funders in Norway and Sweden (Research Council of Norway, 2017; Swedish Research Council, 2022), though, unlike the REF, these evaluations are not tied to research funding. In academia, the evaluation of knowledge by its applicability outside of research is a relatively recent phenomenon, at least discussed in terms of impact, competing with other lofty justifications for research, such as the growth of human knowledge.[36] At the LO research departments, however, applicability is the entire point of research. Their epistemic claims are only valuable insofar as they are used by non-researchers. And similarly to how impact discourses in science leads to a new relationship to the published paper, where the paper becomes the starting point of further

[36] Motivating scientific inquiry by its non-scientific usefulness naturally predates the term impact in this context. One of Thomas Gieryn's cases of boundary work, for example, is 19th century physicist John Tyndall justifying science in contrast to religion as leading to technological innovation (Gieryn, 1983).

work with stakeholders and influential actors rather than the end-goal of research (McLellan, 2021), reports require further work once published.

The second thing that the quote indicates about the epistemology of LO research is that their knowledge has a shelf-life, which can be extended through the maintenance work of their researchers. The implication of this is that the epistemic claims they produce have little pretence of being true under the aspect of eternity, as the philosophers say, and that LO researchers are not in the business of adding facts to an imagined repository of shared human knowledge. Their claims are rather understood as knowledge which is spatially and temporally situated. This follows from applicability being a core justification for their research, since application is necessarily situated in time and place, and since the setting of the application constantly changes; in the context of LO research, there is little use in producing an argument against a proposed political reform that has already been passed, or has failed to be passed and been forgotten in the public debate.

The situations in which reports are used varies, and there are consequently different felicity conditions for reports. Reports are frequently used educationally within the labour movement, for example in trade union courses or self-organised study circles facilitated by the organisation ABF (e.g. ABF, 2021; ABF Skåne, 2019). Reports may also spark discussions within the unions or in other trade union organisations, which lead to the writing of motions to the Congresses of LO, the Social Democratic party, or other organisations; in this way the reports indirectly influence the formulation of policy. Reports can also be used outside of their own movement, by being picked up by the media, either through being reported as news, or by being rewritten into opinions pieces by LO representatives (e.g. Micu, 2019; Nandorf, 2018; Ösbrink, 2021); some reports are written with this kind of use in mind, and others are not.

The felicity conditions for reports once they are published thus have little to do with if they stand the test of public debate with political opponents, and more to do with how the reports come to be used by other actors; primarily actors within the labour movement. LO research is valuable to the extent that it is useful in specific contexts, and these contexts tend to be within the movement the researchers are embedded in, rather than in a public agora or marketplace of ideas.

The relationship between knowledge and policy

Throughout this chapter, I have looked at the production and conditions for success of two different kinds of linguistic utterances in LO reports: epistemic claims and policy statements. The distinction between policy statements and other text in LO reports is made emically, in the presentation of policy statements in the printed reports, and in that policy statements need a democratic mandate behind them to be valid, normally through getting approved by an elected body.

One way to characterise the difference between epistemic claims and policy statements, grounded in the work of the researchers, is to say that the production of the two categories of utterances are different games following different rules. The work of producing epistemic claims relies on finding and analysing a large amount of information from diverse sources; frequently but not exclusively public statistics, other trade union reports and academic writing. Claims are made based on these sources and are consequently presented as consistent with them, while synthesising them into qualitatively new claims. They are generally felicitous within the organisation in the sense that they are approved for publication, and their success once they have left the organisation is based on the extent to which they are used as sources of relevant information, primarily within the labour movement.

Policy statements, on the other hand, are produced with reference to only one source of published texts: earlier established LO policy. If policy statements are highly consistent with earlier policy, they do not need approval of an elected body to be felicitous but are considered to be restatements of the organisation's existent positions. For production of policy which is understood as being qualitatively new, other kinds of information goes into the production, namely what is taken to be the shared beliefs of the LO member unions, as their representatives on the Executive Council need to approve the policy before publication. The felicity of policy statements within the organisation consequently depends upon the statements being either sufficiently consistent with earlier policy to not need approval, or being formulated in such a way that they are agreeable to the member unions' representatives. Their success once published can take many forms, for example being picked up as policy by other organisations, or sparking public debate by being published in newspapers.

While LO researchers engage in both of these two games when writing reports, they seem like separate activities, following different rules. For a reader of the printed reports, however, the outcomes of these two games do not seem unrelated. Reports commonly present policy statements as if they follow from the epistemic claims made, for example by presenting policy statements

as the conclusions of the epistemic account, at the end of the report. This implies a linear relationship between knowledge and policy: first we find out the facts, and then we decide what to do based on these facts (cf. Durant, 2016). This does not seem to be what actually goes on when reports are written, as the two games seem to be played somewhat independently. The concerns LO researchers have expressed related to the production of policy is not if it follows from the epistemic accounts, but if it needs and how it will get approval from an elected body.

How, then, should we understand the relationship between epistemic claims and policy, if the two classes of statements are produced independently of each other? First of all, some degree of consistency between epistemic claims and policy statements is needed for the report to make sense as a text. If epistemic claims point in a completely different direction from, or are entirely and obviously irrelevant for, policy statements, the logical coherence of the report would be at risk. While such consistency can presumably be achieved by adjusting both the epistemic claims and the policy statements towards an equilibrium while still following the rules of their respective games, it should be recalled that LO researchers "know approximately where the starting and finish lines are" of the reports beforehand; they are generally not doing highly exploratory studies in uncharted empirical domains, but solving puzzles where they already have an approximate idea of what the solution is, aiming, like many academic social scientists, for deeper understandings of problems they are already familiar with. The topics and questions of the reports are decided with a high degree of familiarity with both the epistemic topics and the policy positions of LO and its unions, meaning that some degree of consistency can be facilitated before the research begins. The episode with an unexpectedly high age of retirement for blue-collar workers – one of the two epistemic controversies between elected representatives and researchers I have encountered – can be understood in light of this: the surprising results were potentially problematic because they risked undermining this consistency.

This is not to say that LO researchers strive to give a partial or biased account of the epistemic topics, at least not necessarily and at least not more than any empirical research is partial. My argument above is not inconsistent with the researchers conducting reasonable research and writing honestly on the topics they think are the most politically relevant or pressing, which, to be abundantly clear, is not the same as being objective. The temptation here is to reach the conclusion that if the policy does not follow from the epistemic claims, then the epistemic claims must follow from the policy, as would be the case if researchers decided which factual claims would be the most politically expedient and then worked their way backwards to find evidence for such

claims. If this is assumed because the production of policy statements is independent from – and thus not a consequence of – the production of epistemic claims, however, one should remember that this independence goes both ways.

A better account of the relationship between epistemic claims and policy statements is that while the production of the categories of specific linguistic statements may be independent from one another, they both start from beliefs about both the state of society and preferable courses of political action for the LO unions which are too deeply entangled for a definitive answer on the relationship of the two to be possible. This account is in line with something like a Mannheimian total ideology (Mannheim, 1960), or Peter Haas' notion of epistemic communities (Haas, 1992): an outlook involving both cognitive and normative assumptions about the world, if a difference can at all be made, which is roughly shared between LO researchers. Starting from a set of beliefs involving what society is, what it should be to further the interests of LO union members, and what political action is necessary to achieve that state, LO researchers play the two largely independent games of producing epistemic claims and policy statements, coordinating the two in such a way that the report as a whole is coherent.

6. Speaking for the workers

One way to describe what a trade union organisation does is to say that they represent the interests of their members. This, one could say with some plausibility, is what their representatives do in collective bargaining, in negotiations over the rights of individual employees, and when they represent the organisation in public debate. It is also what the LO researchers that are in focus in this thesis see themselves as doing; most of their activities can be summed up under the heading of speaking for their members, or, since LO is the national blue-collar union organisation, for the Swedish workers. That they claim to speak for the interests of members is apparent when LO researchers are the authors of statements that are to be understood as political positions taken by the organisation as a corporate body, that is, when they produce the policy statements of the preceding chapter; such policy statements need a democratic mandate behind them as they are taken to express the will of the members. Less obviously, the production of epistemic accounts of Swedish society in the form of factual statements in reports and other texts is also understood by the researchers as a way of speaking for the LO union members, through producing knowledge that is relevant to the interests of this group. Doing so is, as I will show, a central justification for the epistemic value of the work of LO researchers. In other words, LO researchers are expected to be, and expect themselves to be, guided by the collective interests of union members in their research. This association between collective interests and epistemic claims resonates with an analytical move which had a prominent place in the sociology of science of the late 1970s, but has since largely been abandoned in the field.

In this chapter, I will make an argument in three parts. First, I will show how speaking for the union members is used as a justification for the epistemic value of LO research; they justify the value of their epistemic accounts and the meaningfulness of their research with reference to it being knowledge production from the perspective of LO union members. In doing so, the researchers make use of the aforementioned theoretical move, in which collective interests are associated with knowledge production and epistemic claims. This leads to the second part of the chapter: a short review of some key

uses of this theoretical move in the sociology of knowledge, in feminist standpoint theory and in the sociology of scientific knowledge. Crucially, the association between collective interests and knowledge led to controversy in the field of science and technology studies, tied to the question of whether the social group and interests taken to explain a knowledge situation exist independently of the actors, and of whether the analyst has epistemic access to them. In the third part of the chapter, I will examine how these theoretical problems, thorny though they may be in the abstract, are treated as practical problems for the LO researchers; problems that they have established methods for dealing with. The third part of the chapter will end with some reflections on these methods and how they enable LO researchers to speak for Swedish workers. Following this, I will reflect on the use of attending to interests both in the study of political researchers and in the sociology of science broadly.

The epistemic value of LO research

"Politics," one researcher told me, "in order to be successful, needs to be grounded in factual knowledge." A few minutes earlier she had, somewhat despondently, posed a rhetorical question: "One can wonder, well, but what are we specifically adding here? Because the knowledge always exists… Whatever area you're working with, there's always such a huge number of things written." Political positions need to be grounded in factual knowledge, but what the point of LO employing their own researchers to produce such accounts, as opposed to drawing on that huge number of things written elsewhere is not immediately clear. The question of the value of LO producing research has come up frequently in the fieldwork for this project, both, quite naturally, when I have asked about it during interviews, but also in conversations with union representatives outside of the interview setting. Although I have received a few different answers to the question of what the point of their research is, one such answer has been the by far most frequently recurring: LO research is valuable because it brings knowledge from a working-class perspective into the nebulous field of public political discourse.

A way to describe this often used by the LO researchers is that they, to quote one variant of it from the head of a research unit, "highlight and break up the data in ways that no one else does". What is meant by this is that while they largely rely on the same statistical material published by Statistics Sweden and other government agencies as most other organisations that they compare themselves with do, they tend to group respondents based on categories they

see as relevant for their own members, for example by comparing white-collar with blue-collar workers or by looking at workplace sexual harassment based on profession. Through this, LO produces knowledge about Swedish society that other organisations do not, explaining and justifying their employment of researchers.

More than a description of how the accounts of Swedish society LO produces differ from those of other organisations, this justification implies a condemnation of understandings of Swedish society found outside of LO, and LO is taken to be able to produce versions of Swedish society that would otherwise not be produced because of their ability to speak for Swedish workers. One researcher, as we were talking about differences in employment security between blue- and white-collar employees, told me that:

> …we need to become very, very much better at describing what our labour market segments look like, because almost everyone who works with government public inquiries, or otherwise at government offices or in parliament, they're white-collar with university degrees. They have, I think, very little idea of what a working-class job looks like today. And maybe we're not good enough there. But this is the kind of thing that I think has changed noticeably during the time that I've worked for the union, that working-class jobs are treated as an afterthought, no one really cares, and the focus is instead on white-collar jobs and on the university-educated.

The concern for this researcher is that ignorance of what blue-collar workers' jobs are like will have political effects, regardless of what political parties are currently in power. She continued: "The Swedish state is proud of having like 75% employees with a university degree. Of course these perspectives will shape politics then, I think, because you obviously carry with you values and relationships that you're part of." The privileged social position of being white-collar and university-educated is thus taken to bring with it certain values and entanglements that shape or limit the understanding of the world – and consequently affect politics and governance – that LO, by (becoming better at) representing the working class, can provide an alternative to.

This critique is not limited to people who work in government agencies or other political organisations. One LO researcher, reflecting on his social science PhD training, extended it to the mainstream of academic social science research:

> And often in the social sciences, well, the eye doesn't see itself. When you're an academic researcher, I thought about this when I came with my values as a working-class kid to university, I thought, why are only certain kinds of

> knowledge interesting here? And often it was knowledge that focused on class differences and power structures and so on that wasn't as interesting as other knowledge talking about incentives, economic growth and so on… And of course, when you look at recruitment to universities, there's a real imbalance in recruitment, a bias. And this clearly affects discussions within academia. Of course.

A similar criticism of the discipline of economics was raised by an LO economist, discussing how Keynesian perspectives have traditionally been more prevalent at LO than at university economics departments:

> It's better for people with low incomes and for workers if you have more than the mainstream model in your toolbox. The one that I was taught studying economics in the 90s. We learned only that and nothing else. So we talk a lot about economic theory and modelling. We're pissed off with some people who use inaccurate models and get results we don't like.

The economist is here expressing a view on mainstream economics as biased, which is taken to be both worse for low-income earners and workers, but also – and crucially – less accurate than the Keynesian models preferred by LO economists. No separation is made between the political consequences and the epistemic values of mainstream economic theories, and both are instead collapsed into the ambiguous attitudinal remark that the LO economists do not like the results of models drawing on such theories. The working-class perspective of LO is thus associated with epistemic virtues, and taking this perspective in their research is treated as a way to overcome this bias of privilege.

The interests-knowledge association in LO research

The fundamental idea in the quotes above is that certain understandings of Swedish society are obscured from the view of other actors because of their privileged social position; a position that LO as an organisation does not share. The idea is that LO researchers, by aligning their research with the interests of the working-class members of LO unions, are able to produce accounts that are otherwise absent from Swedish public political discourse, and that are taken to be potentially more accurate than those produced by other organisations. This claim to epistemic privilege relies on a theoretical move that has been prominent in the sociology of knowledge since the very beginning of the field, and that has had a central position in feminist standpoint theory and in the influential Edinburgh school of the Strong Programme in the sociology of

scientific knowledge. I will call this theoretical move the interests-knowledge association.

The fundamental idea of interests-knowledge associations is that the collective interests of a social group that a specific actor belongs to will determine, influence or otherwise correlate with how this actor understands the world, and conversely, that certain aspects of the world are unknown – and possibly unknowable – to specific social groups. In addition, some variants of the interests-knowledge association afford an epistemic privilege to some groups; normally, it is dominant groups which are taken to be unable to see certain aspects of reality, and groups that are otherwise less privileged which are given the specifically epistemic privilege of seeing things as they truly are.

In the field of science and technologies, the interests-knowledge association had a central position in the late 1970s, as the Edinburgh school of the Strong Programme picked up Thomas Kuhn's treatment of scientific research as open to social science inquiry. Interest-explanations of knowledge were, however, largely abandoned in the 1980s, following two sets of criticism within the field. In the following pages, I will first review a few important uses of interests-knowledge in the sociologies of knowledge, science and feminist theory. Like any summary, this review is influenced by the purpose of making it: the aim of this review is to show some often-overlooked similarities between different perspectives associating collective interests with knowledge, and that the controversies surrounding interest-explanations in the Edinburgh school consequently can be generalised to other interests-knowledge associations. This discussion will lead to an answer to the question of how the issues at the core of this controversy are handled in LO research.

Interests, standpoint and knowledge

The first introduction of the relationship between collective interests and knowledge to the sociology of knowledge can be found in the very beginnings of the field, through the Marxist theory of György Lukács, expanding on similar ideas in the works of Karl Marx (e.g. Marx, 2014). In Lukács' philosophy, the social knowledge of the bourgeoisie as the dominant social class would necessarily be limited by the ideology required to justify their own dominant position, and they would be forced to either "consciously ignore insights which become increasingly urgent or else they must suppress their own moral instincts in order to be able to support with a good conscience an economic system that serves only their own interests" (Lukács, 1971). The

proletariat, however, had in Lukács' analysis at least the potential to achieve a true understanding of social relations. The reason for this was that their material interests was to ultimately abolish all social classes, with the somewhat dubious implication that the proletariat's interests were not bound to their particular class but instead universally human, which was taken to be the same as objective (Lukács, 1971). The perspective of the proletariat was thus epistemically privileged in that it, unlike the bourgeoisie sciences, was in principle able to accurately and comprehensively see society as it was.

Lukács' association between collective interests and epistemology was picked up and reworked by sociologist of knowledge Karl Mannheim (Mannheim, 1960; Sárközi, 1986). Mannheim generalised Lukács ideas in two crucial ways. First, he took collective interests to be broader than the Marxist economic class interests, and included social groups like generations of age as collectives with a potential association with knowledge. While treating interests as an important resource for understanding the knowledge of actors, he did not think that all epistemic claims could be understood with reference to the interests of the person making them (Mannheim, 1993). Second, he took no group to be epistemically privileged, treating instead all epistemic beliefs as related to the thought style of social groups, correlating with, among other things, their collective interests.[37]

To Lukács, there was a Marxist reality out there which was occluded from view for the bourgeoisie by their interests but visible for the proletariat, while to Mannheim, interests and social position universally affected the knowledge of actors. These two different perspectives on epistemic privilege have reverberated in later uses of the interests-knowledge association drawing on Lukács, Mannheim or both. The epistemic justification of LO research is perhaps best described as agnostic to this distinction; whether or not the working-class perspective of their research is truer or just different, it is seen as lacking from the world of public political debate in Sweden.

Feminist standpoint theories

One of the most influential uses of the interests-knowledge association, drawing on both Lukács and Mannheim, is found in feminist standpoint

[37] Mannheim famously excluded mathematics and science from his interests-knowledge association, as he took knowledge in these fields as too formally structured to be affected by the social position of the individual human subject. This argument was rejected by authors in the sociology of scientific knowledge that later came to adapt his sociology of knowledge to scientific inquiry (Barnes, 2015).

theories. Feminist standpoint theories grew out of and were supported by empirical studies demonstrating masculinist bias of science and technology (e.g. Cockburn, 1983; Martin, 1991), which developed into a general epistemological framework associating gendered collective interests with knowledge (Harding, 1986; Hartsock, 2019). Feminist standpoint theories is a rich and varied field of philosophical and empirical inquiry (see Hekman, 1997 and Sismondo, 2010 for overviews), and theories have differed in whether they, like Lukács, see certain standpoints as being epistemically privileged or if they, like Mannheim, simply see different groups as having access to different knowledges. Two prominent examples of theories that fall on different sides of this issue are the standpoint theories of Sandra Harding and of Patricia Hill Collins, both drawing on the works of Lukács and Mannheim.

Harding argued that the supposed value-neutrality of mainstream science has led to the obscuration of "widely held androcentric, Eurocentric and bourgeois assumptions that have been virtually culture-wide across the culture of science" (Harding, 1995, p. 339), which in turn have led to limitations and distortions in scientific method and theory. By delinking value-neutrality from the concept of objectivity, Harding argued that such interest-determined distorting assumptions can be both made visible and corrected for by taking the perspective of women and other non-dominant groups, leading to a better, and more objective science (Harding, 1995; Harding, 1986).

Hill Collins, on the other hand, focused on the knowledge and epistemology of black women as a social group. Like Harding, she argued that the ways of knowing the world in mainstream academia was shaped by the interests of white men. She contrasted this epistemology with the knowledge world of black women intellectuals, primarily found outside of academia, which differed from the academic mainstream in its answers to questions such as who or what is taken to be a reliable source of knowledge, what the relationship between the knowing subject and known object is, and how knowledge is validated. Unlike in Harding's strengthened objectivity, there was not necessarily a difference in epistemic value between different epistemologies for Hill Collins; instead, she took a meta-epistemological perspective in which there is an inherent value to giving space to the knowledges of different social groups, and in which better knowledge could be reached by acknowledging the social positions from which we speak (Hill Collins, 2014).[38]

[38] Calling the analytical move of feminist standpoint theory an association between interests and knowledge is somewhat inconsistent with the language of most standpoint theories, who generally speak of collective experiences as the foundation of different epistemologies rather than interests. As the epistemological position of white male academics is generally associated with a patriarchal social system where their epistemology affirms and supports

Interest explanations in the Edinburgh school

If feminist standpoint theories have differed in whether they, drawing on Lukács, have assigned an epistemic privilege to less privileged groups or, like Mannheim, have associated interests with knowledge universally, authors in the Edinburgh school of the Strong Programme in the sociology of scientific knowledge were decidedly on the side of Mannheim. As discussed in chapter 3, Edinburgh school authors such as David Bloor and Barry Barnes picked up Thomas Kuhn's (1970) application of social scientific analysis to the contents of science, contrasting themselves with the Mertonian approach in which only the so-called social aspects of scientific research – notably the norms of scientific conduct – could be studied (Merton, 1974). While Kuhn's work on scientific revolutions is frequently cited as an inspiration for the Strong Programme, Mannheim's sociology of knowledge functioned as a large part of the theoretical foundations of the programme (Barnes, 2009, 2015). In the second half of the 1970s, the Edinburgh school played a central role in the sociology of scientific knowledge, which eventually developed into the field of science and technology studies.

For the Edinburgh school, collective interests were broadly used as an explanatory resource when studying controversies between different groups of scientists, and differences in epistemic beliefs were taken to be an effect of the collective social interests of scientists (e.g. Bloor, 1973; MacKenzie, 1978). As for Mannheim and Hill Collins, collective interests were not associated with epistemic privilege, but only taken to be a cause of differences in beliefs. Unlike feminist standpoint theories, the conclusions drawn from associating interests with knowledge were not overtly politically normative, and the goal was rather to open up scientific knowledge to sociological inquiry with the help of Mannheim's sociology of knowledge (Bloor, 1991).

Feminist standpoint theories – of both the Lukácsian and the Mannheimian variants – have some resonance with how LO researchers justify the epistemic worth of their research. Standpoint theories focus on how the knowledge of dominant groups are determined by their collective interests, and argue for the value of less privileged perspectives, either as correction (e.g. Harding, 1995) or as a complement (e.g. Hill Collins, 2014), which is similar to how the value of LO research is argued for. While the Edinburgh school's use of interest explanations of scientific knowledge is less immediately relevant to the

domination, however, interests are implied; the different epistemologies are not mere historical accidents, but benefit certain groups over others. That at least both of the theorists discussed here also draw on Lukács and Mannheim to different degrees also speaks for the implicit role of interests in their analyses.

empirical case of LO research, what I want to point to here is the shared use of the theoretical move of associating collective interests with knowledge, and the shared history in the works of Lukács and (especially) Mannheim. This common theoretical move brings the Edinburgh school into conversation with the justification for LO research in speaking for the workers, as the justification relies on a similar association. More importantly, it opens up for the extension of a controversy in the field of science studies concerning the use of interest explanations to the LO researchers' justification. Interest explanations in the Edinburgh school were the target of two related sets of criticisms in the early 1980s, coinciding with its decline from a central position in the nascent field of science and technology studies to the relatively marginal position it has today.

Two criticisms of interest explanations of knowledge

The first prominent criticism of the use of interest explanations in the sociology of scientific knowledge was made by Steve Woolgar (Woolgar, 1981b). The core of Woolgar's argument, or at least the part of it that is the most central to the argument I am making here, was that the interests used as an explanatory resource in the Edinburgh School were treated as having a stable existence independent of the analysis, unlike the scientific facts and events that they were taken to explain, which were treated as being socially constructed and contingent on these interests. This was argued to be a consequence of seeking a type of natural-science like causal explanation, in which the explanans and explanandum need to be independent of each other and in which the explanans is necessarily unexplicated. As an alternative to causal explanations of this kind, Woolgar suggested ethnomethodological and ethnographic approaches, in which the ways in which scientists work and engage with interests should be treated as an object of study.[39]

[39] The other central feature of Woolgar's argument was that specific interests used as explanatory resources in the Strong Programme were inferred from the scientific facts and practices they were meant to explain, and that a number of rhetorical devices were employed to avoid the accusation of circularity that doing so seems likely to lead to. The analysis of these rhetorical devices are an early example of what later developed into the reflexive approach in science and technology studies, in which the sociology of science itself is analysed alongside the sciences under study (Ashmore, 1989; Woolgar, 1988). Two Strong Programme authors used as examples in Woolgar's argument responded to his paper (Barnes, 1981; MacKenzie, 1981), and Woolgar, in turn, replied to one of these responses (Woolgar, 1981a). While the details of this rather heated disagreement are not particularly relevant for this chapter beyond Woolgar's initial criticism, it led to a further response in which an early and interest-focused version of actor-network theory was

The second criticism was made by Bruno Latour over a series of publications in the 1980s and 1990s (Latour, 1983, 1992, 1993a, among others). In Latour's criticism, the central problem with the Strong Programme was that they accepted a Kantian subject–object division in which phenomena were explained either with reference to a social subject or a natural object. The Edinburgh School, according to Latour, took a radically social position in this division, in which scientific facts were wholly explained with reference to sociological phenomena, predominantly interests. The problem with this, similarly to in Woolgar's criticism, was that the interests used as explanatory resources were given an objective quality that was not extended to the scientific phenomena they were meant to explain. As a consequence, the sociologist of science was treated as having access to true knowledge about objective facts, unlike the scientists, who were seen as being causally influenced by these facts. Latour's suggested alternative was to reject the division between subject and object, and between society and nature, and to attempt to treat both social and natural – or human and non-human – entities as undetermined and as constantly reworking and redefining each other (Latour, 1993b).

Woolgar's and Latour's respective criticisms of the Edinburgh school were successful insofar as the markedly decreased use of interest explanations in science and technology studies throughout the 1980s and onwards can be attributed to them. The crux of both criticisms was that the Strong Programme treated their explanatory resources as existing independently of the knowledge situations that were being explained, and as something that the analyst has epistemic access to; that is, as Durkheimian social facts (Durkheim, 2013). In the case of interests-knowledge associations, such as the interest explanations of the Edinburgh school, the key social facts concerned what the relevant social group is and how it is defined, and what its interests are. Expressed in this way, the criticism can be extended interests-knowledge associations more generally: Lukács, Mannheim, and feminist standpoint theorists such as Harding and Hill Collins all assumed the independent existence of, and their own epistemic access to, the relevant social groups and their interests, and treated this knowledge as more or less unproblematised explanatory resources, unlike the knowledge that they aimed to explain.[40]

formulated (Callon & Law, 1982). This version of actor-network theory was rather curiously positioned as closer to the Strong Programme than to Woolgar's ethnomethodology, despite the affinities between the latter's criticism of interest explanations and later criticism from actor-network theorists.

[40] For some uses of interests-knowledge associations, there are explicit explanations for what constitutes the relevant social group and what their interests are; for Lukács, for example,

This brings us back to the LO researchers' claim that their research represents the interests of Swedish workers. The criticisms of Woolgar and Latour could be extended to the interests-knowledge association LO researchers use too, if they, similarly to the interests-knowledge associations reviewed above, assumed that they had epistemic access to independently existing social groups and their interests. LO researchers, however, seem to be doing something more similar to the alternative treatments of interests that Woolgar and Latour argue for: they treat interests as emerging in the organisation of LO, rather than as independently existing explanatory resources. For the rest of the chapter, I will explore how LO researchers find answers to the questions of what the relevant social group – the Swedish working class – is, and what this group's interests are, and how they maintain the epistemic privilege of speaking for the workers by aligning their work with these interests.

Knowing the working class and their interests

Unlike the uses of interests-knowledge associations in my review here, epistemic access to the social group and its interests are treated as practical problems for LO researchers, with a specific set of solutions to, rather than something that needs to posited in the abstract for an explanation to work. These solutions are not particularly theoretical in the sense that they do not primarily rely on social scientific or other literature; the answers to the questions of what the relevant social group is and what their interests are rather emerge within their own organisation. The relevant group – the Swedish working class – is quite simply defined as members of the LO trade unions; a definition which in the context of the Swedish labour movement as well as in Swedish political discourse generally is uncontroversial. The question of this group's interests is more of an ongoing question for LO researchers, the answer to which needs to be continuously found. This answer too, however, has answers found within the organisation: the interests of LO union members are expressed by those actors within the organisation who have a mandate to speak

the groups are given in the relationship to the means of production and interests are assumed to be either the preservation or the overthrowing of the current economic system (Lukács, 1971), while for example Harding bases the social groups on shared experiences and leaves the question of their interests more open (Harding, 1995). This does not solve the problem of privileging the social facts that the analyst has access to over the knowledge claims that are to be explained.

for the LO union members. LO researchers do not need, for example, a Marxist theory of class relations explaining what the interests of the Swedish working class are, as long as they have something like a formal decision made by the LO Congress that they take as legitimately expressing the will of LO members to refer to. I will here examine the answers to both of these questions more closely in turn, starting with the definition of the worker as the LO union member.

The LO unions members as the working class

The categories of LO union members and of blue-collar workers are frequently treated as more or less coextensive by LO representatives, as in the quotes in the beginning of this chapter where a working-class perspective is afforded to LO research; the researchers in these quotes shifted between union members and the working class when describing who they represent. Similarly, when LO researchers write reports, they generally define the working class as those "employed within LO unions' field of collective agreement", that is, those who are or by virtue of their line of work could be members of an LO union.[41] This is not an idiosyncrasy of LO, nor is it easily dismissed as an inflation of their own importance as an organisation. The definition of blue-collar workers as based on (actual or potential) LO membership is almost universally used in Swedish political contexts and in the production of public statistics (Statistiska centralbyrån, 2023). While the categorisation has been occasionally challenged in academic contexts, for example on Marxist grounds with reference to class interests that are argued to map poorly onto this categorisation (Ekerwald et al., 2018), it has proven durable in the Swedish political system, and is generally uncontroversial. The groups organised or potentially organised by LO unions quite simply are the blue-collar workers of Sweden, in bills for the Swedish Riksdag (e.g. Bill 2022/23:1507), in news reporting (e.g. Dagens Nyheter, 2023), and in public debate (e.g. Gustavsson, 2022). Given this broadly accepted definition, the question of what the relevant group is and who belongs to it has a clear and simple answer within LO: the groups that LO claims to speak for – their actual and potential members – are by definition the Swedish working class.

Compared with the interests-knowledge associations described above, in which belonging to a social group has a causal effect on the epistemology and

[41] Which groups are organised by which union, in turn, is decided through negotiations between the three main Swedish trade union confederations LO, TCO and SACO, and while smaller adjustments are occasionally made, these boundaries are relatively stable.

epistemic beliefs of an actor, LO researchers are in a curious position: while they draw legitimacy from producing knowledge from the perspective of the working class, they do not themselves belong to this group. They are, by the definition of LO union membership, not working class themselves, but rather as white-collar and as university-educated as the state employees that were seen as carrying their privileged values and relationships with them into the state and political bureaucracy. Their epistemic privilege thus stems not from having a working-class standpoint themselves, but rather from something like having a standpoint-by-proxy; they produce knowledge that no one else does because they speak for the workers, rather than because they are workers.[42] This leads us to the more complicated question of how they know and align their work to the interests of the LO union members.

Democratic representation

When I have asked LO researchers how they know that what they do is in line with the interests of LO union members, the answers have, as earlier mentioned, been quite different from those of Lukács, Mannheim and their later followers. Rather than positing the existence of certain collective interests in the abstract (with or without reference to a broader theory of social class and class interests), LO researchers have talked about their specific and concrete methods for finding out what the interests of members are. The three most frequently used such methods are summarised in one researcher's answer to how they know the members' interests, which started by referring to the grounding work discussed in chapter 5: "[W]ell, the interests are again this grounding work you do when you write reports, that you actually have focus groups from the unions, or meet union representatives in different ways."

These union representatives and (mostly informal) focus groups most frequently consist of researchers at the LO unions when the union in question is large enough to have their own researchers; otherwise, it consists of other union employees. From the perspective of LO researchers, researchers and other employees are taken to be able to speak for the interests of the members

[42] When LO employed their first non-working class professional in the early 20th century – a lawyer, rather than a researcher – this was the object of heated debate at the LO Congress, and opponents to the hiring argued that white-collar professions could never represent the workers (Hellberg, 1997). Today, however, the employment of non-blue-collar experts by the unions – and researchers are now the largest such group at the LO central office – is uncontroversial, though, as mentioned in chapter 2, a clear distinction is made between *ombudsmän* on the one hand, who are employed from the trade union member ranks, and white-collar employees, such as the researchers, on the other.

of that particular union by virtue of working for that union, and the purpose of the grounding work is to make sure that the research produced at LO will not spark controversy at the unions, as discussed in chapter 5. Something to note here is that the employees consulted formally have a similar mandate to speak for the members of their respective unions as the LO researchers themselves have to speak for all LO union members; like the LO researchers, they are employees rather than spokespersons for the members, and they too need to use methods similar to the ones described here to know that they speak for their union members. This is a difference from the second method that the researcher described, where the actors consulted are seen as having a democratic mandate to speak directly for union members. The researcher continued:

> But this, when it comes to knowing that what you do is relevant, then another thing is that […] it's the Congress that has decided that you should focus on these questions during this time period and try to bring them up to the agenda. So, for example, that I work with and write reports about labour market politics right now is something that's actually been decided at the Congress, that this is what [the LO researchers] should do. It's a proposal from the Executive Council, of course, but the decision was made by the Congress. And it's in this way that you also know that what we do is at the request from the start of the LO unions' members.

Here, another method for making sure that what they do is in line with LO union members' interests is introduced: the interests are known through elected bodies of representatives within LO; the Congress and the Executive Council specifically in the quote. These bodies are taken to be able to speak for LO union members by virtue of consisting of people who are elected – by the unions in the case of the representatives at the Congress and the General Council, and by the Congress for the Executive Council – and that the researchers' topics of inquiry are decided by them is taken to mean the researchers being tasked with doing research by LO union members "from the start". The researcher makes note of the fact that even though the motion to the Congress was written by the Executive Council – and, one might add, in all likelihood authored by an LO researcher – the decision was formally made by the Congress. This is a distinction worth making since the Congress is seen as having a stronger democratic mandate than the Executive Council, the latter deriving their democratic mandate from being elected by the former.

Finally, the researcher continued by presenting one more method for knowing the interests of union members:

> And if nothing else, when it comes to the quicker, day-to-day work, topics will usually come from the leadership, and they're elected by the Congress, so in that case it's their job to keep track of what it is that they work for, or that what they ask us to do is for the best of the LO unions. […] The leadership, naturally, are at the top, and they're elected officials, so they are the ones who in the final instance decide what we do. Even if they don't make decisions in the day-to-day work, they set the direction. And we sometimes talk to them directly, but sometimes it's [through] the heads of our departments, who often have meetings with the leadership and with the Executive Council, where representatives from all unions are present.

In this third method, yet another group of people are taken to be a source of knowledge about union members' interests: the LO leadership, who through their status as elected officials are taken to be able to speak for the members. The difference between this and the second method is that the actor seen as representing the members to the researchers is not a formal decision made by a decision-making body, expressed in the minutes of a meeting, but rather a small group of persons working at the LO head office taken to embody the will of the members in the sense that they are able to make decisions on their own that represent the will of the members. That the elected leadership sets the general direction for the topics of inquiry is treated as a guarantee for the relevance of research topics to the interests of LO union members, and the responsibility for this is then also placed with the leadership. Something to note here is that the researchers' managers – the research directors – are in the quote taken to be able to speak for the members in relation to the researchers, despite being white-collar employees, as they have frequent meetings with the elected leadership at the top of the organisation, and with the Executive Council.

Representation through identity and experience

The three methods outlined above – grounding work, referring to formal decisions, and being managed by and in contact with elected individuals – have recurred throughout my fieldwork and interviews with LO researchers as the main ways of knowing that what they do is relevant for the interests of union members. A fourth such method is worth mentioning, despite being less universally used in their research than the others: for specific topics of inquiry, LO researchers collaborate closely with union *ombudsmän* at the LO head office. Ombudsmän, not quite the same as the English ombudsman, are the most common form of employee within Swedish unions; they are in principle (though I am aware of one exception) employed from the LO union member ranks – unlike the researchers who are employed largely for their academic

credentials – with some training for the role organised by the unions. They are by virtue of coming from the members, and by having personal experience of the work that LO union members do, taken to be able to speak for the members. Ombudsmän are employed directly by the Executive Council rather than by the head of a department, and at the unions they perform all tasks that are not taken to require a specific academic competency at all levels of the organisations; they represent the members as individuals or as a collective in labour market conflicts, in negotiations with employers and in public communication.

At the research units, however, their job is not too different from that of the researchers. Their offices are on the same floor as the researchers they work with, and they have frequent meetings with the researchers who work on the same topics. While they rarely write reports, they may also be part of larger research projects and in charge of doing the grounding work discussed above for that project, or they represent LO in meetings with other organisations. Like the researchers, they develop a certain expertise within the questions they work with. As the ombudsmän I have talked with see it, trade union researchers are always at risk of becoming detached from the interests of union members, and close collaboration with the ombudsmän is a way to mitigate that risk as the ombudsmän have experiential knowledge of the union members' working conditions.[43] The researchers I have talked with who work closely with ombudsmän agree that they help in bringing the union members' perspectives into the research process. Working with these non-elected representatives of union members is thus a fourth method of knowing their interests, though only used by some of the researchers at LO.

The ombudsmän are a clear example of when the ability to speak for the worker is legitimated by experiential knowledge as opposed to a democratic mandate, as they are employed rather than elected.[44] The distinction between a democratic mandate and a mandate based on experiential knowledge or identity is not, however, as clear as it may seem in the LO researchers' accounts of how they find out what the interests of union members are, as I discuss them above. Elected representatives, too, use working-class experience, identity and attributes for bolstering claims to speak for the union members. One example

[43] Another similarity with feminist standpoint epistemologies can be noted here, as experience is generally taken to be the mechanism behind group epistemologies in these theories.

[44] One could argue that their employment by the Executive Council, as opposed to by a head of department, gives them a form of indirect democratic mandate relative to the researchers, but what has been highlighted by the researchers when discussing the role of ombudsmän is their experience from working within the groups organised by LO unions rather than such an indirect mandate.

of this can be seen in a brief excerpt from my fieldnotes, from a car ride with two LO researchers, LO president Karl-Petter Thorwaldsson and a communications specialist:

> During the ride from the hospital, the communications specialist and Karl-Petter are planning an interview that [public television broadcaster] SVT wants to do concerning statistics published by SCB today showing that the Sweden Democrats is the largest party among male LO union members. The communications specialist believes that SVT wants to meet at a conference centre in the city centre, but she wants them to do the interview at the construction site we're visiting in the afternoon. She gets on the phone, trying to make them come to us while we are there.

Better for the LO president to be seen wearing a hard hat at a construction site than in a comfortable conference room![45] That he is elected by the LO Congress is not enough; he should ideally also be seen with the people he is representing in their place of work, reminding the television viewers that he belongs to the group he speaks for. Even with a strong mandate through election, identity and experience helps.

Experiential and identity grounds for being able to speak for the members can also be used by the researchers themselves, despite their university degrees and white-collar jobs. More than a few of the researchers I have interviewed at LO have stressed their own working-class background in interviews. One example of this is from the first few minutes of one of my interviews, in the biographical questions I have asked to start the conversation in most interviews with new participants:

> Staffan: Then I thought I'd begin by asking a bit about your background, how you came to LO and how long you've worked here.
> Researcher: Well... My background is that I, I actually started my career as a waitress. And I worked with that, and came into contact with the union then [...], and eventually became an active union member. And then I lost my job in the economic downturn in the 90s, which made me go back to school.

The school in question was one where she could get her high-school diploma, as preparation for university studies. What stood out to me with this particular interaction was that she started her account quite far in time before she actually started working as a union researcher, a few years before she started studying for the university degree that enabled her employment at LO. While her

[45] Cf. fig. 2, p. 76.

experience in a blue-collar job was an unexpected starting point to me in the interview, it was understandable as a way of foregrounding working-class experience as researcher.

Different actors speak for the members

Shared between these different methods for finding out the interests of LO union members is that other individuals or groups of people than the researchers themselves are taken to be able to speak for union members, and through either consultation with or management by these people, the LO researchers can make sure that what they do is in the interests of union members. Three things are worth noting about these processes.

First, the participatory structure of LO and its member unions – their organisational democracy – is used as an important source of legitimacy for designating who can speak for their members. The "little tools of democracy" (Asdal, 2008) employed for achieving this are by no means unique to the LO unions; the organisational structure, the expected conduct during meetings, and the relationships between meeting delegates, employees and elected officials are similar to other organisations in Swedish civil society, and particularly in the Swedish labour movement. While the importance of the participatory structure of the organisation is unsurprising, however, not all actors who are taken to represent the members for LO researchers are elected, and a democratic mandate is not the only source of legitimacy for representing union members. In the case of ombudsmän, their ability to speak for the members is based on experiential knowledge associated with having worked in the labour groups represented. The heads of the research units, on the other hand, are taken to be able to represent the members to the researchers by virtue of having frequent meetings with the elected leadership and the Executive Council that they work for. While the various tools and technologies of democratic practices play an important role in enabling the multitude of LO union members to speak with one voice, not all such representation relies on them.

Second, and perhaps again unsurprisingly, the relationship between those represented – the LO union members – and those who speak in their name in relation to the researchers is never direct. The ability to represent union members instead relies on a chain of representational mandates, where one actor is given to ability to speak for the LO union members by another actor. The persons in the LO leadership, for example, speak for the union members by virtue of being elected by the Congress, and the delegates of the Congress, in turn, are chosen within the individual unions to represent their members' interests at LO. A useful comparison is Latour's concept of the circulating

reference: the repetition of translations of empirical findings that enable scientists to produce scientific statements out of empirical observations, and that Latour takes to explain the gap between language and reality (Latour, 1999b). Similarly to the circulating reference, this chain of representational mandates explains the perceived gap between employees and elected representatives on the one hand and organisation members on the other (Mellquist & Sörbom, 2023), by filling it out with intermediary transformations. Another similarity is that representation always can be further extended; the ability to speak for members is in some contexts extended to the trade union researchers themselves, mostly relying on the same methods for aligning themselves with the interests of members described here. This is the case when researchers and other employees at the individual unions serve the function of speaking for their members in relation to the LO researchers in the grounding work; here union researchers who within their organisations have a similar relationship to their members as the LO researchers come to speak for their respective unions' members when speaking to the LO researchers. That speaking for the members can be extended is crucial for the epistemic justification of LO research as speaking for the workers: while they treat others as being the ones speaking for the members to them, they in turn see themselves as having the ability to speak for the members in their research, which is why it is epistemically valuable.

The chain of representation discussed here, however, should not be expected to follow the logic of scientific reference (Latour, 2003, 2013). I here take representation to enable the expression and production of collective interests, rather than the enabling of comparison and generalisation of particular observations.[46] This leads us to a third point to note about the researchers' methods of aligning their work with the LO unions' members. If a problem for theoretical perspectives associating collective interests with knowledge is that they need an explanation for how the analyst has epistemic access to the interests of the group in question, an assumption then is that these collective interests have the quality of existing independently of the production of knowledge. The methods of aligning their work with the members' interests rather treat interests as constantly emerging in the organisation of LO through other actors speaking for them, and as something that they as researchers

[46] While I follow Latour's suggestion that scientific reference is different from political representation in that they transport different things (Latour, 2013), the political representation I describe here is different from politics as understood by Latour, in that he – in line with the Deweyian approach to politics discussed in chapter 3 – is more concerned with the production and maintenance of publics through political speech and less with how mandates of representation is delegated between actors (Latour, 2003).

engage with as part of their work. When the LO researchers refer to a decision made by the Congress as guiding them in the direction of union members' interests, for example, the interests, in the form of a decision, is taken to be the outcome of the Congress, rather than a passive reflection of members' interests as existing independently of the Congress. Interests are not only reflected, but more or less successfully produced by the individual or collective actors who speak for them.

Interest explanations in LO and beyond

The argument I have tried to make throughout this chapter is made in three parts, as follows:

1. LO researchers rely on the theoretical move I call interests-knowledge associations to justify their research.
2. This theoretical move as used in its most well-known theoretical applications comes with the difficulty of requiring the analyst to privilege certain social facts as objective, stable and independent, while treating the knowledge that is to be explained as socially constructed.
3. LO researchers do not treat these social facts – the identity of the relevant social group and their interests – as objective, stable and independent of the situation where they are used, but rather as emerging in the organisation of LO and known through practical methods, avoiding the theoretical problems associated with the move.

Despite expressing, and broadly agreeing with, Steve Woolgar's (1981b) and Bruno Latour's (1983) respective criticisms of the interest explanations used in the Edinburgh school of SSK, I have throughout this chapter tried to maintain an agnostic position towards interest explanations of knowledge as a theoretical move in general. Let me here go one step further: interest explanations of why actors hold or express certain beliefs have persuasive force and are difficult to ignore. For a project of political epistemology, paying attention to the relationship between interests and epistemic work seems unavoidable, and the sociology of science more broadly could probably benefit from a renewed interest in interests. The argument of this chapter, however, can in its simplest expression be generalised: if interests are to have an effect on a situation, the interests must exist in that situation and there must be causal mechanisms between the interests and what they have an effect on. For LO researchers, union member interests are stabilised through the legitimacy of

their spokespersons on democratic or experiential grounds in such a way that they can be treated as something within the broader situation of analysis, but external to the researchers themselves, and for the researchers, interests are produced with reference to these external interests similarly to how the epistemic claims in any empirical research are produced with reference to external actors in the form of objects of study and their related data. The interests, in other words, are something that researchers have to find out, and they have specific methods for doing so, drawing on modes of speaking for the union members which are relatively stable within LO. While the researchers still need to interpret the interests as they are expressed by union member spokespersons – regardless of whether these spokespersons are documents or persons – there are also, as the discussion on felicity conditions in chapter 5 showed, mechanisms for holding the researchers accountable to such spokespersons for these interpretations; reports often need to be approved before publication, and when they do not, spokespersons may react negatively to them within the organisation. Similar mechanisms are likely to be in place in other organisations where interests are explicitly expected to guide the production of knowledge, and paying attention to the methods employed by researchers for finding out what the interests are and to how the ability to speak for the interests in question are delegated between actors – and ultimately to the researchers themselves – seem like useful ways to find these mechanisms.

One can imagine political researchers setting out to produce knowledge in the interests of the working class or other social groups based on a more theoretical understanding of what these interests are than the LO researchers, drawing on social science or political literature rather than spokesperson internal to the organisation. Such researchers would be more likely to exist in smaller interest groups, think tanks, or other political organisations without large groups of members. The general principle that interests are within situation of knowledge production would hold in that case too, but the interests would then plausibly be produced with reference to literature rather than to spokespersons in the form of persons or documents as in the case of LO. Consequently, the methods for finding out what the interests are, and the principles of extending the ability to speak for them to the researchers would be different, and the empirical and analytical approach would need to be amended accordingly.

Finally, it is worth noting that LO research seems likely to be an unusually accessible case for empirical inquiry of this kind, for a few different reasons. First of all, because of the democratic organisational structure of the organisation interests are expressed in public or semi-public settings such as the LO Congress. This would probably not be the case at, for example, a think

tank. Second, they are unusually willing to talk about how they go about engaging with the interests of their members, as they see this engagement as a source of legitimacy rather than a liability; getting access to how, for example, scientists at a pharmaceutical company engage with the interests of shareholders would probably require a different kind of work. Even Swedish employer organisation researchers, who in some ways are very similar to the trade union researchers of LO, would probably be less willing to talk about how they engage with employer interests, as business interests in politics are broadly seen as less legitimate than employee interests in the Swedish context. While interests, to the extent they affect the production of epistemic claims, necessarily do so through intermediate mechanisms, the empirical access to these mechanisms is likely to differ depending on if the persons producing the claims expect the interests they represent to be treated as legitimate or not. In empirical cases less accessible for empirical study of the relationship between interests and research, however, the mechanisms should still be assumed to exist.

7. Bridging

Political researchers are often portrayed as being in-between different roles or places; in-between politicians, academics, civil servants and activists (Garsten et al., 2015, p. 103), for example, or in-between the fields of academia, media, politics and business (Medvetz, 2012). What the spatial metaphor of being in-between implies is that political researchers through their work connect different sites or social realms. In this chapter, I will explore such connections by looking at what goes into and what is sent out from LO research to their four most relevant such other realms: academia, politics, government agencies, and the rank and file of the union movement.

One specific such spatial metaphor is commonly used for political researchers: that they act as a bridge between academia and politics, or between knowledge and power. This metaphor is especially used for think tanks and their employees, by think tankers themselves and sometimes by social scientists studying them (Rich et al., 2011; Stone, 2007), but can be extended to other political researchers. The idea of the metaphor is that knowledge produced in the world of academia is transported to the world of politics, where it is used to formulate policy; a linear model of expertise is implied (cf. Durant, 2016; Soneryd & Sundqvist, 2023, pp. 44–46), similar to the broadly criticised linear model of technological innovation where scientific facts are turned into technological artefacts (Godin, 2006). Academia and politics are assumed to be otherwise disconnected, and consequently in need of a bridge connecting them in order for such knowledge transfer to be possible.

Something close to the spirit of the metaphor has been used by the LO researchers themselves in my interviews with them. One economist, when I asked what characterises a good LO economist, explained that you need to "know the economics toolbox and be able to understand the debate and research papers and so on. But that you also develop ideas, and are able to attach policy and opinions or even our values to that toolbox." Apart from being a skilled economist, you need to "have a nose for politics to be a good LO economist, and be able to understand the game of politics". Another researcher, an analyst, talking about his unlikeliness to return to academia, told me that:

> In my current job I get to work with the kind of analysis that I find interesting and that you do at universities, plus that I get to be there and see when a big part of this work makes a political difference. [...] Here, I work much closer to that change, social change. And that dynamic, I think it's damned fascinating to get to work both with pretty in-depth analysis and also see the connection to practical politics. That dynamic is very attractive and interesting.

To these researchers, being in-between academia and politics is both what characterises their work, and what makes it attractive. In light of statements like these, the metaphor of the bridge seems apt, and it consequently served as my starting point when looking for connections to other places in LO research. I will refer to it throughout the chapter as the bridge metaphor, and use it as a point of comparison to LO research. In line with the metaphor, I will throughout this chapter discuss the relationships to these different sites and what kinds of traffic cross the bridge of LO research.

There are, on the face of it, a couple of problems with the metaphor. The first one has been widely pointed out by social scientists interested in think tanks: the metaphor has been said to obscure strategic agency on the part of policy researchers, portraying them as apolitical and non-agentic infrastructure (Bhatnagar, 2021; Medvetz, 2012; Stone, 2007). To me, this does not seem like a problem. Infrastructure, while often invisible, can be studied for how it affects the world (Star, 1999), and bridges specifically, or one set of bridges, have been rather famously used as an example of the strategic political consequences of material artefacts (Winner, 1980), albeit potentially fictitious (Woolgar & Cooper, 1999). Criticism of the bridge metaphor on these grounds seems to rely on a misunderstanding on the nature of bridges.

Another problem, more pressing for my analysis, is that the metaphor assumes that academia and politics are separate and disconnected domains; the implication is that academic research is apolitical and political work is unepistemic without the aid of such bridges. Beyond seeming like an implausible overstatement of the importance of political research, the assumption that academia and politics are disconnected and non-overlapping parts of the world has – as discussed in chapter 3 – been repeatedly shown to be false in science studies. In the work of LO research, academia and politics are nonetheless treated as more or less discrete and coherent places existing outside of their own work, and as places that they relate to. The symbolic-interactionist concept of social world may be of help here. Social worlds are generally defined as coherent social domains with specific technologies and shared purposes (Clarke & Leigh Star, 2008; Strauss, 1978; Unruh, 1980), which is more or less what these sites are treated as in LO research.

The coherence of such worlds might fall apart if one were to get close to them; to cross the bridge, as it were, into the domains of academia and politics. While these may appear as coherent social worlds from the outside, they seem likely to fall apart fractally into smaller worlds as you approach them, for example into disciplines and sub-disciplines, into movements and parties, or into parts of the legislative process. For my analysis, however, it is enough to state that they are treated, or enacted, as social worlds in LO research, and the point of understanding them as such is to avoid holding these sites stable with an existence independent of their local enactments, while still acknowledging that they are treated as more or less coherent worlds in LO research. To be clear, some degree of inference is done in my treatment of these domains as social worlds as enacted in LO research; they do not explicitly talk about them using those words, but they do largely treat them as coherent sites with predictable outcomes and practices.

Finally, as should be clear from the preceding chapters, LO research both draws on and sends things out to other such imagined sites beyond academic research and politics: more specifically government agencies, which serve both as a frequent source of data and an avenue for influence and expertise, and the members of the LO unions, represented through different forms of mandates to speak for them. While the bridge metaphor assumes that academia and politics are the two places that are connected in political epistemology, these worlds, too, need to be added to complete the picture of LO research.

With these qualifications in mind, is LO research a bridge between different worlds? For the rest of this chapter, I will discuss how their research connects to these other sites, starting with academia and politics, returning to the bridge metaphor as more connections are added.

Academic social science

In the bridge metaphor, scientific results are assumed to be repackaged by political researchers into a format that is more easily digestible for policy-makers, who can then use these results to inform their decision-making. The bridge, in this understanding, is a one-way road, bridging the perceived gap between scholars and politicians by sending things from the world of research to the one of policy. This view resonates with at least a partial LO researcher understanding of what they do: "there is so much [research] there that isn't being used, because a middleman, like us, or like some think tank, or an interested politician is needed," as one economist told me of economics

specifically, lamenting the lack of participation in public debate by academic economists in Sweden. Part of how LO researchers understand their relationship with academic research is as disseminators of it, especially to the world of politics.

In line with the critique that the bridge metaphor obscures agency, however, the researchers I have interviewed have not been unaware that they are selective in what academic research they draw on. One clear example of this is in how one LO economist described their use of academic research, highlighting the strategic aspects of it: "I think that in practice we contribute to taking results, condensing them and passing them on. And then there's probably an element of cherry picking going on." Later in the interview, he repeated this idea drawing on an example from the work of 6F, a cooperation between construction unions in LO:

> I think that we partly work with strengthening our understandings and theories with the help of academic research. And sometimes I think that we actually help in disseminating it. But then it's often because it's research that we think is beneficial to our interests, that we think supports our arguments. This isn't us, but when it comes to wage formation, of course the 6F construction unions have [well-known economist] Lars Calmfors write a report, and that's because his theories are in line with what they think. Calmfors' argument is that the wage Benchmark disadvantages the domestic sector.[47] So 6F gets help from the renowned Lars Calmfors and have him write a report. That's an extremely obvious example, but that's how it goes. And it's also about who we give a platform to in various settings, who we invite to seminars too.

Compared to other researchers I have talked to, this economist had an unusually cynical view of how they use social science: while he is not the only researcher who has described their use of science as being selective, he is the one who most clearly presents it as being strategic, even characterising it as cherry picking. As these quotes show, however, the use of social science is not unambiguously strategic. On the one hand, research is described as supporting beliefs already held, as in the case of 6F and their use of economist Lars

[47] The Benchmark – *Märket* – is a model for wage negotiations in Swedish collective bargaining, in which wage increases negotiated between unions and employers in the export sector function as a norm for all other collective bargaining, in practice limiting nominal wage increases in domestic sectors – such as the construction sector that 6F represents – at times when they would have been able to demand higher wages. There is broad agreement between Swedish unions and employer organisations on the benefits of this model, which has been in use since the late 1990s. The Benchmark is claimed to increase real wages over time, and – barring the high-inflation negotiations of 2023 – real wages have consistently increased since the Benchmark was first introduced.

Calmfors. On the other hand, the use of research is described as strengthening their understandings and their theories, implying that they use research to form more valid beliefs about the world around them. The effect of disseminating scientific results is treated as something normatively good, indicated by words like "help" and "contribute" when describing LO researchers' role in this. Over a few sentences, the description of the use of academic research shifts from academic research being a way to understand the world and something that researchers help disseminate, to something that can be used strategically to further priorly held convictions. The ambiguity in these different accounts of the relationship to academia is indicative of a larger problem in determining when the use of academic research is distortive and when it is not. Similarly to the conceptual difficulties in drawing the line between research and popularisation, and in deciding when research popularisation is being reductive or misleading (Davies & Felt, 2020; Hilgartner, 1990), it is difficult to decide in practice when research results are being used selectively in a way that distorts the sum of scientific results. In addition, while the use of published research may be strategic, instances of pointing out how strategic it is are too (Hilgartner, 1990). What is notable here, though, is that the strategic use of research is not taken to be mutually exclusive to using research to understand the world.

While the economist quoted above was unusually cynical – or forthright – in his evaluation of their use of academic research, others have, similarly to in these quotes, shifted between describing strategic use and treating academic research as an important tool for understanding the world. One economist, who later in the same interview described their use of the results of others as being selective, talked about following what goes on in academic research as what defines not only the LO researchers but Swedish labour movement trade unions overall: "I mean, the opposite would be that we would be out burning car tyres on the street and shout and get in the way, like some other unions in other countries do". Associations between academic research and their own knowledge production is here taken to be in line with the Swedish labour movement virtues of political pragmatism and a preference for negotiations and consensus-seeking over confrontation.

When descriptions of LO research as selective or strategic have come up in my interviews, they have invariably done so in comparisons between their own research and academic research; the description of their work as selective is in comparison with what LO researchers perceive academic research to be like. The interviews were conducted in Swedish, and such comparisons draw on two different Swedish words for knowledge production, as discussed in chapter 2: on the one hand *utredning*, which denotes knowledge production in the

political and legal domains, including LO research, and on the other hand *forskning*, which is the word generally used more narrowly for academic knowledge production. Most of the times the distinction has come up, LO researchers have told me that what they do is not forskning, and that there is a clear distinction between forskning and the utredning they do. There has, however, also been some ambiguity in how this distinction has been used. A couple of researchers saw few practical differences between the two kinds of research, or perceived the level of expertise of LO researchers as comparable to that of academic experts. Others have gone from describing what they do as different from forskning at one point to then indirectly calling it that, as when one researcher who had earlier affirmed the distinction described one specific report, as opposed to others in the same series, as being motivated by "a kind of communicative reasoning, because it's not, it's not research [forskning]".

The somewhat ambiguous distinction made between their own activities and those of academics is an example of what Thomas Gieryn has called boundary work. Gieryn, starting from philosophy of science's broad failure to clearly define what is and is not science, described how scientists and other actors strategically draw and re-draw boundaries between science and non-science, with large differences in how these boundaries are drawn from case to case, and from situation to situation. To Gieryn, this strategic boundary-drawing was motivated by scientists' ideological interests (Gieryn, 1983, 1999). Unlike the cases Gieryn has studied, however, LO researchers generally draw the boundary in such a way that their own knowledge production is placed outside of science.[48] As discussed in chapter 2, this is probably indicative of the relatively high epistemic status of the word utredning in Swedish; LO researchers do not need to present what they do as forskning in order for their results to carry a certain weight, and LO researchers very rarely publish in academic journals, unlike, for example, think tank researchers who often treat academic publishing as an important source of epistemic legitimacy. It is when this boundary is drawn that their own use of results has been described as selective; they use research results selectively in contrast with their perception of academic research, in which they believe that results going against your own argument are given equal weight to those supporting it.

The epistemic results of academic research are not the only traffic going on the bridge of LO research from the world of academia. Another form of

[48] Strictly speaking, they draw the boundary in such a way that their own knowledge production is placed outside of forskning, which is broader than science, including research in the humanities, but narrower than the English word research, generally excluding non-academic knowledge production such as LO research.

interaction is the hiring of academics to write reports for LO; the traffic, in this case, is in the form of research tasks and money from LO to academic settings, and of report drafts from academic settings to LO. If the comparisons made between academic research and LO research when discussing selectivity were favourable to academic scholarship, comparisons in these interactions have been less so. Academics are – perhaps unsurprisingly – seen as likely to use impenetrable prose and jargon, and as sometimes needing guidance in writing in a way that is accessible to broader audiences. Less expectedly, academics are also seen as being epistemically lacking in a couple of ways relevant to LO knowledge production. One economist, describing what makes a good LO economist, told me that:

> You need to have breadth both in economics and in policy, so that you're not just deep in your own little area, if that's tax policy or whatever, but that you have breadth in and is interested in like economics policy questions broadly, but also generally interested in politics and the labour market and so on. So unlike if you're doing academic research, you need to have that breadth. I think that's important.

While she went on to list two other characteristics of a good LO economist – being able to work in a democratic organisation with elected leaders, and being well-read in the academic literature and able to relate that to the politics of the organisation – the first quality of a good LO economist, to her, was that they are less hyper-specialised as academic researchers; an LO economist needs to be able to write about many different topics. Another LO researcher found a related, but more damning, problem with academic research:

> Getting things together to a kind of story is important, because that's not always the case in academic research. [...] It's often very siloed, you can find one thing like, this method had this effect in this context. But politics is never like that, it's always a system.

A little while later in the interview, she associated the lack of systemic explanations in academic research with a kind of unintentional relativism in social science research:

> If you try to read up on an area that has to do with society, you're very often overwhelmed by the feeling, when you try to see what the research is saying, that you can't say anything about anything. It can be everything and nothing, it's "on the one hand, on the other hand", and there are always contradictory results, and it's always also kind of like, well, everybody is looking at their own detail, and there are maybe twenty studies in this area but they all have their

> own specific perspective, so it's not necessarily that they contradict each other, but still, they do. I mean, it's very, I think you can become very relativist and say like "there is no truth" when you read these things often, and then it's like, stop! Of course there are truths! Even if you can't show them, it's like, we know that- well, take right-wing populism. There's an incredible amount of research. And it points in all directions. And you can get the feeling that you can't say anything about why these political movements are currently growing. But in reality, there are of course causes for why they're growing. Maybe there's rarely only one cause, and it can differ and be complex and all that. But there are obviously causes. But that tends to be lost if you only go to the research and try to remove ideology, then you can't evaluate one thing against the other. But in reality, things happen, there is, after all, causality in reality, even if it's awfully complicated.

Academic social science research is to this researcher not just too complicated or too reliant on jargon to be useful to a non-academic audience, but it is also overly cautious in making coherent causal claims, and fundamentally contradictory as different social scientists make different claims based on different methods and assumptions, meaning that you cannot take it as it is and apply it to policy-making. This multivocality means that political action is underdetermined by the versions of society produced in academic social science; listening to the science – to borrow a phrase from the climate movement – is impossible when the science is cacophonous. A solution to this problem, however, is presented in LO research. She continued:

> So I think that the research isn't enough when it comes to social phenomena. When it has to do with people and communities and so on. So, well, there's something making it so that things don't really come through if you only read the research literally. You need some way to fill it in. And I think you can do that politically, but then you're doing it from a clear position, and someone else could do it from another position and get another story. That's often the case. But then I think that there's a task here, to do that as well as possible, so that people who already have certain political beliefs can use that to make arguments and to understand the world better.

In order to make sense of directionless mess of social science research, the academic literature needs to be complemented with something: with a political position. The way out of the perceived relativism of social science research – and the way to reach the real causal mechanisms of society – is thus through making sense of selected scientific results as a coherent political story. To this LO researcher, the relationship between academic research and political beliefs is neither that the research is simply turned into policy, nor that the research is

interpreted strategically to fit political beliefs, but that the politics is what makes the research able to say anything at all about a complicated reality.

Taken as an epistemological position, there are some arguments that could be made against this perspective. For one thing, the relativism of contradictory political stories does not seem more easily managed than the relativism of contradictory social scientific accounts. For another, this view of social science only works from a position where academic social science is treated as cohesive whole that can be consulted for finding out how society works, that is, if academic social science is treated as one social world which can be consulted as a whole. Taken individually, social scientists are less contradictory than if you try to listen to many different social scientists giving them all equal weight, and individual social scientists are in their work presented with the same problem of a multitude of theories and results to evaluate and integrate into arguments. What is notable here though is that the selectiveness, or cherry picking, of LO research is rehabilitated into something epistemically meaningful: by being selective in what research they rely on in order to tell a politically motivated story, contradiction and multivocality can be exorcised, and real causal explanations can be found. Popularisation of science is, again, an instructive comparison. In the dominant view science popularisation, "scientists develop genuine scientific knowledge; subsequently, popularizers disseminate simplified accounts to the public" (Hilgartner, 1990, p. 519). Any differences between scientific accounts and popularised accounts are interpreted as degradation or distortion (Hilgartner, 1990). This standard view of science communication is close to the bridge metaphor, where scientific knowledge is simply transmitted to the world of politics. As this LO researcher understands their work, however, social science research cannot be transmitted as it is, or even in a simplified form, but must be selected and reworked into a coherent narrative in order to be useful. The implication is that LO use of research transforms social science results rather than passively transporting them, and results are transformed for reasons that are here presented as being more epistemic than political: social science research results are not useful for understanding reality taken as they are. This is quite different from both the standard view of science popularisation and the bridge metaphor, as traffic across the bridge needs to be reworked in order to cross it. The politics of the organisation is thus turned into an epistemic resource; politics do not just form a value-framework to the fact-finding of empirical research, but are central for research to be able to say true things about reality.

One further thing to note in the quote above is that the stories constructed from social science research are meant to both give arguments to people who

already see themselves as being ideologically close to LO, and to help them understand the world better; political and epistemic ends are treated as being the same thing. This reflects the rapid shift in earlier quotes from using research to understand the world to using it strategically, and it resolves the ambiguity of these two different accounts of the use of research: if there is no clear distinction between epistemic ends and political ends, going from one to the other no longer seems like an ambiguous account of their use of research.

To summarise, traffic from academia is most prominently in the form of knowledge produced in academic research drawn upon in LO research, as in the bridge metaphor. As in the critique of bridge metaphor, such knowledge is drawn on strategically. The epistemic products of academic research are not always useful as they are; they need to be selectively reworked into political narratives in order to produce coherent and systemic accounts of social problems, which is necessary if research is to be used in the production of policy recommendations. Relatedly, LO research is seen as being more selective but also epistemically superior to academic social science, at least in its own context, because their own researchers are less specialised or siloed than academic researchers, which enables them to understand their issues systemically.

Politics

Looking at the other side of the bridge metaphor, politics, presented an immediate problem in my analysis. If the world of academia was seemingly collapsed into one coherent social world in LO research, as a totality of research that can be consulted for answers, politics was unmanageably multiple in my empirical data. When LO researchers have talked about politics, the concept has seemed to variously contract and expand, so that it sometimes encompassed all LO activities, sometimes only the activities of specific other organisations, and sometimes other things in between and beyond. Going through my interview transcripts, I tried to entertain myself by counting the various uses of the word politics and its cognates, and found at least eleven more or less distinct and contradictory uses.[49] This confusion is mirrored in the

[49] The uses I counted were: politics as party politics, politics as advocacy in relation to the perceived public opinion, politics as the interests of members, political areas within the body of LO policy, politics as LO policy overall, politics of collective bargaining, fiscal and monetary policy in economics discourses, politics as the public sector employer,

fields of political science and political philosophy, where different definitions of politics abound.[50] Political scientist Adrian Leftwich has tried to bring order to this conceptual mess by making two distinctions between different meanings of politics: first, politics either as an arena or as a process, and second, politics either as a limited phenomenon relating to the state or as an extensive phenomenon covering many or all interactions (Leftwich, 2004). The meaning of politics that seems to be the one LO researchers talk about when they discuss themselves as bridging different domains is a somewhat limited meaning that takes place in specific arenas: the parliamentary proceedings of the Riksdag and in local and regional political bodies; the work of the Swedish cabinet; and public debate related to these sites, for example, in national mass media. This meaning is narrower than conceptualisations of politics which identify any or almost any human interactions as political. It is very frequently used by LO researchers, and it clearly distinguishes what politics is from what LO does as a trade union. This usage of the word politics is perhaps most clearly seen in the well-established Swedish labour movement concept of "union-political cooperation", referring to the organised forms of cooperation between LO as an organisation and the Social Democratic Party, which in my fieldwork has been talked about as something obviously and universally desirable by union representatives. In this phrase, the trade union movement is clearly distinguished from politics; trade unions are not political organisations, but labour organisations that sometimes cooperate with a political organisation.[51]

Affecting politics, in this sense of the word, is an important outcome of LO research, and many of their activities are geared towards that goal. One way of affecting politics is by affecting public discourse, for example by getting the results of a report reported as news in one of Sweden's larger media outlets. Another way is to speak directly to groups or individuals with political power, such as parliamentary politicians, cabinet members or high-ranking political aides. When I asked an LO research director to speculate on how their work would differ if their unit were instead an organisationally independent think tank, one of the differences pointed to was access to such people: "[I]t can be

politics as any and all kinds of governance, organisational politics, and politics as what is politically strategic.

[50] I started listing prominent examples here – Arendt, 1998; Aristotle, 2000; Lukes, 1974, etc. – but it quickly felt a bit ridiculous.

[51] I should point out that while this meaning of politics is reasonable for analysing how LO research connects different worlds for the reasons stated here, it is not the one predominantly used in this thesis; it is, for example, different from the somewhat more loosely defined sense of the word implied when I refer to LO research as political research.

difficult for a left-wing think tank to get an audience with a state secretary[52] in a right-wing government. That's not at all impossible for me, I've had meetings with many. As LO, as an interesting social partner, you're invited." Representing a major actor on the Swedish labour market – a social partner – helps with access to such direct advocacy. The quote also points by negation to another central feature of who the LO has access to: when they present their work to groups of politicians, they are generally, but not always, Social Democrats, as implied by the relationship to the party discussed in chapter 2.

While the research director in the quote above pointed out that they have access to non-Social Democratic politicians and political aides, the politicians they meet are primarily in the Social Democratic Party. When other politicians from other parties are mentioned, it is talked about as exceptions to this rule, as when one researcher talked about invitations to talk to politicians: "And then, of course, there are sometimes special ones, [a researcher] got one the other day from the Centre Party parliamentary group which was here [at the LO office], and [the researcher] was to present our research project." Another researcher, relating how she was surprised that a group of people from the Liberal Party had described LO reports as something to be taken seriously, said: "I would've thought that they, one, didn't read [our reports], two, would think that we're super biased." Contact with other parties is not unwelcome, but also not expected.

While LO presents itself as a Social Democratic organisation, and the party and LO are seen as the two main parts of the Swedish labour movement, the ideological proximity between the two organisations has shifted over time, and they have at times been in conflict over important issues, sometimes in the public eye. One economist told me, after making sure that there would be some years between the interview and the publication of my thesis, of one such conflict between the LO economists, LO and the Social Democrats.

> Economist: This year has been pretty rough for us, not within the group because we've agreed completely, but within LO, since when this January Agreement was made between the Social Democrats, the Liberal Party, the Centre Party and the Green Party, we reacted negatively. Very negatively, immediately.
> Staffan: You as LO, or you as-
> Economist: We as LO economists. Some of us at first, and then I think we eventually all thought more or less the same thing. And that was really difficult since we then had both our own leadership and the LO Executive Council, and

[52] A state secretary is the highest-ranking political aide of the Prime Minister or of another cabinet member. They are formally employed by the Government Offices, but chosen by the political party of the minister.

> then the whole party, the Social Democrats I mean, that we were on a collision course with. And then we felt- no, I shouldn't speak for the group, I felt that this is really dangerous, because [Prime Minister] Stefan Löfven has just, you know how it was, been able to form a cabinet, and the Social Democrats will stay in power, but this deal they've struck, it's all shit. I mean, so terribly bad.

After outlining how the LO economists publicly described this political deal – mainly comprised of promises of economically liberal political reforms in exchange for liberal support for a Social Democratic government – as harmful for LO members, she continued:

> And then when people ask us, but does this mean that you should dismantle the union-political cooperation or that you should leave the January Agreement and so on, then I guess we've dodged the question and said that that's not within the frame of what we do, we're just saying that this is really bad, and then we hand that over to someone else who can act on that evaluation. But this far, what has happened is that the party has been very, very critical [of us]. Let me put it like this: high-ranking representatives of the government and of the Social Democratic Party have been very upset with our disloyalty.

This exchange shows a few different things. First, that the autonomy of the LO economists enables them to express opinions that go against the rest of the organisation, but doing so is seen as a potential problem by the LO economists, and not as normal occurrence. Second, that while the relationship between LO and the Social Democrats is such that going against the party is seen as disloyalty by party representatives, the LO economists do not feel that they have to be loyal no matter what. And third, that the LO economists at least in this situation defined their role as researchers presenting the consequences of certain policies rather than as political actors; they produce results for someone else to act on. What should especially be noted here though is that even when the relationship between the LO economists and the Social Democratic Party is as strained as at the time of this interview, the politicians that they meet with are still primarily Social Democrats; in regional union-political cooperative conferences, to groups of visiting politicians, to the Social Democratic parliamentary group, and in other contexts. The political side of the bridge metaphor, for LO, primarily refers to the Social Democratic Party, and the party is treated as something external to LO research.

The LO researchers enact the Social Democratic Party, and the world of politics more broadly, as different from their own in part for reasons that have to do with their respective relationships to knowledge production and epistemic claims. When I presented my research project to an LO researcher,

and said that I was interested in the knowledge production of political organisations, she told me that while political organisations in a broader sense – including LO and think tanks – produce knowledge, she does not think that the political parties produce or even communicate knowledge. A bit later in the interview, she came back to this topic: "I don't want to add to the general contempt of politicians, but I don't trust any claims made by a politician, because it always feels like they've just read something somewhere, they're so far down the chain of knowledge." Politicians, then, are seen as being quite different from the those in the world of knowledge-producing political organisations such as LO; as in the bridge metaphor, they are recipients, or end users, of knowledge produced or transmitted by others.

Another observation about the differences to politics was made by a researcher who had worked for the Social Democratic Party as an analyst before coming to LO. When I asked her to compare her work as a party researcher with working as a trade union researcher, she described the two as quite different:

> Partly because you don't have a context, even if we tried to build one, we were two researchers at this policy unit that was quite large, most people working there were political aides for the different parliamentary committees. [...] Even if there was a consciousness that work should really be more future-oriented than you have time for when you work with high-speed parliamentary politics, it was still pretty hard for us to find like, this is what we should do. And we didn't write reports as such, but different policy programmes and shorter [documents], a lot of PMs that you send to different places or try to get journalists to realise that they should be interested in, but it was difficult.

Being a researcher was difficult in the Social Democratic Party in part because of how few researchers were employed at her unit, and in part because of a difference in the temporality between trade union research and work close to the Riksdag. The speed of parliamentary politics was seen as being incompatible with the kind of work that she thought that a researcher should do, and led to the writing of shorter and more policy-oriented texts, as opposed to the comparatively lengthy reports of LO research. This, again, describes the world of party politics as something different from the world of trade unions in their relationship to research, affirming the view of the political world as users of knowledge produced in the research of other places.

As in the bridge metaphor, the predominant traffic with the world of party politics – most commonly with the Social Democratic Party – is in the form of epistemic claims and policy recommendations sent from LO research to the world of politics. From the perspective of LO research, the political world

appears to be populated by end-users of knowledge, and epistemically meaningful research is hindered within politics by the high speed of the political world.

The bridge metaphor revisited

The picture painted this far in the chapter is not too far off from the bridge metaphor. Epistemic claims produced in academic social science are transported, but also transformed on the way, from academia through LO research, with the world of politics as its final destination. They are transformed into coherent epistemic narratives relevant to the political issues the organisation works with, and the transformation is done for epistemic reasons as well as political ones; when social science is treated as one social world, as a machine that can be consulted for truths about society, its multivocality needs to be shaped into a singular voice to make sense. Politics, on the other side of the bridge, is seen as a place that knowledge is sent to, but with little capacity to produce or even communicate meaningful knowledge on its own. So far, the bridge metaphor seems to present, if nothing else, a coherent description of the relationships between LO research, academia and politics.

As a description of what goes in and out of LO research, however, the bridge metaphor is, as noted earlier, incomplete. As is clear from the two chapters preceding this, two other notable worlds need to be added to the picture, multiplying the exits of the bridge and stretching the metaphor towards implausibility: those of government agencies, and of the LO members. The social world of government agencies needs to be added primarily as a source of epistemic resources, as important as the world of academic research. The perceived world of LO members needs to be added primarily as a receiver of the products of LO research equal in importance to politics. Both of these worlds, however, are treated as both sources of material – data and other information – for LO research, and as places where LO research can have an effect, meaning that traffic both enters and exits the bridge to both government agencies and the union members.

Government agencies

When I have asked LO researchers about their methods for keeping up to date with academic research, there has been a pattern in their responses: they have started talking about reading research articles, going to conferences and following academics on social media, and then, without prompting, moved on to talking about the information they get from government agencies. This is the first multiplication of the bridge metaphor I encountered: knowledge and information, as traffic on the bridge, comes from academic social science, but also from the epistemic activities of Swedish government agencies.

Traffic to and from government agencies take a few different forms. The most important epistemic resource taken from government agencies is in the form of statistics; the vast majority of data presented in LO reports come from whatever government agencies are related to the work of individual LO researchers, and often from the public statistics of Statistics Sweden. One economist told me, when asked how they decide what data to use for a report, that he spends a significant part of his workday exploring statistical data primarily from different government agencies and then decides what to write based on what he sees. He said:

> Just this morning I was looking at the Employment Agency's latest, that is, the statistics file that the lady at the Employment Agency sends me every month, because I realised that I hadn't explored it for, say, six months. So that's how it goes, you explore the data, and you form an impression of what reality is like. Now, when I was exploring it again, I realised that reality is more or less as I believe it is, but some things have happened that we need to start thinking about. And that goes for all data, you explore it, and then you form a picture of how it works and what it looks like, a simplified picture of course, and then the next time you sit down you start revising the picture a bit, because things happen.

If the epistemic products of academic research are seen as a mess that needs some sorting out in order to tell us anything about the world, the lady at the Employment Agency offers a direct link to reality: by exploring the spreadsheets she sends, the economist forms an impression of reality and how it changes. A few different explanations can be found for the ability of government statistics to, unlike academic social science, show reality directly. The perceived specialised knowledge and impersonality of the bureaucrat probably helps (cf. Weber, 2015); a social science text, even one which claims objectivity, is attached to an individual author making claims, while public statistics are the impersonal products of a bureaucracy. Another explanation is pointed towards in Alain Desrosières's study of administrative statistics.

Desrosières showed how the dual history of statistics as an administrative and political tool on the one hand and as a tool of science on the other forms a complex and stable network that enables a shared language for public debate (Desrosières, 2011). While the categorisations that go into the production of administrative statistics – such as the numbers sent to this economist from the Employment Agency – have an obviously constructed quality in the sense that there is an explicitly accounted method for producing them, their results have an objectivity that is difficult to challenge. The social objects of these statistics, like unemployment, have specific histories, and the objects as well as their operationalisations are contestable, but broad agreement on objects and operationalisations is highly valuable for actors who want to engage in public political discourse, and contestation is made difficult by the web of other phenomena that such objects are entangled in. If an actor wanted to challenge a widely accepted definition of, in the case of this economist, unemployment, they would not be arguing against a singular person making a claim, but against a large number of government agencies and academics using the definition in their research, as well as political organisations and other actors drawing on the epistemic claims produced. Even if the challenger could get others to agree to flaws in the definition, the fact that a definition is widely used is a strong argument for it in itself, as continued use of the definition enables comparison over time and between contexts.

Curiously, something similar to the multivocality that was described as an epistemic problem of academic research exists in the unemployment statistics referenced by this economist, without being perceived as a problem in this context. The Employment Agency's unemployment numbers regularly differ from Statistics Sweden's Labour Force Survey – another source of employment statistics very frequently employed by LO researchers – due to methodological differences; the Employment Agency reports population data for all individuals registered at job centres, while the Labour Force Survey is based on sampling of the Swedish population. Unlike for academic social science, multivocality does not seem to present a problem for LO researchers when it comes to employment statistics, and both are instead treated as direct and accurate representations of reality. This can probably be understood with reference to the different uses of the epistemic products of academia and of government agencies respectively. Academic knowledge claims synthesised into coherent narratives by LO researchers is in the form of synchronic, contextually stable objects. They represent entities like causal mechanisms taken to exist in the world, such as the causes of right-wing populism, or the effect of your classmates on your grades in school, to take two examples from LO reports which to a large degree mobilise social science results (Boguslaw,

2018; Nilsson & Nyström, 2018). The relevant question for LO researchers to ask to these claims is if the things they refer to really exist as presented in research. Public statistics, on the other hand, are used in a more diachronic way; they are consulted to see what happens from one month to the next, more than to see what currently is. The question asked to the claims made in the public statistics is if they have changed from the last measurement. This means that their usefulness for seeing reality is predicated primarily on their methodological consistency: as long as the same kinds of numbers are produced from one month to the next, they can show that something has changed, enabling LO researchers to, as the economist above describes, constantly revise their picture of reality. While both of these kinds of claims are relevant for researchers, they imply different criteria for validity.

While the statistics of government agencies are central to LO research, traffic with agencies is not unidirectional; government agencies are also an important avenue of political influence. One of the research directors I talked to described four different routes of political influence for LO researchers: supporting collective bargaining, public advocacy, affecting politicians through union-political cooperation, and finally what she called "the dry government agency route":

> We get to comment in the referral system, we sit in the reference groups for commissions of inquiry, we get called to advisory boards and reference groups at different agencies, and we can affect how they in turn give proposals to the government in their own areas of responsibility.

Compared to the meetings with and presentations for politicians in union-political cooperation, this way of affecting politics is rather indirect. What is referred to in the quote above is in part when LO researchers represent the organisation in bodies that have no formal decision-making power but that advise government agencies. Some government organisations, such as the public pensions funds, also have representation of social partners in their boards of directors, and LO researchers may act as board members there too. Such representation is a remnant of Swedish corporatism, which, as described in chapter 2, is largely perceived as something of the past. A more common way in which corporatist influence remains, however, is in the advisory boards at government agencies referred to in the quote above, to which organisations taken to represent broad social interests such as LO are invited. This mode of influence is largely invisible to the public, and its efficacy is difficult to judge, but the basic idea is that LO researchers can influence both governance and politics, through participating in expert discussions in agencies, and through affecting the feedback government agencies' give to the cabinet. One form of

traffic from LO to the agencies is thus in the form of advice given by LO researchers as experts on such advisory boards and committees.

Finally, the work of government agencies also forms a large part of the epistemic category of utredning that LO researchers either compare themselves to or situate themselves within. When I asked one LO economist to compare what they do with academic research, for example, she objected to the comparison:

> I don't think it's relevant to compare with academic research [forskning] because this isn't an academic research institute. Academic research is something else, it's more long-term, it needs to meet the publication requirements of international journals, it's not even what we have as an ideal. On the other hand, there are the standards for public research [utredning] methodology of researchers at lots of different places, [...] at government agencies and elsewhere, and that's the closest point of comparison. And there the difference from our work is that the methodology is the same, but the questions we choose and the final policy recommendation chapter we write is based partly on the results of the research but also on our ideology, which emphasises equality, perhaps more than efficiency. [...] And at government agencies, they say "that's up to the politicians, we've shown the consequences in terms of equality and efficiency, and we now leave it to the politicians to decide how they value the different effects". We don't leave that up to the politicians, but say what we think, that's the difference.

This economist painted a picture of a larger context of Swedish public research, the activities of utredning, with certain methodological conventions and standards that LO research follows. While a distinctive feature of researchers at LO compared to those at political agencies is that they prescribe political action as well as describing consequences of action, there is, according to this researcher, a perceived set of shared standards for methodology and analysis between LO research and political research in other places, placing their activities closer to those of government agencies than to those of academia. That government statistics are treated as being unquestionably real in the context of LO research implies that this is a source of legitimacy. Not only political research is treated as a coherent social world in the quote above, but also academic research: academic research is seen as having, among other things, shared publication requirements, that LO research is implied not to live up to. Academic research is also seen as different from LO research in its temporality: in academia, research is slow, or long-term, compared to the world of utredning that LO researchers situate themselves in; conversely from the discussion of politics above, where the political world was too fast for meaningful research to be done. In this way, distinctions are made between

both LO's political research and academic research, and their political research and politics. As earlier quotes in this chapter have shown, however, not all LO researchers agree with this division, with some finding their work closer to, or comparable with, academic research, despite how uncommon it is for LO researchers to publish their work in academic contexts.

The traffic to and from government agencies thus takes three main forms. First, statistical data are imported from government agencies for use as empirical material in the writing of reports. These data are taken to be very real; keeping up with the latest published data is the same as keeping up with what is going in the world out there. Second, government agencies are a target for political influence through expertise, which is a form of traffic from LO research to the agencies. Third, the perceived methodological standards of government research are used in LO research, serving as a point of comparison to and a source of legitimacy in their own research.

LO union members

If academic research is not the only source of traffic coming onto the bridge of LO research, politics is not the only exit. An equally important set of imagined readers, or targets for influence and advocacy, are the members of the LO trade unions. In one of the first interviews I did for this project, I asked an LO research director if all reports aim to influence public political debate:

> No, not always, no. We're not just a political organisation, but it's also about influencing, or informing, members, informing the unions about the state of things, [ultimately] in order to affect our counterpart on the labour market, that's another genre. There, the purpose isn't really to achieve political results, but to have better outcomes in bargaining, so the purpose is different from [a report] directed to the public in general, to political decision-makers and maybe also to employer organisations.

The notion of politicians and the general public as one audience and union members and unions as another expressed here follows the same conceptualisation of politics as a distinct world from that of trade union activities drawn on in the discussion of the world of politics; in this, for LO very frequent, way of talking about politics, the political world is that of parties and public debate and is distinct from that of collective bargaining between social partners on the labour market. While this research director described texts directed at these different audiences as different genres, and gave an

example of a report directed specifically at the public in general and politicians, this distinction is not always clearly defined in the process of research. Another research director, when asked if they make distinctions between reports directed at different audiences, told me that they do not. She continued:

> Sometimes I think we should do it more, nail down the target audience more clearly, but I don't think we've been great at that. Instead, we decide to produce and publish a report, with the target audience as something like the interested public and politicians and others interested in these questions. And it's not very specified who we have in mind.

Unlike the first research director who talked about reports as different genres with different specific target audiences, this research director described a research process in which imagined readers are left undefined. In other interviews, this seems to be the most common way of working with LO reports, with reports written for specific purposes treated as exception rather than rule. While LO union members generally being a part of this imagined audience, the actual readers of LO reports are in practice rarely individual union members. One LO researcher, when asked what the main audience for a specific report was, lamented this state of affairs:

> Researcher: Well... I'd actually like for it to be the LO unions' members. I'm always glad when I get asked to come and talk about it and I realise that it's because some union chapter has read it and worked with it. But always glad and a bit surprised, actually. This thing with target audiences is difficult because most of what we LO economists do is for the debate here in Stockholm, so to speak.
> Staffan: But the debate in like...?
> Researcher: We say that this is and we want this to be for our own members, for our own organisation, but it's, I mean, when I discuss the issues that I work with, then it's always other political elite people questioning me, it's actually very rarely LO members who contact me.

The people who respond to their reports are in practice the participants of a perceived debate in Stockholm between members of the political elite, which is a category that this researcher includes LO researchers in. On the other hand, LO researchers present themselves as wanting to speak to LO members as well as to this elite debate.

If individual LO members generally do not read their reports, however, this does not mean that the members as a collective are not an important target audience. As discussed in chapter 6, chains of representation mediate the interests of LO union members for the LO researchers. Similarly, contact with

the members is often mediated by different forms of representation. While it may be rare that members of local union chapters read their reports, or at least that they respond to them, talking to people who are taken to be able to speak for the members is less so. An LO economist, comparing what they do with the work of economists at other organisations, talked about such contacts in relation to the value of being able to write in an accessible manner:

> We often target politicians and journalists and so on, or other economists, but we must also be able to write in such a way that LO members for example, or their elected representatives, can take in what we write... We have a pretty big group, but still a group, on the labour market that we see as our audience, they are who we work for, the workers.

What to take note of here is the mediation of the workers through their elected officials; even if it is rare that members at local union chapters have read their work, the members are still the audience mediated through representatives within the organisation. When I have asked LO researchers where they have held presentations or otherwise acted as experts, contexts with union representatives such as union courses, regional offices and other union settings seem to be as frequent and central as political audiences.

Seemingly at odds with the notion that LO researchers work for the workers, LO members are – as the research director at the beginning of this section indicated – a target for influence or advocacy as well as for information. By providing epistemic accounts of society relevant for union interests, LO researchers also strive to affect what issues their own movement is concerned with. In one of my interviews, I talked with a researcher about a series of presentations of a larger research project I had followed. After having learned that they do not evaluate the outcome of their presentations as much as they think they should, I asked what such an evaluation would look like if they were to conduct one:

> In the best of all possible worlds, I think that, since we need to find ways to activate our own organisation, then one could've thought that this is for the purpose of that, and then we could've had an evaluation or at least a follow-up, like, six months later: how is it going, have you, for example, found any issues that you work with and what are they, how is that going, why isn't it working, what can we do? But (laughing) apparently, we're not that professional, so we haven't done that.

The presentations of this large research project, containing both epistemic findings and policy suggestions, should ideally have led to local organisations

picking up the issues presented by the researchers for continued work within the union movement and directed towards politics. Despite the lack of follow-up or evaluation of LO researchers' communication to their own members, such influence is sometimes made visible when motions are written for the LO Congress; before the Congress, analysts and economists are charged with drafting responses to motions written by the member unions, and there they can see if the issues they have raised are picked up by the members and their representatives or not. Somewhat later in the interview, the researcher described what happens when influencing the members in this way fails: "We do [research] for a while, and then we put the published reports on the bookshelf, and we start a new research project. There's a big risk that that happens. It's what we normally do, unfortunately."

This kind of influence on the organisation and its members is in line with a finding in the Swedish field of research on policy professionals: professional employees of political organisations are there taken to have an influence on their own organisations, supposedly limiting the democratic influence of members (Garsten et al., 2015). The dichotomy presented by the LO researcher above between influencing what issues the members work with and parking reports in the bookshelf suggests an alternative interpretation to this negative evaluation of the influence of political professionals: if political professionals did not have an influence on the organisation, their work would be meaningless. If they were not able to tell the members things they did not already know or suggest new political action – two forms of influence through research – they would have failed as LO researchers, and there would be little point for the organisation to employ them. They do not see their influence on union members as a threat to the democratic functioning of the organisation, because influencing the union members is the same as working for them.

Compared with the relationship to the world of politics, however, where traffic is unidirectional, the union members are not just the target of influence, but also a source of epistemic resources. Union members – again generally mediated through people taken to represent them – influence the research in several ways. One of these ways is through suggesting or affecting research topics and questions. One LO researcher, for example, colourfully described his meetings with *ombudsmän* as a "hoovering of the social reality", leading to his picking up and "reportification" of social issues important to the union members. The process implied by this is that the (representatives of) the union members suggest relevant issues in their union work, which is taken to be the same as social reality and which the researchers pick up as topics when writing reports. As discussed in chapter 6, the ombudsmän acting as representatives to this researcher are not elected representatives, but speak for the workers

through experiential knowledge; the basis for mediation can, as in the case of mediated interests, be both a democratic mandate and a mandate through experiential knowledge and identity.

A different way in which meetings with people taken to represent the experiences of LO members is used in research is that they can function as a test of the validity of the epistemic claims of LO research. Later in the interview, the same researcher compared his work with academic research, finding the interactions with union members the main difference:

> You get a dialogue and feedback that you don't get as an academic [...] the analysis you do is tested against a very concrete reality. So people say, but does this really work in elderly care as you say? Practically, that is, do the employees really think that what you claim is a problem actually is? Those discussions are incredibly interesting, it's very dynamic and exciting, I think, as a researcher, because it's not the kind of grassroots dialogue you have as an academic.

Here, meetings with members function as what Latour has called a trial of strength (Latour, 1987, pp. 74–79) of the epistemic claims made in LO research: presenting your claims to people taken to represent the social reality of LO members is a way to find out if the claims hold or break when hit with the "very concrete reality" of members' representatives. By treating the experiential knowledge of LO members as ontologically privileged, interactions with members are taken as a form of direct access to reality. Curiously, such interactions are enacted as being more real than the epistemic results of LO research, and, as the quote above indicates, of academic research; if not, they would not function as a test of validity. In other words, the strength of their research is tested by hitting it against the hard reality of the members' experiences, as this reality is expressed in meetings with union members and their representatives.

Interactions with members as a reality test is seen as an important contributor to the epistemic value of LO research. One LO researcher described the problems with union-funded think tanks as being that they, unlike LO research, run the risk that "some of the research and analysis they do won't correspond to the reality out there in the unions"; a risk that is avoided at LO through their mediated interactions with members. Yet another researcher, when we talked about ombudsmän at his research unit, stressed their importance for their research, explaining that "it's a huge resource that you have that experience and knowledge of what it's actually like in the professions and for the members we speak for, having that close to us in that way is a huge resource"; for this researcher, too, spokespersons for the

members are treated as having access to a reality out there that LO researchers can draw on and compare their epistemic work to.

As such comparisons require the reality of union members – accessed through their representatives – to be stronger than the epistemic claims made by researchers, they tell us something about the relationship between political representation, whether democratic or experience-based, and epistemic representation in LO research. The representation of members through ombudsmän, elected representatives and others is taken to be more valid than the representation of reality drawing on public statistics and academic research in the epistemic claims of LO researchers. The problem of expertise discussed in chapter 3 thus has a particular solution in the kind of political research done at LO: the displacement of political authority by and to epistemic authority is avoided, not by making the researchers yield to representatives of a body politic for political reasons, but by affording an epistemic privilege to those who speak for the members. The solution is not that epistemic actors have to obey political actors, but that those who are taken to legitimately represent the members are taken to also accurately represent reality.

If LO research is a bridge connecting the members with other worlds, part of the traffic on this bridge is in the form of a perceived direct access to a social reality from the members to the LO research. The members function as both a target of influence and information, and a direct channel to an imagined out there – in relation to the in here of the research – which is treated as an epistemic resource. For both of these forms of traffic, the individual union members are generally mediated through people who are taken to be able to speak for them. As discussed in chapter 6, the basis of such mediation can differ; it can either be based on a democratic mandate, as when it comes to elected representatives, or on experiential knowledge, as in the case of ombudsmän.

Is LO research a bridge?

With the four main other worlds that LO research connects to in place, we can once again, as the conclusion to this chapter, take stock of the bridge metaphor. Considering only the relationships to academia and politics, the metaphor seemed more or less apt – albeit in a version of the metaphor where political agency and transformative capacities of bridges is assumed – in the sense that traffic primarily went from academia and to politics, entering the bridge as epistemic claims and leaving it as advocacy and influence. Adding government

agencies and the LO union members as sources and recipients of traffic made the metaphor significantly messier, with things that can be variously described as epistemic and political both being drawn from and sent out to the other worlds. The picture, as a whole, is of LO receiving, from different worlds, things with very different qualities: academic research is a source of epistemic claims in the forms of contradictory conclusions of research; government agencies are a source of public statistics that serve as unproblematic representations of reality; and LO union members are used as a direct link to the lived realities of Swedish workers. These different materials are then synthesised into epistemic claims and policy positions that are in turn sent out into the worlds of politics and to that of the LO members in the form of reports and other textual products, and presentations of the same texts, and to government agencies through researchers acting as expert representatives on councils and in reference groups. In this way, different epistemic resources are turned into something that is both knowledge and political influence. Something like a workshop might be a better metaphor than the bridge; a place where ideas and materials are gathered to construct new objects, which are in turn sent out to those interested in using them. Those who criticise the bridge metaphor for obscuring agency would presumably agree. Less metaphorically, Latour's concept of the oligopticon is a better description yet; the centre of a star-shaped configuration translating objects from many other sites in order to produce narrow but durable versions of reality (Latour, 2007a, pp. 181–182).

What is especially notable compared to the linear model of knowledge and policy in the bridge metaphor is not the number of sites interacted with but the kinds of things that go in each direction. Epistemic resources are not just taken from academia, as one might assume if one believed that universities were the epicentres of knowledge in contemporary societies, but also from government agencies, which through their own research produce statistics and reports that are taken to be more direct representations of a reality out there than the epistemic claims of academia. This source of epistemic resources is perhaps not too surprising; academic social scientists also often rely on public statistics in their work. Crucially, however, mediated contact with union members, too, serves as a source of epistemic components in LO research; the experiential knowledge of union members, as mediated through their representatives, is used both when deciding on report topics, and in the process of research and beyond as a test of the validity of LO research findings. And when academic research is used, political positions are seen as a necessary epistemic tool for making sense of it. Beyond separating academia and politics as sites, the linear bridge metaphor implies a clear division of labour between the epistemic and the political more generally: research is where knowledge is produced, and

politics is where decisions are made. While this division of labour was affirmed in the LO researcher description of politicians as "far down the chain of knowledge", it does not seem to hold generally, and the distinction between the epistemic and the political is not always clear. In contrast to such a separation, democratic mandates and political positions are built into the research, not only bookending the process by initially defining research interests and drawing policy conclusions once the research is done, but for epistemic reasons throughout the research process; they are meant to make the knowledge produced more valid. LO researchers do not just use research to make policy, they also use politics to make knowledge.

The use of the experiential knowledge of mediated union members can be compared with discussions on the limits of expertise in the field of science and technology studies. In their essay on technical democracy, Michel Callon, Pierre Lascoumes and Yannick Barthe explored how the technical knowledge produced by experts on topics like nuclear waste storage and bovine spongiform encephalopathy can overflow into the world beyond the sites of knowledge production, and be met with resistance from lay publics, leading to renegotiations over the boundaries between the technical and the social (Callon et al., 2009). The privileging of experiential knowledge of union members serve to prevent such overflows, and to make the meetings between the technical knowledge of LO researchers and the union members out there lead to resonance rather than resistance; they bring the out there into the research process by treating the political representation of union members as an epistemic resource. A similar reason for bringing such perspectives external to the group of experts is to find a framing of issues which is stable; Linda Soneryd and Göran Sundqvist have proposed that such stability in framing as more stakeholders are involved in an issue is a good measure of successful meetings between science and democracy (Soneryd & Sundqvist, 2023). As implied by the controversies discussed in chapter 5, however, such ways of bringing actors out there into the research process are not always successful within LO, and even when they are, they hinge on the legitimacy of those who speak for union members.

How did the relationship between research and politics change, then, as worlds were successively added to the picture? When looking only at the relationships to academia and politics, these were enacted as coherent social worlds that seemed to map on relatively well to the bridge metaphor's understanding of knowledge and power as separate domains on the two sides of the bridge. Academia and politics as stable entities external to LO research were enacted as separate social worlds, connected by political research. Unlike in the bridge metaphor, however, the knowledge claims of academia needed to

be selected politically in order to produce an epistemically meaningful picture of reality in the setting of policy-making. As contact with the members was added to the picture, the relationship between the epistemic and the political shifted from being a relationship between the external social worlds of academia and politics, to a more complex picture. Advocacy and knowledge-production merged in relation to the members, as informing and affecting were the same thing, and the political representation of union members was used as epistemic resource in the research. While the epistemic and the political were bounded to the domains of academia and politics in the original bridge metaphor, they were less so as more worlds were added. From the vantage point of LO researchers, they are – like a bridge – situated on the boundary between knowledge and politics. But how the boundary is drawn, what is on each side of it, and what moves between them changes depending on what part of their work one looks at.

8. Political research

The researchers at LO are knowledge producers employed by a political organisation. I have proposed that studying political research – the production of versions of the world drawing on academic social science done within political organisations – is a worthwhile extension of the political turn in science and technology studies. Such an extension can produce a richer understanding of the work of such actors than those provided by perspectives in political science and political sociology that otherwise highlight the importance of the knowledge in politics. In line with this ambition, the three preceding chapters have approached the work of LO research with different questions and from different perspectives. Across these different approaches, I have been motivated by a broader interest in one empirical question: How do politics and knowledge interact in LO research? By way of answering this question, I have tried to paint the following picture of the work of LO researchers from different angles:

They produce knowledge and policy. They speak for the workers. They connect different domains, or social worlds, to each other. None of these things are achieved lightly, nor are any them purely discursive claims on the part of the researchers. They all rely on different technologies of representation: of representing social reality through the techniques of social science (e.g. citations to academic literature, use of statistical data), and of representing political subjects – the members of the organisation – through the techniques of organisational democracy (e.g. elections, democratic mandates, minutes and protocols) and through experiential knowledge and identity. Both of these kinds of representation draw on varied resources from sites within and outside of LO research. The distinction between these different modes of representation is not always clear, and the researchers use both when producing epistemic claims as well as when speaking for union members.

In relation to this picture, two different answers to the question of the relationship between politics and knowledge can be found. The first answer is that distinctions between research and politics are fluid and local, and while such distinctions may rely on common assumptions on the relationship between knowledge and power, they are not absolute. Consequently, questions

of whether the research is political, or the politics are informed by research cannot be answered outside of particular moments when the distinction is made, as such questions rely on these distinctions. The second answer concerns the role of the political expert, which is a creature which challenges differences made between epistemic expertise and political representation. In the case of LO researchers, the well-explored problems of expertise and democracy seem to have solutions in their own setting, allaying the tensions between knowledge and power without taking one to be foundational to the other.

In this concluding chapter of this thesis, I will explore these answers to the broader question behind the empirical analysis of this thesis. Following that, I will discuss some of the potential implications of the political epistemology I am suggesting with this thesis, and some useful starting points for such research beyond the case of LO.

Knowledge and politics

While the preceding three chapters of this thesis all explored the relationship between epistemic and political phenomena, they have done so drawing on different meanings of both politics and knowledge. In chapter 5, I discussed the production of two analytical categories of linguistic statements in reports: epistemic claims, which make up the parts of a report presented as factual accounts, and policy statements, which are solutions to political problems presented in the voice of LO as a corporate body. Epistemic claims and policy statements have different conditions for success and failure, and the production of the two seem to be separate games following different rules. In chapter 6, I looked at how the LO researchers as epistemic actors rely on the democratic and experiential representation of members for epistemic authority, drawing on a standpoint-like epistemology where speaking for the workers gives them an epistemic privilege. By using specific methods for finding out what the interests of union members are, LO researchers themselves become spokespersons for the members in their research, producing knowledge from a working-class perspective. In chapter 7, I analysed how key sites enacted as coherent and separate from LO research – academia, politics, LO members, and government agencies – are treated as sources of epistemic resources and targets of advocacy and political influence, in part through LO research acting as a bridge between perceived other worlds. Engagement with these different worlds vary both in what the researchers take in from them, and in what they send out.

At the end of chapter 7, I described how the boundaries between the political and the epistemic shifted as my analytical attention moved between different relationships to other worlds. LO researchers approached academia and politics as the source of epistemic resources and the end-users of epistemic claims respectively, affirming the boundary between knowledge and power implicit in the metaphor of the bridge. When looking at what goes into their research from LO members, and at the use of political narratives for making epistemic sense of social scientific results, however, the boundaries seemed less firm: political representation of different kinds was used as epistemic resources. The movement between affirming and dissolving boundaries between knowledge and politics, however, is not isolated to how they engage with different social worlds. Across the different meanings of both politics and knowledge of the three analytical approaches in the preceding chapters, there is a similar movement between moments of affirmation of boundaries between the epistemic and political, and moments of dissolution of the same boundaries.

In the production of epistemic claims and policy statements in reports, a clear distinction is made between what is political and what is epistemic. The boundary is drawn between knowledge claims and policy as two classes of linguistic statements in the reports; both in the production of the two classes of statements, which follow different rules of production with different felicity conditions, and in the design of the reports, which present the factual accounts as leading up to the policy conclusions. Once the reports are printed and sent off into the outside world, however, the boundary is less firm: the report as a whole succeed or fail by being picked up as a package, both as epistemic understanding of the political issues the report concerns, and as suggestions for political action. Reports risk failure by becoming shelf-warmers, and effort is spent to keep them alive, but they fail – or succeed – as a whole.

And in the achievement of a standpoint-by-proxy position in which the researchers speak for the workers, the boundary is drawn between the epistemic researchers on the one hand, and the political representatives of union members on the other. Only by engaging with actors which, relative to the researchers, are legitimate spokespersons for the members can they support their own claim of speaking for the workers, making a distinction between epistemic researchers and political spokespersons in those interactions. Through this engagement, however, the mandate to speak for the workers is extended to the researchers themselves, and the political ability to speak for the members becomes an epistemic justification of the research, dissolving the boundary once again.

As noted, what is epistemic and what is political differs between these different aspects of LO research, but the distinctions are made and unmade

across different meanings of politics and knowledge. Some degree of caution may be warranted here: it is I as analyst who has focused on these distinctions, and who is here terming the two sides of the distinction epistemic and political. There is, however, good reason to believe that the distinction between the epistemic and the political is relevant, not just to LO research, but in the knowledge practices of politics generally. As noted in chapter 1, a sharp distinction between facts and values – and knowledge and politics – are central to understandings of both research and politics in Western societies. This distinction is what Bruno Latour has described as the two chambers of Science and politics in the Western constitution (Latour, 2004a), and what Soneryd and Sundqvist described as a representative division of labour between science and democracy: "Science represents nature, and democratic governance represents the will of the people" (Soneryd & Sundqvist, 2023, p. 4). That a central impetus of science studies since the 1970s has been to demonstrate that science is political, as discussed in chapter 3, affirms this distinction; the force of such an argument is in challenging an assumption of science and politics as mutually exclusive categories. The dual threat of politics polluting epistemic practices and of epistemic authority undermining political representation invites the drawing of boundaries between the epistemic and the political.

One thing is especially notable with the drawing of such boundaries in LO research. While the boundaries between research and politics are recurringly drawn, they are drawn differently in different aspects of their work, implying that something like an underlying material-discursive structure (cf. Law, 1994) of a distinction between science and politics informs the production of such distinctions but that the dichotomy is not absolute and universal; differences are made locally – drawing on the dichotomy of research and politics – rather than having a ubiquitous and passive existence. Boundaries are recurringly drawn at some points, but dissolved or non-existent at others. This brings us back to the two solutions to the relationship between research and politics that I discussed in the first chapter of this thesis: as that of either research determined by political positions, which is implied when dismissing political research as mere politics, or of apolitical research informing political decisions, which is implied by the notion of evidence-based policy. As the distinction between knowledge and politics is only made situationally, with different actors moving across it, questions of causality between the two, which these two solutions are answers to, only become answerable in those instances when the boundaries are clear. Questions like "Are the epistemic beliefs of a political actor determined by their politics?" or "Are the politics informed by the epistemic beliefs?" may in principle be possible to answer at certain moments, when the boundaries between the two are relatively firmly drawn,

but the questions become nonsensical both at moments when they are not, and for political research activities taken in total. Looking at specific instances, one could – though I have not attempted to do so in this thesis – draw a line of causality between political sites or actors within the organisation and the epistemic work of researchers, as when issues are suggested by *ombudsmän* or when paragraphs are removed from a report by the Executive Council; these would be examples of when the political side of the distinction affects or determines the epistemic side. Conversely, in particular instances, one can see how the outcomes of research affect the worldview or actions of political actors in the organisation. When the boundaries are dissolved, however, questions of causality become muddled; when the researchers, for example, are delegated the ability to speak for union members and act as representatives on government advisory boards, they act both as experts and as spokespersons for the union members at the same time. If questions of causality between politics and knowledge can be answered only at particular moments, it makes little sense to ask if political research at LO in general is about producing knowledge or affecting politics: it is both. They produce arguments which are both epistemic and political, drawing on both technical expertise and political representation with neither of the two being foundational to the other. Consequently, solutions to the relationship between knowledge and politics which starts from an assumption of a causal relationship between the two consequently seem meaningless outside of particular instances of research.

One can note that this observation is only possible to make by paying attention to how these distinctions are made situationally, as opposed to starting the analysis with fixed theoretical definitions of terms like knowledge and politics. This is in contrast to, for example, Latour's categorisation of the political mode of existence as a way of speaking and acting with the purpose of public formation, distinct a priori from other modes in Western societies (Latour, 2013). In analysis drawing on a predefined understanding of what it is to act politically, distinctions made by the actors only become interesting to the extent that they may be wrong or confused, which is how Latour understood the threat of epistemic reference to the necessary public formation of the political mode.

The political researcher

The second answer to the question of the relationship between knowledge and politics is found in the role of the political researcher at LO. More specifically,

it is found in the role of the LO researcher in relation to democratic concerns over the authority of science. In chapter 3, I discussed such concerns in the field of science and technology studies as part of the normative project of the field, and the argument is about how experts and expertise affect democratic decision-making, without democratic accountability (Callon et al., 2009; Collins & Evans, 2002; Latour, 2004a; Wynne, 1989). Similar concerns are found outside of the field of science and technology studies, for example as expressed in Robert Dahl's warning against the technocratisation of complex democracies (Dahl, 1989), or, closer to the case of LO research, in worries over the professionalisation of membership organisations in civil society (Mellquist & Sörbom, 2023). Despite different scales and points of entry, the general problem is the same: how to square expertise with (democratic) politics?[53]

One poignant expression of this problem related to the role of the researcher is found in Isabelle Stengers' essay The Cosmopolitical Proposal (2005). There, she evoked the image of the palaver – a negotiation between groups without a shared language or culture – to discuss meetings between those with differing ontological and epistemological beliefs, to discuss interactions between scientists and concerned publics. Such meetings, she argued, should be done slowly and with great care and humility. When discussing the role of the scientist in such meetings, she contrasted two possible roles: that of the expert, and that of the diplomat. Experts present the state of things, without themselves being affected by the consequences of the meeting. Diplomats speak for what groups can live with, mindful that they may always lose their ability to represent them.

Compared to Stengers' dichotomy, LO researchers seem closer to the diplomat than the expert, at least in their meetings with elected representatives within the organisation. Their ability to speak for the facts is not absolute in the sense that union member spokespersons are obligated to listen to them without argument, and they are far from isolated from the consequences of the epistemic claims they make. They speak for how the world is – in their own setting and from the LO member standpoint – but they are also held accountable to those who speak for the interests of the union members. They achieve this accountability in part by integrating political and epistemic representation in their research, and in part by mechanisms of review and evaluation of the claims they make by those with a stronger mandate to speak for the groups they represent. In other words, the threat of expertise

[53] While such concerns have uniformly been expressed concerning democratic influence and expertise, similar conflicts seem likely to arise regardless of what the justification for political authority is, unless this justification is expertise itself.

undermining political representation seems to be managed in the role of the LO researcher; they contribute epistemically with political accountability.

With this, I am not trying to argue that the role of the LO researcher and its relationship to spokespersons for the members is a good model for research in general. While their ways of dealing with the potentially problematic meeting between expertise and political representation seems well-suited to their own form of knowledge production, it would be severely limiting for many other kinds of research. The vast majority of the work done by academic sociologists or in science studies for example, including the study leading up to the present thesis, would not have been possible to do within LO research; not just for lack of resources, but because it is not interesting to those who speak for their members. Calls for humility and carefulness in research are hard to disagree with, but it may well be that political accountability necessarily limits the scope of research to those issues which are relevant within specific political worldviews. For all its problems, Karl Mannheim's notion of the free-floating intelligentsia – an epistemic collective with a plurality of epistemologies and backgrounds – may in comparison point in the direction of research open to new impulses and self-questioning (Mannheim, 1960).

My argument is rather that the role of the LO researcher shows that in practical interactions between expertise and political representation, there are situational solutions to be found to the problems of democratic expertise. Looking beyond the particular political research of LO, I would expect other solutions to be found at other sites where knowledge and politics meet. While there are certainly situations where the meetings between epistemic and political authority are problematic, the problems may seem thornier when phrased in the abstract – as they often have been (e.g. Guston, 1993) – than in their specific instances. To those concerned with such problems, the empirical phenomenon of political research seems like a generative field of inquiry.

Political epistemology beyond LO

Throughout the first half of this thesis, I argued for a sociological understanding of researchers and experts at political organisations as knowledge producers; an understanding which does not preclude treating them as actors engaged with politics, but which resists reducing their activities to Machiavellian strategy and which refuses assumptions of politics and knowledge as mutually exclusive categories. More specifically, I have suggested that theoretical perspectives from science studies are valuable for

understanding the epistemic work of political organisations. The proof of any such argument is in the pudding, and in the second half of this thesis, I have strived to show the value of such research, through exploring different relationships between politics and epistemics in LO research. The case I have studied has a long history in Swedish politics, but I believe that such research may be timely. While different forms of producing knowledge about the world have always existed in politics, and knowledge drawing on the kind of social scientific expertise that LO researchers draw on has been produced in and for politics at least throughout the 20th century (Medvetz, 2012; Rueschemeyer & van Rossem, 2017), organisations working with both research and political advocacy seem to not only become more common, but to become increasingly closely situated to academia. As way of ending this thesis, I will first propose the sketch of an argument for why this might be the case, what some of the implications of such a development would be, and why the kind of empirical political epistemology I am suggesting with this thesis may be useful for understanding it. Following that, I will suggest a starting point for research in this field, foregrounding interactions between expertise and other ways of knowing the world.

Hybridisation of politics and science

While LO research is an old institution in the Swedish political setting, it seems more than likely that actors best described as epistemic policy actors will proliferate in the coming years both in Sweden and elsewhere, in continuation of a trend visible in, but not limited to, the rapid expansion of the number of think tanks over the last few decades (Åberg et al., 2021). Social science research on such actors has struggled with deciding which organisational actors are empirically relevant, as can be seen in the analytical problems of defining the term think tank in the field of think tank studies. For think tanks, such problems arise both from the strategic use and non-use of labels by the organisations themselves, and from the demarcation-problem-adjacent difficulties in distinguishing think tanks from research institutes or even universities (Åberg et al., 2020; Medvetz, 2012; Stone, 2001); problems which seem likely to arise in the study of any kind of epistemic policy actor.

My suggestion is to forego such broad categorisations, and instead focus on the activities of epistemic work done in close proximity to political settings, in the comparatively narrow sense of politics discussed in chapter 3. Besides sidestepping conceptual problems, such a focus enables the sociologist to move beyond an understanding of epistemic policy actors as necessarily existing on the boundaries between science and politics (e.g. Guston, 1999) – boundaries

which, to the extent they were ever stable, seem likely to be increasingly blurred by the proliferation of epistemic policy actors – and better capture the heterogeneity of activities and actors in the intersection of science and politics. Leaving behind the sharp distinction between academia and politics as bearers of knowledge and power respectively enables analysis both of how scientists act politically – a topic which has been highlighted in science studies scholarship (e.g. Latour, 1993a) – but also of how political actors act epistemically, as producers and not mere learners, users or brokers of knowledge. It follows from the contested meanings of the words politics and knowledge, however, that striving for clear delineations of which phenomena and sites belong to this category does not seem like a worthwhile endeavour.

One form of increased engagement between academia and politics can be found in university researchers who by commission or on a permanent basis work for think tanks or other political actors, or who otherwise integrate political work with their research, by producing epistemic claims for or in other ways acting as experts in political settings. While this is hardly a new phenomenon – Alva and Gunnar Myrdal are prominent examples of academics acting in the sphere of politics and governance in Swedish 20th century history (Wisselgren, 2008) – one could speculate that such engagement would be likely to increase as demands for the demonstrated, or at least proclaimed (Wróblewska, 2021), usability or impact of research become more common; as use of research by political organisations and interest groups is a straightforward impact for large parts of research in the social sciences, increased calls for demonstrated impact provides an incentive for researchers to engage with such organisations. Without the assumption that such engagement only takes the form of transmittance of knowledge from academia to politics, one could ask how such political engagement affects the epistemic practices of researchers, both within political organisations and in academia.

In parallel with a potentially increasing engagement with politics from academic research, a survey of think tanks and policy research institutes in the Nordic countries that I conducted in parallel with the present study indicates that many such organisations are in an ongoing process of academisation.[54] They aim to increase their numbers of employees with terminal degrees, to give a platform to academic social scientists by publishing their work to broader audiences, and to have their own employees publish through academic

[54] The survey was based on interviews with representatives of trade-union-financed think tanks and research institutes in the Nordic countries, for an unpublished report I wrote for Swedish think tank Arena Idé, making me one of the aforementioned academics who work by commission for political organisations.

publication channels to bolster their organisation's epistemic legitimacy. Beyond further adding to the dissolution of the boundaries between academia and politics, such an academisation would be likely to have repercussions on both academia and on politics. By providing career paths and opportunities for collaboration for academic researchers, academic research and epistemic-political organisations seem likely to become more integrated. If people working for such organisations increasingly look like, and publish like, academic social scientists, while still preferring a career outside of academic settings, they form an interesting example of researchers working beyond the incentive structures and career paths of academia. Alternatively, if employment for such organisations become a way for individual researchers to accumulate publications to merit oneself for future employment at an academic institution, they seem likely to develop into something like academic research institutes in their own right, while still depending on funding from interest groups and political parties.

If I am right in pointing these developments out, it should be stressed that academia and politics have never been isolated parts of society; the two have been closely linked at least since the beginnings of experimental science (Shapin & Schaffer, 2011). The relationship between the two may, however, be changing. More than politicisation of science and scientification of politics as parallel processes taking place within the realms of academic research and politics respectively (Weingart, 1999), these developments point towards the proliferation of hybrid sites and actors, challenging the boundedness of academia and politics as separate fields.

Interactions between ways of knowing

LO researchers are not an example of such a hybrid actor, as they draw on social science expertise but have relatively little direct engagement with academia through publishing or collaboration. One feature of their work, however, points to an empirical phenomenon that seems likely to be analytically relevant for understanding the knowledge activities of politics: the interaction between different technologies of representation, or ways of knowing. At several points throughout this thesis, I have related LO researchers' work to other ways of representing members and issues: specifically, through democratic representation and through experiential knowledge. What has been striking with these other social technologies of representation in the context of LO is that they – counter to concerns over the epistemic authority of experts – carry as much as weight as the technical expertise of researchers; experiential knowledge, for example, is taken to be

more real than the results of LO research, when it is used as a test of the validity of research findings, as discussed in chapter 7.

With sufficient generousness in what is called a technology of representation, any number of such technologies could be found in any setting. Political organisations, however, especially those based on membership, seem likely to have relatively habitualised and formalised ways of representing both their members and questions surrounding the political issues they deem relevant to their interests, and in the hopes of clarifying what I mean with technologies of representation, I will give two more examples that I have encountered in my fieldwork. The first is that I, in following the researchers and leadership around at workplace visits, was fascinated to see how the president and vice-presidents asked local and regional union representatives about ongoing conflicts at their workplaces, in their municipalities and in their regions, as a kind of informal expert interview repeated multiple times per week throughout the year. Such meetings can be understood as a massive flow of information through a capillary system of union representatives from the workplaces through local and regional representatives to the national organisation. As any infrastructure, such a system both enables transportation and limits and shapes what passes through it. Knowledge gained through such meetings is taken seriously within the organisation, and elected representatives are expected to be familiar with the situation of the members through them. The second example is a member panel organised by the LO communications office, where, drawing on the technologies of market research, supposedly average union members answer surveys to produce versions of the lifestyles and opinions of the typical union member. The representations of the members created thereby are then used in the communication strategy of the organisation, which sometimes causes friction with the researchers who do not necessarily agree with how members are represented in the panel. These two ways of knowing – along with those already discussed throughout the analysis of this thesis – drawing on different methods of production and justification, serve to make epistemic claims about actors and issues relevant to the trade union movement without directly relying on social science expertise. Paying attention to different epistemic technologies in political organisations is a good starting point for the study of political-epistemic work, and especially looking for how they interact with the production of epistemic claims drawing on technical expertise. The two examples above are specific to LO, but hardly unique; both marketing research tools such as the panel, and more or less formalised interviews or conversations are commonly used in larger organisations. What kinds of methods used and how important they are in relation to one another, however, is likely to differ between different

organisations and settings, and how such non-technical ways of producing versions of the world reverberates in epistemic practices drawing on social science expertise is an open question.

Conclusion

Beyond serving as a suggested topic of inquiry for those interested in the epistemic work of political organisations, the two alternative ways of knowing the members discussed above point towards a broader theme in this thesis: political research does not exist in an epistemic vacuum. While the production of epistemic claims through research is an important tool for producing versions of the world in which an organisation like LO acts, it is not the only means for political actors to see the world around them, and political research drawing on social science is necessarily implicated in networks of relations involving other epistemic practices, technologies of representation, and epistemologies. With this, I will make what is as much a final point in the argument of this thesis, as a prerequisite for it: as much as science is political, politics – with or without research – is epistemic. Paying attention to its epistemologies helps us situate expertise properly, as one important mode of knowledge among others, instead of as the antipode to political representation.

Svensk sammanfattning

Avhandlingen *Political Epistemology: Knowledge and Advocacy in Trade Union Research* handlar om LO:s utredningsarbete i modern tid, vilket tas som ett fall av när politiska organisationer anställer experter för att producera kunskap och policy. Genom intervjuer, deltagande observation och dokumentanalys utforskar jag hur politik och kunskap hänger ihop i utredningsarbetet på LO-borgen. Utifrån ett kunskap- och vetenskaps-sociologiskt perspektiv, till stor del grundat i det tvärvetenskapliga fältet teknik- och vetenskapsstudier, diskuterar jag hur det praktiska kunskapsarbetet går till i LO:s utredningar, och hur gränser både dras och löses upp mellan politik å ena sidan och kunskap å den andra.

LO anställde sina första fasta utredare – LO-ekonomerna Richard Sterner, Gösta Rehn och Rudolf Meidner – i början av 1940-talet, men anställde ekonomer för kortare utredningar redan på 1930-talet. Dessa första ekonomer tillskrivs ofta väldigt stor påverkan på den svenska arbetsmarknadspolitiken under efterkrigstiden. Idag har LO:s utredningsenhet cirka 20 personer anställda, varav ungefär hälften har titeln LO-ekonom och hälften är anställda som utredare. Dessa arbetar med att producera kunskap, att utveckla policy och med påverkansarbete, genom att bland annat skriva rapporter, förbereda underlag för förhandlingar och beslut, och agera som experter i offentligheten eller inom arbetarrörelsen. I avhandlingen följer jag detta arbete mellan 2018 och 2024, genom tre olika analytiska spår.

Kunskap och policy

Mitt första analytiska spår är att studera produktionen av å ena sidan kunskapspåståenden och å andra sidan organisationens politiska ställnings-taganden, eller policy, i de rapporter som LO publicerar. Dessa två kategorier av språkliga yttranden följer olika regler för hur de produceras, och har olika kriterier för när de anses vara lyckade eller misslyckade inom organisationen: kunskapspåståenden bygger på utredarnas egen expertis och utrednings-

metodik, medan policy behöver formellt godkännande av en aktör med demokratiskt mandat. Kunskap produceras och rättfärdigas med samhällsvetenskapliga metodologiska och litterära tekniker – exempelvis explicita referenser till datakällor och andra texter, formellt språk och ofta redovisningar av den metod som använts – medan policy produceras med implicita referenser till tidigare fattad policy inom organisationen, och genom att på förhand förankra de normativa positionerna hos representanter för de enskilda LO-förbunden. De två kategorierna av yttranden är tydligt separerade i de tryckta rapporterna, genom rubriksättning och i den grafiska presentationen.

Trots att produktionen av kunskapspåståenden och av policy framstår som två parallella spel med olika produktionsmetoder och strategier för att lyckas så vävs de två samman i de rapporter där båda förekommer så att policyn presenteras som en följd av kunskapen; kunskap och policy presenteras därmed som enhetliga i de färdiga rapporterna. Hur de två separata produktionerna sina olikheter till trots kan sluta på samma ställe kan förklaras med att den typ av kunskapsproduktion som görs har relativt förutsägbara resultat, vilket inte är samma sak som att resultaten är bestämda på förhand. Utredarna arbetar med frågor som de känner väl till innan utredningsprocessen har börjat, och till skillnad från i akademisk forskning värderas inte kvalitativt nya eller oväntade forskningsresultat högt i utredningsverksamheten. Detta gör att de relativt sällan kommer fram till resultat som strider mot deras tidigare förståelse, och ens när de gör det presenteras inte resultaten som nya i rapporttexten.[55] Tillsammans med att de har en förförståelse för den policy som organisationen har och som medlemmarnas representanter är villiga att ställa sig bakom gör detta det möjligt att producera rapporter där kunskap och policy är enhetliga trots separata metoder för produktion och rättfärdigande. Ett annat sätt att uttrycka det på är att LO-utredarna utgår från såväl normativa föreställningar om vilken politik som är önskvärd för LO-medlemmarna, som deskriptiva föreställningar om hur samhället ser ut och fungerar, och att dessa två är sammanvävda i en gemensam världsbild.

[55] Värt att notera här är att värderandet av banbrytande resultat i akademisk forskning i regel tillhör vetenskapens ideologi snarare än dess praktik; att resultaten är mer eller mindre förutsägbara på grund av tidigare bekanthet med det empiriska området, med de metoder som används och med teoretiska antaganden gäller i hög grad även inom vetenskapen.

Kunskap från ett arbetarperspektiv

Det andra analytiska spåret jag följer utgår från LO-utredarnas anspråk på att tala för LO-förbundens medlemmar i sin kunskapsproduktion. Som en motivering till att LO producerar sin egen kunskap trots den stora mängd utrednings- och samhällsvetenskaplig text som skrivs om svensk arbetsmarknad och svensk politik utanför organisationen framförs att LO-utredarna producerar kunskap från ett arbetarperspektiv som annars saknas i det offentliga samtalet. Bakom en sådan motivering finns ett antagande om att samhällsgruppers kollektiva intressen påverkar den kunskap de har tillgång till, i det här fallet på ett sådant sätt att kunskap från ett arbetarperspektiv skiljer sig från kunskap från ett tjänstemannaperspektiv.

Samma grundläggande antagande har varit återkommande i akademiska fält som studerat kunskapens sociala aspekter under 1900-talet, och återfinns exempelvis i Georg Lukács marxistiska filosofi, i Karl Mannheims kunskapssociologi, i feministisk ståndpunktsepistemologi och i 1970- och 1980-talens vetenskapssociologi. I fältet teknik- och vetenskapsstudier kom sådana antaganden att ifrågasättas kraftigt under 1980-talen. Kritiken grundade sig i att den som för fram en sådan analys förutsätter sig ha tillgång till objektiv kunskap om vilka de relevanta samhällsgrupperna är och vad deras intressen är, samtidigt som den (i fältet oftast naturvetenskapliga) kunskap som ska förklaras ses som ett utfall av dessa variabler. Kritikerna menade att det var ett vetenskapsteoretiskt problem att tillskriva sociologiska förklaringar ett sådant kunskapsmässigt privilegium i motsats till den naturvetenskap man ansåg sig kunna förklara.

Utifrån denna kritik ställer jag mig frågan om samma problematik återfinns i LO:s utredningar, och finner att så inte är fallet. Anledningen till detta är att utredarna inte tar de sociologiska variablerna "arbetare" och "arbetares intressen" som fenomen som de har tillgång till objektiv kunskap om, utan i stället som utfall av aktiviteter som görs inom LO:s organisation. Arbetare definieras helt enkelt som de som är eller skulle kunna vara organiserade av ett LO-förbund, vilket är en definition som är väldigt etablerad i svensk politik och förvaltning; samma definition används exempelvis i SCB:s arbetsmarknadsstatistik. Arbetares intressen ses som något som produceras inom LO, och som utredarna får tillgång till genom olika specifika metoder som alla involverar de aktörer – personer, beslutsfattande organ, och dokument – som inom organisationen anses ha legitimitet att representera arbetare politiskt.[56]

[56] Notera att förståelsen av detta som politisk representation, som något annat än kunskapsmässig representation i exempelvis statistik om förhållanden på den svenska

Sådan politisk representation sker antingen med hänvisning till de organisationsdemokratiska principer som råder inom svenska civilsamhällesorganisationer, eller med hänvisning till erfarenhets- och identitetsbaserad kunskap hos de som företräder arbetare. Fyra specifika metoder används för att få kunskap om LO-medlemmarnas intressen: att rådgöra med personer som anses kunna tala för de enskilda förbunden, att följa beslut om utredningar tagna på LO:s kongress eller i styrelsen, att indirekt arbetsledas av förtroendevalda och direkt av enhetschefer som arbetar nära förtroendevalda, och att interagera med ombudsmän på utredningsenheterna. Som ett utfall av dessa metoder anser de sig kunna tala för arbetarna i sitt utredningsarbete, trots att de, som högutbildade tjänstemän, inte tillhör gruppen själva.

De aktörer som inom organisationan har legitimitet att tala för arbetarna i förhållande till utredarna i metoderna ovan får denna legitimitet genom kedjor av talespersonskap som medierar medlemmarnas intressen, så att exempelvis utredarnas enhetschefer utgör representanter för medlemmarnas intressen för att de arbetar nära LO-ledningen, som i sin tur är representanter för medlemmarna för att de är valda av kongressen, vars ledamöter i sin tur kan tala för medlemmarna genom att vara valda av de enskilda förbunden. Genom metoderna ovan förlängs denna kedja så att även utredarna fungerar som talespersoner för arbetarna, och genom detta kan de göra anspråk på att bedriva utredningar från ett arbetarperspektiv.

Kopplingar till andra sociala världar

Mitt tredje analytiska spår handlar om hur LO:s utredningsverksamhet skapar kopplingar till andra sociala världar: sociala områden som behandlas som att de följer gemensamma regler och har gemensamma mål. Jag utgår från en vanlig metafor för att förstå den typ av politiskt orienterad kunskapsproduktion som LO-utredningarna utgör: att sådant arbete är en bro mellan akademi och politik. Metaforen utgår dels från en linjär modell för förhållandet mellan kunskap och politik, där den kunskap som produceras i akademin kan omsättas till beslut i politiken, och dels från att en sådan överföring av kunskap inte sker av sig själv utan att det behövs mellanhänder – såsom politiska organisationers utredningar – för att överföringen ska hända. Utifrån denna metafor ville jag

arbetsmarknaden, i och för sig bygger på en uppdelning som återfinns hos LO-utredarna själva, men att benämna det som just politisk representation är en analytisk inramning som jag gör i min analys.

undersöka hur LO-utredningarna förhåller sig till akademi och politik, båda förstådda som att de behandlas som enhetliga sociala världar av LO-utredarna. Jag letade därför efter vad utredarna tog in från och vad de skickade ut till dessa andra platser, för att kunna jämföra resultatet med brometaforen för kunskapsöverföring.

Vad som framkom av undersökningen var att LO-utredarna i och för sig såg akademin som en källa till kunskapsresurser för sitt utredningsarbete, och politiken – förstådd som partipolitik, och framförallt i form av Socialdemokraterna – som mottagare av kunskapspåståenden, men att bilden att de tar den kunskap som produceras i akademin och gör den tillgänglig och användbar för politiken bara delvis och bara i vissa situationer var en god beskrivning av deras arbete. I mina intervjuer framkom det att utredarna inte betraktade samhällsvetenskapliga forskningsfynd som i sig själva användbara för att förstå den politiska verklighet de befinner sig, inte för att de var för komplicerade utan för att de var alltför motstridiga och alltför beroende av specifika metoder eller frågeställningar: olika forskare säger olika saker om liknande frågor, utifrån vad de har undersökt och hur. För att skapa en bild av hur exempelvis kausala samband kring politiska frågor de arbetar med ser ut upplevdes vetenskapen behöva kompletteras och struktureras efter politiska narrativ, som ett ramverk för att kunna använda sig av olika samhällsvetenskapliga studier för att förstå en politisk fråga. Redan där skedde ett brott med brometaforen: kunskap ompaketerades eller förenklades inte bara för politisk användning i utredningsarbetet, utan politiska antaganden behövdes i stället för att kunskapen skulle vara begriplig just som kunskap. När de utredare jag intervjuat i stället talade om politiken – exempelvis om möten med politiker eller om tidigare erfarenhet i liknande roller i partiorganisationer – stämde deras förståelse bättre överens med brometaforen. Politiken, förstådd som partipolitik, sågs som en plats med lite utrymme för kunskapsproduktion och där de kunskapspåståenden som framfördes hade relativt svag förankring i den forskning eller de utredningar som de utgick från; med andra ord sågs aktörer inom partipolitiken som mottagare eller användare av kunskap som producerades på andra ställen, om än ofta en delvis förvanskad användning.

I min undersökning av vad utredarna tog in och skickade ut till andra platser framkom det att två ytterligare sådana världar var av central betydelse i utredningarna: myndigheter och LO-förbundens medlemmar. Myndigheter användes både som en källa till kunskapsmässiga resurser, såsom statistiska underlag för rapporter, och som en mottagare för politisk påverkan, bland annat genom de rådgivande grupper med korporativ representation som många myndigheter har. Även LO-förbundens medlemmar betraktat som enhetlig

social värld fungerade både som en mottagare för påverkan och som en källa till kunskap: det första genom att LO och förbunden använder utredningarnas resultat, och det senare genom att kontakt med LO-medlemmarna ses som en väg till direkt kunskap om arbetares arbetsplats- och arbetsmarknads-förhållanden, som används som validering eller på andra sätt som kunskapsmässig resurs inom utredningarna. Liksom när det gäller de metoder som utredarna använder sig av för att konstruera medlemmarnas intressen är dock sådan kontakt med LO-medlemmarna i princip uteslutande medierad genom de personer som utredarna anser vara legitima talespersoner för medlemmarna, exempelvis ombudsmän eller förtroendevalda i förbunden.

Den sammantagna bilden av kopplingarna till andra platser är att LO-utredningarna i och för sig fungerar som en bro på så sätt att de kopplar till olika centrala platser, men att uppdelningen mellan vad som är kunskap och vad som är politik, till skillnad från i brometaforen, inte är särskilt tydlig sett till deras arbete som helhet. Även om situationsbundna gränser kan dras mellan akademin som källa till kunskapsresurser och politiken som mottagare för kunskapspåståenden så är sådana gränser inte absoluta, och verkar upplösta eller icke-existerande för andra delar av utredningsverksamheten, såsom när politiska narrativ används för att finna kausala förklaringar i akademiska forskningsresultat eller när förbundsmedlemmarnas talespersoner betraktas som en källa till tillförlitlig kunskap.

Avslutande diskussion

Utifrån dessa tre analytiska spår avslutar jag avhandlingen med att koppla analysen till två större frågor: dels vad sambandet mellan politik och kunskap är i LO-utredningarnas typ av politisk kunskapsproduktion, och dels vilken roll den politiska experten har.

I förhållande till den första frågan ifrågasätter jag rimligheten i att leta efter generella kausala samband mellan politik och kunskap inom politiska utredningar. Sådana typer av kausala samband ligger till grund både hos de som avfärdar politiska organisationers kunskapsarbete som ren politik och därmed ser den kunskap de producerar som en kausal effekt av organisationens politiska övertygelser, men också i förespråkandet av så kallad evidensbaserad policy, där man eftersträvar att apolitisk forskning ska ligga till grund för politiska beslut och därmed menar att politikens verktyg (om än inte dess riktning) ska följa forskningen. Ett genomgående tema genom avhandlingen är att politiska aktörer inom organisationen och utredarnas kunskapsproduktion

samverkar på sätt som delvis skapar gränser mellan politisk representation och kunskap, men samtidigt löser upp dessa gränser. Ett tydligt exempel på detta ur min analys är hur utredarna som kunskapsproducenter förhåller sig till talespersoner för medlemmarna för att skapa sig en bild av vad förbundsmedlemmarnas intressen är, men genom detta själva blir talespersoner för medlemmarna i hur de för fram kunskap från ett arbetarperspektiv. Om det inte går att göra en stabil och allmängiltig uppdelning mellan å ena sidan politik och å den andra kunskap går det inte heller att tala om kausalitet mellan de två, varken om kunskap som en följd av politik, eller om politik som följd av kunskap.

Den andra större frågan jag belyser utifrån analysen i avhandlingen handlar om förhållandet mellan expertis och demokratisk representation. I tidigare forskning presenteras dessa två storheter som att de står i ett spänt förhållande: expertis, i synnerhet vetenskaplig expertis, framställs som auktoritär i motsats till demokratin, som framställs som egalitär, samtidigt som vetenskap och expertis spelar en allt större roll i demokratiska samhällen. I min undersökning av LO:s utredningsarbete verkar inte samma spända förhållande mellan expertis och demokrati, eller i fallet LO kanske snarare kunskap och politik, finnas. LO-utredarna producerar kunskap utifrån sin samhällsvetenskapliga expertis utan att framstå som ett teknokratiskt hot mot den politiska representationen i organisationen, och de arbetar under ansvarsutkrävande från organisationens valda representanter utan att vara strikt politiskt styrda. Med det vill jag inte säga att LO:s utredningar utgör en allmän modell för hur förhållandet mellan expertis och demokrati bör se ut, utan snarare ett exempel på lokala lösningar till den typ av demokratiska problem som expertis kan föra med sig. För den som är intresserad av förhållandet mellan expertis och demokrati borde den typ av politisk kunskapsproduktion som LO-utredarna arbetar med därmed vara ett intressant område för vidare empirisk forskning.

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Appendix

The following are translations of the preparatory notes I did before two rounds of interviews around the mid-point of the data collection for this study. For most rounds I prepared something like the examples below, which also served as the outline of interview guides for the interviews, though I jotted down the relevant questions on paper right before the actual interview. I used the guides as reminders of what topics I wanted to know more about rather than reading the questions as written here word-for-word in the interviews. For a few interviews, the guides were just single words as topics, and I asked questions more loosely around those themes. Names mentioned in the text below are not research participants, but refer to participants in a public debate taking place around the time of the interviews.

Interview preparation February

About the tour

Practical questions: How many tour stops were you on? What did you do at the different districts?

What was the point of the tour? What's the best possible outcome? How do you know if it's successful or not? What stops were the best ones, and why?

Are the presentations at the tour similar to other things you do as researcher? If so, what other things? Do you reuse the same presentations in different contexts?

How did you write the presentation? How did your group decide what to include from the report?

More general questions

Do you get surprised by your research results? Do you have an example of when you were? (*Is this really a good question?*)

When do you know something, and what does it take to be convinced. I plan to ask about this in relation to specific examples of reports.

The relationship between policy and knowledge [in reports].

I'm also interviewing a researcher who was involved in an earlier Congress project, preceding the Equality Project. I want to know more about the process of that project, and how it differs from the Equality Project. I also want to know if there were others before it; they present the big projects as recurring, but the only one I know of from before this was in the 90s. On the face of it, this other project is quite similar to the Equality Project. It's based on 24 so-called basic reports. A difference, as I understand it, is that the results were reported to the Congress which then made policy decisions based on the report, while the Equality Project was decided on by the Executive Council before the Congress.

What I want to know:

Have there been other congressional reports since the 90s? Were there others before the one in the 90s?

How was the project run? Was the researcher involved from the beginning? Who else was involved, who wrote the reports (internal/external)?

What was the most difficult part of the project? How much of her workdays were spent on the project?

Preparation, May interviews

I'm doing another round of interviews with researchers in May. I've mentioned it to some of the people I talked to last time. I'm planning to ask about more general things than the Equality Project, and things which are closer to what I'm actually interested in. I'm thinking that I want to know more about the following:

Follow-up from earlier: Why are controversies relatively rare? Does LO and the employer orgs argue over factual things, in line with Sandro Scocco's public debate with Almega over whether the RUT tax deduction is self-financing or not? Are there examples? If they're as rare as I think they are now, how come?

What is quality in research? How do they decide if a text should be included in, for example, the Equality Project or not? (Loosely after one of the last

interviews were a researcher commented that it's an interesting question how they decide what to include.)

Are sources differently trustworthy? Which ones are the best ones to cite? Why? Maybe not that interesting since I think I already know the answer, but I've got to ask. I'll think about it.

Besides these questions, I have some specific persons I want to interview. I want to talk to a couple of ombudsmän to find out what their job actually is, and how it differs from the researchers. I want to talk to a person at the communications unit, and ask about their strategy in relation to Timbro. (There are other interesting questions to communications, open questions about their work would be good.) I might have some questions for the research directors too.

Maybe I should talk to Sandro Scocco and Almega too, but I'm not sure it would lead anywhere. Yet another thing to consider.

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POLITICAL EPISTEMOLOGY explores the relationship between politics and knowledge, with a study of researchers employed by the Swedish trade union organisation LO. Political organisations like LO routinely employ researchers to produce knowledge about the world they operate in, and to act as epistemic advocates for those the organisation speaks for. This thesis looks to the LO research offices to understand the knowledge produced there. It draws on interviews, participant observation, and document analysis to show how political representation interacts with the work of representing reality.

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