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DP12586

**FROM CONFLICT TO COMPROMISE: THE
IMPORTANCE OF MEDIATION IN
SWEDISH WORK STOPPAGES 1907-1927**

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ECONOMIC HISTORY



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Abstract

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JEL Classification: N33, N34, J52

Keywords: industrial relations, strikes, mediation, labour market institutions

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“I am here, I am there, wherever I am is but troubles and fuss”.¹

1. Introduction

Strikes have arguably been the most common, and well-documented, form of social protest in industrial societies. The increasing importance of strikes, often associated with the formation of trade unions, were seen in a number of countries from the second half of the nineteenth century onwards (Card & Olson, 1995; Geraghty & Wiseman, 2008, 2011; Mikkelsen, 1992; Shorter & Tilly, 1974). Institutions intended to prevent and settle strikes and lockouts were created in many countries. These institutions showed great variety, with different degrees of state intervention and compulsion, which in turn may have had far-reaching and unintentional consequences.

Great Britain relied on private initiatives that emerged in certain branches of industry and the establishment of quasi-parliamentary bodies, so called conciliation boards (Hicks, 1930). In New Zealand and Australia the state took a more active part in regulating the labor market by the means of arbitration. Other countries, like Sweden, established institutions for mediation, the involvement of a third party to assist opposing trade unions and employers in reaching settlements. An interesting aspect of labor-market mediation, as it emerged in Sweden, is that the institution basically left the power balance between the interest groups unaffected. This was not the case in Spain and France where mediators

¹Own translation from Swedish. This verse mocked the expected struggles of the mediators of industrial conflicts, was recalled and published in a popular journal (Hvar 8 dag, 1909: 548).

could use political means to induce the parties to reach settlements (Domenech 2006; Friedman 1988).

Among contemporary observers, as well as present-day scholars, there are different opinions about the importance of mediation, and similar institutions, for preventing and solving labor conflicts. Analyzing American strikes in the period 1880-1945, Geraghty and Wiseman (2011), maintain that the U.S. Conciliation Service from 1917 changed the preconditions for labor conflicts, signaled a less hostile attitude towards trade unions and promoted negotiations. Looking at France around 1900, Friedman finds that mediators typically favored workers, which he believes to have contributed to a radicalization of trade unions. Domenech (2006) findings for Spain 1880-1915 points in the same direction. State-sponsored mediation helped workers to win, which “fueled the radicalism of both unions and employers’ associations”.² A third view is that mediation and conciliation services are toothless measures with little real impact. Such a view is implicit in Korpi and Shalev's (1979) account of the development in Sweden.

The lack of consensus about the impact of mediation may partly be attributable to differences in institutional design, context, and the basic nature of labor conflicts. Lack of consensus may also be related to methodological difficulties. Since mediation is not randomly applied to disputes, selection effects may influence empirical estimations. As established by Wall and Lynn (1993: 174), “mediation is more likely to produce settlement whenever the parties are highly motivated to negotiate”. And as pointed out by (Greig, 2005), much of the empirical literature on mediation has failed to address this problem.

² See also Houpt and Cagigal (2014) for the period 1914-1936.

Referring to the setting studied in this paper, Mikkelsen (1992: 120) has shown that conflict outcomes were correlated with the length of work stoppages and whether the parties were organized. Long conflicts involving organized parties were more likely to end in compromises. If mediation more often took place in such conflicts, it is hard to establish a causal effect of mediation. We are not aware of any studies in the literature on mediation in industrial relations that deals with the problem of selection bias and identifies causation econometrically between mediation and compromise outcomes.

In this paper, we take a closer look at the first 20 years of state-sponsored mediation in Sweden. More precisely, we analyze the importance of mediation on the likelihood of compromise outcomes in work stoppages for the period 1907-1927.³ We use a new database with geocoded information on the cause, characteristics and outcome of all recorded work stoppages, supplemented with information on the location of mediators. In order to identify the effect of mediation on compromise outcome we instrument mediation with geographical distance between the location of the work stoppage and the mediator's office.

³ The focus on the compromise outcome means that our assessment of the importance of mediation is partial. For example, we do not study whether mediators influenced the duration of conflicts or managed to prevent conflicts. We cannot study the relationship between mediation and conflict duration since our data does not reveal when mediators became involved in conflicts. The issue of prevention and other long-run effects can be studied, but that would require a different research design.

The setting we are studying was a society in transition. On the one hand, the labor market was characterized by frequent and long conflicts. In these days, Sweden may actually have been the most strike-prone country in the industrialized world (Shorter & Tilly, 1974). On the other hand, the period also saw the spread of collective bargaining between organized workers and firms.

Our contribution is threefold. Firstly, we contribute to the literature on conflict resolution more generally with a case where mediation was not associated with the credible use of punishment to enforce cooperation (for a review, see Wall & Lynn 1993). Secondly, our case and application of an IV-approach to previously unused micro data on work stoppages builds on, and add to, the literature on the economics of strikes (Hicks 1932; Ashenfelter & Johnson 1969), where institutions of conflict resolution has been discussed but seldom with proper econometric methods to adress causality (c.f. Friedman 1988; Domenech 2006; Geraghty & Wiseman 2011). Thirdly, we contribute to the literature on the origins of the Swedish Model, which is often described as a class compromise, manifested in two hallmark events: the so called December compromise in 1906 and the Saltsjöbaden Agreement in 1938. We show that compromises also took place in hundreds of local conflicts in between and that mediators had a role to foster such settlements. Our study adds substance to governace-based accounts of the origins of the Swedish Model.

2. Mediation and the origins of the Swedish Model

During the Golden Age of the 1950s and 1960s, the Swedish labour market was characterized by centralized and coordinated wage negotiations between trade unions and employer organizations without direct state interference (Lundh 2010; Nycander 2017).

Strikes and lockouts were rare and representatives of labour and capital cooperated on many issues. The origins of this Swedish Model – with the transition from conflict to peace in the labour market as a central theme – has attracted considerable scholarly attention.⁴ The most common view is to situate the birth of the Swedish Model to the 1930s. According to Korpi (1978), the Social Democratic Party’s political dominance, achieved at the national level in the 1930s, was crucial. In contrast, Swenson (2002) emphasizes the establishment of cross-class alliances between firms and unions in export industries. Although focusing on different actors and mechanisms, Korpi and Swenson share an essentially material, interest-based perspective, and a view of the 1930s as the main phase of change. There are, however, reasons to look closer at institutional features of the Swedish labour market and developments that took place in the preceding decades.

Adlercreutz (1954) and Nycander (2017) observe that the Swedish state already around the turn of the century 1900 tried to avoid direct intervention in labour market issues, allowing for the regulation of wages and other issues through collective bargaining. An important event in this regard was the December Compromise in 1906, where the confederation of blue-collar workers (LO) and the main employer organization (SAF) recognized each other. The spread of collective agreements did not immediately reduce the

⁴ There is a discussion whether it is a ”Swedish” or a ”Nordic” model. When it comes to institutions for conflict resolution, it is fair to say that the Swedish labour market was distinctive, also in comparison with Denmark, Norway and Finland (see Aarvag Stokke 2006 and Nycander 2006). Moreover, it should be noted that we use the term “the Swedish Model” in a restrictive sense, referring to the labour market.

number of conflicts, as reminded by Fulcher (1991) and Hamark (2014) the conflict level peaked in the 1920s and thereafter showed a clear downward trend (see figure 1). More generally, Rothstein (2005) observes indications of mutual trust between the labour market parties, which he attributes to various corporatist institutions, such as public employment offices, where both parties were represented.

The Mediation Act of 1907 has so far not received much attention in general accounts of the origins of the Swedish Model. Korpi and Shalev (1979) mentions the Act in passing, while Swenson (2002) does not even touch upon it. Neither Nycander (2017), nor Rothstein (2005) are discussing the Mediation Act, although it appears to fit well into their stories.⁵

In the first years of the twentieth century, LO and SAF, were united in their rejection of arbitration as a way of achieving more peaceful conditions in the labour market (Lundh 2006). Neither mediation, most forcefully advocated by liberal politicians, was uncontroversial (Westerståhl 1945: 267-268). A popular journal recalled that the mediation initially was associated with “an air of joke and a certain degree of ridicule” (own translation, “Våra förlikningsmän I”, 1909: 548). One Member of Parliament argued that similar legislation “had shown to be totally worthless” abroad. In his defense of the reform, minister of internal affairs Axel Schotte, emphasized that “it had often been regarded as a sign of weakness for a warring party to offer mediation” and that “an official mediation institution, would certainly make mediation take place more often” (“Medling i arbetstvister”, 1906). This argument is interesting as it reveals a perception of labor

⁵ See however Nycander (2006) and Lapuente and Rothstein (2014: 1428).

conflicts as what could theoretically be described as hawk-dove games, an idea that we will discuss further below.

Supported by a newly established majority of liberals and social democrats, the Mediation Act was passed in 1906, about half a year before the December Compromise. The fact that the Act raised so much debate suggest that the reform was not just a codification of existing practices but represented a new feature in the Swedish labor market.

Despite obvious international influences, the Swedish mediation institution got an original design. In other countries, mediation was most often done by committees with representatives for employers and workers, chaired by an impartial person and mediation was often done by industry-level committees. Sweden opted for a different solution. According to the Swedish legislation on mediation from 1906, mediation services were to be offered by individual mediators with geographical areas of responsibility.

Another basic feature of the Swedish system of mediation was its limited economic resources. The legislation included a number of specifications on what the mediator should do but remuneration was not directly related to the work load. The mediators received 2500-3000 SEK annually, roughly equivalent to the earnings of an ordinary white collar worker at the time, which was low considering the social background of the would-be mediators. The number of disputes that the mediator was involved in did not influence their reward, nor did the length and geographical distance to the conflict.

The persons who were appointed as mediators were prominent figures in society, with prestigious occupations and marks of distinction. The most common occupational experience was to have served in the legal system. Ten out of 25 mediators in our period

of investigation had such a background. Experiences from private business, the military and academia were relatively rare. Only one of the mediators in this period had a working-class background, but at the time of his appointment he had already achieved a high position. As noted by Åmark (1994: 150), the mediators often had liberal sympathies. We have been able to identify connections to the liberal parties for about half of the 27 mediators that were appointed in the period 1906-1927.⁶

The offices of the mediators were located to the places where the mediators happened to live. Most of the mediators lived in cities but not all of them. Four lived outside cities during whole or parts of their periods in duty (26 years in total). It may also be mentioned that the mediator did not always live in the biggest city of the district. Malmö, the biggest city in the southern district, had for example no mediator during the period of investigation. Most of the mediators served for fairly brief periods of time. The median tenure was five years and almost a third of the mediators served only for one year. New appointments were frequent and we observe no less than 12 instances where the new mediator resided in another place or residence than the former.

The map in figure 2 shows the seven districts and the location of mediators. If a mediator changed residency, or if a new mediator was appointed at a location different from the previous mediator, this is indicated in the map with the first year of the new mediator's residency.

⁶ There were two liberal parties in Sweden at the time. The background information on the mediators reported in this section has been drawn from biographical encyclopedias.

<Figure 1 about here>

<Figure 2 about here>

Since the initiation of the institution of mediation was formalized, the number of work stoppages with mediation increased, from 37 conflicts in 1907 to above 70 in the mid-1920s, as seen in figure 2. Thus, the share of work stoppages receiving mediation increased from 15 percent in 1907 to 45 percent in 1927. At the same time, the number of work stoppages also fluctuated considerably. The mediators received a substantial grant increase in 1920, when the Mediation Act was revised. Nominally, the amount was doubled, but in real terms the increase became even larger due to the price deflation in the coming years. With more resources, the mediators were not only able to mediate in more work stoppages but could also intensify their efforts to prevent labour conflicts.⁷

During our period of investigation the mediators followed a “strict parliamentary procedure” with negotiations in plenum on general conditions, followed by specific negotiations in smaller committees and “compromise attempts of more informal nature” (Ekblom 1956: 741-742). Secrecy was important. As one mediator recalled: “Nothing could be revealed externally, as long as the negotiations proceeded”. In the final round, the parties handed over their responses to the mediator’s proposal in sealed envelopes, which were opened and recited by the mediator at a solemn plenary-meeting.

⁷ Yet, despite this increase, the institution remained a “minimal” and “without permanently employed mediators”, as observed by Nyström (2006: 58), until the end of the twentieth century.

According to their own reports, the mediators were successful in making parties in a dispute reach compromises. Thörnqvist (2006: 40) gives a similar view, claiming that the "role of the mediators was considerable" and that "as much as a fourth [of the work stoppages] were solved with the assistance of a mediator". Needless to say, the claims made in the reports and by Thörnqvist are impressionistic and do not take into account the problems of reverse causality and omitted variables. Mikkelsen (1992: 119-120) shows greater awareness of these problems when discussing the correlation between mediation and compromise outcomes in work stoppages. From a theoretical perspective, one may also wonder how mediators could make any difference in the Swedish case, given the fact that they had no means of coercion. This issue is discussed in the following section.

3. Economic theories on labor conflicts and mediation

For neoclassical economists, the existence of strikes about wages has been a puzzle (Kennan, 1986). As pointed out by Hicks (1932), strikes tend to decrease the overall size of the surplus that is to be distributed, therefore union and employer may in most cases benefit from avoiding conflict. The fact that conflicts do occur is often explained by the existence of information asymmetries between employers and trade unions, or between union leaders and members. Depending on the nature of these asymmetries, mediation and similar institutions may be able to *prevent* strikes and lockouts. The issue we address in this paper is whether mediation can help to *solve* conflicts once they have started.

Whether we can expect mediation to make a difference depends on the basic nature of labor conflicts: are conflicts essentially all-out brawls between employers and workers,

where the winner takes it all, or are the parties rather struggling over the distribution of a surplus that can be divided in various ways? The former situation can be described as a war-of-attrition, where the parties fight against each other until one party is exhausted and surrenders (Maynard Smith, 1974). In this model, conflicts have binary outcomes – one party wins, the other loses. In economic history, war-of-attrition models have been used to characterize and analyze labor conflicts in the late nineteenth century America, in early twentieth century Spain and in Canada in the 1920s; contexts where employers did not recognize trade unions (Card & Olson, 1995; Geraghty & Wiseman, 2008; Domenech 2006; Huberman & Young, 1999, 2002). In a war-of-attrition framework, there is hardly any scope for negotiation since the price that is contested (most often union recognition) is indivisible. Thus, mediation cannot be expected to solve conflicts.

The latter situation, where employers and trade unions recognizes each other and, at least in principle, bargain and reach compromises can be described as a hawk-dove game.⁸ In a hawk-dove game compromise offers are seen as a sign of weakness and avoided by the parties. Third party involvement can remove some of the weakness associated with making unilateral compromise offers and thus increase the likelihood of a compromise outcome.

We regard the hawk-dove game as a plausible characterization of Swedish labor conflicts in our period of observation. This implies that mediation could have made a

⁸ See Rasmusen (2007) for a general discussion on the hawk-dove game and Geraghty and Wiseman (2011: 520-521) for the application to labor conflicts that we follow here. Rothstein (2005) uses the term ‘social trap’ with a very similar meaning as the hawk-dove game.

difference. However, this does not mean that all conflicts were hawk-dove games or that the reward structure was the same in all kinds of conflicts. Moreover, as pointed out by Geraghty and Wiseman (2011), government involvement in the labor market need to be perceived as impartial, otherwise it can have effects opposite from those originally intended.

4. Swedish statistics on labor conflicts 1907-1927

Systematic and continuous collection of statistics on work stoppages in Sweden began in 1903.⁹ The gathering of data was initially based on information in daily journals and trade journals ("Arbetsstatistik. E, Arbetsinställelser i Sverige", 1909: 9–10). If a conflict was encountered in a journal, questionnaires were sent out to the involved parties. The response rate was high; in the period 1903-1923 responses were gathered from both employer and worker representatives in almost 60 percent of all work stoppages (Mikkelsen, 1992: 439). Other informants, such as local agents of the Board of Trade (*Kommerskollegiums lokalombud*) supplied complementing information. Occasionally annual reports from the trade union confederations and employers' organizations, as well as various price lists and collective agreements, were consulted. In contrast to some other countries, the Swedish

⁹ The first report was published in 1909, covered the years 1903-1907, but also included a retrospective view of work stoppages in the period 1859-1902 and the general political strike of 1902 ("Arbetsstatistik. E, Arbetsinställelser i Sverige", 1909).

statistics on work stoppages had no limitations with regard to industry, number of involved workers or duration (Mikkelsen, 1992: 438). In principle, the statistics should include all conflicts – small and large.¹⁰

The gathering of information through questionnaires served a twofold purpose: (1) to establish whether a conflict actually had resulted in a work stoppage, which was what the statistics aimed to cover, and (2) to uncover the causes, characteristics and outcomes of the conflict. Cases where the parties have supplied contradictory information on whether a conflict-related work stoppage actually had occurred were included in the statistics, but with notes about how the parties have described the events. Until 1927, extensive information was included in the published reports for each individual work stoppage, namely: the beginning and end of a stoppage (dates), nature (strike or lockout), involved occupation(s), location, reason for conflict, number of directly involved employers and workers, whether workers were organized, the outcome of the conflict, the source of information and additional notes.

In this paper we mainly use statistics on work stoppages for the period 1907 to 1927. The beginning of our period of investigation coincides with the beginning of state mediation. The period ends when the official statistics no longer includes information on individual conflicts.

¹⁰ In practice, there are of course possible gaps. It was, for example, recognized that conflicts concerning woodsmen in the North may have been underreported ("Arbetsstatistik. E, Arbetsinställelser i Sverige", 1909: 12). Another likely omission in the statistics is, as pointed out by Hamark (2014), work stoppages that had political purposes.

We have extracted, digitized and geo-coded each recorded work stoppage in the period. We exclude work stoppages where information is missing in any of the relevant variables used in the analyses (most often geographical location). This restriction leaves us with a sample of 4,752 observations out of a total of 5,533 recorded work stoppages (86 percent) from the official sources. In figure 3 the number of strikes, their geocoded locations and the district boundaries of the mediators' offices are featured in a map. The figure suggests that the number of work stoppages were relatively similar among district, since the districts were probably drawn to ensure a relatively even workload for the bureaucracies of the mediators.¹¹

We have constructed a dummy for compromise outcome based on information in the column labelled *Resultat* in the official strike statistics. In about 70 percent of the cases we could rely directly on the word *kompromiss* in the mentioned column.¹² Where *kompromiss* was not explicitly mentioned, we made a judgement based on a comparison of the demands of the involved parties (given in the column labelled *Tvistepunkt*) and the

¹¹ The only exception is the upper north district (located in an extreme part of Sweden in terms of weather and geographical conditions), where strikes were relatively less frequent. Table A1 in the appendix gives a more detailed description of the number of conflicts per district and year.

¹² Sometimes the word was followed by a qualification, for example a statement that the compromise was mainly in favour of the workers. In this paper, we have not differentiated between various kinds of compromises.

stated result.¹³ All cases where the original demands of a party were not completely fulfilled were classified as a compromise. For example, when saw mill workers in Luleå in 1917 received a 10 percent wage increase after having demanded a 20 percent increase we have classified it a compromise.¹⁴

Classification of conflict outcomes in terms of compromises is common in historical strike data (cf. Card & Olson, 1995: 32–33; Geraghty & Wiseman, 2011: 519). Theoretically, one may question what a compromise outcome actually mean. Would not both parties in a conflict have reasons to enter negotiations with much more far-reaching claims than what they are eventually ready to accept? If so, many reported compromises may actually be wins or losses. In response to this objection, we may refer to the fact that historical actors, including the statisticians at the Board of Trade, obviously thought that it was both possible and relevant to classify conflict outcomes. We may also add a theoretical mechanism that served to restrain unrealistic claims. In the case of labour conflicts, and especially so in a setting such as Sweden in our period of study, negotiators are typically agents representing either workers or firms. If they enter the conflict with too high claims, they also find it difficult to explain the final outcome of the conflict to their principals (see Ashenfelter & Jonhson 1969). This mechanism makes us to believe that compromises indeed are meaningful outcomes of labour conflicts.

¹³ This was done in 653 cases. The results in the subsequent analysis are not sensitive to excluding these cases.

¹⁴ Such details about the size of wage increases/decreases are, however, exceptions in the Swedish statistics on work stoppages.

<Figure 3 about here>

On average for the whole period, 41 percent of the work stoppages end in a compromise. As seen in figure 5, there is a positive long-term trend of compromise outcomes in strikes, similar to United States in the same period (Geraghty & Wiseman, 2011: 521), but with a clear difference: the level of compromises, both in the beginning and end of the period, was considerably higher in Sweden (in most years by 10-20 percentage points). The depression of 1920-22 stands out in the figure. The downturn is associated with the end of the First World War and deflationary pressure and has been described as one of the larger crisis in Swedish economic history (Schön, 2010). In 1921, GDP fell by 5 percent, with exports, investments and industrial production being the components most affected. The economic downturn was accompanied by a tougher situation on the labor market as we see a marked drop in the share of compromises. However, as the 1920s progressed and the economy was brought back on track, the previous higher share of compromises were restored. In 1927, about half of the work stoppages ended in a compromise outcome.

<Figure 4 about here>

We have coded the information on conflict cause into nine categories, whose absolute and relative frequencies are summarized in table 1. The first column of the table shows the full period, while the next two columns separate the period before and after the crisis of the

early 1920s. Most work stoppages in our dataset were initiated by demands for wage increases. The share of strikes for wage increases was larger during the inflationary period up to 1920, whereas the share of strikes against wage decreases became relatively more frequent after 1920, with a particular concentration in the crisis years 1920-22. In the same years, the share of conflicts relating to demands for wage increases dropped substantially. Also, the share of conflicts concerning multiple issues decreased whereas causes classified as ‘other issues’ increased.¹⁵ Conflict causes other than those mentioned above were relatively stable over time.¹⁶

<Table 1 about here>

5. Empirical strategy and results

In this section we analyse the importance of mediation on the probability of compromise outcomes in work stoppages. Our analysis proceeds in the following steps. We begin in section 5.1 by studying the relationship between compromise outcome and control

¹⁵ This category includes a variety of issues, for example those regarding managerial prerogatives, understanding of existing agreements and wage system (time rates vs piece rates).

¹⁶ This also applied to conflicts over working hours, in spite of the political importance of the issue. Like many other countries, Sweden introduced a legislation of working hours in 1919. As noted by Bengtsson and Molinder, (2017), this reform was preceded by union demands for higher hourly wages. In our dataset, most of these cases are found in the category multiple causes.

variables, such as duration, size and cause of work stoppage, without including mediation. In section 5.2 we include mediation in the equations, first directly and thereafter using geographical distance between the work stoppage and the location of the mediator as an instrument. In section 5.3 we conduct a number of robustness checks, including clustering of standard errors on parish level, controlling for the administrative status of locations and comparing the outcomes of the first years of mediation with the outcomes of work stoppages during the years before the Mediation Act. In section 5.4 and 5.5 we explore whether mediators were neutral and if there were individual differences between the mediators.

5.1 The nature of conflicts and compromise outcomes

In table 2 we present the general picture of compromise outcomes and their correlates in our 4752 work stoppages 1907-27. The regression to be estimated has the following function:

$$Compromise_i = f(duration_i, size_i, cause_i, organization_i)$$

where the subset i refers to each stoppage in our sample. The dependent variable is dummy taking on the value of 1 if the conflict ended in a compromise, duration measures number of days in conflict, size measures the number of workers (in 1000's) involved in strike or lockout and organization is a dummy taking on the value 1 if both sides of the conflict were

organised (i.e. unionised or belonged to workers association). Descriptive statistics for the variables are found in table A2 in the appendix.

We see duration and size as measures of conflict costs, which we expect to be positively associated with compromise outcome.¹⁷ The cause of the conflict may influence the likelihood of compromise outcome in either direction. Generally, we expect struggles over rewards that are divisible, such as workers' demands for a wage increase or conflicts that involve many issues, to be associated with compromise outcome.

In addition to theoretically motivated variables, we control for time, geography and industry specific factors by including dummies for years, industry codes (following SNI-code system, dividing the stoppages into 14 main industries) and county or district level.

Since reaching a compromise outcome is a binary choice variable we run regressions as probit models and as OLS alternatively. There are no qualitative differences between the models estimated using probit or OLS, suggesting that the correlates of compromises can be reasonably well estimated using OLS. For presentation purposes we include one probit

¹⁷ We have chosen to include duration and size, since they appear in the theoretical Hawk-Dove model as specified by (Gerarchy and Wiseman 2011). See appendix A1. However, there is a concern that duration and size should also be seen as outcome variables of the strike, or at least determined jointly with the strike outcome. If so, they could be considered "bad controls" since they themselves are outcome variables. There is a concern that these controls are picking up some of the effect we are interested in (see Angrist and Pischke 2009: 64-68). We have decided to keep them in the regression to control for as much as possible, but the results qualitatively the same if we drop them from the regression.

specification in column 1, but focus on showing alternative specifications controlling for time, county and industry in column 2-5. OLS was chosen as an alternative for ease of interpretation, although probit estimations yield similar results.

<Table 2 about here>

Table 2 shows that the nature of the stoppage, i.e. its cause, duration and size largely determined whether a conflict ended in a compromise or not. Compromises were, positively associated with longer stoppages, larger stoppages (measured as the number of workers on strike or the number of workers on lockout) and whether both sides on the conflict were organized. The coefficients measuring the size of the conflict (1000s of workers in strike or lockout) are statistically significant and have the expected positive sign. These results are in line with the descriptive evidence presented by Mikkelsen (1992) and suggest that compromises were the likely outcomes when workers and employers were of similar strength. If one party had the upper hand in the conflict, for example if only the workers were organized in unions but the employer remained unorganized, conflicts were more likely to end quicker and involve less workers.

In addition, the cause of the conflict was critical in determining the probability of reaching a compromise. We have divided the causes of the work stoppages into the nine categories that appeared in table 1. The reference category is wage increase, which was the most common cause of conflict as previously mentioned. Apart from conflicts with multiple causes, work stoppages that started as a quest for wage increase were also the most likely to end in compromises, followed by the issues of wage decreases and working

hours. Conflicts about collective agreements had a medium likelihood of resulting in a compromise, perhaps since collective agreements contain many different aspects of the employment relationship that may be negotiated. Conflicts about personal matters, against layoffs and other issues were less likely to end in compromise. Conflicts that concerned one of the fundamental rules of the game – the right to organize – were the least likely of all to end in a compromise. Combining the evidence of the changing nature of compromise causes in the 1920s from table 2 with estimated coefficients explained above suggests that, all else equal, the 1920s offered a tougher situation on the labor market in terms of reaching compromise outcomes, since the nature of conflict causes had largely shifted from concerning wage increases to other matters.

These results are robust through a number of differing specifications (probit, OLS, with yearly controls, industry controls and with county controls) and paints a picture of compromises being more likely whenever the nature of the conflict was negotiation on a continuous scale (wages, hours) than whenever the outcome was more binary (right to organize or not, etc.).

5.2 Identifying the causal effect of mediation

In table 3, a dummy that equals 1 whenever a mediator was present at the conflict is added to the baseline specification in equation 1. The estimated marginal effect of mediation is an increase in the probability of compromise by about 17-18 percent in the probit model in column 1. The estimated coefficients in the OLS models in column 2-5 are of a similar size. The effect is robust to adding year, industry and county dummies. Thus, even if the probability that a conflict ended in a compromise was largely determined by the

nature of the conflict (its duration, size and cause), inviting a third party mediator had an additional positive and significant effect.

However, correlation is not causation. A potential concern is that work stoppages involving a mediator, exhibited some other unobservable characteristic that made them more likely to reach a compromise. For example, the mediator was perhaps only invited to participate in conflicts where the parties were quite close to reach a compromise anyway. Since the mediators had no legal rights to force their services on anyone, the fact that the mediator was allowed to mediate may in itself signal some kind of willingness to reach a compromise between the parties.

Thus, in order understand whether mediators actually played a causal role in conflict resolution, we need to look for some measure that is correlated with mediation but uncorrelated with the source of conflict or other confounding factors that may influence the likelihood of reaching a compromise. Here we take advantage of the geographical demarcation of the mediation districts.

As mentioned in section 3, the mediators were responsible for one district each and were paid a moderate sum for their work. Since Sweden is a vast country, it is not improbable that a mediator with limited access to resources would allocate more of his time to conflicts at a closer distance. Information access is also likely to travel slowly and if the conflict occurred far from the mediator's home, it may have taken longer time before mediators were aware of it.

<Table 3 about here>

We have calculated distances between each work stoppage and the district mediator's residency by geocoding all locations in our database¹⁸. From official sources, we know about the mediator's place of residency¹⁹ and we then calculate the straight line distance from the place of stoppage to mediator's residency. Admittedly, the straight line distance does not take actual travelling routes and time into consideration. Yet, we still think it yields a reasonable approximation of the actual travelling distance.

Figure 5 shows a boxplot over the distance between the place of the stoppage and the mediator's place of residence. As seen from the boxplot, the median distance is lower for stoppages involving mediation. In addition, the relationship is clearly non-linear, with mediation occurring to a much larger extent at short distances from the mediator's residency.²⁰ Shorter distances probably meant that the mediator could reach the conflict within one office day's travel back and forth, and conflicts occurring at such short distance were thus probably more likely to be visited by a third party. At very long distances, for example when exceeding 100 kilometres, there is not much discernible difference. This seems intuitively right since longer distances involved a decision to travel, potentially by train or coach, to seek out the parties. Once, the decision to travel by train was taken, whether the distance was 100 or 200 km may have played less role. Thus we may consider stoppages

¹⁸ <http://www.findlatitudeandlongitude.com/batch-geocode/#.VzLxH4SLTmE>

¹⁹ Meddelanden från K. Kommerskollegii Afdelning för Arbetsstatistik, årg. 1910: 30; Sveriges befolkning 1910.

²⁰ The entire sample shows that occurring in 31 percent of all the strikes if they took place where the mediator lived, compared to the total average of mediation in 18 percent of the strikes.

occurring closer to mediator's residency as being more likely to fall under the influence of mediation than stoppages further away with a non-linear relationship between distance and probability of mediation.

<Figure 5 about here>

A potential concern is of course that the location of the mediators could be correlated with some unobservable factor that also affects compromises, for example if mediators generally lived in cities, and strikes in cities have a larger chance of ending in compromise. As seen in section 3, however, the appointment of mediator relied solely on the person and his qualities, regardless of place of location. In addition, in several instances the place of mediation changed as a new mediator was appointed. In order to further test this assumption we will carry out some robustness exercises, controlling for compromises with a city dummy and using a placebo instrument, in section 5.3.

In table 4 we use the logarithmic distance to the mediator's residency as an instrument for mediation. In order to save space, we only present the IV-reg results for the model's second stage.²¹ In models 1-3 we present the results of the second stage of the regression, when mediation was instrumented with the log of distance to the residency of a mediator. We add year, industry and county controls. We also try a dummy for whether the work stoppage took place at the residency of the mediator as an instrument. The result from these regressions is presented in models 4-6.

²¹ IV probit estimations yield similar results.

There is a significant negative correlation between the log of distance to the mediator's residency and the occurrence of mediation at a work stoppage. The F-statistic of the first stage in all specifications is far above the 5 percent critical value as defined by Stock and Yogo (2005) at 16.38. The instrumented coefficients of mediation are reported in the first row in the table. Comparing IV-estimates to the baseline estimates in table 3, it is evident that the instrumented effects become larger (point estimates between 0.32-0.48 compared to previously estimated 0.18-0.19). An estimated 30 percent increase in compromise outcomes due to mediation shows significant impact in both economic and statistical terms.

There are some possible reasons why instrumented effects are larger than the baseline estimates using OLS. The first is that the mediators actively approached conflicts with lesser prospects of finding compromises. If there is a negative bias in the OLS-estimate of mediation, then the instrumented coefficients will yield a larger effect. Secondly, the instrument might capture some of the beneficent effects on compromises that are not solely transmitted through the act of mediation. For examples, mediators might have visited the work place without actually mediating formally in the conflict, still mediators presence might nudged parties into compromising mood and it is also possible that the mediator could have helped in carrying information to the striking parties. Third, there might have been an underreporting of whether mediation actually took place at the work stoppage, especially in cases that were not clear-cut as the one described above. Finally, there is the potential that mediators could have positive local spill-over effects on compromises even in conflicts where they did not actively take part as mediators.

<Table 4 about here>

5.3 Placebo-effects of mediation

A potential concern is that the geographical instrument of mediation is picking up some unobservable factor that drives compromises and is spuriously correlated with the mediator's location. To try to address this issue, we construct a placebo sample of work stoppages utilizing the fact that the collection of statistics of work stoppages in Sweden began in 1903, thus four years before the institution of mediation was introduced. In this placebo sample, we have geocoded all locations of work stoppages taking place between 1903 and 1907. We have also added a placebo-treatment of mediation by constructing dummies measuring the log of distance to the future mediators of 1907's residency. If our instrument is really picking up the causal effect of mediation, we would not expect to see any significant correlation between the placebo-treatment and the compromise dummy *before* the institution was actually put in place. Any correlation between the share of strikes ending in a compromise and the distance to a future mediator's home should only be driven a higher chance of strikes near mediator's homes having a larger probability of receiving mediation. Thus, this placebo test is intended as a demonstration of the exclusion restriction of our instrument.²²

²² Naturally, this is not a formal test of the exclusion restriction, since such tests are not available. The placebo check merely serves to demonstrate that the instrument has no other effect on the outcome variable than through the first stage-channel (Angrist and Pischke, 2009: 117). The

In table 5 we show the regressions of the “placebo distance” dummy on the share of compromises in work stoppages between 1903 and 1906 (column 1). We also add a placebo dummy to test if there are any spurious correlation between share of compromises and the location of a future mediator’s residency between 1903 and 1906 (column 4). All specifications include the same variables as our baseline regression, including controls for year, industry and geography.²³ In column 1, the correlation between the distance to the mediator and the compromise dummy is insignificant for 1903-1906, indicating the absence of any spurious correlation between future mediator’s homes and the compromise dummy. However, after the mediation act of 1907 the coefficient takes on the expected negative sign (a larger distance to a mediator’s location should reduce the probability of receiving mediation, hence the probability of a compromise outcome) and significant at 10 percent level, as displayed in column 2.²⁴ Thus, it seems that the fact that the work stoppage

assumption that the instrument is as good as randomly assigned (conditional on covariates) is justified in section 2 of the paper where the process leading to the assignment of the mediators’ locations is explained.

²³ However, before 1907 there are no instances reported for which both parties were organized, so we have to drop this explanatory variable for the placebo sample.

²⁴ Since the placebo mediation only covers the four closest preceding years to the initiation of the mediator’s office, we also add the impact of the location of the office for the following four years for comparison (1907-1911) in column 2. The effect is smaller, but still with the right sign, and more precisely estimated in terms of standard errors, if we look at the impact of location on office for the full period with mediation (1907-27), see columns 3 and 6.

took place at the same location as a mediator is picking up some real effect that influences compromises after 1907. In column 4 we show that the work stoppages that took place in a future mediator's location were not correlated with the compromise outcome 1903-1906. Column 5 shows that they became positively correlated 1907-1911 (at 10 percent level). Thus, the placebo checks suggest that any correlation arising between mediators' location and the compromise variable only takes the expected sign after 1907, i.e. after mediator's location actually start to proxy for the chance of a work stoppage receiving mediation. We believe that these check shows that the distance to a mediator's location only affects the chance of a compromise outcome *after* the institution of mediation was actually put in place.

However, one might still worry that the fact that the majority of mediators resided in cities might influence the results, at least if there are some unobservable compromise-willingness that is larger in cities than elsewhere that we are unable to capture in our model. Yet, recall from the map in figure 2 that the mediators did change locations on several occasions. In addition, there are many cities in our sample that were never were not "treated" with the presence of a mediator. Controlling for whether the location of the work stoppage was a city does neither influence the chance of reaching a compromise nor change the size and significance of mediation. The results are displayed in table A4 in the appendix. Similarly, one may worry that observation units are not independent over space, if spatial autocorrelation may influence the results. We address this issue by clustering the

standard errors at various geographical levels, but this does not change any of our obtained results. The results are found in table A5 in the appendix.²⁵

<Table 5 about here>

5.4 Were mediators neutral?

The dependent variable in the regressions is the likelihood of a compromise outcome, i.e. that a conflict is neither won nor lost by either part. With this design we do not know if the mediators tended to favor any of the parties, as is said to have been the case in France (Friedman 1988) and Spain (Domenech 2006). This issue is interesting in the light of previous claims made by Nycander (2017), Rothstein (2005) and Lapuente and Rothstein (2014) that the Swedish state was relatively impartial. To find out more about this issue we run two additional regressions, replacing the compromise dummy as dependent variable for a dummy indicating if the strike was either lost or won by the workers. The results are presented in table 6.

<Table 6 about here>

²⁵ We cluster at the level of 24 counties and 812 geocodes. However, clustering at county level is not optimal since the number of clusters is insufficient to calculate robust covariance matrix, as shown by for example Cameron et al (2008).

The top panel of the table displays how mediation correlates with workers losing a strike, using the same battery of specifications and controls as in table 3. As seen from the table, strikes with mediation were about 12 percent less likely to end in workers losing. Similarly, from the bottom panel, we may infer that workers were about 5 percent less likely to lose a strike with mediation. Both effects are statistically significant and suggests that the Swedish mediators were instrumental in transforming both losses and wins into compromises, although the coefficient for workers loss is slightly larger, suggesting if anything a certain bias towards the workers' side.

The above-mentioned example naturally suggests that both workers and firms were less likely to invite mediators to conflicts that they estimated having large changes of winning without interference. In table 7 the instrumented coefficient on mediation appears. While the instrumented coefficients have large standard errors compared to the once in table 6 that were estimated using OLS, they do show that mediation appears to have reduced the likelihood of workers losing the strikes while increasing the chance that they won it. The coefficient of workers winning is however not statistically significant in the specifications when county and industry effects are controlled for. It is very improbable that contemporary observers noted these patterns.

<Table 7 about here>

5.5 Were all mediators equally successful?

Table A3 in the appendix lists the 25 mediators by district and the time they held office. The table shows that some mediators were in office several years, while others had shorter

appointments. It is beyond the scope of this paper to analyse the determinants of appointment and tenure of the mediators, but it might be reasonable to assume that their perceived “success rate” (in terms of finding solutions to conflicts agreed to by both parties) could be influence tenure time. The share of compromise and the number of conflict be each mediator is shown in figure 6. The figure shows that number of strikes differed between mediators, but typically the shares of compromise outcomes were between 20 and 40 per cent for each mediator.

We may however expect that individual characteristics mattered in the process of finding compromise solutions. Some people may just be more skilled in the work of calming down heated emotions and finding compromise solutions than others. Referring to the years around World War I, three individual mediators have, for example, been mentioned as “the great canons” (Schmidt 1956: 756). In order to test how much of the estimated effects in table 4 that can be explained by individual heterogeneity among mediators, we add 25 dummies (one for each mediator) to control for these individual effects. The resulting coefficient of mediation measure the average effects of receiving mediation, after we have controlled for individual heterogeneity. The results are found in table 8 and shows that the instrumented coefficient of mediation reduces to a magnitude of 0.13-0.14. Yet, controlling for individual heterogeneity also reduces the standard errors of the estimates markedly, resulting in a statistically significant effect even after the abilities of individual mediators have been controlled for. The results plausibly shows that, in line with our theoretical model, there is a positive effect of mediation on compromise outcomes that is independent of the individual abilities of the mediators. Yet, the results also show that personal skills mattered in the job. The point estimate of the dummy coefficients

pertaining to individual mediators are sometimes larger than the average effect of receiving mediation (although standard errors are large too). However, mediators may have differed in strategies and how effective they were in reducing the chance of a conflict taking place in the first place. Thus if some mediators were more efficient in preventing conflicts, they might have had a harder job finding compromise solutions to those conflicts that nevertheless did erupt.

6. Conclusions

In this paper we analyse the impact of mediation on work stoppage outcomes in Sweden 1907-1927. The context may be described as one with frequent conflicts, here defined as work stoppages, of long duration, often between relatively well-organized parties (trade unions and employers' organizations). The Swedish mediators were personally appointed, enjoyed high levels of social prestige, and were responsible for conflict prevention and resolution within geographical districts, but had limited authority and access to economic resources. The mediators relied on the parties' willingness to negotiate and their remuneration was not related to the actual costs of their mission.

Yet, we find that the involvement of a mediator was significantly associated with a compromise outcome. The causal effect of mediation that we establish in this paper is not obvious. Among researchers, there are diverging opinions about the impact of mediation, and similar institutions. When mediation was discussed in early twentieth century Sweden, many observers doubted that it would have any effect. This paper has shown that the mediators in the Swedish setting actually made a difference; in the short run, they helped to settle work stoppages. We find it likely that short-run successes in turn

had long run implications, for example in making conflicts less likely to occur and contributing to the transition from a state of violent antagonism in the labor market, to an atmosphere where negotiations became a natural way of solving conflicts.

As it was designed, the Mediation Act is a clear illustration of how the Swedish state tried to stay neutral in labour market conflicts. Our study suggest, that mediation was more associated with compromises than any of the parties winning it. The mediators were not necessarily without political connections, but they managed to remain impartial and gain the confidence of both workers and employers. This sets Sweden apart from countries such as Spain and France, where conflict resolution was handled by politically appointed agents whose actions tilted the balance of power in the labour market.

To sum up, mediation is an overlooked aspect of the pre-history of the Swedish Model. When the labor market parties in 1938 signed a General Agreement, it had not only been preceded by some years of lower conflict frequency, but those conflicts that actually broke out ended remarkably often in some kind of compromise.

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Figure 1. Number of total work stoppages and the number of stoppages receiving mediation

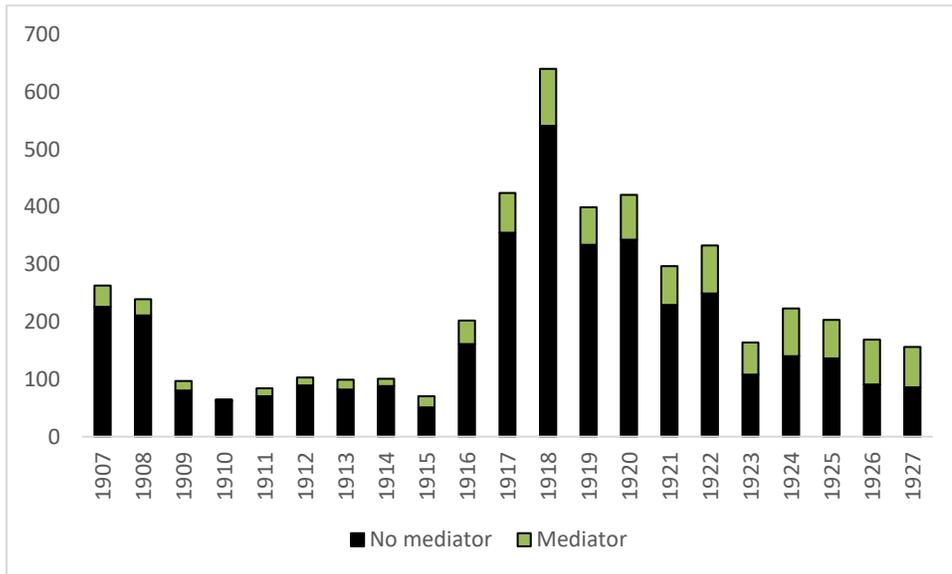


Figure 2. Map of the mediators districts and the location of the mediator's residency

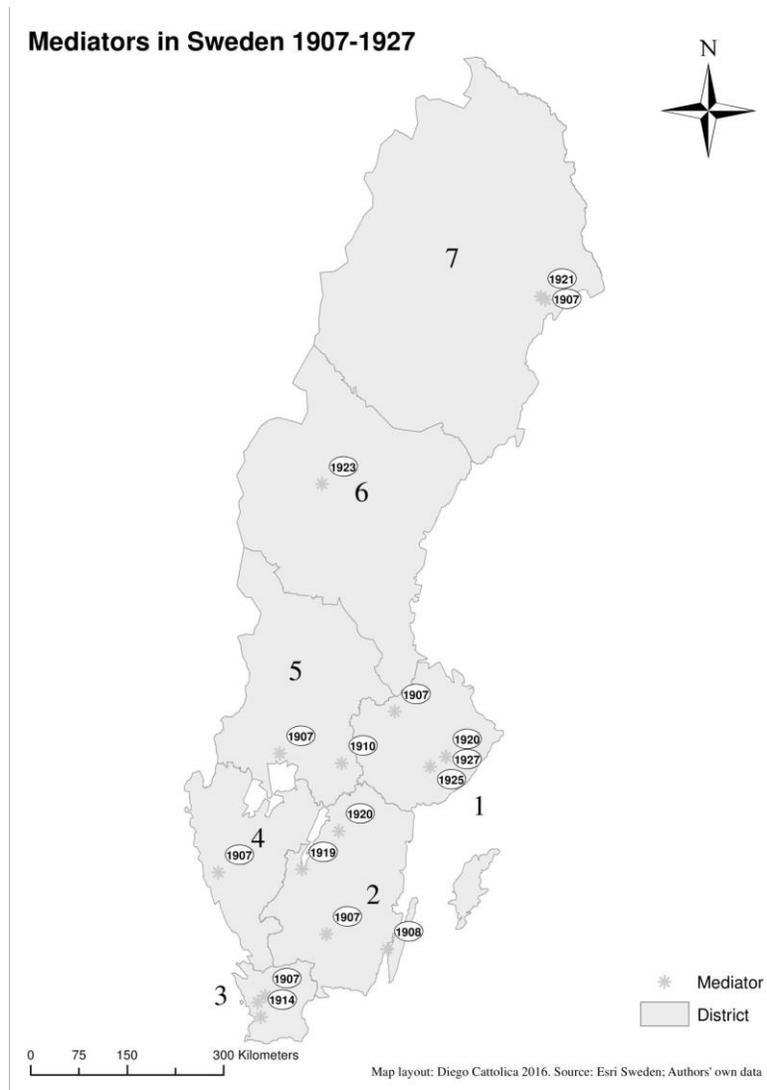


Figure 3. Work stoppages in Sweden 1907-1927

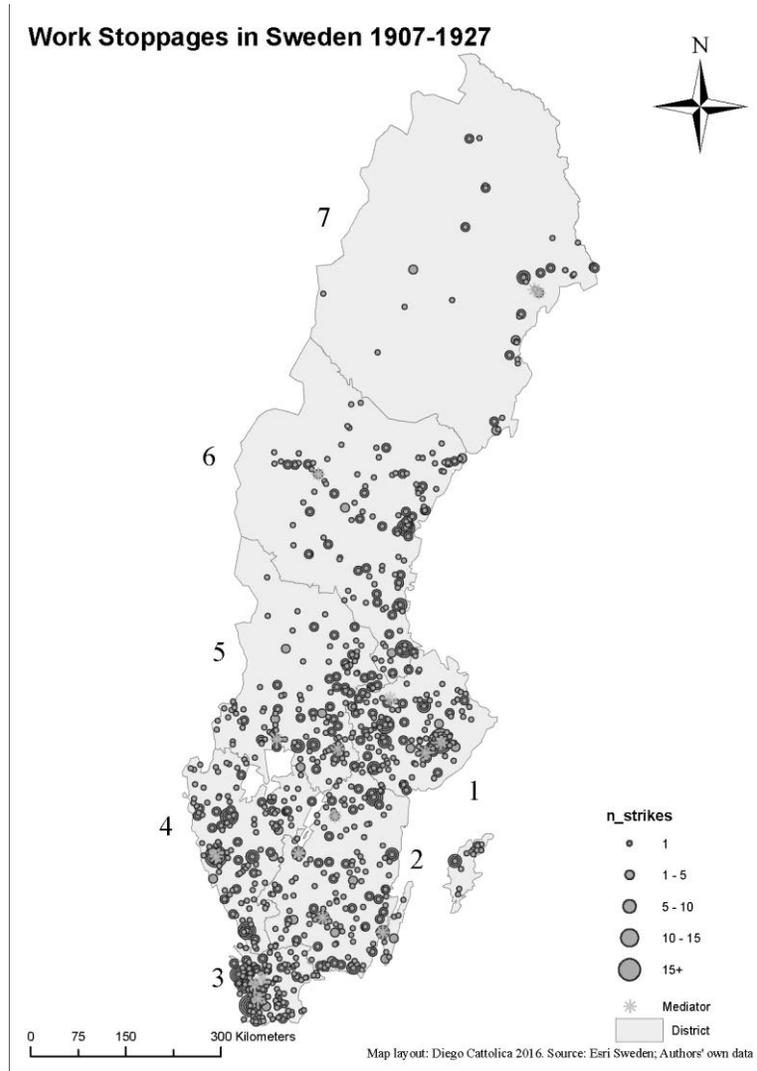


Figure 4. Share of Compromise in conflict outcomes

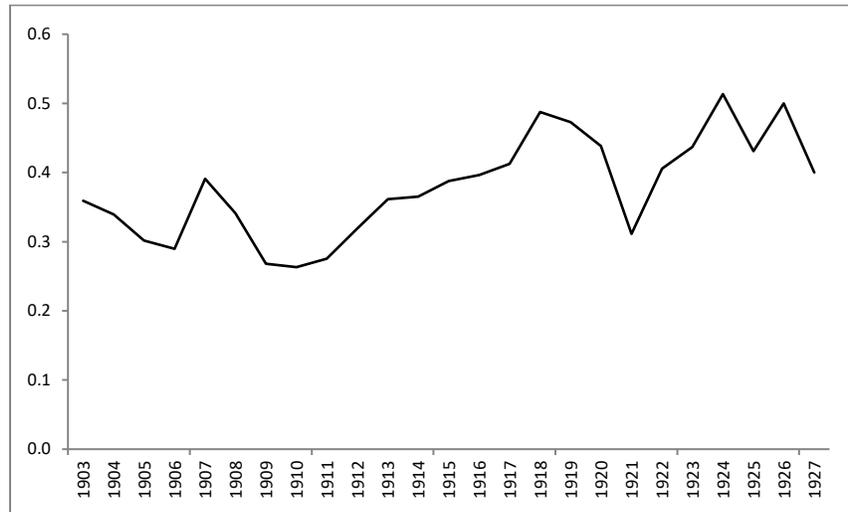


Figure 5. Boxplot over distance between conflict location and mediator residency in conflicts without mediation (=0) and involving mediation (=1). Kilometres (Top panel) and in logarithms (bottom)

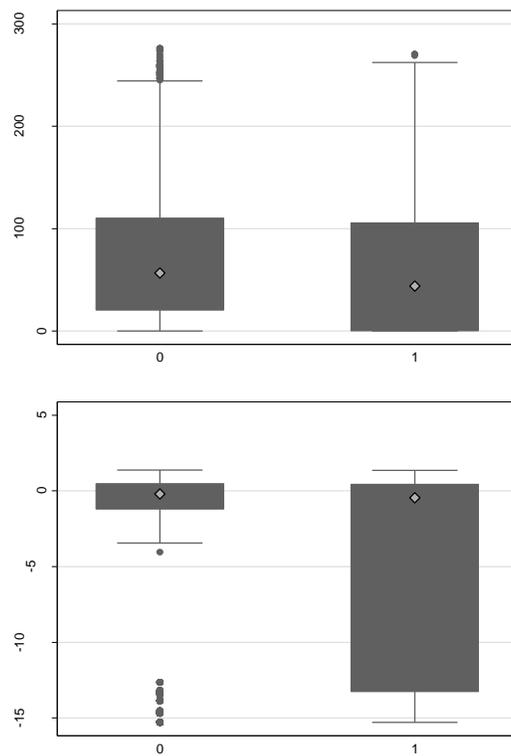


Figure 6. Share of compromise outcomes per mediator (numbered 1 to 25 as listed in table A3 in the appendix). Dummy equals 1 if conflict ended in a compromise.

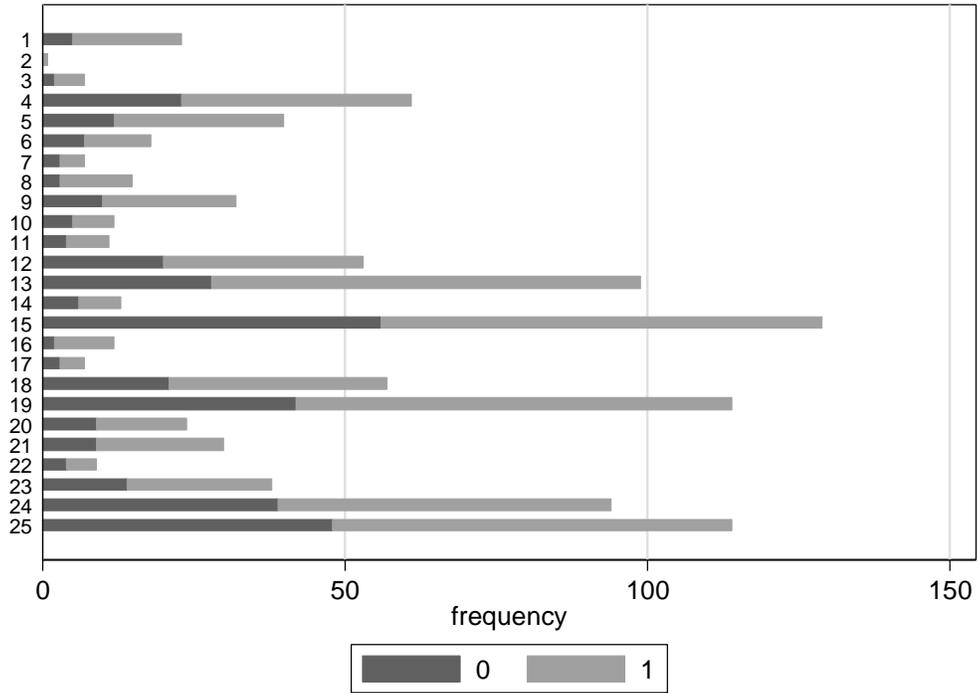


Table 1. Conflicts by cause (N and shares in percent) and sub-period

VARIABLES	1907-1927		1907-1919		1920-1927	
	N	%	N	%	N	%
For wage increase	2399	50	1605	58	794	40
Against wage decrease	544	11	79	3	465	24
Right to organize	33	1	18	1	15	1
For collective agreement	344	7	218	8	126	6
Against layoffs	293	6	158	6	135	7
Working hours	121	3	74	3	47	2
Personal issues	123	3	83	3	40	2
Other issues	582	12	293	11	289	15
Multiple causes	313	7	258	9	55	3

Source: Authors' calculation based on Statistics Sweden. Arbetsstatistik. E, Arbetsinställelser i Sverige.

Table 2. Correlates of compromise outcomes, 1907-27

VARIABLES	(1) Probit	(2) OLS	(3) OLS	(4) OLS	(5) OLS
ln(duration)	0.159*** (0.015)	0.059*** (0.005)	0.057*** (0.005)	0.057*** (0.006)	0.057*** (0.006)
Striking workers	0.480*** (0.144)	0.052*** (0.020)	0.051** (0.020)	0.050** (0.020)	0.051** (0.020)
Lockout workers	0.853 (0.609)	0.265 (0.220)	0.206 (0.255)	0.198 (0.255)	0.149 (0.256)
Both organized	0.091** (0.039)	0.039*** (0.014)	0.042*** (0.015)	0.043*** (0.015)	0.040*** (0.015)
Wage decrease	-0.231*** (0.061)	-0.087*** (0.023)	-0.038 (0.027)	-0.039 (0.027)	-0.053* (0.028)
Right to organize	-1.113*** (0.267)	-0.379*** (0.083)	-0.353*** (0.083)	-0.365*** (0.084)	-0.376*** (0.084)
Collective agreement	-0.555*** (0.077)	-0.207*** (0.027)	-0.210*** (0.028)	-0.214*** (0.028)	-0.216*** (0.028)
Lay offs	-0.637*** (0.084)	-0.232*** (0.029)	-0.226*** (0.030)	-0.225*** (0.030)	-0.233*** (0.030)
Working hours	-0.315*** (0.120)	-0.118*** (0.044)	-0.111** (0.044)	-0.111** (0.044)	-0.111** (0.045)
Personal	-0.825*** (0.134)	-0.284*** (0.044)	-0.284*** (0.044)	-0.285*** (0.044)	-0.292*** (0.044)
Other	-0.727*** (0.064)	-0.259*** (0.022)	-0.250*** (0.023)	-0.250*** (0.023)	-0.252*** (0.023)
Multiple	0.180** (0.077)	0.070** (0.029)	0.066** (0.029)	0.064** (0.029)	0.063** (0.029)
Constant	-0.471*** (0.048)	0.332*** (0.017)	0.386*** (0.033)	0.350*** (0.039)	0.365*** (0.043)
Observations	4,752	4,752	4,752	4,752	4,752
R-squared		0.088	0.097	0.101	0.105
Year dummies	NO	NO	YES	YES	YES
County dummies	NO	NO	NO	YES	YES
Industry dummies	NO	NO	NO	NO	YES

Note: Standard errors are in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

Table 3. Mediation correlation with compromise

VARIABLES	(1) Probit	(2) OLS	(3) OLS	(4) OLS	(5) OLS
Mediation	0.507*** (0.049)	0.193*** (0.018)	0.189*** (0.018)	0.188*** (0.018)	0.187*** (0.018)
ln(duration)	0.117*** (0.015)	0.042*** (0.006)	0.041*** (0.006)	0.041*** (0.006)	0.042*** (0.006)
Striking workers	0.228 (0.147)	0.032 (0.020)	0.033 (0.020)	0.033 (0.020)	0.035* (0.020)
Lockout workers	0.378 (0.632)	0.094 (0.218)	0.129 (0.252)	0.121 (0.253)	0.075 (0.253)
Both organized	0.071* (0.039)	0.028** (0.014)	0.027* (0.015)	0.029* (0.015)	0.027* (0.015)
Wage decrease	-0.254*** (0.061)	-0.094*** (0.022)	-0.041 (0.027)	-0.042 (0.027)	-0.052* (0.027)
Right to organize	-1.127*** (0.268)	-0.377*** (0.082)	-0.355*** (0.082)	-0.368*** (0.083)	-0.374*** (0.083)
Collective agreement	-0.584*** (0.077)	-0.214*** (0.027)	-0.217*** (0.028)	-0.218*** (0.028)	-0.217*** (0.028)
Lay offs	-0.633*** (0.084)	-0.227*** (0.029)	-0.216*** (0.029)	-0.215*** (0.029)	-0.221*** (0.030)
Working hours	-0.309** (0.121)	-0.115*** (0.044)	-0.109** (0.044)	-0.109** (0.044)	-0.106** (0.044)
Personal	-0.812*** (0.135)	-0.275*** (0.043)	-0.271*** (0.044)	-0.272*** (0.044)	-0.277*** (0.044)
Other	-0.695*** (0.064)	-0.245*** (0.022)	-0.231*** (0.022)	-0.230*** (0.022)	-0.232*** (0.023)
Multiple	0.189** (0.078)	0.071** (0.028)	0.064** (0.028)	0.062** (0.029)	0.064** (0.029)
Constant	-0.437*** (0.048)	0.342*** (0.017)	0.397*** (0.033)	0.357*** (0.039)	0.367*** (0.042)
Observations	4,752	4,752	4,752	4,752	4,752
R-squared		0.111	0.118	0.121	0.125
Year dummies	NO	NO	YES	YES	YES
County dummies	NO	NO	NO	YES	YES
Industry dummies	NO	NO	NO	NO	YES

Note: Standard errors are in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

Table 4. IV estimates for mediation on compromise outcomes

VARIABLES	(1) IV distance	(2) IV distance	(3) IV distance	(4) IV same location	(5) IV same location	(6) IV same location
Mediation	0.330*** (0.128)	0.478*** (0.182)	0.485** (0.193)	0.324** (0.134)	0.474** (0.211)	0.476** (0.222)
ln(duration)	0.030*** (0.012)	0.018 (0.016)	0.017 (0.017)	0.031** (0.012)	0.018 (0.018)	0.018 (0.019)
Striking workers	0.019 (0.023)	0.006 (0.026)	0.010 (0.026)	0.020 (0.024)	0.007 (0.028)	0.010 (0.028)
Lockout workers	0.071 (0.258)	0.002 (0.268)	-0.042 (0.269)	0.073 (0.258)	0.004 (0.272)	-0.039 (0.272)
Both organized	0.017 (0.018)	0.007 (0.020)	0.006 (0.020)	0.017 (0.018)	0.007 (0.022)	0.006 (0.022)
Wage decrease	-0.043 (0.027)	-0.046* (0.028)	-0.051* (0.028)	-0.043 (0.027)	-0.046 (0.028)	-0.051* (0.028)
Right to organize	-0.356*** (0.083)	-0.373*** (0.085)	-0.370*** (0.085)	-0.356*** (0.083)	-0.373*** (0.085)	-0.370*** (0.085)
Collective agreement	-0.221*** (0.028)	-0.225*** (0.029)	-0.220*** (0.029)	-0.221*** (0.028)	-0.225*** (0.029)	-0.220*** (0.029)
Lay offs	-0.209*** (0.030)	-0.198*** (0.032)	-0.203*** (0.032)	-0.209*** (0.030)	-0.199*** (0.032)	-0.203*** (0.033)
Working hours	-0.109** (0.044)	-0.106** (0.045)	-0.097** (0.045)	-0.109** (0.044)	-0.106** (0.045)	-0.098** (0.045)
Personal	-0.262*** (0.045)	-0.252*** (0.046)	-0.255*** (0.047)	-0.262*** (0.045)	-0.252*** (0.047)	-0.255*** (0.048)
Other	-0.217*** (0.026)	-0.199*** (0.030)	-0.200*** (0.031)	-0.218*** (0.026)	-0.200*** (0.032)	-0.201*** (0.033)
Multiple	0.063** (0.029)	0.060** (0.029)	0.065** (0.029)	0.063** (0.029)	0.060** (0.029)	0.065** (0.029)
Constant	0.405*** (0.034)	0.368*** (0.040)	0.371*** (0.043)	0.404*** (0.034)	0.368*** (0.040)	0.370*** (0.043)
Observations	4,752	4,752	4,752	4,752	4,752	4,752
R-squared	0.107	0.074	0.076	0.108	0.075	0.079
Cragg-Donald						
Wald F-stat	95.398	49.06	44.564	36.35	36.348	33.23
(1st stage)						
Year dummies	YES	YES	YES	YES	YES	YES
County dummies	NO	YES	YES	NO	YES	YES
Industry dummies	NO	NO	YES	NO	NO	YES

Note: Standard errors are in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

Table 5. Placebo mediation

VARIABLES	(1) (placebo) 1903-1906	(2) 1907-1911	(3) 1907-1927	(4) (placebo) 1903-1906	(5) 1907-1911	(6) 1907-1927
ln(distance)	0.001 (0.005)	-0.010* (0.006)	-0.005*** (0.002)			
Same location				-0.043 (0.071)	0.137* (0.077)	0.059** (0.026)
ln(duration)	0.026** (0.012)	0.055*** (0.014)	0.059*** (0.006)	0.035*** (0.011)	0.061*** (0.014)	0.059*** (0.005)
Striking workers	0.186* (0.095)	0.382** (0.166)	0.055*** (0.020)	0.236*** (0.084)	0.361*** (0.138)	0.040** (0.017)
Lockout workers	1.638*** (0.595)	0.078 (0.394)	0.176 (0.254)	0.641** (0.307)	-0.093 (0.246)	0.127 (0.126)
Wage decrease	-0.257*** (0.088)	-0.186** (0.087)	-0.056** (0.028)	-0.248*** (0.085)	-0.181** (0.084)	-0.065** (0.027)
Right to organize	-0.437*** (0.130)	-0.339** (0.139)	-0.377*** (0.084)	-0.460*** (0.129)	-0.366*** (0.129)	-0.396*** (0.082)
Collective agreement	-0.071 (0.079)	-0.125** (0.063)	-0.223*** (0.028)	-0.094 (0.078)	-0.096 (0.061)	-0.221*** (0.028)
Lay offs	-0.200** (0.082)	-0.234*** (0.078)	-0.228*** (0.030)	-0.213*** (0.079)	-0.235*** (0.076)	-0.234*** (0.029)
Working hours	-0.005 (0.144)	-0.006 (0.103)	-0.105** (0.045)	-0.014 (0.144)	-0.005 (0.099)	-0.090** (0.043)
Personal	-0.177** (0.080)	-0.112 (0.122)	-0.286*** (0.044)	-0.193** (0.078)	-0.135 (0.118)	-0.289*** (0.043)
Other	-0.266*** (0.055)	-0.225*** (0.057)	-0.251*** (0.023)	-0.243*** (0.053)	-0.230*** (0.056)	-0.255*** (0.022)
Multiple	0.092* (0.049)	0.096 (0.065)	0.060** (0.029)	0.080* (0.047)	0.091 (0.063)	0.043 (0.028)
Constant	0.297*** (0.101)	0.185* (0.110)	0.328*** (0.046)	0.303*** (0.097)	0.184* (0.104)	0.333*** (0.045)
Observations	716	666	4,772	760	699	4,980
R-squared	0.151	0.143	0.105	0.157	0.154	0.105
Year dummies	YES	YES	YES	YES	YES	YES
County dummies	YES	YES	YES	YES	YES	YES
Industry dummies	YES	YES	YES	YES	YES	YES

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 6. Mediation correlated with non-compromise outcome (workers lost or workers won)

VARIABLES	(1) Probit	(2) OLS	(3) OLS	(4) OLS	(5) OLS
Outcome: Workers loss					
Mediation	-0.485*** (0.058)	-0.128*** (0.016)	-0.116*** (0.016)	-0.118*** (0.016)	-0.122*** (0.016)
Outcome: Workers won					
Mediation	-0.106* (0.055)	-0.048*** (0.016)	-0.050*** (0.016)	-0.049*** (0.017)	-0.043*** (0.017)
Observations	4,752	4,752	4,752	4,752	4,752
Year dummies	NO	NO	YES	YES	YES
County dummies	NO	NO	NO	YES	YES
Industry dummies	NO	NO	NO	NO	YES

Standard errors in parentheses.

Note: All regression includes the same controls as in table 3.

*** p<0.01, ** p<0.05, * p<0.1

Table 7. IV estimates for mediation on non-compromise outcomes

VARIABLES	(1) IV distance	(2) IV distance	(3) IV distance	(4) IV same location	(5) IV same location	(6) IV same location
Outcome: Workers loss						
Mediation	-0.601*** (0.124)	-0.602*** (0.173)	-0.534*** (0.178)	-0.561*** (0.129)	-0.609*** (0.201)	-0.520** (0.205)
Outcome: Workers won						
Mediation	0.361*** (0.124)	0.224 (0.167)	0.141 (0.173)	0.334** (0.130)	0.250 (0.195)	0.151 (0.201)
Observations	4,752	4,752	4,752	4,752	4,752	4,752
Year dummies	YES	YES	YES	YES	YES	YES
County dummies	NO	YES	YES	NO	YES	YES
Industry dummies	NO	NO	YES	NO	NO	YES

Standard errors in parentheses.

Note: All regression includes the same controls as in table 4.

*** p<0.01, ** p<0.05, * p<0.1

Table 8. The effects of mediation allowing for dummies for each mediator

VARIABLES	(1) IV distance	(2) IV distance	(3) IV distance	(4) IV same location	(5) IV same location	(6) IV same location
mediation	0.132*** (0.046)	0.144*** (0.050)	0.142*** (0.050)	0.132*** (0.046)	0.144*** (0.050)	0.142*** (0.050)
1.mediator	0.148 (0.109)	0.132 (0.113)	0.130 (0.113)	0.148 (0.109)	0.132 (0.113)	0.130 (0.113)
2.mediator	0.253 (0.468)	0.242 (0.470)	0.219 (0.470)	0.253 (0.468)	0.242 (0.470)	0.219 (0.470)
3.mediator	0.091 (0.185)	0.073 (0.186)	0.077 (0.187)	0.091 (0.185)	0.073 (0.186)	0.077 (0.187)
4.mediator	0.039 (0.076)	0.066 (0.081)	0.063 (0.081)	0.039 (0.076)	0.066 (0.081)	0.063 (0.081)
5.mediator	0.088 (0.087)	0.088 (0.087)	0.084 (0.087)	0.088 (0.087)	0.088 (0.087)	0.084 (0.087)
6.mediator	0.095 (0.125)	0.110 (0.128)	0.103 (0.128)	0.095 (0.125)	0.110 (0.128)	0.103 (0.128)
7.mediator	0.046 (0.184)	0.063 (0.186)	0.066 (0.186)	0.046 (0.184)	0.063 (0.186)	0.066 (0.186)
8.mediator	0.220* (0.130)	0.171 (0.135)	0.180 (0.135)	0.220* (0.130)	0.171 (0.135)	0.180 (0.135)
9.mediator	0.140 (0.093)	0.122 (0.103)	0.122 (0.103)	0.140 (0.093)	0.122 (0.103)	0.122 (0.103)
10.mediator	-0.044 (0.142)	-0.056 (0.150)	-0.058 (0.150)	-0.044 (0.142)	-0.056 (0.150)	-0.058 (0.150)
11.mediator	0.033 (0.149)	0.006 (0.152)	0.010 (0.153)	0.033 (0.149)	0.006 (0.152)	0.010 (0.153)
12.mediator	0.020 (0.079)	-0.002 (0.083)	-0.002 (0.083)	0.020 (0.079)	-0.002 (0.083)	-0.002 (0.083)
13.mediator	0.137** (0.066)	0.098 (0.073)	0.099 (0.073)	0.137** (0.066)	0.098 (0.073)	0.099 (0.073)
14.mediator	0.046 (0.142)	0.007 (0.145)	-0.010 (0.145)	0.046 (0.142)	0.007 (0.145)	-0.010 (0.145)
15.mediator	0.031 (0.062)	0.016 (0.068)	0.024 (0.068)	0.031 (0.062)	0.016 (0.068)	0.024 (0.068)
16.mediator	0.208 (0.144)	0.232 (0.146)	0.222 (0.146)	0.208 (0.144)	0.232 (0.146)	0.222 (0.146)
17.mediator	-0.148 (0.183)	-0.126 (0.186)	-0.120 (0.185)	-0.148 (0.183)	-0.126 (0.186)	-0.120 (0.185)
18.mediator	0.052 (0.076)	0.012 (0.082)	0.005 (0.082)	0.052 (0.076)	0.012 (0.082)	0.005 (0.082)
19.mediator	0.059 (0.062)	0.037 (0.068)	0.037 (0.068)	0.059 (0.062)	0.037 (0.068)	0.037 (0.068)

20.mediator	-0.018 (0.106)	0.007 (0.110)	0.011 (0.110)	-0.018 (0.106)	0.007 (0.110)	0.011 (0.110)
21.mediator	0.124 (0.097)	0.148 (0.101)	0.140 (0.101)	0.124 (0.097)	0.148 (0.101)	0.140 (0.101)
22.mediator	-0.120 (0.163)	-0.115 (0.163)	-0.109 (0.163)	-0.120 (0.163)	-0.115 (0.163)	-0.109 (0.163)
23.mediator	0.067 (0.089)	0.051 (0.094)	0.055 (0.094)	0.067 (0.089)	0.051 (0.094)	0.055 (0.094)
24.mediator	0.010 (0.065)	-0.010 (0.071)	-0.009 (0.072)	0.010 (0.065)	-0.010 (0.071)	-0.009 (0.072)
Constant	0.393*** (0.033)	0.346*** (0.040)	0.357*** (0.043)	0.393*** (0.033)	0.346*** (0.040)	0.357*** (0.043)
Observations	4,752	4,752	4,752	4,752	4,752	4,752
R-squared	0.121	0.124	0.127	0.121	0.124	0.127
Year dummies	YES	YES	YES	YES	YES	YES
County dummies	NO	YES	YES	NO	YES	YES
Industry dummies	NO	NO	YES	NO	NO	YES

Standard errors in parentheses.

Note: All regression includes the same controls as in table 4.

*** p<0.01, ** p<0.05, * p<0.1

Appendix

A1. A Hawk-Dove model

The game is illustrated in figure A1, for a case where the contested surplus is equal to 1.²⁶

In the game each party chooses whether to fight or compromise. If both parties choose compromise, each party receive the reward α . If both parties choose to fight, the surplus is outweighed by the costs of the conflict and the parties receive $-\beta$. Note that this outcome results in a war-of-attrition situation where the strike will only end when one of the parties have exhausted all their resources. If one party choose confrontation and the other concession, the surplus is unevenly divided; the conceding party shows weakness receives ε and the party that chooses to continue the fight who receives the rest of the surplus $(1-\varepsilon)$. The nature of the game is such that the best outcome for a party is obtained by showing strength (continue to fight) when the other is showing weakness (offer to compromise). The second best outcome for a party is obtained when both parties choose to compromise. The third best outcome is associated with sending a unilateral offer to compromise when the other parties decides to fight and the worst solution occurs when both parties choose to fight.

²⁶ This section is based on Geraghty and Wiseman (2011).

Figure A1. Illustration of a hawk-dove game in the labor market

		Workers	
		<i>Compromise</i>	<i>Fight</i>
Firm	Compromise	α, α	$\varepsilon, 1-\varepsilon$
	Fight	$1-\varepsilon, \varepsilon$	$-\beta, -\beta$

Source: Adapted from Geraghty & Wiseman (2011: 521).

Basically, the hawk-dove game suggests that the likelihood of compromise outcomes depends on the costs of conflict ($-\beta$), and the rewards for bilateral compromise (α) and unilateral compromise (ε), respectively. Third-party intervention in the labor market in the form of mediation would in the hawk-dove game remove some of the weakness associated with making a unilateral compromise offer; or in other words, increase the value of ε , and the likelihood of a compromise outcome. Such an effect of mediation has been demonstrated by Geraghty and Wiseman (2011) for the American government policy during World War I. They also speculate about similar results of government intervention had earlier been achieved in the UK and France.

Table A1. Number of conflicts per district and year

Year	Mälarprominserna	Östra	Södra	Västra	Mellersta	Nedre norra	Övre norra	Total
1907	39	41	72	62	14	27	8	263
1908	35	43	60	36	24	36	5	239
1909	24	7	14	15	13	22	2	97
1910	8	13	9	8	12	12	3	65
1911	11	21	8	14	7	18	5	84
1912	11	15	9	18	14	31	5	103
1913	18	16	15	12	12	23	3	99
1914	9	11	16	23	14	24	4	101
1915	5	16	7	17	3	18	4	70
1916	21	25	35	38	25	47	11	202
1917	77	56	62	72	67	68	22	424
1918	98	108	98	105	106	103	22	640
1919	81	40	66	67	69	65	11	399
1920	83	62	60	51	86	62	17	421
1921	59	64	41	50	47	32	4	297
1922	73	55	38	52	53	51	11	333
1923	25	25	27	28	22	27	10	164
1924	31	42	27	38	34	42	9	223
1925	40	42	41	26	22	26	6	203
1926	21	27	20	25	28	36	12	169
1927	35	25	17	22	25	23	9	156
Total	804	754	742	779	697	793	183	4,752

Table A2. Descriptive statistics

Variable	Obs	Mean	Std. Dev	Min	Max
Compromise	4,752	0.43	0.50	0	1
Mediation	4,752	0.21	0.41	0	1
ln(duration)	4,752	2.65	1.33	-1.39	8.47
Striking workers	4,752	0.07	0.34	0	22
Lockout workers	4,752	0.00	0.03	0	0.8
Both organized	4,752	0.43	0.49	0	1
<u>Causes</u>					
Wage decrease	4,752	0.11	0.32	0	1
Right to organize	4,752	0.01	0.08	0	1
Collective agreement	4,752	0.07	0.26	0	1
Lay offs	4,752	0.06	0.24	0	1
Working hours	4,752	0.03	0.16	0	1
Personal	4,752	0.03	0.16	0	1
Other	4,752	0.12	0.33	0	1
Multiple	4,752	0.07	0.25	0	1

Table A3. List of all mediators and their districts 1907-27

Id	Location	Years of service	Name	Occupational title(s)
1	Lund	1914-1917	Andersson, Nils	city notary
2	Växjö	1907	Bergendahl, Klas Hugo	chief magistrate
3	Ask/Svalöv	1907-1913	Berger, Frans Justus	lieutenant-colonel
4	Stockholm	1908-1918	Cederborg, Allan	city notary/magistrate
5	Örebro	1910-1919	Djurklou, Nils Gustaf Otto	lawyer
6	Stockholm	1927-	Ekblom, Olle	secretary
7	Stockholm	1907	Frölander, Theodor	deputy managing director
8	Göteborg	1920	Hamberg, Karl Gustaf	magistrate
9	Gammelstad	1921-	Hansén, David	wholesale dealer
10	Luleå/Stockholm	1907-1920	Hellström, Paul	superintendent/secretary/professor
11	Jönköping	1919	Johansson, Carl Robert	school inspector
12	Östersund	1923-	Johansson, Johan Peter	member of parliament
13	Göteborg	1907-1919	Karlsson, Karl Gustaf	tradesman
14	Göteborg	1927-	Kjellman, Hilding	professor
15	Härsta/Sundsvall	1907-1922	Kvarnzelius, Svante Herman	sheet-metal workers/member of parliament
16	Stockholm	1919	Lagercrantz, Claës Gustaf	insurance councillor
17	Stockholm	1924	Lagerström, Otto E	lawyer
18	Göteborg	1921-1926	Mellgren, Olof Erik August	law clerk
19	Mjölby	1920-	Petersson, Erik Gustaf Hjalmar	district judge
20	Södertälje	1925-1926	Petersson, Jakob	chief magistrate
21	Stockholm	1920-1923	Rabenius, Lars Per Teodor	former under-secretary of state
22	Karlstad	1907-1909	Schotte, Axel	former minister of state
23	Kalmar	1908-1918	Waldenström, Alfred	district judge
24	Lund	1918-	Wallengren, Mårten Johan Sigfrid	professor
25	Örebro	1920-	Wijkman, Anders Victor Benedict	county accountant

Source: *Sveriges statskalender*. (various years). Stockholm: Fritzes offentliga publikationer

Table A4. Controlling for city effects

VARIABLES	(1) IV distance	(2) IV distance	(3) IV distance	(4) IV same location	(5) IV same location	(6) IV same location
Mediation	0.320** (0.146)	0.525** (0.227)	0.523** (0.247)	0.312** (0.156)	0.531* (0.278)	0.518* (0.303)
ln(duration)	0.031** (0.013)	0.014 (0.019)	0.014 (0.021)	0.032** (0.014)	0.013 (0.024)	0.014 (0.026)
Striking workers	0.020 (0.025)	0.001 (0.030)	0.006 (0.030)	0.021 (0.025)	0.001 (0.033)	0.007 (0.033)
Lockout workers	0.076 (0.260)	-0.022 (0.278)	-0.061 (0.280)	0.079 (0.261)	-0.024 (0.287)	-0.059 (0.289)
Both organized	0.017 (0.018)	0.004 (0.022)	0.004 (0.023)	0.018 (0.018)	0.004 (0.025)	0.004 (0.025)
Wage decrease	-0.043 (0.027)	-0.045 (0.028)	-0.049* (0.028)	-0.043 (0.027)	-0.045 (0.028)	-0.050* (0.029)
Right to organize	-0.356*** (0.083)	-0.376*** (0.086)	-0.371*** (0.085)	-0.355*** (0.083)	-0.376*** (0.086)	-0.371*** (0.085)
Collective agreement	-0.221*** (0.028)	-0.225*** (0.029)	-0.220*** (0.029)	-0.221*** (0.028)	-0.225*** (0.029)	-0.220*** (0.029)
Lay offs	-0.209*** (0.030)	-0.196*** (0.033)	-0.201*** (0.034)	-0.209*** (0.030)	-0.196*** (0.034)	-0.201*** (0.035)
Working hours	-0.108** (0.044)	-0.108** (0.045)	-0.098** (0.046)	-0.108** (0.044)	-0.108** (0.045)	-0.098** (0.046)
Personal	-0.262*** (0.045)	-0.249*** (0.048)	-0.252*** (0.049)	-0.263*** (0.045)	-0.249*** (0.049)	-0.252*** (0.051)
Other	-0.218*** (0.027)	-0.194*** (0.033)	-0.196*** (0.035)	-0.219*** (0.027)	-0.194*** (0.037)	-0.197*** (0.040)
Multiple	0.062** (0.029)	0.060** (0.030)	0.066** (0.030)	0.062** (0.029)	0.060** (0.030)	0.066** (0.030)
City	0.003 (0.016)	-0.011 (0.020)	-0.010 (0.023)	0.003 (0.016)	-0.012 (0.022)	-0.009 (0.026)
Constant	0.402*** (0.036)	0.378*** (0.046)	0.378*** (0.047)	0.401*** (0.037)	0.379*** (0.047)	0.377*** (0.048)
Observations	4,752	4,752	4,752	4,752	4,752	4,752
R-squared	0.108	0.057	0.062	0.109	0.055	0.064
Year dummies	YES	YES	YES	YES	YES	YES
County dummies	NO	YES	YES	NO	YES	YES

Note: Standard errors are in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

Table A5. Clustering the standard errors geographically

VARIABLES	(1) IV distance	(2) IV distance	(3) IV distance	(4) IV distance	(5) IV distance	(6) IV distance
Mediation	0.324 (0.221)	0.472** (0.222)	0.477** (0.228)	0.330 (0.208)	0.478** (0.236)	0.485** (0.246)
ln(duration)	0.031* (0.017)	0.019 (0.019)	0.018 (0.019)	0.030* (0.017)	0.018 (0.021)	0.017 (0.021)
Striking workers	0.018 (0.020)	0.005 (0.023)	0.009 (0.022)	0.019 (0.019)	0.006 (0.020)	0.010 (0.020)
Lockout workers	0.060 (0.213)	-0.008 (0.218)	-0.056 (0.224)	0.071 (0.151)	0.002 (0.153)	-0.042 (0.150)
Both organized	0.019 (0.019)	0.009 (0.021)	0.008 (0.021)	0.017 (0.021)	0.007 (0.024)	0.006 (0.024)
Wage decrease	-0.041 (0.026)	-0.044 (0.027)	-0.049* (0.027)	-0.043 (0.030)	-0.046 (0.032)	-0.051 (0.035)
Right to organize	-0.353*** (0.079)	-0.373*** (0.085)	-0.370*** (0.086)	-0.356*** (0.067)	-0.373*** (0.074)	-0.370*** (0.074)
Collective agreement	-0.218*** (0.025)	-0.221*** (0.026)	-0.216*** (0.026)	-0.221*** (0.018)	-0.225*** (0.017)	-0.220*** (0.019)
Lay offs	-0.209*** (0.029)	-0.198*** (0.032)	-0.203*** (0.032)	-0.209*** (0.032)	-0.198*** (0.031)	-0.203*** (0.034)
Working hours	-0.109** (0.043)	-0.107** (0.044)	-0.096** (0.044)	-0.109*** (0.030)	-0.106*** (0.030)	-0.097*** (0.028)
Personal	-0.261*** (0.034)	-0.252*** (0.036)	-0.254*** (0.036)	-0.262*** (0.041)	-0.252*** (0.038)	-0.255*** (0.040)
Other	-0.218*** (0.038)	-0.200*** (0.035)	-0.201*** (0.037)	-0.217*** (0.038)	-0.199*** (0.039)	-0.200*** (0.042)
Multiple	0.065** (0.031)	0.063** (0.031)	0.069** (0.030)	0.063** (0.032)	0.060* (0.033)	0.065** (0.033)
Constant	0.403*** (0.045)	0.367*** (0.051)	0.368*** (0.053)	0.405*** (0.037)	0.368*** (0.035)	0.371*** (0.030)
Observations	4,714	4,714	4,714	4,752	4,752	4,752
R-squared	0.108	0.075	0.078	0.107	0.074	0.076
Year dummies	YES	YES	YES	YES	YES	YES
County dummies	NO	YES	YES	NO	YES	YES
Industry dummies	NO	NO	YES	NO	NO	YES

Robust standard errors in parentheses. Models 1-3 cluster on geocode (812 clusters), models 4-6 cluster on county level (24 clusters)

*** p<0.01, ** p<0.05, * p<0.1