Place-based policies and the housing market*

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SUMMARY — We study the economic effects of place-based policies in the housing market taking into account search frictions. Theory indicates that beneficial policies increase house prices, but *temporarily* reduce sales times of owner-occupied properties. We investigate both effects for a place-based programme that improved public housing in 83 impoverished neighbourhoods throughout the Netherlands. We combine a first-difference approach with a fuzzy regression-discontinuity design to address the fundamental issue that these neighbourhoods are endogenously treated. Place-based policies increase house prices with 3.5 percent and, in line with theory, temporarily reduce sales times with 20 percent. The sales time effect dissipates within 7.5 years. The programme's welfare benefits to homeowners are sizeable and at least half of the value of investments in public housing.

JEL-code — R30, R33

Keywords — amenities; house price bargaining model; spatial equilibrium; house price; sales time; place-based policies.

I. Introduction

In many countries *place-based policies* have been developed that make large public investments in poor neighbourhoods. Economists are not necessarily in favour of these policies. It has been argued that governments should help people, rather than places, and "not bribe people to live in unattractive places" (Glaeser, 2011). However, if nonmarket interactions are important, then this may justify place-based policies. For example, through local spillovers, a neighbourhood participation programme may decrease negative externalities. European place-based policies often improve the quality of the public housing

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stock through new home construction replacing an obsolete building stock, or through substantial renovations to the existing stock.[1] This does not only benefit public housing tenants but also nearby residents through a higher neighbourhood quality.

In the literature, there has been ample attention paid to the effectiveness of place-based labour market programmes (see e.g. Neumark and Kolko, 2010; Mayer et al., 2012; Busso et al., 2013; Kline and Moretti, 2013, and Neumark and Simpson, 2015 for an overview). However, the effects of place-based housing policies on local residents are hardly researched. There are few studies that confirm that place-based investments have led to higher house prices (Ioannides, 2003; Schwartz et al., 2006; Rossi-Hansberg et al., 2010). This does not imply, however, that place-based policies are always effective. For example, a number of studies, including Briggs (1999), Lee et al. (1999), Santiago et al. (2001) and Ahlfeldt et al. (2016), find no statistically significant, or even small negative, effects of place-based policies that subsidise housing.

Many of these empirical studies focus on a specific programme with a small number of neighbourhoods in a specific city. Furthermore, because neighbourhood selection is endogenous – only the worst performing neighbourhoods receive subsidies – the estimates of the benefits of place-based policies may not be causal. The studies also focus exclusively on house prices, in line with spatial equilibrium models that measure welfare gains of local policies through changes in land prices. This approach is particularly attractive when assuming absentee landowners and frictionless markets.[2]

In this paper, we analyse the effects of place-based policies on house prices and sales times. The first contribution of the paper relates to the scale and the type of the programme under study. We evaluate changes in local amenity levels due to a large-scale nationwide urban revitalisation programme in the Netherlands, starting in 2007, which improves the quality of public housing. In this so-called *krachtwijken*-programme (henceforth: KW-investment scheme), 83 neighbourhoods were selected for revitalisation with funding from the national government.[3] The government and (not for profit) public housing associations announced to invest about € 2.75 billion in these neighbourhoods, on average about € 3.5 thousand per household in receiving neighbourhoods. Although, in the end only € 1 billion was spent (Permentier et al., 2013). The main objectives of the programme were to transform these neighbourhoods into pleasant places to live and to reduce social inequality (Department of Housing, Spatial Planning and the Environment, 2007). In practice, the bulk of the money was spent on improving of the public housing stock. The remainder was used for expenditures on green spaces, social empowerment programs and the conversion of public to private housing (Wittebrood and Permentier, 2011). We utilise a nationwide

[1] Public housing is extremely common in Europe, as it covers 47 percent of the rental housing market (Van Ommeren and Van der Vlist, 2016). In the Netherlands, this percentage is about 90 percent, such that 30 percent of the whole housing market is public housing.
[2] Conditional on the housing stock, house prices (which reflect building costs as well as land prices) can then be interpreted as land prices.
[3] The scheme was also known as *aandachtswijken*-scheme or *Vogelaarwijken*-scheme.

dataset with information on (privately-owned) house transactions from 2000 to 2014, including the house price and sales time. The private housing stock, to which our data refer, was *not* improved by the programme.[4] We use a first-differences estimation strategy based on thousands of repeated sales observations. In essence, we compare changes in house prices, as well as sales times, between *many* targeted and non-targeted neighbourhoods. Hence, the results of our study are likely to have external validity, and because neighbourhood sampling error is eliminated, the estimates are likely more precise.

The second, but important, contribution of the paper is to the identification of causal effects of place-based policies. We take into account that areas targeted by place-based policies are not randomly chosen, but are *explicitly* chosen because of undesirable characteristics. We employ a fuzzy regression-discontinuity design (FRD) by using information on an eligibility criterion to receive investments.[5] This criterion is dependent on so-called deprivation scores, calculated by the national government for the whole of the Netherlands. Although the neighbourhoods with the highest deprivation scores were not always treated, there is a discrete and substantial jump in the probability to become treated when the deprivation score exceeds a certain threshold (the jump is about 0.7 percentage points).[6]

The third contribution relates to the distinction we make between the short-run and long-run effects of place-based policies, and between the impact of place-based policies on house prices and sales times. Based on a fairly standard theoretical model including housing search and matching in the spirit of Wheaton (1990), we argue that place-based policies should increase house prices (in the short and long run). By contrast, these policies reduce sales times *temporarily* (in the short run), but not in the long run *if* search costs are proportional to amenity levels but with a delay. The latter result is useful as a consistency test: if one does not find a temporary effect of place-based policies on sales times, then this will put doubt on the causality of an effect on prices.[7] Information on the effects on sales time are also indicative how much time it takes before the market returns to a steady state, so that we can identify the long-run price change, and to what extent reductions in sales time are beneficial to incumbent homeowners. In a model with search frictions, house prices are not necessarily one-to-one related to welfare measures. We argue, however, that the percentage price effect can be interpreted as a percentage welfare effect if search costs are proportional to the

[4] We do not analyse the effect on rents, because about 90 percent of the Dutch rental housing stock is rent-controlled, and hence would yield very little information. Moreover, data on rents is not available.

[5] Hence, we allow house price *trends* to be neighbourhood-specific, so trends in neighbourhood unobserved variables are allowed to be correlated with the selection of targeted neighbourhoods.

[6] To further control for potential biases, we control for housing and neighbourhood attributes, a flexible function of the deprivation score. Moreover, we add an extensive set of robustness checks to test the main identifying assumptions in Section VI.

[7] This is particularly true because changes over time in house prices and sales times are negatively correlated (Koster and Van Ommeren, 2016). If the results of the hedonic price model indicate a permanent effect of place-based policies, but these results are spurious due to omitted variable bias, then one would expect to find a permanent effect of place-based policies on sales time as well.

amenity level. When the market is in spatial equilibrium, absolute changes in prices are underestimates of absolute welfare changes, but when vacancy rates are low – as is usually the case in empirical data – then price changes are almost exact measures of welfare changes.

We find that due to investments (mainly in public housing), house prices increased by about 3.5 percent. We also find that the effect on sales time is strong as sales times are reduced temporarily with 15-20 percent (about a month). The latter result indicates that selling time and matching is a non-negligible feature of the housing market. We show that the sales time effect is temporary and disappears after 7.5 years. The empirical results survive remarkably unaltered when we extensively check for robustness, for example by conducting quasi-placebo experiments, using propensity score matching rather than a FRD, and by testing whether presence of potential spatial spillovers invalidates our results. A counterfactual analysis indicates that the welfare benefits to homeowners induced by the programme are at least half of the value of the investments in public housing.

The remainder of the paper is organised as follows. In Section II we discuss the theoretical implications of a place-based investment that increases the amenity level when housing search, bargaining and matching are present. In Section III we discuss the features of the KW-investment scheme, the data, some descriptive evidence and the econometric framework. Section IV turns to the empirical results, which is followed by a counterfactual analysis in Section V. We subject the baseline results to an extensive sensitivity analysis in Section VI and Section VII concludes.

II. Place-based policies, prices and sales times: theoretical considerations

What are the welfare effects of place-based investments on the housing market when search and matching are important? In Appendix A we set-up a theoretical model in the spirit of Wheaton (1990) incorporating search, matching and bargaining. Not only house prices in this model are endogenously determined, but also search effort, the matching rate, and importantly, selling time on the market. We then evaluate what is the impact of a change in the (exogenous) amenity level due to place-based investments on house prices, sales times and welfare in the presence of housing search. Moreover, we make a distinction between *long-run* and *short-run* effects.

We assume a neighbourhood with two symmetric types of housing. Each neighbourhood supplies a given number of houses. Households have a preference for one housing type, but they change this preference at a given rate (e.g. due to birth of a child). We then distinguish between three household states: matched, mismatched and dual-ownership households. Matched households own one property, occupy their preferred housing type and receive a utility flow based on the amenity level from living in a certain neighbourhood. Dual-household own two houses of a different type. They occupy their preferred housing type, enjoying the same utility flow as matched households per unit of time, but they aim to sell the property of the other type, which is vacant. Mismatched households own one property of the non-preferred type with an utility flow less than, but proportional, to the utility flow of being matched. Mismatched households search for the other housing type incurring search costs which are an increasing convex function of effort level.

Let us first focus on the long-run. Search costs are assumed to be proportional to the amenity level.[8] This assumption aims to capture *long-run* conditions and has a range of justifications, but mainly captures that search costs for households increase with house prices which are determined by amenities. For example, real estate agents usually charge fees that are proportional to housing prices which are higher in locations with more amenities.

This model leads to two testable empirical predictions *for the long run*:

(i) the price is positively influenced by amenity-increasing place-based investments;

(ii) the expected sales time will not be affected by these place-based investments.

In the absence of search frictions included in this model, it is well known that standard hedonic theory indicates that increases in house prices due to marginal place-based investments are an accurate measure of welfare increases. To calculate the welfare effects of place-based investments taking into account search frictions is not standard. We will focus on the long-run steady-state welfare changes of these investments. Given that search costs are proportional to the amenity level in the long run and therefore sales times are unaffected, it holds that

$$\text{(1)} \qquad \frac{\mathrm{d}\log p}{\mathrm{d}k} = \frac{\mathrm{d}\log \mathcal{w}}{\mathrm{d}k} \quad \text{and} \quad \frac{\mathrm{d}p}{\mathrm{d}k} < \frac{\mathrm{d}\mathcal{w}}{\mathrm{d}k},$$

where p is the house price, k is the amenity level and $\mathcal{w}$ denotes welfare.

Hence, *percentage* price changes are an exact measure of *percentage* welfare changes in the long run. Because search effort, and therefore search frictions, do not change in the long-run given place-based investments, the effect of search frictions is therefore a proportionality constant given changes in k. Furthermore, in levels, price changes are always smaller than welfare changes. To be precise, the underestimate of the price changes as a proxy for welfare changes is proportional to the vacancy rate. So when the observed vacancy rate is small – which will be the case in the market we analyse – changes in welfare are essentially identical to changes in prices. We use this insight to estimate the welfare impact of place-based policies in the counterfactual analysis.

We are also interested in out-of-steady-state effects predicted by the theoretical framework. In the short run, the conditions about job search differ from those assumed above. For example, it makes sense to assume that search cost do not depend on amenity levels in the short run and then slowly adapt. To capture that, we assume that search costs are proportional to amenity levels with a delay. When we numerically solve the model, we obtain two additional testable empirical predictions given an unannounced increase in the amenity level:

(iii) prices adjust quickly to the new steady-state value;

[8] Similarly, long-run assumptions in the labour market search literature state that search costs are proportional to productivity levels (Pissarides, 2000).

(iv) sales time drop in the short run, while this effect disappears in the long run.

These results indicate that welfare implications allowing for out-of-steady-state search effort levels will hardly differ from the steady-state results derived above, because search levels only differ from their steady-state levels for a short period.

III. Empirical framework and data

A. The urban revitalisation programme

There is ample empirical evidence that households with low incomes and associated social problems are disproportionally concentrated in certain urban neighbourhoods. For example, many US inner cities contain large concentrations of low-income households and score low on almost every measure capturing social dysfunction (Mills and Lubuele, 1997; Glaeser et al., 2008; Rosenthal and Ross, 2015). In the Netherlands, we observe a similar but less extreme pattern.[9] About 70 percent of the most deprived neighbourhoods are located in the four largest cities of the Netherlands (Amsterdam, Rotterdam, The Hague and Utrecht). The share of public housing is much higher in these neighbourhoods than in other parts of the Netherlands. The gap between poor neighbourhoods and other neighbourhoods in terms of unemployment, crime rates and income, has widened in the last decade. Therefore, in 2007, a substantial national investment programme was launched by the Dutch secretary of state that was responsible for housing and labour: € 216 million was planned to be invested in the 83 worst performing postcode areas, which we refer to as neighbourhoods (The Court of Audit, 2010). The average size of a targeted neighbourhood is 1.43 square kilometre, so neighbourhoods are rather small. The investment fund was used to assist municipalities in restructuring and revitalisation of neighbourhoods. On 14 September 2007 the secretary of state agreed with large public housing associations that they would invest another € 2.5 billion in the selected neighbourhoods over a course of ten years (in total about € 3.5 thousand per household residing in these neighbourhoods) (The Court of Audit, 2010).[10] Although the exact monetary value of the investment is unknown, experts estimate that eventually about one billion Euros has been invested in these neighbourhoods between 2007 and 2012 (Permentier et al., 2013). Arguably, the physical restructuring of public housing also has a beneficial effect on nearby residents as these residents prefer to live in a well-maintained building environment (Rossi-Hansberg et al., 2010). Such an environment not only improves views within neighbourhoods, but also may improve physical and mental health, according to a large environmental psychology and health literature (see e.g. Srinivasan et al., 2003). Apart from physical restructuring and sale of public housing, a small amount of the investments was targeted at poor households directly through empowerment programs (Department of Housing, Spatial Planning and the Environment, 2007;

[9] Due to substantial benefit transfers, differences in Dutch household income are less pronounced than in the US.

[10] We consider 14 September 2007 as the start of the investment programme, but we will check for robustness of the assumed date later on.

TABLE 1 — DEPRIVATION SCORES FOR NEIGHBOURHOODS

	All neighbourhoods		KW-neighbourhoods		Non-KW-neighbourhoods	
	μ	σ	μ	σ	μ	σ
Social deprivation	0.000	0.654	1.167	0.322	-0.0246	0.636
Physical deprivation	0.000	0.611	2.070	0.660	-0.0437	0.529
Social problems	0.000	0.924	2.612	1.053	-0.0551	0.838
Physical problems	0.000	0.950	3.087	0.976	-0.0651	0.834
Overall	0.000	2.414	8.935	1.340	-0.188	2.047
Number of neighbourhoods	4016		83		3933	

Notes: Social deprivation includes three indicators: income, unemployment and low education share. Physical deprivation includes three housing quality indicators: the shares of small houses, old houses (constructed before 1970), and of public housing stock. Social problems consists of five indicators: two vandalism indicators, two nuisance-from-neighbours indicators, and one indicator relates to feelings of insecurity. Physical problems includes seven indicators: house and living environment satisfaction, the inclination to move, and indicators relating to noise and air pollution, traffic intensity and traffic safety. For details, see Brouwer and Willems (2007).

Wittebrood and Permentier, 2011).

Another benefit of the programme may be indirect: if the social composition of a neighbourhood changes due to the programme, this may have impacts on house prices. For example, there is empirical evidence that suggests that high income households are disproportionally attracted by amenities (Gaigné et al. 2017). We will show that there are indeed some changes in the social composition in the treated neighbourhood, but controlling for demographics leaves the price and sale time effects unaffected. Hence, most of our effect is likely explained by improvements in the physical appearance of neighbourhoods.

The selection criteria of the deprived neighbourhoods were based on deprivation scores consisting of 18 indicators that were organised in four categories: social deprivation (income levels, education and unemployment), physical deprivation (quality of housing stock), social problems (vandalism and crime) and physical problems (noise and air pollution, satisfaction with living environment). It is important to note that our outcome variables (house price, sales time) were *not* part of the selection criterions. Brouwer and Willems (2007) use data from 2002 and 2006 to calculate so-called *z-scores* for each postcode area in the Netherlands with at least 1,000 inhabitants (about 4,000 areas), where each of the four categories is weighted equally and standardised with mean zero and unit standard deviation. Because the overall z-score is the sum of the standardised scores of four categories, the average score for The Netherlands is zero, but the standard deviation of the overall z-score exceeds one.

The selection of the KW-neighbourhoods was based on the deprivation score which were known to be disadvantaged (Permentier et al., 2013). The idea was to target neighbourhoods with a z-score of at least 7.30. However, twelve neighbourhoods were removed from the list after discussions with local governments, while two other neighbourhoods (in Amsterdam and Enschede) were added although they had z-scores below the threshold (respectively

6.84 and 5.00).[11] Table 1 shows that targeted KW-neighbourhoods have scores that are about average score for these neighbourhoods is 8.94, more than 3.5 times the standard deviation above the Dutch average.

B. Data

Our analysis is based upon a house transactions dataset from the NVM (Dutch Association of Real Estate Agents). It contains information on about 80 percent of all transactions between 2000 and 2014, so roughly seven years before and after the investment took place.[12] For 1,796,542 transactions, we know the transaction price, asking price, the sales time (in days on the market), the exact location, and a wide range of house attributes such as size (in square meters), type of house, number of rooms and construction year. We exclude transactions with prices that are above € 1.5 million or below € 25,000 or a square meter price below € 250 or above € 5,000. Furthermore, we exclude transactions that refer to properties smaller than 25 square metres or larger than 250 square metres. We drop a few properties that are sold more than five times in our study period or more than twice in one year and are listed for more than five years on the market or were listed zero days on the market. Based on the distribution, we also drop observations for which the percentage transaction to asking price is below 70 or above 110 percent. These selections do not influence the results. On average, properties in our sample are sold 1.29 times in our study period. In the analysis, we focus on repeated sales, so properties that are sold at least twice, leaving us with 434,033 transactions.[13]

In Table 2, descriptives are reported for observations outside and inside (targeted) KW-neighbourhoods. About 3.8 percent of the observations in the repeated sales sample is in a targeted KW neighbourhood whereas 1.6 percent in this sample is in a KW-neighbourhood (including observations both before and after the treatment). It appears that the price per square metre in non-KW-neighbourhoods is 3.5 percent higher than in KW-neighbourhoods. The difference seems fairly small, but is explained by the observation that most deprived neighbourhoods are located in urban, rather than rural, areas, where prices are generally higher. Properties in KW-neighbourhoods tend to have a lower quality: they are more often apartments, are older, have less often central heating and are of a lower maintenance quality. Also, 34 percent of the properties in these areas have been constructed between 1961 and 1970, a building period which is in the Netherlands associated with low building quality.

[11] There was substantial criticism on the selection of the specific neighbourhoods. According to opponents, the selection criterions were not well chosen and the postcode areas were too large to capture meaningful neighbourhoods. In contrast, we think that neighbourhoods are fairly small: the average distance to the centroid of a neighbourhood is only 286 meter.

[12] In the (large) cities we focus on, the NVM has a more dominant position, so the 80 percent is likely an underestimate. The figure may be as high as 90 percent.

[13] Using repeated sales may imply a selection problem, because certain house types may be sold less often. In Section VI.J, we check whether our results are robust with respect to this selection.

TABLE 2 — DESCRIPTIVE STATISTICS FOR REPEATED SALES SAMPLE

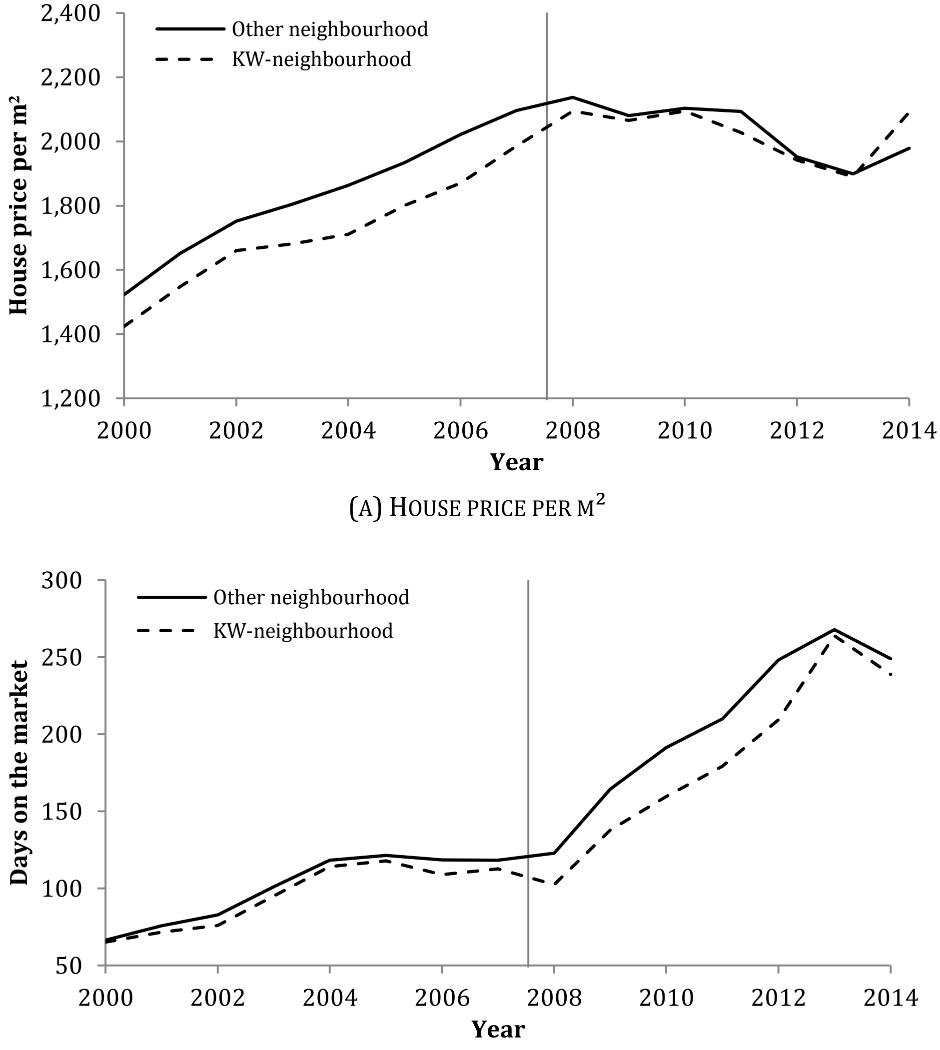
	Observations outside KW-neighbourhoods				Observations inside KW-neighbourhoods			
	μ	σ	Min	max	μ	σ	min	max
House price per m² *(in €)*	1,910	597.0	500	5,000	1,846	601.1	504.2	4,972
Days on the market	136.2	173.5	1	1,823	126.8	159.4	1	1,816
KW-investment received	0				0.418			
Deprivation z-score	0.431	2.829	-6.600	10.60	8.684	1.181	5	12.98
Size in m²	105.1	32.87	26	250	83.44	26.14	27	250
House type – apartment	0.406				0.803			
House type – terraced	0.324				0.145			
House type – semi-detached	0.213				0.0492			
House type – detached	0.0567				0.00335			
Garage	0.205				0.0557			
Garden	0.988				0.989			
Maintenance quality –good	0.909				0.874			
Central heating	0.932				0.886			
Listed	0.00497				0.00508			
Construction year <1945	0.226				0.293			
Construction year 1945-1960	0.0710				0.143			
Construction year 1961-1970	0.177				0.344			
Construction year 1971-1980	0.166				0.0432			
Construction year 1981-1990	0.152				0.0515			
Construction year 1991-2000	0.167				0.0894			
Construction year >2000	0.0408				0.0358			

Notes: The number of observations outside KW-neighbourhoods is 417,307 and inside KW-neighbourhoods 16,726. Note that the house type variables, garage, garden, and construction year are time-invariant, so they will drop in the first-differences equations.

Table B1 in Appendix B.1 also reports descriptive statistics for the full sample, including properties that are transacted only once during the study period. It appears that there are few systematic differences between the full sample and the repeated sales sample.[14]

In Figure 1 we plot the house price and the sales time for KW and other neighbourhoods over time. In Figure 1A, it is confirmed that prices in KW-neighbourhoods were lower than in other neighbourhoods, but this price gap is substantially reduced after 2007, while from 2009 onwards house prices seem almost identical. Although suggestive, one may not conclude that this reduction in price gap is due to the investment programme, because it ignores that other factors may play a role (e.g. gentrification, disproportionate construction of new houses). In Figure 1B, it is shown that the sales time for targeted and non-targeted

[14] Properties in our repeated sales sample tend to be somewhat smaller, have a somewhat higher maintenance quality and are more often constructed between 1961 and 1970. The share of recently constructed properties is somewhat lower.



(A) HOUSE PRICE PER M^2

(B) DAYS ON THE MARKET

FIGURE 1 — HOUSE PRICES AND SALES TIME INSIDE AND OUTSIDE KW-NEIGHBOURHOODS

neighbourhoods are pretty similar until 2007. After the investment, the sales time is much lower in KW-neighbourhoods than in other neighbourhoods. Although this difference seems to become somewhat smaller over time and disappears in 2013.

C. Econometric framework and identification

We are interested in the causal effect of the KW-investment scheme on house prices and sales times. Let $y_{\ell t}$ be an outcome variable, which it is either the logarithm of the house price per square meter or the logarithm of the days on the market in neighbourhood ℓ in year t. The outcome variable is a function of whether the neighbourhood has received investments $k_{\ell t}$ in year t. We control for unobserved time trends, captured by year fixed effects υ_t. A naïve regression would yield:

(2) $$y_{\ell t} = \alpha k_{\ell t} + \upsilon_t + \epsilon_{\ell t},$$

where α is the parameter to be estimated and $\epsilon_{\ell t}$ is assumed to be an identically and independently distributed unobserved shock. If the assignment of neighbourhoods would be random and the effects of the policy would be immediate and permanent, we would identify a causal effect of α. However, only deprived neighbourhoods are selected, which implies a correlation between $\epsilon_{\ell t}$ and $k_{\ell t}$. We therefore employ a first-difference approach, where the change in the outcome variable, $\Delta y_{\ell t}$, is regressed on the change in the investment, $\Delta k_{\ell t}$, which equals one when we observe a property located in a targeted area *before* and *after* the starting date of the programme and is zero otherwise. To control for changes to the house (e.g. improvements in maintenance that may disproportionally occur in neighbourhoods with older houses), we will include changes in housing variables $x_{\ell t}$ implying:

(3) $$\Delta y_{\ell t} = \alpha \Delta k_{\ell t} + \beta \Delta x_{\ell t} + \Delta \upsilon_t + \Delta \epsilon_{\ell t},$$

The above specification ignores the possibility of spatial spillovers. However, houses close to a targeted area may also experience changes in $y_{\ell t}$ because positive effects are likely to decay over space (Rossi-Hansberg et al., 2010). We therefore exclude observations within two and a half kilometres of a targeted neighbourhood.[15] When estimating (3), the crucial identifying assumption for consistent estimation of α is that unobserved trends are uncorrelated with the change in treatment $\Delta k_{\ell t}$. This assumption may be problematic, e.g. because of demographic trends such as gentrification. We therefore need to find neighbourhoods that are almost identical to the KW-neighbourhoods but are not targeted by the investment scheme.

An identification strategy which comes close to random sampling is a regression-discontinuity design (RDD), implying that we compare the change in the outcome variable close to the z-score threshold. We therefore combine first-differencing with a RDD based on the deprivation score of the neighbourhood.[16] This approach approximately provides the causal effect of the investment if neighbourhoods are not able to manipulate the score. The latter is plausible because the deprivation score was a function of 18 indicators that are very difficult to influence in the short run (including subjective feelings about the neighbourhood, level of education and housing stock). What is more important, the investment programme was announced in 2007, based on data from 2006 and 2002. It is therefore highly unlikely

[15] In the sensitivity analysis (Section VI.D), we investigate whether the presence of potential spatial spillovers invalidate our results.

[16] One may also estimate a cross-sectional RDD by comparing treated neighbourhoods with non-treated neighbourhoods after the treatment has taken place. We think that the latter set-up requires stronger identifying assumptions because all time-invariant *and* time-varying unobservable factors should be uncorrelated to the treatment, rather than time-varying unobservables only. Nevertheless, if the RDD set-up is valid, this should not affect the consistency of the parameters. However, because many (unobservable) factors that influence prices and sales times are omitted, the approach may be quite inefficient. Indeed, in Section VI.I, we show that point estimates are similar to the baseline estimate, but the confidence intervals are substantially wider.

that local governments anticipated the exact selection criteria. The McCrary tests will also confirm that manipulation is not an issue here.

In principle, to avoid any bias, one would prefer to only include observations that are *at* the z-score threshold, so $c = 7.30$. However, this would lead to a few number of observations and therefore to large standard errors. Hence, we estimate (3) using a weighted regression, which can be interpreted as a local linear (LL) regression approach, where observation close to the threshold receive a higher weight (Hahn et al., 2001). This implies:

$$(\hat{\alpha}, \hat{\beta}, \hat{\upsilon}_t) = \underset{\alpha,\beta,\upsilon_t}{\arg\min} \sum_{i=1}^{N} K\left(\frac{z_{i\ell} - c}{h}\right) \times (\Delta y_{\ell t} - \alpha \Delta k_{\ell t} - \beta \Delta x_{\ell t} - \Delta \upsilon_t)^2, \tag{4}$$

where $K(\cdot)$ denotes the kernel function. We use a uniform kernel:

$$K\left(\frac{z_{i\ell} - c}{h}\right) = 1_{|z_{i\ell} - c| < h}, \tag{5}$$

where h is the bandwidth that indicates how many observations are included on both sides of the threshold. The estimated parameters are usually sensitive to the choice of the bandwidth. We use the approach proposed by Imbens and Kalyanaraman (2012) to determine the optimal bandwidth.[17]

Although local governments could not directly manipulate the neighbourhood score, some neighbourhoods were removed from the ultimate list and replaced by others after discussions with the local governments (as discussed in the previous section). This makes a standard sharp regression-discontinuity design (SRD) potentially invalid, as it assumes a one-to-one relationship between the assignment and the z-score. We then employ a fuzzy regression-discontinuity design (FRD), because the neighbourhoods that were removed may be a non-random selection of eligible neighbourhoods. A FRD can be interpreted as an instrumental variables approach (Imbens and Lemieux, 2008). Hence, in the first stage, we regress the change in investment status on a dummy whether the neighbourhood was eligible based on the scoring rule and timing:

$$\left(\hat{\tilde{\pi}}, \hat{\tilde{\beta}}, \hat{\tilde{\upsilon}}_t\right) = \underset{\tilde{\pi},\tilde{\beta},\tilde{\upsilon}_t}{\arg\min} \sum_{i=1}^{N} K\left(\frac{z_{i\ell} - c}{h}\right) \times \left(\Delta y_{\ell t} - \tilde{\pi} \Delta s_{\ell t} - \tilde{\beta} \Delta x_{\ell t} - \Delta \tilde{\upsilon}_t\right)^2, \tag{6}$$

where the $\sim$ indicates first-stage coefficients and $\tilde{\pi}$ is the parameter of interest. Here, $\Delta s_{\ell t}$ equals one when $z \geq 7.30$ and when a property is sold before and after the investment. In Figure 1, it was shown that $\tilde{\pi}$ was highly statistically significant at the neighbourhood level. The coefficient was about 0.7; note that when we would have a SRD, $\tilde{\pi}$ must have been equal to one. In the second stage we then insert $\Delta \hat{k}_{\ell t}$ (and calculate standard errors taking into account that $\Delta \hat{k}_{\ell t}$ is estimated):

$$(\hat{\alpha}, \hat{\beta}, \hat{\upsilon}_t) = \underset{\alpha,\beta,\upsilon_t}{\arg\min} \sum_{i=1}^{N} K\left(\frac{z_{\ell} - c}{h}\right) \times \left(\Delta y_{\ell t} - \alpha \Delta \hat{k}_{\ell t} - \beta \Delta x_{\ell t} - \Delta \upsilon_t\right)^2. \tag{7}$$

[17] See the Appendix B.2 for the derivation of the optimal bandwidth.

Because we employ a FRD, the formula to determine the optimal bandwidth is somewhat modified (see Imbens and Kalyanaraman, 2012 and Appendix B.2). We note that the optimal bandwidth in a FRD is usually very similar of the optimal bandwidth in a SRD

Note that with a FRD we only identify the *local* average treatment effect at the threshold. If treatment effects vary across targeted areas (for example, a euro invested in the most deprived neighbourhood is more effective than a euro invested in the 83rd deprived neighbourhood), the local average treatment effect would differ from the average treatment effect of the policy. Nevertheless, when α would be similar to the estimation procedure where we include all neighbourhoods (see equation (3)), this would suggest that the local average treatment effect at the threshold is equal to the average treatment effect.

Recall that because we look at changes in prices and sales times, each observation refers to two housing transactions. Because we have an unbalanced panel, only a certain percentage of the observations in treated neighbourhoods are referring to transactions *before* and *after the treatment*. In the empirical analysis, we also estimate an equation where we *only* include observations that refer to changes before and after the starting date of the programme.

To get more insight into the mechanism of the effects we also gather data on demographic variables of the neighbourhood, such as population density and share of foreigners. If the place-based investment mainly refers to an improved quality of the neighbourhood, we expect that adding these variables will not change the coefficient of interest. This will add to the credibility of the regression-discontinuity design: in a valid RDD adding control variables does not affect the consistency of the estimated parameter. On the other hand, if sorting effects are very important, part of the positive effect of place-based policies might be explained by changes in the demographic composition of a neighbourhood (Rossi-Hansberg et al., 2010).

We are also interested in adjustment effects after the investment has taken place. Recall that according to theory, the price effect is permanent whereas the sales time effect is temporary. We then define a variable $d_{\ell t}$ that indicates how many years after the investment the transaction has taken place and estimate:

$$\begin{aligned}(\hat{\alpha},\hat{\beta},\hat{\delta}_{\wp},\hat{\upsilon}_t) = \underset{\alpha,\beta,\delta_{\wp},\upsilon_t}{\arg\min} \sum_{i=1}^{N} K\left(\frac{z_{i\ell}-c}{h}\right) \\ \times\left(\Delta y_{\ell t}-\alpha\Delta k_{\ell t}-\sum_{\wp=1}^{\mathcal{P}}\delta_{\wp}\Delta(k_{\ell t}\times d_{\ell t})^{\wp}-\beta\Delta x_{\ell t}-\Delta\upsilon_t\right)^2.\end{aligned} \tag{8}$$

where α indicates the immediate effect and $\delta_{\wp}$ are parameters that capture adjustment effects. The above equation indicates that we have $\wp+1$ endogenous variables. The instruments are then changes in the scoring rule dummy and the change in the interaction of the scoring rule and the $\wp$'th polynomial of years after the investment.

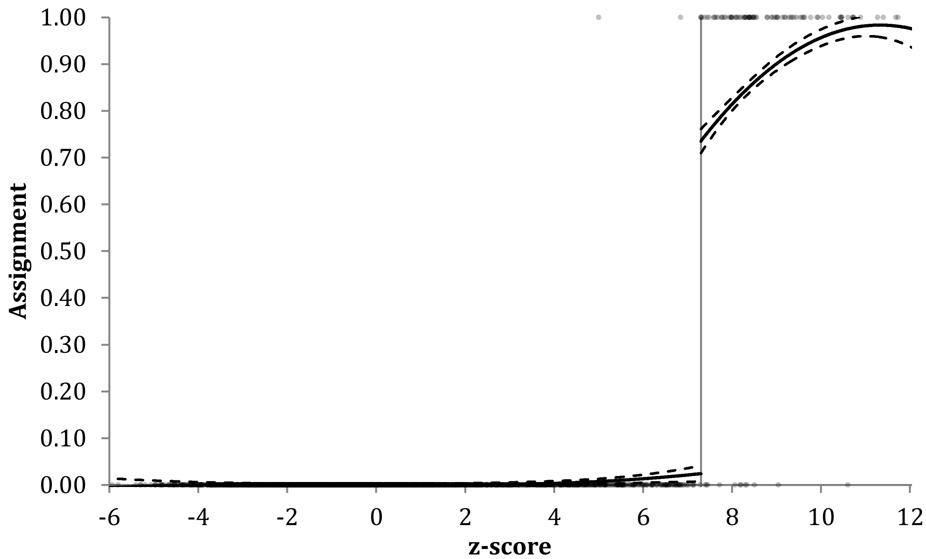


FIGURE 2 — THE Z-SCORE AND SELECTION

Notes: This is a regression of the assignment of a neighbourhood on the scoring rule dummy and a third-order polynomial of the z-score on the left side the threshold and a second-order polynomial on the right side of threshold. The number of observations is 4,016.

D. Graphical analyses

Before we turn to the main regression results, we first illustrate some of the features of our research design, as well as testing some assumptions underlying a RDD. We start the analysis by plotting the assignment as a function of z-scores in Figure 2. While controlling flexibly for the z-score on both sides of the boundary, it is shown that there is a substantial discrete jump in the probability to become selected when $z \geq 7.30$. For example, a neighbourhood with a z-score of 7.29 has a probability of 2.4 percent to be included, whereas for a neighbourhood with a z-score of 7.30 this probability is 73.5 percent. An important assumption of a RDD is that the density of the z-score is continuous at the threshold. Otherwise, neighbourhoods may have manipulated the z-score and therefore the propensity to become treated. By estimating the McCrary (2008) test, Figure 3 shows that the density of z-scores around the threshold is continuous.[18]

In Figure 4 we then plot price changes and changes in time on the market around the threshold, while controlling for the z-score. Note that this is not a standard RDD-design in levels. The latter would require stronger identifying assumptions because all time-invariant

[18] When we concentrate on the neighbourhoods around the threshold using a bandwidth that is in the same order of magnitude as in the empirical application ($h = 3.5$), Figure B1 in Appendix B also strongly suggests that the distribution of z-scores around the threshold is continuous.

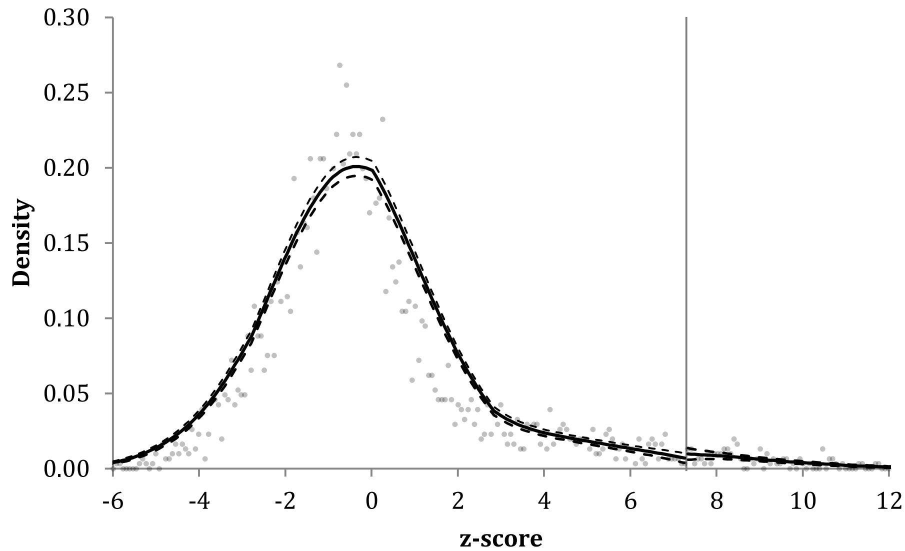


FIGURE 3 — MANIPULATION TEST FOR Z-SCORES

Notes: We estimate the test developed by McCrary (2008) to investigate whether the running variable (the z-score) is continuous around the threshold. The dotted lines represent 95 percent confidence intervals.

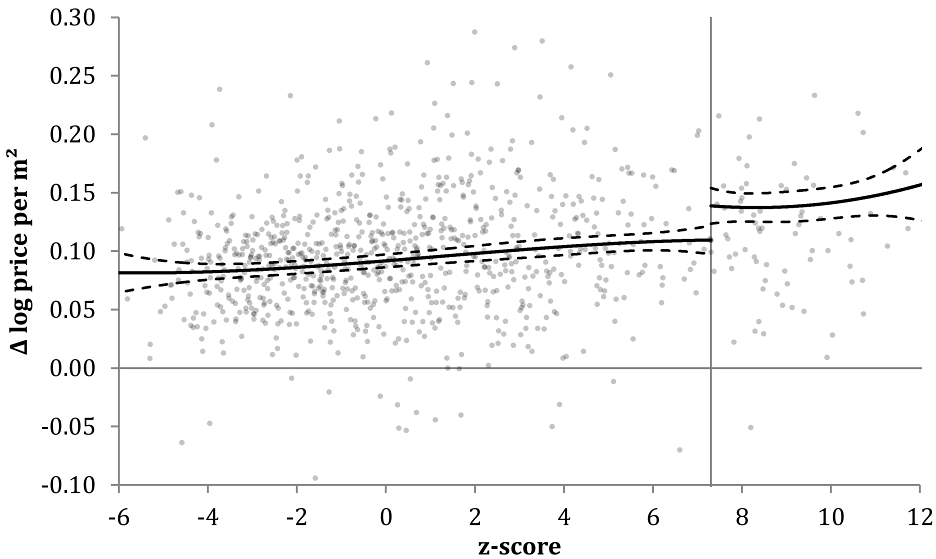
and time-varying unobservable factors should be uncorrelated to the treatment around the cut-off. When we identify the effect based on changes, then only time-varying unobservables should be uncorrelated to the treatment around the cut-off. Moreover, because many (unobservable) factors that influence prices are omitted, the approach using variation in price and sales time *levels* may be inefficient and lead to large standard errors (Imbens and Lemieux, 2008). We illustrate this in more detail in Appendix B.1 where we analyse prices and selling times in levels around the threshold. It can be seen that prices seem to be lower and selling times longer in neighbourhoods above the threshold, before the treatment. However, after the treatment this difference is not statistically significant anymore.[19]

We therefore exploit variation in changes in prices and selling time before and after the treatment and around the threshold. Price changes seem to be about 3 percent higher when a neighbourhood exceeds the z-score threshold. For days on the market we observe an economically significant drop in sales times of about 13 percent, but the confidence bands are quite wide.[20]

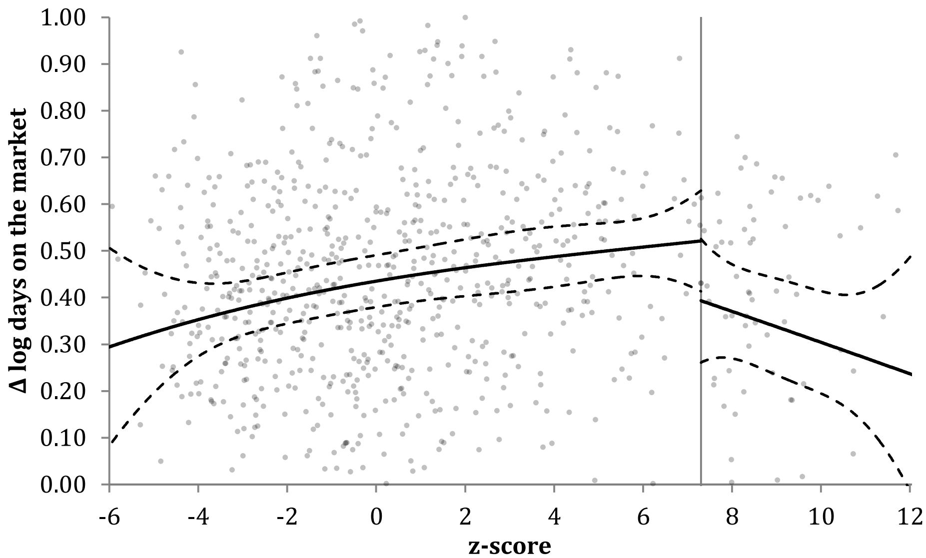
We also test whether the change in other covariates is continuous at the threshold. We

[19] One may be worried that because neighbourhoods seem not to be fully identical before the threshold there may also be unobservable trends that are correlated to the treatment status. We address this issue in depth in Section VI.F.

[20] These results are essentially identical if we use higher order polynomials. We illustrate this in Appendix B.3 with fifth-order polynomials on both sides of the threshold.



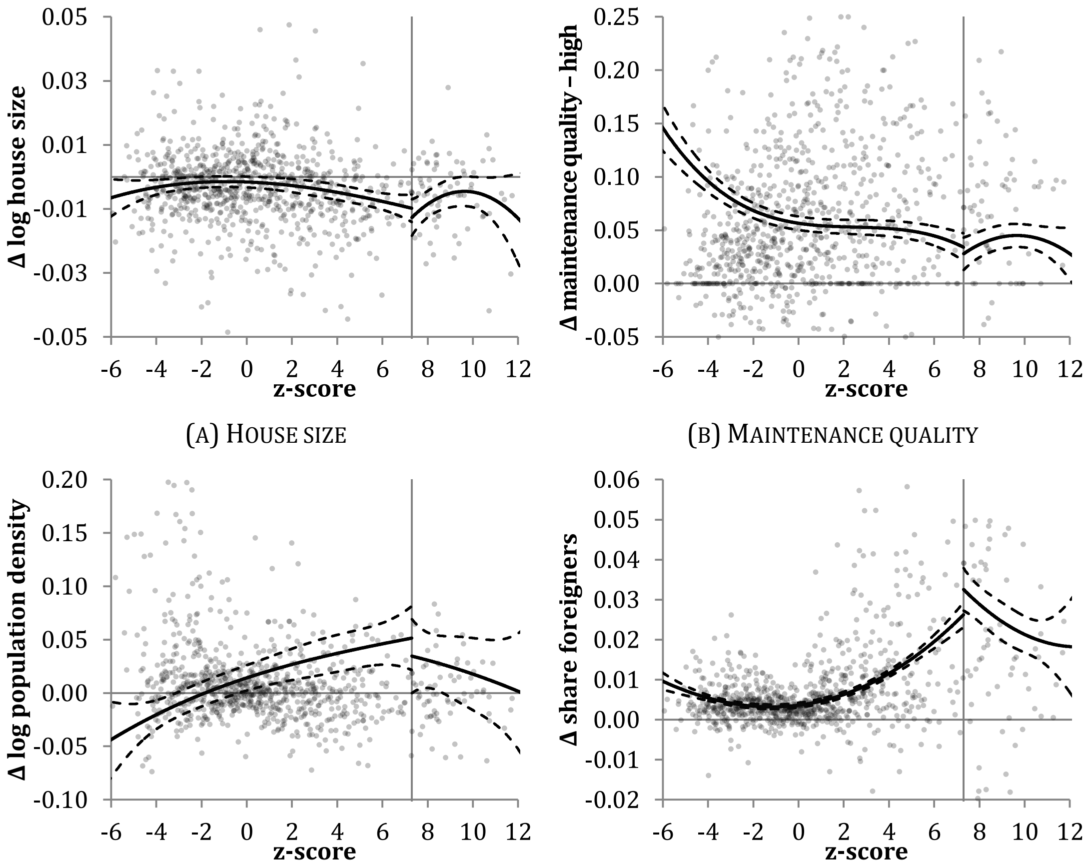
(A) HOUSE PRICE PER M²



(B) DAYS ON THE MARKET

FIGURE 4 — CHANGES IN HOUSE PRICES AND SALES TIMES AROUND THE THRESHOLD

Notes: We estimate weighted regressions of the change in either log prices or log days on the market on year fixed effects, a third-order polynomials of the z-score on the left side of the threshold and a second-order polynomial on the right side of the threshold, as well as a dummy indicating the change in treatment status. The weights are equal to the inverse of the number of observations in a neighbourhood. Each dot represent the conditional average for a given z-score.



(A) HOUSE SIZE

(B) MAINTENANCE QUALITY

(C) POPULATION DENSITY

(D) SHARE FOREIGNERS

FIGURE 5 — CHANGES IN COVARIATES AROUND THE THRESHOLD

Notes: We estimate weighted regressions of the variable of interest on year fixed effects, a third-order polynomials of the z-score on the left side of the threshold and a second-order polynomial on the right side of the threshold, as well as a dummy indicating the change in treatment status. The weights are equal to the inverse of the number of observations in a neighbourhood. Each dot represent the conditional average for a given z-score.

look at the change in house size, whether the house is maintained well, the change in population density and the share of foreigners. It can be seen that for the first three covariates we do not detect any statistically significant changes around the threshold. For the share of foreigners we observe that the change in share foreigners is higher in neighbourhoods that are above the threshold. Note that this may also be a direct result of the policy, as due to the expenditures on public housing, the social composition of the neighbourhood may have be changed. In the empirical analysis we therefore will estimate specifications where we control for a number of demographic characteristics of the neighbourhood and show that the effects on prices and sales times are essentially the same.

IV. Results

A. Baseline results – house prices

We expect a positive price effect in the neighbourhood that received the KW-investment compared to the non-treated neighbourhoods in line with *prediction (i)* from the discussion in Section II. Table 3 reports the regression results. In all specifications, we cluster the standard errors at the neighbourhood level, because the treatment varies at the neighbourhood level. For now, we ignore differences between short-run and long-run effects.

We start with a naïve regression of the change in house price on the change in the treatment status. The coefficient in column (1) shows that investments seem to have generated a positive effect on prices of 4.5 percent.[21] When we control for changes in housing attributes (column (2)), prices in targeted neighbourhoods have increased with 3.8 percent, relative to prices in other neighbourhoods. In column (3) we employ a sharp regression-discontinuity design by excluding non-KW-neighbourhoods with a z-score above the threshold and KW-neighbourhoods with a z-score below the threshold. We find an optimal bandwidth of 5.13, which implies that we only include about 25 percent of the observations. The price effect is 3.4 percent and somewhat lower than in previous specifications.[22] Because the neighbourhoods that were not treated while they have a sufficiently high z-score might be a non-random sample of the neighbourhoods with $z \geq 7.3$, it is preferable to employ a fuzzy regression-discontinuity design. In the first stage we regress the change in the assignment variable on the change in the scoring rule of a property (see Table B3 in Appendix B.4). In all the specifications, having a z-score above the threshold is a very strong instrument of being treated ($F > 2500$), with a coefficient close to one: houses in neighbourhoods that are in a neighbourhood with $z > 7.3$ have an approximately 98 percent higher probability to become treated. Note that the jump in probability to become treated is higher than recorded in Figure 2, because neighbourhoods are not of equal size (in terms of the number of housing units). Hence, we have relatively fewer observations in non-KW-neighbourhoods with a z-score above the threshold and KW-neighbourhoods with a z-score below the threshold. The second stage results are in line with previous specifications. The result in column (4), Table 3, implies that prices in KW-neighbourhoods have increased with 3.3 percent due to the investment programme. In column (5) we explore the robustness of the findings further by removing the observations that are referring to transactions that both occur before or after the treatment date. While this reduces the sample size with about 50 percent, this hardly has an impact on the price effect (3.6 percent).

The final column (6) sheds some light on the potential mechanisms driving the price effect. Place-based policies may increase the amenity level, but may also influence the

[21] The marginal effect is calculated as $e^{\hat{\alpha}} - 1$.

[22] One may argue that controls are not necessary in a valid RDD. Indeed, the point estimates are essentially identical if exclude control variables, but slightly less precise. Nevertheless, the estimates are always at least statistically significant at the five percent level. Those results are available upon request.

TABLE 3 — REGRESSION RESULTS: THE EFFECT OF PLACE-BASED POLICIES ON HOUSE PRICES
(Dependent variable: change in log house price per square meter)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	SRD	FRD	FRD	FRD
Δ KW-investment	0.0441***	0.0372***	0.0338***	0.0329***	0.0358***	0.0334***
	(0.0114)	(0.0104)	(0.0117)	(0.0122)	(0.0122)	(0.0118)
Δ Size *(log)*		-0.877***	-0.885***	-0.889***	-0.887***	-0.876***
		(0.00586)	(0.0138)	(0.0139)	(0.0177)	(0.0194)
Δ Rooms *(log)*		0.00296***	0.00362**	0.00297*	0.00515***	0.00389*
		(0.000475)	(0.00157)	(0.00156)	(0.00195)	(0.00204)
Δ Maintenance quality – high		0.106***	0.0978***	0.0940***	0.0990***	0.0958***
		(0.00151)	(0.00334)	(0.00351)	(0.00378)	(0.00408)
Δ Central heating		0.0648***	0.0676***	0.0688***	0.0804***	0.0738***
		(0.00250)	(0.00501)	(0.00508)	(0.00636)	(0.00687)
Δ Listed building		0.00239	0.0107	0.00855	0.000337	-0.00890
		(0.00805)	(0.0163)	(0.0188)	(0.0187)	(0.0203)
Δ Population density *(log)*						0.0635
						(0.0815)
Δ Share foreigners						-1.045***
						(0.154)
Δ Share young people						0.213
						(0.485)
Δ Share elderly people						-0.703**
						(0.293)
Δ Average household size						0.0609
						(0.114)
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Land use variables (4)	No	No	No	No	No	Yes
Number of observations	169,664	169,664	24,353	22,589	12,766	10,484
Number of clusters	3199	3100	235	186	250	195
R^2-within	0.375	0.538	0.549			
Kleibergen-Paap F-statistic				5444	8063	2571
Bandwidth h^*			4.099	3.383	4.312	3.547

Notes: We exclude observations within 2.5 kilometres of targeted areas. In Column (3) we exclude non-targeted neighbourhoods with a z-score above 7.3 and targeted neighbourhood with a z-score below 7.3. In Columns (4)-(6) the change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level and in parentheses.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

composition of the population. For example, when the type of houses in the neighbourhood increases due to the place-based policy, age composition of the households may change. These indirect effects may partly explain the effects on prices. In Appendix B.5 we explore whether neighbourhood demographics are influenced by the policy. We find evidence that KW-neighbourhoods have seen a relative increase in the share of foreigners, as well as an decrease in the share of elderly people (>65 years). Also the average household size seems to have increased. To test whether those have caused price changes, we control for additional demographic variables. More specifically, we include the changes in population density, the share of foreigners, share of young (<25 years) and elderly people and the average household size and land use.[23] Increases in population density are associated with price increases. Furthermore, the share of foreigners is correlated with price decreases. More importantly, the coefficient of interest is hardly affected by inclusion of these controls (3.4 percent), which suggests that sorting on observable neighbour characteristics is not a main determinant of the statistically significant effect of place-based policies. This seems to suggest that the effect of the place-based investments is mainly due to a direct change in the quality of nearby public housing rather than due to sorting effects.

B. Baseline results – sales time

In most empirical analyses, the effects of sales time are ignored. We hypothesised that sales time effects may be present, at least in the short run (*prediction (iv)*), because it takes time for the market to adjust to a new steady state. For now, as above, we ignore differences between short-run and long-run effects and just estimate the average treatment effect over time. Table 4 reports the baseline results.

In column (1) we start again with a naïve regression of the change in the logarithm of days on the market on whether a property has experienced a change in the treatment status. This specification suggests that the sales time has been reduced with 8.1 percent due to the investment. If we control for housing attributes in column (2), the coefficient is essentially the same. In column (3) we employ the sharp regression-discontinuity design and exclude non-KW-neighbourhoods with a z-score above the threshold and KW-neighbourhoods with a z-score below the threshold. The effect then becomes somewhat stronger (−13.9 percent). Next, we do not exclude neighbourhoods but use an instrumental variable approach instead, with the change in the scoring rule as the instrument. Note that the first stage results are almost identical to the price regressions (see Table B3 in Appendix B.4). The fuzzy regression-discontinuity design leads to similar second stage results: column (4) in Table 4 suggests that the investment has led to a 13.8 percent decrease in sales time. The optimal bandwidth is somewhat larger than in the price regressions, possibly because of a greater variance of the dependent variable. In column (5) we only include observations for which transactions occur before and after the treatment date leading to similar results: the place-based investment seems to have reduced sales times with 17.6 percent. This effect is very similar (−14.9 percent) if we control for changes in demographics in column (6).

[23] We include variables related to changes in land use using data from Statistics Netherlands for 2000, 2003, 2006, 2008 and 2010. We match each transaction year to the nearest preceding year of the land use data. This may lead to some bias, but as the average time difference between transactions of the same property is almost four years, we expect that the bias is limited. We then calculate the share of land used for housing, commercial activities, infrastructure and open space for each neighbourhood.

TABLE 4 — REGRESSION RESULTS: THE EFFECT OF PLACE-BASED POLICIES ON SALES TIME
(Dependent variable: change in log days on the market)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	SRD	FRD	FRD	FRD
Δ KW-investment	-0.0843*	-0.0843*	-0.150***	-0.149***	-0.193***	-0.161***
	(0.0473)	(0.0470)	(0.0529)	(0.0501)	(0.0533)	(0.0562)
Δ Size *(log)*		0.198***	0.165	0.161	0.0996	0.0885
		(0.0658)	(0.144)	(0.103)	(0.167)	(0.130)
Δ Rooms *(log)*		-0.0271***	-0.0352***	-0.0382***	-0.0383***	-0.0262**
		(0.00535)	(0.0134)	(0.00966)	(0.0149)	(0.0113)
Δ Maintenance quality – high		0.0642***	0.0535**	0.0453**	0.0531*	0.0871***
		(0.0126)	(0.0260)	(0.0183)	(0.0298)	(0.0238)
Δ Central heating		-0.0538***	-0.103***	-0.103***	-0.137***	-0.112***
		(0.0162)	(0.0317)	(0.0238)	(0.0375)	(0.0307)
Δ Listed building		0.0186	0.0535	0.0596	0.111	0.0724
		(0.0554)	(0.0842)	(0.0673)	(0.0893)	(0.0830)
Δ Population density *(log)*						-0.113
						(0.140)
Δ Share foreigners						0.564
						(0.671)
Δ Share young people						-1.682
						(1.145)
Δ Share elderly people						-0.362
						(0.731)
Δ Average household size						-0.118
						(0.316)
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Land use variables (4)	No	No	No	No	No	Yes
Number of observations	169,664	169,664	34,569	64,324	22,447	36,905
Number of clusters	3100	3100	351	838	498	1242
R^2-within	0.057	0.057	0.060			
Kleibergen-Paap F-statistic				16228	14819	9660
Bandwidth h^*			5.153	6.950	6.147	7.645

Notes: We exclude observations within 2.5 kilometres of targeted areas. In Column (3) we exclude non-targeted neighbourhoods with a z-score above 7.3 and targeted neighbourhood with a z-score below 7.3. In Columns (4)-(6) the change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level and in parentheses.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

C. Adjustment effects

We will now explicitly distinguish between short-run and long-run effects by allowing for adjustment effects. We estimate equation (8) and use the local linear approach without

TABLE 5 — REGRESSION RESULTS: ADJUSTMENT EFFECTS

	Panel 1: Δ Price per m^2 *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Δ KW-investment	0.0199**	0.0215*		-0.275***	-0.257***	
	(0.00892)	(0.0119)		(0.0676)	(0.0975)	
Δ (KW-investment × years after investment)	0.00393*	0.00265		0.0364***	0.0222	
	(0.00230)	(0.00471)		(0.0134)	(0.0467)	
Δ (KW-investment × years after investment)^2		0.000170			0.00174	
		(0.000704)			(0.00540)	
Δ KW-investment × I(0.0-2.5 years after investment)			0.0251**			-0.235***
			(0.00998)			(0.0631)
Δ KW-investment × I(2.5-5.0 years after investment)			0.0381***			-0.150**
			(0.0120)			(0.0602)
Δ KW-investment × I(5.0-7.5 years after investment)			0.0406**			-0.0231
			(0.0184)			(0.0698)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	22,589	22,607	22,589	61,950	60,837	63,643
Kleibergen-Paap F-statistic	2717	1537	2065	6359	3570	31324
Bandwidth h	3.385	3.408	3.393	6.780	6.749	6.884

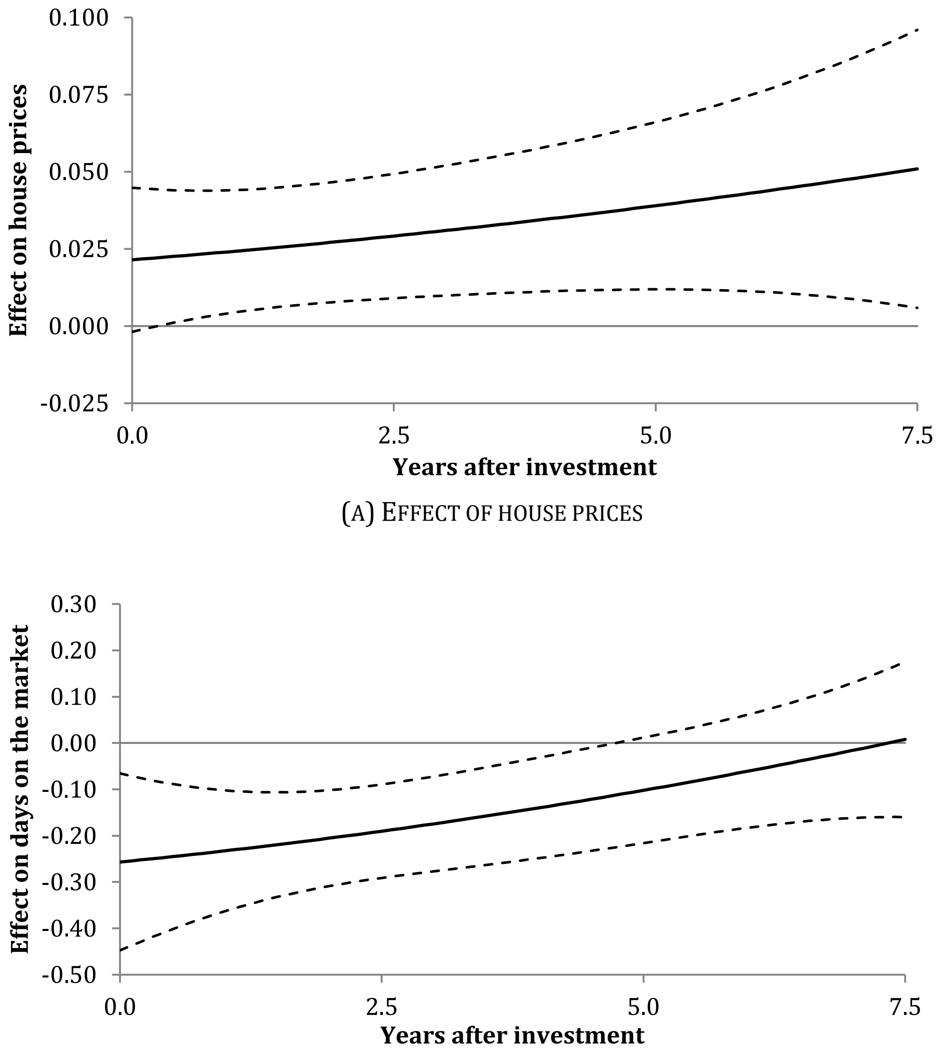
Notes: The instruments are Δ Scoring rule and the change in interactions of the scoring rule with the days after the investment. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

neighbourhood variables, which corresponds to the specification listed in column (4) in Table 3 and Table 4. We report the estimated coefficients in Table 5.[24] Recall that according to theory, we expect that the price effect is immediate and permanent (*prediction (iii)*). On the other hand, sales times are expected to become smaller over time and disappear in the long run (*predictions (ii)* and *(iv)*).

In column (1) we include a linear interaction term of the treatment status with the time after the investment (measured in years). It is shown that there is an immediate price effect (2.0 percent). The linear interaction term is positive, but small and only marginally statistically significant. The specification predicts that after five years the price effect is 4.0 percent (and statistically significant at the one percent level), which is similar to the baseline estimate. Column (2) includes also a second-order term leading to statistically insignificant

[24] The bandwidth is optimised assuming that the interaction terms are exogenous variables. Given that the bandwidth is very similar for the SRD and the FRD, we do not expect that this has any impact on the results.



(A) EFFECT OF HOUSE PRICES

(B) EFFECT OF SALES TIME

FIGURE 6 — EFFECT OF HOUSE PRICES AND SALES TIME AFTER THE INVESTMENT

Notes: The black line indicates the main effect over time. The dashed lines indicate the 95 percent local confidence bands computed using the delta method.

coefficients. However, it is more insightful to test the joint significance of these coefficients over time. The results are presented in Figure 6A. After five years the price effect is 3.8 percent, while the immediate price effect is 2.2 percent. In column (3) we include interaction terms of the treatment variable and 2.5 years interval dummies. The same pattern emerges: the price effect is increasing over time, but not so strongly and the price coefficients are only marginally statistically significantly different from each other (p-value = 0.0659). The price effect in the first 2.5 years might also a bit lower because of uncertainty about the exact starting date of the programme (an issue which we discuss in more detail in Section VI.G).

Hence, the results seem to confirm that the price effect is permanent (*prediction (i)*) and that the price jumps once the policy was announced (*prediction (iii)*).

Let us now investigate the adjustment effects of sales times after the announcement of the investment programme. It seems that the sales time effect is immediate and substantial (see Column (4), Table 5). The decrease in sales times is 22.4 percent, which is on average about a month reduction in sales times. The effect of sales times tends to become less pronounced over time. After five years, the effect is 9.6 percent and only marginally statistically significant (p-value $= 0.0931$). After 7.5 years, the effect is essentially zero. The same holds if we include a second-order term in Column (5). Figure 6B shows the effects over time, which displays results that are very similar to the previous specification. Column (6) includes interaction terms, resembling the same pattern. The sales time effect is the strongest in the first period, while it converges to zero within 7.5 years. Hence, these outcomes confirm *predictions (ii)* and *(iv)* that place-based investments have a permanent effect on house prices, whilst only a temporary effect on sales time effect because the market has to adjust to a new steady state. The results for sales time give us also more confidence in the results for house prices. Recall that house prices and sales time tend to be negatively correlated. Let us suppose now that our house price results are completely spurious due to omitted variables. In that case, one would also expect to observe a permanent effect on sales time, in contrast to our results which show a temporary effect on sales time.

V. Counterfactual analysis

We aim to gain insight in the rate of return of the external effect of the revitalisation policy using a counterfactual analysis. We reiterate that we measure external effects because we focus on investments in the public housing stock on the prices and sales times of owner-occupied properties. Expenditures through the KW-programme were financed from additional and external sources and were not part of the municipal budget or the budget of housing associations. In contrast, when expenditures are e.g. raised by limiting expenses in other neighbourhoods, this may imply that externalities are negative in non-targeted areas (Rossi-Hansberg et al., 2010). In any case, one should be very careful in interpreting the results as an overall measure of general equilibrium welfare benefits of the investment programme, but we consider them as partial equilibrium results.

We use additional data on the number of housing units from Statistics Netherlands. We estimate the benefits and costs in 2007 prices, by deflating house prices by the consumer price index, obtained from Statistics Netherlands. We assume that the average price is constant across the study period, so $p_{\ell t} = p_{\ell}$. To estimate the average price for owner-occupied housing in each neighbourhood, we take the average of deflated prices of all transactions in our study period. In the Netherlands, for the large majority of rental properties the rents are controlled. Although the subsidy does not capitalise in controlled rents, renters enjoy the positive neighbourhood effects that are caused by the programme.

TABLE 6 — COUNTERFACTUAL ANALYSIS: BENEFITS OF THE PROGRAMME

	Benefits per household *(in €)*		Total benefits *(in billion €)*	
	Owner-occupied	*All properties*	*Owner-occupied*	*All properties*
Price effect				
Baseline estimate	5223	5063	0.481	1.939
Long-run estimate	6345	6151	0.585	2.355
Welfare effect				
Baseline estimate	5438	5396	0.501	2.066
Long-run estimate	6607	6555	0.609	2.510

Notes: The estimated benefits are in 2007 prices. The data on the number of housing units are from 2012 and obtained from Statistics Netherlands. To obtain the welfare estimates we use a vacancy rate of 3.96 percent for owner occupied housing and 6.20 percent for all properties, based on data from Statistics Netherlands.

Because the share of owner-occupied housing is small in KW-neighbourhoods (only 24 percent), the benefits must be substantially larger once we allow for effects on renters. To include these social benefits, we make the strong assumption of an identical percentage price effect for the rental market. Furthermore, we gather data on the average house prices of all properties in each neighbourhood, including rental properties, which are somewhat lower than the price for owner-occupied properties.[25] We interpret the results for all housing units as an upper-bound estimate. Table 6 reports the back-of-the-envelope calculations.[26]

We start with the parsimonious estimate of the benefits. The average increase in house prices is then about € 5 thousand, which is indeed approximately 3 percent of the mean house price. The results indicate gains of about € 5 thousand per house owner. The effect is somewhat higher once we use the long-run estimate. To calculate the welfare effects, we multiply the price effects with a factor $(1 + v/\bar{S})$, in line with equation (A17). As the average vacancy rate is about 4 percent for owner-occupied housing in the Netherlands, this factor is about 1.04. Hence, the welfare effect is very similar to the price effect. For the average effect on all properties, the price effect is somewhat lower, because the average house price for all properties is lower than the average house price of owner-occupied housing. Relatively, the welfare effect is a bit higher than the price effect, because the average vacancy rate for all properties is 6.2 percent. The total benefits for home owners are about €

[25] We ignore that house owners can deduct their interest mortgage payments from their income, so prices of owner-occupied housing may somewhat exceed house prices compared to an unregulated market.

[26] One may argue that the welfare calculation is incomplete because we do not take into account the welfare benefits that arise in neighbourhoods that are close but did not get the subsidy (Glaeser and Gottlieb, 2008). We show in the sensitivity analysis that there is weak evidence for spatial spillovers, although the confidence intervals are quite large. Hence, the estimates presented here are, if anything, underestimates of the total effects of place-based policies.

0.5 billion. The results indicate the gain-to-funding ratio is about 0.5 *if* the realised investments are indeed € 1 billion.

To also include the social benefits on renters, we use the average house prices of all properties. Because the share of owner-occupied housing is small in KW-neighbourhoods (only 24 percent), the benefits are now substantially larger. The results suggest a gain-to-funding ratio of 1.9, in line with Rossi-Hansberg et al. (2010). This might be accidental, because the programmes are different in many aspects. Welfare effects of the programme are again somewhat higher. When we take into account the effect on welfare on the total housing stock, the maximum gain-to-funding ratio is 2.5. However, the latter estimate is probably an overestimate when the external price effects on rental housing are less pronounced than in the owner-occupied market.

Hence, given the assumptions we have to make to arrive at these estimates, the long-run benefits to homeowners induced by the place-based policy programme are about half of the value of the investments. The price effects may be interpreted as a good approximation of welfare effects, as the observed vacancy rates are rather low.

VI. Sensitivity analysis

A. Introduction

In this sensitivity analysis, we subject the baseline results to a wide range of robustness checks. First, we will conduct a series of quasi-'placebo' experiments based on previous investment programmes selecting different neighbourhoods. Second, we will inspect whether our results are robust to the identification strategy by employing a nonparametric propensity score matching method, rather than a regression-discontinuity approach. Third, we test for whether the presence of spatial spillovers of the investment programme changes our conclusions. Fourth, we will estimate city-specific regressions for the largest cities in the Netherlands to show that our results are robust across cities. Fifth, we control for unobserved trends related to distance to the city centre, as well as municipality-specific trends that might be correlated to the treatment status. Sixth, we will test robustness of our results with respect to the starting date of the investment. Seventh, we will test robustness of our results to assumptions with respect to the bandwidth of the local linear regression approach. Eighth, we employ a RDD based on price *level* differences between KW and other neighbourhoods using the full dataset. Finally, we investigate whether using the full sample, rather than repeated sales influences our results. We consider the specification in column (4) in Table 3 and Table 4 as the baseline specification because we identify the effect of interest using all available data while excluding potentially-endogenous neighbourhood attributes.

B. Quasi-placebo experiments

We first conduct a series of quasi-'placebo' experiments using different classifications used in the past of deprived neighbourhoods and differences in timing of programmes to test

TABLE 7 — SENSITIVITY ANALYSIS: QUASI-PLACEBO EXPERIMENTS

	Panel 1: Δ Price per m^2 *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Δ Winsemius neighbourhood	-0.00702			0.192***		
	(0.00564)			(0.0339)		
Δ Kamp neighbourhood		0.00199			0.00266	
		(0.00638)			(0.0400)	0.00980
Δ KW-plus neighbourhood			-0.0125			(0.0882)
			(0.00784)			
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	100,248	59,945	82,722	100,248	59,945	82,722
Number of clusters	2439	2560	2687	2439	2560	2687
R^2-within	0.545	0.444	0.460	0.060	0.063	0.038

Notes: Standard errors are clustered at the neighbourhood level.
*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

whether the effect we found is attributable to the KW-investment programme. Table 7 reports the results.

A list of 340 deprived neighbourhoods was published by the Dutch secretary of state Pieter Winsemius in 2006, of which the 83 neighbourhoods were selected in the end. In the first placebo-experiment we treat the non-targeted neighbourhoods as if they are KW-neighbourhoods and received funds in 2007 and exclude the observations in and close to (within 2.5 kilometres) of a KW neighbourhood. To avoid the possibility that spatial spillovers lead to a bias towards zero of the placebo-estimate, we also exclude observations within 2.5 kilometres of a neighbourhood on the Winsemius list. Columns (1) and (4) highlight that there is no general trend in prices in deprived neighbourhoods that were not targeted. Sales times seem to have increased in non-treated Winsemius neighbourhoods on the Winsemius list. One may therefore be worried that the baseline estimate is identified based on the spurious positive sales time trend of non-treated neighbourhoods. However, when we exclude non-treated neighbourhoods on the Winsemius list from the baseline specification, the coefficient related to sales times is very similar. Hence, this does not seem to be a problem in the main analysis

In 2003 the Dutch secretary of state, Henk Kamp, published another list of the most deprived neighbourhoods in the Netherlands, which received some funding at that time (the size of the programme was however an order of magnitude smaller). There was substantial overlap (about 57 percent of the observations that are in a KW neighbourhood are also in a

‘Kamp’-neighbourhood). Neighbourhoods that are a ‘Kamp’-neighbourhood but not a KW neighbourhood are a feasible ‘placebo’-group. We therefore treat these neighbourhoods as if they are KW-neighbourhoods and received funds in 2007 and exclude the observations in and close to (within 2.5 kilometres) of a KW neighbourhood and before 2003. Again, we also exclude observations within 2.5 kilometres of a ‘Kamp’-neighbourhood to avoid biases due to spatial spillovers. Columns (2) and (5) in Table 7 show that the coefficients for house prices and sales time are highly statistically insignificant. This result is particularly convincing for house prices, where the standard error of the estimate is smaller than in the previous specifications. This supports the conclusion that our results indeed are driven by the KW-investment and not by other investments or a general price trend in deprived neighbourhoods.

The last quasi-placebo experiment relies on another definition of deprived neighbourhoods. There was a substantial controversy around the selection of the 83 deprived neighbourhoods. One critique was that most of these neighbourhoods were located in the suburbs of the largest cities in the Netherlands. By the end of 2009 26 additional neighbourhoods were selected that received some funding from 2010 onwards. These so-called KW-plus neighbourhoods might also be considered as a valid placebo group. We therefore again treat these neighbourhoods as if they are KW-neighbourhoods and exclude the observations in and close to (within 2.5 kilometres) of KW and KW-plus neighbourhoods and exclude transactions after 2009. The results in Columns (3) and (6) suggest that there is no meaningful price effect and sales time effect in these neighbourhoods before 2010, which again point to the conclusion that there seem no specific trends that are correlated with the KW-programme.

C. Propensity score matching

Throughout this paper we have used a regression-discontinuity design to estimate the causal effects of investments in deprived neighbourhoods on sales time and house prices. We also investigate robustness of our results to another identification strategy. We will use a propensity score method to select similar ‘control’ neighbourhoods. Rosenbaum and Rubin (1983) propose to estimate a probit model, where a dummy indicating whether a neighbourhood is selected is regressed on a flexible function of covariates, including relevant selection criteria. Based on the idea that neighbourhoods that have similar propensity scores are similar in their attributes, the propensity score is used to match targeted and control neighbourhoods. The neighbourhood attributes are obtained from Statistics Netherlands and include population density, average income, share of people with low income, the share of unemployed people, and the share of households that receive social allowance in 2007 at the neighbourhood level. To capture the degree of social integration, we furthermore include the share of foreigners, the share of young people and share of elderly. The quality of the housing stock is measured by the median construction year, as well as the share of houses that are constructed before 1945 and between 1945 and 1970 (houses in the latter category are thought to have lower quality). We also include a variable indicating the share of open space in the neighbourhood, as well the share of owner-occupied houses. We then estimate the following probit model:

$$\Pr(\ell = 1 \mid a_\ell) = \Phi\big(\Upsilon_\ell(a_\ell)\big), \tag{9}$$

where $\Pr(\ell = 1 \mid a_\ell)$ is the probability that a neighbourhood ℓ is selected, $\Phi(\cdot)$ is the cumulative distribution function of the normal distribution and $\Upsilon_\ell(\cdot)$ is a nonparametric function of attributes a_ℓ. We estimate this model using local likelihood estimation, implying that we estimate for each neighbourhood a weighted probit model (see Fan et al. 1995; 1998). We let the weights depend on geographical location to capture unobserved spatial heterogeneity. Consequently, the impact of a_ℓ on $\Pr(\ell = 1 \mid a_\ell)$ depends on the location of the neighbourhood. The kernel weights for ℓ are equal to $\omega_\ell = 1/d_\ell$, where d_ℓ is a vector capturing the kilometre distance between the centroid of ℓ and the centroids of all other locations (see similarly Fotheringham et al., 2003). To select the control neighbourhoods, we use three different matching techniques (see Rosenbaum and Rubin, 1985; Rosenbaum, 2002). First, we use calipher matching by assuming that the difference in the propensity score between targeted and non-targeted neighbourhoods should be lower than 0.01. We also assume that control neighbourhoods should have at least a propensity score of 0.01. Second, we use nearest neighbour matching without replacement. This implies that we will have 83 KW-neighbourhoods and 83 control neighbourhoods. The third approach also uses nearest neighbour matching, but with replacement. Because we do allow for replacement, the number of control neighbourhoods is lower than the number of targeted neighbourhoods. Table B2 in Appendix B.1 reports the means and standard deviations at the neighbourhood level for the KW-neighbourhoods and three different sets of control neighbourhoods. It appears that the control neighbourhoods are relatively similar to the KW-neighbourhoods in most neighbourhood attributes.[27] Table 8 reports the results.

Columns (1) and (4) use the set of control neighbourhoods based on Caliper matching. The price effect is then 4.4 percent, similar to baseline specifications. The effect on sales times is somewhat larger and 18.5 percent. In Columns (2) and (5) we use nearest neighbour matching without replacement. It can be seen that the price effect of place-based policies is again similar to the baseline specification, while the effect of sales time is again somewhat larger in magnitude. The results suggest that the investments have led to a decrease in sales time of 18.8 percent, which is still in the same order of magnitude as our baseline estimates. In Columns (3) and (6) we use nearest neighbour matching with replacement. This implies

[27] There are two notable differences between the targeted and control neighbourhoods. The first is that population density is about a third lower in the control neighbourhoods. Indeed, targeted areas are on average located in larger cities. Also, the share of foreigners is much lower. We note that the propensity scores of non-control neighbourhoods are very close to zero, suggesting that our model performs reasonably well.

TABLE 8 — SENSITIVITY ANALYSIS: PROPENSITY SCORE MATCHING

	Panel 1: Δ Price per m² *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	PSM	PSM	PSM	PSM	PSM	PSM
Δ KW-investment	0.0426***	0.0500***	0.0407***	-0.204***	-0.208***	-0.190*
	(0.0118)	(0.0101)	(0.0105)	(0.0570)	(0.0719)	(0.0965)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	15,295	11,385	9,851	15,295	11,385	9,851
Number of clusters	144	115	97	144	115	97
R^2-within	0.519	0.507	0.487	0.063	0.066	0.067
Matching method	Caliper	NN no repl.	NN repl.	Caliper	NN no repl.	NN repl.
Control neighbourhoods	116	83	38	116	83	38

Notes: We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

that we have only 38 control neighbourhoods. The price effect, however, is still very similar. The effect on sales times is also similar but only marginally statistically significant due to a relatively large standard error.

D. Spatial spillovers

It is not our purpose to investigate the spatial decay of housing externalities for which one needs the exact location of housing investments (as in Rossi-Hansberg et al., 2010). We aim to show that our results are robust when spatial spillovers are present. Spatial spillovers are defined here as effect on prices of houses located close to, but outside, KW-neighbourhoods (note that houses close to KW-neighbourhoods benefit will not be affected by negative stigmatisation effects, so it is possible that the effect on their house prices even exceeds the effect on the KW-neighbourhoods) . Allowing for spatial spillovers needs to take into account that several KW-neighbourhoods are located close to each other, so that properties outside KW-neighbourhoods benefit from spatial spillovers from multiple treatments. Hence, we include the number of spatial spillovers within 500 m rings of the property.

Note that the identification and the calculation of standard errors of spatial spillover effects for houses which benefit from multiple spatial spillovers is not very clear. To mitigate this issue, we first exclude observations within 2.5 of more than one KW-neighbourhood in column (1) of Table 9. It is shown that the treatment effects (within KW-neighbourhoods) are

TABLE 9 — SENSITIVITY ANALYSIS: SPATIAL SPILLOVERS

	Panel 1: Δ Price per m^2 *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	SRD	FRD	FRD	SRD	FRD	FRD
Δ KW-investment	0.0375***	0.0351***	0.0318**	-0.135**	-0.114**	-0.151***
	(0.0112)	(0.0118)	(0.0151)	(0.0527)	(0.0496)	(0.0545)
Δ Number of KW neighbourhoods, <0.5km	0.0147	0.0123	-0.000438	-0.248***	-0.183***	-0.0412*
	(0.00927)	(0.0113)	(0.00711)	(0.0699)	(0.0589)	(0.0250)
Δ Number of KW neighbourhoods, 0.5-1.0km	0.00782	0.00504	0.000770	-0.158**	-0.139**	-0.0244
	(0.00850)	(0.0103)	(0.00523)	(0.0696)	(0.0546)	(0.0185)
Δ Number of KW neighbourhoods, 1.0-1.5km	0.00148	-0.00210	0.0183***	-0.0197	-0.122**	-0.0205
	(0.0110)	(0.0133)	(0.00438)	(0.0989)	(0.0570)	(0.0150)
Δ Number of KW neighbourhoods, 1.5-2.0km	0.00655	0.0135	0.0189***	0.0919	0.00121	-0.0319**
	(0.0144)	(0.0167)	(0.00463)	(0.0779)	(0.0521)	(0.0138)
Δ Number of KW neighbourhoods, 2.0-2.5km	0.00111	-0.00142	0.00404	0.128	0.0645	-0.0268*
	(0.00967)	(0.0110)	(0.00811)	(0.0844)	(0.0621)	(0.0142)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	51,239	39,918	28,156	49,143	114,536	84,114
Number of clusters	500	355	204	484	1828	676
R^2-within	0.537			0.059		
Kleibergen-Paap F-statistic		3582	193.8		6337	273.2
Bandwidth h	5.260	4.059	2.521	5.162	7.843	6.057

Notes: In Columns (1) and (4) we exclude non-targeted neighbourhoods with a z-score above 7.3 and targeted neighbourhood with a z-score below 7.3. In Columns (2), (3), (5) and (6) the change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level and in parentheses.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

very similar to the baseline estimates (a price effect of 3.8 percent). We do not find any evidence that spatial spillovers are relevant. In columns (2) and (5) we repeat the previous specification, but now using the fuzzy set-up, so we instrument for the change in the treatment status with the change in the eligibility status. The results are very similar then.

In column (3) of Table 9 we do not exclude these observations, so that observations outside KW-neighbourhoods can be within a close distance of multiple KW-neighbourhoods (note that because of a smaller bandwith the number of observations included is reduced). Again, the main effect is unaffected. The coefficients still indicate that that there are no price effects within one km outside treated neighbourhoods. We find statistically significant effects between 1 and 2 kilometres, which may be interpreted as suggestive evidence of spatial spillovers. We repeated the above analysis for days on the market. Again we find that the

TABLE 10 — SENSITIVITY ANALYSIS: CITY-SPECIFIC REGRESSIONS

	Panel 1: Δ Price per m² *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	SRD	FRD	FRD	SRD
	Amsterdam	*Rotterdam*	*The Hague*	*Amsterdam*	*Rotterdam*	*The Hague*
Δ KW-investment	0.0510**	0.0552***	0.0221	-0.300***	-0.139*	-0.0381
	(0.0245)	(0.0161)	(0.0238)	(0.0650)	(0.0735)	(0.0886)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	4,097	8,976	4,944	23,930	9,810	17,005
Number of clusters	31	98	31	418	114	154
R^2-within			0.286			0.001
Kleibergen-Paap F-statistic	108.4	759.6		18958	763.5	
Bandwidth h	2.917	5.367	5.318	8.322	5.884	10.793

Notes: We exclude observations within 2.5 kilometres of targeted areas. In columns (1), (2), (4) and (5) the change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

main effects are not affected by allowing for spatial spillovers. The results for spatial spillovers are not robust for different specifications. For example, specification (5) suggest that there are substantial spatial spillovers up to 1.5 km, whereas specification (6) indicates that spatial spillovers are small and most likely only within 500 m of the treatment area.

E. City-specific results

We wonder whether the baseline results are driven by a few instances where neighbourhood investments were successful, while in general place-based investments may not yield positive effects. We therefore estimate the baseline specification for the three largest cities in The Netherlands (Amsterdam, Rotterdam and The Hague) in which there were multiple treated areas.[28] We include observations that are within 10 kilometres of each of the respective city centres. The results are reported in Table 10.

For Amsterdam and Rotterdam we find that the average price effects are a bit higher than the baseline specifications. However, we have much fewer observations. This means that the results are not statistically significantly different from the baseline specification.[29] Also the

[28] For smaller cities we have too few observations to obtain reliable estimates.
[29] One may note that the number of clusters is small for Amsterdam and The Hague, so that clustered standard errors may overstate the precision of the estimates. We therefore also run specifications with bootstrapped clustered standard errors, leading to slightly higher, but very similar, standard errors.

effects for sales times (columns (4) and (5), Table 11) are in line with our expectations. For The Hague, there are no neighbourhoods that are below the threshold and are treated or are above the threshold and are not treated, implying that we have a sharp regression-discontinuity design (SRD). For The Hague we do not find a statistically significant price or sales time effect. However, this is mainly due to precision. If we for example increase the bandwidth the price effect becomes statistically significant. However, the sales time effect is to imprecise to draw any conclusions.

In general, the consistent results for the three cities show that it is unlikely that our results are driven by a few positive instances where the treatment is successful, but that this is a more general finding.

F. Unobserved trends

Despite the RDD, one might be worried that our results are driven by either city-specific price trends or by the more general trend that city centres seem to become more attractive. Because many treated neighbourhoods are close to the historic city centre, they may benefit from trends like gentrification that occur in and near the city centre. In columns (1) and (4) of Table 11 we control for the distance to the nearest city centre of a city with at least 50,000 inhabitants. It appears that places closer to the city centre have indeed become more expensive: doubling the distance to the city centre leads to a price decrease of 0.67 percent. Surprisingly, for sales times the effect is also negative, suggesting that sales times closer to the city centre have also slightly increased. The treatment effects, however, are essentially unaffected.

We investigate this issue further by including a set of one kilometre distance band dummies based on the distance to the city centre, implying that we control very flexibly for distance to the city centre. The results reported in columns (2) and (5) of Table 11 are very much in line with the baseline results.

Another concern is that there are municipality-specific trends that play a role in explaining the positive price effects and negative sales time effects, because these trends may be correlated in some way to the treatment. Of course, when our identification strategy is valid, this should not make any difference. Indeed, when we include 455 municipality dummies in addition to city centre distance band dummies, the results are essentially unchanged, strongly suggesting that municipality-specific trends do not play a role in explaining the effect.

However, the coefficient for Amsterdam is still statistically significant at conventional significance levels. The results are available upon request.

TABLE 11 — SENSITIVITY ANALYSIS: CITY-CENTRE SPECIFIC TRENDS

	Panel 1: Δ Price per m² *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Δ KW-investment	0.0360***	0.0348***	0.0376***	-0.137***	-0.130***	-0.116**
	(0.0120)	(0.0116)	(0.0111)	(0.0496)	(0.0489)	(0.0583)
Distance to city centre *(log)*	-0.00967***			-0.0159**		
	(0.00286)			(0.00775)		
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Centre 1km distance bands (67)	No	Yes	Yes	No	Yes	Yes
Municipality fixed effects (455)	No	No	Yes	No	No	Yes
Number of observations	21,275	22,356	15,945	78,576	113,642	33,794
Number of clusters	174	183	136	1345	2274	306
Kleibergen-Paap *F*-statistic	4460	4013	850.5	17266	18075	1904
Bandwidth *h*	3.183	3.344	2.719	7.517	8.772	4.782

Notes: We exclude observations within 2.5 kilometres of targeted areas. The change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

TABLE 12 — SENSITIVITY ANALYSIS: STARTING DATE OF INVESTMENT

	Panel 1: Δ Price per m² *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Δ KW-investment	0.0325***	0.0330***	0.0393***	-0.186***	-0.172***	-0.165***
	(0.0110)	(0.0111)	(0.0123)	(0.0509)	(0.0507)	(0.0571)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	22,589	22,562	15,795	64,150	64,810	89,742
Number of clusters	186	185	155	830	844	2082
Kleibergen-Paap *F*-statistic	3245	2256	2047	9129	9062	14025
Bandwidth *h*	3.382	3.380	3.047	6.949	6.973	8.551

Notes: We exclude observations within 2.5 kilometres of targeted areas in Columns (1), (2), (4) and (5). Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

G. Starting date of programme

The exact starting date of the KW-programme was not very clear. Although the official announcement of the programme was on March 22, 2007, it was not clear when and how much money would be invested in the neighbourhoods. As the starting date of the KW-scheme we therefore use the date at which the secretary of state agreed with large public housing associations that they would invest in the KW-neighbourhoods (September 14, 2007). However, it took a while before the programme was launched in the targeted neighbourhoods. If the starting date is wrongly chosen by us, this may lead to an underestimate of the effects of the investment. In Columns (1) and (4) in Table 12 we take the official announcement as alternative starting date. It is shown that the effect on house prices and sales times is very similar to the specifications reported in Column (4) in Table 3 and Table 4. Columns (2) and (5) take January 1, 2008 as a starting date. The effects are very again very similar. In Columns (3) and (6) we just avoid the problem by excluding transactions that took place in 2007. The price and sales time effects are again very comparable to the baseline estimates. Hence, although the exact starting date of the programme is somewhat unclear, it does not seem to bias our results.

H. RDD set-up

The baseline specifications use local linear estimation techniques, by only selecting neighbourhoods that have z-scores that are close to a threshold, based on a bandwidth. To guide the bandwidth choice h, we have used the procedure as outlined by Imbens and Kalyanaraman (2012). Nevertheless, the results may be sensitive to the choice of bandwidth. If the results are critically dependent on a particular bandwidth choice, they are clearly less credible than if they are robust to such variation. In Table 13 we report results that investigate sensitivity with respect to the bandwidth choice.

In columns (1) and (4) we do not use local linear estimation techniques. Following Van der Klaauw (2002), we also include neighbourhoods away from the threshold and add a nonparametric control function $G(\cdot)$ of the z-score to (3). The idea is that z_ℓ is the only determinant of the treatment status, implying that $G(\cdot)$ will capture any correlation between $\Delta k_{\ell t}$ and $\Delta \epsilon_{\ell t}$. Hence:

(10) $$\Delta y_{\ell t} = \alpha \Delta k_{\ell t} + G(z_\ell) + \beta \Delta x_{\ell t} + \Delta v_t + \Delta \epsilon_{\ell t},$$

As suggested by Trochim (1984) and Lee and Lemieux (2010), we use a conventional power series approximation of $G(z_\ell)$ on both sides of the z-score cut-off, so that:

(11) $$G(z_\ell) = \sum_{\wp=1}^{\mathcal{P}} \gamma_p^+ (z_\ell - c)^{\wp} 1_{z_\ell \geq c} + \sum_{\wp=1}^{\mathcal{P}} \gamma_p^- (z_\ell - c)^{\wp} 1_{z_\ell < c},$$

where $\wp = 3$ and γ_p^+ and γ_p^- are additional parameters to be estimated. Columns (1) and (4) indicate that this procedure leads to very similar results. The price effect is 3.7 percent and the sales time effect is −15.3 percent.

TABLE 13 — SENSITIVITY ANALYSIS: BANDWIDTH SELECTION

	Panel 1: Δ Price per m^2 *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Δ KW-investment	0.0363***	0.0344**	0.0350***	-0.166***	-0.187***	-0.0825*
	(0.00904)	(0.0137)	(0.0110)	(0.0531)	(0.0609)	(0.0483)
$G(z_\ell)$ included	Yes	No	No	Yes	No	No
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	169,664	8,912	61,831	169,664	22,744	169,664
Number of clusters	3100	76	744	3100	192	3100
Kleibergen-Paap F-statistic	8581	594.9	15993	8581	5515	18727
Bandwidth h	∞	1.692	6.767	∞	3.475	13.900

Notes: We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

TABLE 14 — SENSITIVITY ANALYSIS: RDD IN LEVELS

	Panel 1: Price per m^2 *(log)*			*Panel 2:* Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	SRD	FRD	FRD	SRD	FRD	FRD
KW-investment	0.0367	0.0203	0.0360	-0.165***	-0.150***	-0.0809*
	(0.0480)	(0.0539)	(0.0516)	(0.0370)	(0.0374)	(0.0462)
Housing characteristics (16)	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood characteristics (5)	No	No	Yes	No	No	Yes
Land use variables (4)	No	No	Yes	No	No	Yes
Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	21,171	22,156	24,705	180,993	119,614	194,491
Number of clusters	62	66	72	752	403	853
R^2	0.352			0.060		
Kleibergen-Paap F-statistic		60.54	41.96		4111	443
Bandwidth h	1.162	1.158	1.244	6.667	5.330	6.856

Notes: We only include observations after the treatment started. We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

Imbens and Lemieux (2008) advise to investigate the sensitivity of bandwidth choice, irrespective of the manner in which it is chosen. Following common practice we show for bandwidths half and twice the size of the optimal bandwidth (based on column (4) in Table 3 and Table 4). Columns (2) and (3) show that the price effect is essentially unaffected when we vary the bandwidth. The sales time effect is also similar once we select a bandwidth that is half the size of the optimal bandwidth. When we double the bandwidth in column (6) the sales time effect is somewhat lower. However, the effect is not statistically significantly lower compared to the baseline estimate.

I. RDD in levels

One may also apply the regression-discontinuity design to price differences in levels. This cross-sectional set-up requires stronger identifying assumptions because all *time-invariant* unobservable factors must be uncorrelated to the treatment around the cut-off, which is not required given the analysis based on changes. Hence, an approach based on levels is less likely to generate consistent estimates. Moreover, because many (unobservable) factors that influence prices are omitted, the approach using variation in levels may be inefficient. While keeping these limitations in mind, we take such an approach and report results in Table 14.

In column (1) we use the sharp regression-discontinuity approach where we exclude neighbourhoods that are above the threshold and are untreated, and the neighbourhoods that have been treated while they are below the z-score threshold. The point estimate ispositive and almost identical to the baseline estimate. The level approach is indeed much less efficient as indicated by a large standard error, so that the coefficient is not statistically significantly different from zero at conventional levels. The same holds if we employ a fuzzy regression-discontinuity design in column (2) and include neighbourhood characteristics in column (3): although the point estimates are very similar, the confidence intervals are too wide to draw strong conclusions. Hence, the approach based on differences is strongly preferred.

In columns (4)-(6) we investigate the effects on sales times. Both the SRD and FRD seem to confirm that sales time effects are important (columns (4) and (5) respectively) with coefficients that are very similar to the baseline estimates. The observation that those estimates are statistically significant, while the price effects are not, may be explained by the fact that spatial (time-invariant) factors generally explain a much lower proportion of sales times, compared to house prices (i.e. the R-squared is much lower in the sales time regressions). Hence, efficiency issues are less of a problem here. Column (6) shows that, once we control for neighbourhood characteristics the sales time effect is somewhat lower but still statistically significant at the 10 percent level.

TABLE 15 — SENSITIVITY ANALYSIS: FULL SAMPLE

	Panel 1: Price per m^2 *(log)*			*Panel 2:* Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	FRD	FRD	OLS	FRD	FRD
KW-investment	0.0575***	0.0429***	0.0385***	-0.163***	-0.198***	-0.225***
	(0.0112)	(0.0127)	(0.0114)	(0.0376)	(0.0396)	(0.0404)
Housing characteristics (16)	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood characteristics (5)	No	No	Yes	No	No	Yes
Land use variables (4)	No	No	Yes	No	No	Yes
Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
PC6 fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,393,246	140,932	140,921	1,393,246	343,395	336,314
Number of clusters	3671	202	202	3671	583	568
R^2	0.444			0.099		
Kleibergen-Paap F-statistic		4989	2579		16268	7849
Bandwidth h		3.228	3.225		6.164	6.137

Notes: We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

J. Full sample

We have used repeated sales and first-differencing to estimate the effects of interest. However, one may argue that repeated sales are a non-random sample of the full sample of houses. For example, it might be that the most attractive houses are sold less often, because people have fewer incentives to move. We showed that there are hardly structural differences between the full sample and the repeated sales sample (see Table 2 and Table B1 in Appendix B.1). Nevertheless, we re-estimate the regressions using the full sample. Instead of first-differencing we include postcode six-digit (PC6) effects (a PC6 contains on average about 25 properties), essentially removing time-invariant spatial heterogeneity (Van Ommeren and Wentink, 2012). Table 15 reports the results.

In Columns (1) and (4) we regress respectively house price and sales time on whether the neighbourhood is treated and a host of housing control variables (listed in Table B1 in Appendix B.1). The coefficients suggest a positive price effect of the programme of 5.9 percent. Sales times have been reduced with 15.0 percent. In Columns (2) and (4) we employ the fuzzy regression-discontinuity design. The price effect is then somewhat lower (4.4 percent), while the sales time effect is somewhat stronger (−18 percent). In Columns (3) and (6), Table 15, we also control for neighbourhood characteristics and changes in land use. The price effect is again slightly lower but similar (3.4 percent). The investment programme has reduced sales times with 20.1 percent. In general, we may conclude that the results using the full sample are very similar to the baseline results.

VII. Conclusions

In many countries, governments invest in deprived neighbourhoods to reduce income disparities within cities and fight social problems. In Europe, this mainly involves an improvement in the public housing stock. There is limited understanding to what extent these policies are effective and have external positive effects on nearby residents. In the current paper we examine these external effects for households in the owner-occupied market. Using a fairly standard housing search and matching model, we show that place-based investments capitalise into house prices and temporarily reduce sales times, given the assumption that search costs are proportional with a delay to house prices. Given the assumption that the market is in spatial equilibrium, we show that price increases due to the investment are a reasonable measure of welfare improvements despite the presence of search frictions.

We aim to empirically measure the effects of place-based policies on the housing market using a nationwide investment programme that aims to restructure and revitalise public housing in the most deprived neighbourhoods in the Netherlands. A rich repeated sales dataset on house sales in the period 2000-2014 is used. We explicitly take into account that treated neighbourhoods are not randomly chosen by governments. We combine a first-differences approach with a (fuzzy) regression-discontinuity design based on a jump in the probability to be treated, which depends on neighbourhood-specific deprivation scores. We find compelling evidence for the presence of positive external effects of the investment scheme. The programme has led to an increase in house prices of 3.5 percent. Place-based investments has also led to reductions in sales times up to one month (20 percent), but this effect is temporary and disappears within 7.5 years. A counterfactual analysis indicates that the welfare benefits to homeowners induced by the place-based policy programme are sizeable and at least half of the value of the expenditure on public housing.

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Appendix A. Theoretical model

A.1 Model set-up

We assume a neighbourhood with two symmetric types of housing. Each neighbourhood supplies a given number of houses equal to $2\bar{S}$. The housing units are occupied by an (endogenous) number of households equal to $2h$, where $\bar{S} > h$. The number of vacant units in each neighbourhood is denoted by $2v = 2\bar{S} - 2h$. We will first focus on the steady-state, but later analyse the model out of steady-state.

Households have a preference for one housing type. Households change this preference at a rate ϕ (e.g. due to birth of a child or change in marital status). We then distinguish between three household states: matched, mismatched and dual-ownership households, which are denoted by h^M, h^N and h^D respectively. Matched households own one property, occupy their preferred housing type and receive a utility flow of k from living in a certain neighbourhood, where k is the amenity level. Dual-household own two houses of a different type. They occupy their preferred housing type, also enjoying a utility flow of k per unit of time, but they aim to sell the property of the other type, which is vacant. Mismatched households own one property of the non-preferred type. Their mismatched utility flow is less than, but proportional, to the utility flow of being matched and denoted by ψk where $0 < \psi < 1$. These households search for the other housing type incurring search costs c which are an increasing convex function of effort level e. Furthermore, these search costs are proportional to the amenity level k. This assumption aims to capture *long-run* conditions and has a range of justifications, but mainly captures that search costs for households vary over time. For example, real estate agents usually charge fees that are proportional to housing prices. Hence, we define search costs as:

(A1) $$c = k \cdot g(e),$$

where $g(\cdot)$ is a continuous function of search effort and $\partial g(\cdot)/\partial e > 0, \partial^2 g(\cdot)/\partial e^2 > 0$.

We assume the existence of a constant returns-to-scale matching function $\mathcal{M}(eh^N, v)$, with two arguments: the product of the search effort and the number of mismatched households $e \cdot h^N$, and the number of vacancies v.[30] This assumption implies that the rate of a mismatched household to find a house, denoted by m can be written as $m(e, v/h^N)$. The rate of a dual ownership household to sell a property is then inversely related to the expected sales time s which is defined by $s = v/mh^N$. Given the value of m and ϕ, the number of households in each state are determined as follows:

(A2) $$\frac{\partial h_t^M}{\partial t} = -\phi h_t^M + h_t^N \left(\phi + m_t \frac{h_t^D}{v_t}\right),$$

(A3) $$\frac{\partial h_t^D}{\partial t} = m_t h_t^N \left(1 - \frac{h_t^D}{v_t}\right),$$

(A4) $$\frac{\partial h_t^N}{\partial t} = -h_t^N(\phi + m_t) + \phi h_t^M,$$

which provides a stable model of changes in household state as well as of residential moving. In steady state it holds that:

(A5) $$h^M = 2\phi(h - v)/2\phi + m, \qquad h^D = v, \quad \text{and} \qquad h^N = m(h - v)/2\phi + m.$$

The household not only enjoys the amenity but also pays for each house mortgage costs rp, where r is the interest rate and p is the house price. Households take into account that they may change state (e.g. by selling their house or finding a new house). The lifetime utilities – i.e. the present values of utility of each state – matched M, owning two houses D, mismatched N – are then given by the following standard Bellman equations:

(A6) $$rU^M = k - rp + \phi(U^N - U^M)$$

(A7) $$rU^D = k - 2rp + \frac{U^M - U^D + p}{s},$$

(A8) $$rU^N = \psi k - c - rp + \phi(U^M - U^N) + m(U^D - U^N - p).$$

where U denotes the lifetime utility. Here, (A6) states that the discounted lifetime utility of being matched is the sum of the flow utility enjoyed in the housing market minus the interest costs, taking into account that the household may become mismatched. In (A7), we take into account that the dual-ownership household has to pay mortgage cost for two houses and will sell the property for a price equal to p, so the increase in lifetime utility when having a match with a mismatched households is equal to $U^M - U^D + p$. In (A8), we take into account that mismatched households have to pay for a property when becoming matched.

We assume that households maximise utility and that house prices are determined given Nash bargaining, where dual-ownership and mismatched households have equal bargaining

[30] We follow the literature using the phrase ‘matching function’. However, it would be more appropriate to call it a ‘contact function’.

power. Consequently, when a dual-ownership household and a mismatched household have made contact with each other, they will settle on a house price by splitting the surplus of the match ($U^M - U^D + p \ = U^D - U^N - p$).

Given the assumptions on bargaining and equations (A6), (A7) and (A8) and conditional on e (and therefore on c, s, m), the house price is given by $p = \big((1-\psi)k + c\big)(1 + rs + 2s\phi)/\big(2rs(m + 2r + 4\phi)\big)$.[31] Using (A1), this can be rewritten as:

(A9) $$p = k\big((1-\psi) + g(e)\big)\frac{1 + rs + 2s\phi}{2rs(m + 2r + 4\phi)}.$$

Consequently, the partial equilibrium effect of the amenity level on house prices is positive. Note that the factor $m + 2r + 4\phi$ in the denominator is a 'correction factor' which discounts the expected changes of the different household states.

A.2 Endogenous search effort and number of households

Let us now assume endogenous search effort and the number of households in the neighbourhood. We only consider symmetric equilibria where all households choose the same search effort level. Using (A8), the first-order condition for search effort for *individual* household i is given by:

(A10) $$\frac{\partial c(e_i)}{\partial e_i} = \frac{\partial m(e_i, v/h^N)}{\partial e_i}(U^D - U^N - p).$$

Consequently, our interest is then in the marginal effect of search effort of a single mismatched household i on its matching rate, conditional on search behaviour of other mismatched households. The *individual* matching rate of a mismatched household preferring a certain housing type is the product of individual search effort and the average number of matches (the number of matches divided $\mathcal{M}$ by aggregate search effort, $h^N e_i$) in the point where $e_i = e$. Then:

(A11) $$\frac{\partial m(e_i, v/h^N)}{\partial e_i} = \frac{M}{e_i h^M} = \frac{m}{e} > \frac{\partial m}{\partial e},$$

where the latter inequality follows, because m is a concave function. Consequently, the marginal effect of search effort of a single mismatched household on its own matching rate exceeds the marginal effect of search effort of all mismatched households on the matching rate. Using equations (A1), (A6), (A7), (A8) and (A11), it can be shown that (A10) simplifies to:

(A12) $$\frac{\partial g(e_i)}{\partial e_i} = \frac{m}{e}\left(\frac{(1-\psi) + g(\cdot)}{m + 2r + 4\phi}\right).$$

Equation (A12) implies that the chosen search effort level is *not* a direct function of the amenity level k. This is intuitive because the marginal benefits and costs of search are both proportional to the amenity level. Given e, it follows that c, s, m, h^M, h^N, h^D and v are determined.

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[31] See similarly Wheaton (1990).

The number of households $2h$ in the neighbourhood will be determined by making a standard spatial equilibrium assumption. Hence, we assume that households move into this neighbourhood until the (expected) utility in this neighbourhood is equal to a reference utility which is standardised to zero. It is assumed that households who consider moving into the neighbourhood do not know in which state they will enter the neighbourhood, but only know the probabilities associated with each state. For example, the probability of being mismatched will be equal to h^N/h. In equilibrium, the following condition must then hold:

(A13) $$\frac{h- h^N}{h}k + \frac{h^N}{h}(\psi k - c) - rp\left(1 + \frac{v}{\bar{\bar{S}}}\right) = 0,$$

where the first two terms on the left-hand side capture the household expected utility flow excluding mortgage costs, whereas the last term captures these costs. Given (A1), this equation can be rewritten as:

(A14) $$\frac{h- h^N}{h} + \frac{h^N}{h}(\psi - g(e)) - \frac{rp}{k}\left(1 + \frac{\bar{\bar{S}} - h}{\bar{\bar{S}}}\right) = 0,$$

which implies that the number of households h does not depend on k, because prices p are proportional to the amenity level k (see (A9)).[32] This result is intuitive: given increases in amenity levels, the increase in the utility flow is fully offset by the increase in house prices. The two other endogenous variables (h^N and e) also do not depend on k.

For the analysis of welfare, discussed in the next subsection, it turns out to be useful to rewrite the above equation as follows:

(A15) $$p = k\left(\frac{1 - \big((1-\psi) + g(e)\big)}{r}\frac{h^N}{h}\right)\left(\frac{\bar{\bar{S}}}{\bar{\bar{S}} + v}\right).$$

A.3 Comparative statics and welfare effects of place-based investments

We are interested in the comparative statics of changes in amenity levels induced by place-based investments. In particular, will place-based investments always have a positive effect on house prices given search imperfections? What will be the effect of place-based investments on sales times? To what extent are place-based investment induced changes in prices indicative of changes in welfare? Usually, it is cumbersome to answer these questions in this type of models, because search effort and the number of households change endogenously, which induces changes not only in c, s, m, h^M, h^N, h^D, but also in h. In the current setup, given the long-run assumptions, the comparative statics as well as the welfare analyses are, however, straightforward, because search effort and sales time do not change. Recall that we focus on the long-run steady-state, because (A1) is essentially a long-run condition. Consequently, (A9) immediately implies that prices p are an increasing function of

[32] We find circumstantial evidence for this statement, as we do not find evidence in Appendix B.5 that population density is affected by the KW-programme.

amenity level k, while according to (A12), sales times do not change.[33] Hence, the model leads to two testable empirical predictions *for the long run*:

(i) the price is positively influenced by amenity-increasing place-based investments;

(ii) the expected sales time will not be affected by these place-based investments.

In the absence of search frictions, standard hedonic theory indicates that increases in house prices due to marginal place-based investments are an accurate measure of welfare increases. To calculate the welfare effects of place-based investments taking into account search frictions is not standard. We will focus on the long-run steady-state welfare changes of these investments.[34]

It is important to distinguish between search levels chosen by the household that are privately optimal, and those that are optimal from a welfare perspective. Because the individual household does not take into account its impact on other households, but the matching function depends on the search behaviour of all individuals, there is usually a difference between privately-optimal and welfare-optimal search levels. We emphasise that we analyse welfare effects given the less restrictive assumption of privately-optimal search effort levels. We define welfare $\mathcal{w}$ per household as:

$$\text{(A16)} \qquad \mathcal{w} = \frac{k - \left((1-\psi)k + c\right)\frac{h^N}{h}}{r}.$$

In the long run, given (A1) and the result that search effort does not change, c is proportional to k. Consequently, (A15) and (A16) imply:

$$\text{(A17)} \qquad p = \mathcal{w}\left(\frac{\bar{S}}{\bar{S}+v}\right) = \mathcal{w}\left(\frac{\bar{S}}{2\bar{S}-h}\right).$$

Because (A14) implies that the numbers of households h does not depend on k, it holds that:

$$\text{(A18)} \qquad \frac{\mathrm{d}\log p}{\mathrm{d}k} = \frac{\mathrm{d}\log \mathcal{w}}{\mathrm{d}k} \quad \text{and} \quad \frac{\mathrm{d}p}{\mathrm{d}k} < \frac{\mathrm{d}\mathcal{w}}{\mathrm{d}k}.$$

Hence, *percentage* price changes are an exact measure of *percentage* welfare changes in the long run. This result is intuitive because search effort, and therefore search frictions, do not change in the long-run given place-based investments. The effect of search frictions is therefore a proportionality constant given changes in k.

Furthermore, in levels, price changes are always smaller than welfare changes. To be precise, the underestimate of the price changes as a proxy for welfare changes is proportional to the vacancy rate. So when the observed vacancy rate is small – which will be the case in the market we analyse – changes in welfare are essentially identical to changes in prices.

[33] Note that the latter result holds given the long-run assumption of a spatial equilibrium, but also holds when the number of households is exogenously given.

[34] Welfare calculations for the short run are less useful, because these investments have a long time span.

A.4 Comparative statistics out of steady state

We will now examine the effect of unannounced place-based investments on prices and sales time allowing for out-of-steady-state effects, so we allow for short-run effects. First note that in the above model, which is formulated given long-run assumptions (such as (A1)), search effort does not change when place-based investments occur. The implication is that there are no out of the steady-state effects, and prices will immediately jump to the new value. However, there are many reasons to believe that in the short run the stated conditions about job search differ from those analysed above. For example, let us explicitly introduce time, denoted by t, and let us suppose search costs are proportional to amenity levels with a delay equal to τ. One interpretation is that search costs have a fixed component (e.g. time costs), which only change slowly over time:

(A19) $$c_t = k_{t-\tau} \cdot g(e_t),$$

Now suppose that in t an unannounced investment is implemented so that $k_t > k_{t-1}$. Given the investment, the market will then need time to adjust to a new steady state. Search effort, matching rates and sales times will then initially deviate from the long-run steady state. The Bellman differential equations then look as follows (see Van den Berg, 1990):

(A20) $$rU_t^M = k_t - rp_t + \frac{\mathrm{d}U_t^M}{\mathrm{d}t} + \phi(U_t^N - U_t^M)$$

(A21) $$rU_t^D = k_t - 2rp_t + \frac{\mathrm{d}U_t^D}{\mathrm{d}t} + \frac{U_t^M - U_t^D + p_t}{s_t},$$

(A22) $$rU_t^N = \psi k_t - c_t - rp_t + \frac{\mathrm{d}U_t^N}{\mathrm{d}t} + \phi(U_t^M - U_t^N) + m_t(U_t^D - U_t^N - p_t),$$

where we now allow the present values of utility to change over time. We solve the system of equations (A20), (A21) and (A22), taking into account future changes in present values of utility. The price of a property is then given by:

(A23) $$\begin{aligned} p_t &= (k_t(1-\psi) + k_{t-\tau}g(e_t))\frac{1 + rs_t + 2s_t\phi}{2rs_t(m_t + 2r + 4\phi)} \\ &+ \frac{\frac{\mathrm{d}U_t^M}{\mathrm{d}t}\big(1 - s_t(m_t + r + 2\phi)\big) + \frac{\mathrm{d}U_t^D}{\mathrm{d}t}\big(s_t(m_t + 2r + 4\phi)\big) - \frac{\mathrm{d}U_t^N}{\mathrm{d}t}\big(1 + s_t(r + 2\phi)\big)}{2rs_t(m_t + 2r + 4\phi)}. \end{aligned}$$

The first part of this equation is similar to (A9). The second part is representing future changes in the present values of each state. The first-order condition for optimal search is given by:

(A24) $$\begin{aligned} \frac{\partial c(e_{it})}{\partial e_{it}} &= \frac{\partial m(e_{it}, v_t/h_t^N)}{\partial e_{it}}(U_t^D - U_t^N - p_t). \\ &= \frac{m_t}{e_t}\left(\frac{(1-\psi)k_t + k_{t-\tau}g(e_t) + \left(\frac{\mathrm{d}U_t^M}{\mathrm{d}t} - \frac{\mathrm{d}U_t^N}{\mathrm{d}t}\right)}{m_t + 2r + 4\phi}\right). \end{aligned}$$

Hence, search effort depends on the present flow utility (of the amenity plus search costs) as well as the time change in the lifetime utility.

Now suppose that at $t = 0$, k_t unexpectedly increases as the result of a place-based policy. For convenience, suppose that τ is infinitely small. The policy then induces an increase in the marginal benefit of search (the marginal cost remains constant, ceteris paribus). Consequently, search effort levels – and therefore the mismatched households' matching rate – *jump up* inducing a fall in the number of mismatched households causing sales time to fall discretely.[35] At time τ, marginal search cost increase due to the policy and hence search effort is reduced in the direction of its steady-state level. At τ, the matching rate (which depends negatively on the number of mismatched households) and therefore the marginal benefit of search as well as the search effort level is higher than at $t = 0$. Consequently, search effort level and therefore sales time *slowly* return to their steady-state level.

We first solve the model numerically for the steady state before and after the policy to determine the long-run effects of changes in the amenity level. We assume values for the exogenous parameters k_t, ϕ, ψ, r, $\bar{S}$ and h. We further assume:

(A25) $$c_t = k_{t-1}e_t^2/2 \quad \text{and} \quad m_t = \sqrt{e_t\, v_t/h_t^N}.$$

To solve the model, we first pick a starting value for m_t and calculate the starting values for h_t^M, h_t^D and h_t^N. Then we determine the present values for each state and calculate the optimal level of search effort using equation (A12). We then update m_t, c_t, s_t, h_t^M, h_t^D, h_t^N and the present values. We iterate this procedure until search effort e_t converges.

We also determine the short-run effect of changes in the amenity level. Because optimal search effort, and therefore the house price, depend on future lifetime utility values of being in each state we first calculate initial values using the steady state values for $t < 0$ and $t \geq 0$. We then use these values and equations (A1), (A3) and (A4) to determine to determine m_t, c_t, s_t, h_t^M, h_t^D and h_t^N in each period. We repeat this whole process for all time periods and update h_t^M, h_t^D and h_t^N in each iteration until e_t converges.

Figure A1 shows the results for an unanticipated 25 percent increase in the amenity level. The long-run price increase is then exactly 25 percent. In the short run, prices jump almost immediately to the new steady state value after the amenity increase. Sales time immediately drops after the amenity increase with 5.5 percent and slowly adjusts to its former steady-state value. These results indicate that welfare implications allowing for out-of-steady-state search effort levels will hardly differ from the steady-state results derived above, because search levels only differ from their steady-state levels for a short period.

[35] Note that the change in the number of mismatched households is given by $\partial h_t^M/\partial t = -\phi h_t^M + h_t^N(\phi + m_t h_t^D/v_t)$.

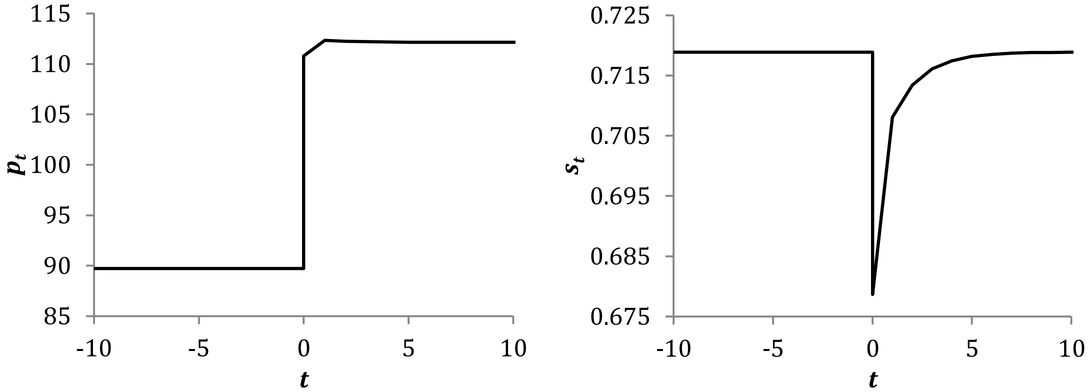


FIGURE A1 — PRICES AND SALES TIMES IN THE SHORT RUN

Notes: We assume $k_t = 100$ for $t < 0$, $k_t = 125$ for $t \geq 0$, $\tau = 1$, $\phi = 0.2$, $\psi = 0.5$, $r = 0.05$, $\bar{S} = 2000$, $h_t = 1900$, $c_t = k_{t-1}e_t^2/2$, and $m_t = \sqrt{e_t\, v_t / h_t^N}$.

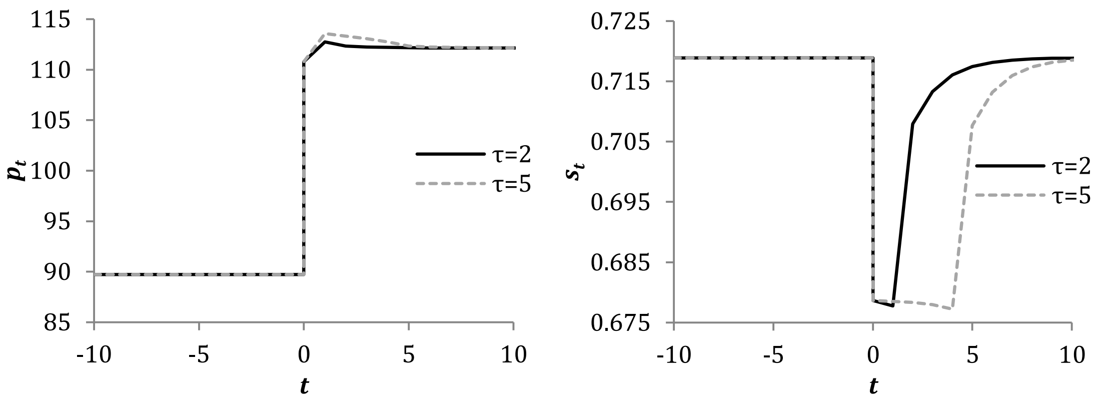


FIGURE A2 — PRICES AND SALES TIMES IN THE SHORT-RUN WITH $\tau = 2$ AND $\tau = 5$

Notes: We assume $k_t = 100$ for $t < 0$, $k_t = 125$ for $t \geq 0$, $\phi = 0.2$, $\psi = 0.5$, $r = 0.05$, $\bar{S} = 2000$, $h_t = 1900$, $c_t = k_{t-1}e_t^2/2$, and $m_t = \sqrt{e_t\, v_t / h_t^N}$.

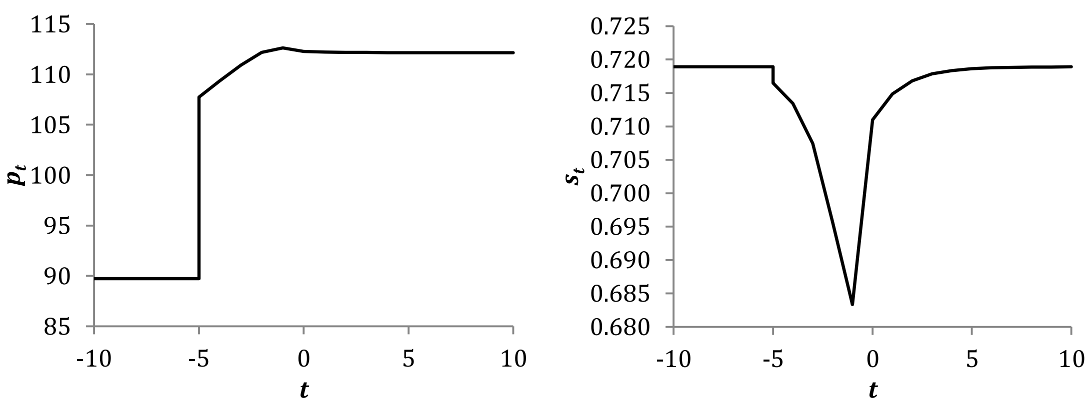


FIGURE A3 — PRICES AND SALES TIMES IN THE SHORT-RUN WITH ANTICIPATION EFFECTS

Notes: We assume $k_t = 100$ for $t < 0$, $\tau = 1$, $k_t = 125$ for $t \geq 0$, $\phi = 0.2$, $\psi = 0.5$, $r = 0.05$, $\bar{S} = 2000$, $h_t = 1900$, $c_t = k_{t-1}e_t^2/2$, and $m_t = \sqrt{e_t\, v_t / h_t^N}$.

Hence, these numerical results yield two additional testable empirical predictions given an increase in the amenity level:

(iii) prices adjust quickly to the new steady-state value;

(iv) sales time drop in the short run, while this effect disappears in the long run.

We do some sensitivity checks with respect to these two predictions. We assume that the time it takes for the search costs to become proportional to the amenity level is one year ($\tau = 1$). However, in practice it may take longer. Figure A2 shows that there is some overshooting of prices in $t + 1$ when it takes longer for the search costs to become proportional to the amenity level again. Sales times are lower as long as $k_{t-\tau} < k_t$ and adjust back to the steady state values once $k_{t-\tau} = k_t$.

It may be the case that place-based investments are announced before the investments actually take place. Prices and sales times then adjust before the actual investments take place. In Figure A3 we show the results. Prices jump once the announcement is made (5 periods before the actual treatment takes place). The immediate drop in sales time is small, and then sales times decrease until $t = 0$. After that, sales time return to the steady-state value.

Appendix B. Econometrics appendix

B.1 Other descriptive statistics

Table B1 reports the descriptive statistics for the full sample. The descriptives of the full sample seem to suggest that houses inside KW-neighbourhoods are somewhat more expensive than properties located outside the treated areas. Again, this is mainly because the targeted areas are disproportionally located in larger cities. The selling time of properties in the full sample is somewhat higher (about 20 percent) than properties in the repeated sales sample. Another difference between the full sample and repeated sales sample is that houses tend to be somewhat smaller and more often apartments in the latter sample. This is, most likely, because housing mobility in cities tends to be higher. Houses in cities are also smaller and the share of apartments is higher.

Table B2 reports the means and standard deviations at the neighbourhood level for the KW-neighbourhoods and three different sets of control neighbourhoods using the propensity score matching method (see Section VI.C). It appears that the control neighbourhoods are relatively similar to the KW-neighbourhoods in most neighbourhood attributes. There are two notable differences between the targeted and control neighbourhoods. The first is that population density is about a third lower in the control neighbourhoods. Indeed, targeted areas are on average located in larger cities. Furthermore, the share of foreigners is much lower in control neighbourhoods. We note that the propensity scores of neighbourhoods that are neither targeted nor control neighbourhoods are very close to zero, suggesting that the method performs reasonably well.

TABLE B1 — DESCRIPTIVE STATISTICS FOR FULL SAMPLE

	Observations outside KW-neighbourhoods				Observations inside KW-neighbourhoods			
	μ	σ	min	max	μ	σ	min	max
House price per m² *(in €)*	1,958	672.2	500	5,000	1,912	673.9	501.0	5,000
Days on the market	153.9	191.9	1	1,826	133.7	165.6	1	1,816
KW-investment received	0				0.505			
Deprivation z-score	0.178	2.803	-6.600	10.60	8.733	1.186	5	12.98
Size in m²	117.0	37.70	26	250	88.36	31.13	26	250
House type – apartment	0.284				0.750			
House type – terraced	0.320				0.177			
House type – semi-detached	0.275				0.0667			
House type – detached	0.120				0.00638			
Garage	0.316				0.0845			
Garden	0.973				0.978			
Maintenance quality –high	0.867				0.832			
Central heating	0.911				0.852			
Listed	0.00603				0.00471			
Construction year <1945	0.236				0.352			
Construction year 1945-1960	0.0710				0.145			
Construction year 1961-1970	0.147				0.227			
Construction year 1971-1980	0.165				0.0373			
Construction year 1981-1990	0.140				0.0530			
Construction year 1991-2000	0.153				0.0873			
Construction year >2000	0.0865				0.0983			

Notes: The number of observations outside KW-neighbourhoods is 1,728,004 and inside KW-neighbourhoods 68,538.

TABLE B2 — DESCRIPTIVE STATISTICS FOR PROPENSITY SCORE MATCHING

	KW-neighbourhoods		Control neighbourhoods					
			Calipher matching, $\Delta\ score < 0.01$		Nearest neighbour matching without replacement		Nearest neighbour matching with replacement	
	μ	σ	μ	σ	μ	σ	μ	σ
Population density *(ha²)*	9,081	5,171	5,601	4,352	5,965	4,233	6,804	4,476
Income	10,965	1,050	11,866	1,166	11,634	1,188	11,670	1,263
Median construction year	1,950	24	1,947	90	1,957	21	1,953	22
Share owner-occupied housing	0.459	0.180	0.249	0.155	0.316	0.171	0.345	0.194
Share foreigner	0.333	0.044	0.300	0.051	0.308	0.054	0.304	0.048
Share young	0.123	0.050	0.150	0.067	0.153	0.076	0.158	0.080
Share elderly	0.170	0.158	0.256	0.226	0.226	0.202	0.191	0.171
Share open space	0.224	0.038	0.202	0.051	0.209	0.051	0.215	0.047
Share social allowance	0.367	0.059	0.319	0.064	0.335	0.069	0.342	0.056
Share unemployed	0.471	0.047	0.451	0.048	0.455	0.050	0.452	0.047
Share low income	0.225	0.092	0.318	0.112	0.261	0.092	0.244	0.088
Share houses constructed <1945	0.326	0.317	0.289	0.274	0.255	0.264	0.304	0.256
Share houses constructed 1945-1970	0.354	0.304	0.400	0.284	0.425	0.295	0.377	0.303
Propensity score	0.622	0.337	0.187	0.243	0.349	0.247	0.399	0.281
Number of neighbourhoods	83		116		83		38	

Note: The analysis is done at the neighbourhood level. The number of observations is 4,011.

B.2 Determining the bandwidth

We use the approach proposed by Imbens and Kalyanaraman (2012), who show that the optimal bandwidth can be estimated as:

$$\text{(B1)} \qquad h^* = C_K \times N^{-\frac{1}{5}} \times \left(\frac{\hat{\sigma}_-^2(c) + \hat{\sigma}_+^2(c)}{\hat{f}(c) \cdot \left(\left(\hat{m}_+^{(2)}(c) - \hat{m}_-^{(2)}(c) \right)^2 + (\hat{r}_- + \hat{r}_+) \right)} \right)^{\frac{1}{5}},$$

where the constant $C_K = 5.4$ and N is the number of observations. $\hat{\sigma}_-^2$ and $\hat{\sigma}_+^2$ are the conditional variances of $\Delta y_{\ell t}$ given $z_\ell = c$ on both sides of the threshold (indicated with '–' and '+'), $\hat{f}(c)$ denotes the estimated density of z_ℓ at c. $\hat{m}_-^{(2)}$ and $\hat{m}_+^{(2)}$ are estimates of the second derivatives of a function of the z-score. $\hat{r}_-$ and $\hat{r}_+$ are estimated regularisation terms that correct for potential error in the estimation of the curvature of $m(z)$ on both sides of the threshold.

Because we employ a FRD, the formula to determine the optimal bandwidth is somewhat modified (see Imbens and Kalyanaraman, 2012):

$$\text{(B2)} \qquad h^* = C_K \times N^{-\frac{1}{5}} \times \left(\frac{\left(\hat{\sigma}_{Y,-}^2(c) + \hat{\sigma}_{Y,+}^2(c)\right) + \hat{\alpha}^2\left(\hat{\sigma}_{T,-}^2(c) + \hat{\sigma}_{T,+}^2(c)\right) - 2\hat{\alpha}\left(\hat{\sigma}_{YT,-}^2(c) + \hat{\sigma}_{YT,+}^2(c)\right)}{\hat{f}(c) \cdot \left(\left(\left(\hat{m}_{Y,+}^{(2)}(c) - \hat{m}_{Y,-}^{(2)}(c) \right) - \hat{\alpha}\left(\hat{m}_{T,+}^{(2)}(c) - \hat{m}_{T,-}^{(2)}(c) \right) \right)^2 + \left(\hat{r}_{Y,-} + \hat{r}_{Y,+}\right) + \hat{\alpha}\left(\hat{r}_{T,-} + \hat{r}_{T,+}\right) \right)} \right)^{\frac{1}{5}},$$

where $Y = \Delta y_{\ell t}$ and $T = \Delta k_{\ell t}$. $\hat{\sigma}_{YT,-}^2$ and $\hat{\sigma}_{YT,+}^2$ denote the conditional covariance of the treatment and dependent variable at $z_\ell = c$ on both sides of the threshold. We note that, as in previous applications, equation (B1) leads to very similar bandwidths as (B2).

B.3 Additional graphical analyses

In Figure B1 we show the McCrary test around the threshold, assuming a bandwidth of 3.5, which is close to the bandwidth estimated in the empirical analyses. It is shown that the density of the z-score is continuous at the threshold.

Figure B2 explores house price and selling time differences in levels around the threshold before and after the treatment. It is shown in Figure B2A that house prices seem to be a bit lower before the treatment when $z > 7.3$, which may suggest that a RDD in levels may not be valid. However, if we use higher-order polynomials the effect completely disappears. After treatment (Figure B2B), we do not detect a significant price difference in levels anymore. Figure B2C shows that time on the market is statistically significantly higher in scores with $z > 7.3$ before treatment. Again, when we use higher order polynomials this effect would disappear. Also, after the treatment we do not detect a statistically significant difference in

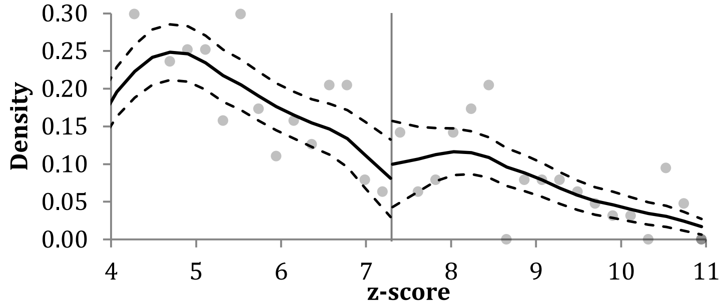
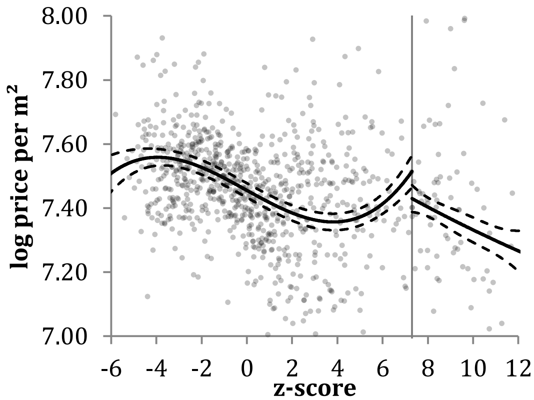
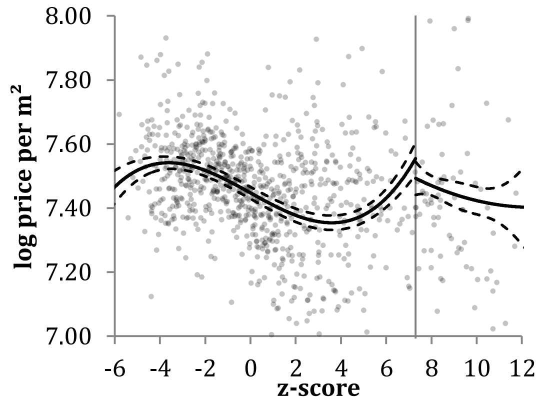


FIGURE B1 — MANIPULATION TEST FOR Z-SCORES WITH $h = 3.5$

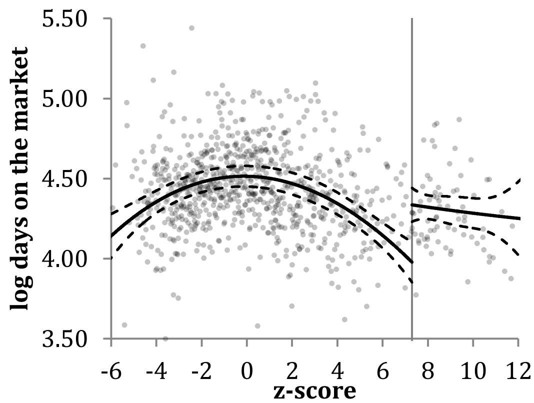
Notes: We estimate the test developed by McCrary (2008) to investigate whether the running variable (the z-score) is continuous around the threshold. We focus on observations $z - h < z < z + h$, with $h = 3.5$. The dashed lines represent 95 percent confidence intervals.



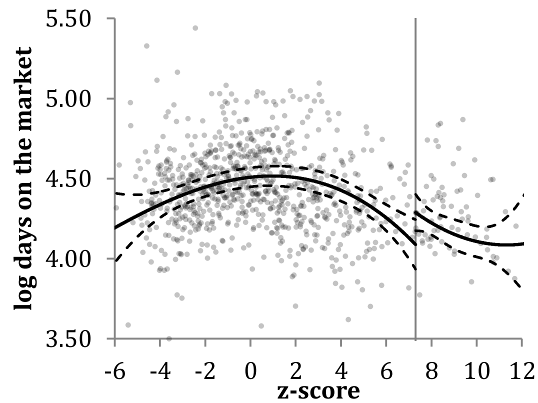
(A) HOUSE PRICES BEFORE TREATMENT



(B) HOUSE PRICES AFTER TREATMENT



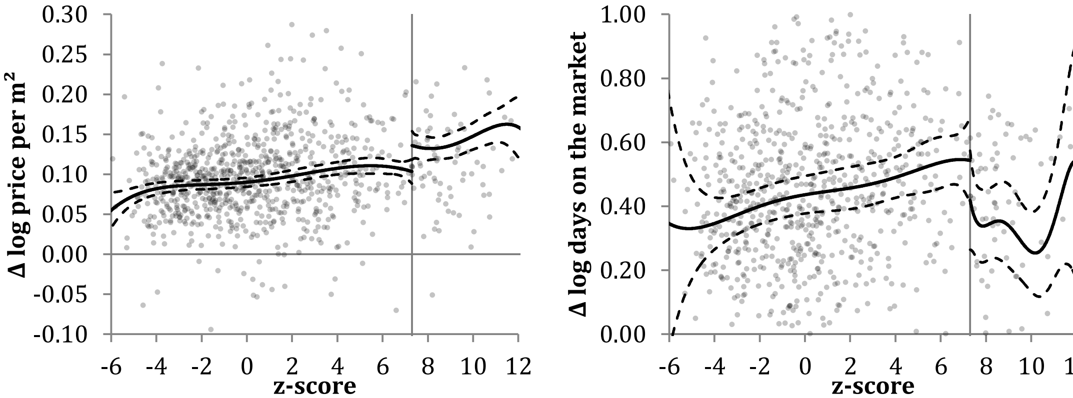
(C) DAYS ON THE MARKET BEFORE TREATMENT



(D) DAYS ON THE MARKET AFTER TREATMENT

FIGURE B2 — HOUSE PRICES AND SELLING TIME AROUND THE THRESHOLD

Notes: We estimate weighted regressions of either log prices or log days on the market on year fixed effects, a third-order polynomials of the z-score on the left side of the threshold and a second-order polynomial on the right side of the threshold, as well as a dummy indicating the change in treatment status. The weights are equal to the inverse of the number of observations in a neighbourhood. Each dot represent the conditional average for a given z-score.



(A) HOUSE PRICES (B) DAYS ON THE MARKET

FIGURE B3 — CHANGE IN HOUSE PRICES AND SALES TIME AROUND THE THRESHOLD
Notes: We estimate weighted regressions of the change in either log prices or log days on the market on year fixed effects, fifth-order polynomials on both sides of the threshold, as well as a dummy indicating the change in treatment status. The weights are equal to the inverse of the number of observations in a neighbourhood. Each dot represent the conditional average for a given z-score.

time on the market (Figure B2D). All in all, because it may be hard to completely control for differences in time-invariant characteristics in an RDD in levels, we prefer to analyse *changes* in house prices and time on the market.

Figure B3 analyses those changes when controlling for fifth-order polynomials of the z-score on both sides of the boundary. It is shown that the results are essentially the same to the ones reported before.

B.4 First-stage regression results

Table B3 report the first-stage regression results, where we regress the change in the KW-investment status on the change in the scoring rule (the scoring rule is zero before the programme was launched). The coefficient related to the scoring rule is close to one, but significantly lower than one (at the five percent level) in all specifications. When the coefficient would be equal to one, the specifications would be identical to a sharp regression-discontinuity design.

B.5 Changes in demographics

In Table B4 we analyse the impact of the KW-investments on the demographic composition of KW-neighbourhoods. We analyse changes in population density, share of foreign population, share young people (<25 years), share elderly people (>65 years) and the average household size. We demean all those variables by neighbourhood averages. The effects are then estimated using the fuzzy-regression discontinuity design.

TABLE B3 — FIRST STAGE REGRESSION RESULTS
(Dependent variable: change in KW-investments)

	Panel 1: Δ Price per m^2 *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Δ Score rule ($z > 7.30$)	0.979***	0.982***	0.970***	0.989***	0.988***	0.985***
	(0.0133)	(0.0109)	(0.0191)	(0.00776)	(0.00811)	(0.0100)
Control variables included (14)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	22,589	12,766	10,484	64,324	22,447	36,905
Number of clusters	186	250	195	838	498	1242
First-stage R^2-within	0.957	0.956	0.951	0.965	0.963	0.966
Kleibergen-Paap F-statistic	5444	8063	2571	16228	14819	9660
Bandwidth h^*	3.383	4.312	3.547	6.950	6.147	7.645

Notes: Standard errors are clustered at the neighbourhood level.
*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level

In column (1) we show that the KW-policy did not imply statistically significant changes in population density. However, in column (2) we observe a statistically significant increase in the share of foreigners. It seems that the KW-programme has led to an increase in the share of foreigners of 2.2 percentage points (about one-fifth a standard deviation). This may be a direct result of the improvement in the quality of housing, which may disproportionally attract foreigners with on average lower incomes. It may also be that foreigners buy properties that are not directly influenced by the investments, because they may have a stronger preference for the amenities generated by the programme. Although the share of foreigners has a direct and negative impact on house prices, controlling for the share of foreigners in the neighbourhoods leaves the effects on prices and time on the market essentially unaffected.

In column (3) we look at the change in the share of young population, for which we cannot detect a statistically significant effect. However, there seems to be a negative effect on the share of elderly people. Also, it seems that the KW-policy has induced an increase in the average household size. It seems that households are 0.036 persons larger than before (about one-tenth a standard deviation).

TABLE B4 — REGRESSION RESULTS: CHANGES IN DEMOGRAPHICS

	(1)	(2)	(3)	(4)	(5)
	FRD	FRD	FRD	FRD	FRD
	Population density (log)	*Share foreigners*	*Share young people*	*Share elderly people*	*Household size*
KW-investment	-0.00406	0.0211***	-0.00442	-0.0111***	0.0363***
	(0.0103)	(0.00610)	(0.00280)	(0.00365)	(0.00921)
Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes
Neighbourhood fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	3766	3542	2240	2240	3920
Number of clusters	269	253	160	160	280
Kleibergen-Paap F-statistic	715	706	509	509	737
Bandwidth h^*	3.897	3.657	2.666	2.684	4.043

Notes: All dependent variables are demeaned. We exclude neighbourhoods adjacent to targeted areas. In Columns (4)-(6) the demeaned KW-investment is instrumented with the demeaned eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level and in parentheses.

*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level