Inclusion and Public Policy: Evidence from Sweden's Introduction of Noncitizen Suffrage*

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Abstract

The largest disenfranchised group in modern democracies is international migrants who lack citizenship of their country of residence. Despite that noncitizen suffrage has been introduced in some countries, and has been the subject of vigorous public debate in many others, there have been no systematic attempts to investigate its policy consequences. Drawing on standard models of political competition, I argue that there will be a selection bias inherent in estimating the impact of noncitizen suffrage on public policy, and analyze data that are uniquely suitable to deal with this methodological problem, namely data on exogenous changes in the composition of the electorates of Swedish municipalities generated by the introduction of noncitizen suffrage. According to the results, the effect of enfranchising noncitizens on public policy was large, causing spending on education and social and family services to increase substantially in municipalities where noncitizens made up a non-negligible share of the electorate.

Keywords: citizenship, voting rights, public spending, selection bias

Universal suffrage is a cornerstone of modern democracy. Yet, democratic states typically deny a large number of adults residing within their boundaries the right to vote. In most cases, the largest disenfranchised group is international migrants and refugees who lack citizenship of their country of residence. As globalization proceeds, moreover, the size of this group will continue to grow. At the same time, a number of countries are bucking this trend. While New Zealand is the only established democracy that allows noncitizens to vote in national elections, the right to vote in local elections has, in some countries, been extended, either by central or by sub-national governments (Earnest 2008). In several other countries, the fact that a large and growing share of the population has no say in the political decisions that affect them the most, has sparked a vigorous debate about noncitizen suffrage (Bauer 2007). Yet, we do not know what would be the policy consequences of the inclusion of this previously excluded group.

Democratic theorists have long argued that public policy will not reflect the interests of those who are excluded from the political process (see, e.g., Dahl 1971, Walzer 1983). It is therefore not surprising that many have suggested that the extension of voting rights to noncitizens would have potentially important consequences for public policy, and that it would improve the relative situation of this group (see, e.g., Layton-Henry 1991, Brubaker 1998, Hayduk 2006). However, history is replete with examples of resistance to franchise extensions grounded in fear of the policy consequences of undertaking such reforms, and which was only overcome when and where this fear had been assuaged (see, e.g., Keyssar 2000, Boix 2003). Indeed, the rise and fall of noncitizen voting in the United States during 'the long nineteenth century' partly conforms to this narrative (Hayduk 2006). Writing about more recent struggles over noncitizen suffrage in Western Europe, Dancygier (2010, 277) observes that "members of the nonimmigrant electorate are suspicious of incorporating a new set of voters who will make claims that impinge on their welfare". In general, then, such examples warn us that the impact on public policy of noncitizen voting rights, *where such rights have actually been granted*, may be small.

Drawing on standard models of political competition I delineate the prerequisites for the existence of an enfranchisement effect and explain why there will be a selection bias inherent in estimating it. Specifically, I argue that the franchise is more likely to be extended in settings where the policy priorities of the majority population differ the least from those of noncitizens, and where the latter group make up a politically insignificant bloc of voters. Therefore, there will be negative self-selection bias in estimating the policy consequences of noncitizen suffrage, in that the latter will only be observed where the policy consequences of such reform are small. More generally, attempts to estimate the enfranchisement effect will be subject to what can be termed 'the political economy threat to internal validity' (Meyer 1995), which comes about because political decisions about whether or not to implement a particular reform are determined by expectations about the reform's impact on future policy outcomes. Most importantly, however, I suggest a novel way out of this conundrum. In particular, franchise extensions that are imposed on sub-national units by the central government ameliorate the methodological problems that spring from self-selection.

To estimate the enfranchisement effect empirically, I turn to credibly exogenous changes in the composition of the electorates of Swedish municipalities generated by one of the earliest and most inclusive examples of noncitizen suffrage. The reform was passed by the national parliament in 1975, and granted foreign citizens with three or more years of permanent residence the right to vote in elections to all of Sweden's municipal assemblies. The first time noncitizens had the opportunity to exercise their new-won right was in the 1976 municipal elections. Due to the uneven dispersion of noncitizens across Sweden, some municipalities were hardly affected at all, whereas in others noncitizens made up as much as 13 % of the 1976 electorate. The sudden change in municipal electorates that was generated by the reform enables me to estimate the impact of the enfranchisement of noncitizens on public policy.

Studying the Voting Rights Reform of 1975 is not only attractive because it presents an opportunity to deal effectively with the problem of self-selection in the study of the

policy consequences of noncitizen suffrage. From a methodological point of view, the greater homogeneity of sub-national units promises a solution to the problem of isolating the impact of franchise extensions on public public policy. By studying variations in local, rather than national, government policies a large number of country-specific influences and developments are automatically held constant (Besley and Case 2003).

To measure changes in policy, I focus on municipal public spending in two policy areas over which municipalities exercised considerable control: education and social and family services. Municipalities were responsible for both primary and secondary education, as well as adult education oriented towards the formation of labor market skills. They also provided a number of social and family services, such as pre-school and after-school services and child and adolescent care. At the time of the reform, municipal educational and social services were considered to be crucial in equalizing the life chances of noncitizens and the majority population. The main reason for this was that noncitizens made up a disproportionate share of the demographic groups—families with children and young adults—that were most directly affected by public policy in these areas.

My results suggest that noncitizen suffrage had important consequences for local spending on education services as well as on social and family services. Consistently with an account that emphasizes the demographic characteristics of the noncitizen population, I also find that the impact of the reform on education services was larger where the percentage of school-aged noncitizens was high, and that the impact on spending on social and family services was increasing in the percentage of noncitizens below school age. The analysis then goes on to show that these empirical results are highly robust. I use instrumental variables and other approaches to show that my results do not appear to be driven by noncitizens' own locational choices. I also perform falsification tests showing that investments in a public good that is likely to have been perceived as equally important by noncitizens and the majority population were unaffected by the reform. Then, I go on to show that my estimates are not unduly influenced by a few particular observations and that they are

robust to alternative specifications of the main independent variable, as well as the inclusion of a number of additional control variables.

The paper is divided into four parts. In the first, I put the Swedish reform in comparative light and discuss why it makes for an ideal case for studying the policy consequences of noncitizen suffrage. In the second, I describe the statistical model, the data and variable measurement. My empirical results are presented in the third. In the fourth, finally, I set out my conclusions.

Studying the Policy Consequences of Noncitizen Suffrage

What makes for an ideal environment in which to study the impact of noncitizen suffrage on public policy? In standard models of political competition, the distribution of policy preferences among voters is the main driver of public policy (Persson and Tabellini 2000, Ch. 3). According to this view, the preconditions for the existence of an effect on public policy of noncitizen suffrage are thus: (a) that the distribution of policy preferences among noncitizens is different from that found among citizens; and (b) that noncitizens make up a significant bloc among the voters. But the view that the distribution of preferences drive public policy also implies that individuals will have preferences about whom should be included among the voters (Acemoglu and Robinson 2006, Ch. 6). In general, we would expect citizens' opposition to the inclusion of noncitizens to be increasing in the extent to which the above preconditions hold. Because citizens typically make up a large majority of the population, their willingness to extend the franchise is likely to be of crucial importance. This highlights the negative selection bias inherent in estimating the impact of noncitizen suffrage on public policy.[1]

In this section, I will put the case of the Swedish Voting Rights Reform of 1975, which granted foreign citizens with a total of three years of residency or more the right to vote and to stand for office in elections to municipal assemblies, in comparative light. In particular,

I will show that it fulfilled the conditions for the existence of an enfranchisement effect while at the same time being unusually suitable for dealing with the threat of selection bias, because the franchise extension was uniformly imposed on all sub-national units by the central government independently of local conditions. The Swedish case thus provides an ideal environment in which to study of the policy consequences of noncitizen suffrage.

Preconditions for an Enfranchisement Effect

If one takes a global view, the average member of the noncitizen population typically differs from the average member of the majority population on a number of demographic and socio-economic indicators that are, in turn, known to be strongly related to political attitudes. In the United States, the average noncitizen is younger, poorer and more likely to work in a low-wage sector than the typical naturalized immigrant and native born (Kandel 2011). Data from the OECD indicate that similar patterns obtain in most European countries (OECD 2008). These findings are strongly suggestive of the notion that, in general, the distribution of policy preferences among noncitizens will differ from that found among the majority population. Direct evidence on the difference between the policy preferences of noncitizens, as well as immigrants in general, and the majority population is, however, exceedingly rare.[2] One important exception is a paper by Leal (2007), who studies attitudes towards education, health care and social insurance programs in the United States. Consistently with the demographic and socio-economic characteristics of this group, he finds that Latinos take more liberal (leftist) positions than the majority population on these issues, and that this is especially true of Latino noncitizens.[3]

Did the distribution of policy preferences among noncitizens differ from that found among citizens in the Swedish case? At the time of the voting rights reform, municipalities had considerable control over outcomes in policy areas that were perceived to be of particular interest to noncitizens at the time of the reform. The motivation of the Voting Rights Reform of 1975 to a large extent goes back to a previous government commission—the 'immigrant

commission'—which presented its final report in 1974. The immigrant commission had been appointed in 1968 with the instruction to chart the situation of immigrants in Sweden, and to suggest measures that could improve their situation. In its final report, the commission argued that municipalities had to make considerable efforts in several areas—most notably in education and social and family services—if the broader goal of equality of life chances between immigrants and the majority population was to be achieved (SOU 1974:69). The 'voting rights commission' that followed the immigrant commission argued that noncitizen suffrage in local elections would stimulate local efforts in these areas, as it would prompt local policy-makers to be more responsive towards the interests of immigrants (SOU 1975:15).

As Figure 1 shows, the demographic structure of the mid-70s noncitizen population differed markedly from that of the general population. Most notably, noncitizens were overrepresented in the age groups towards which primary and secondary education, as well as adult education oriented towards the formation of labor market skills, was directed. Moreover, studies also show that noncitizens clearly overrepresented among those who were actually enrolled in these various forms of municipally organized education (Opper 1983). As the immigrant commission pointed out, education policy, including home language instruction and bilingual education, was one of the most important political issues for organizations representing various immigrant groups (SOU 1974:69). With regards to municipally provided social and family services, their primary targets were also groups in which noncitizens made up a disproportionate share. In particular, many of these services, such as pre-school and after-school services and child and adolescent care, were aimed at families with children.[4] According to the immigrant commission, immigrant organizations argued for increased municipal efforts in these areas too.

[Figure 1 about here.]

Whereas the first condition for the existence of an enfranchisement effect is that the distribution of policy preferences among noncitizens is different from that found among citizens, the second is that noncitizens make up a significant bloc among the voters. In

the United States, around 7% of the population are noncitizens (Kandel 2011). While noncitizens are not allowed to vote in most elections in the United States, studies show that when it comes to other modes of political participation, this group is far from inactive (Verba, Schlozman and Brady 1995, Leal 2002). Taken together, this suggest that noncitizens would make up a significant bloc of voters if voting rights were more widely extended. Turning to the Western European countries, we find that whereas some have resisted immigration even during the postwar era, and where noncitizens therefore make up a small fraction of the population, many others have large noncitizen populations (OECD 2008). Clearly, noncitizen voting would, or already does, enfranchise non-negligible shares of the population in these countries. Studies of European countries that have introduced noncitizen suffrage show that although noncitizens do not vote as frequently as citizens, they do turn out in significant numbers (Messina 2007). In general, therefore, the potential political importance of noncitizens as a group will many times be large. The fact that noncitizens are often geographically concentrated in particular communities also suggests that noncitizens may wield more influence than their aggregate numbers and rates of participation at the national level suggest.

In the Swedish case, the first municipal elections following the reform were held in 1976. That year, as a result of the reform, well over two hundred thousand noncitizens were entitled to vote; the newly enfranchised made up about 3.5 % of the total electorate.[5] Due to the spatial dispersion of immigrants, the impact of the reform differed markedly across municipalities. To give the two most extreme examples: Åsele, a relatively small municipality in the northern administrative county of Västerbotten, saw 12 noncitizens eligible to vote in the 1976 municipal elections, accounting for less than 0.2% of the electorate. The same year, 1523 noncitizens (making up about 13% of the electorate) had the right to vote in the municipal elections of Olofström, another relatively small municipality located in the southern administrative country of Blekinge. Noncitizens did also exhibit political involvement. Even before the reform, some noncitizens were active in parties, and many

were union members. In the municipal elections of 1976, one estimated that around 60 % of the noncitizens exercised their new-won right.[6] While the noncitizen turnout rate was higher than contemporary experts had predicted, there were few noncitizen candidates, and of those who did stand for office, only a few managed to get in (Hammar 1979).

Selection Bias

Whereas the preconditions for the existence of an enfranchisement effect seem to obtain in many cases, including the Swedish one, noncitizen voting is nonexistent or severely restricted in most countries. As a recent survey by Earnest (2008) shows, New Zealand is the only established democracy that allows noncitizens to vote in national elections. However, sub-national political authorities in several countries have chosen to enfranchise noncitizens. At first blush, these examples may seem to provide the researcher with within-country variation that can be used to estimate the policy impact of noncitizen suffrage. However, as I have already argued, it is preferable to study franchise extensions that are imposed on, rather than endogenously chosen by, the political units under study. When the franchise is extended locally, estimates of the enfranchisement effect would in all likelihood suffer from self-selection bias, and would thus have little to say about the causal effect of noncitizen suffrage on public policy. If, on the other hand, the franchise is extended independently of the factors that determine its expected policy consequences, such as is the case when it is imposed on sub-national units by the central government, the prospects of obtaining consistent and unbiased estimates of the impact of noncitizen suffrage on public policy improve dramatically.

The controversial nature of noncitizen suffrage suggests that the threat of selection bias is very real. One case in which sub-national political authorities have, at various times, enfranchised noncitizens is the United States. Here, noncitizen suffrage dates as far back as to the colonial period and was practiced in many states during the first 150 years of independence (Hayduk 2006). Around the turn of the previous century, a sweeping retreat

from the institution of noncitizen voting took place, in many cases following public referenda. Opponents were especially doubtful of including the Catholic working-class immigrants that first came from Ireland, and later on from Southern and Central Europe, into the polity. One important reason for this was noncitizens' preferences with regards to the contested policy issues of the day, such as the slavery question, and the related concern with their impact on election outcomes (Keyssar 2000, Hayduk 2006). As Hayduk (2006) shows, there are many similarities between these historical struggles and present-day campaigns to reinstate noncitizen suffrage. From his detailed case studies of campaigns for noncitizen voting rights in United States municipalities, he concludes that resistance to such reforms has been widespread, and that it has often been grounded in fear of their potential impact on election outcomes and public policy.[7] Studies of the policy consequences of noncitizen suffrage using these, and similar, cases would therefore, in all likelihood, result in selection bias. Rath (1990), for instance, discusses how popular resistance to noncitizen suffrage varies within and between a number of West European countries.[8]

Since noncitizen suffrage was imposed on Swedish municipalities by the national parliament, the risk of selection bias is minimized. The debate was initiated in 1968 by a Social Democratic member of parliament, but the party as a whole did not adopt this position until 1974. With the exception of the Communist party, other major parties were also skeptical initially. In the end, however, the parliamentary decision in 1975 was supported by all parties. This turn of events has been considered remarkable by commentators, given the initial opposition of the majority population (Rath 1990). According to the most comprehensive account of the events that led up to the Voting Rights Reform, Swedish municipalities were not active participants in this process. Rather, the issue was pushed by a coalition of top-level bureaucrats at central government agencies and high-ranking politicians within the Social Democratic party. The latter also appear to have been strongly influenced by discussions within the diplomatic context of the Nordic Council, where Finland had argued that the large group of Finns permanently residing in Sweden should be allowed the vote

(Hammar 1979).

A related issue concerns the scope of the Swedish reform, which was, and still is, exceptionally inclusive. All that is required to be able to vote and stand for office in municipal elections is a total of three or more years of permanent residence in Sweden. Earnest (2008) shows that only about a dozen advanced industrial countries have taken the step of enfranchising all resident noncitizens in local elections, irrespective of their country of origin. In addition, most of these countries have longer residency requirements than Sweden's. To give one example of limited suffrage, consider the European Union member states. Following a debate that had been initiated in the 1970s, the 1993 Maastricht Treaty granted citizens of an EU country residing in another member country local suffrage. Even this limited version of noncitizen suffrage was resisted for a long time by countries with a large number of resident noncitizens, such as the United Kingdom, France and Germany (Neuman 1992). Estimating the enfranchisement effect in countries with limited noncitizen suffrage, such as is the case in all European Union members states, must also, almost by definition, result in bias. The reason is analogous to that which militates against comparing cases that have endogenously extended the franchise to those that have not. In particular, we would expect the franchise to be extended mainly to those groups whose inclusion are unlikely to disturb the prevailing political equilibrium.

Statistical Model, Data and Variable Measurement

I consider a two-period statistical model where the term of office that lasted from 1973 to 1976 is the first, and the term that lasted between 1976 and 1979 is the second. In the 1973 elections, noncitizens were not allowed to vote, whereas the Voting Rights Reform allowed noncitizens to vote in the 1976 elections. Letting s stand for the share of noncitizens in the electorate and recognizing that during the first period $s = 0$ for all municipalities, the pre-

and post-reform empirical equations can be written

$$\begin{aligned} g_{j1973} &= \lambda_{1973} + \gamma_{1973} X_{j1973} + v_j \\ g_{j1976} &= \lambda_{1976} + \beta s_{j1976} + \gamma_{1976} X_{j1976} + v_j + \mu_{j1976} \end{aligned} \tag{1}$$

where the terms of office are indexed $t = 1973, 1976$, specific municipalities are subscripted by j, λ_t is a term-specific intercept, v_j is a municipality-specific effect and μ_{j1976} is the error term. For each time-period I have included a limited number of control variables (see more below), which are captured by the vectors X_{1976} and X_{1973}. Attentive to concerns about assuming that variables have essentially the same impacts over time (Ray 2003), I have allowed the parameters (γ_{1976} and γ_{1973}) associated with the control variables to vary across time. I have allowed the intercept to vary both across terms of office, as captured by λ_t, and municipalities, as captured by v_j, because there may be common time trends as well as factors that are more or less constant within municipalities, that have an impact on spending.

As a standard precaution to get rid of the abovementioned municipality-specific factors, I estimate the model in (1) in differences. This is important to the extent that the municipality-specific effects have an impact on the share of noncitizens in 1976 electorate.[9] Specifically, I opt for a differences-in-differences approach

$$\Delta g_{j1976} = \Delta\lambda_{1976} + \beta s_{j1976} + \gamma_{1976} X_{j1976} + \gamma_{1973} X_{j1973} + \mu_j \tag{2}$$

where $\Delta g_{j1976} = g_{j1976} - g_{j1973}$ and $\Delta\lambda_{1976} = \lambda_{1976} - \lambda_{1973}$.

I estimate (2) by applying OLS to data compiled from publications issued by *Statistics Sweden*. All variables are expressed as averages for the relevant term of office. The main reason for this averaging is that studies show that collapsing the pre- and post-reform periods lessens the risk of Type I error that might otherwise result from applying differences-in-differences methods to serially correlated outcomes (Bertrand et al. 2004). The Voting

Rights Reform took place in the wake of a large-scale municipal boundary reform that was implemented in 1969, and which gradually reduced the number of municipalities by almost 70%—from 848 to 278—by 1974. Of these 278 municipalities, I can only include those municipalities that did not change after 1972, leaving me with a sample of 183 municipalities.[10]

The following variables are used in the analysis:

Dependent Variables. To measure changes in policy, I focus on changes in municipal spending decisions. I have been able to construct comparable time-series on aggregate per capita spending by the municipal offices of education and culture (*Education Services*) and the municipal offices of social and family issues (*Social and Family Services*).[11] Spending on primary and secondary schooling typically made up the vast majority of spending on education services, whereas day care and pre-school accounted for the lion's share of spending on social and family services. Together, education and social and family services typically account for almost 40% of the municipal budget during the period of study.[12]

Independent Variable. The crucial independent variable is the fraction of noncitizens in the municipal electorate. The variable *Noncitizens in Electorate* is created on the basis of data from the electoral register.

Control Variables. Even though I am studying a sample of comparatively homogenous political units for a limited period of time, there may be concerns that the main independent variable, *Noncitizens in Electorate*, is correlated with a number of variables that are, in their turn, correlated with trends in public spending. However, and as is well known, including controls that might be affected by my main variable of interest might lead to over- or underestimation of it's total effect. Therefore, I have aimed to include a limited number of controls that are correlated with, but not causally affected by, the share of noncitizens in the post-reform electorate.

First, I control for the number of inhabitants in the municipality measured in 100 000s (*Population*). The reason for the inclusion of this variable is that urban areas and the bigger cities did, to some extent, attract noncitizens. Most of the municipalities whose electorates

were very little affected by the reform, on the other hand, were small and located in the countryside. Examples of large municipalities where noncitizens made up a considerable share of the electorate are the municipality of Stockholm (5%) and surrounding municipalities, such as Järfälla (8%), Huddinge (8%), Haninge (8%) and Södertälje (10%).

Second, I include the per capita municipal taxbase (*Taxbase/capita*). Although many of the labor force immigrants that came after World War II, and especially during the 1960s, came to do low-wage jobs, their market incomes were not markedly lower than those of citizens at the time of the reform. The enfranchisement of noncitizens therefore did not cause the decisive voter to shift down the distribution of income and this remained true throughout the 1970s.[13] Given the geographical dispersion of noncitizens, the share of noncitizens in the electorate may, however, still be correlated with the municipal taxbase.

Third, I include the share of the workforce that were in manufacturing and mining (*Population in Manufacturing*). Around the mid-70s, the Swedish immigrant population was, to a large part, made up of labor migrants who had come since the 1950s to do blue-collar jobs in the manufacturing sector (Lundh och Ohlsson 1999; Lundh 2005). During the periods studied here, there was thus a clear relationship between the spatial dispersion of the manufacturing sector and that of immigrants at the time of the Voting Rights Reform of 1975. The municipality in which noncitizens came to make up the largest share of the electorate (13%) as a consequence of the reform was the previously mentioned Olofström, where a large number of Finns and Yugoslavs had come to work in the auto industry. The auto or steel industry dominated many of the other municipalities that received an especially large inflow of noncitizens into the electorate, such as Södertälje (10%), Surahammar (12%) and Hallstahammar (11%).

Descriptive statistics for all variables used in the empirical analysis can be found in the Supporting Information to this paper.

Empirical Analysis

In this section, I report the results of my empirical analyses. First, I estimate the model described in the preceding section. These are the main results. I then go on to estimating whether the impact varies with the demographic characteristics of the noncitizen population. Finally, I perform a series of robustness checks of the main results.

Main Results

Recognizing the risk of including intervening variables, while at the same time realizing the need to control for confounding variables, I estimate one sparse model and one full model for each of the dependent variables. Table 1 presents the results from these estimations.

[Table 1 about here.]

The first two columns show the results from estimating the model in equation (2) with education spending as the dependent variable. As can be seen, the coefficient estimates for the proportion of noncitizens in the 1976 electorate are positive and statistically significant in both models, although the coefficient is slightly larger in magnitude when including the control variables. To gauge whether the impact of the reform on spending on education services is substantively important, consider a 'typical' municipality, which I define as one where noncitizens made up 3% (the empirical mean) of the 1976 electorate. All else being equal, the estimates suggests that average annual per capita education spending in such a municipality rose by between 200 and 270 SEK, or between 25 and 35 in 2009 US$, as a result of the reform.[14] Given that the typical municipality in our sample annually spent 6000 SEK, or US$ 785, per capita on education during the 1976–1979 term, this is a substantial effect.

In columns (3) and (4) of Table 1, I have substituted spending on social and family services for education spending. As is clear, the coefficient estimates for the proportion of noncitizens in the electorate are positive and statistically significant here too. Moreover,

they are very similar across the two specifications. Using these estimates, we see that in our typical municipality, where noncitizens made up 3% of the electorate, expected annual per capita spending on social and family services rose by about 300 SEK, or US$ 40, as a consequence of the reform. This figure is also large, especially when we consider that social spending was low compared to spending on education. Specifically, the typical municipality in our sample annually spent about 3400 SEK, or US$ 460, per capita on social and family services during the 1976–1979 term.

Turning to the control variables, we see that when a municipality becomes richer and more populous spending on education and social and family services goes up. The results for population are, however, statistically insignificant in the case of education services. The share of the population in manufacturing is positively related to spending on education and negatively related to social spending. However, none of these coefficient estimates are statistically significant.

Summing up so far, the empirical results show that the Voting Rights Reform of 1975 had a positive impact on municipal education and social spending. The estimated effect of enfranchising noncitizens on education services is substantively large and statistically significant and the impact on social sand family services appears to have been even larger.

Does the Impact Vary With Demographics?

As I have argued above, spending on primary and secondary schooling typically made up the vast majority of spending on education services at the time of the reform. And since noncitizens made up a disproportionate share of the school-aged population, it was hypothesized that noncitizen suffrage would have a positive impact on education spending. Similarly, day care and pre-school accounted for the major part of the spending on social and family services, and since noncitizens were highly overrepresented among children below school age, the reform should have an impact on this too. The degree to which noncitizens were overrepresented among children of school age and below does, however, vary considerably

across municipalities. This suggests that the impact of the voting rights reform on education services should have been larger in municipalities where a large share of noncitizens were school aged, and that its impact on social and family services share should have been increasing in the share of noncitizens below school age.

For each of the two spending outcomes, I therefore estimate a multiple interaction version of the full model. In the case of education services, I let the impact of the share of noncitizens in the electorate depend on a measure of the fraction of noncitizens that are between the ages of 5 and 14 (*School-Aged Noncitizens*). In the case of social and family services, the impact of the share of noncitizens in the electorate is allowed to vary wtih the fraction of noncitizens that are below the age of 5 (*Preschool-Aged Noncitizens*).[15] Following the advice of Barmbor et al. (2006), I include both interaction terms and constitutive terms in the analyses. The results are shown in Table 2.

[Table 2 about here.]

As can be seen, the coefficient estimates for the interaction terms have the expected signs. In particular, it can be seen from the first column that the impact of the reform on education services was larger where many noncitizens were school-aged, even if the interaction term is not statistically significant. From the second column, it can be seen that the reform's impact on social and family services was larger where a large share of noncitizens were preschool-aged. In order to facilitate the interpretation of the estimates I have, for each model, calculated the marginal effect of the share of noncitizens in the electorate for the entire empirical range of the conditioning variable. The results from these calculations are shown in Figure 2, together with 95% confidence intervals and the distribution of observed values for the respective mediating variables. In the case of education services, the marginal effect increases more than tenfold as we go from a situation where 8% (the empirical minimum) of noncitizens are school-aged, to a situation where 38% (the empirical maximum) are school-aged. However, the 95% confidence interval is fairly wide and the marginal effect is only

statistically significant when more than 18% of noncitizens are school-aged. On the other hand, this result makes substantive sense, given that this shows that the reform only had a statistically significant impact on education services where the proportion of school-aged noncitizens lies clearly above the proportion of school-aged citizens at the national level (see Figure 1). Turning to spending on social and family services, a move from a situation where 4% of noncitizens are school-aged, to a situation where 20% are, leads to an almost threefold increase in the marginal effect of the share of noncitizens in the electorate. Again, the 95% confidence interval is wide, and the marginal effect becomes statistically significant where the preschool-aged make up 6% or more of the municipal noncitizen population. This result, too, makes substantive sense, since it is only then that the proportion of school-aged noncitizens lies above the proportion of school-aged citizens (see Figure 1).

[Figure 2 about here.]

The results in this section support the notion that the impact of the voting rights reform varied with local demographics. In particular, the effect of the voting rights reform on education and social and family services, while being somewhat imprecisely estimated, was considerably larger in those municipalities where the noncitizen population, because of its demographic characteristics, is likely to have demanded increased efforts in these policy areas.

Robustness Checks

Despite the clear results of the preceding sections, I perform a number of robustness checks. Full information on these analyses are presented in the Supporting Information to this paper.

I begin by addressing the potential endogeneity that would arise if noncitizen locational choices are predicated on municipalities that are improving their services. For this purpose, I use an instrumental variables approach. Immigration settlement patterns are highly persistent, partly because the settlers tend to stay in their initial location and partly because new

settlers will tend to be drawn to previous settlements because of social networks and family motives. In particular, I instrument the share of noncitizens in the electorate using county-level census data on the initial settlement locations of immigrants during the two post-war periods prior to the 1970s when immigration inflows spiked. The first spike occurred in the 1940s, when WWII-refugees from the neighboring occupied Scandinavian countries, as well as Germany, Poland and the Baltic states fled to Sweden. This instrument should, for the above mentioned reasons, be relevant in predicting the location of noncitizens in the 1970s. Furthermore, It is unlikely that the initial locations of these refugees were affected by the level of local public services, suggesting that the instrument is also valid. The second spike occurred in the 1960s, during which Sweden saw a sharp increase in labor immigration, primarily from Finland and Yugoslavia. Since most of these workers were actively recruited by corporations or Swedish public authorities for the purpose of satisfying personnel needs in the manufacturing sector, it is unlikely that their initial residential choices reflected their taste for local public services.[16] As the joint F-tests of the instrumental variables in the first four columns of Table 3 show, the instruments are relevant in that they together explain a considerable part of the variation in the share of noncitizens in the 1976 electorate. The F-values surpass Staiger and Stock's (1997) rule of thumb, which states that they should be above 10. The complete first-stage regression results are shown in the Supporting Information to this paper. Moreover, tests of the overidentifying restrictions do not reject the hypothesis that the instruments are valid, except for that in the fourth column, which is borderline significant.[17] Using this instrumental variables approach, the coefficient estimates are qualitatively similar to those found the models in Table 1, and the coefficient estimates for the proportion of nonicitizens in the electorate even increase. I have also performed additional instrumental variable analyses, using the two above-mentioned instruments one at a time. The results for these just identified analyses, which can be found in the Supporting Information, are similar to those reported here.

[Table 3 about here.]

In addition, I have also reestimated the models in Table 1, replacing the share of noncitizens in the 1976 electorate with an estimate of what would have been the share of noncitizens in the 1973 electorate if the reform had been implemented before this election; the voting age noncitizens' share in the 1973 voting age population. The purpose is to further probe the potential for endogeneity resulting from noncitizen locational choices. In particular, it minimizes the risk that my estimates are biased upwards because of internal migration by noncitizens to areas where their policy preferences will have a larger impact. The results from the analyses using the proportion of noncitizens in the 1973 voting-age population are in the last four columns of Table 3, and are similar to those found using their share in the 1976 electorate, although the coefficient estimates are slightly smaller.

Furthermore, I have reestimated the share of noncitizens in the electorate with an estimate of the share of noncitizens among those who actually voted. Actual data on the share of noncitizens in the voting population is not available. To construct a measure, I have utilized the fact that, since 1970, national and municipal elections have been held at the same day. Because the Voting Rights Reform of 1975 only enfranchised noncitizens in municipal elections, one can use the turnout-differential between national an municipal elections to obtain an estimate of the share of noncitizens among those who actually voted. Because some citizens only vote in national elections, this difference overestimates the number of noncitizens who actually voted in municipal elections. I therefore adjust this figure with information on the turnout-differential between national an municipal elections in 1973, when only citizens were allowed to vote. The results using the estimate of the share of noncitizens among those who actually voted are similar to those found when using the share of noncitizens in the electorate as the main independent variable, although the coefficient estimates become slightly larger.

Next, I turn to regressing an outcome that, given my proposed theoretical mechanism, is unlikely to have been affected by the Voting Rights Reform, on the share of noncitizens in the 1976 electorate. Specifically, I consider municipal investments in waste handling facilities on

the assumption that there will be no systematic tendency for noncitizens to place a higher value than citizens on such investments. The impact of the share of noncitizens in the 1976 electorate on this outcomes is substantively small, and not statistically significant. This is also evidence that the expansion in spending on education and social and family services were not simply the result of some general tendency towards higher overall levels of service in municipalities where noncitizens made up a large portion of the population.

The results are also robust to the addition of political control variables. In particular, I add political control variables that I was reluctant to include in the main results because they may be consequences of noncitizen suffrage. The first is per capita government grants to the spending areas under study (*Education Grants* and *Social Grants*) which accounted for about 45% of municipal spending on education and culture, and about 28% of spending on social and family assistance, in the typical municipality. The second is a dummy variable indicating whether or not the Social Democrats and the Left Party together held a majority of the seats in the municipal council. Unsurprisingly, the coefficient estimates for the share of noncitizens in the electorate do decrease by more than a factor of two in the case of education, and by slightly less in the case of social and family services, upon the inclusion of these controls. However, both are still statistically significant, and still substantively large.

Finally, when drawing conclusions from small or medium sized samples, there is always the risk that one's parameter estimates are strongly influenced by a minority of one's cases. In order to detect any municipalities that exert a strong influence on my estimates, I turn to the method of detecting influential cases suggested by Bollen and Jackman (1990, 267): For all models in Table 1 I look for cases that alter one or more regression coefficient by at least one standard error. Applying this criterion, I find that the municipalities of Värmdö, Mellerud and Stenungsund exert an influence that is above the threshold in one or more of the models in Table 1. To gauge the impact of these municipalities on the regression coefficients, I re-estimate these models including dummy variables for the influential obser-

vations. The results from these analyses show that the coefficient estimates for the share of noncitizens in the electorate decrease somewhat, but remain statistically and substantively significant, upon the inclusion of these dummy variables. In conclusion, then, this section has shown that my main results are highly robust to various model specifications. They do therefore not appear to be artefacts of the specific empirical approach used.

Conclusion

The largest disenfranchised group in modern democracies is international migrants who lack citizenship of their country of residence. While noncitizen suffrage in local elections has been introduced in some countries, and has been the subject of vigorous public debate in others, there have been no systematic scholarly attempts to investigate its policy consequences. I develop an argument about the political and economic considerations inherent in studying the policy consequences of franchise extensions, and which illustrates that noncitizen suffrage is only likely to be adopted in contexts where the policy consequences of such reform are small. I also argue that this negative self-selection can be dealt with by studying franchise extensions that are imposed on, rather than endogenously chosen by, sub-national units. This condition is met in the case of the Voting Rights Reform of 1975, which generated exogenous changes in the composition of the electorate of Swedish municipalities. According to my analysis, giving noncitizens the right to vote significantly changed local politics. The reform substantially raised local spending on education and social and family services. Consistently with an argument that emphasizes demographics, the impact of the reform on education spending was larger where more noncitizens were school-aged, and the impact on social and family services was larger where many noncitizens were pre-school aged.

The methodological point that emerges from my study is novel, and can also be applied to franchise extensions to other groups. In general, researchers studying the policy consequences of various suffrage laws should accord higher credibility to results obtained

from studies that investigate reforms that are imposed on, rather than voluntarily chosen by, the political unit in question. Consider, for example, the study of Husted and Kenny (1997), in which they estimate the impact of the removal of various suffrage restrictions primarily intended to disenfranchise African Americans, on state and local policy in the United States. Since some of these removals were imposed from above, as a consequence of the Voting Rights Reform of 1965, their approach, like mine, has the advantage of ameliorating the selection bias that is inherent in the study of the policy consequences of franchise extensions. Future studies of the policy consequences of enfranchising minority groups are well advised to choose their cases of study with care, given the negative selection bias that is likely to threaten the internal validity of their results.

How generalizable are my results? The demographic structure of the Swedish noncitizen population during the 1970s bears many similarities to immigrant populations in many OECD countries today. Immigrants in general, and noncitizens in particular, often make up a disproportionate share of men and women of child-rearing age. It is therefore likely that removing legal and administrative hurdles to naturalization and/or noncitizen suffrage would lead to increased voter demand for the types of educational, social and family services studied in this paper. However, the typical noncitizen in the present day United States or Western Europe is also poorer than the typical citizen. This was not the case in Sweden during the mid-70s, suggesting that noncitizen suffrage may have an even stronger impact on public policy today. Although my study suggests that the extension of voting rights to noncitizens is a viable strategy for making public policy more responsive to the interests of this group, the issue of external validity would be best addressed by conducting additional internally valid studies. It is my hope that my study will stimulate such endeavours.

Finally, my findings carry implications for the broader discussion about the consequences of immigration for public policy. A perusal of this large and growing literature shows that the evidence for such a correlation is variable and controversial (see, e.g., Alesina and Glaeser 2004, Banting and Kymlicka 2006, Hopkins 2009, Gerdes 2011). By empirically

demonstrating the impact of noncitizen suffrage on public policy, this paper joins recent theoretical work by Ortega (2010) in suggesting one reason for this inconclusiveness; namely, that the ease with which immigrants can acquire political rights is a potentially important mediating variable in the relationship between immigration and public policy.

Notes

[1]In a previous version of this paper, I draw on the probabilistic voting framework (Persson and Tabellini 2000, Ch. 3) to construct a more detailed formal model of the adoption and consequence of noncitizen inclusion to clarify the point made in the main text.

[2]This lacunae is largely due to lack of data containing a sufficiently large number of noncitizen—or, for that matter, immigrant—respondents.

[3]No comparable study of the political attitudes of noncitizens in Europe exists. Dancygier and Sanders (2006) conduct a pioneering study comparing the policy preferences of immigrants to those of the majority population using data from the German General Social Survey (GGSS) and the British Election Study (BES). They find that immigrants are more likely to favor social and redistributive spending in Germany, and that their attitude is explained by demographic and socio-economic characteristics. Their findings for Great Britain are more mixed, although they find results that are qualitatively similar to those found in Germany for an item measuring attitudes towards redistributive spending.

[4]During the 1970s, noncitizen women had employment rates that were on par with, or higher than, those of native women (Ekberg and Hammarstedt 2002).

[5]The majority came from the other Nordic countries—mostly Finland—but there had also been significant inflows from Southern European countries such as Greece and Italy, as well as Eastern European countries such as Poland and Yugoslavia (Lundh och Ohlsson 1999; Lundh 2005)

[6]The turnout of noncitizens was, however, lower than corresponding figure for the entire eligible population, which was 90 %

[7]Even in cities like San Francisco, which has a long history of progressive politics, noncitizen suffrage in local school board elections has failed twice in public referenda. And the few cases where campaigns to introduce noncitizen suffrage have succeeded are brought up by opponents to reform elsewhere. For example, noncitizen voting in Chicago school elec-

tions was brought up by opponents of the measure in San Francisco raising "the specter of noncitizens controlling school funds and setting policy" (Hayduk 2006, 106).

[8]Studies of the the closely related issue of naturalization of resident noncitizens are also illuminating of the popular opposition that exists to enfranchising noncitizens. For the case of Switzerland, in which a large part of the responsibility for naturalization decisions has been devolved to municipalities, some of which even use referenda to decide on individual naturalization requests, Helbling and Kriesi (2004) find that rejection rates increase dramatically when requests are put to a popular vote. Hainmuller and Hangartner (2011), moreover, find that low-skilled workers from Turkey and Central and Eastern Europe are increasingly likely to get their application turned down, as the share of immigrants originating from these countries has grown.

[9]To the extent that municipality-specific factors have an impact on both spending, and our main independent variable of interest, estimating a cross-sectional version model using data only on the post-reform electoral term would result in biased estimates of β.

[10]As one reviewer points out, this restriction of my sample might be problematic if municipal boundary reforms are driven by voters' sentiments towards immigrants or towards public policies, analogous to the arguments in e.g. Alesina, Baqir and Hoxby (2004). However, the municipal boundary reform was, in the Swedish case, was not voluntary, but imposed on local governments by the central government. There was thus no scope for an individual municipality to affect the outcome of the reform (Tyrefors-Hinnerich 2009).

[11]The difficulty in piecing together comparable time-series for different municipal spending areas is a result of changes in the municipal accounting systems.

[12]During the 1970s, education accounted for close to 30% of spending in the typical municipality. The corresponding figure for social spending was close to 10%.

[13]This information was obtained using the registry-database *Longitudinal Individual Data for Sweden 1960-1997* (LINDA) and was based on an annual sample of between 6175–9047 noncitizens and 189 926–203 147 citizens. For information on LINDA, see Edin and

Fredriksson (2000).

[14]To calculate the US$ amount, I have used the average exchange rate for 2009, according to which a dollar cost approximately 7.65 SEK.

[15]Existing data from *Statistics Sweden* does not allow for further disaggregation of these age groups.

[16]More precisely, the instruments measure the total immigration inflow into the county during the periods 1940–1950 and 1960—1967 divided by the total county population in 1950 and 1967, respectively. Unfortunately, the data provided in the publications by *Statistics Sweden* cannot be disaggregated beyond the county level.

[17]However, as Sovey and Green (2011) note, it is in principle impossible to empirically test an instrument's validity, because tests of overidentifying restrictions when using multiple instruments rely on the assumption that at least one of the instruments is valid.

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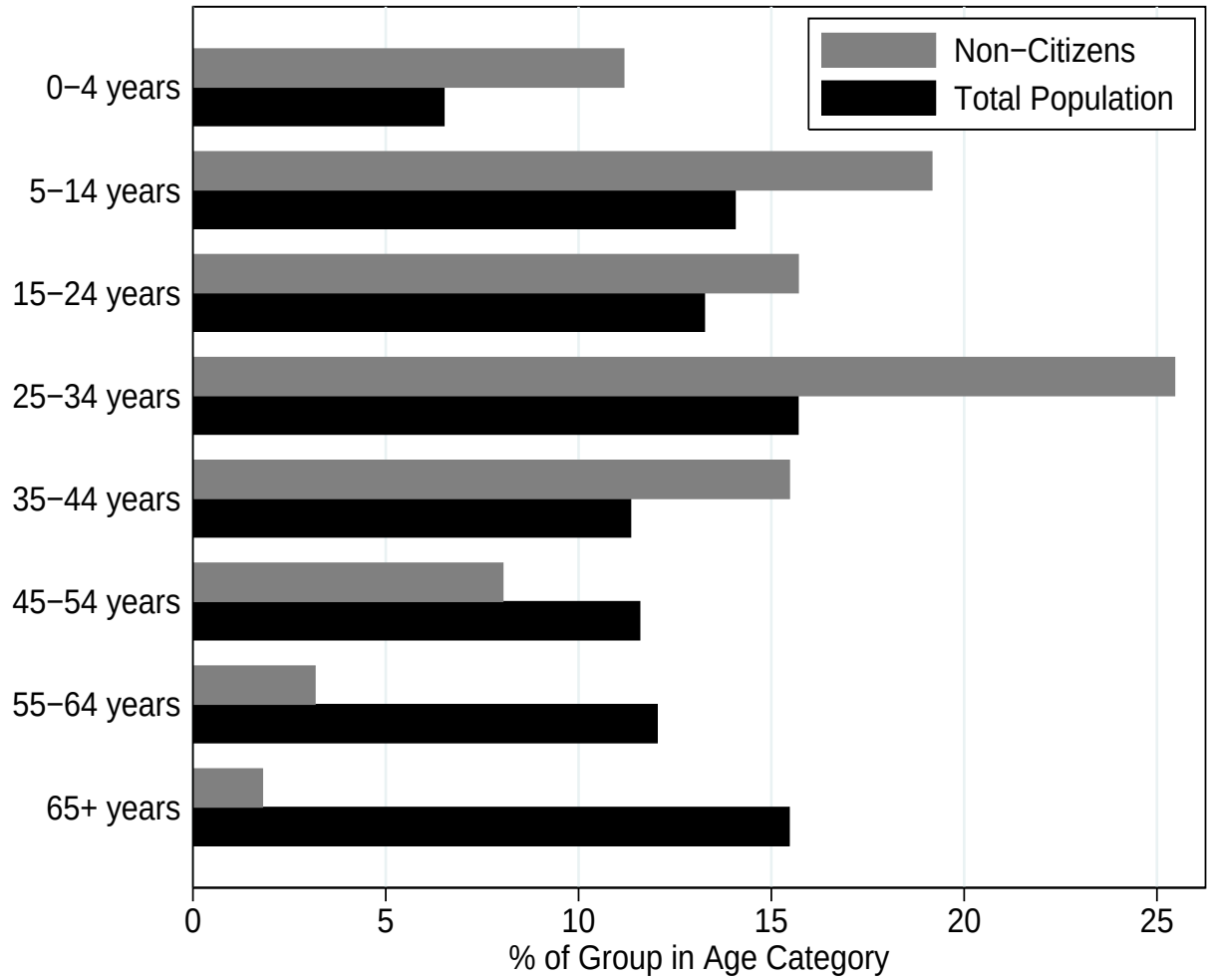

Figure 1: The Age Distribution of Non-Citizens Compared to the Total Population in 1976

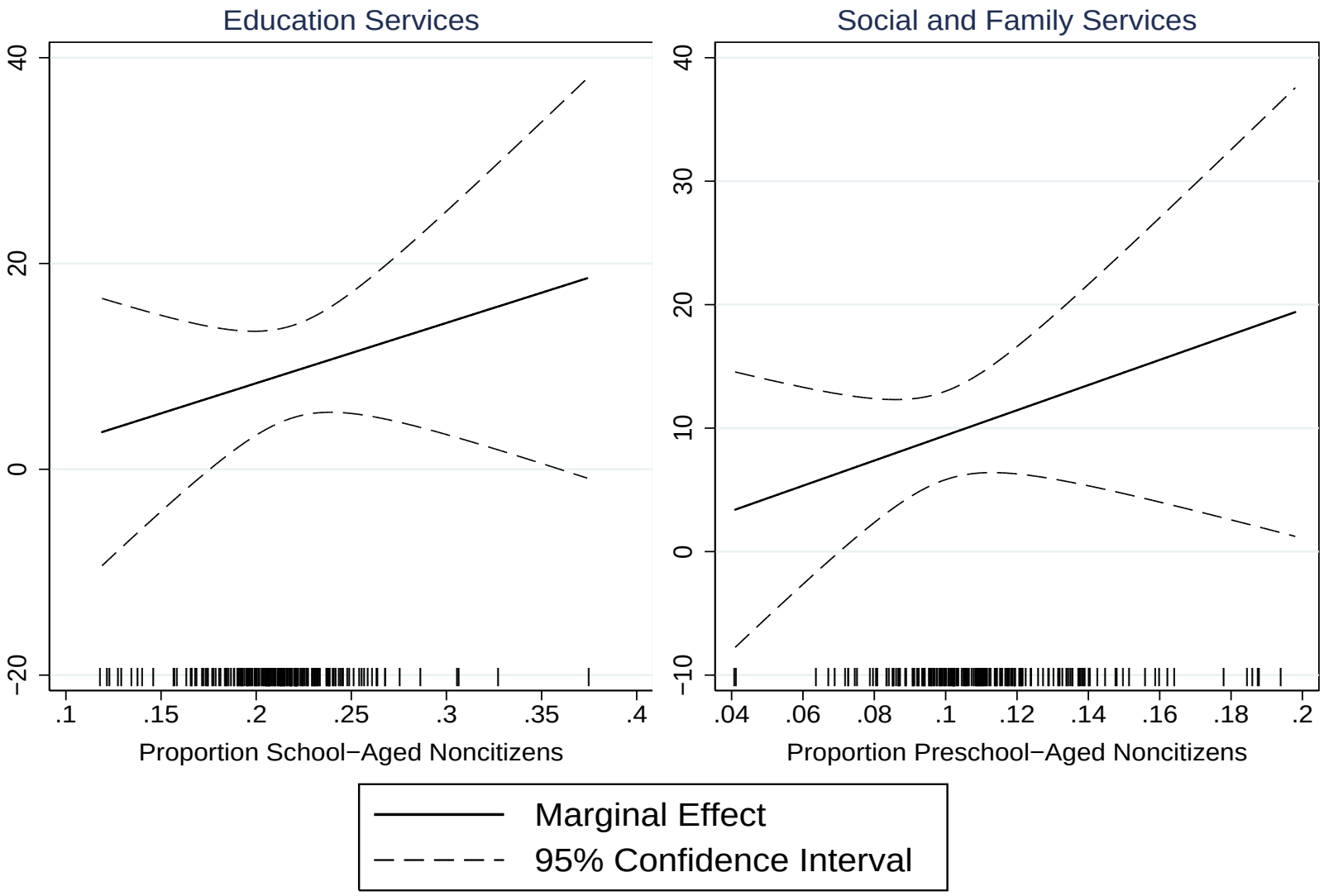

Figure 2: Marginal Effect of the Share of Noncitizens in the 1976 Electorate Conditional on Demographics. The proportion school-aged and preschool-aged noncitizens in the municipalities are plotted (in a "rug") along the bottom axis.

Table 1: Differences-in-Differences Estimates of the Effect of the Voting Rights Reform on Municipal Education and Social Spending

	ΔEducation Services		ΔSocial and Family Services	
	(1)	(2)	(3)	(4)
Noncitizens in Electorate	6.702***	8.933***	10.30***	9.962***
	(2.183)	(2.265)	(2.109)	(1.881)
Taxbase/capita		3.127		3.182**
		(2.147)		(1.287)
$\text{Taxbase/capita}_{1973}$		-3.912*		-3.203**
		(2.138)		(1.344)
Population		1.361		15.86***
		(4.686)		(3.936)
Population_{1973}		-1.372		-15.47***
		(4.578)		(3.863)
Population in Manufacturing		4.425		-3.943
		(3.965)		(2.918)
$\text{Population in Manufacturing}_{1973}$		-5.209		2.853
		(3.602)		(2.798)
Constant	0.649***	1.060*	0.565***	0.197
	(0.0686)	(0.549)	(0.0625)	(0.403)
Observations	183	183	183	183
R-squared	0.083	0.188	0.220	0.389

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Differences-in-Differences Estimates of the Effect of the Voting Rights Reform on Municipal Education and Social Spending Conditional on Demographics

	ΔEducation Services	ΔSocial and Family Services
	(1)	(2)
Noncitizens in Electorate	-3.360	-0.780
	(13.81)	(9.350)
School-Aged Noncitizens	2.063	
	(1.559)	
Preschool-Aged Noncitizens		-1.134
		(1.692)
Noncitizens in Electorate×School-Aged Noncitizens	58.63	
	(62.20)	
Noncitizens in Electorate×Preschool-Aged Noncitizens		101.9
		(92.22)
Taxbase/capita	3.737*	3.289**
	(2.171)	(1.283)
$\text{Taxbase/capita}_{1973}$	-4.059*	-3.212**
	(2.121)	(1.317)
Population	1.627	14.95***
	(4.665)	(4.170)
Population_{1973}	-1.590	-14.59***
	(4.549)	(4.090)
Population in Manufacturing	4.071	-4.359
	(3.813)	(2.988)
$\text{Population in Manufacturing}_{1973}$	-5.052	3.454
	(3.501)	(2.919)
Constant	0.169	0.201
	(0.793)	(0.453)
Observations	183	183
R-squared	0.216	0.393

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Differences-in-Differences Estimates of the Effect of the Voting Rights Reform on Municipal Education and Social Spending Using and Instrumental Variables Approach and an Alternative Independent Variable

	Instrumental Variables				Alternative Independent Variable			
	ΔEducation Services		ΔSocial and Family Services		ΔEducation Services		ΔSocial and Family Services	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Noncitizens in Electorate	8.752***	10.59***	15.67***	13.59***				
	(2.345)	(2.840)	(3.334)	(2.257)				
Noncitizens in VAP$_{1973}$					5.824***	7.796***	8.883***	8.587***
					(1.815)	(1.853)	(1.726)	(1.512)
Taxbase/capita		3.200**		3.341***		3.222		3.238**
		(1.529)		(0.964)		(2.101)		(1.253)
Taxbase/capita$_{1973}$		-4.067***		-3.542***		-3.867*		-3.208**
		(1.573)		(1.089)		(2.071)		(1.299)
Population		0.254		13.44***		0.820		15.74***
		(4.910)		(3.204)		(4.557)		(3.852)
Population$_{1973}$		-0.294		-13.11***		-0.855		-15.36***
		(4.800)		(3.148)		(4.451)		(3.781)
Population in Manufacturing		4.915		-2.871		4.098		-3.865
		(4.286)		(2.598)		(3.940)		(2.862)
Population in Manufacturing$_{1973}$		-5.949		1.234		-4.950		2.824
		(3.760)		(2.337)		(3.549)		(2.730)
Constant	0.587***	1.123*	0.404***	0.335	0.646***	0.931*	0.550***	0.127
	(0.105)	(0.664)	(0.0967)	(0.318)	(0.0686)	(0.542)	(0.0611)	(0.384)
Observations	183	183	183	183	186	186	186	186
R-squared	0.075	0.184	0.160	0.368	0.089	0.190	0.236	0.402
F Excluded Instruments	63.02***	79.64***	63.02***	79.64***				
Hansen J (Overidentification)	1.32	0.70	2.27	2.98*				

Robust standard errors in parentheses. Clustered on county in columns 1–4.
*** p<0.01, ** p<0.05, * p<0.1