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Electoral Re-registration, Disenfranchisement and Public Service Provision

Dissertação de Mestrado

Dissertation presented to the Programa de Pós-Graduação em Economia of the Departamento de Economia, PUC-Rio as partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor: Prof. Claudio Ferraz

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To everybody that helped me.

Resumo

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Esse trabalho avalia se um programa com o objetivo de reduzir fraudes eleitorais pode excluir eleitores pobres das eleicões e, por consequencia, afetar a provisão de bens públicos para esses eleitores. Nós exploramos um programa de recadastramento de eleitores em municipalidades brasileiras onde havia suspeita de fraude. Usando uma estratégia empírica de diferenças em diferenças, estimamos que o programa reduziu taxas de registro em 10 pontos percentuais (13 porcento) e taxas de participação em 4.5 pontos percentuais (8 porcento), especialmente em municípios com níveis baixos de educação e penetração de mídia. Também mostramos que o programa reduziu a taxa de registro de eleitores pouco educados em 13.3 porcento e de eleitores educados em 2.8 porcento. Além disso, os resultados sugerem que prefeitos responderam a essa mudança no eleitorado reduzindo gastos que beneficiam desproporcionalmente eleitores mais pobres (educação e saúde). Finalmente, mostramos que essa redução de gastos gerou uma deteriorações da infraestrutura de escolas municipais e piorou indicadores de saúde de indivíduos menos educados.

Palavras–chave

Disenfranchisiment;   Participação Eleitoral;   Provisão de Serviços Públicos;

Abstract

This paper examines whether reforms aimed at reducing electoral fraud can have the unintended effect of disenfranchising poorer citizens and, as a consequence, affect public services delivered to poorer households. We exploit a large program of re-registration of voters in Brazil's municipalities where the electoral commission suspected the presence of electoral fraud. Using a difference-in-difference strategy, we compare the electoral registration and turnout in 1186 Brazilian municipalities that went through electoral revision with those that did not in elections prior to the re-registration and after the change. We find that the program reduced registration rates by 10 percentage points (13 percent) and participation rates by 4.5 percentage points (8 percent), specially in municipalities with low education levels and with low media penetration. Using an unique feature of Brazilian electoral registration data, we also show the program reduced the registration among voters with low education in 13.3 percent and among voters with high education in just 2.8 percent. Moreover, we find that the newly elected mayors responded to this change in the electorate by reducing public expenditure in areas that disproportionately benefits poor and uneducated voters (education and health). Finally, we show that the reduction in expenditures deteriorated the infrastructure of public schools and worsened health outcomes of less educated citizens.

Keywords

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1
Introduction

Democracies face a trade-off between controlling fraud and reducing the costs of registration and voting and, thus, extending the franchise to a large number of citizens. Strict registration and voting laws, such as those requiring that voters go to a registration office with a photo identification and proof of residence, and identification requirements to vote on election day, can reduce fraud. But these same rules are likely to increase the costs of registering and voting and might reduce political participation (Lijphart (1997)). Moreover, registration and voting laws do not affect all citizens equally. Less educated and low income individuals tend to be disproportionately affected by these laws (Braconnier et al. (2014) and Nickerson (2015)). Thus, reforms that implement stricter rules for voting can reduce fraud but at the same time take the power away from poorer citizens and affect the identity of the median-voter. Existing theoretical models such as Acemoglu and Robinson (2005) and Meltzer and Richard (1981) suggest that politicians will react to this by reducing policies that affect poorer citizens.

This paper examines whether reforms aimed at reducing electoral fraud can have the unintended effect of disenfranchising poorer citizens and, as a consequence, affect public services delivered to poorer households. We exploit a large program of re-registration of voters in Brazil's municipalities where the electoral commission suspected the presence of electoral fraud. The electoral revision took place in 1186 Brazilian municipalities in 2007 when the TSE (Brazil's Electoral Court) ordered the re-registration of all registered voters in municipalities that attended 3 criteria: the ratio of electorate to population was greater than 80 percent, the electorate was at least double the summed population of citizens aged 10-15 and over 70 years, and voter transfers increased at least 10 percent over the previous year. To re-register, a voter had to go in person to an electoral office with an identification document and a proof of residency. If the voter failed to show up before the deadline, her electoral registration was canceled and she would not be able to participate in the following election.

We use a difference-in-difference strategy where we compare the electoral registration and turnout in municipalities that went through electoral revision and those that did not in elections prior to the re-registration and after the change. We find that the program reduced registration rates (registered voters over the population) by 10 percentage points (13 percent) and participation

rates (votes casted over population) by 4.5 percentage points (8 percent). Using an unique feature of Brazilian electoral registration data, we also show the program reduced the registration among voters with low education in 13.3 percent and among voters with high education in just 2.8 percent. Moreover, we find that the Electoral Revision had heterogeneous effects across municipalities: those with low education levels and with low media penetration had larger falls in registration and participation rates consistent with a disenfranchisement hypothesis.

We then examine whether these changes in the identity of voters in affected municipalities changed the way politicians targeted public services. Using the same difference-in-differences approach, we find that politicians responded reducing municipal public expenditures in education by 4.1 percent and health by 3.0 percent. We then examine whether the changes in spending pattens affected public service delivery. We find a significant deterioration of municipal public school infrastructure (number of schools with access to water, electricity and Internet, for example) and health outcomes (number of mother's prenatal visits and low weight births). Consistent with the hypothesis that politicians distribute less resources to disfranchised voters, only mothers with low levels of schooling had a deterioration in the proportion of low weight births while mothers with at least a high school degree were not affected.

The timing of these effects also largely corroborates the disenfranchisement hypothesis. We find that most of the impact on public service delivery happens on the first two years of the term of the newly elected mayor.

At first glance, an alternative explanation for the reduction in the electoral participation could be attributed to a reduction in electoral fraud: irregular registrations were purged from electoral rolls [1]. The heterogeneous effects that we find are consistent with the disenfranchisement of uninformed voters but not clearly related to the presence of fraud. Also, although fraud could be an alternative explanation for the reduction of the registration and participation rate, it is not easy to argue that a reduction of fraud caused a reduction in the provision of education and health care. If anything, one might expect that a reduction in electoral fraud might improve public service provision. A particular case in which a reduction of electoral fraud could result in a reduction of the provision of health care and education, would be if politicians who engage in fraud, systematically spend more money in this areas than the politicians that do not engage in fraud. We test this hypotheses by verifying whether the impact of the program on reelection chances of mayors of left parties, which generally spend more money in these areas (Pettersson-

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[1]This is suggested by Hidalgo and Nichter (2014).

Lidbom (2008)), differs from the impact on right parties. We find that the re-registration program reduced the reelection rates of right-wing mayors but not the reelection rates of left-wing parties.

Moreover, there are other suggestive evidences that fraud is not driving the results. Electoral fraud is associated with high turnout and high vote share for incumbents (Klimek et al. (2012); Enikolopov et al. (2013)). We explore historical turnout and vote share of the winner mayor candidate and find no evidence that high turnout and high vote share in the past is associated with more electoral registrations cancellations.

As a robustness check to the effects on registration and turnout, we also verify if a similar re-registration program based on biometrics that has been rolling-out in Brazil for the last four elections had similar effects on the registration and participation rates. The important difference between the "biometric re-registration" and the Electoral Revision is that the first one eventually will be implemented in all Brazilian municipalities and do not specially target municipalities with indication of fraud. We find similar results for the "biometric re-registration", including the heterogeneous effects, suggesting that a reduction of fraud is not the main driver of our results.

Additional tests provide evidence against other possible confounding effects. First, there is no evidence of a reduction of municipal expenditures in budget areas that do not affect disproportionately poorer and uninformed voters, hence there was not a general reduction in expenditures. Second, the reduction of expenditures was larger in cities that had a larger fall in registered citizens, reinforcing the idea the the reduction in expenditures is caused by the disenfranchisement. Third, the infrastructure of public state schools are not affected, only municipal schools infrastructure deteriorated, suggesting the deterioration in school infrastructure was specific to schools under the control of mayors and local councils. Fourth, placebo tests for mayor's terms before the program do not show any effect of the "program" on schools infrastructure or health outcomes, indicating that there were no difference in the trends of these variables in municipalities that went through the program and the ones that did not before the program was actually implemented.

This paper contributes to three strands of literature. First, it relates to a vast literature that investigates the relation between electoral rules and electoral participation and composition(Ansolabehere and Konisky (2006), Brown and Wedeking (2006), Brians and Grofman (2001), Burden and Neiheisel (2011), Jackman (1987), Leon (2011), Lijphart (1997), Wolfinnger (1980)). Like in more recent studies that use field experiments, this paper finds that making voting more difficult disproportionately affect the participation of poor

and uneducated voters (Braconnier et al. (2014) and Nickerson (2015)).

Second, it relates to papers that examine whether legally mandated enfranchisement extensions affect policy outcomes(Cascio and Washington (2014), Husted and Kenny (1997),Miller (2008), Naidu (2012)). This paper differs from them because these legally mandated enfranchisement had the clear objective of empowering citizens that could not vote (i.e. woman and black voters), whereas the disenfranchisement that we study is a result of an unintended effect of a policy designed to combat fraud. Therefore, this work might be more insightful in modern debates about electoral rules and its consequences to public police. For example, the long standing debate about how much stringent registration rules in some US states hurts electoral participation and, consequently, public policy directed to individuals of lower socio-economic status.

Finally, this paper relates more closely to Fujiwara (2014) and Hidalgo and Nichter (2014) who study the effects of the introduction of electronic voting in Brazil and its consequences on fraud reduction, valid votes, and the redistribution of public resources towards poor and uneducated voters. While the policy examined by Fujiwara (2014) suggest a win-win situation where the electoral reform reduced fraud and enfranchised the poor, re-registration policies aimed at reducing fraud might disenfranchise the poor suggesting that it is important to examine the trade-offs for citizens.

2
Institutional Framework

2.1
Brazilian Electoral System

Brazil has over 5,500 municipalities spread across 26 states. Every 4 years, each municipality elects a mayor and a local council of legislators. Mayors are elected by direct ballot in a one-round majority system in most cities.[1] Local legislators are elected under an open-list proportional representation. Mayors can run for reelection one time while local legislators can run indefinitely for reelection.

Voting is mandatory for all Brazilian literate citizens older than 18 and younger than 70 and non-compulsory for citizens older than 70 and between 16 and 18. Citizens who do not vote and do not justify their absence are not allowed to have a passport issued, apply for a public service job or enroll in a public university, for example. These sanctions are largely incipient for poor voters.

The Electoral Justice automatically cancels registrations of voters who fail to vote in 3 elections in a roll[2] (or do not justify their absenteeism).

To be able to vote, a citizen must first register himself as voter. To register the citizen must personally go to an electoral office with an identification document and proof of residency. Once registered, the electoral office will establish a voting location for the voter. On the election day, the voter must personally go to the predetermined voting location with a photo ID.

Voters can transfer from the municipality they are registered to a new municipality. They must apply in person and present an identification document and proof of residency. Transfer requests must be filed at least 150 days prior to an election.

Brazil uses an electronic voting system to cast and count votes. The technology is constituted of a machine with a screen and a keypad, people vote by typing the candidate's number into this keypad. There is virtually no evidence of electoral fraud related to the electronic system[3] and its introduction is generally perceived as great success (Fujiwara (2014)).

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[1]Municipalities with more than 200,000 registered voters have a two-round system
[2]Each round of an election counts as a missed election
[3]The system makes impossible to engage in traditional types of electoral fraud such as ballot stuffing or invalidation.

2.2
Electoral Revisions

This paper will study a re-registration program called Electoral Revision that occurred in 1186 Brazilian municipalities in 2007. The objective of the program was to avoid citizens that do not live in a municipality to vote on that municipality. The idea is that local politicians might induce voters to transfer their registrations in exchange of rewards and artificially inflate the electorate of that city with supporters (Hidalgo and Nichter (2014)). Engaging in this kind of fraud demands considerable effort by the voters, they must travel to a municipality they do not live and provide false documentation such as false proof of residency. Although there has been some anecdotal evidence of this kind of fraud in Brazil (Hidalgo and Nichter (2014)), the uncovered fraudulent schemes usually do not seem to involve a substantial number of voters

The TSE (Brazilian "Electoral Supreme Court") ordered the re-registration of all registered voters in municipalities that attended 3 criteria: a ratio of electorate to population greater than 80%, electorate is at least double the summed population of citizens aged 10-15 and over 70 years and voter transfers increased at least 10% over the past year [4].

Once the TSE identifies the municipalities that will have to go through the Electoral Revision, it orders the states electoral courts (TRE) to arrange the re-registration of citizens of these municipalities. Citizens are made aware of the re-registration process through TV and radio advertising. The length of the revision process varies, but it has to last at least 30 days. Most of the re-registration processes started in the later months of 2007.

To re-register, a voter has to personally go to an electoral office with an identification document and proof of residency. If the voter fails to show up with the proper documentation until the deadline, his electoral registration is canceled and the voter cannot participate in the following election.

This paper argues that the electoral revision disfranchised legitimate voters, mainly the ones with less education and with less access to media. There is an extensive literature showing that voters with lower socio economic status are less likely to be registered and to vote. Less educated voters might have more difficulty with the bureaucratic task involved in the re-registration process, such as providing the necessary documentation. Moreover, voters with less access to media might also be disfranchised, since the program is publicized by TV, radio and Internet. Therefore, people who do not have access to these

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[4]99% of Brazilian municipalities fulfill the second criteria (Hidalgo and Nichter (2014)), therefore the other two criteria are the relevant ones

medias might not even know that the program is been implemented in their municipalities.

Figure 1 maps the municipalities in Brazil that went through the Electoral Revision in 2007. It is clear that the program was implemented throughout the Brazilian territory.

Table 1 compares electoral and demographic characteristics of the municipalities that went through the Electoral Revision in 2007 and the ones that did not. The municipalities are similar in most characteristics, with the exception of two: registration rate [5] and population. Although it is good that the two groups are similar in observable characteristics our identification hypothesis will come from the "parallel trend assumption", as it will become clear in the next Section.

2.3
Public Service Provision by Municipal Governments

Brazil is one of the most decentralized countries in the world (Ferraz and Finan (2009)). Mayors and local legislators receive large sums of money from the federal government to provide public services. Two of their core responsibilities are the provision of education and health care.

Education and health care are disproportionately important to poor and uneducated voters. For example, due to its bad quality, the public health care system is only used by citizens who do not have resources to use the private one. Fujiwara (2014) argues that in the presence of this arrangement, theories of redistributive politics will predict that an increase in political participation of less educated voters should raise government spending on health care[6]. A similar argument can be made for education, since, also due to it's bad quality, the primary public education system is used by citizens who do not have resources to use the private one.

Therefore, the disfranchisement of uninformed voters at the municipal level could have an important negative impact in the provision of these services by local governments.

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[5]This is expected since this is one of the criteria to go through the Electoral Revision
[6]The argument in this paper is symmetric since the Electoral Revision reduced the political participation of less educated voters

3
Data

The empirical analysis in the Section 5 will use municipal level data of a variety of sources.

Electoral outcome variables come from the TSE and yearly IBGE population estimates. The number of registered voters by level of education by municipality also comes from the TSE. We aggregate voters that completed high school, started college or finished college as voters with high education. Voters with less than a high school diploma are considered as low education voters. The registration rate is the number of registered voters in a municipality for the first round of a election according to the TSE divided by the population of the municipality according to yearly IBGE projections. The participation rate is the number of votes casted in a municipality on the first round of an election according to the TSE divided by the population of the municipality according to yearly IBGE projections.

The demographic characteristics used to estimate the heterogeneous effects of the Electoral Revision come from the 2010 census. The variable TV is the percentage of households in a municipality with at least one TV equipment at home according to the 2010 census. The variable radio is the percentage of households in a municipality with at least one radio equipment at home according to the 2010 census. The variable Internet is the percentage of households in a municipality with at least one PC with access to the Internet at home according to the 2010 census. . The variable Primary School is the percentage of citizens with at least 15 years of age that completed Primary School according to the 2010 census. The variable Literate is the percentage of citizens with at least 10 years of age that are literate according to the 2010 census.

The municipal expenditures data comes from the National Treasure. The variables created for our main results are the log of the average per capita municipal expenditures for the years of 2005-2007 and 2009-2011 in health care, education and social assistance. The last year of both mayor's terms are excluded due to lack of expenditure data for 2012.

The municipal public school infrastructure data comes from the annual "Censo Escolar". The variables created are the percentage of municipal schools with no access to water in the municipality; the percentage of municipal schools with access to the public electric grid in the municipality; the percentage of municipal schools with no sewer system in the municipality; the percentage

of municipal schools with an Info Lab in the municipality; the percentage of municipal schools with a library in the municipality; the average number of computers to students in municipal schools in the municipality ; the percentage of municipal schools with access to the Internet in the municipality; the percentage of municipal schools with a Science Lab in the municipality.

Health outcomes comes from the "DataSus" data base. The first variable created is the percentage of mothers in the municipality that made less than 4 prenatal visits. The second variable created is percentage of babies born with less than 2.5 kg in the municipality.

4
Identification Strategy

Using a differences in differences strategy, we will identify the effect of the Electoral Revision on registration and participation rates estimating the model below by OLS:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \alpha X_{ist} + e_{ist} \qquad (4\text{-}1)$$

Where Y_{ist} is the outcome of interest i, in state s, in year t; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t \geq 2008$ and the municipality i went through an Electoral Revision in 2007; X_{ist} is the log of the municipality population.

Therefore, our identification hypothesis will be that, in the absence of the Electoral Revisions, the trend in registration and participation rates of the "control" and "treatment" groups would be the same (the "parallel trend assumption").

Figure 2 and 3 show the evolution of the registration rate (registered voters/population) and participation rates (votes casted/population) of municipalities that went through the Electoral Revision and municipalities that did not. The two groups show similar trends before the Electoral Revision in both graphs, suggesting that a differences in differences framework is adequate to estimate the effect of the re-registration process[1].

In Annex .1, we also provide the graphs of the evolution of the economic outcomes variables since we will be also using a differences in differences strategy to identify the impact of the electoral revision on them [2].

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[1]A more formal test of the parallel trend assumption is presented in Section 6.1, with the estimation of a model that includes leads and lags of a treatment dummy that equals one in 2008 in municipalities the went through the re-registration program.

[2]A more detailed discussion about the timing of the Electoral Revision impact on the economic outcomes will be avaible in Subsection 5.3.

5
Results

5.1
Effect of the Electoral Revisions on registration and participation rates

The objective of this subsection is to provide empirical evidence that the Electoral Revision disfranchised legitimate voters, mainly the ones with less education and with less access to media.

There is an extensive literature showing that voters with lower socio economic status are less likely to be registered and to vote. Less educated voters might have more difficulty with the bureaucratic task involved in the re-registration process, such as providing the necessary documentation. Moreover, voters with less access to media might also be disfranchised, since the program is publicized by TV, radio and Internet. Therefore, people who do not have access to these medias might not even know that the program is been implemented in their municipalities.

The sample in all estimations of this subsection includes the first round of two elections (2004 and 2006) before the Electoral Revision and two elections after (2008 and 2010). All estimations have the municipality as the unit of observation[1] and includes municipalities fixed effect, year-state dummies and the log of the population of the municipality as a control.

Column 1 of Table 2 reports the estimated treatment effect of the Electoral Revision on the registration rate according to the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \alpha X_{ist} + e_{ist} \tag{5-1}$$

Where Y_{ist} is registration rate of municipality i, in state s, in year $t = 2004, 2006, 2008, 2010$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2008, 2010$ and the municipality i went through an Electoral Revision in 2007; X_{ist} is the log of the municipality population.

The estimation result suggests that the Electoral Revision reduced in 9.9 p.p. (13% of the dependent variable mean) the registration rate.

Column 2 of Table 2 estimates the same model above using as dependent variable the log of the number of registered voters.The estimation result suggest that the Electoral Revision reduced in 12.7% the number of registered voters.

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[1]9 municipalities with registration rates above 2 are excluded from the sample in all estimations. Anyway, the results are robust to the inclusion of these outliers.

The TSE provides data for the number of registered voters by education level by municipality. Therefore, to test if less educated citizens were disproportionately disenfranchised, we split the effect of the Electoral Revision on the number of registered voters according to their education level.

Column 3 of Table 2 reports the estimated treatment effect of the Electoral Revision on the log of the number of registered voters that did not complete high school. The estimation suggests that the Electoral Revision reduced in 13.3% the number of less educated registered voters.

On the other hand, Column 4 reports the estimated treatment effect of the Electoral Revision on the log of the number of registered voters that completed high school. The estimation suggests that the Electoral Revision reduced in 2.8% the number of educated registered voters.

Therefore, the reduction in the registration of less educated voters is almost 5 times larger than of the educated voters, suggesting the Electoral Revision disproportionately affected less educated voters.

As discussed above, we should also expect the re-registration process to have a more pronounced impact in municipalities with low levels of education and media penetration. Columns 1 through 5 of Table 3 report the heterogeneous effects of the Electoral Revision according to the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \beta D_{ist} * H_{is} + \alpha X_{ist} + e_{ist} \qquad (5\text{-}2)$$

Where Y_{ist} is registration rate of municipality i, in state s, in year $t = 2004, 2006, 2008, 2010$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2008, 2010$ and the municipality i went through an Electoral Revision in 2007; H_{is} = Media Penetration, Education Level; X_{ist} is the log of the municipality population

All interaction coefficients are positive and significant as expected, municipalities with low levels of education and media penetration had bigger declines in registration rates. For example, results suggest that a municipality with 59% of radio penetration (10% percentile) had a decline in the registration rate 3.4 p.p. larger than a municipality in which radio penetration is 93% (90% percentile).

This reduction in the numbers of registered voters translated into a reduction in the actual number of votes casted in the municipalities that went through the Electoral Revision[2].

Column 1 of Table 4 reports the estimated treatment effect of the Electoral Revision on the log of the number of votes casted. The estimation

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[2]Conceivably the electoral revision could have excluded only registered voters that would not show up at the poll anyway.

suggests that the Electoral Revision reduced in 6.5 percent the number of votes casted.

Column 2 of Table 4 reports the estimated treatment effect of the Electoral Revision on the participation rate. The estimation suggests that the Electoral Revision reduced in 4.6 p.p. (8% of the dependent variable mean) the participation rate.

Columns 3 through 7 of Table 4 report the heterogeneous effects of the Electoral Revision on the participation rate. The interaction coefficients of tv penetration, radio penetration and percentage of literate population are positive and significant as expected, suggesting the Electoral Revision had a stronger impact in the participation rate of municipalities with low levels of education and media penetration.

Therefore, the results of this subsection corroborate the idea that the Electoral Revision disfranchised legitimate voters, mainly less educated and with less access to media ones.

5.2
Effect of the Electoral Revision on the provision of education and health care by municipal governments

Subsection 5.1 provided evidence that the Electoral Revision disfranchised uninformed voters. In this subsection, using a differences in differences strategy, I will provide evidence that this disfranchisement resulted in a reduction of the provision of public services that disproportionately benefits these citizens, namely education and health care.

Since the Electoral Revision took place in late 2007, the first municipal election after the intervention is in 2008. Therefore, our estimations will compare economic outcomes (municipal expenditures, public school infrastructure, health outcomes) in the term of the last mayor elected before the electoral revision (2005-2008) with the term of the first mayor elected after the Electoral Revision (2009-2012).

Table 5 reports the estimated treatment effect of the Electoral Revision on municipal expenditures in education (column 1), social assistance (column 2) and health care (column 3) according to the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \alpha X_{ist} + e_{ist} \qquad (5\text{-}3)$$

Where Y_{ist} is the log of the average per capita expenditures in municipality i, in state s, in term $t = 2005 - 2007, 2009 - 2011$[3]; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if

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[3]The last year of both mayor's terms are excluded due to lack of expenditure data for 2012.

$t = 2009 - 2011$ and the municipality i went through an Electoral Revision in 2007; X_{ist} is the log of the municipality population.

The results suggest that the Electoral Revision reduced municipal expenditures in education in 4.1% and in health care in 3.0%. The effect on social assistance expenditure is not significant at usual confidence levels.

Therefore, politicians seem to have responded to the change in the composition of the electorate by reducing expenditures in areas that disproportionately benefits poor and less educated citizens.

Now, we will investigate if this reduction in expenditures translated in a real deterioration of education and health care services.

Table 6 reports the estimated treatment effect of the Electoral Revision on municipal public school infrastructure (percentage of schools without water, percentage of schools with public electricity,percentage of schools without access to a sewer system, percentage of schools with a computer lab, percentage of schools with a science lab, percentage of schools with a library, number of student's computer per school, percentage of schools with access to the Internet) according to the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + e_{ist} \tag{5-4}$$

Where Y_{ist} is a infrastructure measure in municipality i, in state s, in year $t = 2008, 2012$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2012$ and the municipality i went through an Electoral Revision in 2007.

The results suggest that the Electoral Revision reduced the percentage of municipal schools with public electricity in 1.3 p.p. (1.9%), computer labs in 2.1 p.p. (7.5%) and access to the Internet in 3.1 p.p. (8.7%). The program also reduced the number of student's computers per school in 8% and increased the percentage of schools without water in 0.61 p.p. (20%).

Therefore, municipalities that went through the re-registration process suffered deterioration in a wide range of infrastructure measures.

Lastly, I will investigate if the reduction in health expenditures translated into a deterioration of health services (percentage of mother's with less than 4 prenatal visits and percentage of newborns with low weight).

Table 7 reports the estimated treatment effect of the Electoral Revision on the percentage of mother's with less than 4 prenatal visits and the percentage of newborns with low weight according to the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + e_{ist} \tag{5-5}$$

Where Y_{ist} is the health outcome in municipality i, in state s, in term $t = 2005 - 2008, 2009 - 2012$; α_i is the municipality fixed effect; λ_{st} is a state-

year fixed effect; D_{ist} is a dummy equal to one if $t = 2008 - 2011$ and the municipality i went through an Electoral Revision in 2007.

Columns 1 and 4 of Table 7 suggest that the Electoral Revision increased the percentage of mothers with less than 4 prenatal visits in 0.26 p.p. (2.7%) and the percentage of low weight births in 0.22 p.p. (2.9%).

In columns 2 and 3 of Table 7, I separately estimate the effect of the electoral revision in the percentage of mothers with few prenatal visits for mothers that did not complete high school and for mothers that did. In columns 5 and 6, I do the same thing using low weight births as dependent variable.

The results are not statistically significant for the percentage of mother's with less than 4 prenatal visits for less educated or educated mothers. Nevertheless, the result for less educated mothers is only marginaly not significant with a p-value of 0.116. On the others hand, the result for educated mothers is not significant with a p-value of 0.307.

The results for the percentage of low weight births only show a statistically significant effect of the Electoral Revision for mothers with less than a high school diploma. Moreover, the point estimate for less educated mothers is 2.5 times larger than the point estimate for educated mothers.

Therefore, these results indicate that the disenfranchisement of less educated voters, generated by the re-registration process, led to a deterioration of the health service provision to these less educated voters.

5.3
Timing of the effect of the Electoral Revision on the provision of education and health care by municipal governments

In this subsection, we will investigate the timing of the effects of the Electoral Revision on public service provision. This exercise has two main objectives: the first one is to show that there is no difference in the trend of the outcome variables before the Electoral Revision took place and the second one is to show that the effects of the electoral revision started in 2009 or later, when the newly elected mayor after the electoral revision started his term.

Each cell in columns 1 and 3 of Table 8 shows the coefficient β estimated by a differences in differences model using pairs of years t and $t - 1$. Columns 2 and 4 show the respective p-value of the coefficient. For example, in the cell of column 1 and line 1, the β reported is estimated by the following model:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \alpha X_{ist} + e_{ist} \tag{5-6}$$

Where Y_{ist} is the log of per capita education expenditure in municipality i, in state s, in year $t = 2002, 2003$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2003$ and the

municipality i went through an Electoral Revision in 2007 and zero otherwise; X_{ist} is the log of the municipality population.

In the cell of column 1 and line 2, the model been estimated is:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \alpha X_{ist} + e_{ist} \tag{5-7}$$

Where Y_{ist} is the log of per capita education expenditure in municipality i, in state s, in year $t = 2003, 2004$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2004$ and the municipality i went through an Electoral Revision in 2007 and zero otherwise; X_{ist} is the log of the municipality population.

The same logic applies to the other lines and columns of the table.

The results suggest that there is almost no statistical difference in the trend of education and health expenditures between municipalities with and without the Electoral Revision before 2009. Moreover, there is a sharp and statistically significant reduction in expenditures exactly in 2009, the first year in office of the mayors elected after the Electoral Revision.

Table 9 follows the same logic of table 8, but now the dependent variables are the school infrastructure variables. The results suggest that there is no statistical difference in the trend of the proportion of schools with Internet, public electricity and without water between municipalities with and without the Electoral Revision before 2009. Moreover, there is a sharp and statistically significant deterioration of these infrastructure measures in the first couple of year of the newly elected mayor. The result is not so clear for the proportion of schools with Info Lab, since there is already some deterioration in this infrastructure variable in 2008.

Table 10 follows the same logic of table 8, but now the dependent variables are 2 years average of the health outcome variables[4].

The results suggest that there is no statistical difference in the trend of the proportion of uneducated mothers with less than 4 prenatal before 2009. Moreover, there is a statistically significant increase in the proportion of mothers with less than 4 prenatal visits exactly in the first couple of years of the newly elected mayor.

The result is not so clear for the proportion of low weight births despite of the positive effect estimated for the first couple of years of the newly elected mayor, since there is already some deterioration in this variable before 2009.

Therefore, results in this Subsection are generally compatible with the idea that there is no difference in the trend of the outcome variables before

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[4]We aggregate the data in two years because the incidence of low-weights births is volatile looking at one year data

the Electoral Revision took place and that the effects of the electoral revision started in 2009, when the newly elected mayor after the Electoral revision started his term.

6
Robustness and Alternative Explanations

6.1
Parallel Trend Assumption

A more formal test of the parallel trend assumption is the estimation of a model that includes leads and lags of a treatment dummy that equals one in 2008 in municipalities the went through the re-registration program.

The idea is that the coefficients of the leads of the treatment dummy should be close to zero, indicating that before the treatment the trends of the control and treatment group were the same. On the other hand, the treatment dummy and its lags should be negative, suggesting the Electoral Revision reduced registration and participation rates.

Figure 4 shows the coefficients of the leads and lags of the Electoral Revision dummy of the estimated model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \sum_{l=-4}^{l=3} \beta_l D_{istl} + \alpha X_{ist} + e_{ist} \tag{6-1}$$

Where Y_{ist} is the log of registered voters of municipality i in state s in year t; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist0} is a dummy equal to one if $t = 2008$ and the municipality i went through an Electoral Revision in 2007; D_{ist-1} is the first lead of D_{ist0} and so on for $l = -4, -3, -2, 1, 2, 3$; X_{ist} is the log of the municipality population.

Figure 4 shows that the coefficients of the leads are very close to zero, suggesting that the parallel trend assumption is valid.

Like Figure 4, Figure 5 and Figure 6 show the coefficients of the leads and lags of the Electoral Revision dummy of models where the dependent variables are the log of less educated registered voters and the log of voters . These graphs also suggest that parallel trend assumption is valid.

Therefore, a differences in differences framework seems like a good identification strategy for the effects of the electoral revisions.

6.2
Biometric Re-registration

The Biometric Re-registration is gradually been implemented by the Electoral Supreme Court in the whole country since 2008. The objective of the program is to electronic register the fingerprints of all Brazilian voters. In

the election day, voters will use their fingerprints as identification. This way, it becomes almost impossible for someone to illegally vote with another person's name.

Just like in the Electoral Revision, a voter has to personally go to an electoral office with an identification document and proof of residency. If the voter fails to show up until the deadline, his electoral registration is canceled and the voter cannot participate in the following election.

Biometric voting happened in 3 municipalities in 2008, 64 in 2010, 291 in 2012 and 753 in 2014. The goal is to Re-register all Brazilian voters in the next years.

Therefore, the Biometric Re-registration differs in a major way from the Electoral Revision, the first one is been implemented in all Brazilian municipalities, whereas, the second one tries to use objective criteria to target municipalities that potentially have electoral fraud.

The Biometric Re-registration already happened in big cities, including state capitals, where it's very hard to believe that local politicians could significantly alter the size of the electorate by engaging in fraud without been noticed. For example, Curitiba (1,864,416 habitants), Recife (1,608,488 habitants), Goiania (1,412,364 habitants), among others.

Therefore, as a robustness check, I will verify if the Biometric Re-registration had similar effects on the registration and participation rate to the Electoral Revision. Similar results would suggest that my previous results are not been driven by a reduction of fraud.

The sample in all estimations of this subsection includes the first round of elections in 2004, 2006, 2008, 2010, 2012 and 2014; all estimations have the municipality as the unit of observation and includes municipality fixed effect, year-state dummies and the log of the population of the municipality as a control.

Tables 11 and 12 estimate the impact of the Biometric Re-registration on the registration and participation rate, just like tables 3 and 4 for the Electoral Revision.

The estimation results in Table 11 suggest that the Biometric Re-registration reduced in 8.7 p.p. the registration rate. All interaction coefficients, with the exception of radio penetration, are positive and significant as expected, municipalities with low levels of education and media penetration had larger declines in registration rates.

Table 12 reports the estimated treatment effect of the Biometric Re-registration on the participation rate. The estimation suggests that the Electoral Revision reduced in 1.6 p.p. the participation rate. The interaction

coefficients of Internet penetration, percentage of population that completed primary school and percentage of literate population are positive and significant as expected, suggesting the Electoral Revision had a stronger impact in the participation rate of municipalities with low levels of education and media penetration. The only result that do not corroborate the results of the Electoral Revision is the coefficient of the radio interaction in column 3, which is negative and significant.

Anyway, the estimated effects of the Biometric Re-registration are largely similar to the effects of Electoral Revision, suggesting that the estimated impacts of the Electoral Revision are not been driven by a reduction of fraud.

6.3
Alternative Explanation: Fraud

Although fraud could be an alternative explanation for the reduction of the registration and participation rate, it is not easy to argue that a reduction of fraud caused a reduction in the provision of education and health care, if anything, one might expect that a reduction in electoral fraud might improve public service provision.

A particular case in which a reduction of electoral fraud could result in a reduction of the provision of health care and education would be if politicians who engage in fraud systematically spend more money in these areas than the politicians that do not engage in fraud. For example, if left parties, which generally spend more money in these areas (Pettersson-Lidbom (2008)), were more engaged in electoral fraud, than a reduction in electoral fraud could be associated with a deterioration in the provision of education and health care.

Hidalgo and Nichter (2014) argue that a reduction of the reelection chances of mayors might be a signal of a reduction of fraud. Therefore, I test if the impact of the program on reelection chances of mayors of left parties and right parties differs.

Column 1 of Table 13 presents the results of the estimation of the following model:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \alpha X_{ist} + e_{ist} \qquad (6\text{-}2)$$

Where Y_{ist} is the percentage of valid votes that the mayor's incumbent party received in the municipality i, in state s, in year $t = 2004, 2008$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2008$ and the municipality i went through an Electoral Revision in 2007; X_{ist} is the log of the municipality population.

The result suggests that the Electoral Revision reduced the percentage of votes to the incumbent party in 2.9 p.p..

In columns 2 and 3, I divide the results. In column 2, I report the effect when the mayor's incumbent party in 2008 is a left party[1] and, in column 3, when it is not.

The results suggest that the electoral revision reduced the incumbent's vote share for right parties, but not for left parties.

In columns 4, 5 and 6, I proceed in the same manner, but now the dependent variable is a dummy that equals 1 if the incumbent party won the election and 0 if it did not.

The results are qualitatively the same. The electoral revision reduced the chance of reelection for right parties, but not for left parties.

Therefore, the hypothesis that electoral fraud of left parties were driving my results is discredited.

Moreover, there are other suggestive evidences that fraud is not driving the results. Electoral fraud is associated with high turnout and high vote share for incumbents (Klimek et al. (2012); Enikolopov et al. (2013)). We explore historical turnout and vote share of the winner mayor candidate and find no evidence that high turnout and high vote share before the program is associated with more electoral registrations cancellations.

Columns 1 and 2 of Table 14 report the heterogeneous effects of the Electoral Revision according to the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \beta D_{ist} * H_{is} + \alpha X_{ist} + e_{ist} \qquad (6\text{-}3)$$

Where Y_{ist} is registration rate of municipality i, in state s, in year $t = 2004, 2006, 2008, 2010$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2008, 2010$ and the municipality i went through an Electoral Revision in 2007; $H_{is} = HighTurnout, HighWinner'sVoteShare$; X_{ist} is the log of the municipality population.

The results of Table 14 do not suggest that a purge of fraudulent registration is reducing the registration rate. Historical high turnout and high vote share for the winner is associated with smaller falls in the registration rate, exact the opposite of what would be expected if fraud were driving the results.

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[1]PT, PDT, PCdoB, PSB, PV, PSOL and PPS.

6.4
Other Robustness

First, we will check if the Electoral Revision reduced spending in areas that did not disproportionately affect disfranchised voters. The idea is that the reduction in expenditures should not happen in areas that did not specially benefit uninformed voters in the first place.

Therefore, we will estimate the same model in Equation 4 using as placebos local council legislature expenditures, culture expenditures and transportation expenditures[2]. The results in Table 15 suggest no negative effects of the Electoral Revisions on this expenditure areas.

Second, we will verify if municipalities which suffered a larger impact from the Electoral Revision (larger fall in the registration rate) also had a larger reduction in expenditures in education, health care and social assistance. The idea is that municipalities with larger reductions in the registration rate had more voters disenfranchised and, consequently, a larger reduction in expenditures.

On Table 16, we will estimate the model below:

$$Y_{ist} = \alpha_i + \lambda_{st} + \beta D_{ist} + \theta D_{ist} * \Delta_{2008-2006} Reg_{is} + e_{ist} \qquad (6\text{-}4)$$

Where Y_{ist} is the log of the average per capita expenditures in municipality i, in state s, in term $t = 2005 - 2007, 2009 - 2011$; α_i is the municipality fixed effect; λ_{st} is a state-year fixed effect; D_{ist} is a dummy equal to one if $t = 2009 - 2011$ and the municipality i went through an Electoral Revision in 2007; $\Delta_{2008-2006} Reg_{is}$ is the difference between the registration rate in 2008 and 2006 in municipality i, in state s.

The interaction between the fall in registration rate and the Electoral Revision is positive and significant as expected, municipalities that had larger reductions in the registration rate had larger reduction in expenditures. For example, a fall of 10 p.p. of the registration rate between 2006 and 2008 reduced municipal expenditures in education in 1.6%.

Third, we will perform a placebo test using state public school infrastructure instead of municipal public school infrastructure as dependent variable.

We argued that uninformed voters were disenfranchised and local politicians (mayor and local council legislative members) responded reducing expenditure on education on municipalities that went through the Electoral Revision. Therefore, municipal public school infrastructure deteriorated. This argument does not make any claim about the infrastructure of state public schools

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[2]In small municipalities, transportation expenditures involves mainly paving roads anecdotally.

in municipalities that went through the program. The median voter changed at municipal level, but at state level this change was very small since the program mainly targeted small municipalities. Thus, we probably should not see a deterioration in state public schools in municipalities that went through the program.

Therefore, we will estimate the same model in Equation 5 using state public school infrastructure as dependent variable.

The results in Table 17 suggest no effects of the Electoral Revisions on state public school infrastructure, as expected.

Fourth, we will perform placebo tests for mayor's terms before the program was implemented, testing if the "program" shows any effect on schools infrastructure or health outcomes before it was actually implemented.

Table 18 presents the estimated effect of the "program" on public school infrastructure[3]. In this placebo, we use the infrastructure data in 2004 and 2008 instead of 2008 and 2012. As expected, the results do not suggest any impact of the placebo "program" on public school infrastructure.

Table 19 performs the same placebo exercise of Table 18 for the health outcome variables. As expected, the results do not suggest any impact of the placebo "program" on health outcomes.

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[3]I include the variables that were actually affected by the real Electoral Revision and were reported in the 2004 "Censo Escolar"

7
Conclusion

This paper estimates the effects on the composition of the electorate of a large re-registration program aiming to combat electoral fraud in Brazil. The results suggest that the program disenfranchised a sizable part of the electorate, especially uninformed voters with low levels of education. Consistent with models of redistributive politics, politicians responded to this change in the composition of the electorate by reducing expenditures in areas that disproportionately benefits poor voters: health care and education. In turn, this expenditure reduction deteriorated public school infrastructure and worsened health outcomes. Although we do not find conclusive evidence that the program indeed reduced fraud, these results highlight the trade off democracies face between controlling fraud and reducing the costs of registration and voting.

8
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9
Appendix

.1
Tables and Figures

Table 1: Descriptive Statistics of Municipalities with Electoral Revision and without Electoral Revision

	Electoral Revision	No Electoral Revision
% w/ Complete Primary School	0.38	0.41
% w/ Inadequate Sanitation	0.21	0.19
Mean Income	429.1	438.9
% with income up to 70 reais	0.10	0.10
% with Internet	0.13	0.15
TV Penetration	0.90	0.90
Radio Penetration	0.79	0.77
% with less than 24 years	0.41	0.43
% with between 24 and 60 years	0.45	0.44
Registration Rate	0.86	0.71
% Presidential PT votes in 2006	0.47	0.46
Population (Median)	5,896	13,217
Obeservations	1,186	4,372

Notes: This table presents descriptive statistics comparing municipalities that went through the Electoral Revision and municipalities that did not. Values reported are the mean of each group with the exception of Population, which the median value is reported.

Table 2: Electoral Revision Effect on Total Registered Voters and Divided by Education Level

VARIABLES	(1) Reg. Rate	(2) Log Regist. voters	(3) Log Regist. voters w/ Low Educ.	4) Log Regist. voters w/ High Educ.
Mean of Dep. Var.	0.74			
Electoral Revision	-0.0989***	-0.127***	-0.133***	-0.0278***
	(0.00286)	(0,0320)	(0.00269)	(0.00616)
Ln (Pop)	y	y	y	y
Fixed Effect	y	y	y	y
Year-State Dummy	y	y	y	y
Observations	22,206	22,206	22,206	22,206

Notes: This table presents the impact of the Electoral Revision on the Registration Rate, the log of the number of registered voters and the log of the number of registered voters by education level. Columns 1-4 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of first round election results in municipalities for the years of 2004, 2006, 2008 and 2010. The dependent variable in column 1 is the the number of registered voters according to the TSE divided by the population of the municipality according to IBGE projections. The dependent variable in column 2 is the log of the number of registered voters according to the TSE. The dependent variable in column 3 is the log of the number of registered voters that did not complete high school in a municipality according to the TSE. The dependent variable in column 4 is the log of the number of registered voters that completed high school in a municipality according to the TSE. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2008 or 2010 and zero otherwise. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 3: Electoral Revision Heterogeneous Effect on the Registration Rate

VARIABLES	(1) Reg Rate	(2) Reg Rate	(3) Reg Rate	(4) Reg Rate	(5) Reg Rate
Mean of Dep. Var.	0.74	0.74	0.74	0.74	0.74
Electoral Revision	-0.216***	-0.177***	-0.108***	-0.134***	-0.213***
	(0.0531)	(0.0167)	(0.00492)	(0.0134)	(0.0277)
Electoral Rev.*tv	0.128**				
	(0.0577)				
Electoral Rev.*radio		0.0982***			
		(0.0205)			
Electoral Rev.*Internet			0.0659**		
			(0.0300)		
Electoral Rev.*Primary School				0.0903***	
				(0.0346)	
Electoral Rev.*Literate					0.135***
					(0.0325)
Ln(Pop)	y	y	y	y	y
Fixed Effect	y	y	y	y	y
Year-State Dummy	y	y	y	y	y
Observations	22,206	22,206	22,206	22,206	22,206

Notes: This table presents the impact of the Electoral Revision on the Registration Rate and how this impact varies depending on the media penetration and education levels of the municipality. Columns 1-5 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of first round election results in municipalities for the years of 2004, 2006, 2008 and 2010. The dependent variable on columns 1 - 5 is the number of registered voters in a municipality according to the TSE divided by the population of the municipality according to yearly IBGE projections. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2008 or 2010 and zero otherwise. The variable TV is the percentage of households in a municipality with at least one TV equipment at home according to the 2010 census. The variable radio is the percentage of households in a municipality with at least one radio equipment at home according to the 2010 census. . The variable Internet is the percentage of households in a municipality with at least one PC with access to the Internet at home according to the 2010 census. . The variable Primary School is the percentage of citizens with at least 15 years of age that completed Primary School according to the 2010 census. The variable Literate is the percentage of citizens with at least 10 years of age that are literate according to the 2010 census. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 4: Electoral Revision Effect on the Participation Rate

VARIABLES	(1) Ln(Votes)	(2) Part. Rate	(3) Part. Rate	(4) Part. Rate	(5) Part. Rate	(6) Part. Rate	(7) Part. Rate
Mean of Dep. Var.		0.62	0.62	0.62	0.62	0.62	0.62
Electoral Revision	-0.0653***	-0.0457***	-0.119***	-0.0911***	-0.0492***	-0.0612***	-0.0897***
	(0.0025)	(0.00222)	(0.0415)	(0.0135)	(0.00380)	(0.0103)	(0.0220)
Electoral Rev.*tv			0.0806*				
			(0.0451)				
Electoral Rev.*radio				0.0571***			
				(0.0168)			
Electoral Rev.*Internet					0.0263		
					(0.0235)		
Electoral Rev.*Primary School						0.0402	
						(0.0267)	
Electoral Rev.*Literate							0.0520**
							(0.0258)
Ln(Pop)	y	y	y	y	y	y	y
Fixed Effect	y	y	y	y	y	y	y
Year-State Dummy	y	y	y	y	y	y	y
Observations	22,206	22,206	22,206	22,206	22,206	22,206	22,206

Notes: This table presents the impact of the Electoral Revision on the Participation Rate and how this impact varies depending on the media penetration and education levels of the municipality. Columns 1-7 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of first round election results in municipalities for the years of 2004, 2006, 2008 and 2010. The dependent variable on column 1 is the log of the number of voters in the first round of the elections in a municipality and on columns 2 - 7 is the number of voters in the first round of the elections in a municipality according to the TSE divided by the population of the municipality according to yearly IBGE projections. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2008 or 2010 and zero otherwise. The variable TV is the percentage of households in a municipality with at least one TV equipment at home according to the 2010 census. . The variable radio is the percentage of households in a municipality with at least one radio equipment at home according to the 2010 census. . The variable Internet is the percentage of households in a municipality with at least one PC with access to the Internet at home according to the 2010 census. . The variable Primary School is the percentage of citizens with at least 15 years of age that completed Primary School according to the 2010 census. The variable Literate is the percentage of citizens with at least 10 years of age that are literate according to the 2010 census. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 5: Electoral Revision Effect on Municipal Expenditures

VARIABLES	(1) ln(Education per cap)	(2) ln(Social Assist per cap)	(3) ln(Health Care per cap)
Electoral Revision	-0.0409***	0.00366	-0.0302***
	(0.00624)	(0.0152)	(0.00715)
Ln(Pop)	y	y	y
Fixed Effect	y	y	y
Year-State Dummy	y	y	y
Observations	11,040	11,031	11,038

Notes: This table presents the impact of the Electoral Revision on the Municipal Expenditure. Columns 1-3 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of the average municipal expenditures for the years of 2005-2007 and 2009-2011. The last year of both mayor's terms are excluded due to lack of expenditure data for 2012. The dependent variable in column 1 is the log of the per capita municipal expenditure in education on the municipality according to the National Treasure. The dependent variable in column 2 is the log of the per capita municipal expenditure on Social Assistance in the municipality according to the National Treasure. The dependent variable in column 3 is the log of the per capita municipal expenditure on Health Care in the municipality according to the National Treasure. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the years are 2009-2011 and zero otherwise. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 6: Electoral Revision Effect on Municipal Schools Infrastructure

VARIABLES	(1) w/o water	(2) Public Electricity	(3) w/o sewer	(4) Info Lab
Mean of Dep. Var.	0.03	0.69	0.03	0.28
Electoral Revision	0.00616** (0.00302)	-0.0130*** (0.00427)	0.00263 (0.00226)	-0.0212*** (0.00739)
Fixed Effect	y	y	y	y
Year-State Dummy	y	y	y	y
Observations	11,080	11,080	11,080	11,080

VARIABLES	(5) Library	(6) PCs per School	(7) Internet	(8) Science Lab
Mean of Dep. Var.	0.19	4.3	0.37	0.02
Electoral Revision	-0.00817 (0.00610)	-0.343** (0.160)	-0.0307*** (0.00661)	0.00123 (0.00219)
Fixed Effect	y	y	y	y
Year-State Dummy	y	y	y	y
Observations	11,080	11,080	11,080	11,080

Notes: This table presents the impact of the Electoral Revision on the Municipal Schools Infrastructure. Columns 1-8 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of school's infrastructure for the years of 2008 and 2012. The dependent variable in column 1 is the percentage of municipal schools with no access to water in the municipality according to the Censo Escolar; in column 2 is the percentage of municipal schools with access to the public electric grid in the municipality according to the Censo Escolar; in column 3 is the percentage of municipal schools with no sewer system in the municipality according to the Censo Escolar; in column 4 is the percentage of municipal schools with an Info Lab in the municipality according to the Censo Escolar; in column 5 is the percentage of municipal schools with a library in the municipality according to the Censo Escolar; in column 6 is the average number of computers to students in municipal schools in the municipality according to the Censo Escolar; in column 7 is the percentage of municipal schools with access to the Internet in the municipality according to the Censo Escolar; in column 8 is the percentage of municipal schools with a Science Lab in the municipality according to the Censo Escolar. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2012 and zero otherwise. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 7: Electoral Revision Effect on Mothers's Prenatal Visits and Low Weights Births

VARIABLES	Prenatal			Low Weight		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Low Educ	High Educ	Total	Low Educ	High Educ
Mean of Dep. Var.	0.097	0.105	0.052	0.074	0.076	0.067
Electoral Revision	0.00258*	0.00240	0.00232	0.00223***	0.00246***	0.000956
	(0.00149)	(0.00153)	(0.00227)	(0.000856)	(0.000954)	(0.00232)
Fixed Effect	y	y	y	y	y	y
Year-State Dummy	y	y	y	y	y	y
Observations	11,097	11,093	11,093	11,105	11,105	11,105

Notes: This table presents the impact of the Electoral Revision on Health Outcomes. Columns 1-6 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of average health outcomes for the years of 2005-2008 and 2009-2012. The dependent variable in column 1 is the percentage of mothers in the municipality that made less than 4 prenatal visits according to DataSUS; in column 2 is the percentage of mothers with less than a high school diploma in the municipality that made less than 4 prenatal visits according to DataSUS; in column 3 is the percentage of mothers with at least a high school diploma in the municipality that made less than 4 prenatal visits according to DataSUS; in column 4 is the percentage of babies born with less than 2.5 kg in the municipality according to DataSUS; in column 5 is the percentage of babies born with less than 2.5 kg from mothers with less than a high school diploma in the municipality according to DataSUS; in column 6 is the percentage of babies born with less than 2.5 kg from mothers with at least a high school diploma in the municipality according to DataSUS. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the years are 2009-2012 and zero otherwise. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 8: Timing of Electoral Revision Impact on Expenditures

VARIABLES	Log Education per capita		Log Health per capita	
	(1)	(2)	(3)	(4)
	Coefficient	P-Value	Coefficient	P-Value
2003/2002	-0.0020	0.72	-0.0003	0.97
2004/2003	0.0019	0.77	-0.0005	0.96
2005/2004	-0.0024	0.71	-0.0150	0.26
2006/2005	-0.0123*	0.08	0.0153	0.18
2007/2006	-0.0087	0.16	-0.0055	0.48
2008/2007	-0.0113	0.85	0.0071	0.32
2009/2008	-0.0149***	0.00	-0.0192***	0.01
2010/2009	-0.0003	0.96	-0.0027	0.75
2011/2010	-0.0167**	0.03	-0.0076	0.74

Notes: Each cell in columns 1 and 3 of the table presents the coefficient β from an OLS estimation of a fixed effect panel with year-state dummies such as equation 5-6. Each cell in columns 2 and 4 shows the respective p-value of the coefficient. The sample in line 1 is composed of municipalities in the years of 2002 and 2003; the sample in line 2 is composed of municipalities in the years of 2003 and 2004; the sample in line 3 is composed of municipalities in the years of 2004 and 2005; the sample in line 4 is composed of municipalities in the years of 2005 and 2006; the sample in line 5 is composed of municipalities in the years of 2006 and 2007; the sample in line 6 is composed of municipalities in the years of 2007 and 2008; the sample in line 7 is composed of municipalities in the years of 2008 and 2009; the sample in line 8 is composed of municipalities in the years of 2009 and 2010; the sample in line 9 is composed of municipalities in the years of 2010 and 2011. The dependent variable in columns 1 and 2 is the log of the per capita municipal expenditure in education on the municipality according to the National Treasure. The dependent variable in columns 3 and 4 is the log of the per capita municipal expenditure on Social Assistance in the municipality according to the National Treasure. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 9: Timing of Electoral Revision Impact on School Infrastructure

VARIABLES	Internet		Public Electricity		Lab Info		Without Water	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Coefficient	P-Value	Coefficient	P-Value	Coefficient	P-Value	Coefficient	P-Value
2005/2004	0.0013	0.74	-0.0038	0.40	0.0109*	0.07	-0.0002	0.81
2006/2005	0.0063	0.16	0.0022	0.59	0.0011	0.70	0.0001	0.79
2007/2006	0.0019	0.71	-0.0021	0.53	-0.0036	0.31	0.0002	0.72
2008/2007	-0.0026	0.58	-0.0003	0.86	-0.0060*	0.09	-0.0001	0.67
2009/2008	-0.0059	0.19	-0.0060***	0.01	-0.0057	0.21	0.0001	0.55
2010/2009	-0.0097**	0.02	-0.0028	0.20	-0.0043	0.35	0.0006	0.30
2011/2010	-0.0095***	0.00	-0.0039**	0.06	-0.0084*	0.07	0.0055*	0.08
2012/2011	-0.0054*	0.07	-0.0001	0.94	-0.0023	0.51	-0.0002	0.88

Notes: Each cell in columns 1, 3, 5 and 7 of the table presents the coefficient β from an OLS estimation of a fixed effect panel with year-state dummies such as equation 5-6. Each cell in columns 2, 4, 6 and 8 shows the respective p-value of the coefficient. The sample in line 1 is composed of municipalities in the years of 2004 and 2005; the sample in line 2 is composed of municipalities in the years of 2005 and 2006; the sample in line 3 is composed of municipalities in the years of 2006 and 2007; the sample in line 4 is composed of municipalities in the years of 2007 and 2008; the sample in line 5 is composed of municipalities in the years of 2008 and 2009; the sample in line 6 is composed of municipalities in the years of 2009 and 2010; the sample in line 7 is composed of municipalities in the years of 2010 and 2011. The dependent variable in columns 1 and 2 is the percentage of municipal schools with access to the Internet in the municipality according to the Censo Escolar; the dependent variable in columns 3 and 4 is the percentage of municipal schools with access to the public electric grid in the municipality according to the Censo Escolar; the dependent variable in columns 5 and 6 is the percentage of municipal schools with an Info Lab in the municipality according to the Censo Escolar; the dependent variable in columns 7 and 8 is the percentage of municipal schools with no access to water in the municipality according to the Censo Escolar. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 10: Timing of Electoral Revision Impact on Health Outcomes

VARIABLES	Prenatal uneducated		Low Weight uneducated	
	(1)	(2)	(3)	(4)
	Coefficient	P-Value	Coefficient	P-Value
(2004-2003)/(2002-2001)	0.0037	0.12	-0.001	0.32
(2006-2005)/(2004-2003)	-0.0017	0.37	0.002*	0.05
(2008-2007)/(2006-2005)	0.0005	0.76	-0.002	0.16
(2010-2009)/(2008-2007)	0.0028**	0.04	0.003**	0.02
(2012-2011)/(2010-2009)	-0.0018	0.28	0.000	0.90

Notes: Each cell in columns 1 and 3 of the table presents the coefficient β from an OLS estimation of a fixed effect panel with year-state dummies such as equation 5-6. Each cell in columns 2 and 4, shows the respective p-value of the coefficient. The dependent variable in line 1 is the average health outcome for years 2002-2001 and years 2004-2003; the dependent variable in line 2 is the average health outcome for years 2006-2005 and years 2004-2003; the dependent variable in line 3 is the average health outcome for years 2006-2005 and years 2008-2007; the dependent variable in line 4 is the average health outcome for years 2010-2009 and years 2008-2007; the dependent variable in line 5 is the average health outcome for years 2010-2009 and years 2012-2011; The dependent variable in columns 1 and 2 is the percentage of mothers with less than a high school diploma in the municipality that made less than 4 prenatal visits according to DataSUS; The dependent variable in columns 3 and 4 is the percentage of babies born with less than 2.5 kg from mothers with less than a high school diploma in the municipality according to DataSUS. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 11: Biometric Re-registration Effect on the Registration Rate

VARIABLES	(1) Reg Rate	(2) Reg Rate	(3) Reg Rate	(4) Reg Rate	(5) Reg Rate	(6) Reg Rate
Mean of Dep. Var.	0.74	0.74	0.74	0.74	0.74	0.74
Bio	-0.0874***	-0.206***	-0.0676***	-0.113***	-0.174***	-0.308***
	(0.00405)	(0.0520)	(0.0188)	(0.00631)	(0.0137)	(0.0308)
Bio*TV		0.129**				
		(0.0553)				
Bio*Radio			-0.0256			
			(0.0229)			
Bio*Internet				0.176***		
				(0.0334)		
Bio*Primary School					0.211***	
					(0.0317)	
Bio*Literate						0.258***
						(0.0358)
Ln(Pop)	y	y	y	y	y	y
Fixed Effect	y	y	y	y	y	y
Year-State Dummy	y	y	y	y	y	y
Observations	33,300	33,298	33,298	33,298	33,298	33,298

Notes: This table presents the impact of the Biometric Re-registration on the Registration Rate and how this impact varies depending on the media penetration and education levels of the municipality. Columns 1-6 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of first round election results in municipalities for the years of 2004, 2006, 2008, 2010, 2012 and 2014. The dependent variable is the number of registered voters in a municipality according to the TSE divided by the population of the municipality according to yearly IBGE projections. The variable Bio is a dummy equal to 1 if the municipality went through the Biometric Re-registration in that year or any year before and zero otherwise. The variable TV is the percentage of households in a municipality with at least one TV equipment at home according to the 2010 census. . The variable radio is the percentage of households in a municipality with at least one radio equipment at home according to the 2010 census. . The variable Internet is the percentage of households in a municipality with at least one PC with access to the Internet at home according to the 2010 census. . The variable Primary School is the percentage of citizens with at least 15 years of age that completed Primary School according to the 2010 census. The variable Literate is the percentage of citizens with at least 10 years of age that are literate according to the 2010 census. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 12: Biometric Re-registration Effect on the Participation Rate

VARIABLES	(1) Part. Rate	(2) Part. Rate	(3) Part. Rate	(4) Part. Rate	(5) Part. Rate	(6) Part. Rate
Mean of Dep. Var.	0.62	0.62	0.62	0.62	0.62	0.62
Bio	-0.0166***	-0.0474	0.0339**	-0.0297***	-0.0710***	-0.0846***
	(0.00299)	(0.0349)	(0.0143)	(0.00465)	(0.0106)	(0.0220)
Bio*TV		0.0335				
		(0.0373)				
Bio*Radio			-0.0651***			
			(0.0175)			
Bio*Internet				0.0903***		
				(0.0248)		
Bio*Primary School					0.133***	
					(0.0248)	
Bio*Literate						0.0792***
						(0.0260)
Ln(Pop)	y	y	y	y	y	y
Fixed Effect	y	y	y	y	y	y
Year-State Dummy	y	y	y	y	y	y
Observations	33,300	33,298	33,298	33,298	33,298	33,298

Notes: This table presents the impact of the Biometric Re-registration on the Participation Rate and how this impact varies depending on the media penetration and education levels of the municipality. Columns 1-6 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of first round election results in municipalities for the years of 2004, 2006, 2008, 2010, 2012 and 2014. The dependent variable is the number of votes casted in a municipality according to the TSE divided by the population of the municipality according to yearly IBGE projections. The variable Bio is a dummy equal to 1 if the municipality went through the Biometric Re-registration in that year or any year before and zero otherwise. The variable TV is the percentage of households in a municipality with at least one TV equipment at home according to the 2010 census. . The variable radio is the percentage of households in a municipality with at least one radio equipment at home according to the 2010 census. . The variable Internet is the percentage of households in a municipality with at least one PC with access to the Internet at home according to the 2010 census. . The variable Primary School is the percentage of citizens with at least 15 years of age that completed Primary School according to the 2010 census. The variable Literate is the percentage of citizens with at least 10 years of age that are literate according to the 2010 census. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 13: Electoral Revision Impact on Incumbent Party Voting

VARIABLES	% votes incumbent party			% probability of party reelection		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Left	Right	Total	Left	Right
Mean of Dep. Var.	0.43	0.39	0.44	0.28	0.21	0.31
Electoral Revision	-0.0320***	0.00714	-0.0395***	-0.0410*	0.0474	-0.0581**
	(0.00993)	(0.0265)	(0.0105)	(0.0228)	(0.0461)	(0.0259)
Ln(Pop)	y	y	y	y	y	y
Fixed Effect	y	y	y	y	y	y
Year-State Dummy	y	y	y	y	y	y
Observations	6,459	1,397	5,062	11,101	2,528	8,570

Notes: This table presents the impact of the Electoral Revision on the probability that the party of the incumbent mayor stay in power. Columns 1-6 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample in columns 1 and 3 is composed of first round election results in municipalities for the years of 2004 and 2008. The sample in columns 2 and 5 is composed of first round election results in municipalities where the incumbent mayor was from a left party in 2008 for the years of 2004 and 2008. The sample in columns 3 and 6 is composed of first round election results in municipalities where the incumbent mayor was not from a left party in 2008 for the years of 2004 and 2008. The dependent variable in columns 1-3 is the number of votes for the party of the incumbent mayor in the mayoral elections according to the TSE divided by the total number of votes casted according to the TSE. The dependent variable in columns 4-6 is a dummy that is equal to one if a candidate from the mayor's party is elected (the mayor himself or some other canditade of his party). The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the years are 2008 and zero otherwise. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 14: Electoral Revision Heterogeneous Effect on the Registration Rate

VARIABLES	(1) Reg Rate	(2) Reg Rate
Mean of Dep. Var.	0.74	0.74
Electoral Revision	-0.136	-0.107***
	(0.0040)	(0.0040)
Electoral Rev.*High turnout	0.0596***	
	(0.0577)	
Electoral Rev.*High winner's vote share		0.0144***
		(0.0049)
Ln(Pop)	y	y
Fixed Effect	y	y
Year-State Dummy	y	y
Observations	22,206	22,206

Notes: This table presents the impact of the Electoral Revision on the Registration Rate and how this impact varies depending on variables that correlates with the presence of fraud. Columns 1-2 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of first round election results in municipalities for the years of 2004, 2006, 2008 and 2010. The dependent variable on columns 1 - 2 is the number of registered voters in a municipality according to the TSE divided by the population of the municipality according to yearly IBGE projections. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2008 or 2010 and zero otherwise. The variable High turnout is a dummy equal 1 if the average turnout on the 2004 and 2000 elections of the municipality is above the national average. The variable High winner's vote share is a dummy equal 1 if the average vote share fo the winning mayor candidate on the 2004, 2000 and 1996 elections of the municipality is above the national average. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 15: Placebo: Electoral Revision Effect on Municipal Expenditures

VARIABLES	Council (1)	Culture (3)	Transportation (5))
Electoral Revision	-0.0299	0.0783	0.0617
	(0.0218)	(0.0481)	(0.0390)
Ln(Pop)	y	y	y
Fixed Effect	y	y	y
Year-State Dummy	y	y	y
Observations	10,158	10,385	10,076

Notes: This table presents the impact of the Electoral Revision on placebo Municipal Expenditures. Columns 1-3 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of the average municipal expenditures for the years of 2005-2007 and 2009-2011.The last year of both mayor's terms are excluded due to lack of expenditure data for 2012. The dependent variable in column 1 is the log of the per capita municipal expenditure in the local council on the municipality according to the National Treasure. The dependent variable in column 2 is the log of the per capita municipal expenditure on Culture in the municipality according to the National Treasure. The dependent variable in column 3 is the log of the per capita municipal expenditure on Transportation in the municipality according to the National Treasure. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the years are 2009-2011 and zero otherwise. The variable Ln(Pop) is the log of the population of the municipality according to yearly IBGE projections. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 16: Heterogeneous Impact of the Electoral Revision on Municipal Expenditures

VARIABLES	(1) ln(Education per cap)	(2) ln(Social Assist. per cap)	(3) ln(Health Care per cap)
Electoral Revision	-0.0355***	-0.00265	-0.0301***
	(0.00730)	(0.0174)	(0.00762)
Electoral Rev.*Δ Registration 2008 - 2006	0.167***	0.235***	0.123***
	(0.0361)	(0.0877)	(0.0349)
Fixed Effect	y	y	y
Year-State Dummy	y	y	y
Observations	11,040	11,033	11,039

Notes: This table presents the impact of the Electoral Revision on Municipal Expenditures according to how much the Electoral Revision reduced the registration rate in the municipality. Columns 1-3 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of the average municipal expenditures for the years of 2005-2007 and 2009-2011. The dependent variable in column 1 is the log of the per capita municipal expenditure in education on the municipality according to the National Treasure. The dependent variable in column 2 is the log of the per capita municipal expenditure on Social Assistance in the municipality according to the National Treasure. The dependent variable in column 3 is the log of the per capita municipal expenditure on Health Care in the municipality according to the National Treasure. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the years are 2009-2011 and zero otherwise. Δ Registration 2008 - 2006 is the registration rate of municipality i in 2008 minus the registration rate of municipality i in 2006. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 17: Placebo: Electoral Revision Effect on State School Infrastructure

VARIABLES	(1) w/o water	(2) Public Electricity	(3) w/o sewer	(4) Info Lab
Electoral Revision	0.000452	-0.00466	0.00234	-0.00918
	(0.00139)	(0.00437)	(0.00232)	(0.00971)
Fixed Effect	y	y	y	y
Year-State Dummy	y	y	y	y
Observations	11,096	11,096	11,096	11,096

VARIABLES	(5) Library	(6) PCs per School	(7) Internet	(8) Science Lab
Electoral Revision	-0.00484	-0.212	0.0183*	0.00670
	(0.00950)	(0.413)	(0.0102)	(0.00886)
Fixed Effect	y	y	y	y
Year-State Dummy	y	y	y	y
Observations	11,096	11,096	11,096	11,095

Notes: This table presents the impact of the Electoral Revision on State Schools Infrastructure. Columns 1-8 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of school's infrastructure for the years of 2008 and 2012. The dependent variable in column 1 is the percentage of State schools with no access to water in the municipality according to the Censo Escolar; in column 2 is the percentage of State schools with access to the public electric grid in the municipality according to the Censo Escolar; in column 3 is the percentage of State schools with no sewer system in the municipality according to the Censo Escolar; in column 4 is the percentage of State schools with an Info Lab in the municipality according to the Censo Escolar; in column 5 is the percentage of State schools with a library in the municipality according to the Censo Escolar; in column 6 is the average number of computers to students in State schools in the municipality according to the Censo Escolar; in column 7 is the percentage of State schools with access to the Internet in the municipality according to the Censo Escolar; in column 8 is the percentage of State schools with a Science Lab in the municipality according to the Censo Escolar. The variable Electoral Revision is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2012 and zero otherwise. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 18: Placebo Test with Years Before the Program Implementation: Municipal School Infrastructure (2004,2008)

VARIABLES	(1) w/o water	(2) Public Electricity	(3) Info Lab	(4) Internet
Placebo	4.62e-05	-0.00382	0.00266	0.00717
	(0.00108)	(0.00656)	(0.00692)	(0.00750)
Fixed Effect	y	y	y	y
Year-State Dummy	y	y	y	y
Observations	11,085	11,085	11,085	11,085

Notes: This table presents placebo tests for the impact of the Electoral Revision on municipal schools infrastructure for mayor terms before the program was implemented. Columns 1-4 presents the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of school's infrastructure for the years of 2004 and 2008. The dependent variable in column 1 is the percentage of municipal schools with no access to water in the municipality according to the Censo Escolar; in column 2 is the percentage of municipal schools with access to the public electric grid in the municipality according to the Censo Escolar; in column 3 is the percentage of municipal schools with an Info Lab in the municipality according to the Censo Escolar; in column 4 is the percentage of municipal schools with access to the Internet in the municipality according to the Censo Escolar. The variable Placebo is a dummy equal to 1 if the municipality went through the Electoral Revision and the year is 2008 and zero otherwise. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.

Table 19: Placebo Test with Years Before the Program Implementation: Health Outcomes (2001-2004,2005-2008)

VARIABLES	(1) Prenatal	(2) Low Weight
Placebo	0.000450	0.000992
	(0.00198)	(0.000809)
Fixed Effect	y	y
Year-State Dummy	y	y
Observations	11,092	11,100

Notes: This table presents placebo tests for the impact of the Electoral Revision on health outcomes for mayor terms before the program was implemented. Columns 1 and 2 present the OLS estimation of a fixed effect panel with year-state dummies. The sample is composed of average health outcomes for the years of 2001-2004 and 2005-2008. The dependent variable in column 1 is the percentage of mothers in the municipality that made less than 4 prenatal visits according to DataSUS; in column 2 is the percentage of babies born with less than 2.5 kg in the municipality according to DataSUS. The variable Placebo is a dummy equal to 1 if the municipality went through the Electoral Revision and the years are 2005-2008 and zero otherwise. Robust Standard Errors in parenthesis. ***Significant at 1% level. **Significant at 5 % level. *Significant at 10 % level.
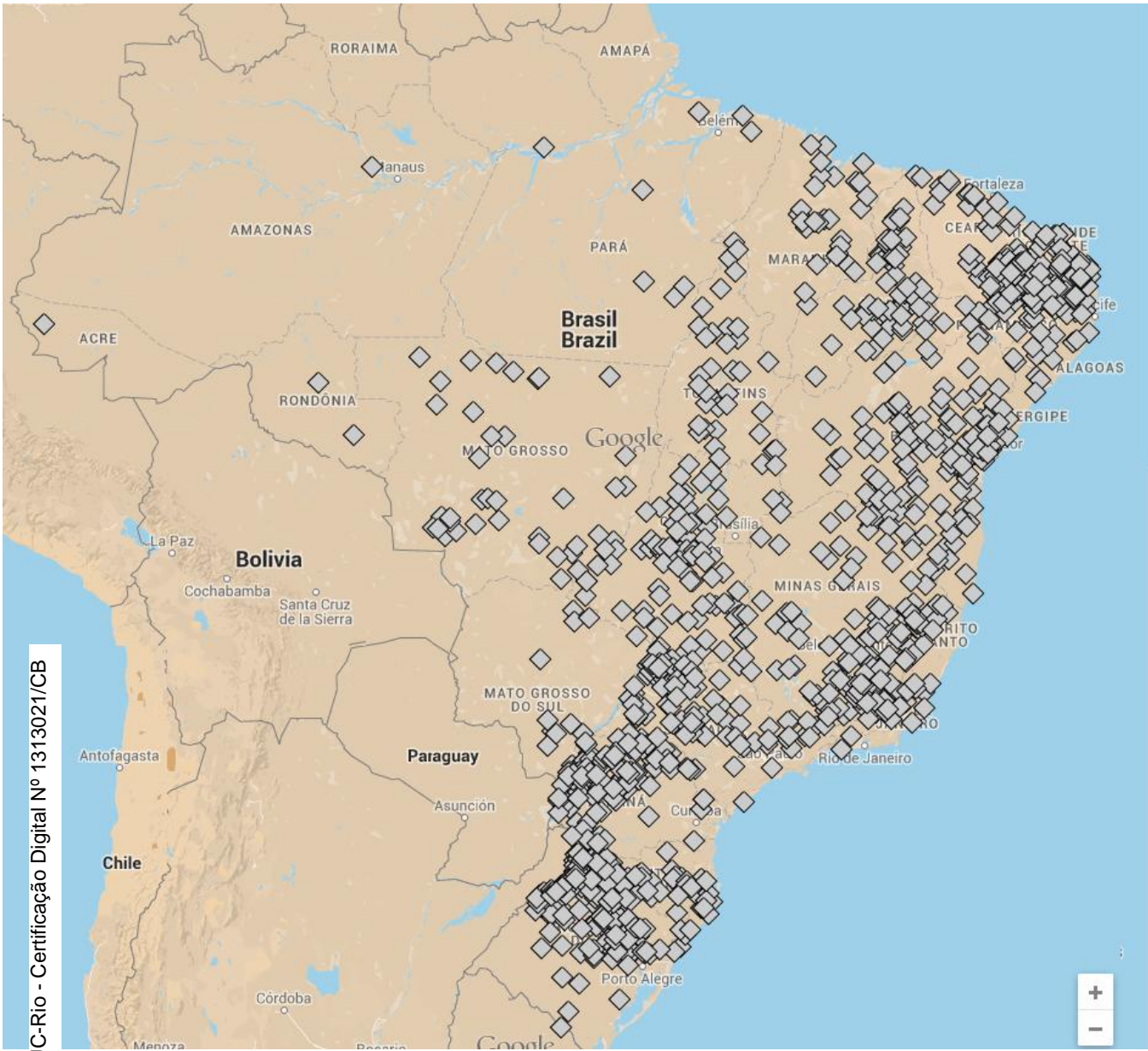
Figure 1: **Municipalities that went through the Electoral Revision**
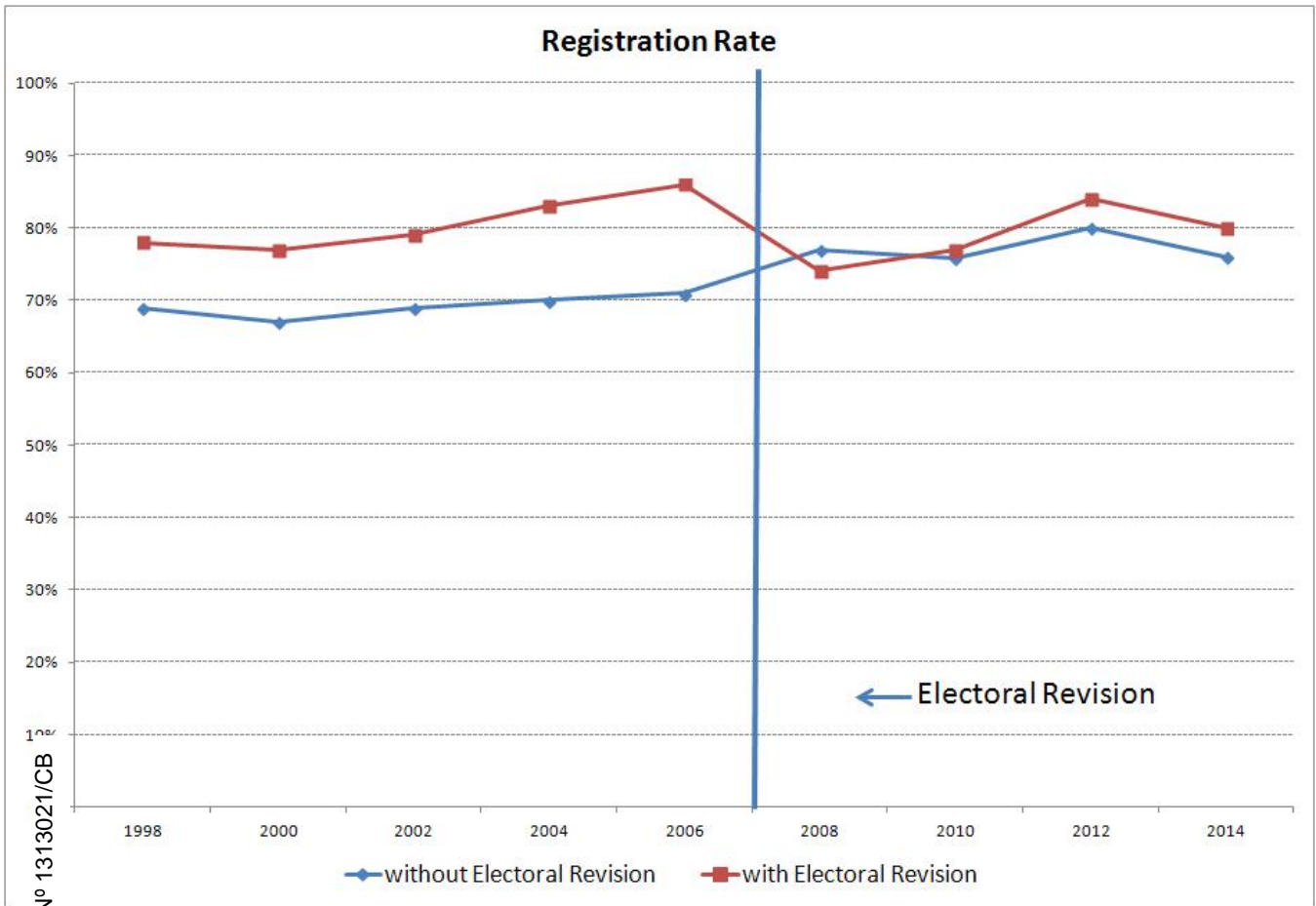
Figure 2: **Registration Rate over time for municipalities with and without Electoral Revision**
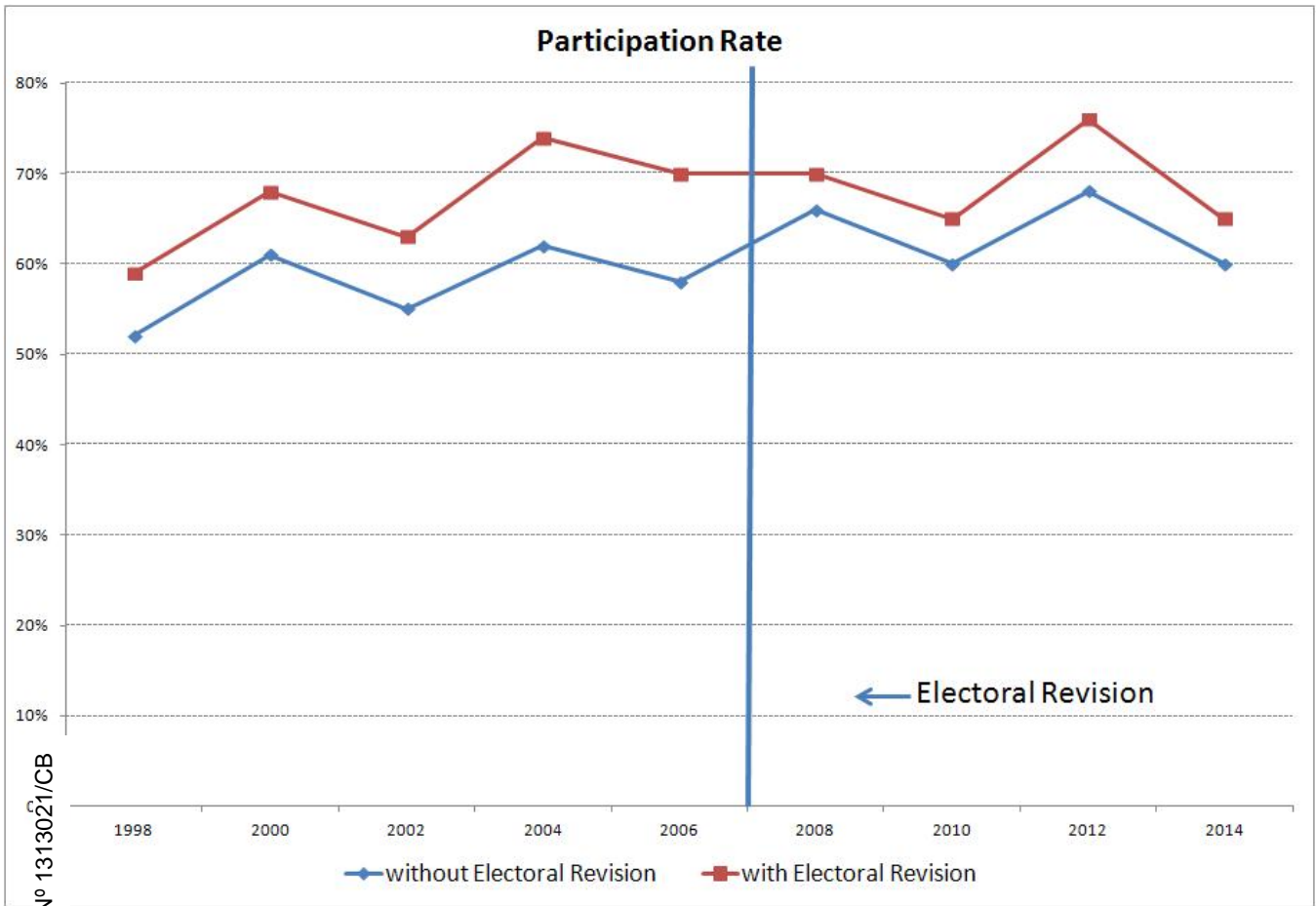
Figure 3: **Participation Rate over time for municipalities with and without Electoral Revision.**
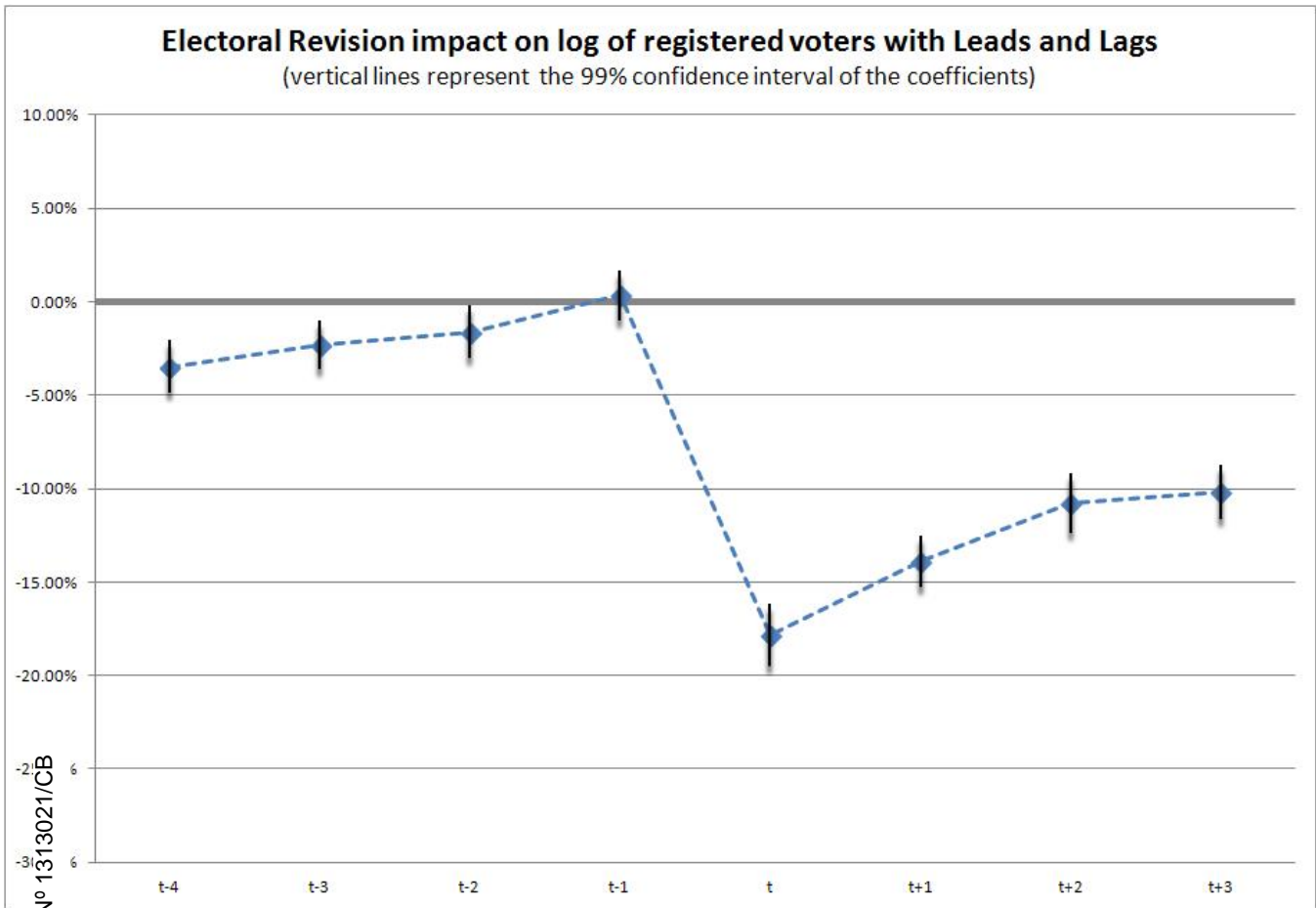
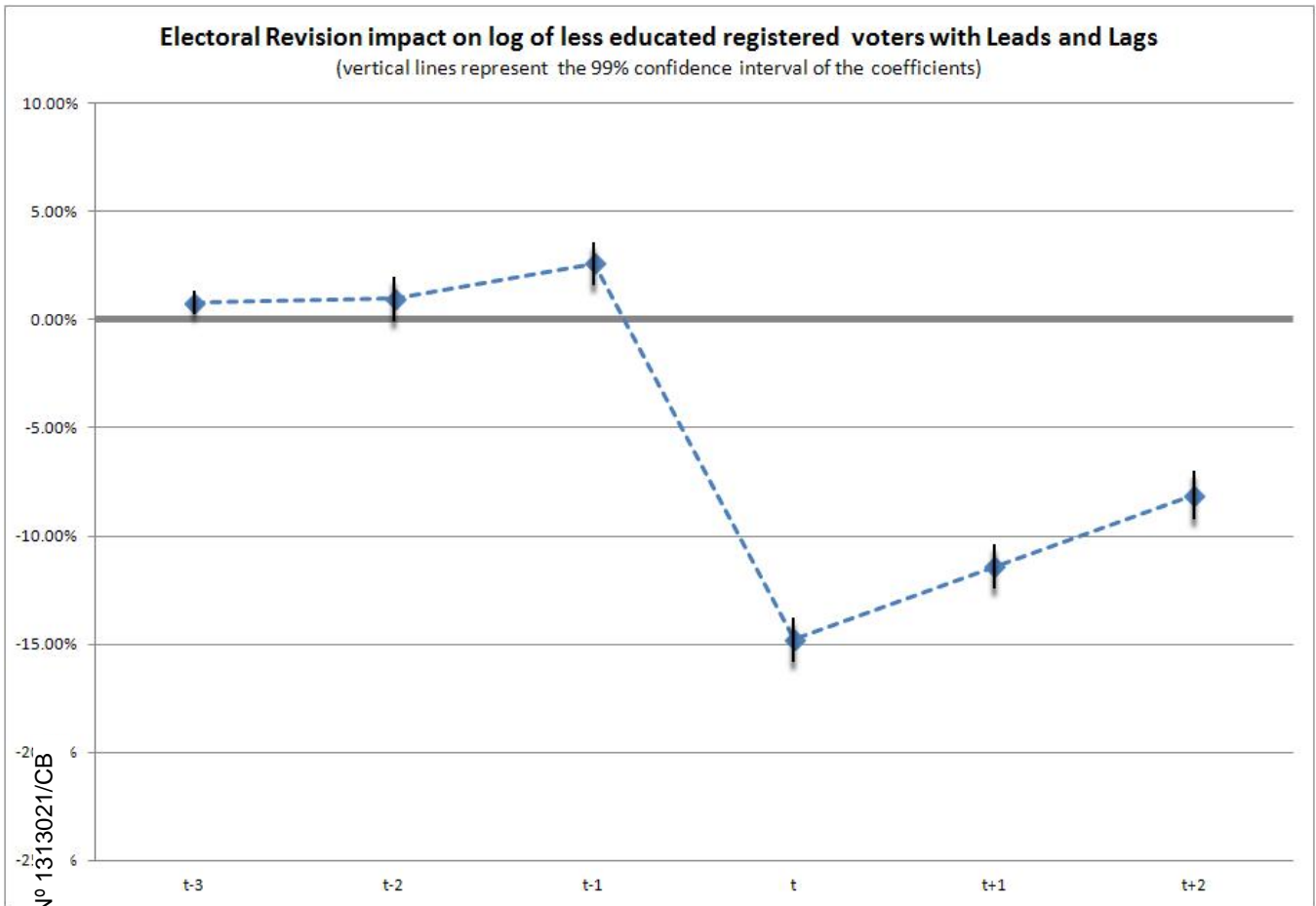Figure 4: **Parallel Trend Assumption Test: Log of Registered Voters**

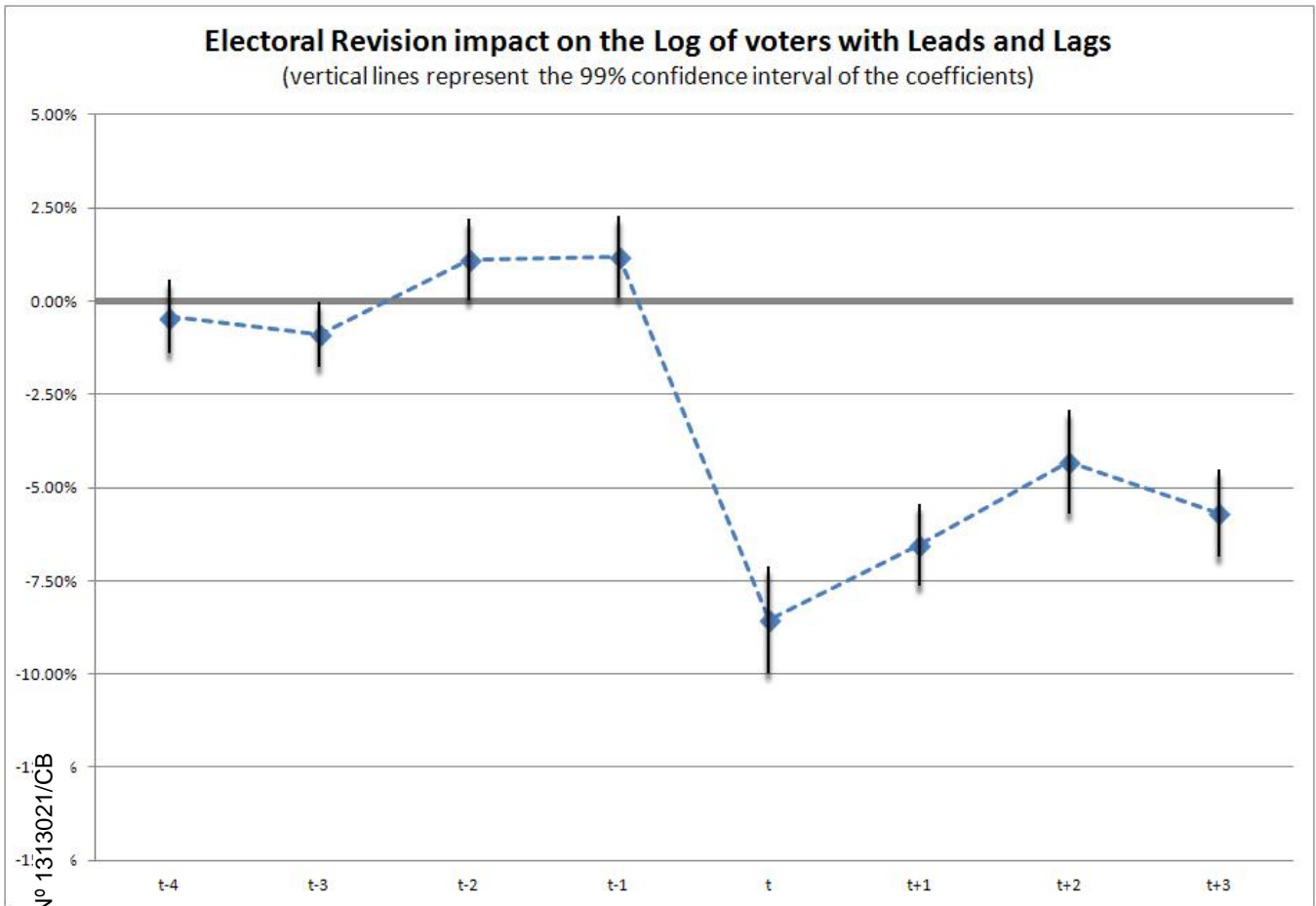Figure 5: **Parallel Trend Assumption Test: Log of Less Educated Registered Voters**
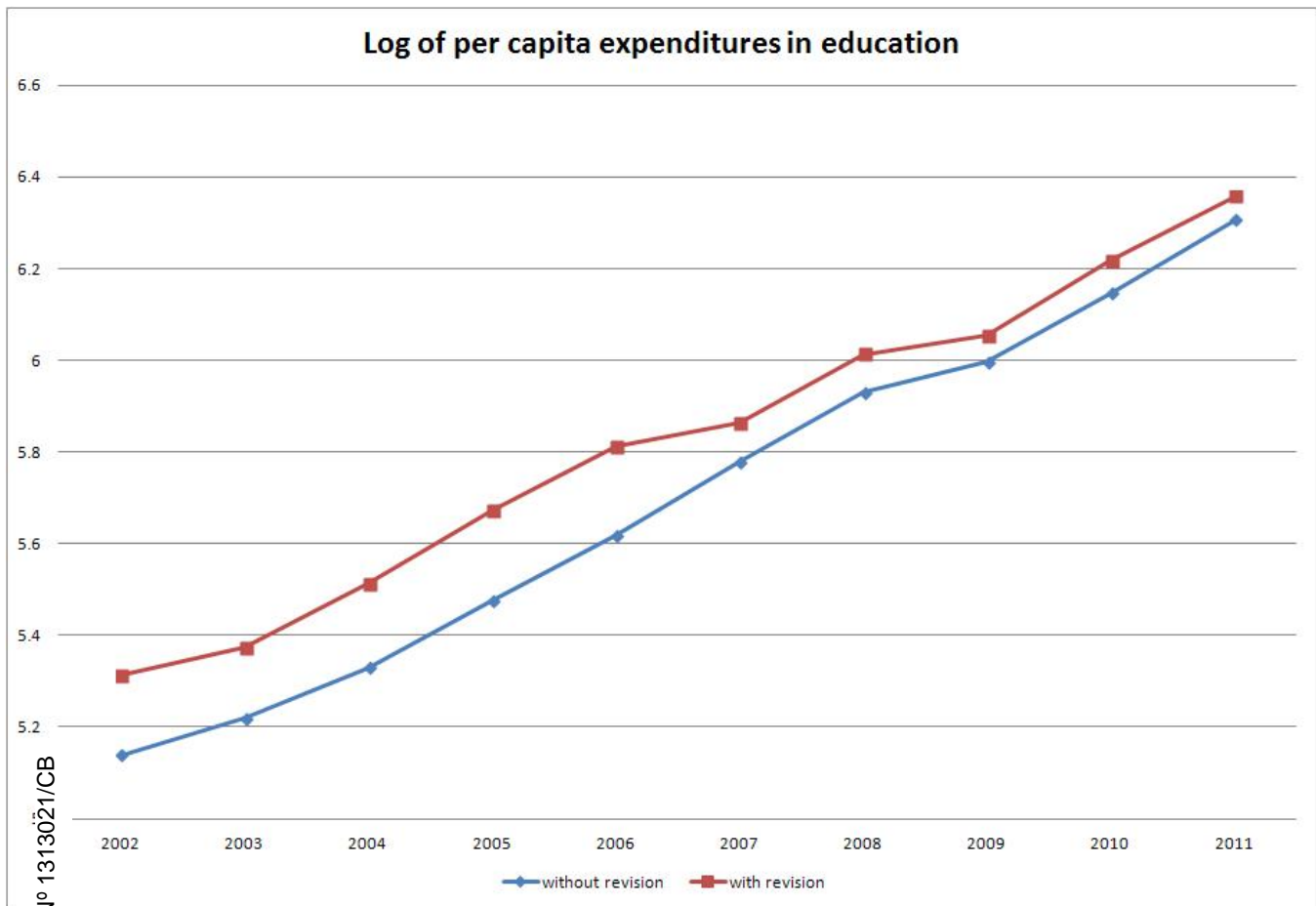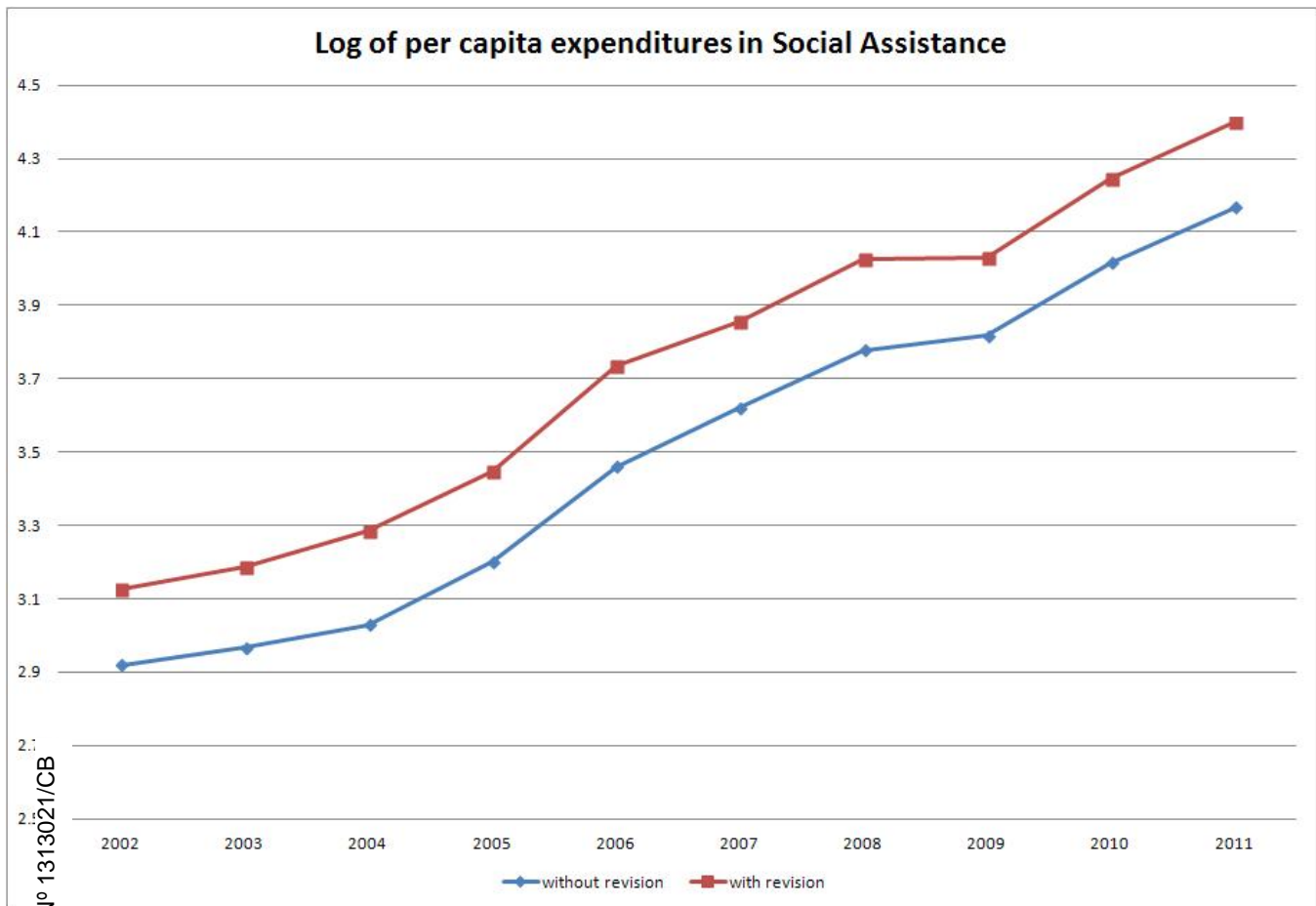
Figure 6: **Parallel Trend Assumption Test: Log of voters**

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Figure 7: **Log of municipal expenditures in education over time for municipalities with and without Electoral Revision**
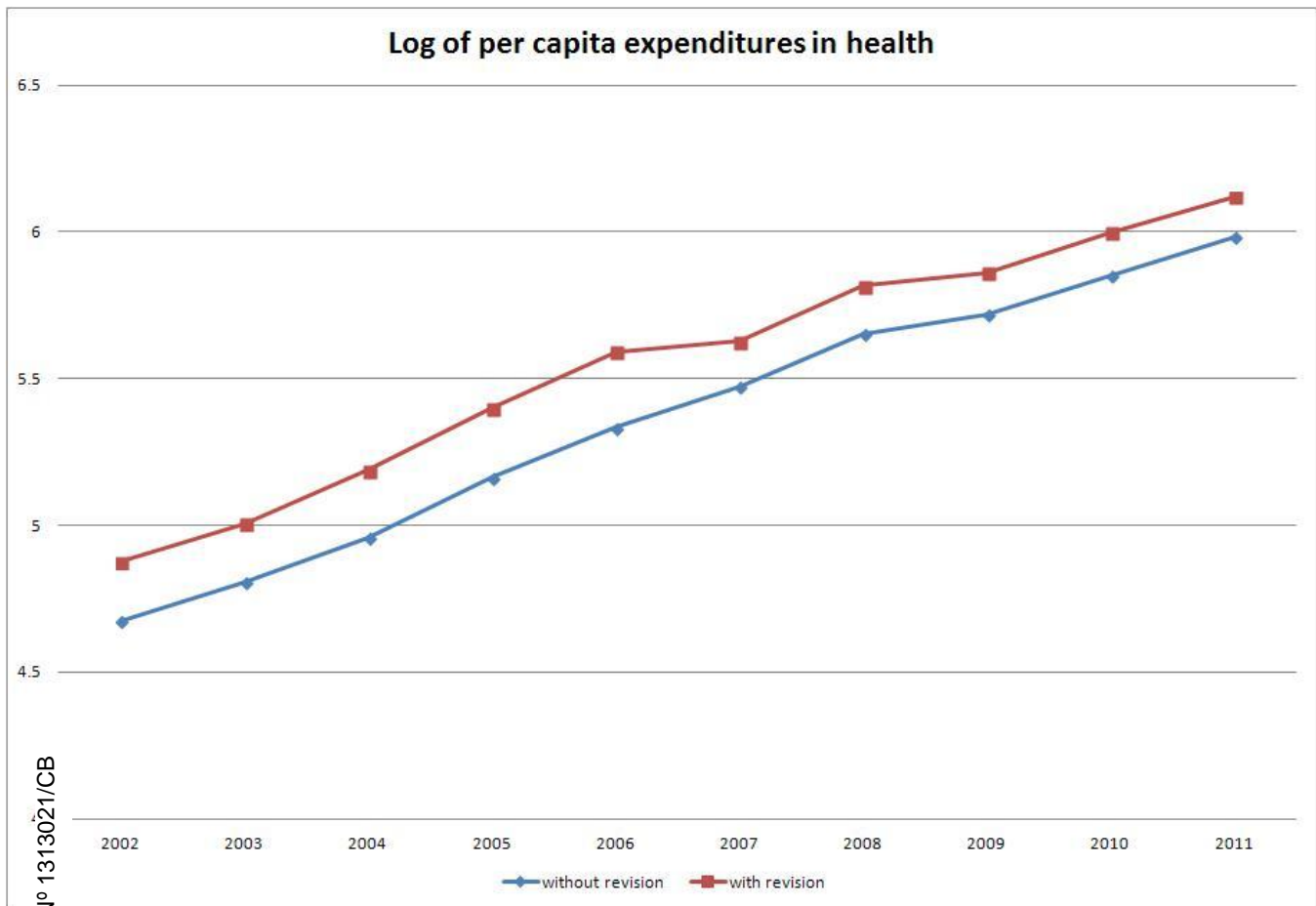
Figure 8: **Log of municipal expenditures in health over time for municipalities with and without Electoral Revision**

Figure 9: **Log of municipal expenditures in social assistance over time for municipalities with and without Electoral Revision**
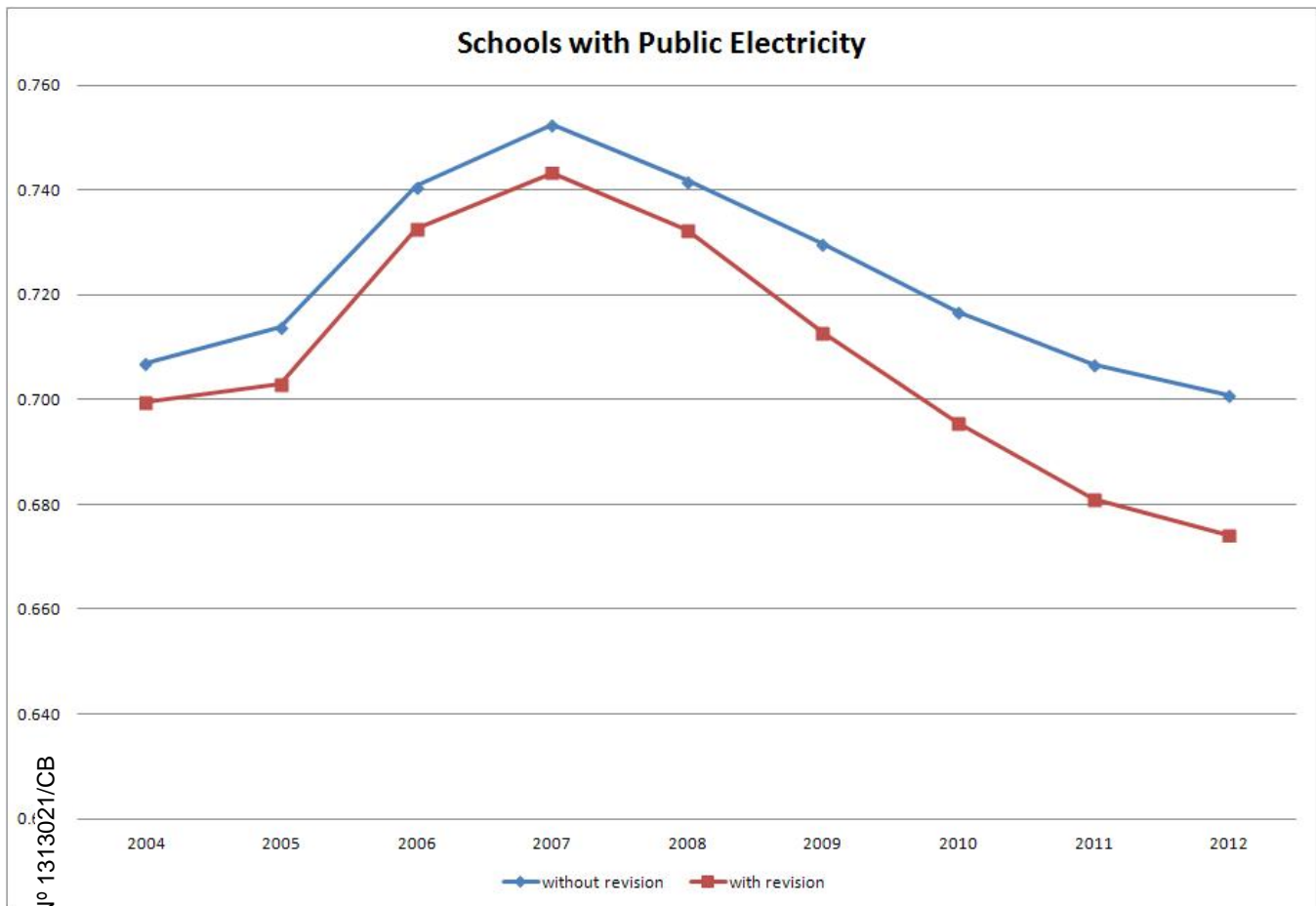
Figure 10: **Proportion of municipal public schools with access to public electricity over time for municipalities with and without Electoral Revision**
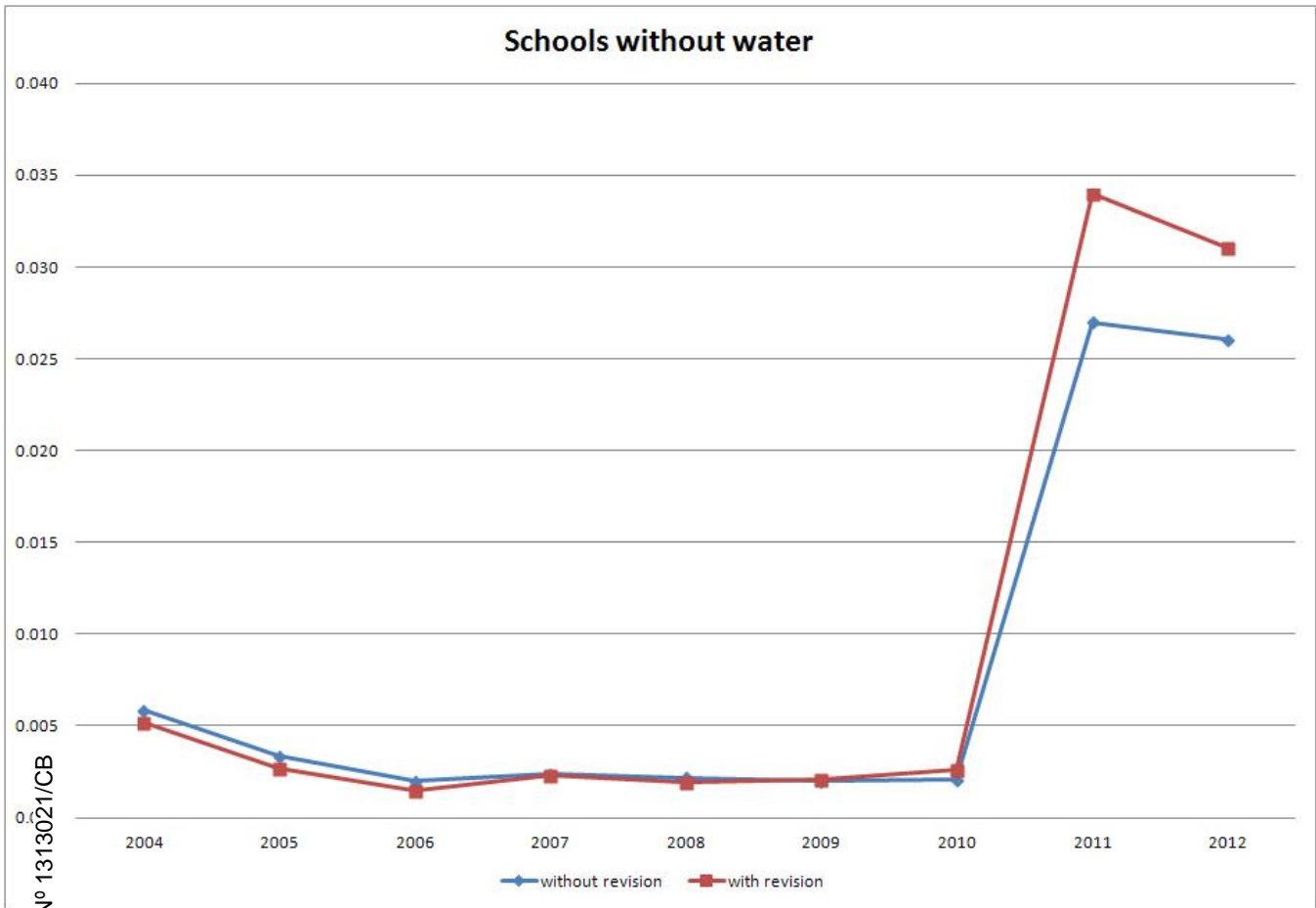
Figure 11: **Proportion of municipal public schools without water over time for municipalities with and without Electoral Revision**
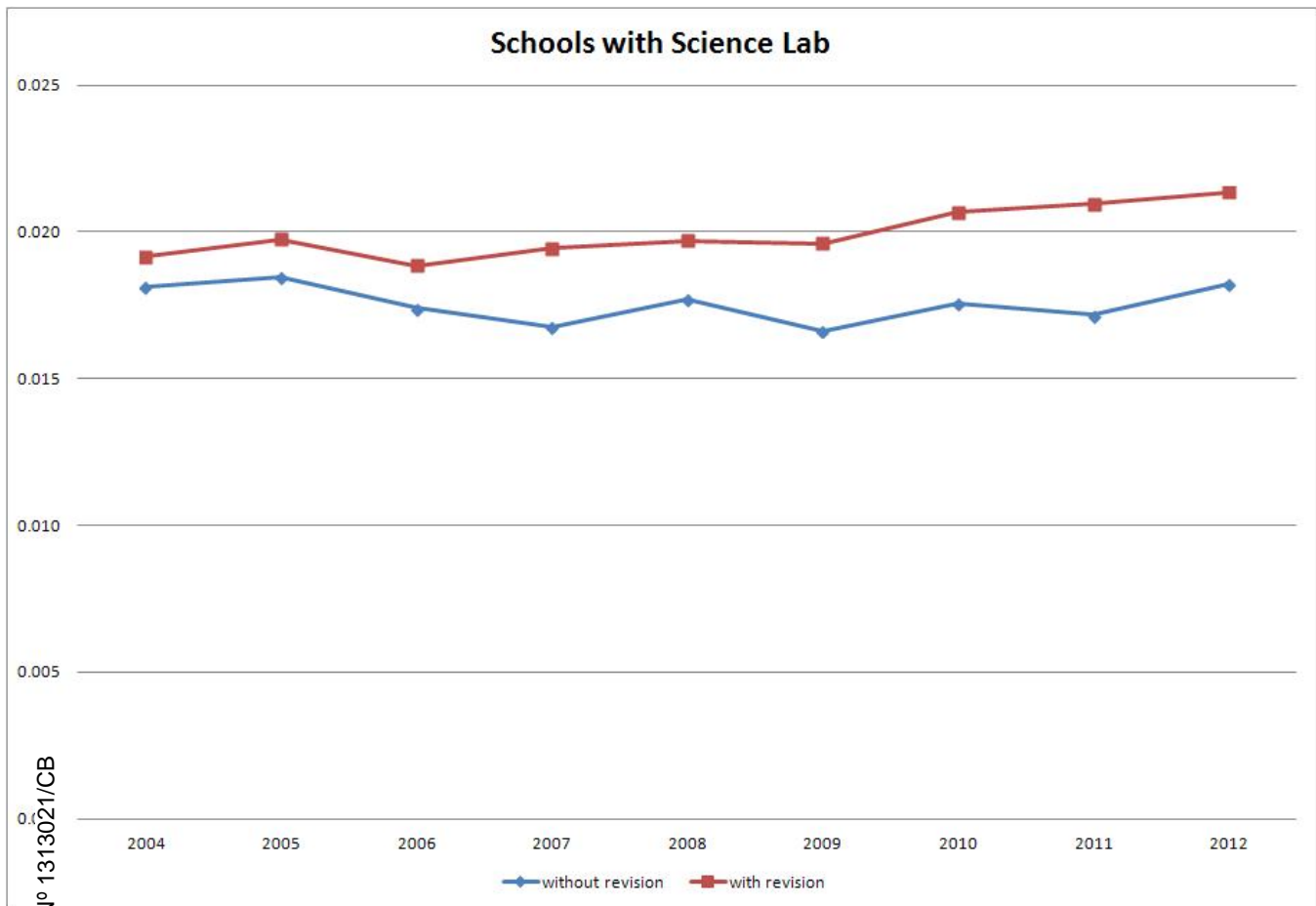
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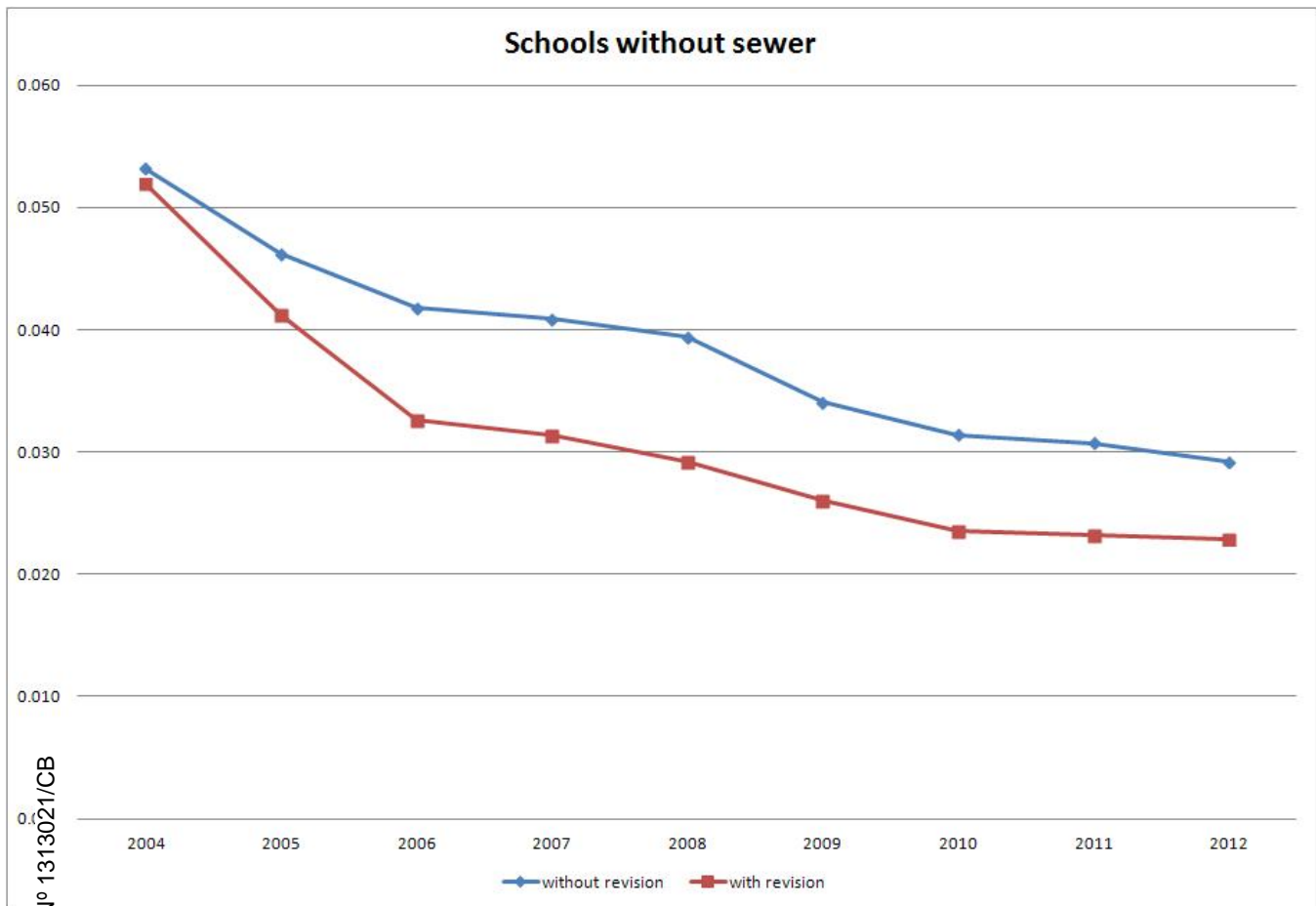
Figure 12: **Proportion of municipal public schools without sewer over time for municipalities with and without Electoral Revision**
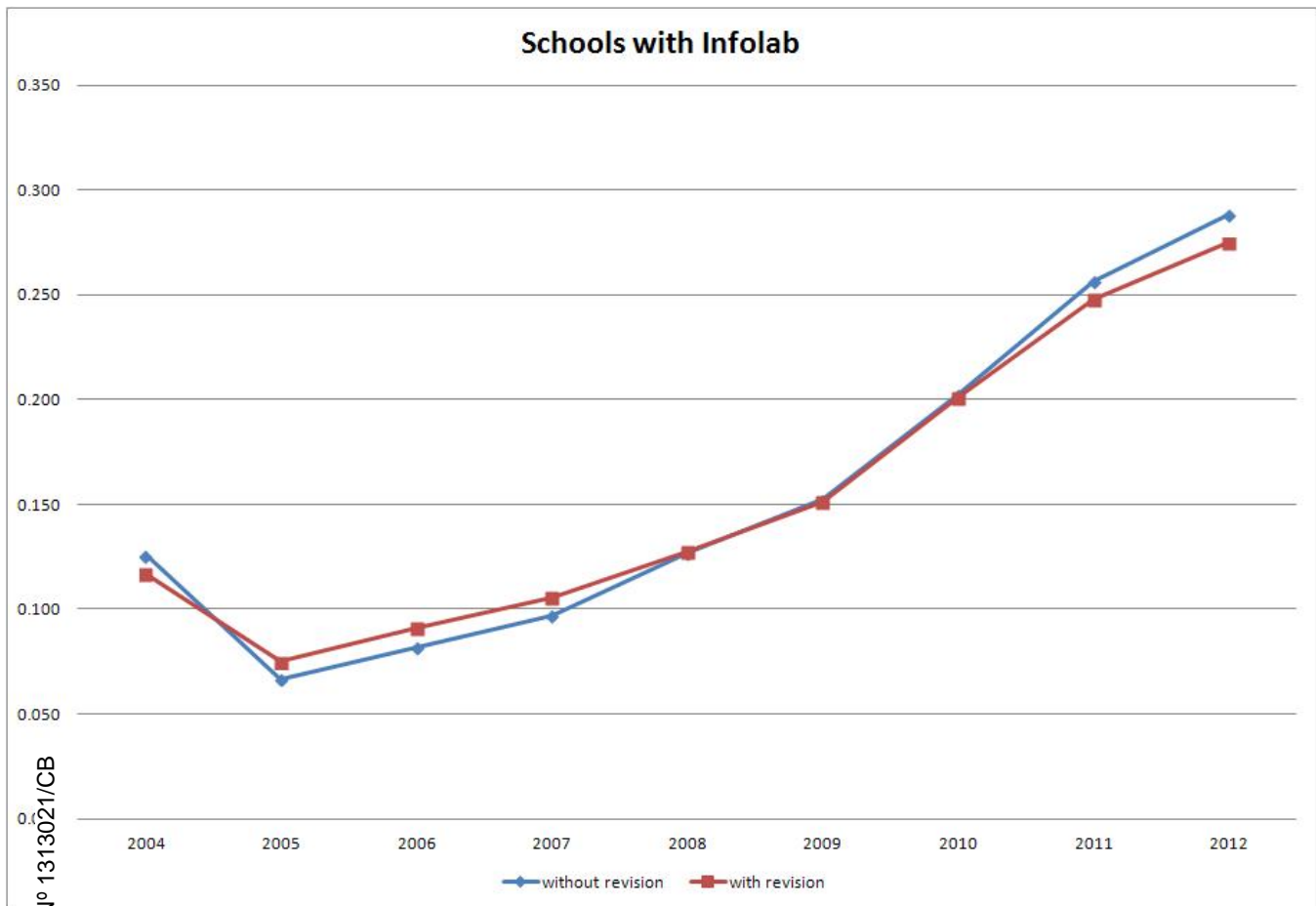
Figure 13: **Proportion of municipal public schools with an Info Lab over time for municipalities with and without Electoral Revision**
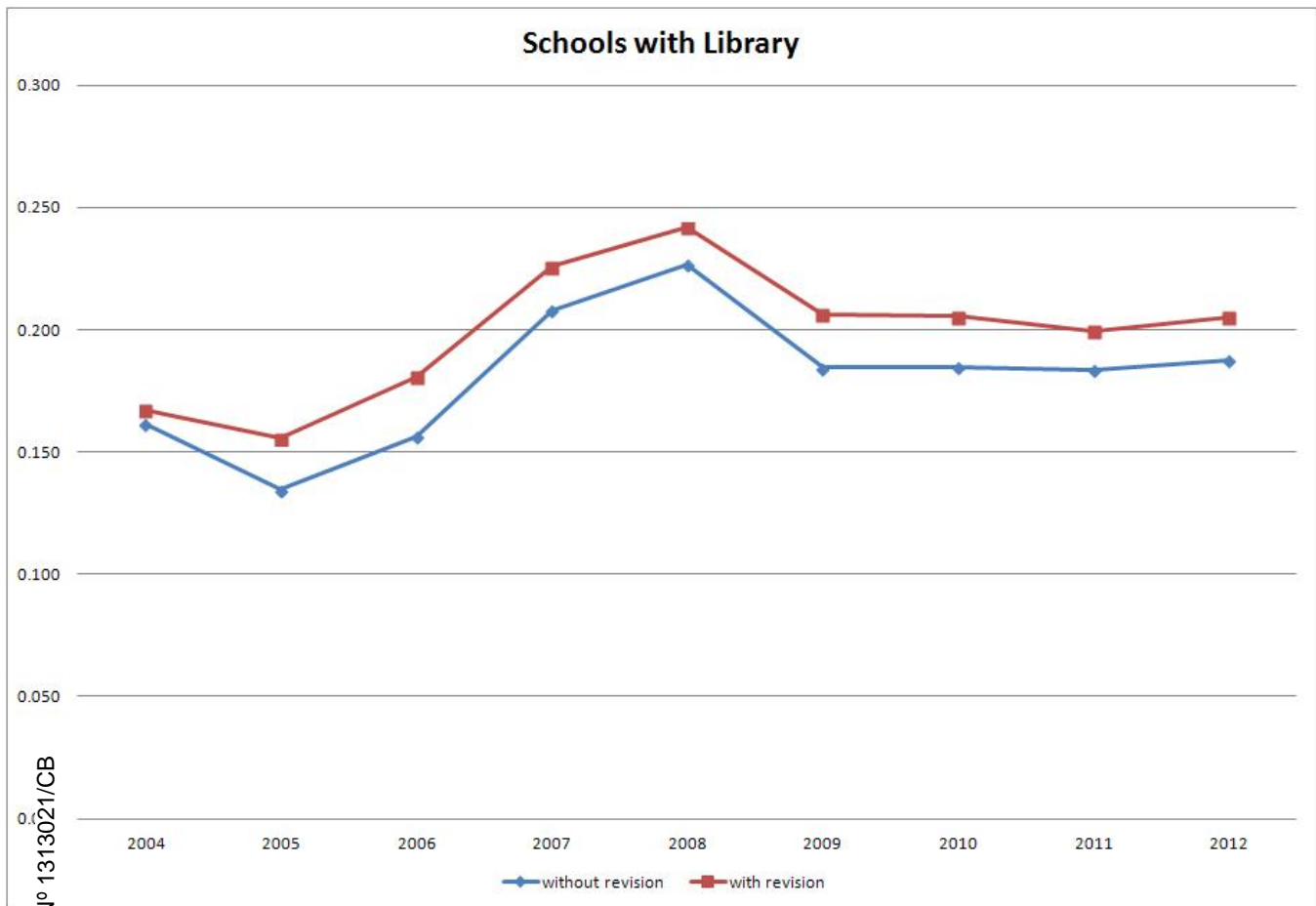
Figure 14: **Proportion of municipal public schools with a library over time for municipalities with and without Electoral Revision**
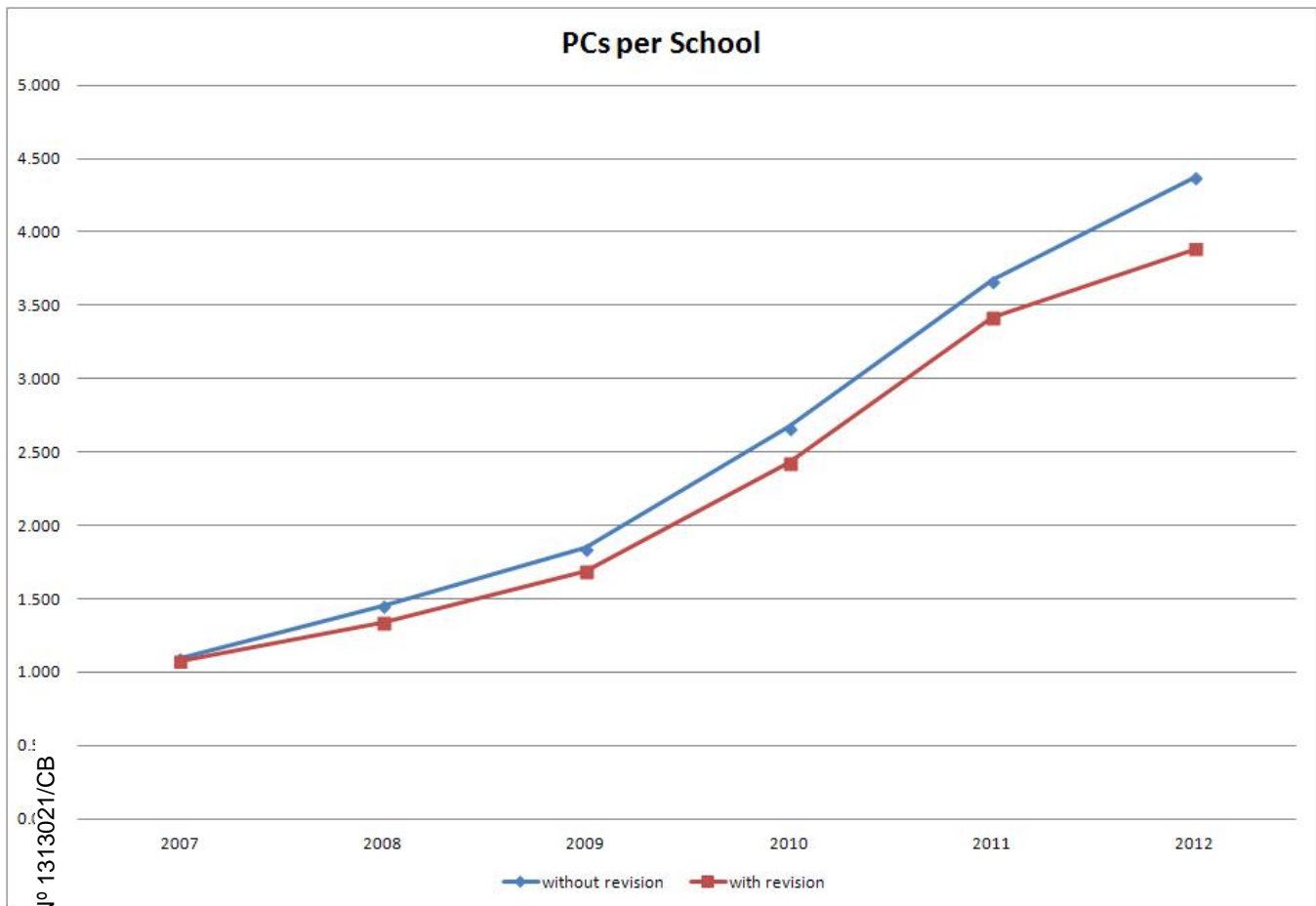
Figure 15: **Average number of PCs for students to use over time for municipalities with and without Electoral Revision**
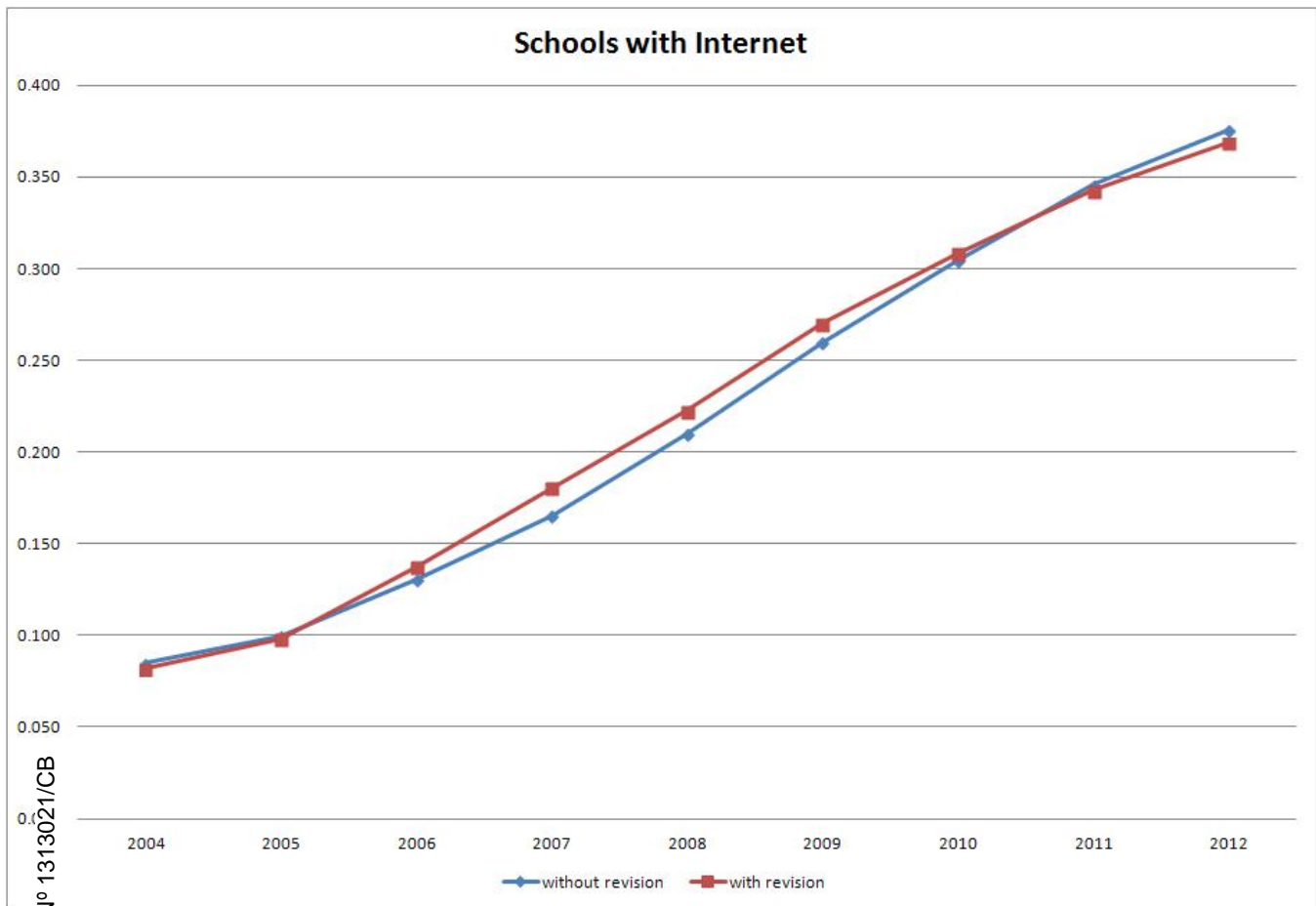
Figure 16: **Proportion of municipal public schools with internet over time for municipalities with and without Electoral Revision**

Figure 17: **Proportion of municipal public schools with a science lab over time for municipalities with and without Electoral Revision**
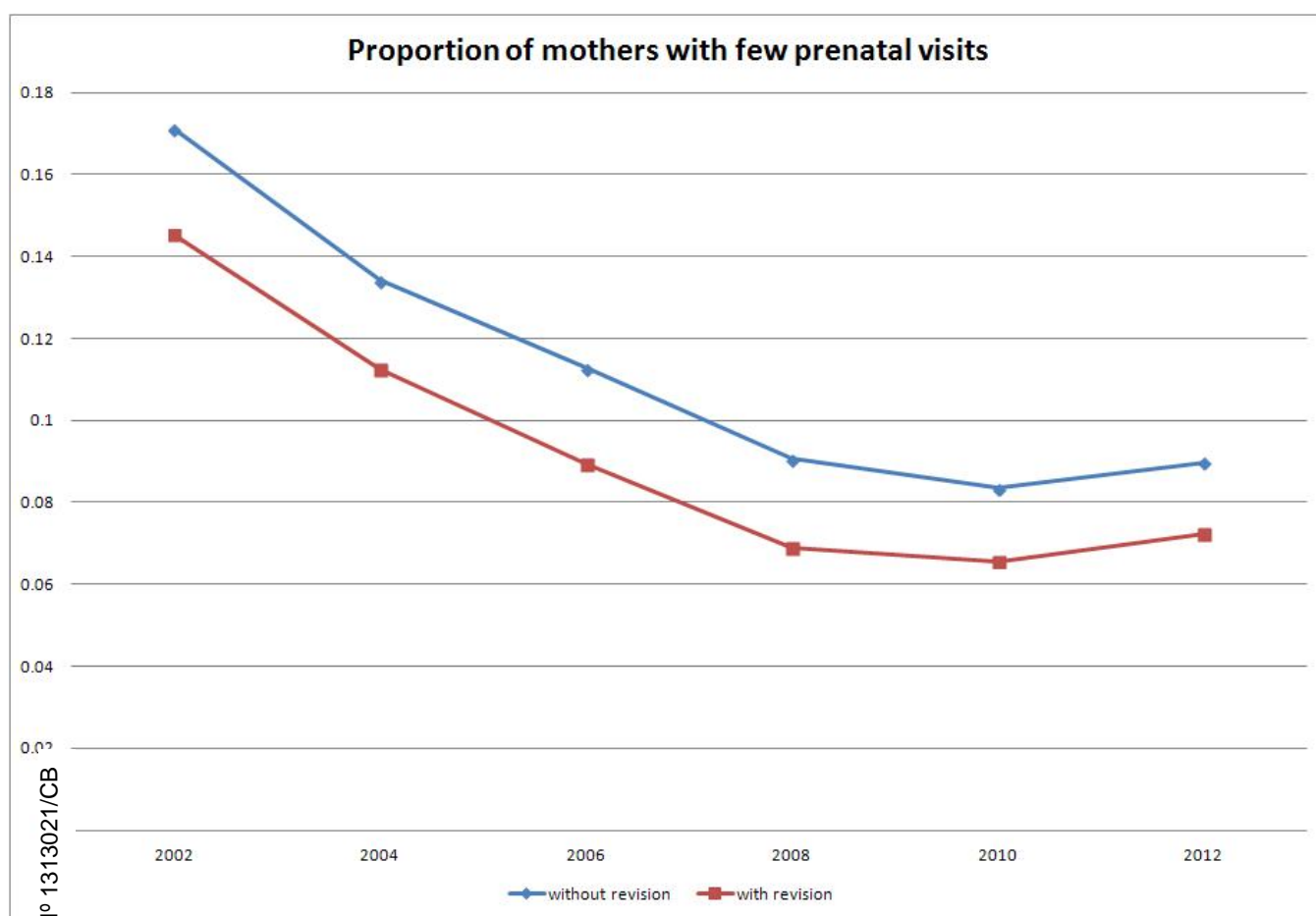
Figure 18: **Proportion of mothers with less than 4 prenatal visits over time for municipalities with and without Electoral Revision**
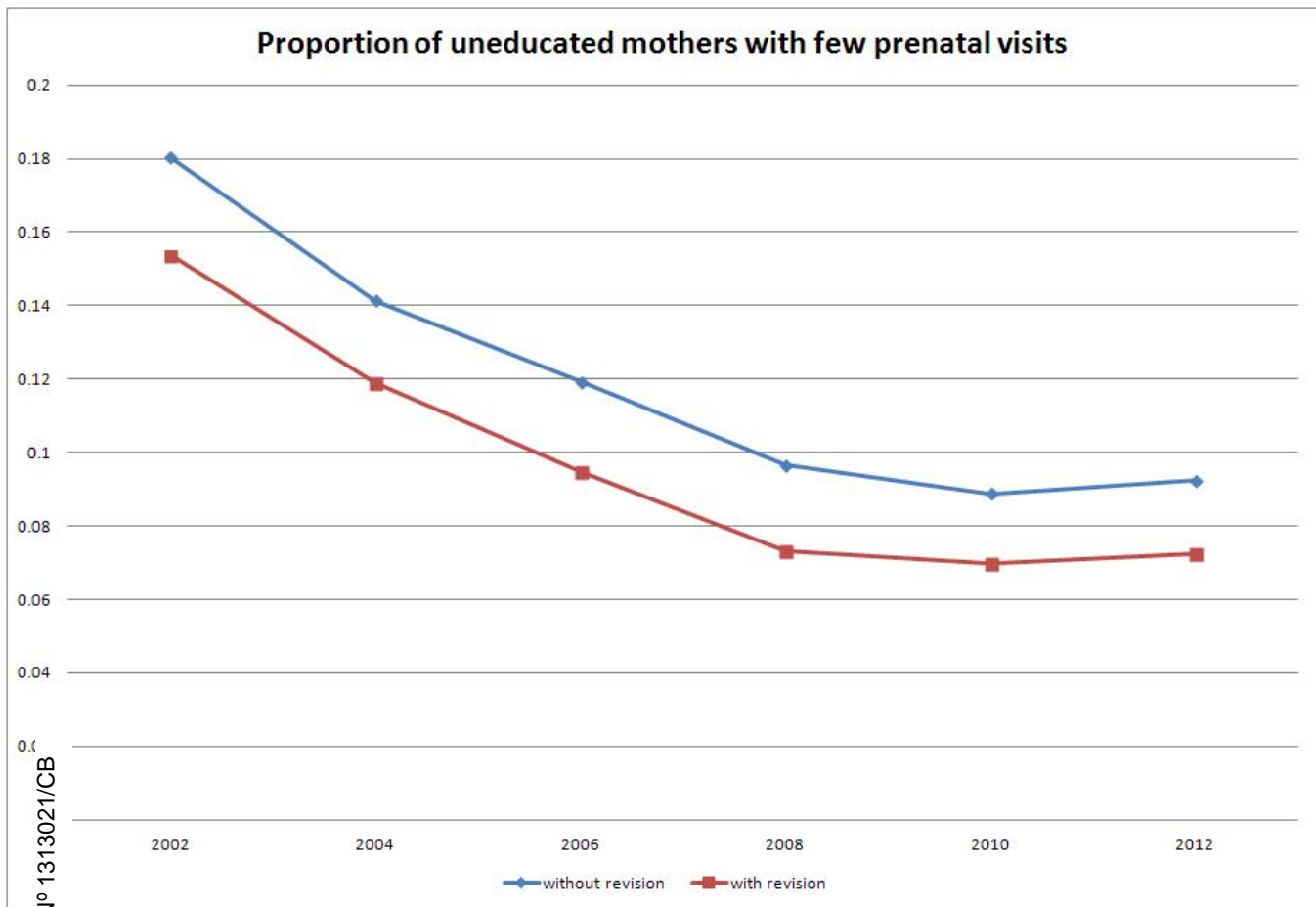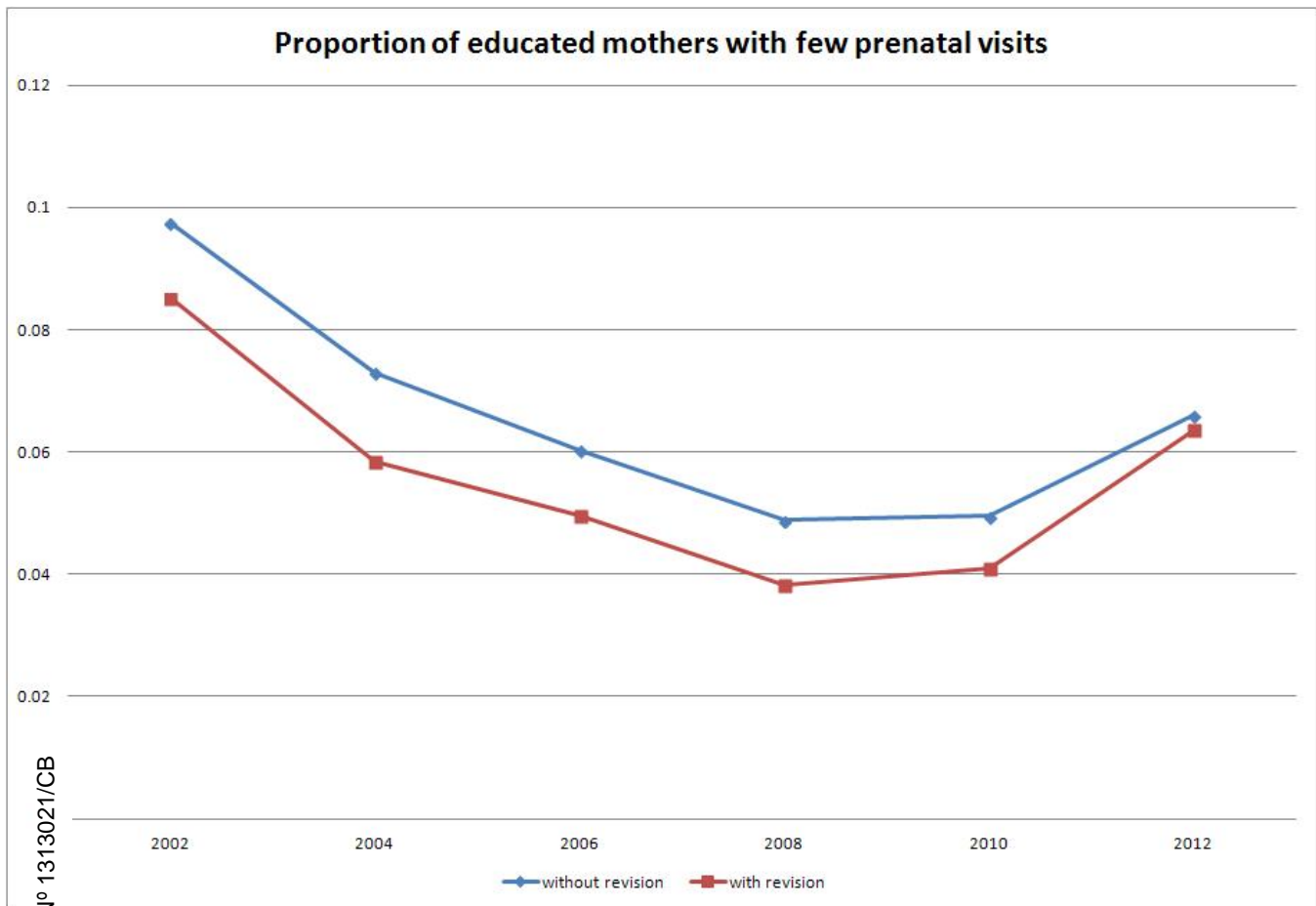
Figure 19: **Proportion of uneducated mothers with less than 4 prenatal visits over time for municipalities with and without Electoral Revision**

Figure 20: **Proportion of educated mothers with less than 4 prenatal visits over time for municipalities with and without Electoral Revision**
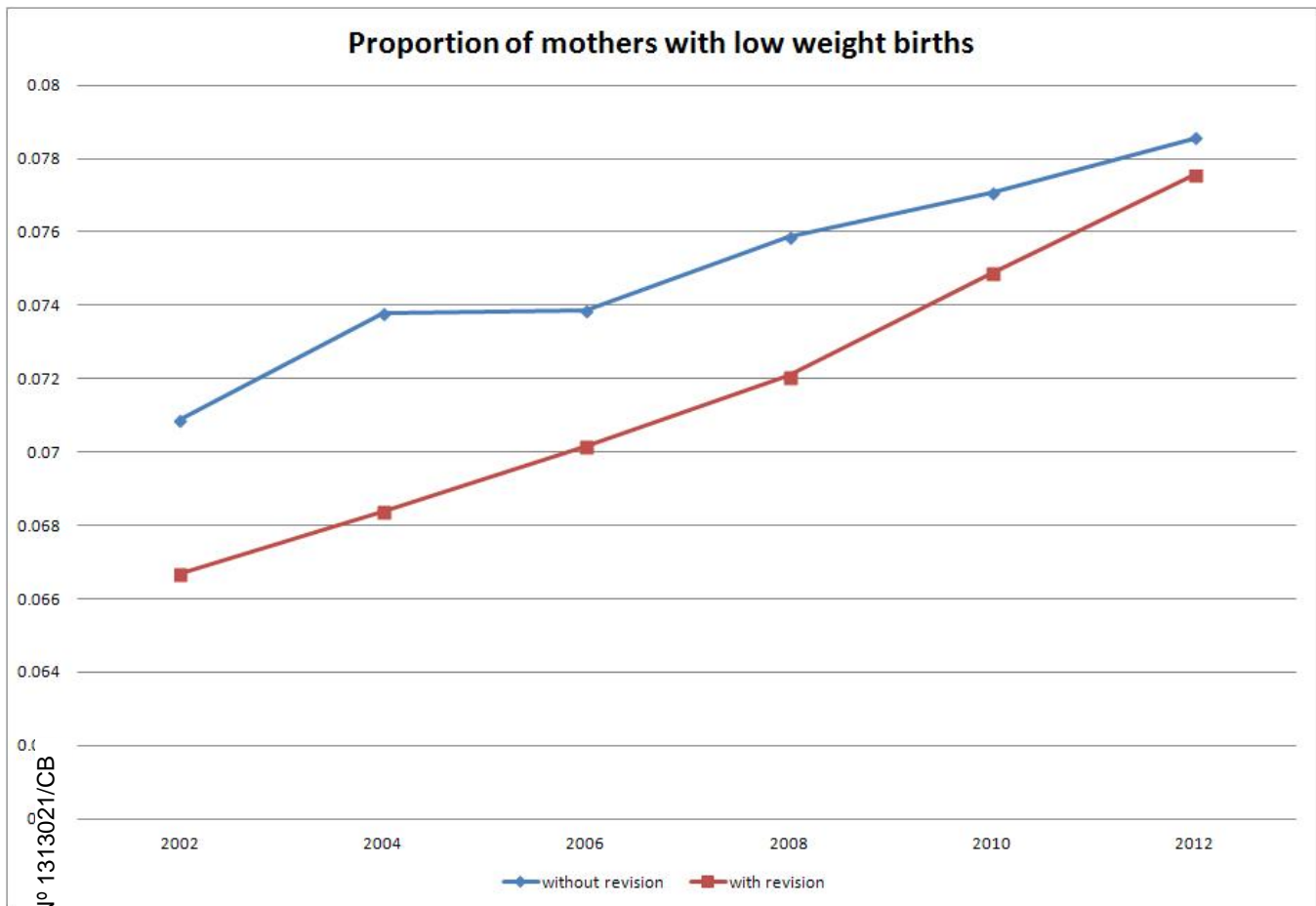
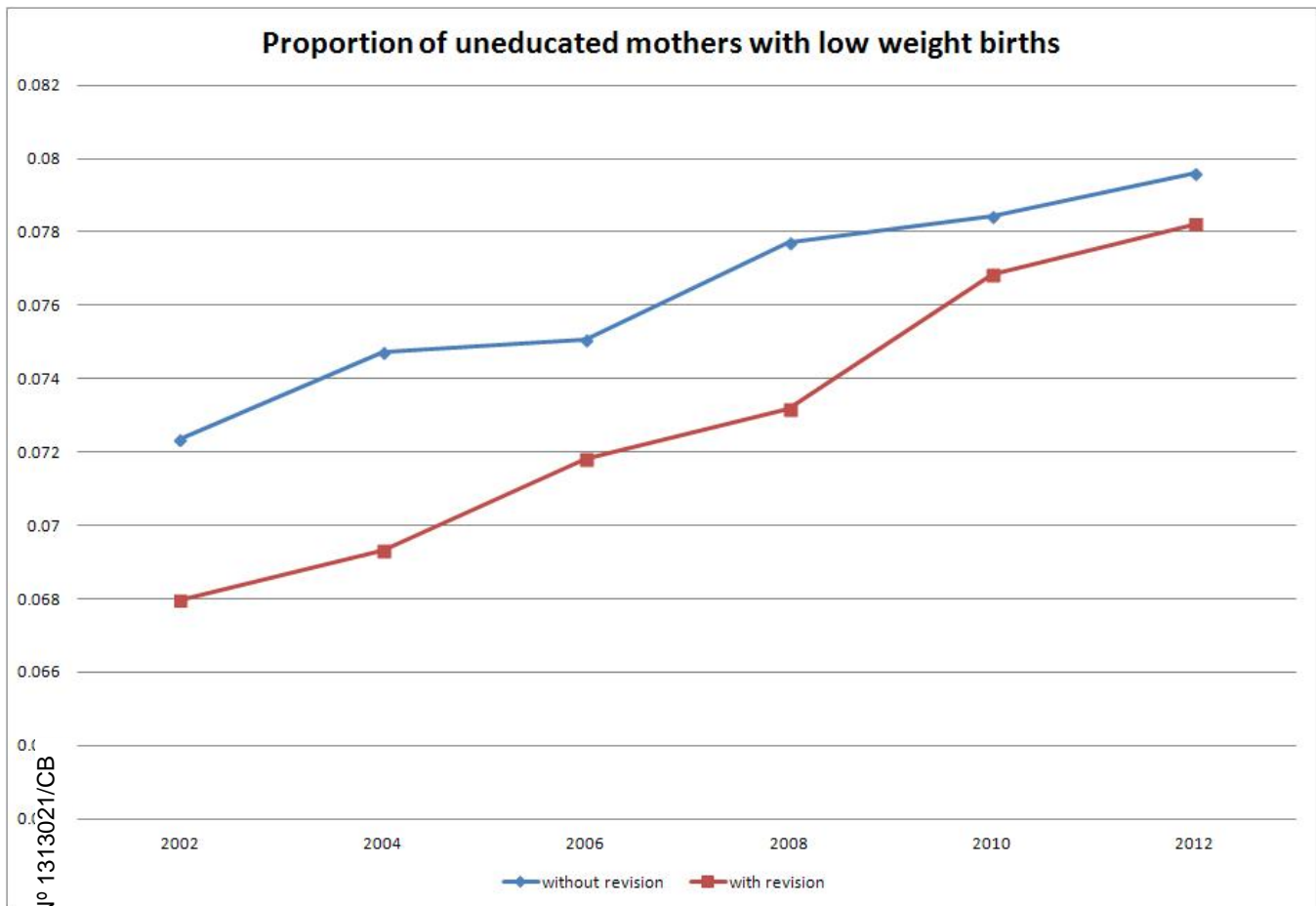Figure 21: **Proportion of mothers with low weight births over time for municipalities with and without Electoral Revision**

Figure 22: **Proportion of uneducated mothers with low weight births over time for municipalities with and without Electoral Revision**
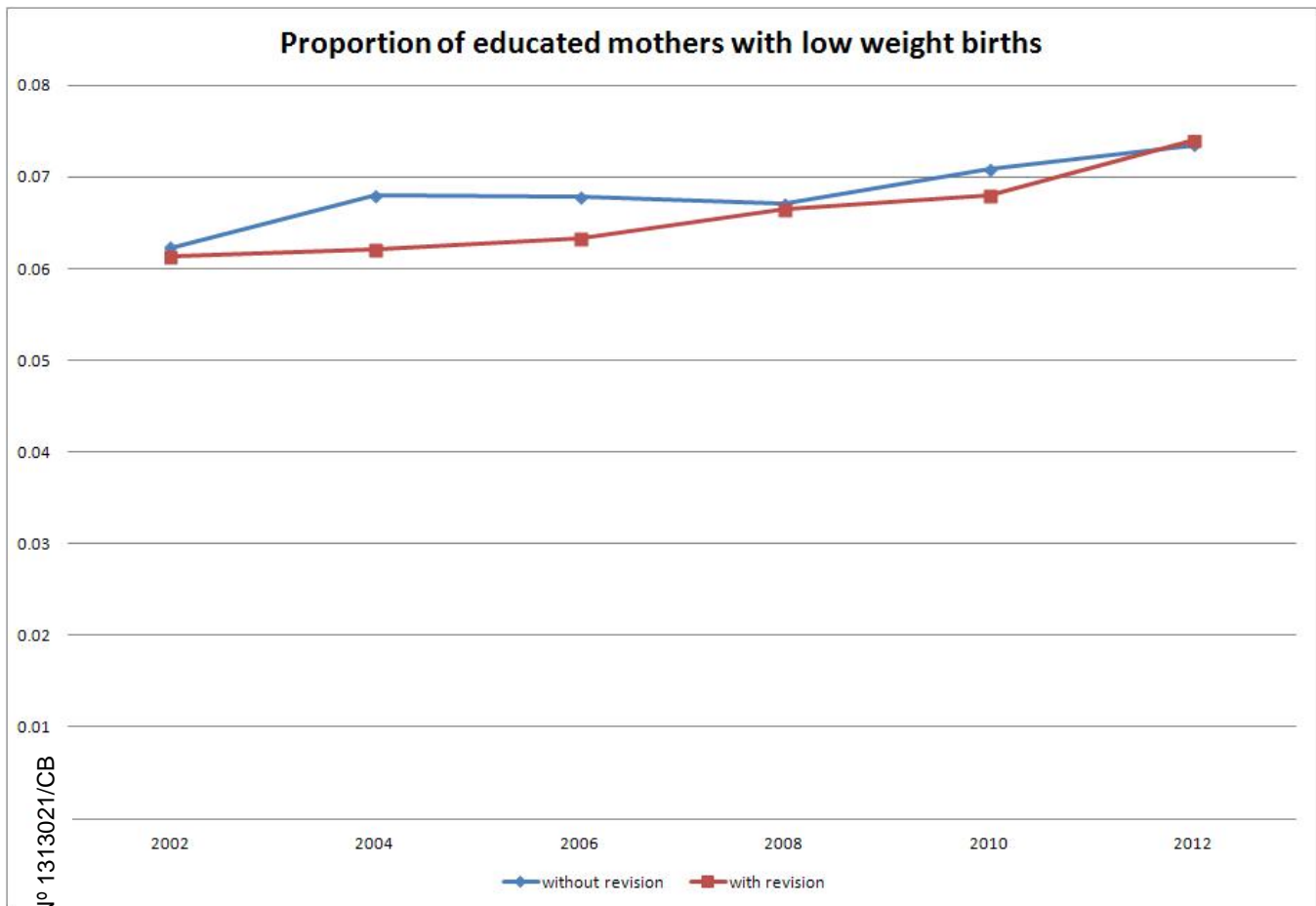
Figure 23: **Proportion of educated mothers with low weight births over time for municipalities with and without Electoral Revision**