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# Policy Feedback and Public Opinion

The impact of Swedish welfare reforms on public attitudes to privatisation

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## Abstract

The concept policy feedback is the idea that policies themselves may be political forces. Instead of the traditional approach of analysing policy as the conclusion of a political process, policy feedback suggests that the relationship between policy and public opinion is reciprocal. Many authors have stressed the methodological challenges related to estimating policy feedback effects due to the risk of reverse causal pathways: how do we make causal inferences, when the policy itself is probably a function of public opinion? I study public attitude toward privatisation following two Swedish welfare reforms (*Friskolereformen* and *Lagen om Valfribetsystem*) that both resulted in an expansion of private service providers in the Swedish welfare sector. I exploit the variation in municipalities that did and did not adopt the two policies, together with the variation in timing of their adoption across municipalities. By applying a staggered difference-in-differences design, I isolate the causal effect of the welfare reforms on public preference for privatisation thereby avoiding the issue of reverse causality. This thesis provides a causal link between policy adoption and public opinion, suggesting that the policies themselves can be an important factor that shape public opinion on privatisation. The overall results support the *existence* of policy feedback effects, but do not indicate any recurring direction of such effects.

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## 1. Introduction

One of the fundamental concepts of democratic governance is *popular sovereignty*: the idea that the preferences of *the people* should be reflected in government policy (Campbell 2011b:272; Dahl 1989:311). Public policy has historically been treated as the outcome of political forces and peoples' preferences, yet the potential for policies to influence *new* politics has largely been ignored (Gusmano, Schlesinger, and Thomas 2002:731; Mettler and Soss 2004:55; Pierson 1992:595). In the last few decades, the nature of the relationship between public opinion and public policy has gathered attention under the concept of *policy feedback* – the idea that policies themselves may be political forces. Literature on policy feedback suggests that the relationship between policy and public opinion may be reciprocal rather than one-sided (Campbell 2011b:271–72). This literature argues that the relationship could also be reversed, so that, just as public opinion influences public policy, the policies themselves may likewise influence public opinion, and therefore restructure subsequent political processes (Campbell 2011b:271; Gusmano et al. 2002:731; Mettler and Soss 2004:60).

Why would policies affect public opinion? It is commonly believed that there are certain social and economic conditions that pre-determine people's general political preferences and opinions (Dalton 2014: 8; Zaller 1992: 22). Public opinion can therefore be reliably predicted by considering information about certain values and attributes such as age, education, religiosity, ideology, partisanship, race, economic status and gender (Dalton 2013:8; Kreitzer, Hamilton, and Tolbert 2014:796). When people make political decisions, they seldom have complete information about the issue, and instead rely on various methods and cognitive shortcuts to make decisions on most issues (Bendz and Oskarson 2020:5; Dalton 2013:33). People develop their opinions about events that are beyond their full comprehension by interpreting – through the lens of their individual attributes – information from authorities on the subject, such as politicians, higher-level government officials, journalists, and policy specialists (Zaller 1992:6). The information from these authorities helps people to form a mental picture of a given issue, whereas political values and other predispositions motivate some conclusions about the issue (ibid:6-13). Mass opinion is thus formed through a balance of people's awareness of an issue and their deeply rooted values and socioeconomic attributes (Dalton 2013:8; Zaller 1992:22).

There is substantial support for the assumption that policies can convey information and signals for the public to respond to. Some policies receive a large amount of media attention, making them highly visible to the public (Pacheco 2013:716). People are also likely to pay more attention to policies that directly affect them (Soss and Schram 2007:122–23). Policies can be directed toward specific target populations, for example by restricting participation in politics

(through disenfranchisement laws), they can distribute important resources (like education and wealth), and they can generate new bases of self-interest (ibid:114). Studies that search for and demonstrate the effect of public policies on mass opinion continue to produce new and exciting results, which move beyond textbook formulations of policy as the end of a sequence of processes. Nonetheless, whether policies affect public opinion, the direction of the feedback effect, and whether the effect varies depending on contextual characteristics, are all still contested questions (Flores and Barclay 2016:43; Larsen 2019:375).

In this thesis, I study public attitude to privatisation following two Swedish welfare reforms (*Friskolereformen* and *Lagen om Valfrihetsystem*) that both resulted in a noticeable increase in private service providers in the Swedish welfare sector. I study individual preferences for the reduction of the public sector and the increase of private service providers in areas relevant to each of the reforms (school system, healthcare, and eldercare). By exploring the question of whether the increase of private actors providing publicly financed services in Sweden caused a shift in public attitudes to privatisation, I empirically study and explain the causal relationship between the adoption of the policies and public preferences.

I have chosen Sweden as a most-likely case where the introduction of private providers in the welfare services may have caused a shift in public opinion on privatisation due to the size and scope of the public sector. The Swedish welfare state is largely encompassing (Bendz and Oskarsson 2020:7; Nilsson 2002:90). In the Swedish model, most of the welfare services are provided by the state (at the national, regional, or local levels) (Nilsson 2002:89), which results in a setting where most Swedish citizens have a close personal experience with many of the welfare services provided by the state (Bendz and Oskarsson 2020:7–8). For the last few decades, the issue of privatisation of welfare services has been at the centre the Swedish political agenda (Bendz and Oskarsson 2020:7). In particular, the two welfare reforms mentioned above resulted in some extraordinary changes to the structure of the Swedish welfare services that received a high amount of public attention. The two reforms also offer an opportunity to study two similar policies implemented relatively shortly after one another under comparable circumstances.

Overall, previous research has established the existence of policy feedback following various types of policies. A variety of methods have been used in this research. However, many authors have stressed that there are still some methodological challenges related to estimating causality (see for example Soss and Schram 2007:114; Barabas 2009:183; Campbell 2012:343-45; Kotsadam and Jakobsson 2011:103-04; Christenson and Glick 2015:882-83; Larsen 2019:389). The main issue originates from the reciprocity of the relationship. How do we make causal inferences, when the policy itself is probably a function of public opinion? Such reverse causality pathways constitute a

problem for panel methods, as the results from such analyses will inevitably be biased (Cunningham 2021).

The issue of reverse causality is a major methodological focus of this thesis. Since it was never mandatory for the municipalities to adopt either of the two welfare policies, we can ask whether the municipalities that adopted these policies did so in response to changes in public preferences for privatisation. My solution to the problem of reverse causality is to exploit the variation in municipalities that did and did not adopt the two policies, together with the variation in timing of their adoption across municipalities. I apply a difference-in-differences research design that allows me to estimate the causal effect of the policy implementations on public attitude to privatisation. The research design allows me to verify that the adaptation of the reforms was not a response to changes in public opinion, something that would have cast serious doubt on the validity of my research design and conclusions.

In this thesis I provide credible causal evidence of how privatisation policies impacted public attitudes towards privatisation. My results show that the two welfare reforms influenced general public opinion in multifarious ways, and that the policies were not simply a response to public opinion. The main contribution of this thesis is to offer a suggestion for a different approach to studying policy feedback effects than what has previously been applied by earlier studies. By using a staggered difference-in-differences design, I am able to isolate the causal effect of policy implementation on public opinion, an issue frequently expressed in the literature about policy feedback. The application of a staggered difference-in-differences design also allows me to study more long-term policy feedback effects than has been possible in previous research. A second contribution of this thesis is to add to the general understanding of public opinion about privatisation in Sweden. My thesis demonstrates that there is a causal link between policy adoption and public opinion, suggesting that the policies themselves can be an important factor in shaping public opinion about privatisation.

The thesis is structured as follows: following this introduction, the second chapter discusses the theoretical background and prior research that have generated the hypotheses of this project. The third chapter describes the general development of privatisation in Sweden, and details the two policies chosen for my study. The third chapter also describes the data used for this project. The fourth chapter discusses the research design and the question of how a natural experimental research design benefits the analysis of my hypotheses. The fifth chapter presents the overall results. Lastly, I conclude the thesis with a discussion of the overall results, and what these results imply for the current scientific debate and future research.

## 2. Theoretical Background and Previous Research

The existence of policy feedback effects has been explored on various kinds of outcomes in a variety of fields such as sociology, psychology, economy, and politics (Mettler and Soss 2004:57). Naturally, empirical studies on the subject interweave across the social sciences. The kinds of outcomes that have been studied include changes in public behaviour, new policies, incentives, distribution of resources, support, political goals, capabilities, and, of course, public opinion. The majority of previous studies on public opinion have either focused on individual preferences (Barabas 2009), or the collective preferences of entire countries (Soss and Schram 2007; Pacheco 2013:715). Much of the empirical research has concentrated on social welfare policy, where several studies have found evidence of a relationship mostly (but not exclusively) centred on redistributive or economic policies (Campbell 2012:336; Kreitzer et al. 2014:796; Pacheco 2013:715). Some other areas where researchers have found evidence that policy affects opinion include environmental policy, employer responsibility in healthcare, retirement and health savings accounts, smoking bans, and same-sex marriage policies (Kreitzer et al. 2014:796).

The direction of the policy feedback effect found in previous research varies. Some studies have found policies to have a positive effect on public opinion (Campbell 2011a; Flores and Barclay 2016; Hetling and McDermott 2008; Kreitzer et al. 2014; Mettler 2002), others have found the effect of policy implementation on public opinion to be short-lived, non-existent, or even negative (Campbell 2012; Gusmano et al. 2002; Kotsadam and Jakobsson 2011; Larsen 2019; Soss and Schram 2007). Even studies of the same policy have found evidence of different outcomes (Soss and Schram 2007). The relationship between public policy and public opinion certainly appears to be a variable one (Campbell 2011b:186; Larsen 2019:383). It is commonly believed that the relationship between policies and public opinion varies depending on the type of policy, the target population of the policy, the public's experience of the policies, and how the policies are realised (Soss 2004:291-93; Campbell 2011b:278; Gusmano et al. 2002:734). The mixed evidence of policy feedback effects could thus be a product of the context and policy, and/or the difficulties associated with empirically studying and measuring policy feedback effects.

In the following section I apply a wide body of theoretical and empirical research to explore how policies can shift opinion, the type of policies that tend to shape public opinion, and the type of people that tend to change their preferences following policy implementation. Based on previous research I hypothesise both positive and negative policy feedback effects of the welfare reforms on public attitudes to further privatisation. The two main hypotheses are followed by three supplementary hypotheses where I explore the idea that the policy feedback effect following the two welfare reforms may be shaped by the design of the policies and vary across populations.

Prior research has theorised four potential shifts in mass opinion following policy implementation: positive, negative, positive and negative, and no shift at all. First, the Legitimacy Model is the idea that the introduction of policies adds acceptability to the issue by making the issue more familiar (Flores and Barclay 2016:43). According to the Legitimacy Model, public opinion about a policy issue will move in the direction of the policy, and individuals will support additional implementations of similar policies (Christenson and Glick 2015:884; Pacheco 2013:730). According to this model, a preference for further privatisation will likely increase following an adoption of the Swedish welfare policies in their municipality.

The Backlash Model reaches the opposite conclusion. It argues that policy development will be met with a negative change in attitudes toward the issue at hand (Flores and Barclay 2016:45). The idea is that the features of the policy let individuals acquire perceptions of their own roles in the community and their status in relation to other citizens. Likewise, the features of the policy features allow people to acquire new perceptions of others (Mettler and Soss 2004:55). The Backlash Model is notable in research on public preferences for the policies themselves. A “thermostatic” pattern has been identified in previous research on American public preferences for spending on defence following policy implementation. Like a thermostat, the public’s preferences for a spending went up when spending went down, and down when the spending on defence went up (Wlezien 1995). According to the Backlash/Thermostatic Model, general preferences for further privatisation will likely decrease following an adoption of the welfare policies discussed in this thesis.

Third, the Polarisation Model proposes that the public debates leading up to a new policy persists after its implementation, ultimately intensifying support or opposition to the issue (Flores and Barclay 2016: 46). This model therefore suggests that policies not only intensify support but also fuel countermobilizations, resulting in conflict and polarisation of public opinion (Pierson 1993:600; Zaller 1992:100–13). Such a shift is unlikely to show in the study of general preferences because a change in preferences in both directions will neutralise the effect.

Finally, the Consensus Model is the null hypothesis of this paper: that the relationship is a one-way street and policies have no effect on public opinion. The Consensus Model embodies the traditional view that policies simply reflect public opinion and they do not affect public attitudes themselves (Flores and Barclay 2016:46; Barabas 2009:182).

A fundamental component for the existence of policy feedback is the visibility of the policy. When people are more exposed to an issue, they are more likely to comprehend and receive political messages concerning that issue (Zaller 1992:42). Likewise, when people spend more time with an issue, they are more likely to change their attitudes toward it because they are elaborating their

understanding of the issue, which leads them to reconsider their previous opinions (Pachecho 2013:716; Flores and Barclay 2016:44). It is important to recognise that not all policies are equally visible to the public (Gusmano et al. 2002:735). Because of their nature, certain policies will be more visible, and some people will be more proximate to certain types of policies. People often pay attention to political events that are directly relevant to them (Flores and Barclay 2016:44). School policies will be more visible and proximate for parents, for example, and pension policies are another example where the target population is evident (Campbell 2011a:967). Other policies are more obscure and are therefore less likely to affect public attitudes. For example, individuals receiving benefits that do not come as a visible service or cash payment (such as savings on their taxes) are less likely to view governmental spending as helpful compared to beneficiaries of direct programmes (Campbell 2011b:279). Although essential, the visibility of the policy has, in some cases, been shown to not be sufficient for policy feedbacks to occur.

In 1996, the United States Congress introduced new social legislation<sup>1</sup> that pledged to “end welfare as we know it” (Soss and Schram 2007:112). The new legislation received a substantial amount of attention and was a focus of a large part of Clinton’s 1992 presidential campaign (ibid:112). It has been suggested that this welfare reform had a positive effect on individual preferences (Shaw Shapiro cited in Hetling and McDermott 2008:475), but equally that it had no impact on general preferences (Soss and Schram 2007). Evidence suggests that the reform caused an increase in individual preferences for poverty spending for people that had a direct experience of the policy, but not for people with little exposure to it (Hetling and McDermott 2008:476). It has been argued that the absence of a policy feedback effect at the general level was the result of the distance between the welfare policy and the general public. Despite the policy being highly visible, most people did not have any direct experience of it (Soss and Schram 2007:121-22).

This proximity-visibility theory has been developed and tested on other policies and in other contexts and countries (see Hetling and McDermott 2008; Hedegaard 2014; Pachecho; Mettler 2002; Gusmano et al. 2002; Kotsdam and Jakobsson 2011). Such studies have provided evidence that further supports the idea that the policy feedback effect “will be highly contingent on [the policy’s] visibility and proximity for mass publics” (Soss and Schram 2007:126). In Denmark, for example, people’s preferences for spending on social benefits has been shown to be influenced by their proximity to recipients of selective policies (Hedegaard 2014). People that either received a social benefit themselves – or had a close family member (or friend) who received a social benefit – had a more positive attitude toward spending on that policy than those who did not (ibid:377-80).

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<sup>1</sup> The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA).

It has been argued that the attributes of the people exposed to the policy is also relevant to the feedback effect. Returning to how public opinion is formed, I depart from the theoretical framework that “attitude change may be understood as a two-step process involving, first, reception of persuasive communications and, second, acceptance or nonacceptance of their contents.” (Zaller 1992:148).

First, a policy has the potential to provide the public with information if it is visible and proximate, but people will only receive such information if they are aware of it. Levels of political awareness can be placed on a spectrum. On the one side of the spectrum is a small minority of citizens who are well informed about politics and pay great attention to it. On the other side of the spectrum, we find people who pay little to no attention to current information about politics. Most people are not part of either category, but fall somewhere between them, paying moderate attention to politics (ibid: 16). A person’s location on the political awareness spectrum influences their receptiveness to the information delivered by the policies. Second, people use cognitive shortcuts to make decisions on the issues presented to them. As such, the possibility for policy feedback effect is thus not just shaped by people being exposed to the policy, for depending on their political pre-dispositions, individuals will either accept or reject the persuasive communications (Zaller 1992:20). People are more (or less) likely to resist the messages conveyed by policies if the information is consistent (or inconsistent) with their political predisposition (Zaller 1992:44; Kreitzer et al. 2014:801–02). As Zaller (1992) argued:

Highly aware persons are heavily exposed to the persuasive appeals of the campaign, but owing to the strength of their pre-existing attitudes, they are difficult to influence. At the same time, persons who pay little attention to politics are also relatively stable – not because they have strong partisan commitments, but because they pay so little attention to politics that they rarely encounter communications that can change their preferences. Finally, moderately aware people pay enough attention to politics to be exposed to partisan communications but are not sufficiently committed to their initial preferences to be immune to conversion. Hence this group tends to be the most volatile of the three. (Zaller 1992:218)

Prior research has indicated that political predispositions can make people more receptive to policy information within the parameters of those predispositions. For example, following the Iowa State Supreme Court’s decision from 2009 to legalise same-sex marriage, people who – based on commonly established predictors of opinion on same-sex marriage such as demographic factors and partisanship – were predicted to support same-sex marriage, but previously did not, were more

likely to accept the legitimacy of the policy implementation, and so change their opinion to align with the Court's decision (Kreitzer et al. 2014:799–803).

In order to identify predispositions that could be of relevance for my project, I will now turn to previous research on predictors of attitudes toward privatisation in Sweden. In describing data about Swedish public opinion around privatisation in 2000 on the basis of party identification, Lennart Nilsson (2002) found a strikingly clear pattern of Swedish citizens' attitudes toward privatisation moving from negative to positive in accordance with the left-right political spectrum (Nilsson 2002:108–09). Other research has also suggested that people in Sweden respond negatively to policy-specific information about privatisation, an effect that – although modest – was most visible amongst participants identifying as centre-left (Bendz and Oskarson 2020:111–15). Based on the theoretical framework of opinion formation and empirical details about the general pattern of Swedish public opinion on privatisation, it seems likely that ideological orientation and political awareness will affect the degree of acceptance of (or resistance to) further privatisation of the Swedish welfare services.

Finally, prior research indicates that the relationship between policy implementation and public attitudes could depend on people's trust in politicians. The argument is commonly found in research about the internalization of legal norms, as well as in legal philosophy (Kotsadam and Jakobsson 2011:105). Yet the empirical evidence that trust in politicians influences the policy feedback effect is mixed. In studying the Supreme Court's ability to change opinion, research suggests that people who already viewed the Court positively were more likely to shift their attitudes in accordance with the Supreme Court's decision (Hoekstra 1995). This hypothesis has also been tested on a Norwegian law from 2009 that criminalized the purchase of sexual services, but the evidence suggested that the law did not have a larger influence on people with higher political trust compared to the general population (Kotsadam and Jakobsson 2011:110).

Based on the theoretical assumptions and empirical evidence from previous research described above, I construct five hypotheses:

First, my two main hypotheses concern the general relationship between policy implementation and public opinion. Evidence from previous research suggests that we can expect the two welfare reforms to have a positive or negative effect on public preferences following their implementation. It seems that preferences will either move in the direction of the policies, a potential result of the policy adding acceptability to the issue by making the issue more familiar. Such process would mean that public preferences for further privatisation increase following an adoption of the policies by the municipality. This leads me to my first main hypothesis:

*H1: Policy implementation causes an increase in public preferences for privatisation.*

On the other hand, the policy implementation could be met with a negative change in attitude to further privatisation. Based on evidence in prior research identifying a thermostatic pattern following changes in public spending, I construct a second competing main hypothesis concerning the direction of the shift in public opinion:

*H2: Policy implementation causes a decrease in public preferences for privatisation.*

Second, I construct three supplementary hypotheses that concern the importance of the type of policy and the type of people exposed to the policy. The proximity-visibility theory and the results showing that some policies that had no effect on preferences in general, but did have an effect on people proximate to highly visible policies, leads me to expect that the effect may depend on people's exposure to the policy. The two welfare reforms introduced private actors into specific sectors of the welfare services, providing clear 'target populations' which are more exposed to the policies than the average person. This leads me to develop a third hypothesis:

*H3: The effect of policy implementation depends on people's exposure to the policy.*

Based on the theoretical framework and results arguing that public opinion is formed in a two-step process involving reception of policy information and ideology to interpret that information, I expect the effect to depend on people's moderate political awareness and ideological disposition. This leads to my fourth hypothesis:

*H4: People's level of political awareness and ideology influences the degree of change in public preferences for privatisation following policy implementation.*

Finally, based on the theoretical arguments found in legal philosophy and the mixed empirical evidence about the impact of high levels of trust in politicians on the effect of policies, I construct my final hypothesis:

*H5: People with a higher level of trust in politicians are more inclined to shift their attitudes in accordance with the policy following implementation.*

### 3. Institutional Details and Data

The contemporary Swedish welfare system has long been one of the most universal and comprehensive public social services (Blomqvist 2004:139–42). The system was largely developed

after the Second World War on a principle of egalitarianism (Blomqvist and Palme 2020:116). In relation to the rest of the world, Sweden devotes more public resources on various social services than most other countries (Blomqvist 2004:139). More importantly, the contemporary Swedish welfare system has institutionalized the values of universalism and social egalitarianism (Esping-Anderson 1990:26).

Until relatively recently, public services produced by private actors were a rarity in Sweden (Ahlbäck Öberg 2008:182). Sweden long “stood out as the country where discouragement of, and even hostility to, private alternatives within the school, health-care and social services sectors was most pronounced” (Blomqvist 2004:140). This was a result of historical circumstances and deliberate political choices made by the reformist Social Democratic Party which governed the country without interruption from 1932 to 1976 (ibid:143). Following an economic crisis and increasing pressure and political criticism in the 1980s and early 1990s, a series of welfare reforms inspired by neoliberal ideas were introduced (Blomqvist and Palme 2020:116). The number of private actors in the Swedish welfare sector has continuously increased ever since (Blomqvist 2008:252).

In 1983, a general legal framework was implemented that approved public financial support for privately operated schools (Angelov and Edmark 2016:25). Initially the regulations for the financial support were highly restrictive, and the amount of financial support for private schools was considerably lower than what was granted to public schools (ibid:26). It was not until *Friskolereformen* (“The Private School Reform”) in 1992 that the structure of the Swedish welfare system started to change dramatically.

The idea behind the reform was to increase a freedom of choice and competition (ibid:19). With *Friskolereformen*, private schools were entitled at least 85% of the financial support that public schools were granted for their students (ibid:27). The regulations were not nearly as restrictive as prior to the reform, resulting in a rapid expansion of the private part of the school sector (Angelov and Edmark 2016:25; Blomqvist 2004:147-148). *Friskolereformen* was considered widely controversial at the time of its implementation in 1992, and has continued to be heavily debated since, with statements such as ‘the Swedish school system is in crisis’ frequently being made by Swedish politicians in the last decades (Angelov and Edmark 2016:17; Ringarp 2017:5). At the centre of the school debate is *Friskolereformen*, the intentions behind the reform as well as its consequences (Ringarp 2017:5).

The data on Swedish private schools was kindly received from Abiel Sebhatu (2021), and is currently unpublished. To measure the effect of *Friskolereformen*, ‘the treatment’ happens when a for-profit private school is introduced in a municipality that previously did not offer one. That a

municipality ‘implemented Friskolereformen’ thus means that the municipality went from having no for-profit school to having a registered for-profit school. Figure 1 below reports the number of municipalities with a for-profit private school reported in the data starting from two years prior to the introduction of the reform in 1992 until 2018. The figure illustrates a steady increase of municipalities with at least one for-profit private school until around 2010.

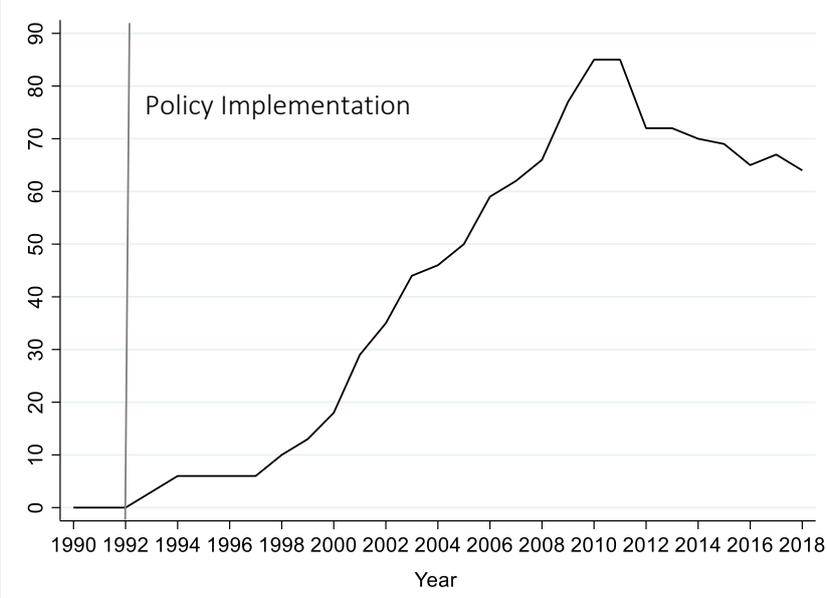


FIGURE 1 – DEVELOPMENT OVER TIME OF MUNICIPALITIES WITH FOR-PROFIT PRIVATE SCHOOLS

The so called “choice revolution” in the welfare services has also been unfolding in other sectors of the Swedish welfare system. In 2009 *Lagen om Valfrihetsystem* (“The Act on Choice Systems”, hereafter LOV) was implemented, creating a legal framework for provider choice in social services (Moberg, Blomqvist, and Winblad 2016:285). Following LOV, the Swedish social services have gone from being almost exclusively provided by the state, or local governments, to the mixture of private and public providers that it is today (ibid: 285). The 21 Swedish regional councils have to adopt LOV in primary healthcare, but it is up to each of the 290 municipalities to individually decide on whether to implement LOV in their social services (Vårdföretagarna 2019:8).

The data on LOV is collected by Sveriges Kommuner och Regioner (SKR) and can be found on their website (SKR 2021). ‘The treatment’ happens when a choice system is introduced in a municipality that previously did not offer one. Before the introduction of LOV, about 40 of Sweden’s municipalities offered some kind of choice system, and by 2018 the number had increased to 160 municipalities (Swedish Government Official Report 2014:11; Vårdföretagarna 2019:8). 114 municipalities have never adopted LOV, and 16 municipalities have decided to cancel LOV (SKR

2021). Sweden has a total of 290 municipalities with a large variation in population size; in 2018 75 percent of the Swedish population lived in a municipality that had adopted LOV (Vårdföretagarna 2019:8). As seen in Figure 2 below, the number of municipalities that have adopted LOV has been stable since 2014 (ibid:8). New municipalities have adopted the policy, and some have chosen to terminate their choice systems, the general trend being that new choice systems are implemented in municipalities that already have a choice system in another welfare sector.

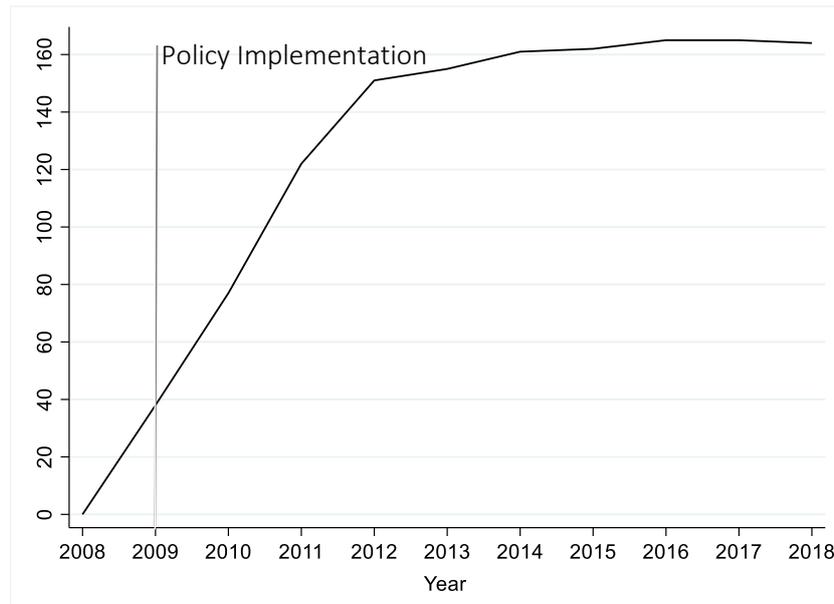


FIGURE 2 – DEVELOPMENT OVER TIME OF MUNICIPALITIES WITH LOV

Unfortunately, the data from SKR does not differentiate between which sectors LOV was adopted in. It would have benefitted the accuracy of my research design had I been able to only include municipalities that adopted LOV, specifically in the eldercare sector. Instead, all categories of LOV were treated as one and the same. The absence of more precise data, although unfortunate, does not present a crucial limitation for my research, since the number one welfare service where LOV is in use is in home-care services and residential-care services (Swedish Government Official Report 2014:15; Vårdföretagarna 2019:9). Out of the 160 municipalities that had adopted LOV in 2018, 158 municipalities offered a choice system in their home-care services (Vårdföretagarna 2019:9). This is followed by daily activities (“*meaningful occupation for people with mental illness or intellectual disabilities*”) offered by 32 municipalities, and residential care services (“*särskilt boende*”) that was offered by 21 municipalities (ibid:9–10).

To measure preferences for privatisation, I use the National SOM Survey Cumulative Dataset 1986-2018. The SOM Survey is a postal survey<sup>2</sup> conducted yearly by the SOM Institute at University of Gothenburg. The survey asks questions about behaviour and attitudes on the themes Society, Opinion, and Mass Media to a systematic probability sample of the Swedish population (Falk, Sandelin, and Marcus 2021:2; Markstedt 2014:3).

The demographic composition of the survey respondents (regarding gender, age, geographic location, and education) is a fairly accurate representation of the Swedish population as a whole, even if women are slightly overrepresented, and men slightly underrepresented from around 2003 and onwards (Markstedt 2014:13). The difference in men and women could have presented a problem for my project had prior research indicated that, for example, women tend to be more favourable to an issue following policy implementation than men. The over- and underrepresentation in the survey respondents could skew the results of my study (enhancing a positive feedback effect). However, as previous research does not indicate that there would be any systematic differences in men and women's reaction to policy implementation, the slight overrepresentation of women is unlikely to have any larger impact on my results.

The age span of the sample has varied somewhat over the years used for my project. An interval of 15–80-year-olds was used for 1992–1999, 15–85-year-olds for 2000–2008, and 16–85-year-olds for 2009 and onwards (ibid:3). The difference in age span is so small that it is unlikely to affect the results of this study. As for age representation, there is a systematic difference in the age group represented in the survey. Starting from the mid-1990s, teenagers and young adults are underrepresented, older people are overrepresented, whereas the age group 35–49-year-olds follows the composition of the population (ibid:13). Despite overrepresentation, the measurements accuracy of questions on attitude and political suggestions is still high, but the accuracy of questions on issues with large generational differences (like reading the morning paper) has gone down somewhat (Falk et al. 2021:21–25).

The consequences of the skewed representation for the accuracy of SOM are currently being studied, and more detailed studies are still to come (ibid: 21). In general, the accuracy of the SOM survey's measurements has been shown to remain high despite the decreasing response rates (Falk et al. 2021:21; Markstedt 2014:29). However, I have taken extra consideration in the construction of my news consumption variable as some of the questions on news consumption have been shown to be less representative of the Swedish population than others. This consideration is further discussed in the operationalisation section below. In general, I rely on prior research that have

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<sup>2</sup> The SOM data collection has been a mix between postal and internet surveys since 2012 (Falk et al. 2021).

shown the differences in demographic composition (so far) to have very little impact (“a fraction of a percent”, My translation) on the generalisability of the data (Markstedt 2014:29).

The dependent variables used for my analysis were all constructed from a main survey question phrased in the following way:

“Below is a number of suggestions that have appeared in the political debate. What is your opinion on each and every one of the suggestions?” (SOM-institutet, Göteborgs universitet. 2019. Super-Riks-SOM 1986-2018 v2019.1., My translation).

I constructed a variable to capture a general attitude to privatisation (i), and two variables for areas specifically affected by LOV (ii) healthcare and (iii) eldercare, and two variables for Friskolereformen. Question (iv) was asked until 1996, after which point question (v) took over. I have therefore combined (iv) and (v) into one dependent variable to measure attitudes to private schools. The questions chosen for my analysis were the following:

- (i) “What is your opinion on reducing the public sector?” (for an overall measurement of public opinion on privatisation of public services).
- (ii) “What is your opinion on increasing the privately provided parts of the healthcare” (for public opinion on privatisation in an area relevant for LOV)
- (iii) “What is your opinion on increasing the privately provided parts of the eldercare” (for public opinion on privatisation in a second area relevant for LOV)
- (iv) “What is your opinion on increasing the number of private schools” (for public opinion on privatisation in an area relevant for Friskolereformen before 1997)
- (v) “What is your opinion on investing in more private schools” (for public opinion on privatisation in area relevant for Friskolereformen from 1997 onwards)

For all questions, the following options were given:

1. Very bad suggestion, 2. Fairly bad suggestion, 3. Neither bad nor good suggestion, 4. Fairly good suggestion, and 5. Very good suggestion.

This provides me with four dependent variables all on a scale of 1-5, where a 1 equals the most negative attitude to privatisation, and a 5 equals the most positive attitude to further privatisation.<sup>3</sup>

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<sup>3</sup> The coding in the original dataset was reversed but for the purpose of visualising my results in a more comprehensive way, I changed the answers so that a high number equals a positive attitude towards privatisation, and a low number equals a negative attitude privatisation.

### *Sample Selection*

For the third hypothesis, I operationalised people's exposure to the two policies by dividing my sample into the following subsets:

- (i) senior citizens (pensioners) and people working for the municipality for LOV, and
- (ii) parents with children of school age for Friskolereformen.

Since private schools were introduced for both compulsory schooling and gymnasium (high school), I created my parent sample in accordance with the general dichotomous variable "regularly share household with one or more children". The people that answered yes to this question then made up the sample selection for exposure to Friskolereformen (parents).

The choice of demographic characteristics used to operationalise exposure for LOV was not as obvious as for Friskolereformen. A system of freedom of choice can be applied in various areas of the welfare sector like healthcare, eldercare, social services, labour market services (Swedish Government Official Report 2014:11–4). To make my decision, I therefore considered the areas where LOV is most commonly found: healthcare and eldercare.

I chose pensioners as a subgroup most proximate to LOV because pensioners are more likely to be exposed to eldercare through direct experience, and/or pay attention to issues around eldercare because of planning for a near future. As for municipality employees, the group was chosen because people working for the municipality are more likely to be aware of any changes in the providers of work opportunities and the structure of their work sector. With municipality employees I also extend the potential exposure beyond health- and eldercare. Ideally, I would have used a variable like the SOM survey question asking about personal experience with healthcare that was included for some years of the survey (Bendz 2015:315). But because of the absence of that survey question from most years used in this study, I have settled with pensioners and municipality employees as my sub-sample group for people that are likely to be most exposed to LOV.

For the fourth hypothesis, I wanted to create my sub-sample group based on people's level of political awareness and ideology. One reliable method to conceptualise and measure political awareness is through simple tests of asking survey questions on neutral information about politics (Zaller 1992:21–2). Such measurement is unfortunately unavailable to me for this project (as no such questions are included in SOM), instead I have chosen to operationalise political awareness with both a question capturing political interest and a question capturing the respondent's level of news consumption. This is another common strategy found in literature on measuring political awareness (ibid:21). Other commonly used variables include political participation and level of education, but I will limit my choice to political interest and news consumption.

News consumption is a natural choice for capturing people's awareness of politics. Prior research suggest that political awareness refers both "to the extent an individual pays attention to politics *and* understands what he or she has encountered" (ibid:21). Thus, news consumption alone may not be an accurate measurement of political awareness, which is why I have chosen to include political interest as well.

I have used the following variables to measure political awareness:

(i) "How interested are you in politics in general?"

1. Very interested, 2. Fairly interested, 3. Not very interested, 4. Not interested at all.

The subsample of people with "moderate political interest" was created from this variable and is made up from survey respondents that responded that they are '2. Fairly interested' or '3. Not very interested' in politics in general.

(ii) "News consumption", is a generated mean of all 15 variables included in SOM under the category 'News'. Asking the respondents about how often they consume different TV and radio news outlets.

1. Daily, 2. 5-6 days/week, 3. 3-4 days/week, 4. 1-2 days/week, 5. More rarely, 6. Never

There is a set of variables asking about news consumption of morning newspapers but the answers to these questions have been shown to less accurately represent the Swedish population as a whole due to increasing generational differences in survey respondents (Markstedt 2014: 29). Therefore, I have chosen to use only the question asking about consumption of news from TV and radio in the creation of my news consumption variable. I created my sample of people with "moderate news consumption" from the respondents that responded that their average news consumption from TV and Radio was between 4 to 1 days per week.

For my subsample of political orientation, self-reported ideology is conveniently captured by the following survey question:

(iii) "Sometimes people talk of political opinion as being placed on a left-right scale. Where would you place yourself on such scale?" (SOM-institutet, Göteborgs universitet. My translation).

The options being:

1. Clearly to the left, 2. Somewhat to the left, 3. Neither left nor right, 4. Somewhat to the right, 5. Clearly to the right.

This subjective left-right placement captures the question of ideology. To avoid creating a sample that is too small in size, I combined the two answers ‘1. Clearly to the left’ with ‘2. Somewhat to the left’ into one sample for respondents identifying to the left, and the same for the sample of respondents identifying to the right.

TABLE 1 – SAMPLE MEANS

Dependent Variables	Obs.	Mean	Std. Dev.
<i>Full Sample</i>			
Reduce the public sector	87 809	2.78	1.24
Increase the privately provided parts of the healthcare system	69 473	2.69	1.25
Increase the privately provided parts of the eldercare system	46 085	2.5	1.21
Increase the privately provided parts of the school system	46 770	2.7	1.15
<i>Respondents Identifying to the Left</i>			
Reduce the public sector	28 909	2.14	1.09
Increase the privately provided parts of the healthcare system	22 591	1.98	1.03
Increase the privately provided parts of the eldercare system	15 111	1.9	1.02
Increase the privately provided parts of the school system	15 256	2.16	1.05
<i>Respondents Identifying to the Right</i>			
Reduce the public sector	30 626	3.40	1.16
Increase the privately provided parts of the healthcare system	23 930	3.38	1.13
Increase the privately provided parts of the eldercare system	15 465	3.12	1.14
Increase the privately provided parts of the school system	16 041	3.2	1.09
<i>Differences between Left and Right</i>			
Reduce the public sector	1 717	1.24	
Increase the privately provided parts of the healthcare system	1 339	1.4	
Increase the privately provided parts of the eldercare system	354	1.22	
Increase the privately provided parts of the school system	785	1.04	

Table 1 describes the sample means for the full sample and divided by ideology. The key conclusion from the description in Table 1 is that left-identifying respondents have a mean response rate around 2 for all outcome variables (with a standard deviation around 1), meaning that the room for change in a negative direction is quite limited. Respondents identifying on the right on the other hand have a mean at around 3 (with a standard deviation around 1), meaning that there is room to change in both directions on the scale.

The final hypothesis regards the respondents’ trust in politicians. Again, I have chosen two variables to operationalise this concept:

- (iv) trust for the municipality executive board.
- (v) trust in the government.

The two variables are measured with the question: “what level of trust do you have in the way these groups and institutions do their job?”

1. Very high trust, 2. Fairly high trust, 3. Neither high nor low trust, 4. Fairly low trust, 5. Very low trust.

The subsample of people with a “higher level of trust in politicians” was created from this question, and is made up from survey respondents who responded that they have a level of trust in the municipality executive board or the government that is ‘1. Very high trust’. By creating sub-samples for both the local and national level, I can check if there is a potential difference in outcomes depending on the level of authority that the respondent’s high trust is placed in.

#### 4. Research Design and Method

Prior research has expressed many difficulties associated with estimating the causal effects of policies on various outcomes. That the Swedish welfare policies probably are a function of public opinion on privatisation presents a major problem for making causal inferences. The issue of studying causality – specifically how to deal with reverse causality – is therefore a key component of the research design of this thesis. To substantiate the importance of my research design in answering this question, the issue of studying causality requires some closer inspection. I begin this chapter by connecting the issue of reverse causality to the theoretical framework outlined above (2. *Theoretical background and Previous Research*), followed by a discussion of some of the concerns that this issue presents for the Swedish case. I then discuss experimental research designs as a solution to the issue of reverse causality, followed by some examples of limitations expressed in previous experimental research on the subject. Finally, I present my proposed solution, and discuss the method of choice for this project.

Regardless of whether the potential shift in public opinion is theorised to be positive, negative, or negative and positive, the fundamental idea expressed in literature about policy feedback is the same: the discussions and debates leading up to the implementation of a policy will continue to feedback into public preferences long after the decision on the policy has been made. The democratic process does not stop once the policy is implemented, rather, people will shift their preferences about the policy issues as a response to their implementation. Studying such feedback effects is a difficult task because the policies are not exogenous to public preferences, i.e. the policies are not “coming from the outside”, but could be a result of public preferences themselves (Huntington-Klein 2021:145). The issue is illustrated in Figure 3 below, where the two continuous

arrow lines between policy implementation and public opinion demonstrate the problem just described – the problem of reverse causality.

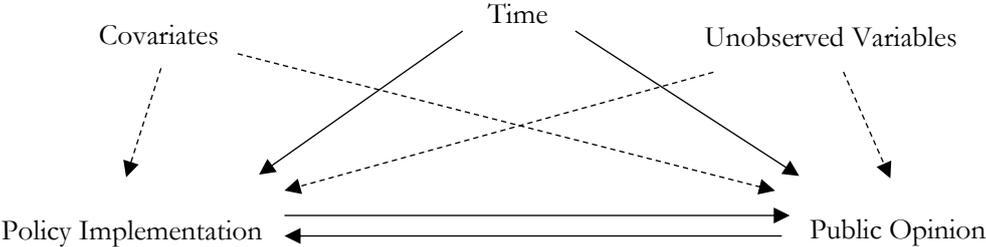


FIGURE 3. DIRECTED ACYCLIC GRAPH (DAG) OF POSSIBLE OPEN BACKDOORS

Notes: The continuous lines represent observable confounders and the dashed lines represent unobservable confounders.

Figure 3 also illustrates additional difficulties associated with estimating the average treatment effect of the two welfare policies on public opinion. First is that we cannot simply compare public opinion before the policy implementation to public opinion after the implementation. A key issue with such a comparison is that there is no guarantee that the changes observed following a policy implementation were actually caused by the policy itself. The changes could have been caused by some other unobserved variable, or the policy may simply reflect the general trend of public opinion on the matter (time). What if, instead, we compare public opinion in municipalities that did adopt the policy to public opinion in the ones that did not? But even here issues remain, because we have not controlled for variables that potentially influence the studied relationship. Often comparisons like these will lead us to observe correlations that have nothing to do with a causal relationship – correlations simply do not reflect a causal relationship (Cunningham 2021).

TABLE 2 – COMPARISON OF MEANS PER POLICY

Dependent Variables	Obs.	Yes		Obs.	No		Difference	
		Mean	Std. Dev.		Mean	Std. Dev.	Mean	Std. Err.
<i>Implemented LOV</i>								
Reduce the public sector	19,805	2.68	1.16	68,004	2.81	1.26	0.13	0.01
Increase the privately provided parts of the healthcare system	13,939	2.56	1.89	55,534	2.72	1.26	0.17	0.01
Increase the privately provided parts of the eldercare system	7,017	2.55	1.19	39,068	2.49	1.21	-0.06	0.06
<i>Implemented Friskolereformen</i>								
Reduce the public sector	43,037	2.73	1.2	44,772	2.83	1.28	0.1	0.008
Increase the privately provided parts of the school system	20,659	2.7	1.14	26,111	2.69	1.16	-0.13	0.01

Table 2 above describes the correlation between public preferences and policy implementation. The differences in average preferences for privatisation in municipalities with and without each of the reforms are statistically significant with a 95% confidence interval for outcomes except preferences for privatisation of the schoolsystem. The correlation between the welfare policies and average preferences for further privatisation indicate a possible reverse causal relationship.

To know that a policy caused a change in preferences the comparison needs to be made under *ceteris paribus* conditions, or *other things equal* (Angrist and Pischke 2014:4). When studying causality, the aim is to make a comparison between the actual outcome and the potential outcome, i.e., the outcome that would have happened, had the policy not been implemented. The most powerful framework for evaluating causal effects under *ceteris paribus* conditions is through random assignment of the treatment (Angrist and Pischke 2014:1). Through random assignment, we can reveal the outcome for the treated group, had they not been treated. Randomisation is a key element for isolating causal effects, as it ensures all possible variables are balanced across the treatment group and the comparison group, and so eliminates selection bias (Cunningham 2021; Angrist and Pischke 2014:12–16).

To estimate the causal effect of policy implementation on public opinion through the framework of randomisation, the ideal would be to actively participate in the collection process of the data with a randomised experiment (Cunningham 2021). The main benefit of experimental data is the control of the assignment of the treatment. For example, a policy could be randomly implemented in some municipalities and not in others. Such comparison would ensure *ceteris paribus* and the measured differences in outcomes between the two groups would thus be the effect of the treatment (the policy). Naturally, such an experiment is unavailable for this project, for the policies were not randomly implemented by the municipalities.

A feasible alternative to such a traditional experiment is a survey experiment. Various studies on policy feedback have used survey experiments to achieve random assignment of the treatment (see Mondak 1994; Hoekstra 1995; Clawson, Kegler, and Waltenburg 2001; Bartels and Mutz 2009; Bendz and Oskarson 2020). Conducting a survey experiment is an effective method to estimate causal effects in a controlled environment. This is because the researcher can be in control and randomly assign a treatment (like policy-specific information) to the survey respondents to measure the respondents' answers against a control group that did not receive the treatment. In doing so, any confounding variables (that could otherwise lead to mistaken inferences) are taken care of by the randomised assignment of the treatment to the sample (Esaïsson 2017:338–39). Randomisation ensures all possible confounding variables are balanced across the treatment group

and the control group, and so eliminates the risk of drawing mistaken conclusions about the treatment effect (Angrist and Pischke 2014:12–16).

However, survey experiments are somewhat limited when it comes to studying policy feedback effects on preferences, as the option is to either provide the respondents with information on made up policies, or risk that some respondents might already know about the policy in question prior to the survey experiment. In such cases, a random assignment of treatment is not guaranteed. This issue was recognised in a 2020 survey experiment of the impact of information about privatisation policies on attitudes and policy preferences for privatisation in Sweden (Bendz and Oskarson 2020:2). To isolate the effect of information of policies, the respondents in the treatment group were provided with information about actual levels of privatisation within “primary school, hospital care, retirement homes for the elder and homes for the disabled” (ibid:10). The respondents in the control group were not provided with any information and were simply asked about their preferences about the further increase or decrease of private service providers (ibid: 11). However, because of the prominence of privatisation policies and the Swedish population’s proximity to the welfare sector (the majority of Swedes have frequent contact with the publicly provided welfare services), the respondents might have already known the information about the actual level of privatisation (the treatment) before the treatment was assigned (ibid:17). The implication is added uncertainty to the causal claims and a risk of biased results.

Regardless of these limitations, the Swedish survey experiment provides a very useful starting point for my analysis, as it examines a very similar research question in a controlled environment. I developed my expectations for a lot of the potential results, largely in response to the evidence suggested by the survey experiment. Altogether, the study provides a great insight into what to expect when moving from the “lab” into the real world. Unfortunately, the method usually comes with the additional limitation of uncertainty on the extent to which the results found in the artificial environment can be generalised to the population as a whole (Esaiasson et al. 2017:339). Looking at previous research on policy feedback effects that have used survey experiments, “there is a noteworthy results gap between experimental and observational studies” (Christenson and Glick 2015:883). Another issue frequently found in the literature is how to make the move into the real world and continue to make valid causal inferences, when the assignment of the treatment is no longer in our control. The limitations and observed results gap emphasize the importance of continuing to approach these questions with other research designs and methods.

Moving from experimental data, the type of data I have decided to use is observational data, which is data collected without any experimental manipulation (Cunningham 2021). To make causal inferences using observational data, I have to rely on other aspects than the collection

process to ensure *ceteris paribus*. One way of achieving a balance of potential confounders between the treatment and control group is with the use of a natural (or quasi-) experimental research design. A natural experiment exploits situations where the process of treatment assignment itself resembles a random or an “as-if random” process (Dunning 2012:11). It is “a ‘design-based’ method of research — one in which control over confounding variables comes primarily from research-design choices, rather than *ex post* adjustment using parametric statistical models” (Dunning 2012:4). The toolbox of methods available for researchers conducting natural experiments include regression discontinuity design, instrumental variables, panel data, and, of course, the difference-in-differences design.

There are a few examples of prior research on policy feedback that rely on observational data to make causal inferences. A comparison made in prior research is that of public opinion immediately before and immediately after a policy implementation. Isolation of the causal relationship has been achieved in prior research by timing the collection of survey data from the same respondents just before a policy was implemented and just after the implementation of that policy (Christenson and Glick 2015; Kreitzer et al. 2014). There is no randomisation involved in such research design, but because of the timing of the surveys (and the same people being asked the same questions), we can be fairly certain that no other events have happened that could cause the observed changes. A major disadvantage with such research design is that it only estimates short-term (immediate) change in public opinion.

In conclusion, there are two issues expressed in prior research on policy feedback that I aim to address with my research design. The first step is to address the issue of reverse causality and isolate the average treatment effect of the implementation of Swedish welfare policies on public preferences for further privatisation. By constructing a natural experimental research design on observational data, I move the empirical evidence from the artificial environment of survey experiments to preferences collected from a larger set of the population while studying real policy implementations. The second step is to design my study so that it captures more long-term policy feedback effects than what previous research conducted within a causal framework has accomplished.

To achieve this aim, I apply a staggered difference-in-differences design (DID<sup>4</sup>) which combines cross-sectional comparisons with a comparison of differences across time. A DID design allows me to identify and estimate causal effects by comparing how much more the treated group changed than the untreated group, when going from before to after the treatment. By combining

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<sup>4</sup> Other abbreviations that can be found in the literature include DD, DiD, and Diff-in-Diff. For no reason other than consistency, I will stick to DID.

time and unit variation, DID successfully removes (or eliminates) the effect of time and the selection bias discussed earlier. A DID design therefore ensures that the comparison in differences is made under *ceteris paribus* conditions, and thus provides credible estimates of the average treatment effect (Angrist and Pischke 2014:204–08; Callaway and Anna 2020:2; Cunningham 2021; Dunning 2012:12–5; Huntington-Klein 2021:432–34). DID with staggered adoption is simply the use of more than two time periods (as is used in a traditional DID), because there is variation in treatment timing as a result of groups of units receiving a treatment at different points in time (Callaway and Sant’Anna 2020:2; Cunningham 2021).

Although Friskolereformen and LOV were implemented on a national level in 1992 and 2009 respectively, Swedish municipalities have never been obliged to introduce private actors in their welfare systems. There is therefore both a variation in the treatment in terms of municipalities that adopted the policies by introducing private schools and user choice systems, and the municipalities that never did, *and* a variation in the timing of the treatment for those that adopted the policies. Municipalities that eventually implemented the policy are thus included in the comparison group until the year when they implemented the policy. Once municipalities have implemented the policy, they remain treated in the following periods (Callaway and Anna 2020: 2; Cunningham 2021). One main benefit of using a DID with a staggered adoption is that the time periods are not lumped into a simple “before treatment” and “after treatment” (as with a traditional DID), which would only allow for an estimation of a single effect that is implied to apply to the entire “after treatment” period (Huntington-Klein 2021:431–61). Instead, a staggered adoption allows for dynamic treatment effects, estimating the average treatment effect that either varies over time, does not show up immediately after the treatment, or fades out in time (ibid:448). My application of this method is further described in the estimation strategy section below.

The key assumption for DID to work is the parallel trends assumption – the assumption that there are no time-variant municipality specific unobservables (Cunningham 2021). The parallel trends assumption is satisfied “*if no treatment had occurred*, the difference between the treated group and the untreated group would have stayed the same in the post-treatment period as it was in the pre-treatment period” (Huntington-Klein 2021:438–40). The parallel trends assumption is thus violated when the treatment is endogenous, and the assignment of treatment status is dependent on potential outcomes (Cunningham 2021).

For the parallel trends assumption to hold in the Swedish case, the implementation of the welfare policies cannot have been a response to changes in the preferences of the public in the municipalities. If municipalities adopted the policies because of a change in public preferences, the parallel trends assumption is violated, and so makes my research design invalid. This presents a

challenge for my project, since policies are generally assumed to reflect changes in public opinion. As seen above (in Table 2), there is a statistically significant correlation between the welfare policies and average preferences for further privatisation, indicating a possibility that the policies were adopted in response to preference changes. I have therefore taken measures to verify that the parallel trends assumption holds in the Swedish case. These measures are detailed in the following estimation strategy together with my general application of a staggered DID.

To estimate the average treatment effect for the treated, the following DID estimation model is applied:

$$OPINION_i = \beta_t POLICY_{i,m} \times T_t + \gamma_i \times T_t + \delta_m \times T_t + \varepsilon_{i,m}$$

$OPINION_i$  denotes one of my measures of public preferences for further privatisation. The independent variable is a binary indicator for adopting the policy. The vector ( $T_t$ ) is a set of dummy variables for each year before and after the policy adoption. Starting four years before the adoption ( $t = -4$ ) and ending four years after the adoption ( $t = 4$ ). I also include two dummy variables for five years or more before and after the policy adoption.

$POLICY_i$  takes on a value of 1 for respondents registered in municipalities that adopted the policy, and 0 for those registered in municipalities that did not. The year before the adoption of the policy is used as a reference category by excluding the time dummy for the respondents in those years. The estimates on the interactions between each time dummy and the policy dummy ( $\beta_t$ ) capture the gap in public opinion in municipalities with and without the policy, relative to the size of that difference in  $t = -1$ .

I examine the estimates for the pre-adoption years,  $t = -4$ ,  $t = -3$ ,  $t = -2$ , to verify that there is no pre-existing difference in the trend in the level of support for privatisation between respondents living in municipalities that did adopt the policy and those that did not. When examining the estimates, there should be no treatment effect among the before-treatment coefficients. If the policies were adopted in response to changes in public preferences on privatisation, such response would be visible in the differences in the preferences trends prior to the implementation of the policies. For the parallel trends assumption to hold, the before-treatment coefficients should be close to zero, and statistically insignificant (Huntington-Klein 2021:449). An effect before the policy implementation would indicate that my estimated effects are not causal, but rather confounded by pre-trends in the outcome variable(s) (Besley et al. 2017:2228), with the exception of a few years that might show effects because I am using a large number of pre-treatment periods – when using many outcomes and pre-treatment periods, there is always the chance that some years might show effects even if everything is fine (Huntington-Klein 2021: 449).

This objection is therefore not a concern, as long as the years showing effects are few and the trends are not systematic. Similarly, if the policies caused a change in public opinion on privatisation, this should show up as positive or negative estimates for  $\beta_t$  in the years after the implementation ( $t = 1$  to  $t = 5$ ).

So, how do we read and interpret the results? First, with the DID estimation, the difference between being treated and not being treated, for the group that actually got treated is isolated (Huntington-Klein 2021: 444). The average treatment effects on the treated illustrated in the figures are therefore among the municipalities that got treated, revealing the counterfactual outcome for the municipalities that implemented the policies but not for those that never did<sup>5</sup> (ibid: 444). If the policy adoptions caused a policy feedback effect, this should show up as positive or negative estimates for  $\beta_t$  in the years after the election ( $t = 1$  to  $t = 5$ ). Standard errors are clustered at the municipal level to account for the average treatment effect being measured on individual level when the treatments are assigned on municipal level. As a sensitivity test, the model includes a municipality fixed effect ( $\delta_m$ ) and a year fixed effect ( $\gamma_t$ ). Because of the randomised timing of the policy implementation and the survey, the coefficient  $\beta$  provides an unbiased estimate of the causal effect of the policy implementation on the average public opinion on privatisation.

Second, the average treatment effects need to be interpreted in relation to the omitted time-0 effect (ibid: 449). As an example, in the LOV estimates in Figure 4a the  $\beta$  coefficients become more and more negative in the years after the reform, which means that the estimated treatment effect is larger when the policy has been implemented for a few subsequent years. The effect is also larger than the treatment effect the year before the implementation of LOV ( $t=0$ ), where there is no treatment effect (as it should not be since the treatment has not happened yet). That the effect becomes more and more negative as the years pass could be because people are being more exposed to the policy as time goes by and have more experience with private welfare services.

Given the three supplementary hypotheses, the comparison is then extended by estimating the model conditional on the three hypotheses: exposure ( $H3$ ), moderate political awareness and ideology ( $H4$ ), and high political trust ( $H5$ ). The same estimation model is used for these comparisons, only the sample is reduced to the subsets defined in the previous section detailing the data (*3. Institutional Details and Data*).

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<sup>5</sup> The DID design does not include any variation in treatment for the untreated group (since they are never treated) making an estimation of treatment effects for the untreated group impossible (Huntington-Klein 2021:444).

## 5. Results and Discussion

The following figures report estimations of the differences in public attitudes to privatisation (measured in the four outcome variables) over time between respondents in municipalities that did and did not (or did not yet) adopt the two policies. The size of the difference between the treatment and comparison groups in the probability of a policy feedback effect in each year is benchmarked against this difference in the reference category (the year before the policy implementation,  $t = 0$ ). The figures plot the estimates of  $\beta_t$  from my DID model described earlier, together with 95 percent confidence intervals. The main results are shown in Figure 4 to 8, with regression estimates provided for each of the four outcome variables. The results for LOV are shown on the left side of the figures and for Friskolereformen on the right.

Before presenting the results for each of the hypotheses, it is necessary to check the credibility of the parallel trends assumption. The figures show that the policies had (mostly) no effect among the before-treatment coefficients – a good indication the parallel trends assumption holds. The before-treatment coefficients are all close to zero, and are statistically insignificant, meaning that the figures show no evidence of pre-existing difference in the trend in public opinion between subsequently responses in municipalities that did and did not adopt the policies before the policy implementation. As expected, a few pre-treatment years show effects, but there are no systematic trends – these effects are likely just a result of my large number of outcomes and pre-treatment periods. The absence of pre-existing differences implies that had the policies not been adopted, public preferences for privatisation would likely have continued to have similar trajectories (Huntington-Klein 2021:439).

Since the verification that the parallel trends assumption holds, I can be confident that the issue of reverse causality is solved – the implementation of the welfare policies was not a response to changes in the preferences of the public in the municipalities. Now that the credibility of the parallel trends assumption has been verified, supporting the validity of my research design, I present the results for each of my hypotheses. I then move on to a discussion of the overall results and their implications for our general understanding of policy feedback effects and offer some suggestions for future research. Beginning with the fundamental question of this thesis: did the Swedish welfare policies cause public preferences for privatisation to shift?

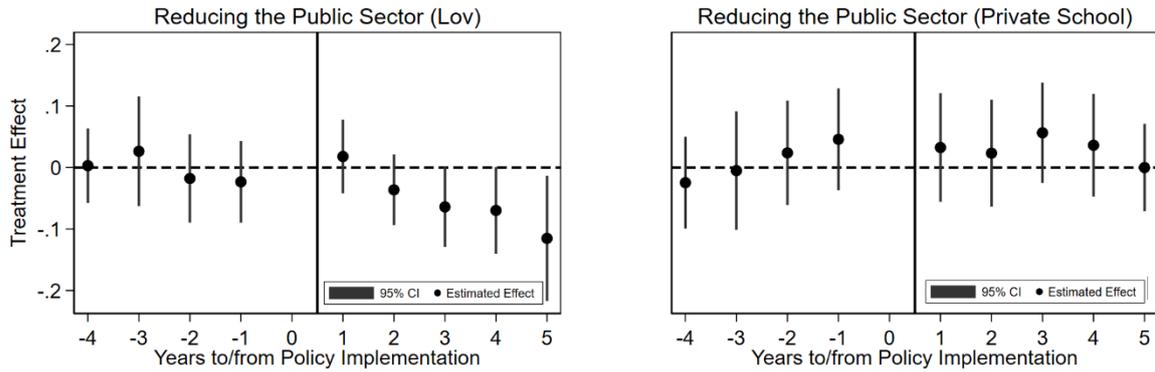


FIGURE 4a. POLICY FEEDBACK EFFECTS ON GENERAL PUBLIC ATTITUDE TO REDUCING THE SWEDISH PUBLIC SECTOR

*Notes:* Contains the estimates from the DID model, run on the scores reported in the National SOM Survey Cumulative Dataset. These are run separately for the municipalities that adopted LOV the year before the survey (left) and for the municipalities that introduced private school the year before the survey (right). The markers show the percentage point difference in the probability of changing levels of public attitude to privatisation between implemented and comparison groups in each year compared to the year before the implementation ( $t=-1$ ). Vertical lines are 95 percent confidence intervals.

In examining Figure 4a closer, the figure shows that LOV caused a moderate decrease in people agreeing with the reduction of the public sector in the years following the policy implementation. The effect before LOV is close to zero followed by a visible decrease in public opinion agreeing with further reduction of the public sector after the policy adoption. The treatment effect in the years following the implementation of LOV is statistically significant in years 3, 4 and 5, indicating that the effect showed up over time rather than immediately after the implementation. For Friskolereformen on the other hand, the results are not as clear. The right side of Figure 4a indicates that the reform may have caused a moderate increase in people agreeing with the reduction of the public sector. However, the effects are not statistically significant, and it is not clear that the effects are not confounded by pre-trends in the outcome variable.

Below, Figure 4b shows the results for the separate outcome variables relevant to each of the policies. To the left, the black markers show the estimated treatment effect for LOV on public attitude to further privatisation of the healthcare system, and the grey markers for the eldercare system. To the right, the black markers show the estimated treatment effect for Friskolereformen on public attitude to further privatisation of the school system. The estimates show the same pattern as in Figure 4a: the pre-implementation estimates are close to zero and lack statistical significance. The directions of the effects are also the same as above: negative for LOV and positive for Friskolereformen. In the years after the policy-implementation, the only statistically significant effect is found for LOV in healthcare. The negative coefficients show that the year after the

implementation of LOV the average public opinion on privatisation of healthcare went down 0.1 steps more on the 5-step interval scale, compared to the difference in average public opinion in municipalities that did not yet implement LOV. Looking back at the difference between people identifying their politics as being on the right to people identifying on the left, the treatment effect is equivalent to 7% of the difference in preferences for further private healthcare between the two groups.

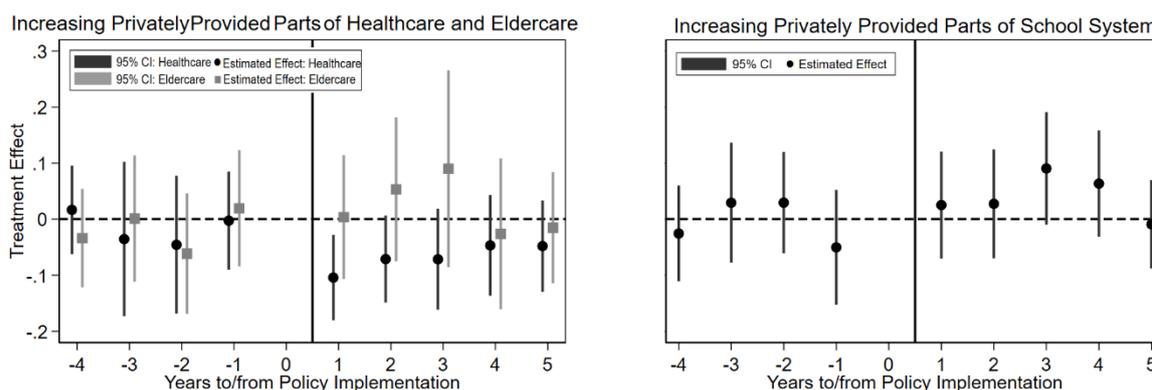


FIGURE 4b. POLICY FEEDBACK EFFECTS ON GENERAL PUBLIC ATTITUDE TO PRIVATISATION

*Notes:* The structure of the plot is described in the notes under Figure 4a. For LOV (left) public opinion on privatisation is measured in attitude to increasing privately provided parts of healthcare (black markers) and eldercare (grey markers). For Friskolereformen public opinion is measured in the combined variable on increasing the number of private schools and investing in more private schools (as described in previous operationalisation section).

The trends shown in Figure 4 continue into the supplementary estimation models. Overall, the results in Figures 4 to 8 suggest a moderately-sized negative reaction to LOV and a moderately positive reaction to Friskolereformen, but most of the coefficients are not statistically significant, and when they are, the standard errors are quite large. Taken together, it is therefore hard to say anything conclusive about the two main hypotheses. It seems that Friskolereformen may have caused a moderate increase in public preferences for privatisation (conforming to  $H1$ ), whereas LOV seemed to have caused a moderate decrease in public preferences for privatisation (conforming to  $H2$ ). The findings support the *existence* of policy feedback effects, but do not indicate any recurring direction of such effects. No conclusion can therefore be drawn on the two main hypotheses.

The possibility that the somewhat absent and inconsistent feedback effect in Figure 4 is due to the effect's dependency on the type of policy and people exposed to the policy is explored in the following supplementary estimation models shown in Figures 5 to 8. The overall results from

the sub-group analyses are similar to the main analysis, in that the direction of the effect and its size do not vary much for any of the populations. The lack of variation across populations is a good sign that the observed treatment effect is not heterogenous. Had there been a large variation across the sub-group samples (Figures 5-8) compared to the full sample (Figure 4), such variation would indicate heterogenous effects. Seeing as this is not the case, we can be reasonably sure that there are no heterogenous effects. There are no clear differential pre-trends for most sub-groups (two years are statistically significant, but again that is most likely a result of the large number of included outcomes and pre-treatment periods). Most treatment effects are not statistically significant, and again the standard errors are rather large.

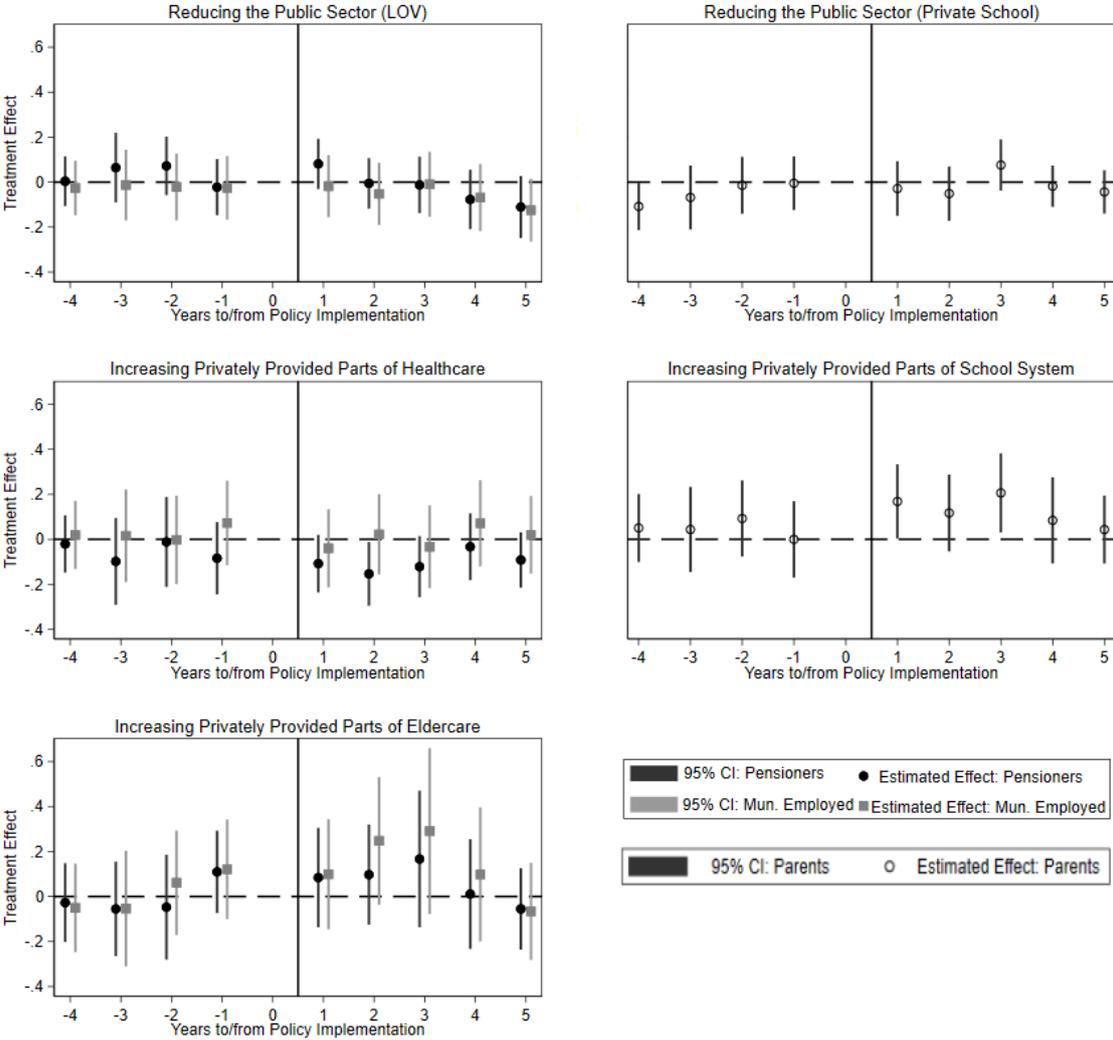


FIGURE 5. POLICY FEEDBACK EFFECTS ON PUBLIC ATTITUDE PRIVATISATION, CONDITIONAL ON EXPOSURE TO THE POLICY

Notes: The structure of the plot is described in the notes under Figure 4a and Figure 4b. For LOV (left) the estimates are run separately for pensioners (black markers) and municipal employees (grey markers). For Friskolereformen (right) estimates are run for parents.

There are some notable exceptions in the sub-group sample analyses, where the effects are statistically significant and last for more than one year. Looking at public opinion on the increase of private providers of healthcare services, there is a continued trend of negative change following the implementation of LOV. Pensioners seem to have decreased their preferences for private healthcare (statistically significant at years 1, 2 and 3). Looking to the right, parents seem to have increased their support for private providers in the school system following Friskolereformen. Here there is a treatment effect that is statistically significant for the first and the third year.

The results seem to modestly indicate that the type of policy and the target population of the policy matter for the policy feedback effect (consistent with *H3*). The observation further supports the idea that the design of the policy may increase the visibility of some social and political issues while obscuring others. It might be that the framing of the two policies varied in a substantial manner which resulted in people's reaction to their implementation. As evident in Figure 5, there seems even to be a difference in the change in attitude to further privatisation of different type of welfare services covered in LOV. Although, as stressed already, the differences in the size of the average treatment effect are not large enough to suspect heterogeneity. Instead, the trends in the subgroups seem to follow similar trends of the general population.

The largest treatment effect found is among respondents who self-identify to the right on the political spectrum (signified by the grey markers in Figure 6) which is consistent with *H4*. Looking back to the descriptive data from earlier chapter, the fact that the left-identifying respondents (signified by black markers in Figure 6) show no effect is perhaps unsurprising. One simple explanation could be the outcome of a *ceiling effect* – the majority of the respondents identifying to the left may already oppose privatisation “to the ceiling” of the five-scale measurement used in SOM and the dependent variable would therefore no longer have an effect on the outcome variables. The mean of around 2 for all outcome variables presents the possibility that a negative change in attitude is unmeasurable due to the lack of extreme response options in the survey. On the other hand, for respondents identifying to the right the descriptive data showed that the mean response for all outcomes was around 3. It should therefore be possible to measure a change in attitude (in either direction) for right-identifying respondents. As is seen in Figure 6, the largest statistically significant average treatment effects are found in this subgroup sample.

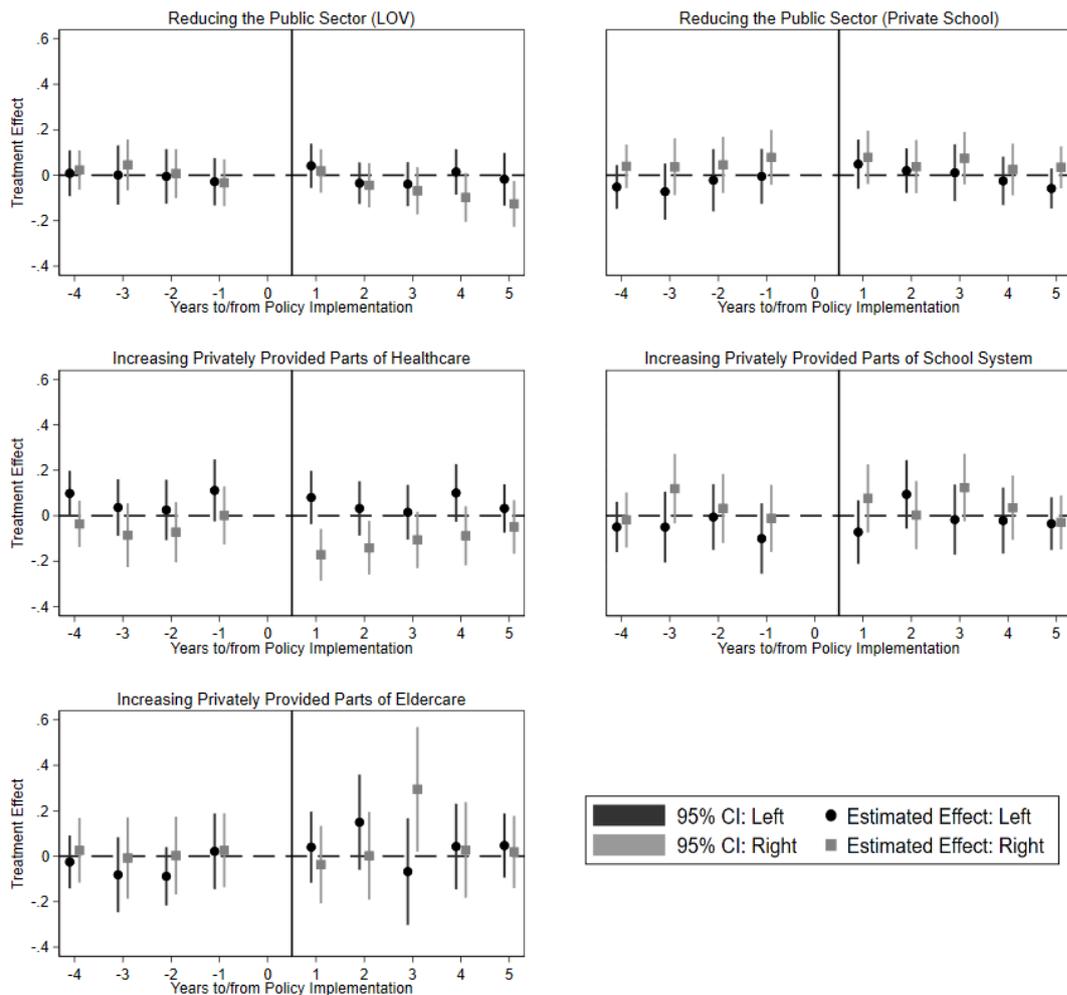


FIGURE 6. POLICY FEEDBACK EFFECTS ON PUBLIC ATTITUDE TO PRIVATISATION, CONDITIONAL ON IDEOLOGY

*Notes:* The structure of the plot is described in the notes under Figure 4a and Figure 4b. The estimates are run separately for respondents identifying to the left (black markers) and to the right (grey markers) of the political spectrum, for LOV (left) and Friskolereformen (right).

The most significant evidence of an average treatment effect is that of the attitudes to healthcare for respondents identifying to the right. Following the implementation of LOV, this subgroup had a significant decrease in the change in political attitude to further privatisation of the healthcare sector (around 0.18 steps) compared to the changes for respondents in municipalities that did not implement LOV. This change is equivalent to around 13% of the difference in preferences for further private healthcare between people identifying to the left and right.

This change could perhaps seem counterintuitive seeing as people on the right generally are more positive toward privatisation than people on the left<sup>6</sup>. But the change shown in Figure 6 does not mean that respondents on the right have a more negative attitude toward privatisation than

<sup>6</sup> Also taking into consideration that the strongest evidence for a negative effect in the Swedish survey experiment was amongst respondents identifying as centre-left (Bendz and Oskarson 2020: 11-15).

respondents identifying to the left. The figure tells us nothing about the changes in one subgroup in relation to another. What the change does suggest is that the assumption that people who were predicted to support the policy (because of their ideological placement) does not hold, as on average people identifying to the right did not change their preferences in accordance with LOV.

Regarding the second half of *H4* about people’s political awareness, these results are presented in Figure 7 below. Figure 7 shows a pattern similar to that found in the general population, suggesting that political awareness had little influence on the general policy feedback effect following the policy implementations. This is perhaps a result of the unusual salience of Swedish welfare policies, meaning that the first step in the formation of public opinion (reception of persuasive communication) was not as dependent on people’s political awareness as in most typical policy cases.

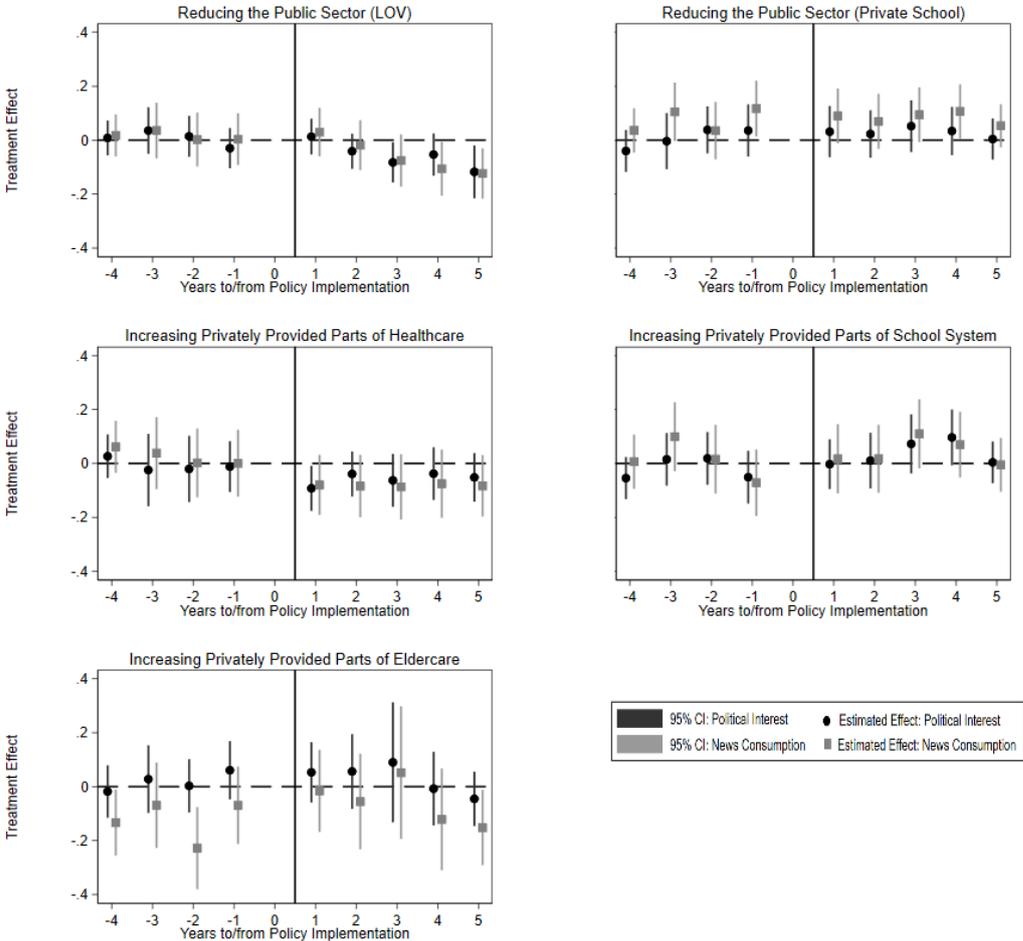


FIGURE 7. POLICY FEEDBACK EFFECTS ON PUBLIC ATTITUDE TO PRIVATISATION, CONDITIONAL ON MODERATE POLITICAL AWARENESS

Notes: The structure of the plot is described in the notes under Figure 4a and Figure 4b. The estimates are run separately for moderate political interest (black markers) and moderate news consumption (grey markers), for LOV (left) and Friskolereformen (right).

Finally, Figure 8 (located in the Appendix to save space and avoid repetition) does not show any particular support for the theoretical argument that the policy feedback effect depends on people's high trust in politicians (*H5*). This provides further support for prior empirical evidence that has suggested the same conclusion (Kotsadam and Jakobsson 2011).

So, what do the overall results suggest in relation to previous research and the theoretical assumptions discussed in the beginning of the thesis? Previous research suggested four models of attitude change following a policy implementation. The sub-group analyses show no indication of a polarisation effect. The Polarisation Model proposed that the public debates leading up to the reforms would remain and ultimately intensify opposition of people on the left, and support from people on the right. Instead, following the implementation of LOV, the average changes in opinion remained the same for people on the left and, contrary to expectations, people on the right had a more negative change than in municipalities without LOV. This perhaps suggests a “thermostatic pattern” in line with the theorisation in the Backlash/Thermostatic Model. The positive effects found in municipalities with Friskolereformen on the other hand, suggests that public opinion moved in the direction of this policy – offering moderate support for the Legitimacy Model.

As for the moderate political awareness suggested by the theoretical framework of the two-step process for opinion formation, the analysis shows no substantial differences for these subsamples (political interest and news consumption) when compared to the full sample. A reason for this absence could be that my model did not adequately capture the concept of moderate political awareness, or it could be a result of the high visibility of the Swedish welfare policies.

Looking at Sweden as a case more specifically, the results support what previous research has stressed in terms of the design of the policy (see for example Soss 2004: 291-293; Soss and Schram 2007; Campbell 2011b: 278; Gusmano, Schlesinger, and Thomas 2002: 734). Despite the two welfare reforms being implemented in the same country, relatively close in time, and concerning the same issue (i.e., introducing private actors in a previously dominated public sector), the impact of these reforms does seem to have varied.

There are various possible reasons for why LOV had an overall negative effect while Friskolereformen seem to have had an overall positive effect. Although Friskolereformen in itself continues to receive substantial attention, the direction of opinion change may relate to the difference in information received about the implementation of a choice system compared to the opening of a new private school. Applying the terminology from the theoretical background – the municipalities' adoption of LOV may have been more *visible* to the public than new private schools. A logical consequence of the design of the policy and perhaps administrative reasons. When a local authority decides to implement LOV they are required by law to inform “every individual about

every service provider that the authority has signed a contract within the frame of the Act on Choice Systems. The information has to be objective, relevant, comparable, and accessible” (Finansdepartementet 2008). For private schools the same requirement (not to inform every individual, but every legal guardian) has only been in place since 2015 (Fridolin 2017). Unfortunately, some municipalities have been shown to not follow the requirements for either reform (Vårdföretagarna 2019:19). Perhaps the difference in the local authorities’ provision of information about the adoptions of the two reforms was a contributing factor to the differences in the impact of the policies.

## 6. Conclusion

In this thesis, I contribute credible causal evidence on how the increase of private actors in the Swedish welfare sectors caused changes in public preferences for privatisation. By applying a staggered difference-in-differences design I have isolated the causal effect of policy implementation on public opinion thereby avoiding the issue of reverse causality. My thesis provides a causal link between policy adoption and public opinion, suggesting that the policies themselves can be an important factor that shape public opinion on privatisation. The possibility for policy feedbacks raises normative questions about how we understand democratic representation and popular rule (Barabas 2009:183; Campbell 2012:342). Policy feedback effects could have serious implications for our understanding of inequalities in citizen voice seeing as the preferences, to which policies supposedly respond to, may be the result of previous policies themselves (Mettler and Soss 2004:59; Soss 2004:291). If policies do have the potential to affect and construct public preferences, policy feedback effects “may force us to think about representation in new ways” (Campbell 2012:342).

Primarily, my analysis shows that the policy feedback effect varied even in a case where the surrounding circumstances of the policy implementations were very similar. Both LOV and Friskolreformen introduced private actors in welfare services that, until the implementation of the reforms, had almost exclusively been provided by public actors. The difference in outcomes lend credence to the assumptions that the design of the policy and the target population matter for the policy feedback effects. The application of a staggered difference-in-differences design also has allowed me to study more long-term policy feedback effects showing that the effects do not always show up immediately, and in some cases increases (in other decreases) over time. The overall results support the *existence* of policy feedback effects, but do not indicate any recurring direction of such effects. No conclusion can therefore be drawn on the two main hypotheses. The three supplementary hypotheses explored the possibility that the somewhat absent and inconsistent feedback effects was a result of the effect’s dependency on the type of policy and people exposed to the policy. The results showed no strong support for the three supplementary hypotheses, with

the exception of the preferences for increased privatisation in healthcare for people identifying to the right on the political spectrum.

Despite the results of this thesis and the work of prior research, there is no certain answer to why the two individual Swedish welfare reforms seem to have had such distinct impacts on the changes in public preferences for privatisation. I have discussed some potential explanations like the information of the policies provided by the local authorities, but I can only speculate on the reasons behind the differences in outcomes found in this thesis. Continuing from my findings, future research could explore the conditions that resulted in such variable outcomes for the Swedish welfare policies. With this thesis, I hope to have added a small puzzle piece to the great puzzle that constitute the policy feedback concept.

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## Appendix

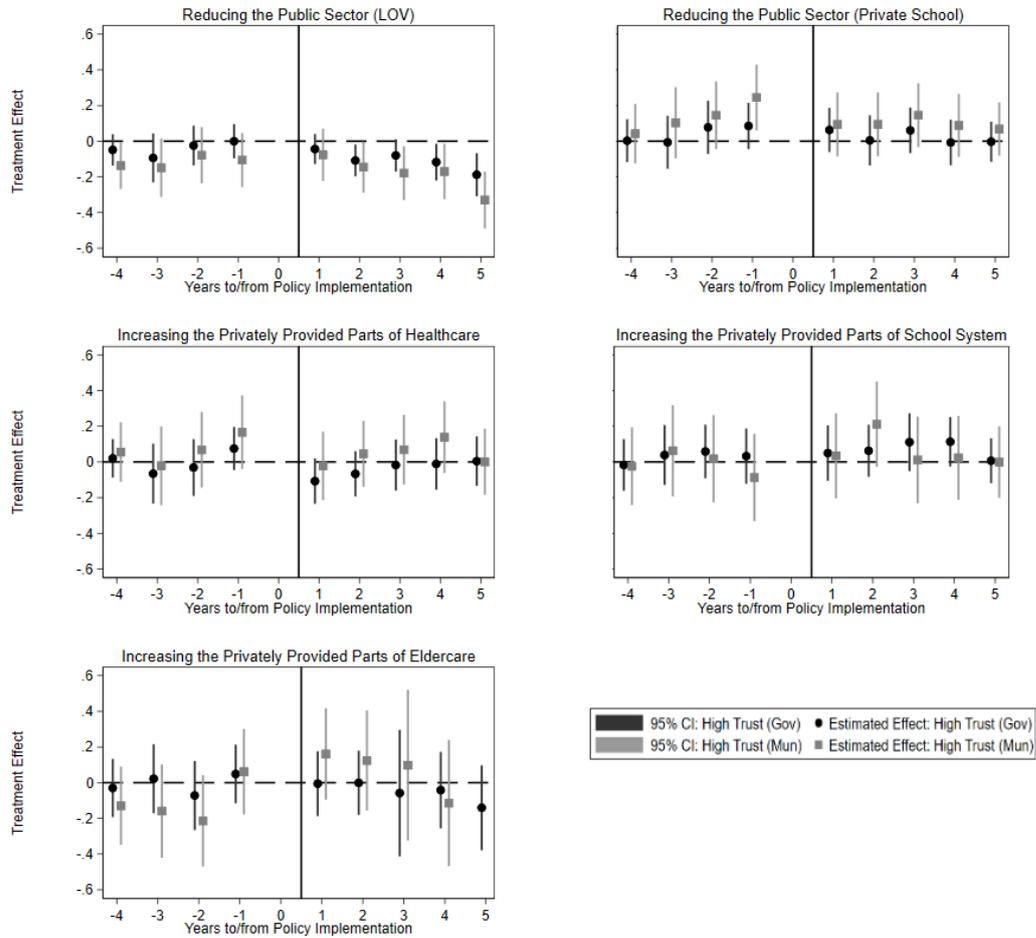


FIGURE 8. POLICY FEEDBACK EFFECTS ON PUBLIC ATTITUDE TO PRIVATISATION, CONDITIONAL ON HIGH TRUST

*Notes:* The structure of the plot is described in the notes under Figure 4a and Figure 4b. The estimates are run separately for respondents with high trust in government (black markers) and high trust in municipality board (grey markers), for LOV (left) and Friskolereformen (right).