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Greedy Politicians? An Empirical Test of the Public Choice Theory

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Abstract
This study aims to empirically test the postulate of Public Choice of politicians as self-interested. To achieve this we examine whether local politicians in Sweden take the opportunity to exploit the compensation systems for municipal assembly meetings. We use two different approaches to examine if this behavior exists. The first is to look at municipalities with a cut-off point system that allows for clear strategies to maximize economic return. The other is to compare whether politicians in municipalities with a fixed compensation arrange their meeting time differently than those with an hourly pay in order to increase their compensation. We find no evidence of such behavior in neither of the cases, however we cannot rule out the possibility that the politicians still have other self-interested motives.

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Content

Introduction

What drives politicians? The postulate of humans as rational self-interested individuals with the aim to maximize their own utility dates back to Adam Smith and the very start of economics as a science, and has been used since. In Public Choice theory, the dominant analytical framework and tool-set used by economists to analyze political science, this is also assumed to be the case for politicians. Public Choice thus suggests that politicians are primarily driven by achieving personal means such as power, economic benefits, office perks and social appreciation rather than serving the public good, something that comes at societal cost (e.g. Mueller, 1976; Buchanan, 1989; Tullock, Seldom and Brady, 2002). However, little empirical evidence has been presented for such a standpoint and the evidence is not always in favor. There are also several contradicting views that argue politicians, in contrast to the majority of other agents in society, are motivated by more intrinsic motivations and are genuinely interested in contributing to the common good (Persson and Tabellini, 2000; Besley 2006; Besley and Larcinese, 2005).

This study empirically examines whether incumbent politicians are motivated by monetary incentives, aiming to shed some light on the larger discussion if politicians are in office to maximize their own utility or to serve the public good. To achieve this we examine the behavior of politicians in municipal assembly meetings in Swedish municipalities that use different compensation systems. The nature of these compensation systems give the politicians the opportunity exploit them for economic benefit. This makes the meetings a suitable study object in testing if incumbent politicians' behavior follows the predictions of the Public Choice theory, that they will exploit the system. We recognize that economic well-being is only one among many things that a politician may maximize in self-interest. However we argue that providing empirical evidence on the role and importance of economic returns for politicians contributes with valuable insights to the discussion of what motivates politicians in their work.

We study three out of six identified compensation systems used for municipal assembly meetings in two separate empirical analyses. The first is a non-linear compensation system where one compensation sum is paid if the meetings are held within a certain time-interval. At the end of the time interval – for the remaining of this study referred to as the "cut-off point" – a second and higher compensation is paid. Thus there is an economic incentive to prolong the meetings close to the cut-off point until the cut-off point has been passed. Previous empirical studies of non-linear incentive systems provide evidence that agents respond to such systems by exploiting them to the fullest (e.g. Healy, 1989; Asch, 1990). However, by estimating regressions on the frequency and hazard rate of meetings' length in relation to the cut-off points, we find no evidence that the studied politicians exploit the system by ending a significantly larger proportion of meetings right after than right before a cut-off point.

The second empirical analysis compares the average number and length of assembly meetings between municipalities that have a fixed pay per meeting to those that have an hourly pay. We estimate regressions of the average length of meetings and the number of meetings on a dummy variable for the compensation system used, both with

and without including several control variables. We do not find any evidence of the politicians adjusting their behavior to maximize their economic benefit in this case either.

Our study is motivated by a general lack of previous empirical studies that investigate if politicians are self-interested, and their responsiveness to economic returns in particular. How and whether politicians respond to economic incentives *while in office* is an especially scarcely researched field. No previous studies have to our knowledge had the main goal to empirically assess whether incumbent politicians respond to economic incentives.

Beasley (2004) provides a theoretical prediction of monetary incentives effects on performance among incumbent politicians, and a handful of previous publications contribute with empirical insights as partial results of their studies (Diermeier, Keane, and Merlo, 2005; Keane and Merlo, 2010; Ferraz and Finan, 2009). Result of these studies support the Public Choice view that incumbent politicians respond to economic incentives.

Furthermore a related group of studies have attempted to assess if there are any economic returns at all to a political career, trying to determine if it could be a factor for politicians in the choice of their career. Most notable, Lundqvist (2011) examines the same group of politicians as this study, Swedish municipal assembly politicians. She tests if there are any economic returns to be elected to the assembly by a close margin compared to that of not being elected by a close margin for politicians within the same party. Her results show that there are no monetary returns to belong to the elected group in her comparison. This result contradicts the results of similar previous studies in other settings (Diermeier, Keane, and Merlo, 2005; Eggers and Hainmueller, 2009).

An additional group of related studies have looked at the effect on the number and quality of candidates that run for political office based on the wage level offered. Results of theoretical predictions are mixed (e.g. Caselli and Morelli, 2004; Messner and Polborn, 2004; Matozzi and Merlo, 2008). Empirical studies have shown that the number and quality of political candidates increase as the wage level increases (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2011).

We hope that this study can add insight to and complement previous studies in the academic discussion. For example, our study acts as an extension of Lundqvist's study in 2011 of Swedish municipal politicians. Our results validate her conclusion by examining the same politician in a different way.

Furthermore our study also provides new empirical evidence on the issue of incumbent politicians and monetary incentives. It is unique in assessing whether politicians exploit economic incentives in their daily work.

To understand politicians' motivations and response to economic incentives is however not only important in an academic context. It also has practical implications. There is a public interest in scrutinizing the compensation systems of politicians, as they in the end are paid by the public through taxes. Furthermore an indication as to whether politicians actually respond to monetary incentives adds depth to a broader discussion whether or not to use incentive systems in the political sector to steer politicians' behavior.

Previous Studies

Besley (2004) uses a political agency model that predicts that incumbent politicians both perform better and stay longer in the political profession as a result of increased wage levels. The model is in itself based on the postulate that our study aims to test, namely that politicians are interested in economic rewards. The reasoning builds on the categorization of politicians into two types: those that always act in accordance with voters' objectives and those that have incentives in the form of rents obtained in deviating from voters' will. The basis of the prediction is then that higher wage will induce the second type of politicians to refrain from deviating from voter's will as the possibility of re-election becomes more attractive with higher salaries. Both performance, as measured by compliance with voters' wills, and duration of political careers therefore increase.

Previous empirical studies have supported the view that incumbent politicians respond to monetary incentives. This is in line with Besley's theoretical predictions. In 2005 Diermeir, Keane and Merlo, as part of a larger study assessing the value of a congressional career using a structural model, conclude that a 20 % increase in the wages of congressmen increases their probability to run for re-election from 91.2 percent to 94.2 percent. In a follow-up study, using the same sample group and structural model, Keane and Merlo (2007) furthermore conclude that a 20 percent reduction in salaries leads to a 14 percent reduction in average duration of individuals' congressional careers.

Finally, Ferraz and Finan (2009) study the effect of wage levels on performance of politicians in office. The result is that higher wages has a positive effect on the performance of incumbent politicians. Performance is defined as bills passed, bills approved, the degree of functionality of legislative commissions and the degree of provisions of public goods. However, as Ferrez and Finan note, it is hard to separate these results from the effect that a higher wage level has on attracting aspiring politicians. The study also finds a positive correlation between the quantity and quality of aspiring politicians and the wage level offered. The higher performance of politicians once in office may thus instead be the result of higher wage levels that have attracted more qualified politicians that naturally perform better.

Furthermore, a group of related studies have been conducted. These can be grouped into (i) those that have attempted to assess the monetary returns of a political career and (ii) those that have assessed the effect of varying wage levels on the number and quality of aspiring politicians. These studies do not address the behavior of incumbent politicians in response to economic incentives. However they nevertheless provide valuable insights on the role of economic returns in motivation politicians in the choice of their career.

Prime examples of the first category of studies (i) are Diermeir, Keane and Merlo (2005), Eggers and Hainmueller (2009) and Lundqvist (2011). Results of these studies have been mixed. Diermeir et al. attempt to estimate the value of a US congressional career. Comparing pre-election to post-election figures on income in a structural model they conclude that the duration stayed in office has a positive effect on wealth. It must however be noted that the study only looks on elected congressmen. Eggers and

Hainmueller's and Lundqvist's studies, on the other hand, also takes into account the alternative of not being elected.

More specifically, Eggers and Hainmueller assess the effect on life-time earnings, as calculated by value of estates at death, of those that have been elected to the British House of Commons by a narrow margin to those that have lost by a narrow margin. The study shows that being elected compared to the alternative of not being elected almost doubles wealth for members of the Conservative party while no wealth effects can be found on Labour members. It must be noted that the study looks on accumulated life-time wealth, and the study further concludes that the wealth-effects for Conservative politicians primarily is due to jobs in the private sector gained as a result of their political careers.

Examining the same political arena as our study, Lundqvist on the other hand examines the monetary return of those that were elected to the municipal council in Sweden with close margins compared to those of the same party that with close margins did not get elected. In contrast to previous studies her empirical tests find that there are no economic gains for the elected group in her comparison. As no monetary return exists in being elected according to the study, Lundqvist reasons that monetary incentives do not act as a motivational factor for politicians. The same result is reached looking at earnings right after election, 15 years after being elected and while exiting political office. However, gaining a municipal electorate nevertheless increases the changes for further, and national, political positions. Lundqvist argues that the conflicting results to previous studies may at least in part be explained by the difference in the groups studied. Lundqvist looks at local politics in Sweden, the other two studies at national politics in UK respectively USA.

The second (ii) category of studies includes several theoretical predictions based on the citizen-candidate model framework. Results of these studies have been mixed. Caselli and Morelli (2004) predict that the competence of the elected body increases with higher wage level for politicians. On the other hand the models of Messner and Polborn (2004) and Matozzi and Merlo (2008) predict the opposite.

Empirical studies on the effect of an increased wage level indicate that higher wages results in both more and more qualified citizens aspiring for office. As previously stated, Ferran and Finan (2009) conclude as a partial result of their study that an increase in the wage level both attracts more numerous and more qualified aspiring politicians. They define quality by education level, relevant professional experience and gender equality. This result is supported by Gagliarducci and Nannicini (2011). Their study finds a positive relationship between the wage level offered and the competition for Italian mayoral seats. Furthermore, the positive effect an increased wage level has on the quality of aspiring politicians is also partly supported by Kotakorpi and Poutvaara (2010), finding similar evidence in Finland for female candidates.

Specification of detailed research focus

Our study examines politicians' behavior in response to compensation systems by looking at a selection of municipal council meetings in Sweden during the year of 2009.

Municipalities are one of three levels of democracy in the governing of Sweden. The other two are the state and the counties. There are 290 municipalities in Sweden and their activities and responsibilities are regulated under *The Swedish Local Government Act*. Compared with its international counterparts, Swedish municipalities exercise a considerable degree of autonomy and provide a large proportion of public services. Main responsibilities include the provision of primary and secondary education, elderly care and social services. Further responsibilities include planning and building issues, health and environmental protection, water and sewerage and emergency services. The activities are founded by a municipal tax, independently set by each municipality.

The Municipal Assembly is the highest decision-making body in each municipality. Its members are elected among nominees presented by each party and elections are held in connection with the general election of the Swedish parliament and the county councils every fourth year. The municipal assembly takes all major decisions with relevance to the municipality, including budgetary decisions and the setting of income tax levels. The members are usually part-time politicians, making the majority of their living on another job (SALAR, 2012). The politicians in the most highly paying municipal assembly in Sweden made less than 50,000 SEK per year in 2009 for their work in the assembly (Wrede, 2009), which can be compared to the average salary of 27,900 SEK per month in Sweden during 2009 (Statistics Sweden, 2012a).

The different compensations systems used to remunerate the members of the municipal assemblies in Sweden are

(i) fixed pay per meeting,[1]
(ii) hourly pay,[2]
(iii) hourly pay together with a compensation for preparatory reading,[3]
(iv) hourly pay with double, triple or even more pay during the first or few first hours of the meeting and normal hourly pay the rest of the meeting,[4]
(v) system with fixed payments within time-intervals, separated by cut-off points,[5]
(vi) monthly/yearly fixed pay in combination with one of the above,[6]
(vii) no pay.

We have chosen to examine compensation system (v) and compare the average number and length of meetings in municipalities using system (i) and (ii, iii).

We will first examine system (v). It is a suitable system to study for our purpose, because there is an economic incentive to prolong the meetings close to the cut-off point until the cut-off point has been passed. This allows for the politicians exploit the system

[1] 100 – 1690 SEK per meeting, depending on municipality (Wrede, 2009).
[2] 50 – 220 SEK per hour, depending on municipality (Wrede, 2009).
[3] Extra pay for preparatory reading ~100-300 SEK per meeting, depending on municipality (Wrede, 2009).
[4] See hourly pay, footnote 2 above, for amounts.
[5] 250 – 860 SEK per meeting shorter than the cut-off point(s) and a 50-100% increase in pay for meetings that have passed the cut-off point(s) in time (Wrede, 2009).
[6] Only a few municipalities, yearly pay ranging from ~5-35 TSEK (Wrede, 2009).

by ending a significantly larger proportion of meetings should en right after than right before a cut-off point.

To our knowledge no study has examined a non-linear system with time elapsed as the defining cut-off point. However several studies have examined result-based non-linear forms of incentive systems. They have shown that these systems have been exploited to the fullest (Healy, 1985; Ash, 1990; Oyer, 1998; Marschke and Courty, 2004). Healy (1985) shows that managers adjust the reporting of financial results through accrual decisions and accounting procedures to maximize the number of years that they receive a bonus. Ash (1990) shows by studying navy recruitment agencies that great effort is put into recruiting up to levels needed to receive a bonus, however little or no effort is made to recruit beyond that level. The studies suggests that if politicians act as other individuals in society, they should exploit the system through not ending meetings right before a cut-off point and instead prolong them until it is reached. If they do not exploit the system, the data indicates that politicians might not be driven by economic returns in carrying out their work.

Secondly, we will compare the average number and length of meetings in municipalities using the systems fixed pay per meeting (i) and hourly rate (ii, iii). The choice to compare municipalities that use these two systems can be motivated by simple reasoning. If politicians act to maximize economic return municipalities with a fixed pay per meeting should have shorter meetings than those that receive hourly pay, as the fixed pay group lack the monetary incentive for longer meetings. If the municipalities with a fixed pay hold shorter meetings on more occasions than the politicians that are paid an hourly wage, this would indicate that politicians are driven by and interested in economic incentives. If not, it would indicate that politicians not are driven by economic incentives.

Our two research questions thus are:

(i) Does a significantly larger proportion of meetings end right after a cut-off point than right before?

(ii) Do municipalities with a fixed pay per meeting have shorter and/or more frequent meetings than municipalities with an hourly pay?

Method

We conduct two separate analyses, one to answer each research question. The first analysis concerns the group of municipalities that use the compensation with time-intervals separated by cut-off points. In this analysis each individual meeting is treated as separate observation with its own cut-off point. The second analysis is a comparison of the groups of municipalities with an hourly pay with those that have a fixed pay per meeting. In this analysis the averages for each municipality are used as the observations. The two analyses are outlined separately below.

Municipalities with cut-off points

Centralization of the data around the cut-off points

The different municipalities use different points in time as cut-off points. Because of this, we centralize the data around a common cut-off point, zero, by subtracting each municipality's cut-off point from the length of each meeting in that municipality. This will center all the results around zero. The meetings that have passed a cut-off point receive a positive value representing the time elapsed from the cut-off point to the end of the meeting. The meetings that have not passed a cut-off point receive a negative value representing the difference in time between the end of the meeting and the cut-off point.

Furthermore, a group of municipalities have two cut-off points. The way we deal with this is to compare each individual meeting with the cut-off points available for that particular municipality and choose the one closest to the end time of each meeting. We motivate this approach by the fact that the cut-off point closest to the end time of the meeting is the one the politicians ought to have had in mind when taking the decision to end the meeting at that particular time. We also get more observations close to centralized common cut-off point. As it is the interval right before and right after the cut-off point that we want to study, it suits the studies aim. We do however recognize that our approach does distort the picture presented of the whole population. Fewer observations will take place far from zero in our centralization of the meeting lengths. To account for this all statistical tests are also performed on two other data sets. One data set uses the lower cut-off point in all municipalities that have more than one cut-off point and the other only uses the higher cut-off point for these municipalities. The results of these analyses are presented in the Appendix A and B.

Frequency plots

Firstly we will intuitively asses the data to see whether the cut-off point has an effect on meeting length through plotting the frequency of meetings before and after time zero. Ideally, we would have thousands of observations and check the frequencies within a few minutes just before and just after zero, but considering the relatively few observations available, the meeting lengths are grouped into time intervals of ten minutes. The plots thus show the frequency of meetings ended in each ten-minute period. We show one plot for the entire population and one only displaying the frequencies one hour before and one hour after time zero, the cut-off point.

Hazard Rate

We also calculate a hazard rate for each ten-minute interval, t_i. This is done to further understand the data through plots and we also use the hazard rate in statistical tests. The hazard rate that we use is defined as

$$h(t_i) = \frac{observed\ events\ in\ interval\ [t_i, t_i + \Delta t]}{N(t_i)}$$

where in our case the observed events are the number of meetings ended. The interval used, Δt, is ten minutes and the number of events left, $N(t_i)$, is the number of meetings that have not yet ended at the start of the time interval t_i. For example if there are 100 meetings left at the start of the time interval t_{10} and five meetings are ended within this time span the hazard rate will be 5/100. If five meetings are ended in the following ten minutes as well, interval t_{11}, the hazard rate will be 5/95. In this example the frequency rate would simply be five for both of the time intervals whereas the hazard rate increases from 5/100 to 5/95. Using the hazard rate can thus give a better picture because it is a proportional measure that captures small changes in frequency that can be significant in proportion to the number of observations left.

We show one plot of the hazard rate for the entire population and one only displaying one hour before and one hour after time zero, the cut-off point.

Regressions on Hazard Rate

We test for statistical significance in the variation of the hazard rate using OLS. In a first estimation, we simply estimate the hazard rate on a dummy variable *HighPay* for having or not having passed the cut-off point. The regression

$$HazardRate_t = \propto + \beta HighPay_t + \varepsilon$$

measures if there is a statistical difference in the behavior of the council members after the cut-off point has been reached. However, in this regression time elapsed is likely to be an omitted variable. Intuitively a meeting is more likely to end the more time that has passed. A meeting can neither be one minute long nor continue forever. To resolve this possible omitted variable bias, a time variable representing each ten-minute interval will be included in the regression

$$HazardRate_t = \propto + \beta HighPay_t + \beta Time_t + \varepsilon$$

and the square and cubed versions of time will also be added to further account for the impact of the time elapsed.

In addition, there is another way to account for the omitted variable bias of time. The smaller the time span, the smaller effect will be from the time variable. Since we primarily are interested to test if there is a statistically significant jump in the hazard rate just after the cut-off point we can adjust the time span so that we only compare the hazard rates only including time-periods surrounding the cut-off point and thereby minimize the effect of time elapsed. We will therefore also run regressions using four smaller time spans ranging from ±180 minutes to ±30 minutes from the time zero, the cut-off point.

Fixed versus Hourly pay

In the second empirical analysis we estimate regressions of average length of meetings and number of meetings on a dummy variable for the compensation system used. We thereafter include several control variables to account for possible omitted variable bias.

Regressions

Firstly, we simply calculate and compare the average length and number of meetings. To gain statistical backing for any result, we then run regressions using OLS. Initially only a dummy variable for having a fixed pay per meeting is used as the independent variable to estimate the dependent variables *Averagelength*, representing the average length of a meetings in municipality *i*, and *No.ofMeetings* representing no of meetings in municipality *i,* in the regressions

$$No.ofMeetings_i = \propto + \beta FixedPay_i + \varepsilon$$

$$AvgLength_i = \propto + \beta FixedPay_i + \varepsilon$$

To account for possible omitted variable bias, we will also run the regressions above including three possibly omitted variables.

The first is the population in the municipality. It could be that this affects how busy the council is as big municipalities may have more issues at hand. This in turn could affect both the length and the number of the meetings.

The second variable included is the taxable income level of the citizens in municipalities. This figure proves as an indication of the money available for the municipality. A bigger budget could result in more and longer discussions in the assemblies as more is to be spent. Or it could be the case that poorer municipalities need more time to discuss and handle all the issues resulting from financial shortages.

A third possible omitted variable is the educational level in the municipality. It is a reasonable assumption to make that the educational level in the general public of the municipality will most likely affect the general educational level of the members of the assembly. Having a higher education can possibly affect the way in which the politicians deal with matters, either shortening the meeting time by being more efficient or prolonging the meetings by being able to analyze the discussed topics in more depth.

The analyses above aim to single out whether politicians that have a fixed pay arrange their meeting time by having shorter and more frequent meetings in comparison with the politicians that have an hourly pay in order to increase their compensation. However, if there is a difference in the total amount of meeting time between the two groups to start with, a difference in the time arrangement could be missed in the previous analysis. We will control for this by running the regression

$$TotTime_i = \propto + \beta FixedPay_i + \varepsilon$$

where total time is the average meeting length times the number of meetings. We also run the regression controlling with the same possible omitted variables as previously. If the correlation would be significant we will consider this when analyzing the results of the regressions with only the number of meetings or average length as independent variables.

Data

An article from the political journal *Dagens Samhälle* (Wrede, 2009) along with complementary details provided by the editorial staff of *Dagens Samhälle*, was used to

initially identify the type of compensation system used by each municipality during 2009. We thereafter manually collected the number of meetings and the length of each meeting for the municipalities in the data sets. This was collected from their web pages, where the meeting minutes usually are available. Each municipality is required by law to keep all minutes of municipal assembly meetings stored and available to the public, and in the cases that it was not on their web page it was usually sent to us by request.

Furthermore, we have checked and validated the data by informing ourselves of the general rules that apply to all municipalities, and of the specific calculation and reporting procedures of each municipality using the system with time intervals separated by cut-off points. We have taken the general information in to account and have adjusted for local differences.

The data set and the result of the validation procedures are presented separately for the municipalities with cut-off points and for the municipalities with a fixed or hourly pay in different sections below.

Municipalities with cut-off points

The final data set used in our analysis of municipalities with cut-off points includes a total of 33 municipalities with 276 meetings during 2009. Of these, 25 have only one cut-off point and 8 have two cut-off points.

We initially identified 39 municipalities using the compensation system. Two municipalities had to be removed from the data set due to that they, despite a legal requirement, did not provide sufficient information. Furthermore, two municipalities were removed from the data set as meeting time was calculated on an individual basis, as further outlined below. Two more municipalities were removed as it was impossible to not reach the cut-off point used, giving a de-facto fixed pay system.

Validation of the data

To secure the quality of the data, we called each municipality in the group and asked the same set of questions to fully understand the calculation used to determine whether the meeting length has passed a cut-off point or not. It strengthens the data set that differences in municipalities' calculations to determine whether the politicians have reached a cut-off point or not, are understood and adjusted for.

Firstly we double-checked that the municipalities had the cut-off points that were presented in the article of *Dagens Samhälle*, and that they kept this system during the full year of 2009. In two cases the cut-off points recorded by *Dagens Samhälle* were one hour off, and this was adjusted for.

It was also important to understand the reporting system. We asked each municipality about who recorded the length of the meetings and reported it to the payroll office, and if the council members themselves had any control over this procedure. In all cases there was a secretary at the meeting recording the meeting length and the presence of each delegate and then reported this to the payroll office. This means that the time written on each minute is the basis for the payroll of all council members and they cannot themselves send in another longer meeting time to the payroll office for their own benefit. Hence, using the recorded time on the minutes is a valid approach for our analysis.

We also made sure to understand if the municipalities deducted any kinds of breaks for lunch or refreshments from the meeting length when calculating the time to reach a cut-off point. If this was the case this was adjusted for, but in the vast majority of the cases the exact time on the minutes was used without any deductions or adjustments.

It was also necessary to learn how the meetings came to an end. In all cases it was the chairman that ended the meeting after all topics had been exhausted and there was nothing more to say. The meetings follow a set agenda, but the council members always have the opportunity to raise more concerns or discussions at each topic of the set agenda and can thus prolong the meeting as long as they can think of viable things to say regarding the issues at hand. They can also of course choose to remain quiet even if they do have things to say if they do not want to prolong the meeting. Hence it can be said the council members do have the power to choose the length of the meeting to a large extent by choosing to raise or not raise concerns and discussions within the agenda at hand. This makes it viable to use the data set for our purpose of seeing whether politicians exploit the non-linear system or not.

De facto cut-off points

During our validation of data and procedure it came up that several municipalities had group meetings within the parties before the assembly meetings. In a few municipalities, these were included in when calculating if the council politicians had reached the cut-off point or not. As the group meetings were held right before the assembly meetings, they started either one, two or three hours before the assembly. We solved the issue by subtracting the relevant number of hours from the concerned municipalities' cut-off points. As the vast majority of all the politicians in the council attended these group meetings, their de facto point is actually one hour less because they always record one hour for the group meetings prior to the council meeting.

Furthermore, two municipalities also allowed the politicians to indicate travel time to the assembly meetings. As this is set individually, it is neither possible to use the recorded time on the minutes as basis for our analysis nor possible to subtract a fixed time from the cut-off point. These two municipalities were instead removed from the data set.

Two municipalities included group meeting length that alone always exceeded their only cut-off point. This guaranteed every assembly meeting to surpass the cut-off point, giving a de-facto fixed pay system. These two municipalities were also removed from the data set.

Fixed versus Hourly pay

In the data set there are 32 municipalities with an hourly pay compensation system. There are over a hundred municipalities that give a fixed pay per meeting, and due to the time scope of the essay, it was not possible to gather the data for all of these municipalities and instead we randomly selected 32. The average meeting length for 2009 was calculated for each of the municipalities, and thus the data set consists of 32 municipalities of each system with a value for average length and number of meetings. Further data set contained the municipalities' income, population and educational level

used as control variables, and was collected from Statistics Sweden's online database (Statistics Sweden, 2012b-d).

Validation of data

For this data there was not the same importance knowing the exact practices of reporting and deductions for breaks. As long as the same rules are applied to both groups we are going to compare, the rules themselves are not that important. We have chosen to use the exact time listed on the minutes when comparing the groups, without subtracting any time.

However, it is important to determine that the assembly members have the opportunity to decide how many meetings are held each year and that they can influence the meeting length, because these are prerequisite conditions for our analysis to be possible. We called the Swedish Association of Local Authorities and Regions and they confirmed that the assembly itself determines how many meetings are to be held each year to fulfill the duties of the assembly. Regarding meeting length, it is a reasonable assumption that the ending of the meetings are determined similarly in all municipalities, since this was the case for all of the municipalities we phoned in the other data set. The assembly members can then influence the meeting length by raising or not raising discussions on the agenda at hand.

Optimally municipalities with a fixed pay should be compared to municipalities that only give an hourly pay. There are however very few municipalities only giving an hourly pay, more common is to give a fixed payment for preparatory reading on top of the hourly pay for the meeting time. If the preparatory payment were very large in relation to the hourly pay for the meeting time the economic incentive would almost equal that of having a fixed pay. However since the preparatory reading compensation is never equivalent to more than one or a couple of hours of hourly pay we argue these municipalities can be used in the analysis.

Certain municipalities gave a much higher hourly rate during the first one, two or three hours that then reverted in to a much lower hourly pay. We removed these municipalities because the economic incentive to continue the meeting changes. We seek to compare municipalities with a linear rate where the politicians are indifferent to whether the meeting ends or continues in comparison with having a fixed rate where the economic incentive is to end the meeting as quickly as possible.

Endogeneity

Since we aim to investigate the effect of having a fixed or hourly pay on the behavior of the politicians in terms of meeting length and number of meetings, it is important to understand how the system of payment was chosen in the first place. The effects of having a certain system could be offset if the party that decides what system should be used wishes to minimize politicians' pay, as could potentially be the case if the municipal bureaucrats set the salaries. We called the Swedish Association of Local Authorities and Regions about this as well, and they confirmed that how the politicians are paid is decided by the assembly itself and is not nationally regulated in any way.

This implies that even if a fixed payment system is chosen in a municipality that traditionally has long and few meetings which would minimize the pay, the politicians

themselves have decided this. Thus the results would still be valuable information to answer the question if they are greedy and exploit the system or not, and possible endogeneity is not a setback for our analysis.

Results

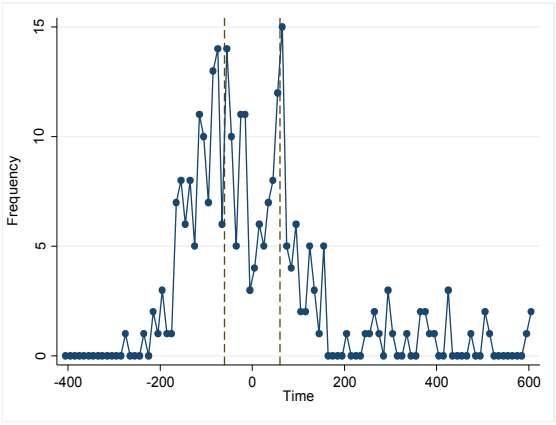
First we present the results from the analysis of municipalities with cut-off points. In the following section we present the results of the analysis comparing municipalities with fixed pay and hourly pay systems.

Municipalities with cut-off points

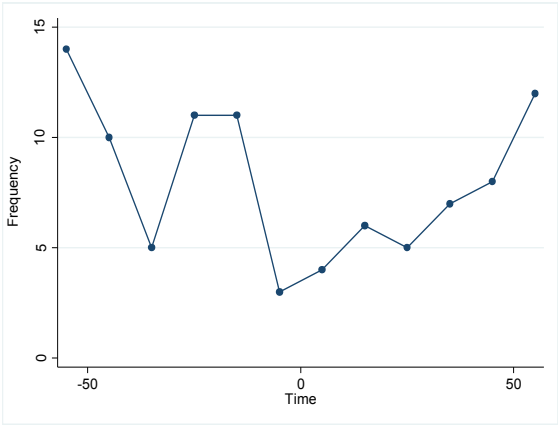
As stated in the method section we begin by plotting the frequency rate of meetings ended in each ten-minute time interval. The cut-off point has been subtracted for each meeting centralizing all data around a common cut-off point, time zero. We have used the cut-off points closest to the end time of each individual meeting for municipalities having two. Separate plots only using the lower and only using the higher cut-off point are presented in Appendix A and B.

Figure 1 – Frequency rate of ended meetings per ten-minute time period

Panel A



Panel B



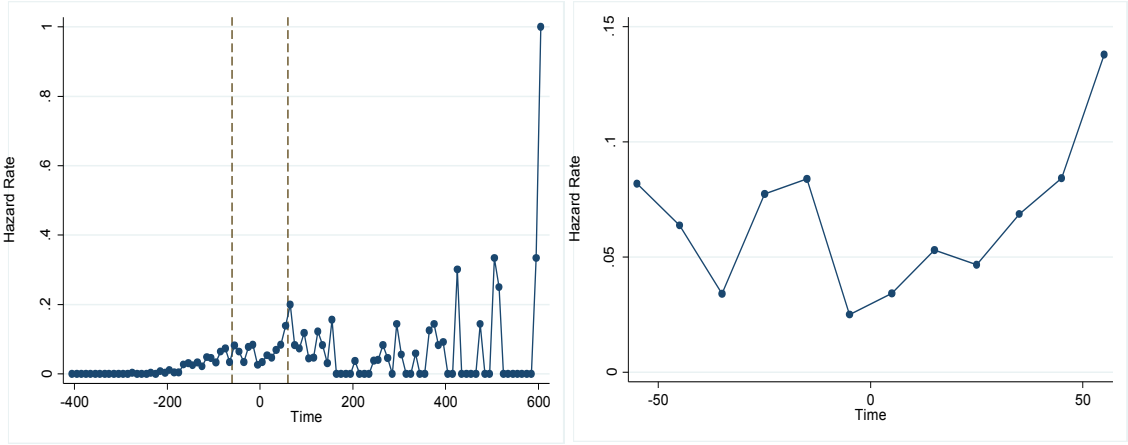
Notes: Panel A plots the frequency rates for all time periods whereas panel B only ±60 minutes surrounding time 0, also marked by the dashed lines.

As can be seen in Figure 1, there is a dip in the number of observations in the ten-minute interval just before cut-off point and an increase in the following time period. There also seems to be the tendency that fewer meetings are ended close to time zero in general in comparison with the periods further away, although we cannot see any obvious trend just by looking at the plots of the frequency rate.

In Figure 2 the hazard rate for each ten-minute interval is plotted against time. As for the frequency rate there is a dip in the interval before time zero and an increase in the following couple of intervals, but we cannot identify any obvious trend by examining the plot of the hazard rates either.

Figure 2 – Hazard rate of meetings per ten-minute time period

Panel A Panel B



Notes: Panel A plots the hazard rates for all time periods whereas panel B only ±60 minutes surrounding time 0, also marked by the dashed lines.

To check for any statistical evidence of the politicians altering their behavior due to the cut-off point, the hazard rate was regressed on a dummy variable *HighPay* that indicates whether or not the cut-off point has been passed. As for the plots previously, the calculation presented is based on the closest cut-off point to each individual meeting in the cases there were two, but calculations using only the lower and only the higher are presented in Appendix A and B.

Table 1 – Estimated Effect of *HighPay* and *Time* on the Hazard Rate

	(1)	(2)	(3)	(4)
HighPay	0.056**	-0.026	-0.009	0.084
	(0.023)	(0.043)	(0.047)	(0.054)
Time		0.161**	0.111	-0.305*
		(0.071)	(0.092)	(0.156)
$Time^2$			0.135	-0.350
			(0.157)	(0.212)
$Time^3$				2.044***
				(0.631)
No. obs.	102	102	102	102
R2	0.047	0.103	0.109	0.196

Notes: All regressions estimated using ordinary least squares. The dependent variable is the hazard rate of ended meetings per ten minute time period. The time variable has been divided by 1000 to give larger coefficients. The standard error for each coefficient is shown below in parenthesis. All regressions include a constant not reported in the table.

***Significant at the 1 percent level in a two sided test.
**Significant at the 5 percent level in a two sided test.
*Significant at the 10 percent level in a two sided test.

Without including any time variable *HighPay* is significant at the 5 percent level, but it becomes insignificant when including a time variable. This is intuitive because meetings naturally have a higher risk of ending the longer the time elapsed, and thus the significant correlation found in regression (1) is due to this and not an actual correlation of *HighPay* and the hazard rate.

Another way to deal with the omitted variable bias of the time elapsed is to estimate the effect of *HighPay* for shorter time spans surrounding the cut-off point instead of examining the whole population. We examined the hazard rates in the time periods ±180, ±120, ±60 and ±30 minutes from the cut-off point, time zero. If the *HighPay* dummy would be significant for the ±30 minute time interval, this could be statistically valid even without controlling for time because the effect of time elapsed is small when examining observations with a small difference in time.

Table 2 – Estimations of *HighPay* and *Time* on the Hazard Rate, Varying Time Spans

Time Span	180		120		60		30	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HighPay	0.032**	0.020	0.027*	0.008	0.010	-0.029	-0.017	0.012
	(0.014)	(0.028)	(0.015)	(0.031)	(0.018)	(0.036)	(0.019)	(0.043)
Time		0.070		0.160		0.648		-0.998
		(0.134)		(0.223)		(0.526)		(1.251)
No. obs.	36	36	24	24	12	12	6	6
R^2	0.139	0.146	0.127	0.148	0.028	0.044	0.168	0.313

Notes: All regressions estimated using ordinary least squares. The dependent variable is the hazard rate of ended meetings per ten minute time period. Only observations within the indicated time span have been included in the regression. The time spans are minutes before and minutes after the cut-off point, time zero. The time variable has been divided by 1000 to give larger coefficients. The standard error for each coefficient is shown in parenthesis below coefficient value. All regressions include a constant not reported in the table.

However, as can be seen in Table 2 the dummy *HighPay* becomes less significant the shorter the time period examined is, and is insignificant for the two shorter time intervals. The significance found in the longer time intervals is also lost when controlling for time. This is further evidence that there seems to be no statistical proof of the politicians responding to the economic incentive of the cut-off point.

Fixed versus Hourly pay

The next part in the analysis is the comparison of the systems of having a fixed pay and having an hourly rate. The average number of meetings during 2009 is 9.00 for the 32 municipalities with an hourly pay rate and 8.94 for the 32 municipalities with a fixed pay. The average length per meeting for the hourly pay group is 2.65 hours and 3.14 hours for the fixed pay group. Hence, municipalities in the data set with a fixed rate have both longer and fewer meetings compared to having an hourly rate, which minimizes their pay. This contradicts the hypothesis of the politicians receiving a fixed pay arranging their time to have more frequent and shorter meetings than politicians receiving an hourly pay.

However, to gain statistical evidence for any difference we estimate the effect of having a fixed pay on both the number of meetings and the average length of meetings, with the results seen in Table 3 below. We also control for three possible omitted variables namely municipal population, income and educational level.

Table 3 – Estimation of *Fixed Pay* on *No. of meetings*, *Average Length* and *Total Time*

Dependent Variable	*No. of Meetings*		*Average Length*		*Total Time*	
	(1)	(2)	(3)	(4)	(5)	(6)
Fixed Pay	-0.063	-0.231	0.511	0.374	4.352	2.416
	(0.524)	(0.521)	(0.317)	(0.314)	(3.359)	(3.139)
Control Variables	No	Yes	No	Yes	No	Yes
No. obs.	64	64	64	64	64	64
R2	0.000	0.090	0.040	0.137	0.026	0.220

Notes: All regressions estimated using ordinary least squares. The dependent variable is either the number of meetings, the average length of meetings or the total amount of meeting time, which we have calculated by multiplying the number of meetings with the average length. The standard error for each coefficient is shown in parenthesis below the coefficient value. All regressions include a constant not reported in the table. The control variables are municipal population, income and educational level. The dummy variable for having a fixed pay is never significant even at the 10 percent level in a two-sided test.

There seems to be no effect of having a fixed payment system on neither the number of meetings nor the average length of meetings regardless of including the control variables or not. None of the coefficients in any of the regressions are significant even at the 10 percent level in a two-sided test.

To see whether any effect of a certain payment system could be masked by the municipalities having different total meeting time to start with we also made regressions with total time as the dependent variable, however no significant correlation was found.

In summary we find no statistical evidence that a significantly larger proportion of meetings are ended right after a cut-off point than right before. We do not either find any statistical evidence that the municipalities that use a fixed pay have significantly shorter or fewer meetings in comparison with the municipalities that have a hourly pay.

Discussion

The empirical results of our study find no evidence that incumbent local politicians in Sweden respond to compensation systems by exploiting them for economic returns. Thus, the politicians in our study are most likely not motivated by economic returns. This contradicts the postulate of Public Choice theory of politicians as rational self-interested agents with the aim to maximize their own utility. Monetary returns are nevertheless only one of many things that a politician may maximize in self-interest. Our study does not provide any insights on other concerns.

The result contradicts previous theoretical and empirical findings on the behavior of incumbent politicians in response to monetary incentives or adjusted wage levels. Besley's theoretical prediction (2004) builds on the assumption that politicians are interested in monetary returns. The result of this study questions that assumption. Diermeier, Keane, and Merlo (2005) and Keane and Merlo (2007) each find a positive correlation between wage level offered and the duration of congressional careers in the USA. Ferraz and Finan (2008) find that a higher wage level improves the performance among Brazilian municipal politicians. Our study, on the other hand, suggests that politicians do not respond to monetary incentives. It must however be noted that our study differs from the previous on several accounts. Firstly, we study a completely

different cohort, Swedish part-time municipal politicians. Secondly, our study is unique in assessing whether politicians in their daily work exploit economic incentives. The previous studies examine full-time and long-time commitments. This could in part explain the contradictory results.

On the other hand our results support the conclusion of Lundqvist's recent study (2011). She empirically shows that there are no monetary returns in being elected to Swedish municipal assemblies. Lundqvist therefore argues that these politicians are most likely not motivated by such returns. Our study reinforces the validity of her conclusion. We show that the municipality assembly politicians in Sweden do not show signs to be motivated by monetary returns also when in office.

One possible reason that our study shows that the studied politicians are not motivated by monetary returns studied could be that the politicians in the study are interested in re-election and/or political advancement that in the future could give them even more utility. If they exploited the payment system, this could give negative press and make them less likely to be re-elected or promoted. The idea that politicians' main goal is to be re-elected is not new. For, example Downs (1957) theoretical models of political life use the assumption that politicians' primary motivation always is to be elected. Other motivations are always secondary. The fact that politicians forego short-term monetary returns in our study does not necessarily contradict the fact that they are self-interested and motivated by economic returns.

The influence of re-election concerns is further supported by the fact that Lundqvist's study shows that gaining a local political office seat increases the probability of advancing on the political ladder. Furthermore Vlachos and Svaleryd (2008) also examine Swedish municipal politics. They show that in municipalities where political competitions is high, the municipal politicians choose a more equal financial party support system than the politicians in municipalities where political competition is low. Here the dominant party chooses a system more biased to its own benefit. This suggests that the municipal politicians behave in a way that is seen by the public as more fair where competition demands it, whereas the safer politicians choose to act in a more self-interested manner.

Since Vlachos and Svaleryd studied the same politicians as in our study, it may suggest that the refrainment of exploiting the system is due to a concern of re-election. However, exploiting the system in our case would affect all parties as negatively and thus not be refrained from due to concerns of political competition. Although, it may be the case that the politicians are concerned with their personal position within their party.

Another element that should be taken into consideration is that income from the municipal assembly meetings cannot be the main income for the politicians. The meetings are not held more than once a month and give at most a couple of thousand SEK per meeting if the politicians are lucky. It may very well be the case that the sums are too small to make the effort of prolonging the meeting before a cut-off point worthwhile, since raising more concerns of an issue just before a cut-off point could make it obvious for everyone that the politicians in question are trying to exploit the system and cause embarrassment.

The study furthermore lends empirical support to alternative theories of political economy that use a more nuanced view of politicians' motivations than Public Choice

(e.g. as discussed Persson and Tabellini, 2000; Besley 2006; Besley and Larcinese, 2005). For example, a public service motivation theory, as used in Francois (1999) fits to explain our results well. Advocates of this theory argue that public servants work for free to some extent because their salary is lower in comparison to an equivalent job in the private sector. People are not willing to put in some of their work for free if the organization's purpose is to make money, thus private companies cannot excerpt this kind of motivation from their workers. If the politicians in the municipalities that have been examined have had a large amount of public service motivation it could explain why they do not react to economic incentives if the economic incentives only are a small part of why they are there in the first place.

Furthermore theories concerning workers and reciprocity can also possibly account for the lack of findings, at least in the comparison of fixed and hourly paid politicians. Fehr and Gächter (2000) have found that giving workers a fixed pay can in comparison with giving a provisional salary motivate workers more, because the workers receiving fixed pay reciprocate the employers' trust of giving them a fixed wage by working harder. This element of reciprocity does not exist in the case of giving a commission based salary. In comparing the cases where the politicians are given a fixed pay in comparison to an hourly rate, one could link the finding of Fehr and Gächter and say that the politicians receiving a fixed rate reciprocate the taxpayers by having longer meetings. Hence the effect of having shorter and more frequent meetings to maximize monetary pay could be offset by a reciprocal behavior of trust.

Conclusion

The purpose of this thesis was to test the postulate of Public Choice of politicians as self-interested. To achieve this we have empirically assessed whether municipal assembly politicians in Sweden exploit their compensation systems for economic gain. We found no evidence of such behavior.

Our study contradicts earlier empirical studies that investigate a similar matter. They show that politicians seem to be motivated by economic returns. However one study of the same group of politicians comes to a similar conclusion as we do, thus regional differences may account for the widely different results to earlier empirical research.

Our result contradicts the predictions of the Public Choice theory that the politicians ought to have exploited the studied payment systems. However we cannot rule out that the politicians are not trying to maximize other self-interested goals by not exploiting the systems. Nevertheless our findings question the Public Choice theory and add empirical insight to a debate where empirical research is scarce.

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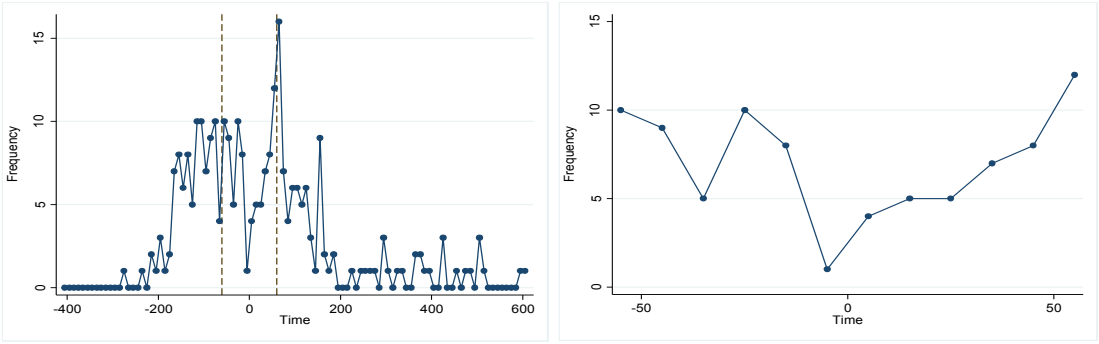
Appendix A

The below figures and tables show the results when consistently using the lower cut-off point for all municipalities having two, instead of using the cut-off point closest to each individual meeting length.

Figure 3 – Frequency rate of ended meetings per ten-minute time period

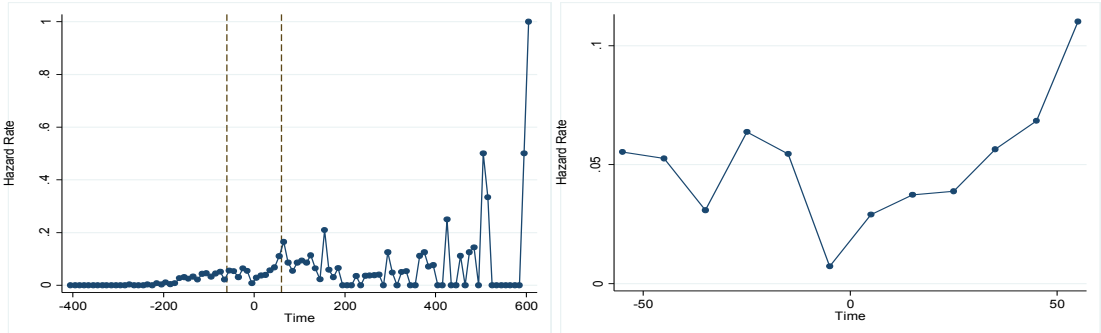
Panel A Panel B



Notes: Panel A plots the frequency rates for all time periods whereas panel B only ±60 minutes surrounding time 0, also marked by the dashed lines.

Figure 4 – Hazard rate of ended meetings per ten-minute time period

Panel A Panel B



Notes: Panel A plots the hazard rates for all time periods whereas panel B only ±60 minutes surrounding time 0, also marked by the dashed lines.

Table 4 – Estimated Effect of *HighPay* and *Time* on the Hazard Rate

	(1)	(2)	(3)	(4)
HighPay	0.070***	-0.035	-0.005	0.097*
	(0.025)	(0.045)	(0.050)	(0.056)
Time		0.206***	0.114	-0.335**
		(0.076)	(0.097)	(0.164)
$Time^2$			0.246	-0.278
			(0.165)	(0.223)
$Time^3$				2.206***
				(0.662)
No. obs.	102	102	102	102
R2	0.065	0.138	0.157	0.244

Notes: All regressions estimated using ordinary least squares. The dependent variable is the hazard rate of ended meetings per ten minute time period. The time variable has been divided by 1000 to give larger coefficients. The standard error for each coefficient is shown below in parenthesis. All regressions include a constant not reported in the table.

***Significant at the 1 percent level in a two sided test.
**Significant at the 5 percent level in a two sided test.
*Significant at the 10 percent level in a two sided test.

Table 5 – Estimations of *HighPay* and *Time* on Hazard Rate, Varying Time Spans

TimeSpan	180		120		60		30	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HighPay	0.042***	0.014	0.034***	0.004	0.013	-0.014	-0.007	0.028
	(0.012)	(0.024)	(0.012)	(0.023)	(0.015)	(0.030)	(0.018)	(0.036)
Time		0.160		0.251		0.449		-1.168
		(0.115)		(0.167)		(0.435)		(1.059)
No. obs.	36	36	24	24	12	12	6	6
R^2	0.267	0.308	0.272	0.343	0.067	0.166	0.035	0.313

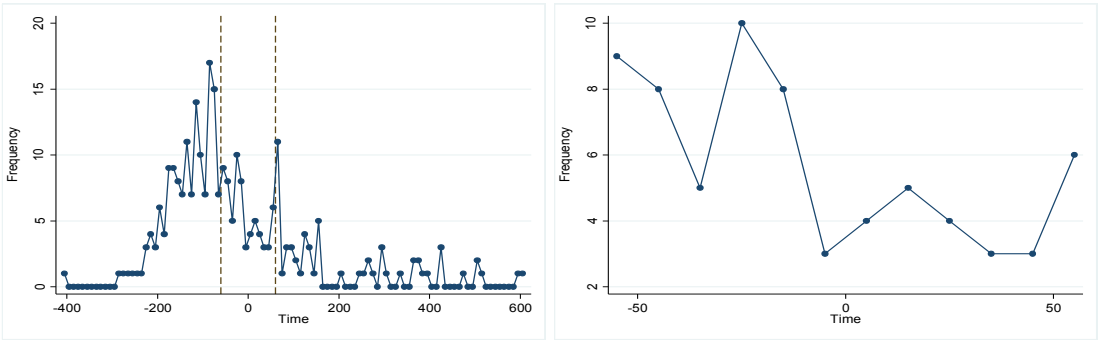
Notes: All regressions estimated using ordinary least squares. The dependent variable is the hazard rate of ended meetings per ten minute time period. Only observations within the indicated time span have been included in the regression. The time spans are minutes before and minutes after the cut-off point, time zero. The time variable has been divided by 1000 to give larger coefficients. The standard error for each coefficient is shown in parenthesis below coefficient value. All regressions include a constant not reported in the table.

Appendix B

The below figures and tables show the results when consistently using the higher cut-off point for all municipalities having two, instead of using the cut-off point closest to each individual meeting length.

Figure 5 – Frequency rate of ended meetings per ten-minute time period

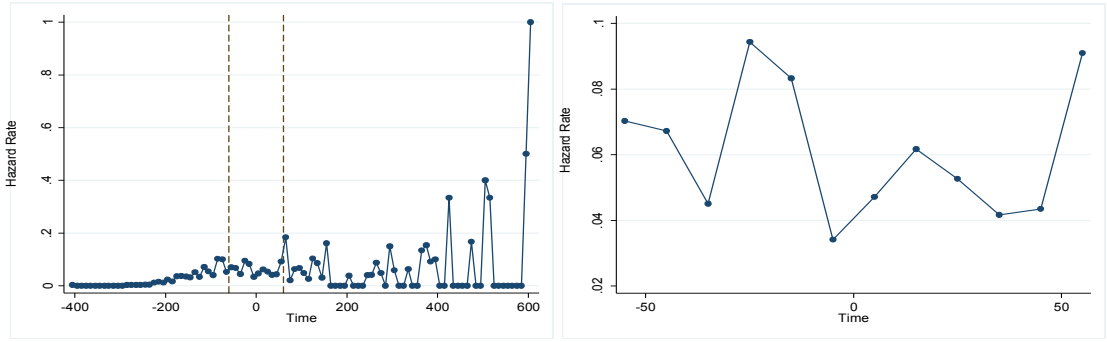
Panel A Panel B



Notes: Panel A plots the frequency rates for all time periods whereas panel B only ±60 minutes surrounding time 0, also marked by the dashed lines.

Figure 6 – Hazard rate of ended meetings per ten-minute time period

Panel A Panel B



Notes: Panel A plots the hazard rates for all time periods whereas panel B only ±60 minutes surrounding time 0, also marked by the dashed lines.

Table 6 – Estimated Effect of *HighPay* and *Time* on the Hazard Rate

	(1)	(2)	(3)	(4)
HighPay	0.052**	-0.066	-0.045	0.053
	(0.025)	(0.045)	(0.050)	(0.057)
Time		0.231***	0.166*	-0.264
		(0.077)	(0.098)	(0.165)
$Time^2$			0.173	-0.330
			(0.166)	(0.225)
$Time^3$				2.114***
				(0.668)
No. obs.	102	102	102	102
R2	0.041	0.124	0.133	0.214

Notes: All regressions estimated using ordinary least squares. The dependent variable is the hazard rate of ended meetings per ten minute time period. The time variable has been divided by 1000 to give larger coefficients. The standard error for each coefficient is shown below in parenthesis. All regressions include a constant not reported in the table.

***Significant at the 1 percent level in a two sided test.
**Significant at the 5 percent level in a two sided test.
*Significant at the 10 percent level in a two sided test.

Table 7 – Estimations of *HighPay* and *Time* on Hazard Rate, Varying Time Spans

TimeSpan	180		120		60		30	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HighPay	0.005	-0.008	-0.006	-0.000	-0.009	-0.015	-0.017	0.024
	(0.013)	(0.026)	(0.014)	(0.029)	(0.012)	(0.025)	(0.019)	(0.037)
Time		0.073		-0.047		0.100		-1.367
		(0.125)		(0.208)		(0.367)		(1.088)
No. obs.	36	36	24	24	12	12	6	6
R^2	0.004	0.014	0.079	0.010	0.059	0.067	0.163	0.452

Notes: All regressions estimated using ordinary least squares. The dependent variable is the hazard rate of ended meetings per ten minute time period. Only observations within the indicated time span have been included in the regression. The time spans are minutes before and minutes after the cut-off point, time zero. The time variable has been divided by 1000 to give larger coefficients. The standard error for each coefficient is shown in parenthesis below coefficient value. All regressions include a constant not reported in the table.