Capitalization of equalizing grants and the flypaper effect ☆

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ABSTRACT

The wide-ranging empirical support for the flypaper effect in local public finance remains a puzzle in spite of various theoretical explanations. We exploit a reform of the fiscal equalization system in the Netherlands to show that the resulting change in grants to municipalities was fully capitalized into local house prices. Nevertheless, only a small fraction was passed on to residents through property taxes, indicating local public service provision as the main adjustment channel. As the marginal homebuyer was apparently willing to pay for these services, capitalization makes rent seeking by local politicians or bureaucrats improbable — thus effectively ruling out one class of explanations for the flypaper effect. The absence of a significant effect on municipal staff provides further evidence against a bureaucratic flypaper effect.

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1. Introduction

Local governments tend to use grants from the central government mostly to increase spending levels. This flypaper effect – money sticks where it hits – has been dubbed an anomaly, as the propensity to spend private income on local public services is much lower.[1] While empirical evidence of the flypaper effect is widespread, its explanation remains unclear. Various theories have been proposed, but none of these is broadly supported by empirical evidence.[2] This paper proposes a novel empirical test for one class of explanations: those based on rent seeking.

The gist of this class of explanations is that citizens are poorly informed about the grant amount received by the community they inhabit, enabling local politicians or bureaucrats to spend some of these grants for their own benefit. A flypaper effect arises as the rents that are extracted from an increase in grants do not accrue to inhabitants through a local tax cut. Based on the seminal work by Niskanen (1971), early contributions in this vein focus on the role of budget-maximizing bureaucrats (Romer and Rosenthal, 1979; Wyckoff, 1988). More recently, building on the political agency model with career concerns by Persson and Tabellini (2000), Brollo et al. (2013) propose a theory in which additional grants allow self-interested politicians to extract more rents from uninformed voters.

Empirically, the disappointing impact of non-tax increases in local government revenues on public service provision and its adverse effect on corruption are well documented for developing countries.[3] Also for the US, Fisman and Gatti (2002) find a strong positive relationship between the proportion of a state's expenditures derived from federal transfers and corruption, as measured by the number convictions of public employees for abuse of public office. More direct evidence on rent seeking is provided by Leduc and Wilson (2015), who find that political contributions from the public-works sector increased dramatically under the American Recovery and Reinvestment Act and that states with more political contributions spent more out of the highway funds made available through this act.[4]

If additional grants accrue to local bureaucrats or politicians as rents, they are unlikely to make local communities more attractive to potential homebuyers. Hence, the empirical relevance of explanations based on

[1] As grants can be converted into private income through the adjustment of local taxes, conventional economic theory predicts that the marginal propensity to spend on local public services is the same for grants as it is for private income, for which 10% is a plausible upper estimate (Inman, 2008).

[2] Hines and Thaler (1995) and Inman (2008) provide an overview of theoretical explanations for the flypaper effect, as well as a survey of earlier empirical work. See e.g. Dahlberg et al. (2008); Lutz (2010); Dahlby (2011); Lundqvist (2015) or Gennari and Messina (2014) for recent contributions.

[3] See for instance Olken (2007); Brollo et al. (2013); Caselli and Michaels (2013); Gadenne (2015) and Martínez (2015).

[4] See also Singhal (2008) on the influence of special interest groups on expenditure out of windfalls to states. She finds that states involved in settlement lawsuits with the tobacco industry, where anti-tobacco interest groups have presumably exerted more effort, spent more out of the resulting windfall on tobacco control programs.

rent seeking may be tested by estimating the extent to which changes in grants capitalize into house prices. Under admittedly stringent conditions, full capitalization implies allocative efficiency of local public service provision, as the marginal euro of grants is converted into one euro of value for residents — consistent with the Samuelson condition.[5] While this result does not hold up in a sorting framework with heterogeneous tastes for local public goods and for locations (see e.g., Bayer et al., 2007), capitalization still indicates the value that the marginal homebuyer attaches to how the marginal euro of grants is spent.[6]

Our paper is the first to relate the flypaper effect to house price capitalization. Exploiting a reform of the fiscal equalization system in the Netherlands, we confirm that exogenous changes in grants are not passed on to residents through local taxes. Residential property tax revenue, which is the only relevant source of local tax revenue in this country, falls with only about 16 cents for each euro of additional grants. While this result may not come as a surprise in view of the voluminous literature on the flypaper effect, we also find that local house prices are strongly affected. Our main estimate indicates full capitalization of exogenous grant changes at a real discount rate of 3%. Apparently, the marginal homebuyer was willing to pay for whatever local governments spent the reform-induced changes in grants on. This is inconsistent with theories that explain the flypaper effect through rent taking by bureaucrats or politicians.

The reform entailed a complete overhaul of the allocation formula for unconditional general grants to municipalities, increasing the weight of socioeconomic characteristics and introducing partial equalization of tax capacity. Reform-induced changes in annual grants varied considerably over municipalities, with a standard deviation of 56 euro per inhabitant. Our identification is based on a non-linearity in the time pattern of these changes. In order to allow municipalities to adjust their policies, transition grants were used to cushion changes and the reform was carried out in two stages, starting in 1997 and 2001, respectively, that targeted different categories of variables in the allocation formula. The resulting nonlinear time pattern allows us to control for any municipality-specific linear trends in unobserved variables that may correlate with grants and local taxes or house prices.

We validate our identification strategy by considering a placebo reform, which made municipalities responsible for school buildings and provided the corresponding resources. As expected, changes in grants induced by this reform do not significantly affect local taxes or house prices because, in contrast to the general grant reform, this reform only changed the way of funding a service that was already provided. Most of the extra money could therefore not be spent on improving public services or lowering tax rates.

As a corroborative test for the absence of a bureaucratic flypaper effect, we also explore the impact of the reform-induced change in grants on the number of municipal civil servants. We do not find a statistically significant effect, while our estimate is sufficiently precise to rule out an economically meaningful effect.[7] Although this finding on its own does not rule out other types of rent extraction, additional grants clearly have not been used to expand local bureaucracies in a substantial way.

The capitalization of grants to local governments is studied in two earlier papers.[8] Hilber et al. (2011) find evidence of substantial to full capitalization in a study of central government grants to local authorities in England. In their analysis, electoral targeting of grants by the incumbent Labour party provides the source of exogenous variation. 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. While our main contribution lies in relating capitalization to the flypaper effect as way to narrow down the list of potential explanations, our identification strategy is arguably also more robust than in this earlier work.[9]

2. Institutional setting and reform of grant allocation

2.1. Local government in the Netherlands

The Netherlands is a highly centralized country. Even though many governmental tasks have been assigned to municipalities, national regulation and guidelines ensure that basic public services are to a large extent uniform across the country.[10] For instance, municipalities administer welfare, yet the norms for assignment and benefit levels are set nationally (Toolsema and Allers, 2014). About a third of the budget of municipalities is spent on welfare and social services. Other important tasks concern spatial planning, urban renewal, local infrastructure, waste collection and disposal, health care and cultural and recreational facilities. In contrast with many other countries, municipal responsibilities in the domain of education are mainly limited to the construction and upkeep of school buildings.

A large proportion of these tasks benefits poorer households disproportionally. It is a well-known tenet of the classical theory of fiscal federalism that policies with a redistributive nature cannot be financed by local taxes without distorting the location choice of households (see e.g., Buchanan, 1950, or Boadway and Flatters, 1982). This institutional setup thus inherently requires 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 at the same property tax rate. It reflects the considerable popular resistance to nonmarginal differences in local public services and taxes in this country, arguably related to its small size and social homogeneity (Goedhart, 1973). Unconditional general grants, which constitute more than a third of municipal revenue on average, are allocated on the basis of extensive formulas that take account of local needs through a wide range of variables. Equalization of local tax capacity was introduced in the 1997 grant reform.

Differences across municipalities are further diminished as central government grants constitute the main source of municipal revenue. Apart from an equalizing unconditional grant, municipalities receive many different conditional grants to finance mandated tasks. The share of local expenditure covered by local taxes does not exceed 10%

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[5] See Brueckner (1979, 1982). Barrow and Rouse (2004) adapt this theoretical framework to equalizing grants.

[6] In such a framework, the capitalization rate is larger in places where housing supply is constrained (see Hilber, forthcoming; Hilber and Vermeulen, 2016). Housing is inelastically supplied in the Netherlands (Vermeulen and Rouwendal, 2007), so that capitalization of the value that people attach to local services is likely.

[7] Using Swedish data, Lundqvist et al. (2014) also find that general grants do not stimulate employment in the local public sector in general, although they do find a positive and statistically significant effect on administrative personnel.

[8] In general, there is a rich literature on the capitalization of fiscal differentials into house prices. See Chaudry-Shah (1988); Ross and Yinger (1999) and Hilber (forthcoming) for surveys.

[9] 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% to 50% is found to reduce grants per capita by 13 British pound (year 2008), corresponding to 16 euros (year 2010). This figure may be contrasted to the standard deviation of reform-induced changes in annual grants of 56 euro per inhabitant in our analysis. Hence, the variation in reform-induced changes in grants in our analysis is considerably larger, enabling stronger and more robust identification. The validity of the identification strategy in Barrow and Rouse (2004) relies on the assumption that changes in district housing values are not correlated with the pre-reform district characteristics that are used to construct the instrument through other channels than capitalization. Our identification requires less restrictive assumptions in this respect, as we control for arbitrary linear time trends in grants and house prices.

[10] Municipal expenditure constitutes about 10% of GDP. See Allers (2011) and Bos (2012) for a detailed discussion of task assignment and local government finance in the Netherlands.

Table 1
Timing of the reform.

Year	Information availability	Local spending data from this year used to develop new allocation	Reform	
			Permanent change	Transition grants
1990	Study on municipal spending needs announced	Yes (stage 1)		
1991	Pilot study published: proposed research method found feasible			
1992	Preliminary spending needs study published	Yes (stage 1)		
1993	Final spending needs study published			
1994	Proposal for stage 1 published; first tentative outcomes for individual municipalities available. Draft law published			
1995	Revised draft law published, with provisional outcomes for individual municipalities	Yes (stage 2)		
1996	Stage 1 of allocation reform enshrined in legislation (October)			
1997		Yes (stage 2)	Stage 1	Yes (stage 1)
1998				Yes (stage 1)
1999	Proposal for stage 2 published, with tentative outcomes for individual municipalities.			Yes (stage 1)
2000	Final outcomes stage 2 published (May)			Yes (stage 1)
2001			Stage 2	Yes (stage 2)
2002				Yes (stage 2)
2003				Yes (stage 2)
2004				Yes (stage 2)

on average, which is exceptionally low from an international perspective (Blöchliger and King, 2006). As a result, municipalities have little income discretion. The only important local taxes are property taxes.[11] Their marginal role implies that a small increase in municipal income through this channel requires a large relative increase in the property tax rate. In order to increase local spending by one per cent, the property tax rate must be raised by 5–42%, depending on the municipality (Allers et al., 2010). The political cost of such an increase is likely to be considerable.[12] Furthermore, as of 2006, the amount by which municipalities can raise their property tax revenue was subject to regulation.[13]

The combination of limited income discretion and a dominant role of centrally assigned tasks may partly explain the high capitalization rate found in this paper. If the amount of funds that municipalities can freely spend according to their own preferences is limited, it is conceivable that a marginal increase in general grants that does not come with additional tasks will be spent on services that are efficiently or even underprovided. Hence, this could induce even more than full capitalization (Brueckner, 1979, 1982).

2.2. The reform of general grant allocation

In 1997, a new system for the allocation of the (unconditional) general grant was introduced that changed the measurement of spending needs, equalized the local property tax base and, most importantly, adjusted 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.[14] Hence, the new

method was partly based on judgement of acceptable spending levels instead — Boerboom and Huigsloot (2008) provide a detailed description.

The 1997 reform introduced the base of the property tax, which constitutes the main source of local tax revenue, as a new variable in the grant allocation formula. Note that it is not actual tax revenue that is equalized but the tax base, and that this base is equalized only partially.[15] Notwithstanding this reform, spending needs still dominate the allocation formula of central government grants, as local tax revenue covers only a small part of municipal expenditure.

When it was introduced, 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 socioeconomic characteristics (poverty, minorities), physical characteristics (soil structure) and catchment areas. These changes were introduced in two stages. About two thirds of the re-allocation of general grants took place in the 1997 stage and the remaining part of the revision came into effect in the 2001 stage. While the first stage focused on measures for socioeconomic composition and the burdens on central cities, the second stage covered mainly physical characteristics. The exact timing of all relevant steps of the reform is specified in Table 1.

Summary statistics for the permanent changes in grants due to these two stages of the reform, divided by the population in 1997, are reported in Table 2. The redistributive nature of the reform is illustrated in Fig. 1, which scatters the per capita change in grants resulting from both reforms together against personal income in 2001. 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. Correlations with personal income in 2001 are reported for each stage separately in Table 3. Given the differences in focus, it is not surprising that the 1997 stage shows a particularly strong negative correlation with income, whereas the correlation of the 2001 stage with income turns out to be positive. The 1997 stage dominates the aggregate effect as this reform was larger.

A scatter of the 2001 stage-induced change in grants against the 1997 stage-induced change is shown in Fig. 2. As seen in this figure and in Table 3, the two stages are weakly negatively correlated. By

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[11] The average tax rate on residential property is about 0.1%, but rates vary considerably across municipalities. See Allers et al. (2010) for a detailed overview for 2010.
[12] Allers and Elhorst (2005) provide evidence of yardstick competition on property tax rates in the Netherlands. The political costs are likely to be enhanced by the salience of the property tax (Cabral and Hoxby, 2012).
[13] In 2006 and 2007, property tax rates were capped at the municipality level. In 2008, an informal agreement between the association of municipalities and the central government introduced a ceiling on the total rise in property tax revenues of all municipalities. Paradoxically, however, individual municipalities can raise property taxes as much as they like.
[14] This 'circularity problem' has been noticed in the UK as well (Hall et al., 1996).

[15] 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.

Table 2
Summary statistics (baseline regression sample).

Variable	Obs.	Mean	Std. dev.			Min	Max
			Overall	Between	Within		
Panel data							
Real house price index	6704	214,951	62,705	49,030	39,158	66,915	516,630
Number of housing transactions	6704	241	363	343	120	1	3687
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
Property tax revenue per capita	5866	0.119	0.038	0.028	0.026	0.034	0.507
Municipal staff per 1000 inhabitants	5445	7.218	2.163	2.067	0.642	0.189	31.847
Share of inhabitants younger than 20	5447	0.252	0.025	0.024	0.006	0.176	0.350
Share of inhabitants older than 65	5447	0.143	0.028	0.025	0.011	0.064	0.269
Share receiving social assistance	5447	0.012	0.009	0.008	0.002	0.002	0.083
Share nonwestern immigrants	5447	0.048	0.045	0.045	0.008	0.004	0.369
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	37,096	56,937			4001	715,148
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), personal income and the property tax revenue are expressed in 1000's of 2010 euros. Municipal staff is measured in full time equivalents. Reforms, permanent changes and transition grants are scaled to the population in 1997.
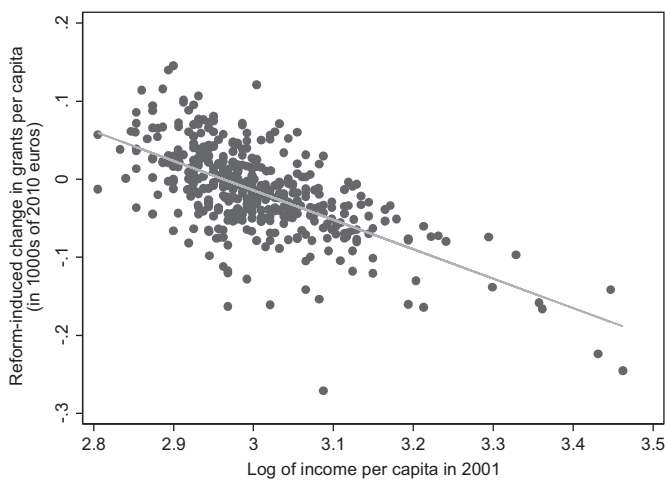
**Fig. 1.** Reform-induced change in grants scattered against personal income.

Table 3
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***	0.161***	
	0.000	0.001	
Log of personal income in 2001	−0.761***	0.366***	−0.630***
	0.000	0.000	0.000

Notes: P-values of a test for deviation from zero are reported below each correlation coefficient. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

implication, we can meaningfully test the robustness of our identification strategy by constructing instruments on the basis of each stage separately.

The reform was introduced gradually in order to allow municipalities to adjust their policies. While the new formulas came into effect immediately, 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%. In the fourth and last year of the transition grant, municipalities where the grant changed more than 25% received a redemption grant to buy off future transition
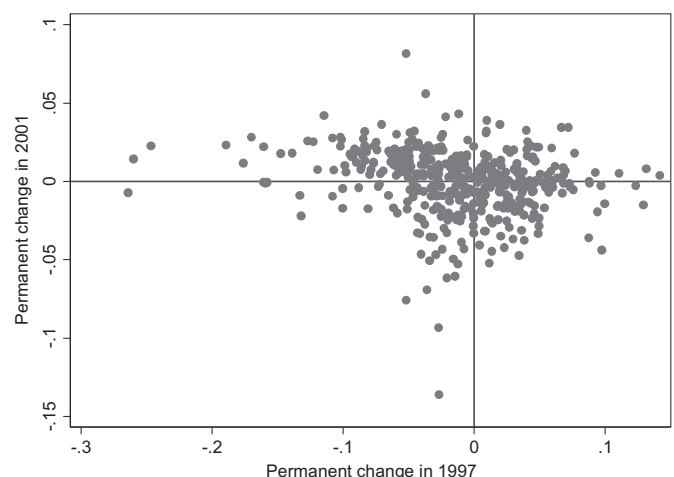
Fig. 2. Permanent change in 2001 scattered against permanent change in 1997.
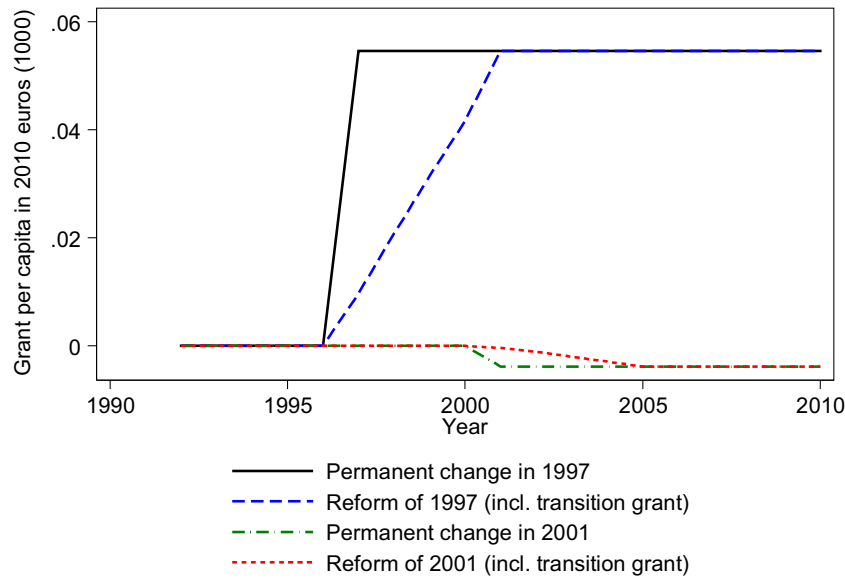
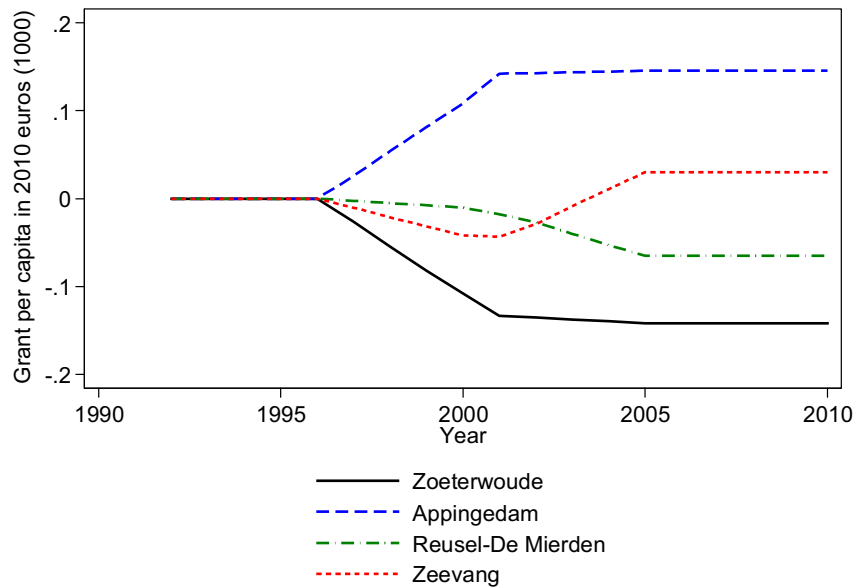Fig. 3. Reforms of 1997 and 2001 separately for the municipality of Amsterdam.

Fig. 4. Reforms of 1997 and 2001 jointly for four municipalities.

grants.[16] Municipalities which gained as a result of the grant revision received negative transition grants. A similar transition applied in the second stage (2001–2005).[17]

As discussed in more detail in the next section, our identification of the causal impact of exogenous changes in general grants uses the

variables 'Reform of 1997' and 'Reform of 2001', defined as the sums of permanent changes due to both stages of the overall reform and the corresponding transition grants. Fig. 3 illustrates the permanent changes and the variables Reform of 1997 and Reform of 2001 for the municipality of Amsterdam. Furthermore, we define the variable 'Reforms of 1997 and 2001 jointly' as the sum of Reform of 1997 and Reform of 2001. In order to avoid simultaneity bias, these variables are all scaled to the population in 1997 rather than to contemporaneous population. Summary statistics are again reported in Table 2. Fig. 4 shows 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 grant. Fig. 5 shows Reform of 1997 for

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

[17] In the second stage, supplementary grants ensured that grant changes became effective in five annual steps, of 10, 15, 25, 25 and 25%, respectively (BZK, 2000). Municipalities losing more than one hundred guilders (45 euro) per capita received a redemption grant in the fourth year, in a similar way as in stage 1.
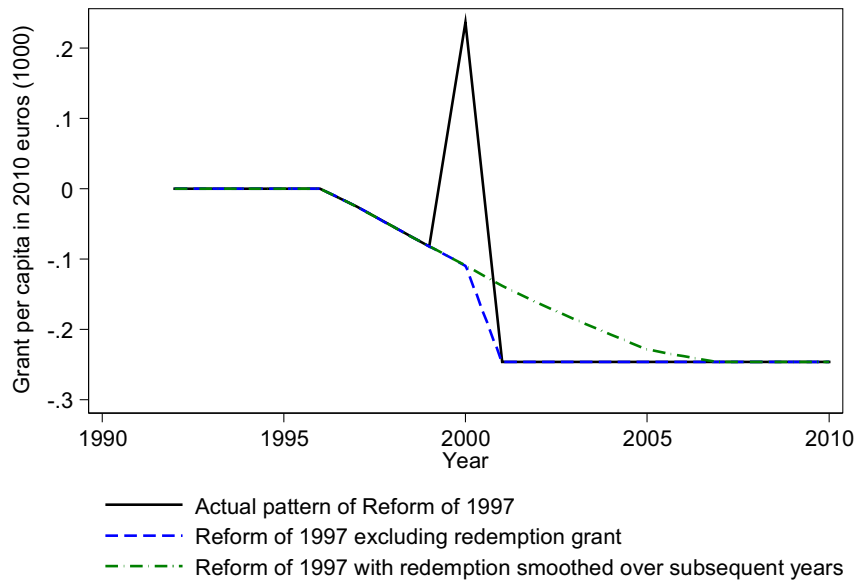
Fig. 5. Reform of 1997 for municipality of Bloemendaal.

the municipality of Bloemendaal, which received the largest redemption grant.

2.3. Placebo: decentralization of school accommodation

We use the decentralization of school accommodation in 1997 to validate our identification strategy. This reform changed the way of funding a service that was already provided. 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%. As the general grant is non-earmarked, municipalities were free to spend the extra funds as they liked, but they had to take over the task of providing adequate school buildings. Therefore, most of the additional money could not be spent on an improvement of public services or a reduction of taxes, so we would not expect to find substantial capitalization.

The extra funds were allocated to municipalities according to a formula derived in a way similar to the new general grant allocation system that was introduced in 1997 and 2001, of which it now forms a part.[18] The new funding system was introduced gradually using transition grants, in a way not unlike the gradual introduction of the new general grant allocation system.[19] Transition grants included some minor additional grants relating to school accommodation and sport parks that could not be separated out. Table 2 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.

3. Empirical analysis

3.1. Specification and identification strategy

The impact of general grants on house prices is estimated using the model.

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

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. The dependent variable will be replaced by local property tax revenue and municipal staff per capita in subsequent analyses, as discussed in Sections 3.4 and 3.5 respectively.

Following Hilber et al. (2011), we use a semi-log specification for the estimation of house price capitalization. Theory predicts a linear relationship (see e.g., Barrow and Rouse, 2004). However, least-squares estimates of a linear model are more sensitive to outliers and the municipality and year fixed effects capture unobserved heterogeneity more accurately in a semi-log model. For instance, changes in macroeconomic variables like the interest rate that have proportionate rather than additive effects on house prices, which vary across municipalities, are not well controlled for in a linear specification. We verify below that the estimation of a linear model yields very similar capitalization rates, once proportionate municipality and year fixed effects are removed from the house price index in a first step, and that a log–log specification also yields quantitatively very similar results. Hence, the potential bias from deviating from the relationship predicted by theory turns out to be negligible, whereas the semi-log model allows us to estimate the model in one step and it is consistent with earlier work.

Estimation of the model in Eq. (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, something that occurred frequently in our research period, house prices are likely to be unaffected if the additional funds just cover additional

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[18] 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.

[19] 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 capped the annual reduction to 0.75% of the amount previously spent within the municipality. This transition grant was paid out during a maximum of 5 years (1997 up to 2001).

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 socioeconomic composition of a municipality affect both grants and house prices. For instance, a rise in the share of wealthy households will reduce the amount of grants through the fiscal equalization scheme, while there may also be an independent effect on house prices.[20] Furthermore, the reform may be targeted at municipalities with low or decreasing house prices, which would also induce systematic correlation with local house price dynamics through some other channel than capitalization.

Our strategy for dealing with endogeneity of per capita general grants consists of two main ingredients. In the first place, we instrument this variable with the change in grants induced by the reform of the general grant allocation – i.e. the variable Reforms of 1997 and 2001 jointly as defined in the previous section. Identification on changes in grants due to this reform does not lead to bias from omitted tasks, because it left tasks unaffected. The reform also predates dynamics in socioeconomic composition that are contemporaneous with the dependent variable, because changes in grants were already fixed before it was implemented (see Table 1).[21] Hence, contemporaneous changes in the socioeconomic composition of a municipality cannot drive both the reform and changes in house prices. However, contemporaneous changes in socioeconomic composition may still be driven by reform-induced changes in grants, so that variables that correlate with socioeconomic composition are 'bad controls' (Angrist and Pischke, 2009).[22] We therefore exclude socioeconomic or demographic control variables from our main specification, although robustness for including sociodemographic controls is verified in a sensitivity analysis.

The inclusion of municipality-specific linear time trends in our model constitutes the second main ingredient of our identification strategy. Municipality fixed effects control for any targeting of the reform on time-invariant characteristics of municipalities that correlate with house prices levels. However, the reform may also favour municipalities in which house prices trend downwards, possibly as a result of deteriorating socioeconomic conditions. As Figs. 3 and 4 illustrate, the reform-induced change in grants varies nonlinearly over time. Our estimates of capitalization may therefore be 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 seems unlikely that policymakers targeted on predicted nonlinearities in this trajectory.

We validate our exclusion restriction by identifying on each of the two stages in which the reform was implemented separately and by identifying on the change in grants due to decentralization of school accommodation. If correlation of the reform with subsequent house price dynamics through omitted variables were an issue even after controlling for arbitrary linear time trends, it seems unlikely that this would affect each of the two weakly correlated stages in the same way. Hence, validity of our exclusion restriction is supported if identifying on either Reform of 1997 or Reform of 2001 yields similar results. Identification on the placebo Reform of financing school buildings instrument should

not yield substantial capitalization, as this reform only changed the way of funding a service that was already provided.

Our identification strategy does not account for the fact that some systematic correlation of the reform of general grant allocation with subsequent house prices may occur through tax base equalization. If a reform-induced rise in grants capitalizes into house prices, then partial equalization of the property tax base will lower subsequent general grants. This would induce a downward bias in our estimate of the capitalization rate. Quantitatively, however, the implied bias is small, because of the limited role of property taxation. Under the assumption of full capitalization, it never exceeds 6% over our period of observation.[23]

Finally, our identification strategy would not work out in a world with full information and perfect foresight. Forward looking behaviour implies 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 1996 (see Table 1), this would mean that most of the capitalization had already occurred before our period of observation. To the extent that information on the reform did indeed capitalize when it was announced, our estimate of the capitalization rate is conservative. However, the literature that explains the flypaper effect through rent seeking behaviour is based on the assumption that citizens are poorly informed about intergovernmental grants, and empirical evidence points in the same direction (e.g., Filimon et al., 1982).

We conjecture that also in our case, most citizens only became aware of the reform in general grant allocation and the way in which it affected their own municipality, 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 full information and perfect foresight, this outcry should have occurred at least seven years earlier.[24] Furthermore, we find that capitalization effects are strongest two years after the reform-induced change in grants has occurred, which seems a plausible time lag for adjusting public service levels and property tax rates. In the year before the grant change, we find no significant effect.

3.2. Data

We estimate Eq. (1) for the period 1995–2010. A number of municipal amalgamations occurred during this period, so we aggregate all variables in our analysis to the 2010 classification of municipalities. The five northern island municipalities are excluded and we merge a few other very small municipalities in our data, because the number of observed housing transactions would otherwise be too small to estimate a meaningful house price index. This leaves us with a sample of 419 municipalities out of a total of 430.

The house price index is estimated on all housing transactions over our period of observation that where 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.[25] This leaves a sample of 1,614,735 observations, or 241 transactions per municipality/year combination on average. Table 2 provides summary statistics on the number of observed housing transactions per municipality

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[20] House prices may rise if people have a preference for living near wealthy or highly educated households, or if the variety of amenities such as shops, restaurants and cultural facilities on offer in an area depends on the income level of its residents (see e.g. Brueckner et al., 1999).

[21] 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 Zaalen, 2002).

[22] For example, municipalities may invest additional grants in local amenities that facilitate gentrification, thus altering their age and income composition.

[23] In early years, the equalized property tax base was not adjusted to contemporaneous house prices (see footnote 13), so that capitalization of general grants could not give rise to bias. Furthermore, there is some variation over time in the rate at which the property tax base is equalized.

[24] Wassenaar managed to put off adapting spending levels to the lower grant until 2003 by consuming its substantial financial reserves.

[25] We impute dwellings as not being on leased land if this information is missing and if less than 5% of all dwellings in the municipality are on leased land. It is verified in a robustness analysis that leaving out the imputed observations does not change our main result (Appendix Table 7).

and year. The log of the transaction price in 2010 euros is regressed on a large number of dwelling characteristics and on 16×419 municipality/year-specific fixed effects.[26] The price index is then constructed from these fixed effects and scaled in such a way that its transactions-weighted average corresponds to the average price in our sample of housing transactions.

General grants are observed for the period 1992–2010. The source of all our data for general grants, reforms and transition grants is the Ministry of Interior Affairs, which has computed the permanent changes in grants by subtracting grant allocation according to the old formula from the allocation according to the new formula (BZK, 1997, 2000). General grants and transition grant amounts are inflated to 2010 euros for each year. 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 because grant amounts grow annually in tandem with central government spending, which takes account of inflation.

The analysis of local property tax revenue per capita uses total residential property tax revenue from 1997 onwards, calculated as tax base (obtained from Statistics Netherlands) times tax rate (obtained from COELO). This variable sums revenues from a tax on owners and a tax on users of residential property, the latter of which was abolished in 2006. The analysis of municipal staff per 1000 inhabitants uses staff employed by municipalities in full time equivalents, which is available from 1998 onwards from the Ministry of Interior Affairs. Information on the number of inhabitants per municipality is provided by Statistics Netherlands.

The sociodemographic controls that are used in a sensitivity analysis, as well as personal income in 2001, which is used for descriptive purposes only, are also obtained from Statistics Netherlands. We consider the share of inhabitants younger than 20 and the share older than 65, the share of recipients of social assistance and the share of nonwestern immigrants. This set of variables is only available from 1998 onwards. Personal income refers to the average disposable income for persons who had a job throughout the year.

3.3. Results on capitalization

Table 4 contains our baseline results. In this table and in the remainder of the paper, reported standard errors are clustered at the municipality level. As seen in the first column of Panel A, estimating the model in Eq. (1) with OLS while excluding linear time trends yields a weakly significant positive association between the house price index and general grants per capita. 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 socioeconomic composition of municipalities. These potential sources of bias are removed by instrumenting grants with the reform. Columns (3) and (4) show that this raises the coefficient considerably, consistent with our expectations. Results turn out to be hardly sensitive to the inclusion of linear time trends, however, indicating that the reform is not systematically correlated with trends in house prices through other channels than capitalization. The Kleibergen–Paap statistic indicates that our instrument is strong.

First stage results are shown in Panel B of Table 4. An additional euro of reform-induced change in grants corresponds to about one additional euro in general grants, although the difference from 1 is statistically significant for the estimate in column (4). A large

deviation would have been worrisome, as it would imply that part of the variation in general grants came from other changes that correlate with the reform and that are potentially endogenous (see e.g. Lundqvist, 2015).

Quantitatively, the coefficient in column (4) of Table 4, which is our preferred specification, implies that a 1 euro rise in grants per capita increases house prices by 0.0333%. At a weighted average transaction price of 235,850 euro, this corresponds to an increase of about 78 euro. Fig. 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 4273 euro per dwelling, while losses amounted to 5916 euro per dwelling at the 10th percentile of this distribution. Using a real discount rate of 3%, 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.[27] In the 95% confidence interval surrounding this estimate, capitalization rates would be at most 29% higher or lower. Furthermore, using a discount rate of 4% would raise the capitalization rate with about 33%.

Results of identifying grants on either of the two reforms are reported in Table 5. Both instruments still yield 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 4, whereas the impact of the included reforms is statistically insignificant. Neither specification rejects our preferred estimate at a significance level of 5%. Nevertheless, the estimate of grants identified on the 2001 stage when linear trends are excluded from the model is considerably lower and the difference with our preferred estimate is statistically significant. This indicates systematic correlation between this stage and trends in house prices through omitted variables, which 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 6. 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% level of significance for the specification that includes linear time trends. These results are consistent with our prior expectation that additional grants should not capitalize in house prices to the extent that they come with additional tasks previously carried out by other government units. Note also that the first stage coefficient for the specification that includes linear trends is close to 1, although this does not hold for the specification without trends.

Appendix Tables 1 to 7 report various alternative robustness checks that overall support our main results. In the first of these tables, we estimate a linear model instead of a semi-log 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 Eq. (1) in which this index replaces the dependent variable. Results are almost identical to the baseline results in Table 4. 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.

Appendix Table 2 shows results for a log–log specification. In order to be able to take the logarithm of the instrument, which is negative in about half of all municipalities, we have added the per capita grant amount in 1997. In the IV specification that controls for municipality-specific linear time trends, a 1% increase in grants per capita raises house prices by 0.271%. At a weighted average transaction price of

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[26] 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.

[27] Over the period 1995–2010, the real rate of return on Dutch government bonds with a ten year maturity was about 2.5% 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% discount rate is conservative.

Table 4
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)
R-squared			0.591	0.659

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

235,850 euro and a weighted average amount of grant per capita of 793 euro, this implies that a 1 euro rise in annual grants raises house prices by about 81 euro.

Robustness for the inclusion of sociodemographic controls is verified in Appendix Table 3. We control for the age composition of the population, as well as for the share of recipients of social assistance and the share of nonwestern immigrants. While data availability limits our sample to observations from 1998 onwards, results are very similar to our baseline findings. Moreover, the table nicely illustrates how controlling for sociodemographics becomes irrelevant once the specification includes linear time trends.

Experiments with alternative time lags are shown in Appendix Table 4. 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. In the year before the grant change (lag = −1), we find no significant effect. This is in line with our assumption that citizens are poorly informed about intergovernmental grants and notice grant changes only when they affect public service levels or tax rates.[28]

Appendix Table 5 explores how our results are affected by the peaks in the reform-induced change in grants for municipalities that received a redemption grant. In the first two columns, we estimate baseline specifications for the subsample of municipalities that did not receive redemption grants. Alternatively, we show results for a specification in which the redemption grant 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% in columns (5) and (6). The last two variants are illustrated for the municipality of Bloemendaal in Fig. 5. Overall, it appears that estimates are somewhat higher than our baseline estimate, particularly for the specifications that include linear time trends, for which the first-stage coefficients are also larger than 1.

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 6 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

Table 5
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	0.322***	0.356***	−0.0158	0.242***
	(0.0616)	(0.0576)	(0.105)	(0.0864)
Reform of 1997, 2 years lagged			0.321***	0.132
			(0.113)	(0.123)
Reform of 2001, 2 years lagged	−0.353***	−0.149		
	(0.137)	(0.133)		
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
Kleibergen–Paap F	662.6	574.7	126.4	42.95
Panel B – First stage corresponding to IV results				
Reform of 1997, 2 years lagged	0.951***	1.155***	0.951***	1.155***
	(0.0369)	(0.0482)	(0.0369)	(0.0482)
Reform of 2001, 2 years lagged	1.046***	1.304***	1.046***	1.304***
	(0.0930)	(0.199)	(0.0930)	(0.199)
R-squared	0.590	0.660	0.590	0.660

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

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 7 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. 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. Finally, observations after 2007 are left out in column (8), as house prices increased steadily until this year but subsequently fell in the wake of the Great Recession. All specifications yield

Table 6
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	−0.201
	(0.0739)	(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***	1.148***
	(0.189)	(0.113)
R-squared	0.513	0.650

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

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[28] The result that about half of the capitalization effect occurs instantaneously may be explained by some forward looking behaviour on behalf of either citizens or municipal governments, who might adjust spending levels already when they see the reform coming.

Table 7
The flypaper effect.

Panel A — dependent variable: property tax revenue per capita				
	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	−0.130*** (0.0241)	−0.0915*** (0.0219)	−0.236*** (0.0398)	−0.165*** (0.0301)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.173	0.0955		
Kleibergen–Paap F			884.7	681.7
Panel B — first stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.887*** (0.0298)	1.150*** (0.0440)
R-squared			0.381	0.345

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.

Table 8
Impact of grants on municipal staff.

Panel A — dependent variable: municipal staff per 1000 inhabitants				
	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	−0.450 (0.519)	0.161 (0.806)	−2.399** (1.078)	−0.0332 (0.821)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.0410	0.0410		
Kleibergen–Paap F			1050	751.0
Panel B — first stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.877*** (0.0271)	1.130*** (0.0412)
R-squared			0.364	0.298

Notes: N = 5445, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1.

statistically significant estimates and none of them rejects our preferred estimate at a significance level of 5%.

3.4. Results on the flypaper effect

The impact of general grants on local taxes is estimated with a variant of Eq. (1), in which the logarithm of house prices is replaced with total residential property tax revenue per capita, which is available from 1997 onwards. We use the same identification strategy. The corresponding exclusion restriction is that after controlling for municipality and year fixed effects, any deviations of the reform-induced change in grants from a linear time trend do not correlate systematically with deviations of local property tax revenue from a linear time trend through other channels.

Table 7 reports estimation results in the same format as our baseline results in Table 4. Grants are found to affect residential property tax revenue negatively and estimates are statistically significant in all specifications. Identification on the reform raises the coefficient, while inclusion of trends reduces it somewhat. First-stage coefficients are close to 1. In our preferred specification in Column 4, a 1 euro rise in grants per capita reduces property tax revenue per capita by 16 cents only, consistent with the flypaper effect.

Appendix Table 8 repeats the validation tests of Tables 4 and 5. Columns 1 and 2 show results identified on the 1997 stage of the reform, while estimates in Columns 3 and 4 are identified on the 2001 stage. In neither of the specifications that include time trends, the impact on per capita property tax revenue exceeds our preferred estimate at a significance level of 5%. The impact of grants is identified on the placebo reform of financing school buildings in Columns 5 and 6. As expected, the estimate is close to zero in the specification that includes trends. Curiously, it is significantly negative in the specification without trends. However, the first stage coefficient deviates substantially from 1 for this specification, so it may be biased because of correlation of the 2001 reform with trends in other changes in grants that are endogenous and for which the specification in Column 6 controls.

3.5. Results on municipal staff

For municipal staff per 1000 inhabitants, available from 1998 onwards, we use the same approach as for residential property tax revenue, implying a similar exclusion restriction. Results are shown in Table 8. The impact of grants on municipal staff is statistically insignificant in all specifications except the specification that is identified on the

reform and excludes trends, in which the estimated effect is negative. Validation tests reported in Appendix Table 9 are passed as, like in our preferred estimate in Column 4 of Table 8, coefficients for the specifications that include trends are all statistically insignificant.

Nevertheless, our preferred estimate is sufficiently precise to rule out a substantial effect of grants on municipal staff. The largest effect in the 95% confidence interval is 1.58, meaning that 1000 euros of additional grants per capita would raise municipal staff with 1.58 FTEs per 1000 inhabitants. Even if a civil servant would cost 60,000 euros per year, which is a generous upper bound, then expenditure on municipal staff would still amount to only 10 cents per euro.

This finding may raise the question how municipalities that see their grants increased can improve public services without hiring more staff. A first answer is that the money may be spent on capital rather than labour, for instance when it is invested in buildings, roads and public transport facilities. In the second place, the provision of many municipal services is contracted out. Real estate development is again a relevant example. Services in the domain of culture and recreation, which are labour intensive, will often be contracted out as well.

4. Conclusions

In line with a vast empirical literature, we document the existence of a flypaper effect in the Netherlands. Our main contribution lies in narrowing down the list of potential explanations for this phenomenon. We find that changes in grants to Dutch municipalities due to a reform of the equalization system fully capitalize into house prices. This makes rent seeking by politicians or bureaucrats an improbable explanation for the flypaper effect. Why would households be willing to pay more for residing in a municipality that spends its increase in grants on self-serving bureaucracy or transfers the money to the pockets of corrupt politicians? The absence of a positive effect of grants on municipal staff provides further evidence against the bureaucratic flypaper effect.

These findings do not rule out the relevance of rent seeking in other countries. In particular, as discussed in the introduction, there is some compelling evidence on the adverse effects of grants on local corruption in the developing world. Rent seeking has also been related to the large propensity to spend out of highway funds in the US. This suggests that explanations for the flypaper effect depend on context and institutional setting. More empirical research on underlying mechanisms and their institutional determinants therefore seems warranted from both academic and policy perspectives.

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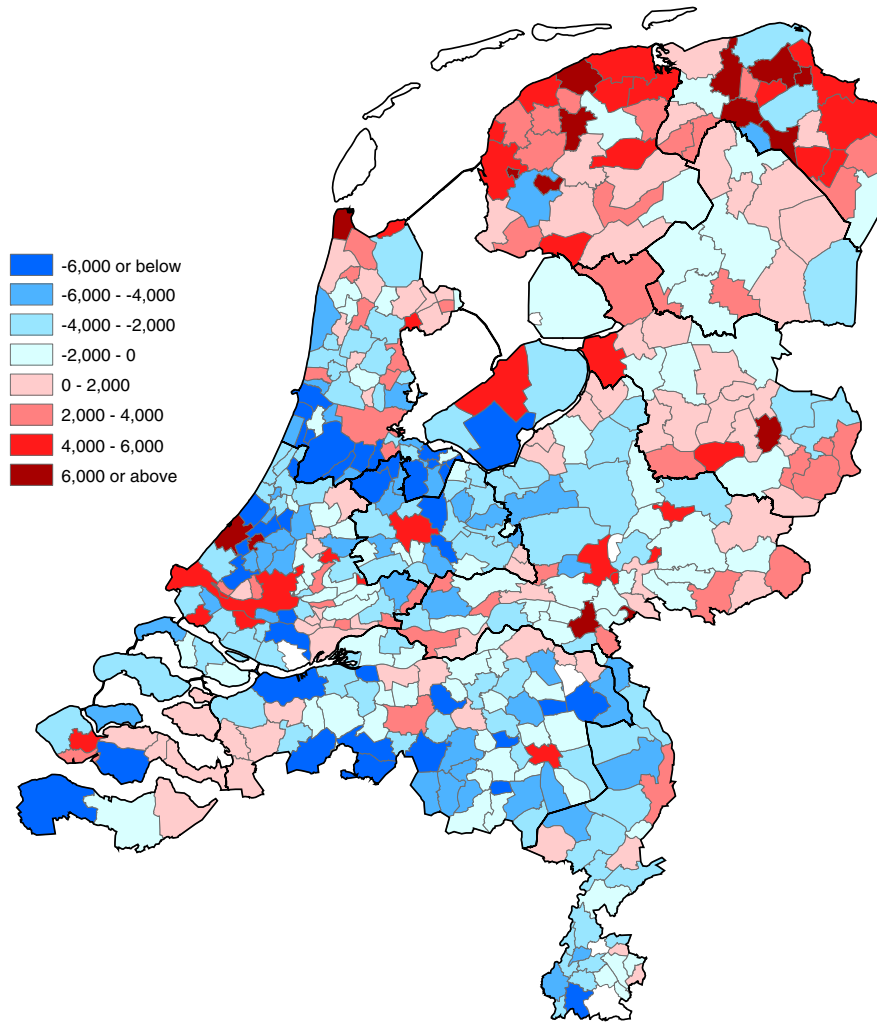
Fig. 6. Estimated effect on house prices of 1997 and 2001 reforms jointly (in 2010 euros).

Appendix A

Appendix Table 1
Baseline results reproduced for real house price index in levels, demeaned for years and municipalities.

	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	14,439*	24,259***	68,502***	77,624***
	(7830)	(6576)	(12,850)	(11,183)
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
Log–log specification.

Panel A — Dependent variable: Log(real house price index)	OLS		IV	
	(1)	(2)	(3)	(4)
Log(general grant per capita), 2 years lagged	−0.00895	0.105***	0.201***	0.271***
	(0.0206)	(0.0198)	(0.0486)	(0.0407)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.403	0.343		
Kleibergen–Paap F			737	620.6
Panel B — First stage corresponding to IV results				
Log(reforms of 1997 and 2001 jointly + general grant per capita in 1997), 2 years lagged			0.714***	0.904***
			(0.0263)	(0.0363)
R-squared			0.955	0.960

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 3
Control for sociodemographics.

Panel A — Dependent variable: Log(real house price index)	OLS		IV	
	(1)	(2)	(3)	(4)
General grant per capita, 2 years lagged	0.0673**	0.190***	0.277***	0.329***
	(0.0307)	(0.0345)	(0.0555)	(0.0624)
Share of inhabitants aged younger than 20	−0.0934	−0.518	0.0822	−0.454
	(0.246)	(0.453)	(0.257)	(0.426)
Share of inhabitants older than 65	−1.084***	−0.875*	−1.053***	−0.771*
	(0.225)	(0.470)	(0.231)	(0.442)
Share of inhabitants receiving social assistance	−3.441***	−0.514	−2.755***	0.0954
	(0.570)	(0.634)	(0.548)	(0.642)
Share of nonwestern immigrants	−0.928***	−0.0605	−0.998***	−0.212
	(0.170)	(0.376)	(0.172)	(0.358)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality-specific time trends		Yes		Yes
R-squared	0.0916	0.0672		
Kleibergen–Paap F			1066	767
Panel B — First stage corresponding to IV results				
Reforms of 1997 and 2001 jointly, 2 years lagged			0.903***	1.104***
			(0.0277)	(0.0398)
Share of inhabitants aged younger than 20			−0.0438	0.0147
			(0.160)	(0.329)
Share of inhabitants older than 65			0.179	−0.348
			(0.135)	(0.347)
Share of inhabitants receiving social assistance			0.887*	−1.754***
			(0.525)	(0.503)
Share of nonwestern immigrants			0.670***	0.0673
			(0.210)	(0.202)
R-squared			0.624	0.100

Notes: N = 5447, 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 4
Results for alternative time lags.

Panel A — IV estimates. Dependent variable: Log(real house price index)						
Number of lags n	−1	0	1	2	3	4
General grant per capita, n years lagged	0.0169	0.151***	0.276***	0.333***	0.331***	0.229***
	(0.0470)	(0.0404)	(0.0398)	(0.0475)	(0.0691)	(0.0713)
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	6285	6704	6704	6704	6704	6285
Kleibergen–Paap F	775	721	650	594	673	924
Panel B — First stage corresponding to IV results						
Reforms of 1997 and 2001 jointly, n years lagged	1.100***	1.123***	1.170***	1.182***	1.150***	1.095***
	(0.0395)	(0.0418)	(0.0459)	(0.0485)	(0.0443)	(0.0360)
R-squared	0.393	0.416	0.568	0.659	0.736	0.750

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

Appendix Table 5
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***	0.398***	0.320***	0.445***	0.324***	0.461***
	(0.0645)	(0.0593)	(0.0558)	(0.0468)	(0.0578)	(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***	1.303***	0.957***	1.275***	0.951***	1.276***
	(0.0495)	(0.0610)	(0.0398)	(0.0542)	(0.0409)	(0.0567)
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 6
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	−0.178
	(0.0794)	(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***	1.751***
	(0.231)	(0.239)
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 7
Miscellaneous robustness checks.

Panel A — IV estimates. Dependent variable: Log(real house price index)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
General grant per capita, *n* years lagged	0.330***	0.333***	0.346***	0.325***	0.285***	0.227***	0.205***	0.322***
	(0.0482)	(0.0473)	(0.0489)	(0.0474)	(0.0479)	(0.0645)	(0.0776)	(0.0608)
Municip. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6704	6704	6640	6160	6379	6704	6704	5447
Kleibergen–Paap F	593.6	605.5	617.6	584.7	551.7	125.2	92.17	1224

Panel B — First stage corresponding to IV results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reforms of 1997 and 2001 jointly, 2 years lagged	1.181***	1.184***	1.173***	1.192***	1.186***	1.142***	1.195***	1.068***
	(0.0485)	(0.0481)	(0.0472)	(0.0493)	(0.0505)	(0.102)	(0.125)	(0.0305)
R-squared	0.659	0.659	0.722	0.672	0.638	0.652	0.651	0.685

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;
(8) Years after 2007 are excluded.

Appendix Table 8
Robustness checks for the flypaper effect.

Panel A — IV estimates. Dependent variable: Property tax revenue per capita

	1997 reform		2001 reform		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)
General grant per capita, 2 years lagged	−0.247***	−0.185***	−0.167***	−0.0483	−0.0971***	0.0123
	(0.0446)	(0.0319)	(0.0431)	(0.0555)	(0.0361)	(0.0343)
Reform of 1997, 2 years lagged			−0.0683	−0.159**		
			(0.0512)	(0.0761)		
Reform of 2001, 2 years lagged	0.0878	0.146**				
	(0.0657)	(0.0621)				
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
Kleibergen–Paap F	728.4	609.5	131	71.66	101.8	99.94

Panel B — First stage corresponding to IV results

	(1)	(2)	(3)	(4)	(5)	(6)
Reform of 1997, 2 years lagged	0.860***	1.164***	0.860***	1.164***		
	(0.0319)	(0.0472)	(0.0319)	(0.0472)		
Reform of 2001, 2 years lagged	1.106***	1.070***	1.106***	1.070***		
	(0.0967)	(0.126)	(0.0967)	(0.126)		
Reform of financing school buildings, 2 years lagged					1.425***	1.071***
					(0.141)	(0.107)
R-squared	0.373	0.343	0.373	0.343	0.242	0.324

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 9
Robustness checks for impact of grants on municipal staff.

Panel A — IV estimates. Dependent variable: Municipal staff per 1000 inhabitants

	1997 reform		2001 reform		Placebo	
	(1)	(2)	(3)	(4)	(5)	(6)
General grant per capita, 2 years lagged	− 2.094*	− 0.0863	− 3.882**	0.315	− 1.938	0.0531
	(1.200)	(0.951)	(1.767)	(1.035)	(1.632)	(1.009)
Reform of 1997, 2 years lagged			1.502	− 0.460		
			(1.766)	(1.707)		
Reform of 2001, 2 years lagged	− 2.000	0.410				
	(2.359)	(1.513)				
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
Kleibergen–Paap F	772.1	695.9	127.9	93.95	101.6	90.43

Panel B — First stage corresponding to IV results

	(1)	(2)	(3)	(4)	(5)	(6)
Reform of 1997, 2 years lagged	0.840***	1.148***	0.840***	1.148***		
	(0.0302)	(0.0435)	(0.0302)	(0.0435)		
Reform of 2001, 2 years lagged	1.118***	1.023***	1.118***	1.023***		
	(0.0989)	(0.106)	(0.0989)	(0.106)		
Reform of financing school buildings, 2 years lagged					1.077***	1.025***
					(0.107)	(0.108)
R-squared	0.353	0.295	0.353	0.295	0.195	0.260

Notes: N = 5445, number of municipalities = 419. Statistics are robust to heteroskedasticity and within-municipality correlation. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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