

The End of the European Welfare States? Migration, Ethnic Diversity and Public Goods

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Abstract

Over the last several decades global migration flows have increased rapidly, resulting in corresponding increases in the number and sizes of ethnic minorities in many places - Western Europe in particular. Given the existing theory and evidence of a negative relationship between ethnic diversity and public goods, a simple extrapolation thus suggests that the large public sectors in Western Europe will shrink. However, stark differences in the histories of ethnic conflict, quality of institutions and timing between the European case and the settings studied in the existing literature raises concerns that such an extrapolation might be misguided. Using data on municipal elections and budgetary outcomes in Danish municipalities 1981-2001 this paper attempts to address these concerns. Employing a rich set of controls and an IV strategy based on historical housing data, the main results of the paper show that ethnic diversity has impacted outcomes of municipal elections in a way consistent with lower public good demand. Using a simple theoretical model to disentangle ethnic diversity effects from other budgetary effects, the paper further shows that the same holds true for budgetary outcomes, although an untestable but plausible auxiliary assumption is required on the budgetary process. The findings have important implications for immigration and refugee policy both in Europe and more broadly.

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In 1981, the yearly net migration to Denmark from non-Western countries was just over 1,000 per year. Twenty years later, in 2001, this number had increased more than tenfold to 11,000, resulting in a corresponding increase in the stock of non Western immigrants in Denmark from 55,000 to 269,000 - almost a fivefold increase in just 20 years.

But Denmark is by no means unique in this regard. Throughout Europe and elsewhere, both voluntary migration and refugee flows have exploded over the last several decades and in many places implied very large increases in the number and sizes of ethnic minorities.

In economics, scholars have had a long standing interest in the effects of such ethnic diversity on economic outcomes. Starting with the seminal papers by Alesina et al. (1999) and Easterly and Levine (1997), scholars have argued and provided evidence that ethnic diversity causes communities to have lower levels of trust and "social capital",¹ poorer policy outcomes² and lower levels of public goods.³

As discussed for example by Alesina et al. (2001) one interesting aspect of the latter link between ethnic diversity and public goods is that it can explain the stark differences in the size of the public sector between Western Europe and the US. According to this "*ethnic diversity-public goods hypothesis*" the existence of the large welfare states in Western Europe is due to the historically very homogeneous populations in the Western European countries.

Having made this connection however, an obvious question to ask is whether the large European welfare states can continue to exist in light of the increased inflows of immigrants and refugees. The widely held notion that European politics have experienced a right-ward⁴ shift in recent years suggests that the large public sectors have in fact come under pressure. That this shift has in large part been fueled by growing support for anti-immigrant nationalist parties further suggests that increased migration and ethnic diversity are among the causes.

More broadly, an important implication of the *ethnic diversity-public goods hypothesis* seems to be that migration can have important effects on (among other things) public goods provision because of its impact on ethnic diversity. In the existing literature on ethnic diversity, however, these implications for the effects of migration have received less attention. For example, the policy

¹Alesina and La Ferrara (2000) Fisman (2003), Lassen (2007)

²Easterly and Levine (1997), Alesina and La Ferrara (2005)

³Alesina et al. (1999), Miguel and Gugerty (2005), Banerjee et al. (2005)

⁴Here and throughout the paper I use, the standard European nomenclature, that "the right" denotes a political position in opposition of a large public sector and "the left" denotes the converse.

implications drawn seems to have centered on the importance of mitigating the adverse effects of *existing* ethnic diversity and the potential pitfalls of blindly enforcing less segregation of ethnic groups *within* a country.⁵

One explanation for this is the lack of credible empirical studies of the effect of changes in ethnic diversity. While many celebrated studies have provided strong cross-sectional evidence that ethnic diversity is negatively related to public goods in the long run, little credible evidence exists from longitudinal studies.⁶ There are several reasons why the existing cross-sectional results might not apply when ethnic diversity is increasing over time due to migration:

First of all, changes in ethnic diversity caused by migration are likely to involve inflows of new ethnic groups with which the existing ethnic group(s) have no prior history of conflict. In particular, this is true in the European case where many of the immigrants and refugees come from countries with which the Europeans have had fairly limited interactions.⁷

Second, as discussed for example by Easterly (2001), there is a clear notion in the literature that good institutions can mitigate ethnic conflict and thus limit the effect of ethnic diversity. Since current migration flows are directed mainly at developed countries with good institutions this suggests that the ethnic diversity effects of migration may be negligible.

Finally, there are several reasons why the long-run effects of ethnic diversity may be different than the effect of short-run changes in ethnic diversity. This again suggests caution in extrapolating from studies of the cross-sectional effect of (stable) ethnic diversity to the short-run effects of changes in ethnic diversity following migration.

To address the above concerns, the present paper undertakes a study of election and budgetary outcomes in Danish municipalities over the period 1981-2001. Danish municipalities experienced very different flows of immigrants and refugees over this period and thus also experienced very different increases in ethnic diversity. Combined with the high-level of fiscal autonomy in Danish municipalities this makes the Danish context an ideal case study for examining the effects of migration-related increases in ethnic diversity on the public sector.

The paper estimates dif-in-dif-type regressions of changes in election and budgetary outcomes

⁵See for example Easterly (2001), Khwaja (2009)

⁶In section 2 I provide a brief overview of the existing literature.

⁷Of course, if one goes back in history far this ceases to be true for almost any ethnic group. The most pressing concern might be the Great Turkish War in the 17th century between the Europeans against the Turks, who today make up a large ethnic minority in many European country.

on changes in ethnic diversity. In addition to municipality "fixed effects", which are automatically controlled for by the use of differences, the paper employs a rich set of controls to isolate the effect of ethnic diversity. In particular, a set of socioeconomic controls rules out the possibility that migration affects outcomes simply because immigrants and refugees are poorer on average or have lower employment rates (an effect which has been studied extensively in the migration literature⁸).

A particular identification problem which seems the bane for empirical studies of migration and ethnic diversity is the possibility of feedback from relevant outcomes to the endogenous location choice of migrants. In the present case this corresponds to the possibility that immigrants and refugees respond to election and budgetary outcomes by moving to a different municipalities.

To overcome this problem the present paper develops a novel IV strategy based on the availability of historical housing stock data and an institutional constraint that essentially forces foreigners to live in rental housing for the first five years they are in Denmark. Since highrise buildings are much more likely to serve as rental housing and since the composition of the housing stock is very persistent over time, the paper uses the share of the 1970 housing stock comprised by highrise buildings as an instrument for later increases in ethnic diversity.

The main results of the paper regard election outcomes. Simple OLS estimates here clearly show that increases in ethnic diversity are associated with increased political success for right-wing anti-immigrant nationalists but decreased success for the rest of the right-wing parties. There is also evidence of increased success for leftist parties. The corresponding IV estimates suggests an even larger positive effect for nationalist but suggests a significant *negative* effect for the left-wing. Although estimates are imprecise, some evidence also remains that part of the success of the nationalists is at the expense of the other right wing parties. The observed difference between OLS and IV is consistent with a feedback bias in the OLS estimates caused by immigrants and refugees preferring to live in left-wing municipalities. The finding of a fairly clear right-ward shift when using IV suggests that voters respond to increases in ethnic diversity by shifting their votes to parties that favor a smaller public sector as predicted by the *ethnic diversity-public goods hypothesis*.

To substantiate the election results, the paper also examines budgetary outcomes. Interpre-

⁸See for example Razin et al. (2002).

tation of results from budgetary outcomes are complicated, however, by two institutional details: The first complication arises because some municipal expenditure items is determined by national rules and because some of these expenditure items increase automatically with ethnic diversity (examples include "second language" classes in public schools and introduction programs for newly arrived refugees). The second complication arises because in order to compensate municipalities for these increases in expenditure, ethnic diversity also directly increases budgetary transfers to the municipality, which adds the possibility of confounding income effects at the municipal level.

In order to separate ethnic diversity effects from these direct budgetary effects, the paper uses a simple theoretical model to discuss the restrictions imposed on the data under various assumptions regarding the budgetary process and the effect of ethnic diversity.

In terms of raw estimates, both OLS and IV very clearly indicate that increases in ethnic diversity are associated with *increases* in municipal expenditure and *decreases* in municipal tax rates and tax revenue. Despite an essential budget balance requirement, this divergence of expenditures and revenues is possible exactly because of the corresponding increase in transfers to the municipality.

Under the untestable but plausible assumption of a negative net income effect of increases in immigrant related expenditures and increases in municipal transfers, the employed theoretical model shows that these results imply that *ceteris paribus* ethnic diversity does lower public good demand. This corroborates the findings for election outcomes.

By providing credible evidence of the short-run effects of migration-related increases in ethnic diversity in a setting where the ethnic groups involved have little prior history and where institutions are of very high quality, the paper makes several important contributions. In relation to the literature on the effects of ethnic diversity, the paper provides evidence of the the extent to which an established history of conflict is a prerequisite for ethnic diversity effects, the extent to which good institutions can mitigate the effects of ethnic diversity as well as the extent to which long run effects of ethnic diversity differ from shorter run effects. Additionally, since the Danish experience is very representative of the European experience in general, the paper speaks very directly to the question of what will happen to the large Western European welfare states in the face of increased inflows of immigrants and refugees.

The layout of the paper is as follows: Section 1 provides a theoretical discussion of the *ethnic*

diversity-public goods hypothesis and presents a simple theoretical model, section 2 surveys the existing evidence on the effect of ethnic diversity and discusses its shortcomings in relation to the effects of migration, section 3 presents the most important institutional details of the Danish context, section 4 describes the data and empirical strategy and discusses identification, section 5 presents and discusses the main results of the paper regarding the outcome of municipal elections and section 6 extends the simple theoretical model and presents and discusses the additional results regarding budgetary outcomes. Section 7 concludes.

1 Theoretical discussion

The literature on the *ethnic diversity-public goods hypothesis* starts from the assumption that there is a negative relationship between the level of ethnic diversity and the desired level of public goods at the individual level. There are several possible explanations for why such a negative relationship might exist:

Alesina et al. (1999) links ethnic diversity to greater disagreement about which *varieties* of public goods to produce, Miguel and Gugerty (2005) suggests that inability to impose social sanctions across ethnic groups lowers the social cost of not contributing to the public good and Vigdor (2002) assumes that individuals are altruistic and gain utility directly from other people's access to the public good but that this gain is smaller for people from other ethnic groups.⁹

As is fairly intuitive (and will be shown formally in a simple model below), all of these explanations lead to the prediction that public good demand decreases with ethnic diversity.

In terms of empirical applications of the *ethnic diversity-public goods hypothesis*, an important question arises in what one defines as a public good. The present paper follows the existing literature in adopting a relatively broad view of public goods.¹⁰ Since the vast majority of municipal spending in the Danish case is used to provide services that are available for virtually anyone (and thus have a clear public goods flavor), the present paper will thus largely equate public spending with public goods. An alternative way of viewing this approach is that focus of the paper is on the effect of ethnic diversity on *public spending* rather than public goods per se (although the

⁹An additional possibility, which to my knowledge has not been emphasized elsewhere, is that ethnic diversity affects lowers the preferred level of redistribution, and thus also the preferred level public goods if public goods imply redistribution (as is the case in most places).

¹⁰This is in opposition to the narrow "textbook" definition that public goods are strictly non rivalrous and non excludable.

term public goods will be used throughout).

However, as will be discussed further in section 3 below, it will be important to distinguish between the part of the municipal budget that is decided locally and the part of the municipal budget which is completely determined by national rules. Clearly, it is only the former part of the municipal budget that can be expected to respond to *local* ethnic diversity.

1.1 A standard model of the ethnic diversity-public goods hypothesis

This subsection presents a standard model of the *ethnic diversity-public goods hypothesis*. The model is similar to many of the models considered in the literature and in particular can be interpreted as capturing any of the negative links between ethnic diversity and preferences for public goods that were discussed above. Besides making precise what is understood by the *ethnic diversity-public goods hypothesis* in the present paper, a simple extension of the model will also be used to disentangle ethnic diversity effects from other budgetary effects when considering budget data in section 6.

Consider the following model: In a municipality there are two goods: a public good, which we will simply call public spending, and a private good, which we will call private spending. Public spending can be decomposed into a locally decided part, G , and a nationally mandated part, Γ , while private spending is homogeneous. The municipality itself receives a transfer, M , from the national level (and the other municipalities) and chooses the level of locally decided spending, G , and a level of total taxation revenue T , subject to the municipal budget constraint:

$$G + \Gamma = T + M \tag{1}$$

When considering empirical evidence from budgetary outcomes in section 6, identification of ethnic diversity effects is complicated exactly because nationally mandated expenditure Γ as well as transfers to the municipality M respond directly to ethnic diversity. For now however, we will treat both as fixed. The tax system is lumpsum with all individuals paying t , so with n people (n odd) in the municipality total tax revenue is just $T = nt$.

Locally controlled municipal spending (and due to budget balance thus also taxation) will be decided by a political process which results in a median voter type outcome. Let x denote

the private spending of whoever is the median voter in equilibrium¹¹ and assume this person has preferences over private spending and locally decided public spending, which depend on the level of ethnic diversity in the municipality θ and can be represented by:¹²

$$U(x, G; \theta) = u(x) + h(G; \theta) \tag{2}$$

$$u'_x > 0, \quad h'_G > 0 \quad u''_{xx} < 0 \quad h''_{GG} < 0 \quad h''_{G\theta}(G; \theta) \leq 0$$

The key thing to about the assumed preferences is that ethnic diversity is allowed to affect the utility of public spending and that the sign of $h''_{G\theta}(G; \theta)$ is non-positive, reflecting the idea that utility from the public good decreases with ethnic diversity.¹³ As discussed earlier, this assumption is what will generate the negative association between ethnic diversity and public good demand. By allowing for $h''_{G\theta}(G; \theta)$ to be zero I am thus allowing for the possibility that ethnic diversity might only have a negligible effect on marginal utility from the public good (in some settings). As will become clear this would imply that there is no association between ethnic diversity and the level of public goods.

A simple budget constraint for the median voter (who is assumed to have fixed income of Y) finishes the model:

$$x = Y - t \tag{3}$$

Solving the model simply involves solving the median voters maximization problem since he

¹¹It is somewhat unusual to impose assumptions on the behavior of the median voter, the identity of which would usually be determined in equilibrium. This is simply done with an expositional motivation, see footnote 13

¹²Obviously this imposes strong assumptions on preferences. In particular nationally mandated public spending do not convey any utility and preferences are additively separable in private and public spending. It will be important that the latter implies that both private and public spending are normal goods. The former assumption has no effect in the present section but imperfect substitutability between locally decided and nationally mandated public expenditure will be important in the extended model of section 6 but qualitatively similar implications obtains if the argument of $h(\cdot; \theta)$ instead was $G + (1 - \beta)\Gamma$, so that $\beta \in [0, 1)$ measures the extent to which Γ does *not* benefit the median voter. Since I will interpret changes in Γ as changes in immigrant and refugee related expenditures imperfect substitutability seems a reasonable assumption as long as the median voter is not from either of these groups.

¹³Depending on which of the underlying explanations for the negative sign of $h''_{G\theta}(G; \theta)$ one wants to entertain, different underlying assumption will imply that the median voter behaves as assumed in (2). The simplest such assumption is to assume that (2) is valid for all members of a homogenous group which is big enough to always contain the median voter (ie it is valid for the majority ethnic group). The effect of ethnic diversity θ on the utility from the public good can then either represents direct effects working through differential altruism (as in Vigdor (2004)) or indirectly through the effectiveness of social sanctions (as in Miguel and Gugerty (2005)). The model can also be interpreted as the reduced form of a model focusing on preferences for redistribution as well as heterogeneous preferences over varieties of public goods as in Alesina et al. (1999). In fact maximization of (2) (once the relevant budget constraint is added in), corresponds closely to equation (5) of Alesina et al. (1999).

decides the level of municipal spending (which from budget balance then pins down taxation and private spending). Thus the solution is obtained by maximizing (2) with respect to G subject to a "combined" budget constraint obtained from (1), (3), and the expression for total taxation. This yields an equation uniquely determining the equilibrium.¹⁴

Applying the implicit function theorem then yields the following comparative static result (for the case where transfers and nationally mandated expenditure does not respond to ethnic diversity):

$$\frac{\partial G}{\partial \theta} = c h''_{G\theta}(G; \theta) \quad , \quad \text{where} \quad c \equiv \frac{-1}{\underbrace{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}_{<0} + \underbrace{h''_{GG}(G; \theta)}_{<0}} > 0 \quad (4)$$

It is clear from (4) that if $h''_{G\theta}(G; \theta) < 0$, so marginal utility from the public good is (for one reason or another) decreasing with ethnic diversity, increases in ethnic diversity are associated with decreases in the level of (locally decided) public good. Since nationally mandated spending and transfers are fixed and the municipal budget must balance, an immediate corollary to the results is that taxation, T and t , as well as total municipal spending $G + \Gamma$ are also decreasing in ethnic diversity. This relationship, that decreasing net utility of public spending at the individual level leads to decreases in actual public spending following increases in ethnic diversity, is what the present paper refers to as the *ethnic diversity-public goods hypothesis*. It is worth noting that this relationship is independent of any effects of immigrant characteristics, such as whether they are high or low-skilled.

2 Existing evidence and its relevance for migration

The paper most often cited as the first to bring ethnic diversity to the attention of economists is Easterly and Levine (1997), which, as part of a larger study of determinants of growth in Africa, finds that ethnic diversity has hampered public goods provision (and because of this also hampered growth). Since then several cross-sectional, cross-country studies have reexamined the effects of ethnic diversity either directly or indirectly.¹⁵ Subject to the usual caveat of cross-country studies they all generally corroborate the finding that ethnic diversity correlates negatively with public

¹⁴Detailed derivations of the results presented in this section are given in Appendix D.

¹⁵See for example Easterly (2001), Alesina et al. (2001), Alesina et al. (2003), and Lassen (2007).

goods provision.

The second seminal paper in the literature is Alesina et al. (1999) which looks at a cross-section of US communities in 1990 and find that ethnic diversity is linked to less spending on a series of public goods. Following Alesina et al. (1999), several other celebrated studies have provided evidence that ethnic diversity is linked to lower levels of public goods using cross-sectional data: In developed countries, Vigdor (2004) examines the response rates to the 2000 US Census, since returning the census form can be seen as contributing to a local public good (because federal funds are allocated in part based on the headcount in the census).

In developing countries Miguel and Gugerty (2005) examine primary school funding and well maintenance in Kenya in the 1990s while using ethnic diversity as measured in 1961 to address endogeneity concerns. Banerjee et al. (2005) study the link between public goods and social diversity more generally using a cross-section of Indian districts in 1991 and also use a historical diversity measure based on caste and religion. Finally, Okten and Osili (2004) use Indonesian survey data on whether individual households contribute time and/or money to local voluntary public good projects in 1997-98.

2.1 The evidence on the effects of changes in ethnic diversity

In contrast to the strong cross-sectional evidence, longitudinal evidence on the effect of *changes* in ethnic diversity over time is severely lacking. In large part this seems to be due to the identification issues with simultaneity between outcomes and migration flows.¹⁶ Below I survey and discuss existing studies.

In terms of cross-country studies, Razin et al. (2002) and Mayr and Böheim (2005) explicitly examine the effects of immigration on the size of the public sector using a panel of EU and OECD countries, respectively, and both find a negative effect of immigration. Since the focus is on the effects of immigration more broadly, neither study attempts to carefully separate out the effect of ethnic diversity from the effects of other immigrant characteristics. Both studies do, however, treat high- and low-skill immigrants separately and do find evidence that the level of education of

¹⁶Obviously, simultaneity can be a problem in cross-sectional studies as well but is usually less pressing. In particular, endogenous location choice of immigrants can often be alleviated, either by examining whether results are being driven by the location choice of individual ethnic groups (Alesina et al. (1999)) or by using historical ethnicity data to predict current outcomes (Miguel and Gugerty (2005) and Banerjee et al. (2005)). In addition, endogeneity of migration flows is obviously always a bigger concern in longitudinal studies where the identifying variation is coming exclusively from people moving during the sample period.

the immigrants matters, in line with immigrant characteristics being the driver rather than ethnic diversity per se. Both papers are aware of potential endogeneity-issues, (in particular that the size of the public sector affects the tightness of immigration policy) but are only able to address them somewhat ad-hoc.¹⁷ Additionally, the "small-sample size"-critique of cross-country studies seems particularly worrisome with samples of only 11 and 18 countries respectively.¹⁸

In terms of within-country studies, Zwane and Sunding (2006) focus on the effect of the Californian Immigration Reform and Control act (IRCA) and the resulting increase in the number of immigrants in rural counties in California. Zwane and Sunding formulate an IV strategy based on the fact that IRCA should have a larger impact in counties with more labor intensive agricultural production, and finds evidence that increasing ethnic diversity has led to decreases in local public good provision. However, very severe problems with the strength of their instrument, undermines the credibility of their findings.

Independently of the current paper, Gerdes and Wadensjö (2010) and Gerdes (2009) have also used the panel of Danish municipalities to study the effect of migration on the size of the public sector.¹⁹ Their empirical strategy focuses on the Danish dispersal policy for refugees, which has been used elsewhere as a source of exogenous variation in the placement of individual refugees.²⁰ Gerdes and Wadensjö (2010) and Gerdes (2009) thus argue that the placement of non-western immigrants forms a natural experiment, making simple regression appropriate (conditional on some observable criteria used in enacting the dispersal policy). This approach seems inappropriate for two reasons: First, while newly-arrived refugees was a very important determinant of changes in ethnic diversity during the sample period, endogenous relocation of existing immigrant and the location choice of newly-arrived immigrants also contributed to changes in ethnic diversity. Second, even though the dispersal policy implied that placement of *individual immigrants* was random, this does not imply that at the municipal level the *total number of immigrants* received was random.²¹ As I discuss in section 3, immigrants in Denmark are generally left-leaning politically

¹⁷Razin et al. (2002) briefly discuss a robustness check using GDP- or growth-differentials between origin and destination countries as an instrument but question the validity of this approach themselves. Mayr and Böheim (2005) use 3SLS to allow for simultaneity but achieves identification using some fairly ad-hoc exclusion restrictions that are not discussed further.

¹⁸While obviously the time-dimension implies that actual sample size in estimations is much larger, the very high auto-correlation of the involved variables suggests that the "effective sample size" might not be. Neither study appears to have considered this issue when computing standard errors.

¹⁹I thank Arindrajit Dube and Lena Nekby for bringing these papers to my attention.

²⁰See for example the discussion of Damm (2009) in section 3.

²¹On the contrary, the aim of the dispersal policy was exactly to affect the aggregate distribution of immigrants

and actually tend to move away from right-wing dominated areas, which therefore makes Gerdes and Wadensjö's results particularly likely to suffer from the sort of feedback bias that I also find evidence of in my empirical analysis (i.e. immigrants moving in response to political outcomes). This can also explain the disparity between their conclusion of little-to-no evidence in favor of the *ethnic diversity-public goods hypothesis* and the findings of the present paper.²²

2.2 The relevance of existing results for the migration case

Given the substantial cross-sectional evidence of a negative effect of ethnic diversity on public goods provision, it seems tempting to conclude that changes in ethnic diversity stemming from migration must also lower public goods provision, even without direct evidence from settings where ethnic diversity is changing over time.

However, this simple extrapolation seems questionable. As discussed above, the *ethnic diversity-public goods hypothesis* starts from a negative relationship between ethnic diversity and individual net utility from public goods, and this relationship, in particular, seems likely to fail when ethnic diversity is increasing due to migration as is the case in Europe. In the language of the theoretical model above it could be that in these cases $ch''_{G\theta}(G; \theta) = 0$. This could happen for a number of reasons:

First of all, changes in ethnic diversity caused by migration are likely to involve inflows of new ethnic groups with which the existing ethnic group(s) have no prior history of conflict. This is in sharp contrast to the settings studied in most of the existing literature where the involved ethnic groups (whites and blacks in the US, castes in India, etc.) all have long established histories of conflict. Conversely in the European case, many of the immigrants and refugees come from countries with which the Europeans have had fairly limited interactions.²³

across municipalities to make it more equal among municipalities satisfying certain criteria. In the extreme case where all refugees are placed according to the policy and do not move subsequently, this implies that when controlling for policy criteria there should be no variation left in refugee placement. In practice, the variation in number of refugees one that is left with when controlling for the policy criteria is exactly discretionary deviations in implementing the policy, refugees moving away from their initial municipality or refugees not affected by the policy. This variation seems particularly likely to be non random.

²²Other differences between their studies and the present paper might also contribute to the differences in conclusions. Beyond differences in specification (and to some extent time period), they ignore the complications with changing mandatory expenditure or municipal transfers that I discuss in section 6. While their use of public spending data from the Danish law model in principle allows them to sidestep the direct effect of mandatory expenditures, the problem of budgetary income effects remain. At the same time, their public spending measure includes spending on items not pertaining to the municipal level (healthcare in particular), which seems inappropriate.

²³Again, if one goes back far enough in history this obviously ceases to be true for almost any ethnic group, see footnote 7

Second, as discussed for example by Easterly (2001), there is a clear notion in the literature that good institutions can mitigate ethnic conflict. Since a very large part of current migration is directed towards countries with very good institutions (as in Europe) this suggests that the effects of increased ethnic diversity may often be small. This is again in stark contrast to the settings studied in the existing literature, which generally involve very bad institutions (slavery in historical United States, the caste system in India, etc.)

Finally, there are several reasons why the long-run effects of ethnic diversity may be different than the effect of short-run changes in ethnic diversity. A simple example of why this might be is that political preferences might respond to changes in the environment with a lag. A more involved example is that there might be feedback effects from public spending to the level of (perceived) ethnic diversity²⁴ Such feedback effects could introduce tipping point dynamics, multiple equilibria and path dependence and imply that short-run effects could be both smaller or larger than long-run effects.

The aim of the present paper is to address the above concerns with the existing literature by providing direct evidence on the short-run effects of migration-related increases in ethnic diversity in a setting where the involved ethnic groups have no prior history of conflict and where institutions are of very high quality.

3 Institutional details of the Danish setting

Before presenting the empirical analysis, a few features of the Danish context will be important to understand. The nature and structure of different kinds of municipal spending and how it is financed will be important to understand when examining evidence from budgetary outcomes. When considering the assumptions necessary for identification, the sources of variation in migration and ethnic diversity across municipalities will also be important.

In Danish municipalities, the municipal budget is controlled by the municipal board, which is elected every four years. Immigrants and refugees can vote in these elections after 2-3 years.

For some parts of the municipal budget the level of expenditure is completely determined by national rules. Examples of this include the social income transfers. In general, expenditure in these areas are reimbursed one-for-one from the national level, exactly because the municipality

²⁴One example of this is that good public schools ensures a common language across ethnic groups.

has no influence on them. Obviously the level of total expenditure on these items will not be affected by local changes in the preferences due to ethnic diversity.

For other parts of the the municipal budget, municipalities have much more influence on the level of spending. They are still bound by the framework of national rules but can vary the quality of the provided services or the fee charged for the service. Two important examples of this are the public schools and the public daycares. For the former the municipality have significant room for varying the quality of education, for example by providing more teachers per student. For the latter, municipalities can vary the level of the (below cost) fee that is charged for daycare. Importantly however, the municipalities generally can not influence who is eligible to benefit from the expenditure since national rules imply that access to most expenditure is "universal". For public schools and daycares for example all children in the relevant age groups are eligible for a spot in a public school or daycare.

For the parts of the budget where municipalities influence the level of spending, municipal expenditures are not reimbursed and the municipalities must finance net expenditures themselves (net here refers to the fact that in some cases the expenditure is partially covered by charging a fee). It is net expenditures on these latter items that one would expect to decrease in response to higher ethnic diversity. Correspondingly it is these net expenditures that are used in the empirical analysis of budget data.

The distinction between nationally mandated and locally decided expenditure, where only the former is reimbursed, is not a complete dichotomy, however. In particular, there are some expenditure items, over which the local municipality has very little influence but which are not reimbursed. Unfortunately this group includes some expenditures that are directly related to immigrants and refugees, which implies that the expenditure measure used in the empirical analysis will contain expenditure items that automatically increase in response to more immigrants and refugees (examples include "second language" classes in public schools and introduction programs for newly arrived refugees). As I discuss further in section 6 this complicates the interpretation of the results from budgetary data, and implies that a theoretical model is needed to disentangle ethnic diversity effects from the direct budgetary effects of having more immigrants and refugees.

Municipal borrowing is severely limited by law, so municipalities essentially face a budget balance requirement. In terms of financing the net expenditures which are not directly reimbursed,

municipalities mainly rely on revenue from the locally set income and land tax. In addition they rely on transfers received from the national level (or other municipalities) as part of a system of "equalizing" transfers. This system is meant to compensate municipalities for differences in tax base and "expenditure needs" (as computed from a set of relevant municipal indicators). Because of the existence of expenditure items that increase automatically with immigrants and refugees, however, the computed expenditure need (and thus the transfer to the municipality) increases directly with the number of immigrants and refugees. This adds an additional complication to the interpretation of the results from budgetary data as is discussed in section 6.

3.1 Sources of variation in migration and ethnic diversity

Since 1973 Denmark has actually had an "immigration stop" policy in effect, which as a rule of thumb has prevented new immigration. Three channels have however continued the migration-related increase in ethnic diversity over the period 1981-2001: Higher fertility among existing immigrants, the possibility for existing immigrants to bring family members to Denmark under the reunification rules,²⁵ and increasing refugee flows.

In terms of the location choice of immigrants, the main determinant of where new immigrants from the fertility and reunification channel locate will obviously be the existing stock of immigrants (since kids/spouses tend to live with their parents/significant other).

For refugees, Damm (2009) has studied their location choice in detail. She uses the fact that the Danish refugee placement policy between 1986-1999 implied that initial placement of most refugees was random (conditional on a set of observables)²⁶ and then uses subsequent relocation decisions to identify determinants of location choice. Her results show that refugees move *away* from areas:

1. With few other immigrants or refugees
2. That are rural
3. With high unemployment

²⁵Since many of the families being reunified are newly formed families, the english term *reunification* is somewhat of a misnomer.

²⁶As discussed in section 2 (footnote 21 in particular), the refugee placement policy does not seem like a good source of variation in the total number of refugees across municipalities, despite its usefulness in generating random variation in placement at the individual level. Thus it does not seem appropriate to rely on the policy for identification in the present paper.

4. With little rental (and social) housing
5. Without institutions for qualifying education
6. That is dominated by the right-wing politically.

A few remarks on these findings are in order: The first bullet point implies that (as with the fertility and reunification channel) the existing stock of immigrants and refugees should be an important predictor of later inflows and thus might be an important control in the empirical analysis because initial settlement patterns of immigrants could be proxying for a host of possible confounders. Similarly points two and three suggest two additional controls for the empirical analysis.

The third point reflects the fact that by law non Nordic citizens are unable to own real estate until they have lived in Denmark for at least five years, making availability of rental housing a main determinant of refugee settlement. This fact will form the basis for the IV strategy discussed in section 4 below.

Finally points five and six imply that one might have to worry about feedback effects in the empirical analysis if refugees are likely to move away from areas cutting back on educational efforts or even more worryingly: move in response to changes the political climate. The IV strategy developed below will be key in assessing and addressing these potential feedback concerns.

4 Empirical strategy

The empirical strategy of the present paper involves estimating dif-in-dif type regressions of changes in election and budgetary outcomes on changes in ethnic diversity. Adding the possibility of including a vector of controls, X_i the basic estimating equation is thus:

$$\Delta(\text{Outcome of interest})_i = \beta_0 + \beta_1 \Delta(\text{Ethnic diversity})_i + X_i' \gamma + \varepsilon_i \quad (5)$$

Here Δ denotes the change between some some base and end years t_0 and t_1 (in most cases $t_0 = 1981$ $t_1 = 2001$) and i indexes the 273 municipalities included in the analysis. The parameter of interest, β_1 , is then the effect of changes in ethnic diversity on the outcome of interest over

the considered time horizon. Intuitively, the empirical analysis thus estimates the effect of ethnic diversity by comparing changes in outcomes over the period in municipalities that experienced large increases in ethnic diversity versus municipalities that did not.

The use of a "long difference" specification involving only the change between two time periods allows for flexibility in the exact timing of effects.²⁷ Given the very high autocorrelation of the involved variables, the long difference approach also sidesteps issues with estimating standard errors in the face of autocorrelation.²⁸

4.1 Data sources, definitions and descriptive statistics

The municipal level data for my empirical analysis are all based on the individual level registry data from Statistics Denmark (SD).²⁹

Out of the 275 municipalities, I drop the two large Copenhagen and Frederiksberg municipalities since these functioned also as counties at the time and thus had a markedly different set of tasks (in particular public health). This leaves me with a sample of 273 municipalities.

For the data on seats won in municipal elections, I group parties into left, right, anti-immigrant nationalists and "others". I define the leftist group by letting it include the obvious far left-wing parties (e.g. communist parties) plus all parties that were part of any of the national left-wing coalition governments serving between 1993-2001 but *was not* part of any the national right-wing coalition governments serving between 1982-1993. I let the nationalists group consist of the *Fremskridtspartiet* party and its later splinter fraction *Dansk Folkeparti* party and then define the right-wing group as the natural opposite of the leftist group but *not* including the nationalists parties. With these three groups defined the "other" group thus consists of the most centrist parties and any party that were not obviously placeable as right or left wing at the national level.³⁰

²⁷As discussed earlier, one part of the motivation for the empirical analysis undertaken here is the possibility that long and short run effects of ethnic diversity are different. In particular the brief discussion earlier in the paper suggests that the outcomes of interest might be governed by a more complicated process involving lags, non-linearities and feedback between (perceived) ethnic diversity and the outcomes of interest. In light of this, (5) should be viewed as a "reduced form" approximation of the relationship between migration-related increases in ethnic diversity and changes in the outcomes of interest over a time horizon like the one considered (20 years).

²⁸The approach is in the spirit of what Bertrand et al. (2004) call "ignoring time series information". Although there is a loss of information in focusing only on two periods, this is likely small because of the near non stationarity of both policy outcomes and stock of immigrants.

²⁹Most of the variables I use are from the publicly available *Statistikbanken* database, which can be found online at <http://www.statistikbanken.dk> although a few variables are computed directly from the individual-level data (average taxable income, Gini coefficients and the fraction of the population receiving social income transfers).

³⁰A few points about the grouping resulting from this methodology are worth mentioning. First of all, the rule requiring parties to *only* have been part of one kind of government in order to classify as either left or right wing

The municipal expenditure data I use are current expenditures and is net of government reimbursements (as discussed in section 3) and fees charged for services such as daycare and utility provision. For all the monetary variables I denominate the amounts in 1,000 kr. and deflate to 1980 prices using the (chain-linked) GDP deflator from the Danish national account.

For the historical housing stock data used in the IV strategy (discussed below), the use of the term "1970 housing stock" is actually a slight misnomer since the variable in fact only includes housing constructed in 1970 and earlier *that is still standing* in the current year, which explains why this variable changes between the 1981 and 2001 cross-sections as is shown in the descriptive statistics below.³¹

The changes in ethnic diversity in my sample are being driven by the initially ethnically homogenous Danish population becoming less homogenous as more and "non Danish" people arrive. Thus as my measure of ethnic diversity I will simply use the percentage of people not of Danish origin (NDO), where I am using SD's official definition of Danish origin as requiring one parent to be both born in Denmark and have a Danish citizenship.³²

Finally, tables 1, 2 and 3 present descriptive statistics for the main variables for the 1981 and 2001 cross-sections as well as for the changes between 1981 and 2001.

(Tables 1, 2 and 3 about here)

The tables clearly show the overall increase in the number of people not of Danish origin, with the mean percentage of NDOs going from 1.81 to 4.41 between 1981 and 2001. They also show the significant variation across municipalities in the number of NDOs received with changes in the percentage NDOs over the period ranging from 0.63 to 15.59 with a standard error of 1.87.

The size of the Danish public sector is reflected in the high mean municipal land and income tax rates which for the 2001 cross-sections are 1.31 and 20.88 percent respectively. For the expenditure

implies that the fairly centrist *Radikale Venstre* party is included in the left group, while two other centrist parties, *Kristendemokraterne* and *Centrumdemokraterne* end up in the "other" group. Further, the definition implies that the right group actually only consists of the two very "core" right wing parties *De Konservative* and *Venstre*, while the left group contains many more parties.

³¹For the main IV approach I will only be using the earliest available year, which is 1981, thus missing any housing constructed before 1970 and torn down between 1970 and 1981.

³²This measure of ethnic diversity differs from standard Herfindahl-type fractionalization index used in the literature. Since the ethnically Danish group is such a large majority in all municipalities, however, the fractionalization index is virtually indistinguishable (beyond a scale factor) from simply the percentage of NDOs. This verified empirically in figure 1 at the back, which plots the Herfindahl measure against the percentage NDOs and shows a clear linear relationship. For transparency I therefore focus simply on the percentage of NDOs and note that in comparing my estimated effects with those of the literature they should be scaled down by about 1.8.

measure I focus on, the mean level of spending per capita in 2001 was 13,050 kroner relative to a mean income in the population of 71,870 kroner in that year.

Finally, as further motivation for questioning whether migration-related increases in ethnic diversity is in fact negatively affecting the level of public goods, it is worth noting that despite the significant increase in the percentage of NDOs in Denmark, both the mean income and land tax rate have actually *increased* over the period.

4.2 Identification issues

Identification of the parameter of interest in (5) will rest on the error term being unrelated to the ethnic diversity measure (once relevant controls are included). There are several concerns with such an assumption:

One concern is that NDOs tend to settle in municipalities with particular characteristics (as was discussed in section 3 above) and that these municipality characteristics might affect election and budgetary outcomes directly, thus causing an omitted variable bias. Since the specification in (5) differences out municipality fixed effects, this concern is alleviated somewhat. The possibility, for example, that NDOs all move to inherently left-wing municipalities does not cause a bias.

On the other hand it does cause a bias if NDOs sort on municipality characteristics which affect the later *change* in election or budgetary outcomes since this is not picked up by a municipality fixed effect. The main issues here seem to be the possibility of mean reversion in political outcomes and differential trends between rural and urban municipalities (since as noted NDOs are much more likely to move to urban areas). I address these concerns by including variables measuring initial characteristics of the municipality in the control vector.

Another concern stems from the fact that higher ethnic diversity in the sample is directly related to having more NDOs, which might have a direct impact on election and budgetary outcomes. Particularly one worries that since NDOs tend to be poorer, have higher unemployment rates and lower labor force participation, the separate effects of lower income, higher unemployment and lower participation will bias estimates.³³ I address this concern by controlling in (5) for changes in a rich set of socioeconomic indicators, including mean income, fraction not in the workforce, unemployment rate, Gini coefficient, and fraction receiving income transfers.

³³Such a direct effect of having more "low-skilled" immigrants has been proposed and documented in the immigration literature, see for example the cited studies by Razin et al. (2002) and Mayr and Böheim (2005).

The fact that NDOs vote in municipal elections is another direct channel through which more NDOs can affect election and budgetary outcomes directly. This problem can not be addressed by including additional controls or by the IV strategy below, however in practice the resulting bias in estimates should be very small.³⁴ Additionally, since NDOs are generally left-leaning politically the direction of the bias actually goes in the direction of finding a *positive* association between percentage NDOs and left-wing political outcomes (such as higher taxes and more public expenditure).³⁵

Finally, the possibility of feedback from election and budgetary outcomes to the endogenous location decision of NDOs seems a major concern. As discussed in section 3 NDOs tend to move away from areas with right-wing political domination, so a particular concern is that NDOs respond to right-ward shifts in election and budgetary outcomes by moving to a different municipality. This could cause a very significant bias in the direction of finding a *positive* association between percentage NDOs and left-wing political outcomes (such as higher taxes and more public expenditure). The next section develops an IV strategy, which will be used to address this feedback bias.

4.3 The IV strategy

As mentioned in section 3, the fact that non Nordic citizens can not own real estate during their first five years in Denmark implies that availability of rental housing is a major predictor of the location choice of (especially newly arrived) NDOs. This suggests using the initial amount of rental housing in a municipality as an instrument for the change in percentage NDOs. Identification would then rest on the exclusion restriction that any association between initial rental housing and changes in the outcomes of interest is exclusively due to changes in the percentage NDOs.

Because property owners are fairly free to choose between renting out their property and selling the property to would-be occupants, however, this exclusion restriction seems likely to fail. The most obvious problem is that the share of rental housing could respond to expectations about

³⁴To see why this is the case note that if all NDOs vote for party X, if *no-one* else does so and if the turnout of NDOs is the same as for everyone else, then an increase in the percentage NDOs of one percentage point will increase the vote share of party X by exactly one percentage point. This, however, is an upper bound on the effect and since NDOs do not vote completely orthogonal to the rest of population the effect will be significantly smaller. Additionally, the fact that turnout in elections is much lower among NDOs further decreases the potential effect.

³⁵By a similar token, the possibility that a specific type of people of Danish origin move out when NDOs move in, would also seem to unambiguously bias the results towards finding a positive association between percentage NDOs and left-wing political outcomes, because of the association of anti-immigrant sentiments with the political right.

future political outcomes and how they will affect the profitability of renting versus owning.

To circumvent the problems with using actual rental housing as an instrument I will instead utilize the fact that detailed data is available on the historical housing stock and the fact that (for obvious reasons) housing stock characteristics are very persistent over time. Combining this with the fact that (probably in part due to tradition and in part due to the segment of the housing market they cater to) highrise buildings are much more likely to serve as rental housing, suggests using the historical prevalence of highrise buildings as an instrument. Thus the IV strategy will entail using the share of the 1970 housing stock that is comprised by highrise homes as an instrument for later changes in the percentage NDOs (with the association working through the availability of rental housing during the sample period 1981-2001). Figure 2 illustrates the principle behind the IV strategy.

(Figure 2 about here)

Using the historical housing stock as the instrument makes the exclusion restriction much more plausible especially due to the introduced time distance between the period of interest (1981-2001) and the dating of the instrument (1970, which in particular is before the inflow of NDOs to Denmark had taken off). For example, one concern with using highrise buildings as an instrument is that highrise buildings attract a particular kind of population to a municipality. However, the exclusion restriction is that the 1970 housing stock can not have an effect on the *change* in outcomes over a period starting *10 years later* so this seems unlikely to be a problem: Any effects on the population composition of having more highrises in 1970 should already be reflected in the 1981 level of the outcome variable and thus should difference out.

Some concerns with the instruments remain, however. One is that the number of highrises might simply proxy for how urban a municipality and only predicts NDO settlement because NDOs were more likely to settle in urban areas. In table 4 below I show that this is not the case. Another concern is that there might be mean reversion in the instrument implying that a large share of highrises in 1970 might imply larger increases in the share of other kinds of housing and thus attract different kinds of people. There are indications of modest mean reversion in highrise share in the data and thus in the empirical analysis I probe the robustness of the results to controlling for later changes in the housing stock composition.

As always with just-identified IV, the exclusion restriction discussed above is untestable. The hypothesized relationship between variables shown in figure 2, however, can be examined. Table 4 shows the result of such an examination. The first two columns examines the hypothesized relationship between the historical housing stock and initial share of rental housing by regressing rental housing in 1981 on highrises in 1970 and shows the expected positive and significant relationship.³⁶ Column two adds in controls for initial population size and density to address the concern that the relationships between the variables is just working through urban/rural differences. The robustness of the positive relationship to the added controls shows that this is not the case.

(Table 4 about here)

Columns three and four examine the relationship between the change in percentage NDOs and initial rental housing. The expected positive relationship is found and is significant also when the size and density controls are included. Column five includes the instrument in this regression and its slightly negative and insignificant estimate shows that once the initial rental share is accounted for, the 1970 housing stock does not predict changes in percentage NDOs.

Finally columns six and seven correspond to "first stages" of the IV-estimation by regressing change in percentage NDOs on the highrise instrument. The positive and significant relationship shows that the instrument satisfies the rank condition, also when population size and density are included as controls. The first stage F-stats for the "weak instrument rule of thumb" are included at the bottom and in both cases there are no indication of weak instruments since the F-stat is above 10 (although, as is evidenced by the large drop in the F-stat between the two columns, the inclusion of controls does significantly reduce the power of the instrument).

5 Evidence from municipal elections

This section presents the main empirical results of the paper, regarding the outcomes of municipal elections. If the *ethnic diversity-public goods hypothesis* applies in the Danish case and increases in ethnic diversity has indeed lowered public good demand, voters should have responded to increased

³⁶Note that the the percentage of housing that is rental is measured as a percentage of total occupied housing while the percentage of the 1970 housing stock that is comprised by highrises is measured slightly differently as the percentage of the *total* number of homes (occupied and unoccupied).

ethnic diversity by shifting their votes rightward, towards political parties favoring a lower level of public goods. This prediction is tested in the current section.

Using outcomes of municipal elections as a "test" of the *ethnic diversity-public goods hypothesis* comes with two caveats. The first one is that by focusing on party-level success, any effects working via changes in the platforms *within* parties are missed. In terms of the magnitude of shifts in actual political platforms of elected candidates, estimates in this section are thus likely to be lower bounds.

The second caveat is that placing the myriad of Danish parties on a one-dimensional left-right political axis is subject to some measurement error. The right, left, nationalist and other group definitions I have opted for (and which are spelled out in the data section above) attempt to address this by defining the left and right groups very narrowly, while treating the anti-immigrant nationalist parties as a separate group from the rest of the right. The separate treatment of the anti-immigrant nationalist is done in part because of the particular interest in the anti-immigrant part of their political platform and in part due to the potential difficulties in placing their political platform firmly on a left-right axis.³⁷ With this caveat in mind however, I will interpret increased political success for anti-immigrant nationals as a rightward shift in political attitudes throughout the rest of the analysis.

5.1 Results using OLS

Table 5 reports estimates of the effect of changes in fraction NDOs on the changes in the fraction of seats won by the different political groups between the 1981 and 2001 municipal elections. The estimates were obtained by OLS estimation of the basic equation (5). Each row of the table corresponds to the estimated effect on a different political group, while columns correspond to different empirical specifications and thus probe the robustness of the results to including different controls.

(Table 5 about here)

³⁷The nationalist *Dansk Folkeparti* party has on many occasions (at least in their rhetoric) branded themselves as working to secure the welfare state and have in particular promoted public programs targeted at the elderly. On the other hand they have been very critical of any kind of public spending, which has (seemingly) disproportionately targeted NDOs even slightly. In addition they have at the national level quite clearly worked with and supported the right-wing coalition government serving since 2001, which has very much worked towards lower taxes and a smaller public sector.

The first column of the table shows the raw estimates without controls, which suggest that an increase in the fraction NDOs of one percentage point causes a drop in the percentage of seats held by the left and right groups of 0.687 and 1.120 respectively and an increase in the percentage of seats held by nationalists and the other group of 0.920 and 0.888. All of these effects are statistically significant (at the 5 percent level). We see however, that in the second column once the changes in the set of social indicators³⁸ are added as controls, only the positive effect for the nationalists remains (but is halved) while all the other significant effects go away. This suggests that most of the strong raw correlations were driven by other socioeconomic factors.

The third column instead controls for the initial (1981) population size and density to address the concerns with NDOs being more likely to settle in big urban municipalities. Compared to column one we see that this decreases the magnitude of the effect for the left group while leaving the other estimates relatively unchanged (the loss of precision due to the added control however makes the effect for the "other" group become only marginally significant).

The fourth column adds in the change in the total population to control for population size effects, while also including the "baseline controls" (changes in socioeconomic indicators and initial population size and density). This causes only relatively minor changes. Column five adds in the initial percentage NDOs since these are known to be highly correlated with later increases in percentage NDOs and could also have an effect on later changes in voting behavior. We see however, that estimates change relatively little, with the right group having a significant negative effect of around one percentage point and the nationalists a positive effect of around half a percentage point.

Column six tries to address concerns with mean reversion in the outcome variables by including as a control the initial level of the outcome variable. This has only a slight effect when the "baseline" controls are included. As exemplified by the results in column seven, however, when the baseline controls are dropped, inclusion of the lagged outcome does tend to have an important impact and in particular increases the positive effect for the left wing (to the point that in column seven it is actually highly significant) as well as the negative effect for the right wing. Thus there is some evidence that mean reversion in political outcomes may account for some of the estimated effects.

³⁸The set of indicators include mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers

Mainly out of completeness and for comparison with the IV estimates below, column eight adds the change in the highrise share of the housing stock to the baseline controls. This changes estimates fairly little.

Overall the OLS estimates (taken at face value) suggest that an increase in ethnic diversity corresponding to a one percentage point increase in the percentage NDOs has a sizeable and significant negative impact on the percentage of seats held by the political right of around one percentage point, while it has a significant positive effect on the percentage of seats held by nationalists of around half a percentage point. For the left and the "other" group results are less clear, with results indicating no to a slight positive effect for both.³⁹

5.2 Results using IV

As discussed above, the possibility that NDOs move in response to election outcomes implies that there might be a feedback bias in the OLS estimates presented above. In particular the fact that refugees prefer *not* to live in right-wing dominated municipalities suggests that NDOs might be moving away from municipalities where the right wing gains politically. This would cause a negative bias in the estimated effect on right-wing seats (and vice versa for the left-wing). To address this issue I thus rerun the regressions of the above section using the highrise share of the 1970 housing stock as an instrument. Table 6 shows the results.

(Table 6 about here)

Looking at the table overall, the results accord well with what could be expected. Although the instrument does predict changes in the percentage NDOs very well (as evidenced by the high first stage F-stat in the first column), its power quickly drops once more controls are added (especially the different initial values). As a result, while F-stats in all but one specification (Column 5) pass the weak instrument rule of thumb of being above 10, the estimated standard errors quickly become fairly large when controls are added.

In terms of the actual estimates, the difference between OLS and IV are also very much in line with expectations given the discussion of feedback bias. The no to slightly positive effect for the left wing suggested by the OLS estimates is generally replaced by large negative effects (although

³⁹As shown in appendix C, these conclusions are robust to considering the shorter time period 1989-2001 and does not seem to be driven by potential outliers.

the relatively large standard errors imply that the estimates are only marginally significant or insignificant in many specifications). The negative and significant effects for the right-wing are replaced by insignificant estimates in all but one specification and the positive effect of around half a percent for the nationalist are replaced by larger estimates in all specifications (although again, larger standard errors imply that significance is lowered in many cases). For the other group estimates are very unstable, varying between large and positive and actually moderately negative, with standard errors also being large. This likely reflects the heterogeneity of the "other" group, which lumps together very different political parties.

Looking more closely at the IV estimates, a couple of additional points are worth noting:

While IV estimates of the effect for the right wing generally are smaller in magnitude than the OLS estimates, the difference is not very big and the lack of significance is mostly due to the larger standard errors. A definite conclusion that there is no negative effect for the right group once feedback bias is accounted for thus seems premature.

Further, looking at columns six and seven where the initial level of the outcome is included, there again seems to be evidence that at least part of the effects for the left and right group can be accounted for by mean reversion. In column six the estimated effect for the left group drops to be only slightly negative and insignificant and, as was the case for the OLS estimates, this tendency is even more pronounced when the "baseline" controls are dropped in column seven.

Finally, In column eight changes in the highrise share of the housing stock 1981-2001 is included to probe for problems of mean reversion in the housing stock. The inclusion of this control is seen to have little effect on estimates although increases in the estimated standard errors make all the results insignificant.

Summing up, while the large standard errors and fairly unstable estimates obtained using IV suggests caution is warranted, the estimates are very much consistent with an interpretation that the OLS estimates do suffer from "anti right-wing" feedback bias and that, in fact, increases in the fraction NDOs decreased the popularity of left wing parties, increased the popularity of nationalists very markedly and had no to a somewhat negative effect on the popularity of the right wing parties.⁴⁰ The finding of a fairly clear right-ward shift in election outcomes when using IV

⁴⁰As for the OLS estimates, appendix C show that these conclusions also seem robust to considering the shorter time period 1989-2001. To preserve power in the instrument for the short sample period I have to use 1980 housing stock data however, which makes the exclusion restriction somewhat less plausible.

suggests that increased ethnic diversity has indeed caused people to vote for parties favoring lower public spending, as is predicted by the *ethnic diversity-public goods hypothesis*.⁴¹

6 Evidence from budgetary outcomes

While the results above for the outcome of municipal elections seem convincing, this section attempts to substantiate the evidence by examining the effect of changes in ethnic diversity on actual budgetary outcomes. This in particular addresses the concern that seemingly right-ward shifts in election outcomes (in particular towards the nationalist group) might not translate into corresponding changes in budgetary outcomes.

As mentioned briefly in section 3, however, interpretation of estimates based on budgetary outcomes is complicated by the existence of nationally mandated but non reimbursed expenditures increasing automatically with the number of NDOs and by the fact that the size of the transfer to the municipality also responds directly to the number of NDOs. For expenditure outcomes, this implies that local expenditures might be inflated by NDO-related expenditures which are beyond the influence of the municipality. For both expenditure and taxation outcomes, it implies that the effect of changes in number of NDOs are confounded by income effects from the joint effect of higher mandatory NDO expenditures and higher transfers.

To disentangle ethnic diversity effects from these budgetary effects, I first extend the simple model of section 1 to include transfers and nationally mandated expenditure that responds to ethnic diversity. Besides illustrating the issues, the model is used to discuss the restrictions placed on the data under different "auxiliary" assumptions about mandatory expenditure and transfers and the effect of ethnic diversity. I then present the corresponding empirical results and discuss which conclusions can be drawn under different auxiliary assumptions.

6.1 A model with responsive mandatory expenditure and transfers

In the simple model of section 1, we saw that when nationally mandated expenditure Γ and transfers to the municipality M were fixed, the *ethnic diversity-public goods hypothesis* implied that total municipal spending and taxation should both decrease with ethnic diversity.

⁴¹As discussed in section 4 the effect on election outcomes could in principle just be caused by NDOs voting differently than the rest of the population. However, the suggested size of the effect and especially the clear positive effect on the popularity of the anti-immigrant nationalist can not be reconciled simply with NDOs voting.

In reality however, transfers to the municipality and nationally mandated expenditures do respond quite significantly to changes in ethnic diversity (as measured by percentage NDOs).⁴² Thus the relevant case for the empirical analysis is to allow for both transfers and nationally mandated spending to increase with ethnic diversity, that is to assume $\frac{\partial \Gamma}{\partial \theta} > 0$ and $\frac{\partial M}{\partial \theta} > 0$.

Allowing transfers and mandatory expenditure to vary with ethnic diversity does not change the maximization problem for the median voter since he takes ethnic diversity as given. The same equation as before still uniquely characterizes the equilibrium. Applying the implicit function theorem however now instead yields:⁴³

$$\frac{\partial G}{\partial \theta} = c h''_{G\theta}(G; \theta) + \nu \left(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta} \right) \quad , \quad \text{where} \quad (6)$$

$$c \equiv \frac{-1}{\underbrace{u''_{xx}(Y - k(G + \Gamma - M))}_{<0}} + \underbrace{h''_{GG}(G; \theta)}_{<0} > 0 \quad \nu \equiv \frac{1}{1 + \underbrace{\frac{h''_{GG}(G; \theta)}{u''_{xx}(Y - k(G + \Gamma - M))}}_{>0}} \in (0, 1)$$

Comparing (6) with (4) we see that in addition to the first term corresponding to the effect of ethnic diversity on the marginal utility of public spending, $\frac{\partial G}{\partial \theta}$ now has a second term whose effect is proportional to $\left(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta} \right)$.

The intuition behind this second term is that it measures the income effect stemming from the change in net municipal income when both the mandatory expenditure Γ and the transfer M changes in response to higher ethnic diversity. When $\left(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta} \right)$ is positive, the increase in transfers following higher ethnic diversity is larger than the increase in mandatory expenditure and thus the municipality experiences a positive income "shock". Since locally decided public expenditure is a normal good, this results in higher locally decided public expenditure G . Conversely if the increase in transfers following higher ethnic diversity is smaller than the increase in mandatory expenditure the municipality experiences a negative income shock and G goes down.

Using the expression for $\frac{\partial G}{\partial \theta}$ it is straightforward to find the effect of ethnic diversity on taxation, $\frac{\partial T}{\partial \theta}$, and total spending, $\frac{\partial(G+\Gamma)}{\partial \theta}$. For taxation we get:

$$\frac{\partial T}{\partial \theta} = c h''_{G\theta}(G; \theta) + (\nu - 1) \left(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta} \right) \quad (7)$$

⁴²This was discussed in section 3 and for transfers is also verified empirically in Appendix C.

⁴³Again, detailed derivations are shown in appendix D

Looking at (7), we see that the income effect is again proportional to $(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta})$ however since $(\nu - 1) < 0$, now with a negative constant of proportionality, implying that the income effect on total taxation T is of opposite sign of the effect on G . The intuition behind this taxation result is straightforward since taxation is simply absence of private spending and private spending is a normal good.

For total spending on the other hand, we get:

$$\frac{\partial(G + \Gamma)}{\partial \theta} = c h''_{G\theta}(G; \theta) + \nu \left(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta} \right) + \frac{\partial \Gamma}{\partial \theta} \quad (8)$$

$$= c h''_{G\theta}(G; \theta) + \nu \frac{\partial M}{\partial \theta} + (1 - \nu) \frac{\partial \Gamma}{\partial \theta} \quad (9)$$

The effect on total spending is most easily interpreted by looking at (8), where in addition to the by now familiar first and second term corresponding to the marginal utility of expenditure and the income effect, there is a now a third term measuring the direct of effect of increases in nationally mandated expenditure. Further, since $0 < \nu < 1$ it follows from (9) that the sum of the direct and the income effect is unambiguously positive.⁴⁴

6.2 Empirical implications

The extended model above shows how the institutional features of the Danish context imply that as ethnic diversity increases total public spending in municipalities are affected by two budgetary effects in addition to the negative effect suggested by the *ethnic diversity-public goods hypothesis*: Increases in nationally mandated expenditure pushes total spending up, while the net income effects from this expenditure increase and the corresponding increase in transfers have an ambiguous effect. Given that the aim of the present paper is to examine whether the *ethnic diversity-public goods hypothesis* applies to the Danish case, the aim of the empirical analysis of budget data will be to determine the independent effect of ethnic diversity on the "underlying public good demand" - that is the public good demand that would have prevailed in the absence of the two budgetary effects. In the language of the model we are interested in examining $c h''_{G\theta}(G; \theta)$.

As is clear from equations (6), (7) and (9) the conclusions that can be drawn regarding this

⁴⁴The intuition here is that while a negative net income shock stemming from higher mandatory expenditure might cause the locally decided part of municipal expenditure to go down, it does so less than one-for-one implying that total expenditure must increase (in the absence of a direct effect of ethnic diversity).

underlying public good demand from any given set of estimated relationships in the data depends crucially on the sign of $(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta})$ that is on whether the combined effect of changes in transfers and mandatory expenditure following changes in ethnic diversity is that of a positive or negative income shock.

While the sign of the income effect can not be credible established in the available data, two things suggest that a negative net income effect is more plausible. First of all, while the system and formulas involved in computing the size of the transfers to municipalities are fairly complicated, they generally involve computing an "expenditure need" for each municipality and then letting the transfer correspond to a fixed *fraction* of the deviation between this measure and the average "expenditure need" across the entire country. This rhetoric strongly suggests that the transfers only partially covers differences in expenditures and thus suggests that the negative income effect is more plausible.

Second, the strong opposition of municipalities to receiving NDOs, which has in large part been based on budgetary arguments, also seems hard to reconcile with a positive income effect.

On the other hand however, it cannot be ruled out that municipalities are in fact being overcompensated for the NDOs they receive (and thus that the net income effect is positive) but still object to receiving more NDOs because their presence adversely affects the utility of existing inhabitants. In interpreting the empirical results I will thus consider which conclusions can be drawn both under the assumption of a net negative income effect as well as under a net positive income effect. For this purpose, table 7 summarizes the restrictions placed on the data under the two different cases of the effect on underlying public good demand (i.e. whether $c h''_{G\theta}$ is negative and thus that ethnic diversity *in itself* lowers public good demand) and under the two different cases for the auxiliary assumption (i.e. the sign of the net income effect $(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta})$)

(Table 7 about here)

Starting with the first row of the table, that is the case where increases in NDOs exert a negative net income effect on the municipal budget, we see that observing a negative effect of percentage NDOs on either total public spending and/or taxation would allow us to conclude that there is a negative effect of ethnic diversity on underlying public good demand (i.e. $c h''_{G\theta} < 0$) since both of these results contradict the predictions in the upper left corner (but not the predictions in the

upper right corner). Conversely we see that under this auxiliary assumption no findings in the data will allow us to conclude that ethnic diversity *does not* affect public good demand since no restrictions are placed on the data in the upper right corner of the table.

Looking next at the second row of the table, corresponding to the positive income effect case, similar reasoning yields that observing a negative effect on total spending implies that ethnic diversity *does* have a negative effect on public goods. Under this auxiliary assumption however, no other result in the data would allow us to draw any conclusions.

6.3 Empirical results

I now examine the relationship between changes in percentage NDOs and changes in taxation and expenditure. Tables 8 and 9 present results similar to the ones presented in tables 5 and 6 for municipal elections.

(Table 8 about here)

Looking at the OLS estimates in Table 8 we see that all specifications suggest a negative effect of percentage NDOs on both tax rates but a positive effect on expenditure per capita. For the tax rates the negative effects range from about -0.1 to -0.3 percentage points for the income tax and about -0.05 to -0.15 percentage points for the land tax depending on specifications. For expenditure per capita the estimated effects range from about 125 to 225 kr. per capita. Except for the income tax estimate in column, 2 all of the estimates are highly significant.

Despite the robustness of the results in table 8 to the included controls, the possibility of feedback bias is a concern for estimates regarding budgetary outcomes as it was for the estimates involving municipal elections. In the present case one particularly worries that NDOs respond to decreases in tax rates and spending by moving to different municipalities, thereby biasing estimates upwards. To address this concern, table 9 shows IV estimates.

(Table 9 about here)

Overall, as was the case for the municipal election results, since the first stage and the power of the instrument is the same, the IV estimates again have much larger standard errors and are also less stable across specifications. As opposed to the case for municipal elections, however, the IV

estimates are fairly similar to the OLS estimates, being smaller in some specifications and larger in others but showing no real systematic deviations in either direction. Due to the the larger standard errors however, the estimates do lose significance in many of of the specifications.

While again the lack of precise estimates is clearly problematic, the relatively similar OLS and IV estimates are at least consistent with there being no or only a very small feedback bias in the OLS estimates.⁴⁵

Summing up, both OLS and IV estimates suggest very clearly that increases in ethnic diversity are associated with decreases in tax rates and increases in expenditure per capita. Despite the essential budget balance requirement, this divergence in expenditure and revenue is possible because of the corresponding increase in transfers to the municipality.⁴⁶

In terms of the model presented above, the signsof the effects imply the following conclusions: Under the auxiliary assumption that the net income effect of higher percentage NDOs on the municipality is positive, these finding does not allow one to draw any conclusion regarding the effect of ethnic diversity on underlying public good demand.

Under the more plausible opposite assumption, that the net income effect is negative, the finding here of a negative effect on taxation implies that *ceteris paribus* underlying public good demand must have decreased in response to increased ethnic diversity.

7 Conclusion

Starting from the literature on the negative effects of ethnic diversity on public goods provision, this paper focused attention on the potential implications for the effects of migration. As a particularly interesting case, a simple extrapolation suggests that the large welfare states in Western Europe will shrink in response to increased immigration of new ethnic groups. The paper then surveyed existing evidence on the effects of ethnic diversity and argued that such an extrapolation

⁴⁵The fairly big difference between OLS and IV estimates for municipal elections contrasted with the relative similarity of OLS and IV estimates for the budgetary outcomes seems somewhat surprising, as the former suggests that there is a feedback bias. One explanation could be that there is within-party shifts in the political platform (e.g. even left wing parties field more right-leaning candidates) and that NDOs respond to the outcomes of municipal elections but does not respond to budgetary outcomes.

⁴⁶In appendix C I show that these results are also robust to considering the shorter time period 1990-2001 and using detailed revenue data for this time period I show that the decreases in tax rates do translate into lower tax revenues. Appendix C also shows that this combination of a decrease in tax revenues and an increase in expenditure is possible, despite the essential budget balance requirement, because of an offsetting increase in the transfers to the municipality. Finally appendix C shows that the results does not seem to be driven by potential outliers.

might be misguided due to stark differences in histories of conflict, in the quality of institutions and in timing between the European case and the settings studied in the existing literature.

The paper then attempted to address these concerns by examining the effects of ethnic diversity on election and budgetary outcomes in Danish municipalities 1981-2001. Using a rich set of controls and an IV strategy based on historical housing data, the main results of the paper show that ethnic diversity has impacted outcomes of municipal elections in a way consistent with lower public good demand. Using a simple theoretical model to disentangle ethnic diversity effects from other budgetary effects, the paper further shows that the same holds true for budgetary outcomes, although an untestable but plausible auxiliary assumption is required on the budgetary process.

Assessing the magnitude of the negative effect on public good demand is complicated by the fact that all estimates are likely to be biased both due to immigrants and refugees voting and due to direct budgetary effects of immigrants and refugees. Ignoring these biases, a back-of-the-envelope calculation suggests that tax revenue of an Danish municipality will drop by about ten percent over the next 50 years due to increases in ethnic diversity.⁴⁷ While clearly being economically significant this suggests that the the Western European welfare states will not be completely dismantled any time soon. The ignored downward biases in this calculation also makes it a lower bound on the magnitude of the effect. Obtaining unbiased estimates of the magnitude of these effects seems like a difficult but important topic for future research.

In terms of normative implications, the paper's findings quite clearly show that the potential ethnic diversity effects of migration are important to consider when formulating immigration and refugee policy. Beyond this, however, the normative implications of the findings are less clear. First of all the initial level of public goods may be inefficiently high (as some would argue is the case in Europe) in which case the negative effect on public goods could be desirable. Second other gains from immigration and ethnic diversity may offset any adverse effects. Finally, depending on the welfare criteria used, it is very possible that the positive welfare effect of migration for immigrants and (especially) refugees may also offset any adverse effect.

⁴⁷Estimates of the effect on the municipal income tax rate all suggest an effect on the order of -0.2 percentage point per percentage point increase in NDOs. The percentage NDOs in Denmark increased by 4.4 percentage points between 1981 and 2001 suggesting that a municipality experiencing the same increase in ethnic diversity as the country as a whole decreased their income tax rate by 0.88 percentage points due to migration. Under such a linear extrapolation, the income tax rate drop of this "representative" municipality over a 50 year period would thus be 2.2 percentage point or about a ten percent drop in tax revenues (based on the mean tax rate in 2001). Obviously, this calculation is very sensitive to non-linearities both in the effect of ethnic diversity and migration.

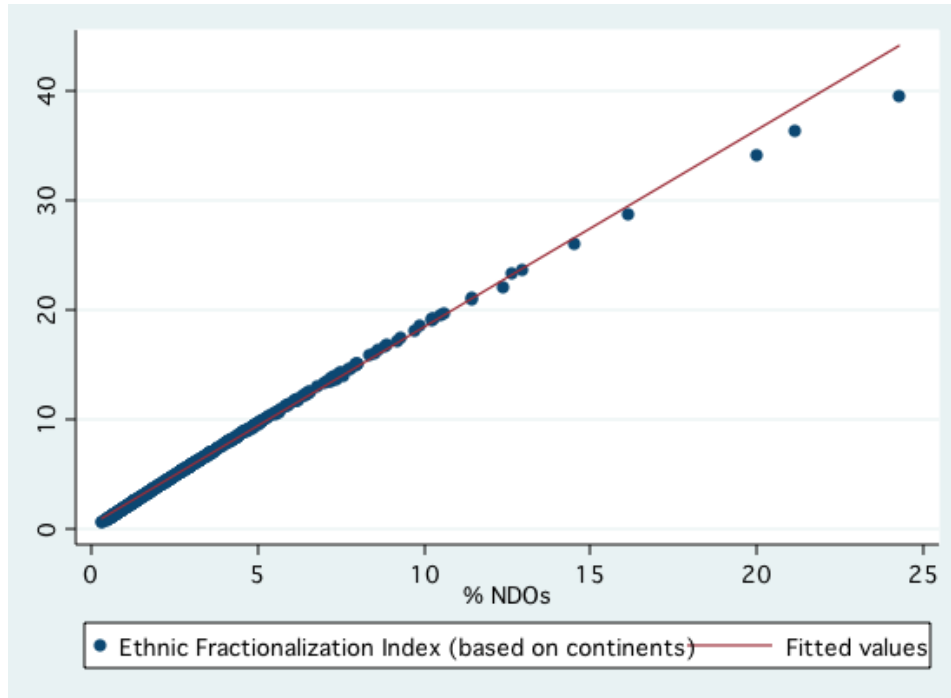
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A Figures and Tables from the main text

Figure 1: Scatterplot of diversity measures



Data points include all 273 municipalities in 1981 and again in 2001. The fitted line is estimated by OLS, and has an estimated slope and robust standard error of 1.83 (0.01).

Figure 2: The IV strategy

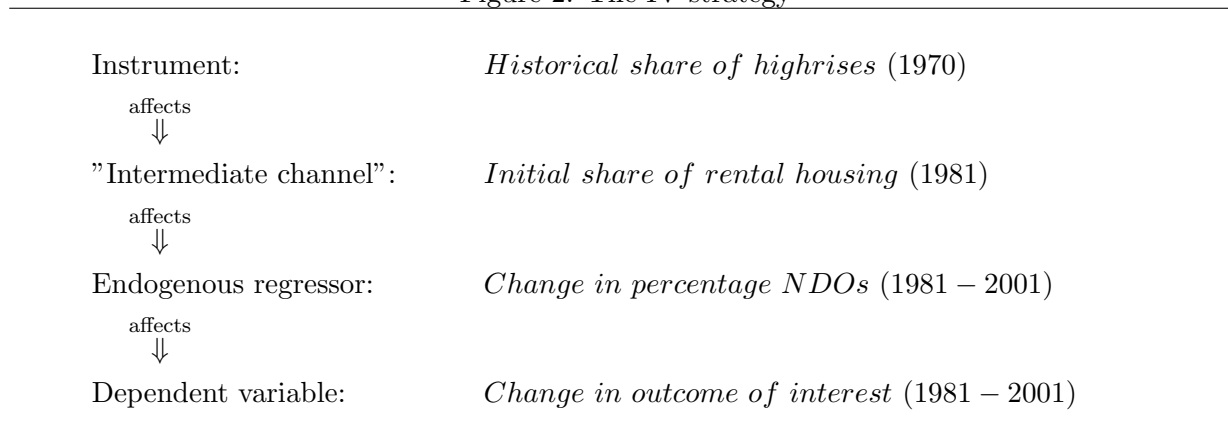


Table 1: Descriptive Statistics, 1981

	mean	sd	min	med	max
Percentage NDOs	1.81	1.56	0.31	1.28	11.41
Total pop. (1,000)	16.64	22.85	2.68	9.82	245.57
Pop. density (1,000/sqkm)	0.20	0.41	0.02	0.07	3.11
Mean income (1,000 kr)	50.95	8.15	37.35	49.18	85.82
Unemployment rate	7.06	2.05	2.16	7.28	15.14
Percentage not in workforce	46.71	2.69	35.00	47.16	55.36
Gini-coef.	0.32	0.03	0.23	0.31	0.52
Percentage rec. inc. transfers	4.09	1.34	0.50	3.91	10.86
Percentage rental housing	24.31	13.65	8.34	18.96	72.70
Percentage highrises (1970)	15.92	16.72	0.15	8.86	70.92
Income tax rate	17.03	1.35	11.50	17.00	21.00
Land tax rate	0.86	0.60	0.00	0.70	5.50
Percentage seats, trad. left	40.23	15.85	5.88	40.00	84.21
Percentage seats, trad. right	39.64	15.04	0.00	41.18	72.73
Percentage seats, nationalists	5.94	3.93	0.00	5.88	19.05
Percentage seats, other parties	14.19	18.38	0.00	6.67	90.91
Expenditure (1,000 kr/person)	7.38	0.93	6.03	7.14	11.24

Table 2: Descriptive Statistics, 2001

	mean	sd	min	med	max
Percentage NDOs	4.41	3.04	1.58	3.40	24.25
Total pop. (1,000)	17.43	25.19	2.27	10.19	286.67
Pop. density (1,000/sqkm)	0.21	0.41	0.02	0.07	3.02
Mean income (1,000 kr)	71.87	9.05	59.36	69.48	119.47
Unemployment rate	4.07	1.58	1.67	3.73	9.90
Percentage not in workforce	46.27	3.10	37.69	46.02	56.28
Gini-coef.	0.25	0.02	0.23	0.25	0.39
Percentage rec. inc. transfers	4.46	1.57	0.40	4.21	10.80
Percentage rental housing	30.61	11.75	12.61	26.39	70.98
Percentage highrises (1970)	16.72	16.04	0.25	10.69	71.17
Income tax rate	20.88	0.93	15.50	20.90	23.20
Land tax rate	1.31	0.52	0.60	1.20	2.40
Percentage seats, trad. left	39.37	14.50	5.88	40.00	84.21
Percentage seats, trad. right	45.93	14.47	0.00	46.67	81.82
Percentage seats, nationalists	3.50	4.13	0.00	4.00	26.67
Percentage seats, other parties	11.20	15.48	0.00	5.88	90.91
Expenditure (1,000 kr/person)	13.05	1.08	10.93	12.84	19.86

Table 3: Descriptive Statistics, Changes 1981-2001

	mean	sd	min	med	max
Percentage NDOs	2.60	1.87	0.63	2.03	15.59
Total pop. (1,000)	0.79	2.96	-3.12	0.22	41.10
Pop. density (1,000/sqkm)	0.00	0.02	-0.15	0.00	0.10
Mean income (1,000 kr)	20.92	4.26	9.48	21.14	37.12
Unemployment rate	-3.00	1.89	-8.29	-3.00	3.04
Percentage not in workforce	-0.45	2.65	-6.18	-0.86	10.08
Gini-coef.	-0.06	0.04	-0.25	-0.06	0.02
Percentage rec. inc. transfers	0.37	1.28	-6.31	0.33	4.38
Percentage rental housing	6.30	3.61	-8.08	6.34	14.46
Percentage highrises (1970)	0.80	2.29	-5.31	0.55	10.47
Income tax rate	3.84	1.38	-1.10	3.80	9.30
Land tax rate	0.44	0.53	-3.10	0.50	1.60
Percentage seats, trad. left	-0.86	12.08	-45.25	0.00	37.25
Percentage seats, trad. right	6.29	13.92	-30.59	5.88	60.00
Percentage seats, nationalists	-2.44	5.14	-18.18	0.00	20.00
Percentage seats, other parties	-2.99	16.36	-71.28	0.00	71.04
Expenditure (1,000 kr/person)	5.67	0.81	2.89	5.71	9.22

Table 4: Examining the IV strategy

VARIABLES	(1) % rental 1981	(2) % rental 1981	(3) Δ % NDOs 1981-2001	(4) Δ % NDOs 1981-2001	(5) Δ % NDOs 1981-2001	(6) Δ % NDOs 1981-2001	(7) Δ % NDOs 1981-2001
% of occupied housing that is rental 1981			0.0997*** (0.0118)	0.0823*** (0.0146)	0.126*** (0.0346)		
% of total housing that is highrises 1970	0.731*** (0.0258)	0.627*** (0.0367)			-0.0241 (0.0247)	0.0680*** (0.00776)	0.0378*** (0.00978)
Population density 1981		6.386*** (1.931)		0.658 (0.438)			1.404** (0.622)
Total population 1981		0.0108 (0.0194)		0.00432 (0.00525)			0.0105** (0.00430)
Constant	12.68*** (0.429)	12.86*** (0.405)	0.176 (0.237)	0.396* (0.234)	-0.0826 (0.430)	1.517*** (0.107)	1.541*** (0.0982)
Observations	273	273	273	273	273	273	273
R^2	0.801	0.825	0.527	0.540	0.536	0.368	0.437
First stage F-stat						76.88	14.92

Robust standard errors in parentheses

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

Table 5: Effect on seats won in municipal elections, OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1981-2001 OLS	1981-2001 OLS	1981-2001 OLS	1981-2001 OLS	1981-2001 OLS	1981-2001 OLS	1981-2001 OLS	1981-2001 OLS
Ch. in fraction seats, left	-0.687** (0.347)	0.153 (0.429)	-0.0939 (0.442)	0.307 (0.472)	0.359 (0.538)	0.512 (0.415)	0.924*** (0.333)	0.337 (0.460)
Ch. in fraction seats, right	-1.120*** (0.332)	-0.769 (0.481)	-1.450*** (0.443)	-1.023* (0.563)	-1.211* (0.665)	-1.155*** (0.419)	-1.874*** (0.309)	-1.027* (0.562)
Ch. in fraction seats, nat.	0.920*** (0.136)	0.487*** (0.170)	0.684*** (0.191)	0.438** (0.200)	0.429** (0.215)	0.341** (0.137)	0.616*** (0.0821)	0.429** (0.203)
Ch. in fraction seats, other	0.888** (0.358)	0.130 (0.492)	0.860* (0.502)	0.279 (0.589)	0.423 (0.689)	0.240 (0.462)	-0.168 (0.281)	0.261 (0.583)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 6: Effect on seats won in municipal elections, IV estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV
Ch. in fraction seats, left	-2.501*** (0.673)	-1.879* (1.043)	-5.297** (2.162)	-4.489** (2.251)	-6.474* (3.928)	-0.251 (1.964)	0.460 (0.789)	-2.704 (2.126)
Ch. in fraction seats, right	-0.736 (0.604)	-0.267 (0.940)	-1.466 (1.582)	-0.389 (1.997)	-1.159 (2.732)	-1.804 (1.891)	-1.759*** (0.616)	-1.940 (1.924)
Ch. in fraction seats, nat.	1.411*** (0.221)	0.951*** (0.328)	1.983*** (0.676)	1.601** (0.737)	2.039* (1.148)	0.889 (0.573)	0.838*** (0.166)	0.895 (0.721)
Ch. in fraction seats, other	1.826** (0.766)	1.195 (1.138)	4.780** (2.072)	3.278 (2.347)	5.594 (3.447)	0.0660 (2.064)	-0.771 (0.636)	3.749 (2.441)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	76.88	65.30	14.92	12.46	5.795	10.78 ¹	65.25 ¹	12.04

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 7: Summary of restrictions

	Ethnic diversity has <i>no effect</i> on public goods: $c h''_{G\theta} = 0$	Ethnic diversity affects public goods: $c h''_{G\theta} < 0$
Neg. income effects: $(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta}) \leq 0$	$\frac{\partial(G+\Gamma)}{\partial \theta} \geq 0$ $\frac{\partial T}{\partial \theta} \geq 0$	$\frac{\partial(G+\Gamma)}{\partial \theta} ?$ $\frac{\partial T}{\partial \theta} ?$
Pos. income effects: $(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta}) > 0$	$\frac{\partial(G+\Gamma)}{\partial \theta} > 0$ $\frac{\partial T}{\partial \theta} < 0$	$\frac{\partial(G+\Gamma)}{\partial \theta} ?$ $\frac{\partial T}{\partial \theta} < 0$

Table 8: Effect on budgetary outcomes, OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1981-2001	1981-2001	1981-2001	1981-2001	1981-2001	1981-2001	1981-2001	1981-2001	1981-2001
OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
OUTCOME:								
Ch. in income tax rate	-0.218*** (0.0670)	-0.296*** (0.0702)	-0.145* (0.0809)	-0.241*** (0.0789)	-0.187** (0.0826)	-0.143*** (0.0356)	-0.103*** (0.0267)	-0.241*** (0.0792)
Ch. in land tax rate	-0.126*** (0.0289)	-0.139*** (0.0330)	-0.114*** (0.0363)	-0.140*** (0.0394)	-0.121*** (0.0377)	-0.0905*** (0.0198)	-0.0611*** (0.0165)	-0.141*** (0.0391)
Ch. expenditure per cap.	0.126*** (0.0250)	0.122*** (0.0268)	0.218*** (0.0345)	0.161*** (0.0328)	0.162*** (0.0362)	0.190*** (0.0329)	0.227*** (0.0252)	0.158*** (0.0321)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls used are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 9: Effect on budgetary outcomes, IV estimates

OUTCOME:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV	1981-2001 IV
Ch. in income tax rate	-0.252*** (0.0738)	-0.387*** (0.103)	0.0333 (0.197)	-0.296 (0.213)	-0.113 (0.305)	-0.280* (0.146)	-0.145*** (0.0494)	-0.189 (0.221)
Ch. in land tax rate	-0.121*** (0.0269)	-0.106*** (0.0395)	-0.0337 (0.0834)	-0.0621 (0.0961)	0.0474 (0.157)	-0.111 (0.0683)	-0.0668** (0.0274)	-0.0516 (0.0883)
Ch. expenditure per cap.	0.0905** (0.0453)	0.147*** (0.0560)	0.413*** (0.129)	0.310** (0.126)	0.511** (0.226)	0.377*** (0.133)	0.224*** (0.0538)	0.367*** (0.130)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	76.88	65.30	14.92	12.46	5.795	11.58 ¹	52.46 ¹	12.04

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

B Details of variables used

Below the detailed construction of each of the main variables is described. Table names refer to the *Statistikbanken* database unless otherwise noted.

- *Percentage NDOs* is constructed as the sum of the *indvandrere* and *efterkommere* categories (immigrants and descendants) divided by the total population, based on the *BEF3* table.
- *Total population* is based on the *BEF3* table.
- *Population density* is constructed as total population divided by the mean of the land area 1985-2001 based on the *ARE2* table. The mean over the years available is taken because the measured land area actually shows some very small changes (less than 1 square kilometer) over the period despite the fact that municipal borders did not change.
- *Mean income* is constructed as the mean of total taxable income (*SKPLINK*) according to the *IDA* database of SDs individual level registry data, deflated to 1980 prices by the chain-linked GDP deflator from the Danish national account, table *NAT01*.
- *Unemployment rate* is constructed as the number of unemployed divided by the sum of unemployed and employed, according to the *RAS1* table.
- *Gini coefficient* is computed across households using total taxable income, according to the *IDA* database of SDs individual level registry data.
- *Percentage receiving social income transfers* is computed using the *QSocYd* or *QBistYd* indicators (depending on year) from the *IDA* database of SDs individual level registry data.
- *Percentage rental housing* is computed as the number of homes rented out divided by the sum of owner occupied homes, rented out homes and non-specified, using the *BOL1* table.
- *Percentage highrises (1970)* is computed as the sum of all highrise homes built before 1970 divided by the sum of all housing built before 1970, as measured in 1981 in the *BOL3* table.
- *Income tax rate* is taken from the *PSKATX* table.
- *Land tax rate* is taken from the *PSKATX* table.

- *Percentage seats, traditional left parties* is computed as the total number of seats won by *Socialdemokraterne, Det Radikale Venstre, Socialistisk Folkeparti, Enhedslisten, Internationalen-SAP, Danmarks Kommunistparti, Arbejderpartiet KAP* and *Venstresocialisterne* divided the total number of seats, based on the *VALG1, VALG9, VALG2* and *VALGK3X* tables.
- *Percentage seats, traditional right parties* is computed as the total number of seats won by *Venstre* and *Det Konservative Folkeparti* divided the total number of seats, based on the *VALG1, VALG9, VALG2* and *VALGK3X* tables.
- *Percentage seats, nationalist parties* is computed as the total number of seats won by *Fremskridtspartiet* and *Dansk Folkeparti* divided the total number of seats, based on the *VALG1, VALG9, VALG2* and *VALGK3X* tables.
- *Percentage seats, other parties* is computed as one minus the percentage of seats won by the traditional left, the traditional right and the nationalist parties, based on the *VALG1, VALG9, VALG2* and *VALGK3X*.
- *Expenditure per capita* is computed as net current municipal expenditure minus reimbursements from the national level divided by total population, based on *REG1, REG1R* and *REG11* tables. This is then deflated to 1980 prices by the chain-linked GDP deflator from the Danish national account, table *NAT01*.
- *Income tax revenue per capita* is based on total income tax revenue from the *REG3* and *REG31* tables divided by total population. This is then deflated to 1980 prices by the chain-linked GDP deflator from the Danish national account, table *NAT01*.
- *Land tax revenue per capita* is based on total land tax revenue from the *REG3* and *REG31* tables divided by total population. This is then deflated to 1980 prices by the chain-linked GDP deflator from the Danish national account, table *NAT01*.
- *Total tax revenue per capita* is based on the sum of tax revenue items from the *REG3* and *REG31* tables divided by total population. This is then deflated to 1980 prices by the chain-linked GDP deflator from the Danish national account, table *NAT01*.
- *Total net transfers per capita* is based on the sum of transfer items from the *REG3* and

REG31 tables divided by total population. This is then deflated to 1980 prices by the chain-linked GDP deflator from the Danish national account, table *NAT01*.

C Additional empirical results

This section presents some additional empirical results and robustness checks.

C.1 A shorter time period with detailed revenue data

In this section I employ detailed data on revenue from transfers and taxes to investigate whether in fact the transfers to municipality respond to the percentage NDOs and to investigate the extent to which the lowering of tax rates in response to more NDOs also lowers the tax revenue. Since detailed data on revenues and transfers are not available for the entire time period, I have to limit attention to the period 1990-2001. As a preliminary step I thus examine whether the other results of the paper are valid also for this shorter period.

For completeness, I present results both using OLS and IV, however, to preserve any power in the instrument I have to use the highrise share of the 1980 housing stock as my instrument rather than 1970. This clearly makes the exclusion restriction more likely to fail since in particular the first wave of immigrants had already arrived in Denmark by 1980.

Tables 10 and 11 repeat the estimations in the main text for the outcome of municipal elections only for the shorter time period. While I omit a detailed discussion of each estimate, the estimates - and in particular the conclusions arising from the tables overall - are very similar to the ones obtained in the main text using the full time period. As could be expected given the loss of variation when moving to the shorter time period, estimates are less precise implying that fewer estimates are significant.

Next, tables 12 and 13 repeat the estimations for budgetary outcomes for the shorter time period. Again we see that although standard errors are slightly larger, the estimates and conclusions are very similar to what is obtained using the full time period.

Finally, tables 14 and 15 presents OLS and IV estimates of the effect of changes in percentage NDOs on income, land and total tax revenue as well as the total net transfers received by the municipality.

Table 10: Effect on seats won in municipal elections, OLS estimates, shorter time period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1989-2001 OLS	1989-2001 OLS	1989-2001 OLS	1989-2001 OLS	1989-2001 OLS	1989-2001 OLS	1989-2001 OLS	1989-2001 OLS
Ch. in fraction seats, left	0.226 (0.351)	1.028** (0.403)	0.491 (0.471)	0.748 (0.485)	1.047 (0.663)	0.967** (0.448)	1.283*** (0.380)	0.801 (0.498)
Ch. in fraction seats, right	-1.029** (0.419)	-0.977** (0.467)	-1.559*** (0.562)	-1.569** (0.630)	-1.534* (0.815)	-1.846*** (0.563)	-1.776*** (0.411)	-1.571** (0.631)
Ch. in fraction seats, nat.	0.651*** (0.191)	0.252 (0.225)	0.526** (0.254)	0.405 (0.273)	0.224 (0.384)	0.459** (0.190)	0.648*** (0.133)	0.379 (0.282)
Ch. in fraction seats, other	0.151 (0.405)	-0.302 (0.470)	0.542 (0.569)	0.415 (0.667)	0.263 (0.964)	0.405 (0.644)	-0.153 (0.381)	0.390 (0.655)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 11: Effect on seats won in municipal elections, IV estimates, shorter time period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1989-2001 IV	1989-2001 IV	1989-2001 IV	1989-2001 IV	1989-2001 IV	1989-2001 IV	1989-2001 IV	1989-2001 IV
Ch. in fraction seats, left	-0.908 (0.672)	0.467 (0.808)	-2.276 (1.437)	-0.898 (1.316)	-3.161 (3.638)	1.253 (1.204)	1.179* (0.671)	-0.347 (1.408)
Ch. in fraction seats, right	-0.308 (0.621)	-0.101 (0.747)	-1.134 (1.172)	-0.723 (1.227)	-0.0544 (2.929)	-2.114* (1.160)	-1.204* (0.650)	-1.054 (1.229)
Ch. in fraction seats, nat.	0.836*** (0.263)	0.111 (0.323)	0.891* (0.539)	0.665 (0.544)	-0.0368 (1.386)	0.892** (0.436)	0.868*** (0.195)	0.0455 (0.555)
Ch. in fraction seats, other	0.379 (0.713)	-0.476 (0.909)	2.519* (1.515)	0.955 (1.464)	3.252 (3.786)	0.0453 (1.414)	-0.821 (0.632)	1.356 (1.558)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	65.21	96.74	28.06	33.39	8.235	31.28 ¹	64.98 ¹	28.79

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 12: Effect on budgetary outcomes, OLS estimates, shorter time period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001
OUTCOME:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Ch. in income tax rate	-0.202*** (0.0362)	-0.229*** (0.0396)	-0.154*** (0.0488)	-0.140*** (0.0502)	-0.188*** (0.0618)	-0.160*** (0.0350)	-0.144*** (0.0261)	-0.134*** (0.0499)
Ch. in land tax rate	-0.0357** (0.0150)	-0.0438*** (0.0158)	-0.0404** (0.0183)	-0.0526*** (0.0193)	-0.0205 (0.0253)	-0.0516*** (0.0166)	-0.0263* (0.0134)	-0.0484** (0.0199)
Ch. expenditure per cap.	0.126*** (0.0247)	0.0839*** (0.0271)	0.216*** (0.0286)	0.165*** (0.0330)	0.148*** (0.0388)	0.202*** (0.0337)	0.195*** (0.0328)	0.167*** (0.0334)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 13: Effect on budgetary outcomes, IV estimates, shorter time period

OUTCOME:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV
Ch. in income tax rate	-0.293*** (0.0685)	-0.388*** (0.0750)	-0.220 (0.169)	-0.330* (0.181)	-0.647 (0.418)	-0.283** (0.132)	-0.229*** (0.0499)	-0.263 (0.183)
Ch. in land tax rate	-0.0238 (0.0272)	-0.0225 (0.0307)	-0.000888 (0.0740)	-0.0497 (0.0712)	0.155 (0.207)	-0.0382 (0.0720)	-0.0187 (0.0268)	0.0445 (0.0868)
Ch. expenditure per cap.	0.0790 (0.0582)	0.0694 (0.0585)	0.495*** (0.144)	0.379** (0.162)	0.822* (0.497)	0.481*** (0.172)	0.173** (0.0811)	0.497*** (0.185)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	60.66	87.41	27.98	33.33	8.540	12.76 ¹	36.13 ¹	30.97

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 14: Effect on municipal revenues, OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1990-2001 OLS	1990-2001 OLS	1990-2001 OLS	1990-2001 OLS	1990-2001 OLS	1990-2001 OLS	1990-2001 OLS	1990-2001 OLS
Ch. in inc. tax rev. per cap.	-0.220*** (0.0276)	-0.183*** (0.0250)	-0.203*** (0.0560)	-0.113*** (0.0367)	-0.113** (0.0444)	-0.0923** (0.0378)	-0.216*** (0.0411)	-0.106*** (0.0370)
Ch. in land tax rev. per cap.	-0.00349 (0.00743)	-0.00279 (0.0114)	-0.0107 (0.0112)	-0.00290 (0.0133)	0.00519 (0.0165)	-0.000365 (0.0131)	-0.00990 (0.00799)	-0.00281 (0.0133)
Ch. in total tax rev. per cap.	-0.184*** (0.0297)	-0.140*** (0.0278)	-0.244*** (0.0614)	-0.105*** (0.0374)	-0.0874* (0.0476)	-0.0916** (0.0387)	-0.256*** (0.0502)	-0.101*** (0.0381)
Ch. in net transfer per cap.	0.330*** (0.0271)	0.245*** (0.0199)	0.363*** (0.0486)	0.213*** (0.0244)	0.229*** (0.0323)	0.221*** (0.0261)	0.339*** (0.0295)	0.210*** (0.0246)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 15: Effect on municipal revenues, IV estimates

OUTCOME:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV
Ch. in inc. tax rev. per cap.	-0.203*** (0.0658)	-0.254*** (0.0447)	-0.106 (0.0979)	-0.205*** (0.0727)	-0.208 (0.185)	0.0422 (0.0796)	-0.179*** (0.0577)	-0.111 (0.0754)
Ch. in land tax rev. per cap.	-0.0134 (0.0130)	-0.0264* (0.0147)	-0.0551** (0.0238)	-0.0631** (0.0251)	-0.123* (0.0724)	-0.0276 (0.0227)	-0.0187 (0.0134)	-0.0606** (0.0274)
Ch. in total tax rev. per cap.	-0.0842 (0.0816)	-0.187*** (0.0459)	-0.111 (0.117)	-0.206** (0.0802)	-0.142 (0.201)	-0.0580 (0.0915)	-0.278*** (0.0742)	-0.124 (0.0847)
Ch. in net transfer per cap.	0.313*** (0.0583)	0.326*** (0.0341)	0.423*** (0.0884)	0.392*** (0.0630)	0.674*** (0.206)	0.344*** (0.0535)	0.428*** (0.0431)	0.361*** (0.0633)
Observations	273	273	273	273	273	273	273	273
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	60.66	87.41	27.98	33.33	8.540	26.47 ¹	59.75 ¹	30.97

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Looking first at the OLS estimates for taxation we see that all the estimates suggest a negative and significant effect of around 100 kr. to 220 kr. on income tax revenue per capita. This magnitude is very much in line with the estimated effect on the income tax rate.⁴⁸ For the land tax however, while all the estimates are negative, they are all of a very small magnitude and insignificant. This likely reflects the much smaller importance of land taxes in municipal revenues. Finally, the estimates for total taxation are almost identical to the estimates for the income tax reflecting that the income tax is by far the most importance source of municipal revenue.

For the changes in net transfers, as expected, the OLS estimates show a significant positive response of around 210 kr. to 330 kr. per capita. It is worth noting that this increase also fairly closely corresponds to the "gap" in the municipal budget left by the increase in expenditure and decrease in tax revenue, which explains how the estimated budget effects are consistent with the essential budget balance requirement faced by municipalities.

Looking at the IV estimates in table 15, we again see the by now familiar pattern that OLS and IV estimates are very similar although IV estimates are less precise. I thus omit a further discussion of these.

C.2 Sensitivity of results to potential outliers

In this section I examine whether the results of the main text are primarily being driven by municipalities experiencing extreme increases in ethnic diversity. Tables 16 and 17 thus repeats the main estimations of the paper on a sample that excludes the 20 municipalities experiencing the largest increase in percentage NDOs between 1981-2001 (corresponding to a restriction that change in NDOs is restricted to be below 5.25). Out of completeness I also present IV estimates in tables 18 and 19, for a sample excluding the 20 municipalities with the highest level of the instrument⁴⁹. although, as will become apparent, with the loss of variation, the instrument generally loses too much power to give meaningful results.

While again I omit a detailed discussion of individual estimates, the OLS estimates for the

⁴⁸One can for example perform the following back of the envelope calculation: The mean taxable income in 2001 is 73,188 kr. One estimate of the drop in tax rate following a 1 percentage point increase in fraction NDOs is 0.241 (table 12, column 7) suggesting a drop in total revenue per capita of 0.14 percent of 73,188 which is 102 kr. Comparing this to the corresponding direct estimate of the effect on tax revenue of a 1 percent increase in NDO which is a drop of 113 kr (table 14 column 7).

⁴⁹Under the assumption motivating IV, namely that changes in percentage NDOs is endogenous, i.e. correlated with the error term, sample selection based on percentage NDOs will cause sample selection bias, thus instead I omit extreme values of the instrument.

Table 16: Effect on seats won in municipal elections, OLS estimates, smaller sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001
	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Ch. in fraction seats, left	-1.535** (0.741)	-0.299 (0.777)	-0.630 (0.876)	0.416 (0.961)	0.439 (0.913)	0.524 (0.778)	0.391 (0.653)	0.544 (0.896)
Ch. in fraction seats, right	-0.907 (0.860)	-0.592 (0.982)	-1.431 (1.098)	-1.041 (1.288)	-1.034 (1.198)	-1.179 (0.948)	-1.799** (0.713)	-0.961 (1.218)
Ch. in fraction seats, nat.	0.843*** (0.308)	0.424 (0.322)	0.532 (0.370)	0.136 (0.398)	0.191 (0.385)	-0.0261 (0.280)	0.482** (0.215)	0.133 (0.386)
Ch. in fraction seats, other	1.600 (0.972)	0.467 (1.030)	1.530 (1.298)	0.489 (1.444)	0.404 (1.379)	0.592 (1.092)	0.379 (0.817)	0.284 (1.369)
Observations	253	253	253	253	253	253	253	253
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 17: Effects on budgetary outcomes, OLS estimates, smaller sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001	1990-2001
OUTCOME:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Ch. in income tax rate	-0.171** (0.0817)	-0.344*** (0.0858)	-0.112 (0.0901)	-0.322*** (0.0952)	-0.297*** (0.0968)	-0.199*** (0.0658)	-0.0844 (0.0526)	-0.325*** (0.0933)
Ch. in land tax rate	-0.0899*** (0.0283)	-0.100*** (0.0298)	-0.0563* (0.0335)	-0.0683* (0.0354)	-0.0721** (0.0349)	-0.0614** (0.0309)	-0.0450* (0.0255)	-0.0816** (0.0342)
Ch. expenditure per cap.	0.152*** (0.0462)	0.0893** (0.0400)	0.218*** (0.0568)	0.121** (0.0509)	0.0898* (0.0476)	0.110** (0.0430)	0.217*** (0.0416)	0.0919* (0.0472)
Observations	253	253	253	253	253	253	253	253
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes

Robust standard errors in parentheses

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

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 18: Effect on seats won in municipal elections, IV estimates, smaller sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
OUTCOME:	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV
Ch. in fraction seats, left	-2.382** (0.997)	-1.502 (1.305)	-10.74 (12.48)	-12.30 (19.83)	392.6 (17,570)	-0.623 (10.27)	0.806 (1.090)	-4.591 (11.17)
Ch. in fraction seats, right	-1.983** (0.868)	-1.735 (1.165)	-0.434 (5.713)	7.319 (14.95)	-203.2 (9,037)	0.137 (11.81)	-2.280*** (0.854)	1.433 (9.893)
Ch. in fraction seats, nat.	1.728*** (0.334)	1.372*** (0.439)	4.268 (4.452)	5.425 (7.771)	-157.7 (7,071)	-0.764 (3.873)	1.016*** (0.272)	2.448 (4.504)
Ch. in fraction seats, other	2.637** (1.130)	1.865 (1.392)	6.907 (9.094)	-0.441 (11.56)	-31.75 (1,513)	-2.587 (13.12)	-0.790 (0.998)	0.711 (11.31)
Observations	253	253	253	253	253	253	253	253
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	68.27	56.51	0.975	0.485	0.000480	0.311 ¹	55.39 ¹	0.522

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

Table 19: Effect on budgetary outcomes, IV estimates, smaller sample

OUTCOME:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV	1990-2001 IV
Ch. in income tax rate	-0.133 (0.103)	-0.239* (0.136)	0.463 (1.036)	-0.980 (1.285)	12.63 (583.0)	-0.744 (1.395)	-0.119 (0.0777)	-0.938 (1.162)
Ch. in land tax rate	-0.150*** (0.0428)	-0.140*** (0.0468)	0.322 (0.531)	0.321 (0.859)	-19.55 (867.5)	-0.133 (0.285)	-0.105*** (0.0400)	0.209 (0.674)
Ch. expenditure per cap.	0.133** (0.0648)	0.188** (0.0751)	1.639 (1.421)	1.561 (1.964)	-51.99 (2,335)	1.319 (1.341)	0.235*** (0.0677)	1.678 (2.021)
Observations	253	253	253	253	253	253	253	253
Socioeconomic controls	No	Yes	No	Yes	Yes	Yes	No	Yes
Init. pop. size. and dens.	No	No	Yes	Yes	Yes	Yes	No	Yes
Change, total pop.	No	No	No	Yes	No	No	No	No
Init. percentage NDOs	No	No	No	No	Yes	No	No	No
Init. value of outcome	No	No	No	No	No	Yes	Yes	No
Change, highrise share	No	No	No	No	No	No	No	Yes
First stage F-stat	68.27	56.51	0.975	0.485	0.000480	0.282 ¹	57.05 ¹	0.522

Robust standard errors in parentheses

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

¹ For specifications including initial value of the outcome F-stats vary by row. The table report the smallest across rows.

The controls use are the changes in a set of socioeconomic indicators (mean income, fraction not in the workforce, unemployment rate, gini coefficient, and fraction receiving income transfers), initial population size and density, changes in total population size, initial percentage NDOs, initial value of the outcome variable and the change in the highrise share of the housing stock.

smaller sample are very similar to the estimates using the entire sample, suggesting the results are not driven by outliers. The loss of precision in estimates does affect the significance level of many estimates however, especially for results regarding municipal elections.

For the IV estimates, we see that the loss of variation in the instrument generally makes the instrument too weak to obtain meaningful estimates. In a lot of specifications the first stage F-stat becomes very small (way below 10) implying that estimates might suffer from weak instrument bias and even when the weak instrument rule-of-thumb-test passes, standard errors are very large. For the few specifications where standard errors do not blow up (column one in particular), however, it is reassuring that estimates are fairly similar to the ones using the full sample.

D Detailed model derivations

The details of the model derivations are spelled out in this section.

With the "combined" budget constraint obtained from (1), (3), and the expression for total taxation, the median voters maximization problem is simply:

$$\begin{aligned} \max_{x,G} U(x, G; \theta) &= u(x) + h(G; \theta) & (10) \\ \text{s.t. } n^{-1}(G - M + \Gamma) &= Y - x \end{aligned}$$

The maximization problem is clearly concave so taking first order conditions with respect to G and x then yields the following equation characterizing the solution:

$$u'_x(x) = \lambda \tag{11}$$

$$h'_G(G; \theta) = n^{-1}\lambda \tag{12}$$

Combining these and substituting in for x from the budget constraint yields an equation defining the level of public spending:

$$\frac{u'_x(Y - n^{-1}(G + \Gamma - M))}{h'_G(G; \theta)} = n^{-1} \tag{13}$$

It will be convenient to work with this equation in logs:

$$\ln u'_x(Y - n^{-1}(G + \Gamma - M)) - \ln h'_G(G; \theta) = \ln n^{-1} \quad (14)$$

Now we use (14) to do comparative statistics with respect to the effect of changes in ethnic diversity, θ . Applying the implicit function theorem yields:

$$\begin{aligned} \frac{\partial G}{\partial \theta} &= \frac{-\frac{h''_{G\theta}(G; \theta)}{h'_G(G; \theta)}}{\frac{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}{u'_x(Y - n^{-1}(G + \Gamma - M))} n^{-1} + \frac{h''_{GG}(G; \theta)}{h'_G(G; \theta)}} \iff \\ \frac{\partial G}{\partial \theta} &= c h''_{G\theta}(G; \theta) \quad \text{where} \quad (15) \\ c &\equiv \frac{-1}{\underbrace{\frac{h'_G(G; \theta) n^{-1}}{u'_x(Y - n^{-1}(G + \Gamma - M))}}_{=1} \underbrace{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}_{<0} + \underbrace{h''_{GG}(G; \theta)}_{<0}} > 0 \end{aligned}$$

Next we consider the case where transfers and nationally mandated expenditure varies with ethnic diversity, that is $\frac{\partial \Gamma}{\partial \theta} > 0$ and $\frac{\partial M}{\partial \theta} > 0$.

Since Γ and M are still exogenous from the point of the view of the median voter, his optimization problem is unchanged and (14) continues to characterize the equilibrium. However applying the implicit function theorem now yields:

$$\begin{aligned} \frac{\partial G}{\partial \theta} &= \frac{-\frac{h''_{G\theta}(G; \theta)}{h'_G(G; \theta)} - \frac{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}{u'_x(Y - n^{-1}(G + \Gamma - M))} n^{-1} \left(\frac{\partial \Gamma}{\partial \theta} - \frac{\partial M}{\partial \theta} \right)}{\frac{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}{u'_x(Y - n^{-1}(G + \Gamma - M))} n^{-1} + \frac{h''_{GG}(G; \theta)}{h'_G(G; \theta)}} \iff \\ \frac{\partial G}{\partial \theta} &= c h''_{G\theta}(G; \theta) + \nu \left(\frac{\partial M}{\partial \theta} - \frac{\partial \Gamma}{\partial \theta} \right) \quad \text{where} \quad (16) \\ c &\equiv \frac{-1}{\underbrace{\frac{h'_G(G; \theta) n^{-1}}{u'_x(Y - n^{-1}(G + \Gamma - M))}}_{=1} \underbrace{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}_{<0} + \underbrace{h''_{GG}(G; \theta)}_{<0}} > 0 \\ \nu &\equiv \frac{\frac{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}{u'_x(Y - n^{-1}(G + \Gamma - M))} n^{-1}}{\frac{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}{u'_x(Y - n^{-1}(G + \Gamma - M))} n^{-1} + \frac{h''_{GG}(G; \theta)}{h'_G(G; \theta)}} \\ &= \frac{1}{1 + \underbrace{\frac{u'_x(Y - n^{-1}(G + \Gamma - M))}{h'_G(G; \theta) n^{-1}}}_{=1} \underbrace{\frac{1}{u''_{xx}(Y - n^{-1}(G + \Gamma - M))}}_{<0} \underbrace{h''_{GG}(G; \theta)}_{<0}} \quad (17) \\ &\in (0, 1) \end{aligned}$$

When transfers and mandatory expenditure were fixed before, the effect on total expenditure $G + \Gamma$ and taxation T and t followed immediately from knowing the effect on G due to budget balance. With transfers and mandatory expenditure endogenized this takes slightly more work.

The effect on total expenditure can be found as:

$$\begin{aligned}
\frac{\partial(G + \Gamma)}{\partial\theta} &= \frac{\partial(G)}{\partial\theta} + \frac{\partial(\Gamma)}{\partial\theta} \\
&= c h''_{G\theta}(G; \theta) + \nu \left(\frac{\partial M}{\partial\theta} - \frac{\partial\Gamma}{\partial\theta} \right) + \frac{\partial(\Gamma)}{\partial\theta} \\
&= c h''_{G\theta}(G; \theta) + \nu \frac{\partial M}{\partial\theta} + (1 - \nu) \frac{\partial\Gamma}{\partial\theta}
\end{aligned} \tag{18}$$

For taxation, differentiating the municipal budget constraint (1) yields:

$$\begin{aligned}
\frac{\partial T}{\partial\theta} &= \frac{\partial G}{\partial\theta} + \frac{\partial\Gamma}{\partial\theta} - \frac{\partial M}{\partial\theta} \\
&= c h''_{G\theta}(G; \theta) + (\nu - 1) \left(\frac{\partial M}{\partial\theta} - \frac{\partial\Gamma}{\partial\theta} \right)
\end{aligned} \tag{19}$$