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Fiscal Equalization and Capitalization: Evidence from a Policy Reform

Abstract

Fiscal equalization through the allocation of central government grants may have adverse distributional implications if these grants capitalize into house values. We investigate the impact of changes in grants induced by a reform of the Dutch grant system. Since this reform was implemented gradually and in two subsequent stages that targeted different policy domains, we are able to identify on the nonlinearity of its impact over time. As robustness checks, we identify on either stage separately, or on a reform of financing school buildings, which should have limited effects on house prices as additional funds came with an additional task for municipalities. Our results indicate full capitalization of grants. It follows that property owners were important beneficiaries in the municipalities that saw their grants increased because of disadvantageous socio-economic composition.

JEL classification: H7, H81, R2, R3, R51

Keywords: fiscal equalization, capitalization, intergovernmental transfers

1 Introduction

In most countries, local governments are partly funded by central government grants and in many cases, the grant allocation system takes account of disparities in either spending needs, fiscal capacity or both.¹ As Oates (1999) points out in his influential essay on fiscal federalism, the primary justification for fiscal equalization must be on equity grounds. Yet he also cautions that from the perspective of redistributing income from rich to poor, equalizing grants are bound to have some perverse effects, as they will inevitably result in some income transfers from poor individuals who reside in wealthy jurisdictions to rich persons in generally poor areas. As noted by Wyckoff (1995), the capitalization of equalizing grants into house prices is a likely channel through which such perverse effects may operate. Mobile households will bid up the value of real estate in communities that use equalizing grants to raise the level of local public services or to cut taxes. This benefits the typically better off property owners, while the welfare gain of intergovernmental aid to poor renters may be completely offset by higher housing costs.

Our paper provides new empirical evidence on the capitalization of equalizing grants, by investigating the impact of a reform of the allocation of grants to municipalities in the Netherlands. This reform introduced the equalization of tax capacity and it increased the weight of socio-economic characteristics, such as poverty rates and the share of minorities. With a standard deviation of 56 euro per inhabitant, the reform-induced change in grants varied considerably over municipalities. Scattering this change in grants per capita against personal income, Figure 1 illustrates the redistributive nature of the reform. The fitted trend in this figure indicates that on average, municipalities in which per capita income was 10% higher saw their annual grant reduced by almost 40 euro per inhabitant.

We identify the causal impact of central government grants on local house prices by exploiting a non-linearity in the reform-induced change in grants over time as a source of exogenous variation. The reform was introduced in two stages, which targeted different policy domains. Furthermore, changes in grants were introduced gradually, in order to allow municipalities to adjust their policies. The resulting pattern of reform-induced changes in grants over time deviates from a linear trend, which allows us to control for any unobserved linear time trends that may correlate with grants and house prices. We validate our

¹ For example, fiscal equalization is common in Europe and it has a long tradition in Canada, Australia and some developing countries like India. While equalizing grants from the federal to state governments are limited in the US, many states provide equalizing grants to local jurisdictions – notably school districts.

identification strategy by using each of the two reform stages separately and by considering a placebo reform, which made municipalities responsible for the accommodation of schools and provided the corresponding resources. Since these grants came with additional tasks for municipalities, we should not find substantial capitalization when identifying on this reform.

Our baseline results are consistent with full capitalization of central government grants into house prices and they turn out to be remarkably robust to the various tests to which we subject our identification strategy. These estimates imply that homeowners in municipalities at the 90th percentile of the distribution of reform-induced changes in grants have gained 4,273 euro, while homeowners at the 10th percentile lost 5,916 euro. In view of a growing literature that points to the importance of housing supply conditions in determining capitalization rates, we also investigate whether reform-induced price changes have led to adjustments in local housing stocks or in local rates of new construction.² However, we find no indication that municipalities that received more grants systematically built more housing. This result helps explaining the high rate of capitalization.

In spite of a rich empirical literature on the capitalization of fiscal differentials into house prices, evidence on the capitalization of intergovernmental transfers is relatively scarce.³ Hilber *et al.* (2011) find evidence of substantial to full capitalization in a study of central government grants to local authorities in England. Furthermore, house prices appear to respond more strongly in locations in which new construction is constrained by physical barriers. In their analysis, electoral targeting of grants by the incumbent Labour party provides the source of exogenous variation. In contrast, we identify on a reform that was designed to make the grant system more equitable, so that our analysis provides direct evidence on the impact of fiscal equalization. Moreover, the variation in reform-induced changes in grants in our analysis is considerably larger, enabling stronger and more robust identification.⁴

Identifying on reforms of state school financing formulas, which have become more equalizing in many US states over their period of observation, Barrow and Rouse (2004) find evidence of full capitalization of state education aid to school districts. They instrument the actual change in state aid with the change in aid that each district would have received on the basis of the post-reform formulas and pre-reform characteristics. The validity of this approach relies on the assumption that changes in district housing values are not correlated with the pre-

² See for instance Brasington (2002) or Hilber and Mayer (2009). Hilber (2011) provides a survey.

³ The capitalization literature is surveyed in Chaudry-Shah (1988), Ross and Yinger (1999) and Hilber (2011).

⁴ Identification in Hilber *et al.* (2011) is based on the positive but diminishing impact of Labour dominance on grants, after controlling for the linear effect of Labour's share of seats. An increase in Labour's share from 40 per cent to 50 per cent is found to reduce grants per capita by 13 pounds (year 2008), corresponding to 16 euros (year 2010).

reform district characteristics that are used to construct the instrument through other channels. Our identification requires less restrictive assumptions in this respect, as we control for arbitrary linear time trends in grants and house prices.

Capitalization has also been used as a test for allocative efficiency in the public sector. Assuming perfect mobility and homogeneous preferences, Brueckner (1979, 1982) demonstrates that a rise in property taxes should not affect property values at the level of local public goods provision that satisfies the Samuelson condition, since its negative impact would be exactly offset by capitalization of the willingness to pay for a rise in the provision of those goods. Barrow and Rouse (2004) extend this theoretical framework to show that full capitalization of state aid implies efficient school spending, so they conclude from their empirical analysis that state aid is valued by potential residents and that school districts do not overspend on education. Under admittedly stringent assumptions, on which we provide more discussion in the concluding section of this paper, full capitalization of central government grants would similarly imply that Dutch municipalities do not overspend on local public goods. Such insights into the social value of local public expenditure are of particular relevance in view of current pressure on central and local government budgets in the wake of the Great Recession.

The next section provides relevant information on the institutional setting and on the reform of grant allocation. Our empirical analysis is contained in Section 3 and policy implications are discussed in the concluding section.

2 Institutional setting and reform of grant allocation

2.1 Local government in the Netherlands

The Netherlands is a highly centralized country in the sense that many policies are determined at the national level, yet the execution of those policies is often devolved to municipalities.⁵ For instance, municipalities administer welfare, yet the norms for assignment and benefit levels are determined nationally (Toolsema and Allers, 2012). About a third of the budget of municipalities is spent on welfare and social services. Other important tasks include involvement in spatial planning, urban renewal, local infrastructure, waste collection and disposal, health care and cultural facilities. Unlike countries such as the US and the UK,

⁵ Allers (2011) and Bos (2012) describe task assignment and local government finance in the Netherlands.

municipalities are only responsible for the accommodation of schools, not for any other aspects of education.

One important thing to note about the tasks of local governments in the Netherlands is that many of them tend to benefit poorer households more. It is a well known result in the classical theory of fiscal federalism that policies with a redistributive nature cannot be financed from a local tax base without distorting the location choice of households (see e.g., Buchanan, 1950, or Boadway and Flatters, 1982). This institutional setup thus inherently warrants some form of fiscal equalization.

Fiscal equalization in the Netherlands is based on the principle that each municipality should be able to provide the same level of services with the same property tax rate. It reflects the considerable popular resistance to nonmarginal differences in local public services and tax rates, arguably related to the small size and social homogeneity of the Netherlands (Goedhart, 1973). Municipalities rely on central government grants as their main source of revenue, while the income share of local taxes does not exceed 10 per cent on average. The only important local taxes are property taxes. The amount by which municipalities can raise their property tax revenue is regulated by the central government.⁶ Unconditional general grants, which constitute more than a third of municipal revenue on average, are allocated on the basis of extended formulas that take account of local needs through a range of variables. Until the reform in 1997, local tax capacity was not equalized, partly because of its marginal role in local government finance.

The share of local expenditure that can be covered by local taxes is thus exceptionally low from an international perspective (Blöchliger and King, 2006). Furthermore, although unconditional grants constitute the largest single source of municipal income, municipalities are legally bound to perform many tasks assigned to them by the central government. It follows that the amount of funds that municipalities can really spend according to their own preferences is quite limited. In this setting, it is certainly conceivable that a marginal increase in general grants that does not come with additional tasks is spent on services that are efficiently or even underprovided, which would induce more than full capitalization in the economic framework developed by Brueckner (1979, 1982).

⁶ In 2008, the central government introduced a ceiling on the total rise in property tax revenues of all municipalities, which is often binding. Paradoxically, however, individual municipalities can raise property taxes as much as they like. In 2006 and 2007, property tax rates were capped at the municipality level.

2.2 The reform of general grant allocation

In 1997, a new system for the allocation of the general grant was introduced that measured spending needs in a different way, equalized the local property tax base and changed the weights accorded to different types of spending needs.

The preceding grant allocation system measured needs on the basis of regression analysis of municipal spending, in a similar way as in various other countries (see e.g., Ladd, 1994). However, the reverse impact of grant revenue on spending levels makes this approach problematic, particularly in the Netherlands where municipalities rely for a considerable part of their revenues on central government grants.⁷ Hence, the new method was partly based on judgement of acceptable spending levels instead – Boerboom and Huigslout (2008) provide a detailed description.

The 1997 reform introduced the property tax base, which constitutes the main source of local tax revenue, as a new variable in the grant allocation formula. The residential property tax base is equalized for 80 per cent and non-residential property is equalized for 70 per cent, leaving a minor incentive for municipalities to nurture their local tax base.⁸ Note that it is not the actual tax revenue that is equalized but the tax base, as tax rates may be set to match local spending preferences. Notwithstanding this reform, spending needs still dominate the allocation of central government grants, as local tax revenue covers only a small part of municipal expenditure.

At the time of introduction, the new grant allocation formula consisted of about 50 variables. It put less weight than its predecessor on municipality size, while strengthening the equalization of spending needs relating to disparities in socio-economic characteristics (poverty, minorities), physical characteristics (soil structure) and spillovers to adjacent municipalities. These changes were introduced in two stages. About two thirds of the general grant was re-allocated in 1997 and the remaining part of the revision came into effect in 2001. While the first stage focused on measures for socio-economic composition and the burdens on central cities, the second stage covered mainly physical characteristics.

Table 1 reports summary statistics for the permanent changes in grants, divided by the population in 1997, due to these two stages of the reform and Figure 2 provides a scatterplot. These permanent changes are official figures from the Ministry of Interior Affairs (BZK,

⁷ This ‘circularity problem’ has been noticed in the UK as well (Hall *et al.*, 1996).

⁸ During 1997-2000, property value assessment was not yet uniform and municipalities still used different base years. In 2001-2004, every municipality used 1999 as a base year; in 2005 and 2006, the base year was 2003, and in 2007, the base year was 2005. Since 2008, property values are assessed annually, the base year being the year before the fiscal year.

1997, 2000), which computed them by subtracting grant allocation according to the old formula from the allocation according to the new formula. Table 2 shows how they correlate with personal income in 2001. Given the differences in focus of the two stages of the reform, it is not surprising that the 1997 stage shows a particularly strong negative correlation with income, while the correlation of the 2001 stage with income turns out to be positive. However, since the 1997 stage was larger, it dominates the aggregate effect, as was already seen in Figure 1. Furthermore, the two stages are weakly negatively correlated.

Both stages of the reform were introduced gradually. The new formulas came into effect immediately, but the resulting changes in grants to individual municipalities were smoothed out over five year periods using transition grants (BZK, 1996). In the first stage (1997-2001), these transition grants ensured that a municipality's annual grant change resulting from the revision of the allocation system was maximized at 5 per cent. In the fourth and last year of the transition grant, municipalities where the grant changed more than 25 per cent received a redemption fee to buy off future transition grants.⁹ Municipalities which gained as a result of the grant revision received negative transition grants. A similar transition applied in the second stage (2001-2005).¹⁰

We define the variables 'Reform of 1997' and 'Reform of 2001' as the sums of permanent changes due to both stages of the overall reform and the corresponding transition grants, as illustrated in Figure 3 for the municipality of Amsterdam. The variable 'Reforms of 1997 and 2001 jointly' refers to the sum of all four components. In order to avoid simultaneity bias, these variables are all scaled to the population in 1997 rather than to contemporaneous population. Summary statistics are reported in Table 1. Figure 4 shows the reforms of 1997 and 2001 jointly for the four municipalities that received the largest increase or decrease in general grants in either the first or the second stage – excluding the 24 out of 419 municipalities in our sample for which the time pattern of the reform-induced change in grants was atypical because of a redemption fee. Figure 5 shows the reform of 1997 for the municipality of Bloemendaal, which received the largest redemption fee.

⁹ For instance, a municipality losing 25 per cent of its grant in the first stage received a transition grant of 20 per cent of the permanent change in 1997, of 15 per cent in 1998, of 10 per cent in 1999 and of 5 per cent in 2000. A municipality that saw its grant reduced by 40 per cent would be eligible for a positive transition grant during 7 years. In the fourth year, the remaining instalments were bought off.

¹⁰ In the second stage, supplementary grants ensured that grant changes became effective in five annual steps, of 10, 15, 25, 25 and 25 per cent, respectively (BZK, 2000). Municipalities losing more than one hundred guilders (45 euro) per capita received a redemption fee in the fourth year, in a similar way as in stage 1.

2.3 Placebo: decentralization of school accommodation

School buildings for children under 12 used to be financed by the central government, which provided funds to the school boards and organisations that built and maintained schools. This task was decentralized to municipalities in 1997. In order to provide them with the necessary means, the general grant was raised by the amount spent previously by the central government, minus an ‘efficiency deduction’ of about 7 per cent. As the general grant is non-earmarked, municipalities were free to spend the extra funds as they liked, as long as they provided adequate school buildings. Because the extra money came with additional spending responsibilities for municipalities, we would not expect it to be capitalized into house values.

The extra funds were allocated to municipalities according to a formula consisting of 8 variables. This formula was derived in a way similar to the new general grant allocation system that was introduced in 1997 and 2001 (see above), of which it now forms a part.¹¹ The new funding system was introduced gradually, in a way not unlike the gradual introduction of the new general grant allocation system described above. To this end, the funds previously spent by the central government within the territory of each municipality were calculated and subtracted from the new budget per municipality. If the result was negative, the municipality received a transition grant that maximized the annual reduction to 0.75 per cent of the amount previously spent within the municipality. This transition grant was paid out during a maximum of 5 years (1997 up to 2001).¹² If the reduction in funds for school buildings exceeded 3.75 per cent, the redemption fee was spread out over the entire period. Furthermore, the transition grant included some minor additional grants relating to school accommodation and sport parks that could not be separated out. Table 1 reports summary statistics for the permanent change in grants due to the decentralization of school accommodation, for the transition grant and for the sum of these variables, which we define as the ‘Reform of financing school buildings’. Again, these variables are all scaled to the population in 1997.

¹¹ Some of the variables of the school buildings formula, like inhabitants younger than 20 and minorities, were introduced simultaneously with the general grant reform. It is possible, however, to split the weights of these variables into the part which is related to school buildings and the part which is not.

¹² For instance, if a municipality received 2.5 per cent less than was previously spent within its territory, it received, in 1997, a supplementary grant of 1.75 per cent of the amount previously spent; in 1998 it received a supplementary grant of 1.0 per cent of this amount, in 1999 of 0.25 per cent of this amount and in 2000 and 2001 nothing. For technical reasons, municipalities where school building budgets increased could not receive a negative supplementary grant. To avoid negative supplementary grants, every municipality received a positive ‘base grant’, to which the rest of the supplementary grant (positive or negative) was added. This base grant was financed by temporarily (1997-2001) reducing the main budget of the general grant.

3 Empirical analysis

3.1 Specification and identification strategy

The impact of reform-induced changes in central government grants on house prices is estimated using the model

$$\log P_{i,t} = \alpha_i + \beta_t + \gamma_i t + \delta G_{i,t-2} + \varepsilon_{i,t}, \quad (1)$$

where $P_{i,t}$ is a hedonic house price index for municipality i in year t , α is a municipality fixed effect, β is a year fixed effect, t is a linear time trend that is allowed a municipality-specific effect, $G_{i,t-2}$ is the general grant per capita lagged by two years and $\varepsilon_{i,t}$ is an error term.

Following Hilber *et al.* (2011), we use a semi-log specification. Theory predicts a linear relationship (see e.g., Barrow and Rouse, 2004), yet least-squares estimates of a linear model are more sensitive to outliers. Furthermore, the municipality and year fixed effects capture unobserved heterogeneity more accurately in a semi-log model. For instance, changes in macro-economic variables like the interest rate that have proportionate rather than additive effects on house prices are not well controlled for in a linear specification. We will verify that the estimation of a linear model yields statistically significant and quantitatively similar capitalization rates, once proportionate municipality and year fixed effects have been removed from the house price index.

Estimation of the model in Equation (1) by ordinary least squares may yield biased results for various reasons. For instance, if grants increase because the central government devolves a task to municipalities, such as the accommodation of schools, house prices may be unaffected if the additional funds just cover additional expenses. In that case, the unobserved change in tasks that comes with the change in grants creates an omitted variables bias. It is also possible that changes in the socio-economic composition of a municipality affect both grants and house prices. For instance, a rise in the share of rich households will reduce the amount of grants through the fiscal equalization scheme, while it may also raise house prices through peer group effects or other positive externalities.

Our strategy for dealing with endogeneity of general grants per capita consists of two main ingredients. In the first place, we instrument this variable with the change in grants that is induced by the reform of the general grant allocation. Identification on changes in grants due to this reform does not suffer from bias from omitted tasks, because it left tasks unaffected. The reform also predates dynamics in socio-economic composition that is contemporaneous with the dependent variable, because changes in grants were already fixed

before it was implemented.¹³ Hence, it cannot be the case that contemporaneous changes in the socio-economic composition of a municipality drive both the reform and changes in house prices. Contemporaneous changes in the socio-economic composition may still be driven by reform-induced changes in grants, so that variables that correlate with socio-economic composition are ‘bad controls’.¹⁴ Contrary to Barrow and Rouse (2004) and Hilber *et al.* (2011), we therefore choose to exclude socio-economic or demographic control variables from our specification.

We have to account for the possibility, however, that the reform correlates systematically with subsequent local house prices through some other channel than capitalization. For instance, its equalizing nature makes it likely to favour municipalities with low house prices. The municipality fixed effects control for this correlation to the extent that it is time-invariant. The reform may also favour municipalities in which house prices trend downwards, possibly as a result of deteriorating socio-economic conditions. In order to account for this potential source of bias, the municipality-specific linear time trends in our model are the second main ingredient in our identification strategy. As Figures 3 and 4 illustrate, the reform-induced change in grants varies nonlinearly over time. Our estimates of capitalization are thus identified on deviations of this pattern from a linear time trend. The exclusion restriction is that after controlling for municipality and year fixed effects, such deviations do not correlate systematically with house price deviations from a linear time trend through other channels than capitalization. While one could imagine the general grant reform to target municipalities on a downward trajectory, it is difficult to believe that policymakers targeted on predicted nonlinearities in this trajectory.

We validate our exclusion restriction in two alternative ways. The two stages in which the reform was implemented focussed on the equalization of different types of variables and as seen in Table 2 and Figure 2, these stages are only weakly correlated. Hence, it is unlikely that if correlation of the reform with subsequent house price dynamics were an issue even after controlling for arbitrary linear time trends, this would have affected each the two stages in the same way. As a first test, we therefore instrument general grants with either of these two stages separately and compare results. Our second test is the use of a placebo: we

¹³ Transition grants were based on municipal characteristics in the last year before each of the two stages of the grant revision came into effect. As a result, they reflect only changes in allocation formulas, not changes in local characteristics. Furthermore, the first stage of the revision was based on an analysis of realized spending in 1990 and budgeted spending for 1992, while the second stage was based on an analysis of realized spending in 1995 and budgeted spending for 1997 (Van Zaalén, 2002).

¹⁴ Essentially, the problem is that these control variables may pick up some of the effect of the change in grants on house prices. See Angrist and Pischke (2009) for an insightful discussion of this bad control problem.

instrument general grants with the reform of financing school buildings. A finding of substantial capitalization would invalidate our strategy, as most of the additional funds had to be spent on an additional task, leaving little financial headroom for the improvement of local public services or a cut in local tax rates.

A final remark on our identification strategy is that it would appear to sit uncomfortably with the forward looking nature of capitalization. Forward looking behaviour would imply that any new information on the reform should have capitalized as soon as it became publicly available. Since the structure of the reform was already by and large decided on prior to 1995, this would mean that most of the capitalization had already occurred before the start of our house price index. However, few Dutch citizens realize the great extent to which their municipalities are financed through central government grants. It seems that most people only started noticing the reform when its effect was felt through a change in public service levels or in property taxes. The public outcry when Wassenaar, a municipality that experienced one of the largest reductions in general grants, raised property taxes sharply in 2003 is exemplary in this respect: in a world with perfect information and forward looking behaviour, this outcry should have occurred at least seven years earlier.¹⁵ More systematic support will be provided by our finding that capitalization effects are strongest two years after the reform-induced change in grants has occurred. This seems a plausible time lag for adjusting public service levels and property tax rates. However, to the extent that information on the reform did already capitalize when it was announced, our estimate of the capitalization rate is conservative.

3.2 Data

We construct a hedonic house price index for the period 1995-2010. During this period, a number of municipalities merged or amalgamated. We use the municipal borders that existed in 2010 for the entire period. Excluding the five northern island municipalities and merging a few very small municipalities leaves us with a sample of 419 municipalities out of a total of 430. We observe all housing transactions that were conducted by members of the Dutch Association of Realtors (NVM), which covers the majority of all owner-occupied housing transactions in the Netherlands. Transactions of both single family units and apartments for permanent residence are considered, while dwellings on land lease are excluded. We impute dwellings as not being on leased land if this information is missing and if less than 5 per cent

¹⁵ Wassenaar managed to put off adapting to the lower grant until 2003 by consuming its substantial financial reserves.

of all dwellings in the municipality are on leased land. This leaves a sample of 1,614,735 observations, or 241 transactions per municipality/year combination on average. Table 1 provides summary statistics on the number of transactions per municipality and year.

The index is obtained by regressing the log of the transaction price in 2010 euro's on a large number of characteristics of the dwelling and on 16×419 municipality/year-specific fixed effects. The dwelling characteristics include plot size (for single family units), size of the dwelling, volume, number of rooms, kitchens and bathrooms, number of floors, dwelling type, period of construction, availability of a balcony or garden, parking space, quality of maintenance and location. Attribute effects are differentiated for single family units and apartments whenever relevant. The price index is constructed from the fixed effects and scaled in such a way that its transactions-weighted average corresponds with the average price in our sample of housing transactions.

General grants are observed for the period 1992-2010. All our data for general grants, reforms and transition grants are supplied by the Ministry of Interior Affairs. We scale them up to the 2010 classification of municipalities, which in general is a matter of adding up figures since most changes in municipal boundaries are mergers. General grants and transition grant amounts are inflated to 2010 euros on an annual basis. Permanent changes are inflated to 2010 euros in the year in which they are introduced. Subsequent years are not deflated relative to this base year as it is assumed that inflation correction occurred through the link of the general grant with central government finances.

Data on population, new construction, the stock of housing units and income are obtained from Statistics Netherlands. Our income measure refers to the average disposable income for persons who had a job throughout the year, which is available from 2001 onwards.

3.3 Results on capitalization

Table 3 contains our baseline results. Estimating the model in Equation (1) with OLS while excluding linear time trends yields a weakly significant positive association between the house price index and general grants per capita, as seen in the first column. Entering linear trends provides a first pass at controlling for the unobserved heterogeneity that may bias this estimate. The second column shows that doing so indeed both raises the coefficient and reduces the standard error of the estimate. However, these linear trends may still be a poor control for omitted tasks to the extent that variation in grants was driven by decentralization of tasks or by changes in the socio-economic composition of municipalities. These potential source of bias are removed by instrumenting grants with the reform. Columns (3) and (4)

show that doing so raises the coefficient considerably, consistent with our expectations. Results are hardly sensitive to the inclusion of linear time trends, however, suggesting that systematic correlation of the reform with trends in house prices is not an issue. The Kleibergen-Paap statistic indicates that these estimates are strongly identified. Furthermore, first stage results indicate that, as expected, an additional euro of reform-induced change in grants corresponds to about one additional euro in general grants.

Quantitatively, the coefficient in column (4) of Table 3 implies that a 1 euro rise in grants per capita increases house prices by 0.0333 per cent. At a weighted average transaction price of 235,850 euro, this corresponds with an increase of about 78 euro. Figure 6 shows the implied reform-induced house price change per municipality. Municipalities at the 90th percentile of the distribution of reform-induced changes in grants gained 4,273 euro per dwelling, while losses amounted to 5,916 euro per dwelling at the 10th percentile of this distribution. Using a real discount rate of 3 per cent, the present value of a 1 euro rise in annual grants equals 77 euro for an average household of 2.3 persons, implying full capitalization of central government grants.¹⁶ In the 95 per cent confidence interval surrounding this estimate, capitalization rates could be at most 29 per cent higher or lower. Furthermore, using a discount rate of 4 per cent would raise the capitalization rate with about 33 per cent.

Results of identifying grants on either of the two reforms are reported in Table 4. Both instruments still yield sufficiently strong identification. Estimates from the specifications that include linear time trends are statistically significant and of similar magnitude as our preferred estimate in column (4) of Table 3. More specifically, neither specification rejects our preferred estimate at a significance level of 5 per cent. The negative and insignificant estimate of grants identified on the 2001 stage when linear trends are excluded from the model indicates systematic correlation between this stage and trends in house prices, while this does not appear to be an issue for the larger part of the reform that took place in 1997.

Our placebo analysis is reported in Table 5. The reform of financing school buildings identifies changes in grants with sufficient strength, yet it does not yield a statistically significant impact of grants on house prices. Our preferred estimate is rejected at a 1 per cent level of significance for the specification that includes linear time trends. These results are

¹⁶ Over the period 1995-2010, the real rate of return on Dutch government bonds with a ten year maturity was about 2.5 per cent on average. Uncertainty about future changes in the allocation of grants may warrant a considerable risk premium on top of this rate, so the 3 per cent discount rate is likely conservative.

consistent with our prior expectation that additional grants should not capitalize in house prices to the extent that they come with additional tasks.

Appendix Tables 1 to 5 report various alternative robustness checks that overall support our main results. In the first of these tables, we estimate a linear model. Proportionate municipality and year fixed effects are removed from the house price index first by regressing the logarithm of this index on municipality and year fixed effects. The residual is then transformed to levels in such a way its transactions-weighted average corresponds with the average price in our sample of housing transactions. Appendix Table 1 reports estimates of variants of Equation (1) in which this index replaces the dependent variable. Results are virtual identical to the baseline results in Table 3. In particular, the estimate for our preferred specification indicates that a 1 euro rise in grants per capita increases house prices by about 78 euro.

Experiments with alternative time lags are shown in Appendix Table 2. Consistent with our expectation that capitalization occurs only when public service levels or tax rates have been adjusted, the capitalization effect is strongest for a 2 year time lag.

Appendix Table 3 explores how our results are affected by the peaks in the reform-induced change in grants for municipalities that received a redemption fee. In the first two columns, we estimate baseline specifications for the subsample of municipalities that did not receive redemption fees. Alternatively, we show results for a specification in which the redemption fee is removed from our instrument in columns (3) and (4), or in which it is smoothed out over subsequent years in such a way that municipality's reform-induced annual grant change is maximized at 5 per cent in columns (5) and (6). The last two variants are illustrated for the municipality of Bloemendaal in Figure 5. Overall, it appears that estimates are somewhat higher than our baseline estimate, particularly for the specifications that include linear time trends. This may be explained by the fact that, unlike permanent changes, gradually introduced through transition grants, the redemption fee was paid out only once. An annual income stream of one euro has a much higher present value than a once-only grant of one euro.

Since the transition grant for the reform of school finances included some minor additional grants relating to school accommodation and sport parks that could not be separated out, we verify in Appendix Table 4 that grants do not capitalize in house prices if we identify them on the reform excluding transition grants. While the transition grant for the reform of school buildings does not formally belong to the general grant, we have added it to the general grant throughout the analysis in order to be able deal with the reform of school

finances in a consistent way. In this table, however, we exclude the transition grant from the general grant as well.

Finally, Appendix Table 5 contains a host of alternative robustness checks. The first column of this table shows results for a hedonic price index that was estimated on the 1,337,728 transactions for which land lease status was not imputed. The second column verifies that our baseline result is unaffected by including the transition grant for the school accommodation reform in the general grant. The four largest municipalities of Amsterdam, Rotterdam, The Hague and Utrecht are left out of the sample in column (3), as these municipalities negotiate separately over grants with the central government. Municipalities that received a bailout at some point during our sample period are left out in column (4). Column (5) verifies robustness to leaving out observations for which the house price index was based on no more than 20 observations. Finally, in columns (6) and (7), observations are weighted with the average number of housing transactions per municipality and the number of inhabitants in 1997 respectively. All specifications yield statistically significant estimates and none of them rejects our preferred estimate in column (4) of Table 3 at a significance level of 5 per cent.

3.4 Results for housing supply

One of the factors that may explain the high rate of capitalization is inelastic housing supply. In a world in which households vary in their tastes for heterogeneous places, local housing demand is downward sloping.¹⁷ Hence, the price effect of shifts in demand depends negatively on supply elasticity. For example, Hilber and Mayer (2009) show that towns in Massachusetts with little undeveloped land, and therefore less elastic housing supply, have larger changes in house prices in response to an exogenous shock in school spending. Hilber and Vermeulen (2012) find that the price effect of changes in earnings is larger in English local authorities where housing supply is constrained by regulatory or physical barriers. Vermeulen and Rouwendal (2007) report that housing supply in the Netherlands is almost perfectly inelastic, arguably as a consequence of restrictive land use regulation. While their estimate is based on aggregate time series analysis, the reform-induced change in grants enables us to obtain a causal estimate of housing supply elasticity at the municipal level.

We investigate the impact of house prices on supply in two alternative specifications. First, Equation (1) is modified as

¹⁷ See for instance Aura and Davidoff (2008), Gyourko *et al.* (forthcoming) or Hilber and Robert-Nicoud (2013) for formal models.

$$\log H_{i,t} = \alpha_i + \beta_t + \gamma_i t + \delta \log P_{i,t} + \varepsilon_{i,t}, \quad (2)$$

where $H_{i,t}$ is the housing stock in municipality i in year t and the interpretation of other symbols remains unchanged. Consistent with our base specification, the endogenous price level is instrumented with the two years lagged reform-induced change in grants. The corresponding exclusion restriction is that after controlling for municipality and year fixed effects and municipality-specific linear time trends, this reform does not correlate systematically with the housing stock through other channels than the price response.

Because new construction must be the main driver of variation in the housing stock over time, we alternatively estimate a direct model for the rate of new construction. Denoting the number of newly constructed units by $C_{i,t}$, we specify this model as

$$C_{i,t}/H_{i,t-1} = \alpha_i + \beta_t + \gamma \Delta \log P_{i,t-1} + \varepsilon_{i,t}. \quad (3)$$

Note that this model is written in first differences. Hence, we do not enter municipality-specific time trends, as the municipality fixed effect α now captures the same unobserved heterogeneity as linear trends in a model in levels.

Tables 6 and 7 report estimation results for Equations (2) and (3) respectively, following essentially the same setup as in our baseline Table 3. As expected on the basis of the preceding analysis of capitalization, the reform-induced change in grants strongly identifies house prices in specifications based on Equation (2). Kleibergen-Paap statistics in Table 7 are not as high as before, but still above the benchmark value of 10. Point estimates of the supply response on house prices are negative for all specifications, although the IV estimates are imprecise. Hence, although the reform does not induce sufficient variation in prices to allow for precise estimation of housing supply elasticity, our analysis provides no indication that municipalities that received more grants systematically built more housing.

4 Conclusions and implications

Our empirical analysis implies that a reform that was designed partly to better compensate for disadvantageous socio-economic composition fully capitalized in house prices. This means that homeowners and landlords benefited in municipalities that received more grants, at the expense of property owners in municipalities that saw their grants reduced. However, the reform has left renters in the private sector and future homeowners by and large unaffected, as housing costs offset any changes in public services levels and taxes for them. Renters in the

social sector, which constitutes about a third of the total housing stock in the Netherlands, have gained or lost as a consequence of the reform only to the extent that housing associations have not passed on the change in housing value.

These results illustrate the limitations of using fiscal equalization as an instrument for reducing real income disparities, as property owners are generally not the poorest inhabitants of municipalities that receive more grants.¹⁸ However, in view of the popular resistance to nonmarginal differences in local public services in the Netherlands, the promotion of categorical equity is arguably a more important objective of fiscal equalization. Although the realization of this objective may appear to be less vulnerable to capitalization, the implication is still that improved local services come at the expense of a cut in private consumption for renters in the private sector and for homeowners who enter the market after the introduction of fiscal equalization – i.e. the majority in the long run. Hence, the promotion of categorical equity for these households is only legitimate to the extent that society values their consumption of public services more than the satisfaction of their other needs (Wyckoff, 1995). It is not obvious why this should be the case, since local voters select the politicians that determine local public service provision.

The efficiency rationale for fiscal equalization is less affected by capitalization, as it does not depend on the distribution of benefits within municipalities. One source of inefficiency arises when local governments engage in redistributive policies.¹⁹ Buchanan (1950) already noted that places with a positive ‘fiscal residuum’, for instance because of a large share of rich households, would attract too many households – see Boadway and Flatters (1982) for a formal analysis. This argument seems also relevant in the Dutch context, because of the substantial redistributive tasks assigned to municipalities.²⁰ For instance, without any equalization of spending needs, municipalities with a large share of households on welfare could hardly afford any other public services, thus inefficiently reducing the quality of life. Moreover, a self-reinforcing sorting process could result that would in the end leave some municipalities deprived and incapable of even providing welfare to those entitled to it. The

¹⁸ See e.g., Oakland (1994) and Ladd and Yinger (1994) for more discussion.

¹⁹ The literature provides various alternative efficiency grounds for fiscal equalization, relating for instance to fiscal externalities and rent sharing (Flatters et al., 1974; Boadway and Flatters, 1982; Albouy, 2012) or interjurisdictional insurance (Persson and Tabellini, 1996a and 1996b; Bucovetsky, 1997; Lockwood, 1999). Allers (2012) argues that fiscal equalization enables proper yardstick competition, as fiscal disparities that are not transparent to voters bias interjurisdictional comparisons of public service levels and tax rates.

²⁰ The decentralization of redistributive tasks is not unique for the Netherlands. For instance, Oates (1999) discusses the decentralization of welfare to the state level in the US. More fundamentally, it may be argued that almost any tax or expenditure decision of local governments has distributive implications (Boadway and Wildasin, 1984).

capitalization of an equalizing grant demonstrates its effectiveness in restoring the attractiveness of such places to the marginal homebuyer.

Finally, full capitalization of a marginal euro that comes without spending obligations indicates that overall, this euro is well spent from the perspective of the marginal homebuyer. Only under strong assumptions does this finding imply efficiency of the local public sector. For instance, imperfect mobility would reduce capitalization of the willingness to pay for local public goods, yielding an underestimate of the social value of municipal expenditure. On the other hand, preferences of this marginal homebuyer may not reflect the preferences of other inhabitants.²¹ Furthermore, the willingness to pay for housing could be enhanced by peer group effects or other externalities associated with socio-economic composition of a municipality, if a rise in the level of local public goods would attract rich households, thus yielding an overestimate of the social value of municipal expenditure.²² Bearing in mind these caveats, our estimates still suggest that, overall, residential property tax payers are willing to pay considerably for the services that are generated from one euro of municipal expenditure. It is therefore hard to see why taxing powers of municipalities should be constrained to their present marginal role – particularly in an era in which strong budgetary pressure limits the availability of funds from the central government.

²¹ See Hilber (2011) for more discussion. Note however that education, which is an important category on which spending preferences of the marginal homebuyer and the median voter tend to diverge (Hilber and Mayer, 2009), is financed by the central government in the Netherlands.

²² The empirical relevance of socio-economic sorting according to the quality of local public services is documented in, e.g., Bayer *et al.* (2007).

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TABLE 1
Summary statistics (baseline regression sample)

Variable	Obs.	Mean	Std. Dev.			Min	Max
			overall	between	within		
Panel data							
Real house price index	6704	214951	62705	49030	39158	66915	516630
General grant per capita	6704	0.663	0.168	0.117	0.120	0.288	1.738
Reforms of 1997 and 2001 jointly	6704	-0.009	0.043	0.033	0.027	-0.271	0.237
Reform of 1997	6704	-0.009	0.043	0.034	0.026	-0.264	0.237
Reform of 2001	6704	0.000	0.012	0.007	0.010	-0.136	0.185
Transition grant 1997	6704	0.002	0.020	0.009	0.017	-0.116	0.483
Transition grant 2001	6704	0.000	0.008	0.004	0.007	-0.073	0.322
Reform of financing school buildings	6704	0.044	0.028	0.009	0.026	0.000	0.180
Transition grant for school buildings	6704	0.002	0.006	0.003	0.006	0.000	0.113
Housing stock	6704	15991	28225	28223	1388	1238	394196
New construction	6704	181	351	313	158	0	6442
Number of housing transactions	6704	241	363	343	120	1	3687
Cross-sectional data							
Permanent change 1997	419	-0.015	0.057			-0.264	0.142
Permanent change 2001	419	0.000	0.022			-0.136	0.081
Permanent change for school buildings	419	0.055	0.011			0.032	0.096
Number of inhabitants in 1997	419	37096	56937			4001	715148
Redemption of 1997 transition grants	419	0.003	0.026			0.000	0.346
Redemption of 2001 transition grants	419	0.001	0.012			0.000	0.232
Personal income in 2001	419	20.226	2.041			16.529	31.886

Notes: All grant amounts (including reforms and transition grants) and personal income are expressed in 1000's of 2010 euros. Reforms, permanent changes and transition grants are scaled to the population in 1997.

TABLE 2
Correlations between reforms and income

	Permanent change 1997	Permanent change 2001	Permanent changes in 1997 and 2001 jointly
Permanent change in 2001	-0.221*** 0.000		
Permanent changes in 1997 and 2001 jointly	0.927*** 0.000	0.161*** 0.001	
Log of personal income in 2001	-0.761*** 0.000	0.366*** 0.000	-0.630*** 0.000

Notes: Test statistics for deviation from zero are reported below each correlation coefficient.

Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 3
Baseline results

Panel A - Dependent variable: Log(real house price index)				
	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	0.0639*	0.106***	0.292***	0.333***
	(0.0335)	(0.0281)	(0.0547)	(0.0475)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.388	0.357		
Kleibergen-Paap F			698.0	593.6
Panel B - First stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.959***	1.182***
			(0.0363)	(0.0485)
Municipality fixed effects			Yes	Yes
Year fixed effects			Yes	Yes
Municipality-specific time trends				Yes
R-squared			0.591	0.659

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 4
Identification on 1997 and 2001 reforms separately

Panel A – IV estimates. Dependent variable: Log(real house price index)				
	1997 reform		2001 reform	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	0.345***	0.353***	-0.220	0.253***
	(0.0663)	(0.0553)	(0.192)	(0.0789)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
Kleibergen-Paap F	645.3	471.0	57.73	28.83
Panel B - First stage corresponding to IV results				
Reform of 1997, 2 years lagged	0.890***	1.189***		
	(0.0350)	(0.0548)		
Reform of 2001, 2 years lagged			0.652***	1.455***
			(0.0859)	(0.271)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.591	0.646	0.460	0.658

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 5
Placebo: identification on reform of financing school buildings

Panel A – IV-estimates. Dependent variable: Log(real house price index)		
	(1)	(2)
General grant per capita, 2 years lagged	0.0467 (0.0739)	-0.201 (0.127)
Municipality fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Municipality-specific time trends		Yes
Kleibergen-Paap F	102.4	102.7
Panel B - First stage corresponding to IV results		
Reform of financing school buildings, 2 years lagged	1.917*** (0.189)	1.148*** (0.113)
Municipality fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Municipality-specific time trends		Yes
R-squared	0.513	0.650

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6
Price elasticity of the housing stock

Panel A - Dependent variable: Log(housing stock)				
	OLS		IV	
	(1)	(2)	(3)	(4)
Log(real house price index), 2 years lagged	-0.0281 (0.0198)	-0.00885 (0.00848)	-0.335 (0.213)	-0.0452 (0.0620)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.00151	0.0113		
Kleibergen-Paap F			28.59	36.06
Panel B - First stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.280*** (0.0524)	0.394*** (0.0656)
Municipality fixed effects			Yes	Yes
Year fixed effects			Yes	Yes
Municipality-specific time trends				Yes
R-squared			0.374	0.371

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7
Impact of house price growth on the rate of new construction

Panel A - Dependent variable: New construction divided by the lagged housing stock				
	OLS		IV	
	(1)	(2)	(3)	(4)
Growth rate of the real house price index, 1 year lagged	-0.0119*** (0.00295)	-0.0110*** (0.00262)	-0.109 (0.103)	-0.0450 (0.0640)
Municipality fixed effects		Yes		Yes
Year fixed effects	Yes	Yes	Yes	Yes
R-squared	0.0389	0.0389		
Kleibergen-Paap F			11.78	10.37
Panel B - First stage corresponding to IV results				
First difference of the reforms of 1997 and 2001 jointly, 2 years lagged			0.120*** (0.0350)	0.121*** (0.0375)
Municipality fixed effects				Yes
Year fixed effects			Yes	Yes
R-squared			0.583	0.583

Notes: N = 5866, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 1
Baseline results reproduced for real house price index in levels,
demeaned for years and municipalities

	OLS (1)	(2)	IV (3)	(4)
General grant per capita, 2 years lagged	14439* (7830)	24259*** (6576)	68502*** (12850)	77624*** (11183)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.000452	0.101		
Kleibergen-Paap F			698.0	593.6

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 2
Results for alternative time lags

Number of lags n :	0	1	2	3	4	5
Panel A – IV estimates. Dependent variable: Log(real house price index)						
General grant per capita, n years lagged	0.151*** (0.0404)	0.276*** (0.0398)	0.333*** (0.0475)	0.331*** (0.0691)	0.229*** (0.0713)	0.105* (0.0611)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6704	6704	6704	6704	6285	5866
Kleibergen-Paap F	721.4	649.6	593.6	673.2	924.4	1498
Panel B - First stage corresponding to IV results						
Reforms of 1997 and 2001 jointly, n years lagged	1.123*** (0.0418)	1.170*** (0.0459)	1.182*** (0.0485)	1.150*** (0.0443)	1.095*** (0.0360)	1.025*** (0.0265)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.416	0.568	0.659	0.736	0.750	0.768

Notes: Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 3
Alternative ways to deal with redemption of transition grants

Panel A – IV estimates. Dependent variable: Log(real house price index)						
	Without municipalities receiving redemption		Redemption removed from transition grant		Redemption grant smoothed over time	
	(1)	(2)	(3)	(4)	(5)	(6)
General grant per capita, 2 years lagged	0.401*** (0.0645)	0.398*** (0.0593)	0.320*** (0.0558)	0.445*** (0.0468)	0.324*** (0.0578)	0.461*** (0.0549)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes		Yes
Observations	6320	6320	6704	6704	6704	6704
Kleibergen-Paap F	406.6	456.3	578.7	553.5	541.8	506.4
Panel B - First stage corresponding to IV results						
Reforms of 1997 and 2001 jointly, 2 years lagged	0.997*** (0.0495)	1.303*** (0.0610)	0.957*** (0.0398)	1.275*** (0.0542)	0.951*** (0.0409)	1.276*** (0.0567)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes		Yes
R-squared	0.590	0.654	0.591	0.659	0.590	0.659

Notes: Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 4
Identification on reform of financing school buildings, excluding transition grants

Panel A – IV estimates. Dependent variable: Log(real house price index)		
	(1)	(2)
General grant per capita, 2 years lagged	0.129 (0.0794)	-0.178 (0.143)
Municipality fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Municipality-specific time trends		Yes
Kleibergen-Paap F	174.0	53.87
Panel B - First stage corresponding to IV results		
Reform of financing school buildings, excluding transition grants, 2 years lagged	3.053*** (0.231)	1.751*** (0.239)
Municipality fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Municipality-specific time trends		Yes
R-squared	0.524	0.648

Notes: N = 6704, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLE 5
Miscellaneous robustness checks

Panel A – IV estimates. Dependent variable: Log(real house price index)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
General grant per capita, n years lagged	0.330*** (0.0482)	0.333*** (0.0473)	0.346*** (0.0489)	0.325*** (0.0474)	0.285*** (0.0479)	0.227*** (0.0645)	0.205*** (0.0776)
Municip. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6704	6704	6640	6160	6379	6704	6704
Kleibergen-Paap F	593.6	605.5	617.6	584.7	551.7	125.2	92.17
Panel B - First stage corresponding to IV results							
Reforms of 1997 and 2001 jointly, 2 years lagged	1.181*** (0.0485)	1.184*** (0.0481)	1.173*** (0.0472)	1.192*** (0.0493)	1.186*** (0.0505)	1.142*** (0.102)	1.195*** (0.125)
Municip. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.659	0.659	0.722	0.672	0.638	0.652	0.651

Notes: Statistics are robust to heteroskedasticity and within-municipality correlation.

Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The columns of this table correspond to the following robustness checks:

- (1) No imputation of zero for missing observations for land lease in municipalities with less than 5% land lease;
- (2) Removal of transition grant for financing school buildings from general grant;
- (3) Four largest municipalities are removed from the sample;
- (4) Municipalities that received a bailout are removed from the sample;
- (5) Observations that are based on no more than 20 housing transactions are removed from the sample;
- (6) Observations are weighted by the average number of housing transactions per municipality;
- (7) Observations are weighted by the number of inhabitants in 1997.

FIGURE 1
Reform-induced change in grants scattered against personal income

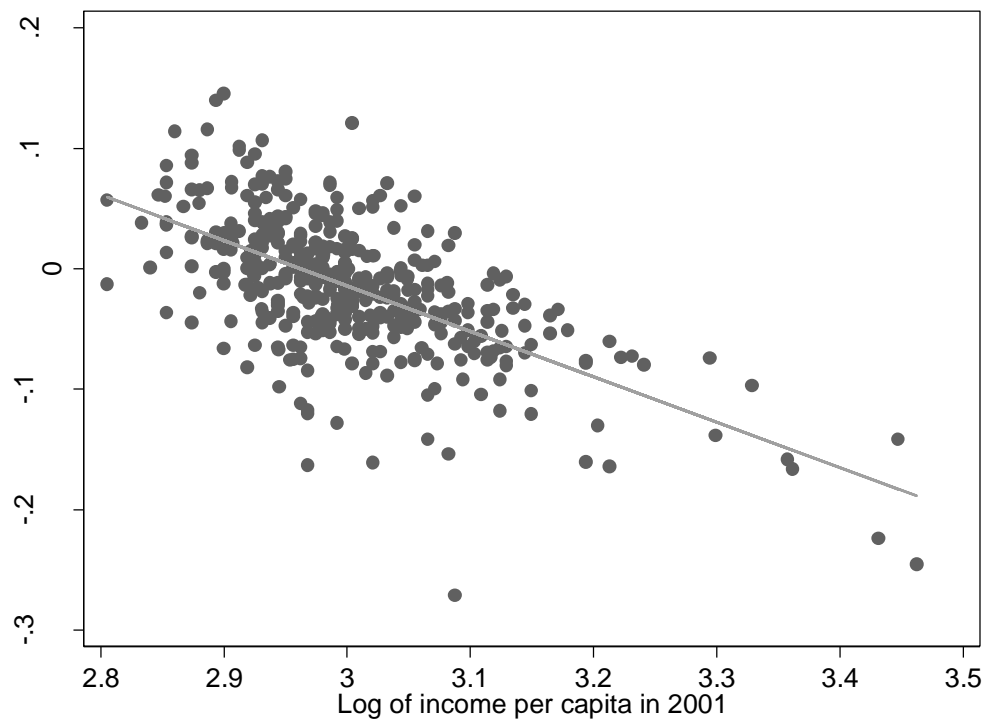


FIGURE 2
Permanent change in 2001 scattered against permanent change in 1997

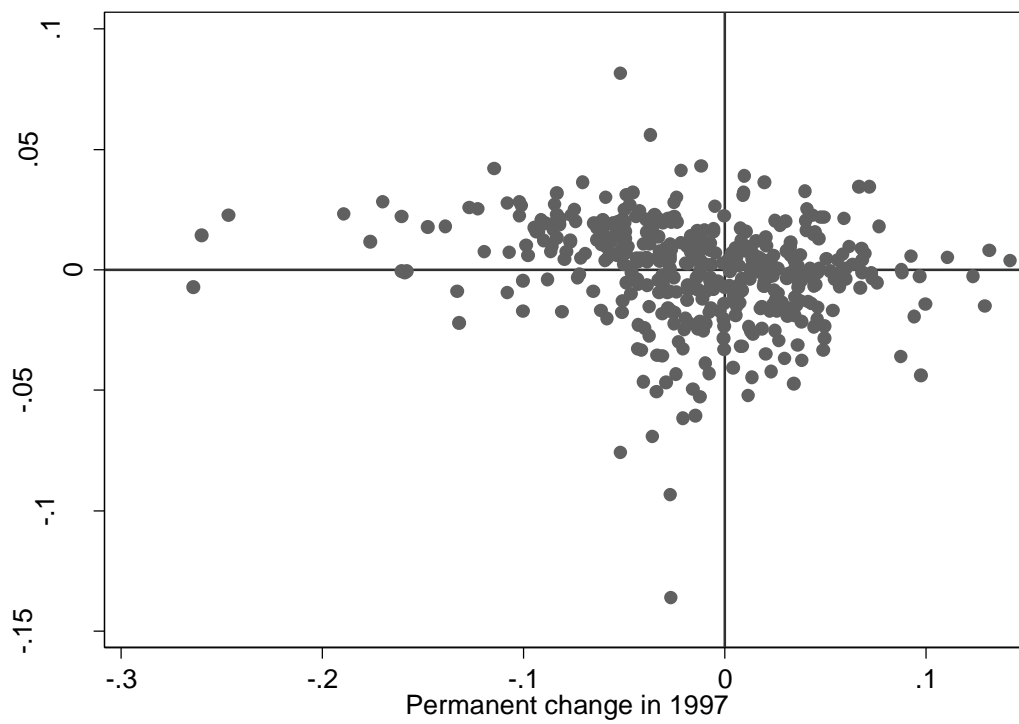


FIGURE 3

Reforms of 1997 and 2001 separately for the municipality of Amsterdam

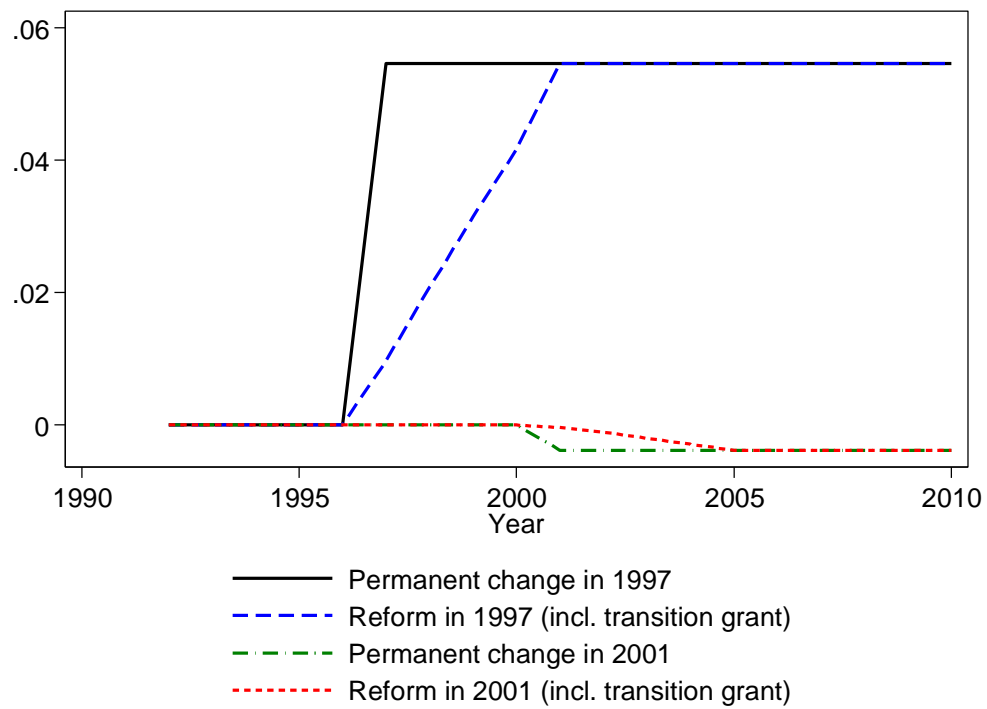


FIGURE 4

Reforms of 1997 and 2001 jointly for four municipalities

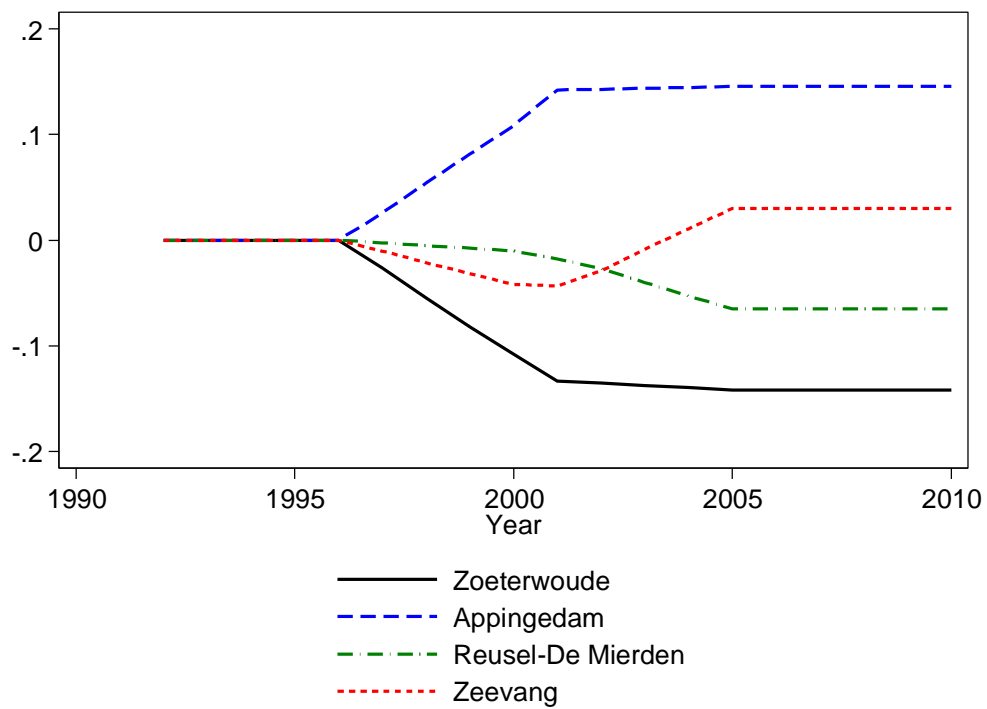


FIGURE 5
Reform of 1997 for municipality of Bloemendaal

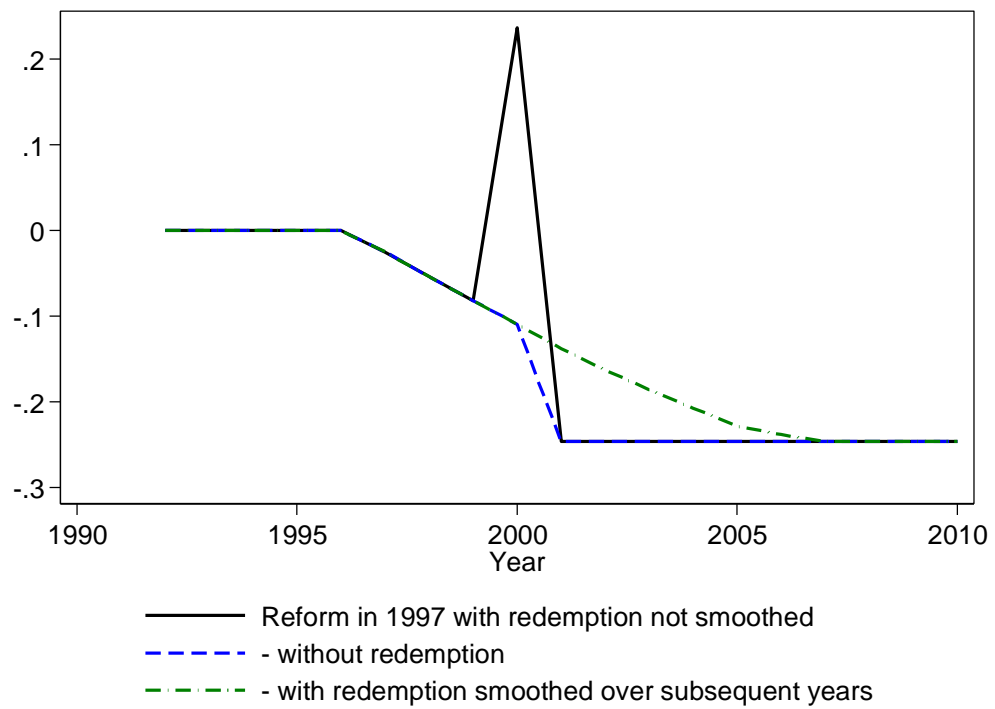
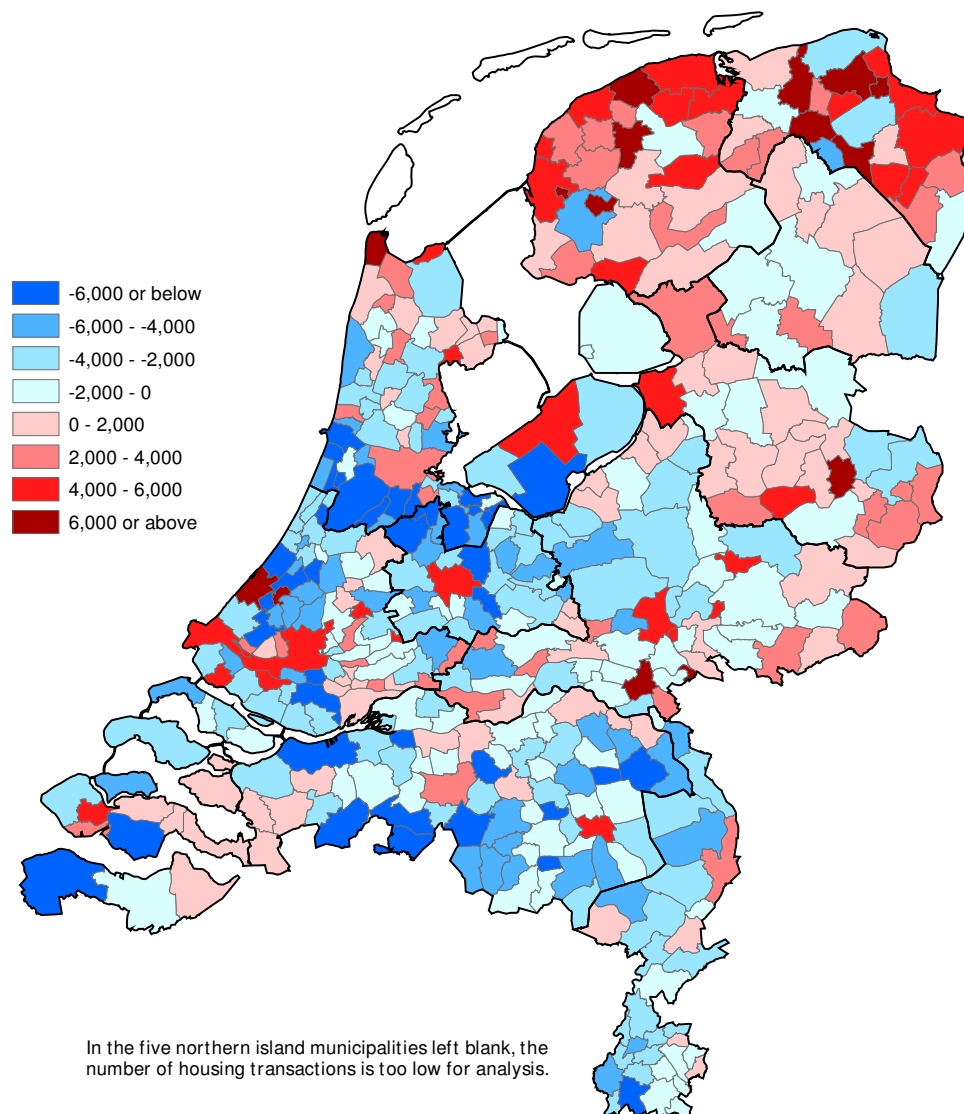


FIGURE 6
Estimated effect on house prices of 1997 and 2001 reforms jointly (in 2010 euros)





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