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Place-based policies and the housing market*

By HANS R.A. KOSTER[a] *and* JOS VAN OMMEREN[b]

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SUMMARY — We study the economic effects of place-based policies in the housing market, by investigating the effects of a place-based programme on prices of surrounding owner-occupied properties. The programme improved the quality of 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. Improvements in public housing induced surrounding housing prices to increase by 3.5 percent. The programme's external benefits are sizeable and at least half of the value of investments in public housing.

JEL-code — R30, R33
Keywords — house prices; place-based policies; public housing; housing spillovers

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. In Europe, place-based policies often improve the quality of the public housing stock through new home construction

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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.

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 (Santiago et al. 2001; 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 De Souza Briggs (1999), Lee et al. (1999), and Ahlfeldt et al. (2016), find no statistically significant, or even small negative, effects of place-based policies that subsidise housing.

While programmes to upgrade public housing are common in many cities (e.g. in Australia, France, Spain, United Kingdom, United States), settings where it is feasible to credibly identify spillover effects from these large-scale housing investments are uncommon. Typically, these 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 sites receive subsidies – the estimates of the benefits of the programmes may not be causal.

We evaluate effects of an unusually large, nationwide urban revitalisation programme in the Netherlands, starting in 2007, which aimed to improve the quality of public housing. We aim to measure external effects, by focusing on changes in prices of owner-occupied housing units, which were *not* improved by the programme. In this so-called *krachtwijken*-programme (henceforth: KW-investment scheme), 83 neighbourhoods were selected for revitalisation with funding from the national government.[2] The government and (not for profit) public housing associations announced to invest about € 2.75 billion in these neighbourhoods, on average about € 7 thousand per household in receiving neighbourhoods, but eventually 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 the end, almost all of the money (90 percent) was spent on improving the quality of the public housing stock. The remainder was spent on green spaces and social empowerment programs (Wittebrood

[1] In Europe, public housing is common and covers 47 percent of the rental market (Van Ommeren and Van der Vlist, 2016).
[2] The scheme was also known as *aandachtswijken*-scheme or *Vogelaarwijken*-scheme.

and Permentier, 2011). We utilise a nationwide dataset with information on thousands of privately-owned repeated-sales observations from 2000 to 2014.[3]

The main contribution of this paper is to the identification of causal effects of place-based policies on property values. We take into account that neighbourhoods targeted by place-based policies are not randomly chosen, but are *explicitly* chosen because of undesirable characteristics. We combine a first-differences estimation strategy with a regression-discontinuity design by using information on an eligibility criterion to receive investments. Hence, we compare the change in housing prices close to the z-score threshold. This criterion is dependent on deprivation scores, calculated by the national government. However, there are fourteen non-complying neighbourhoods that had too low scores but were selected or had sufficiently high scores but did not receive treatment in the end. We therefore use a fuzzy regression-discontinuity design (FRD), for which it is necessary to observe a substantial jump in the probability to be treated. Indeed, at the neighbourhood level, we observe a more than 90 percent increase in the probability to become treated when the deprivation score exceeds a certain threshold. Moreover, we show that there is no bunching at the threshold confirming that z-scores could not be influenced by local governments.

We will generalise the results into two directions. First, we make a distinction between the short-run and long-run effects of place-based policies. This distinction is relevant, because one is mainly interested to what extent beneficial place-based policies increase house prices in the long run. These policies are thought to reduce sales times *temporarily* – i.e. in the short run – but do not affect sales time in the long run, while prices should adjust almost immediately when new information becomes available.[4] Estimates of the temporal policy effects on sales time and prices are then indicative how much time it takes before the market returns to a long-run steady state and can be used as an internal consistency test: if one does not find a temporary effect of place-based policies on sales times, while finding a permanent effect on prices, then this will put doubt on the causality of an effect on prices.[5]

Second, we pay attention to treatment heterogeneity, by investigating whether treatment is more effective in more deprived neighbourhoods and by including interactions of

[3] About 90 percent of the Dutch rental housing stock is rent-controlled and about 60 percent of the housing stock is owner-occupied. We do not expect to detect any effect on the controlled public housing rent, but there may be effects on rents of private rental housing. Because of lower data quality for private and public housing rents, we examine this in the sensitivity analysis (see Appendix C.4).

[4] See Appendix A for a formal derivation of these results based on a stylised model extending the search and matching framework by Wheaton (1990). This model combines a standard spatial equilibrium framework with buyer search costs that are in the long run proportional to house prices.

[5] One conceptual difficulty is that our observations of sales times imply the presence of housing vacancies, so changes in house prices do not necessarily reflect changes in welfare. We demonstrate in Appendix A that given the assumption of a spatial equilibrium, one may ignore this issue because changes in long-run prices induced by place-based policies are slight underestimates of welfare changes when the housing vacancy rate is low, which is usually the case.

neighbourhood demographics, such as population density and neighbourhood income, with the treatment effect.[6]

We find that due to place-based investments that improve the quality of public housing, house prices increased by at least 3.5 percent. Sales times are reduced temporarily and bounce back to their initial levels in about 7.5 years. We also find that the effect is much stronger in dense areas. For example, when population density doubles, the treatment effect is 4.3 percentage points higher. This is likely explained by spillovers that are more pronounced when properties are closer to each other. We further find that neighbourhoods that have received more funding per square metre also have experienced stronger price increases. For example, the treatment effect for the average spending is 2.1 percent, while it can be easily triple that for neighbourhoods for which the treatment intensity is higher. Eventually, we calculate that the welfare benefits to property owners induced by the programme are at least half of the value of the investments in public housing. Moreover, we show that renters in public housing have not faced a rent increase, but have seen an increase in neighbourhood quality. Hence, they are definitely better off, in particular because they may also have benefited from direct improvements in their properties. In other words, the programme seems to have been effective in increasing the utility of poorer households. An extensive sensitivity analysis confirms these results.

The remainder of the paper is organised as follows. In Section II we discuss the features of the KW-investment scheme and the selection of the neighbourhoods. Section III elaborates on the econometric framework, the data, and some graphical descriptive evidence. Section IV turns to the empirical results followed by a summary of sensitivity analyses in Section V. Section VI is a calculation of the overall gains of the programme and Section VII concludes.

II. Local context

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 most measures of 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 due to the existence of substantial benefit transfers and the universal provision of public housing. About 70 percent of the most deprived neighbourhoods are located in the four largest cities (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

[6] A recent study by Diamond and McQuade (2016) analyses the effects of increases in the *quantity* of subsidised houses and finds heterogeneous effects due to the increase of low-income households in the area: housing subsidies cause house prices to decrease in higher income areas, while generating house price increases in lower income areas, which is mainly due to sorting effects

between deprived 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 secretary of state 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 in public housing over a course of ten years (in total about € 7 thousand per household residing in these neighbourhoods) (The Court of Audit, 2010).[7] Although the exact expenditure is unknown, experts estimate that in the end about € 1 billion has been invested between 2007 and 2012 (Permentier et al., 2013). About 90 percent of the money was spent on reinvigorating public housing. Upgrading entails painting the exterior and upgrading the outside appearance of the buildings, adding double glazing and insulation, adjusting gardens belonging to apartment blocks and sometimes demolishing deteriorated housing and replacing it with new apartments. After 2012 the programme was abolished.

Arguably, the physical restructuring of public housing has a beneficial effect on nearby residents who prefer to live in a well-maintained building environment (Rossi-Hansberg et al., 2010). Such an environment not only improves views, but also may improve physical and mental health, according to a large environmental psychology and health literature (Srinivasan et al., 2003). Apart from physical restructuring and sale of public housing, a small share of the investment was targeted at poor households directly through empowerment programs (Department of Housing, Spatial Planning and the Environment, 2007; Wittebrood and Permentier, 2011).

Another effect 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). Furthermore, it may be that upgrading of public housing will have a differential effect in high and low income neighbourhoods, as is documented by Diamond and McQuade's study on an increase in the quantity of subsidised housing. We will show that there are minor changes in the social composition in the treated neighbourhood, but controlling for demographics, including neighbourhood income, leaves the price effect essentially unaffected. Heterogeneity of the treatment effect related to the demographic composition is also minor. For example, we do not find any evidence that the effects of KW-investments depend on the neighbourhood

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

income level. Hence, most of our effect seems to be explained by improvements in the physical appearance of neighbourhoods.

B. Selection of neighbourhoods

To select eligible neighbourhoods so-called deprivation scores consisting of 18 indicators were used. The indicators 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 prices, rents, sales times) were *not* one of the indicators. Brouwer and Willems (2007) use data from 1998, 2002 and 2006 to calculate so-called deprivation *z-scores* for each postcode area in the Netherlands with at least 1 thousand inhabitants (about 4 thousand 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 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. Only neighbourhoods are considered that have a lower than average z-score *for each category* (hence: a z-score for each of the categories lower than zero) were considered (Permentier et al., 2013). Furthermore, neighbourhoods with a z-score of at least 7.30 were eligible. However, four neighbourhoods were removed from the list because they did not had a lower than average z-score on *each* of the categories. Eight other neighbourhoods, after discussion with local governments, were also removed. These were mainly downtown neighbourhoods for which the recorded nuisance was related to retail, nightlife and entertainment activities, which are not characteristics of deprivation. In addition, the local governments of Amsterdam and Enschede argued that two neighbourhoods in their locality for which the z-score was sufficiently high (above 7.3) should be replaced by two neighbourhoods that were below the z-score (respectively 6.84 and 5.00) because the latter neighbourhoods were argued to experience more deprivation.[8] In the end, this implies that fourteen neighbourhoods did not comply with the scoring rule. More information on the selection procedure and the non-complying neighbourhoods is listed in Appendix B.1.

Table 1 reports the z-scores for each of the categories. Unsurprisingly, targeted KW-neighbourhoods have scores that are much higher than the Dutch average for each of the categories.

[8] 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.

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).

In our empirical analysis we will exploit exogenous variation using the arbitrary threshold of 7.3 to identify the causal effect of the programme. We illustrate some of the features of our research design, as well as testing some assumptions underlying the regression-discontinuity design we employ later. We start the analysis by plotting the assignment as a function of z-scores in Figure 1. 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 78 percent. In the empirical analysis we will exclude observations within 2.5 kilometres of a treated neighbourhood. Because many non-complying neighbourhoods are relatively close to treated the neighbourhoods, the jump will then increase to more than 90 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. 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, 2002 and 1998. It is therefore highly unlikely that local governments anticipated the exact selection criteria. More formally, we estimate a McCrary (2008) test for bunching

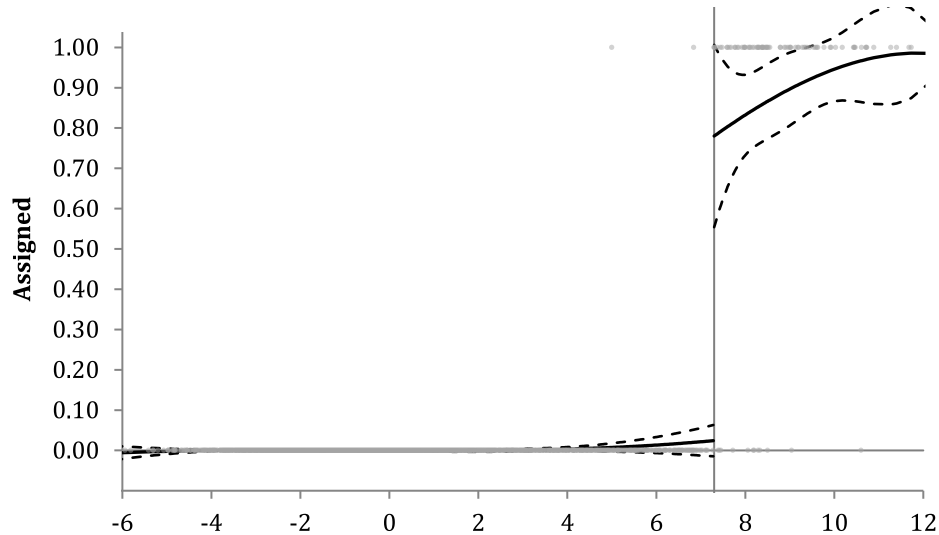


FIGURE 1 — THE Z-SCORE AND SELECTION

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.

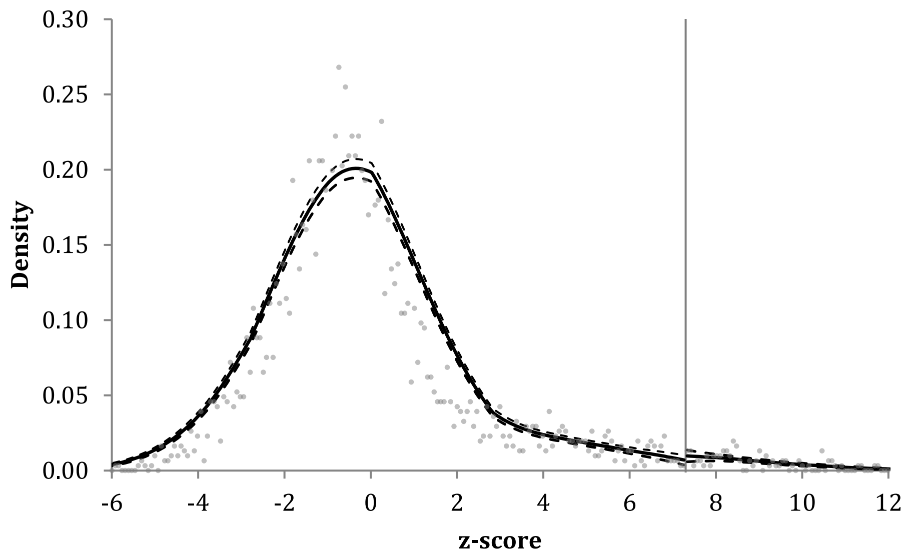


FIGURE 2 — MANIPULATION TEST FOR Z-SCORES

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.

around the threshold. This test investigates whether the density of the z-score is continuous at the threshold. Figure 2 shows that this is indeed the case, which supports our claim that local governments could not manipulate the z-scores.[9]

III. Empirical framework, data and graphical analysis

A. A regression-discontinuity design

We are interested in the causal effect of the KW-investment scheme on surrounding house prices. Let $\log p_{\ell t}$ be the logarithm of the house price per square meter for a housing unit in neighbourhood ℓ in year t. The house price is thought to be a function of whether the neighbourhood has received investments $k_{\ell t}$ in year t.

When estimating the causal effect of $k_{\ell t}$ on prices one faces three main issues. The first is that spatial spillovers of the KW-programme may exist: houses close to a targeted area may also experience changes in $p_{\ell t}$ due to investments in adjacent neighbourhood (see Rossi-Hansberg et al., 2010). Not controlling for spatial spillovers may lead to a strong underestimate of the programme's benefits. In the preferred specifications we therefore exclude observations within two and a half kilometres of a targeted neighbourhood.[10]

The second issue is that the treatment is explicitly non-random as that the most deprived neighbourhoods are targeted. To resolve this issue we employ a first-differences approach, where the change in the price, $\Delta p_{\ell t}$, is regressed on the change in the investment. By construction, $\Delta k_{\ell t}$ then equals one when we observe a property located in a targeted area *before* and *after* the starting date of the programme and equals zero otherwise. When looking at changes in prices, 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, the preferred specifications therefore *only* include observations that refer to changes before and after the starting date of the programme. To further 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 $\Delta x_{\ell t}$.

The third issue is that, while first-differencing may control for all time-invariant differences between neighbourhoods before treatment, it does not address the issue that unobserved trends may be correlated with the change in treatment, $\Delta k_{\ell t}$. This may be problematic when demographic trends such as gentrification are correlated to the probability

[9] The discontinuity estimate is only 0.349 with a standard deviation of 0.324, so this jump is highly statistically insignificant. When we just concentrate on the neighbourhoods around the threshold it also appears that the distribution of z-scores around the threshold is continuous, as is shown in Figure B1 in Appendix B.1.

[10] In a sensitivity analysis (Appendix C.7), we investigate the presence of potential spatial spillovers.

to become treated. To address this issue, we need to find neighbourhoods that are almost identical to 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). In this paper we *combine* first-differencing with a RDD based on the deprivation score of the neighbourhood.[11] This implies that we compare changes in prices close to the z-score threshold. This approach approximately provides the causal effect of the investment if neighbourhoods are not able to manipulate the score. We already argued and showed in the previous section that it is extremely unlikely that manipulation is a problem.

Although local governments could not 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) 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).

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 use a local linear (LL) regression approach, where observation close to the threshold receive a higher weight (Hahn et al., 2001).

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:

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

where the $\sim$ indicates first-stage coefficients and $\tilde{\pi}$ is the parameter of interest and $K(\cdot)$ denotes the kernel function.

Note that $\Delta s_{\ell t}$ equals one when $z \geq 7.30$ and when a property is sold before and after the investment. 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):

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

Throughout the analysis we adopt a uniform kernel:

$$(3) \qquad K\left(\frac{z_{i\ell} - c}{h}\right) = 1_{|z_{i\ell}-c|<h},$$

[11] One may also estimate a cross-sectional RDD by comparing treated neighbourhoods with non-treated neighbourhoods after the treatment has taken place. The latter set-up requires stronger identifying assumptions because time-invariant unobservable factors should be uncorrelated to the treatment. We therefore prefer the current approach.

where h is the bandwidth that determines 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. Because we employ a FRD, the formula to determine the optimal bandwidth is somewhat modified, but note that the optimal bandwidth in a FRD is usually very similar of the optimal bandwidth in a SRD. See the Appendix B.3 for the derivation of the optimal bandwidth.

Note that a regression-discontinuity design identifies 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 ($h \to \infty$), this would suggest that the local average treatment effect at the threshold is equal to the average treatment effect.

To get more insight into the mechanism of the effects we also gather data on demographic variables of the neighbourhood, such as average neighbourhood income, 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; Diamond and McQuade, 2016).[12]

B. Adjustment effects and treatment heterogeneity

We are also interested in adjustment effects after the investment has taken place. In Appendix A we outline a standard spatial equilibrium model that we combine with the search and matching framework of Wheaton (1990). When we assume that search costs are proportional to amenity levels *with a delay*, we show that sales time drop in the short run, while this effect disappears in the long run. The time it takes for sales time to adjust to the former value is indicative the time for the housing market to return to a steady state. This helps to identify the long-run price effect. Let $d_{\ell t}$ be a variable that indicates how many years after the investment the transaction has taken place. We estimate:

[12] In a standard hedonic regression, changes in neighbourhood demographics are usually endogenous. However, because of our research design, this should not be the case as changes in neighbourhood demographics close to the threshold should be (almost) identical in absence of the programme. In Appendix C.2 we instrument for potentially endogenous neighbourhood characteristics with shift-share instruments and show that the results are then virtually identical to the results where we do *not* control for neighbourhood demographics.

$$(4)\quad \left(\hat{\alpha},\hat{\beta},\hat{\delta}_{\wp},\hat{\upsilon}_t\right)=\underset{\alpha,\beta,\delta_{\wp},\upsilon_t}{\arg\min}\sum_{i=1}^{N}K\left(\frac{z_{i\ell}-c}{h}\right)\times\left(\Delta\log p_{\ell t}-\alpha\Delta k_{\ell t}-\sum_{\wp=1}^{\mathcal{P}}\delta_{\wp}\Delta(k_{\ell t}\times d_{\ell t})^{\wp}-\beta\Delta x_{\ell t}-\Delta\upsilon_t\right)^2,$$

where α indicates the immediate effect and $\delta_{\wp}$ are parameters that capture adjustment effects. We define $\log s_{\ell t}$ to be the logarithm of days on the market. We also estimate:

$$(5)\quad \left(\hat{\zeta},\hat{\eta},\hat{\theta}_{\wp},\hat{\varphi}_t\right)=\underset{\zeta,\eta,\theta_{\wp},\varphi_t}{\arg\min}\sum_{i=1}^{N}K\left(\frac{z_{i\ell}-c}{h}\right)\times\left(\Delta\log s_{\ell t}-\zeta\Delta k_{\ell t}-\sum_{\wp=1}^{\mathcal{P}}\theta_{\wp}\Delta(k_{\ell t}\times d_{\ell t})^{\wp}-\eta\Delta x_{\ell t}-\Delta\varphi_t\right)^2.$$

where ζ, $\theta_{\wp}$, η and φ_t are parameters to be estimated. The above equations indicate that we have $\wp+1$ endogenous variables. The instruments are then the change 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.

Besides adjustment effects we also will pay attention to treatment heterogeneity by investigating *(i)* whether the treatment is different for higher ranked neighbourhood in terms of z-score to investigate whether place-based policies are more effective in more deprived neighbourhoods, and *(ii)* how the treatment varies with demographic characteristics. This is interesting because the effects may be different for high and low income households, in line with Diamond and McQuade (2016), or may be stronger in denser areas where spillovers are more likely to arise because properties are closer to each other.

C. Data and descriptives

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 transactions between 2000 and 2014, so roughly seven years before and after the investment took place. For 1,796,542 transactions, we know the transaction price, list 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 a few outlier observations.[13] These selections do not influence the results. On average, properties in our

[13] 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 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 or are listed for more than five years on the market or were listed zero days on the market. We also exclude observations for which the transaction to list price ratio is below 0.7 or above 1.1.

sample are sold 1.29 times in our study period. In our main analysis, we focus on repeated sales, so properties that are sold at least twice, leaving us with 434,033 transactions.[14]

We report descriptives in Table B2 in Appendix B.1. It appears that about 3.8 percent of the observations in the repeated sales sample – 16,726 observations – is in a KW neighbourhood of which 42 percent after the investment. The price per square metre in KW-neighbourhoods is 3.5 percent lower than in non-KW-neighbourhoods. The difference is small, but is consistent with the observation that most deprived neighbourhoods are located in urban, rather than rural areas, where prices are generally higher.[15] Table B3 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.[16]

In Figure B2 in Appendix B.1 we plot the house price for KW and other neighbourhoods over time. 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).[17]

To allow for changes in neighbourhood demographics and for treatment heterogeneity, we gather data from *Statistics Netherlands* on demographics, including average income, population density, the share of foreigners, age composition and household size. For income, data is missing before 2004. Hence, we impute income data using national growth and 2004 income level. Our results are identical if we exclude years before 2004. We also obtain detailed land use data from *Statistics Netherlands* on the share of residential land, industrial land, land used for infrastructure, open space and water bodies.[18]

[14] Using repeated sales may imply a selection problem, because certain house types may be sold less often. In Appendix C.10 we check whether our results are robust with respect to this selection.

[15] 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.

[16] 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.

[17] In the Appendix B.1 it is also shown that the sales time for targeted and non-targeted neighbourhoods are pretty similar until 2007. After the investment, the sales time is much lower in KW-neighbourhoods than in other neighbourhoods. This difference seems to become somewhat smaller over time and disappears in 2013.

[18] The land use data is only available for the years 2000, 2003, 2006, 2008 and 2010, so we impute land use for the intermediate years and assume that land use has not changed after 2010.

D. Graphical analysis

In Figure 3 we plot price changes around the threshold, while controlling for the z-score using a third-order polynomial. Note that our identification strategy is not based on a standard RDD-design in levels. The latter would require stronger identifying assumptions because it requires that not only time-varying but also time-invariant unobservable factors should be uncorrelated to the treatment around the cut-off. Because we identify the effect based on changes, only time-varying unobservables should be uncorrelated to the treatment around the cut-off, whereas we allow time-invariant unobservables to be correlated. Moreover, because many (unobservable) factors that influence prices are omitted, the approach using variation in price *levels* may be less efficient and lead to larger standard errors than the approach using variation in price *changes* (Imbens and Lemieux, 2008). We therefore exploit variation in prices 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.[19] The price difference is statistically significant at the one percent level. We will also focus on sales time to examine adjustment effects. In Panel A of Figure 4 we show that sales times are statistically significantly lower (at the five percent level) when $z > 7.3$. This graphical analysis hides that the price and sales time effects might differ in the short and long run, something which we address in Section V.B.

We also test whether changes in covariates are continuous at the threshold. In Panels B and C we look at differences in changes in house size and maintenance quality respectively. If owner-occupied properties would be directly targeted by the place-based investment, one would expect a change in maintenance quality or house size. We do not observe significant changes confirming that home owners are only indirectly impacted by the policy.

In Panels D, E and F of Figure 4 we investigate changes in demographics. We emphasise here that those changes do not necessarily have to be continuous at the threshold as changes in demographics may be a direct result of the policy.[20] It can be seen that neighbourhood income is about 1.5 percent lower after the investment, which is statistically significant at the five percent level. We do not know whether this reduction is due to changes in the composition of the public housing tenants, for example because public housing associations accepted different tenants, or due to changes of households in non-public housing, but it implies that after a place-based investment, neighbourhoods become slightly more attractive to poor households, in line with arguments of Diamond and McQuade (2016). For population density and share of foreigners we do not observe statistically significant changes. In

[19] These results (available upon request) are essentially identical if we use higher-order polynomials.

[20] Gerritsen et al. (2017) provide some evidence that the share foreigners (in levels) may be discrete at the z-score threshold. Because we investigate the effect of the change in the treatment on the change in house prices this is not a major problem. Nevertheless, we have also estimated ancillary regressions where we control for demographics in levels. The estimated coefficient is similar, albeit even somewhat stronger than the baseline estimate.

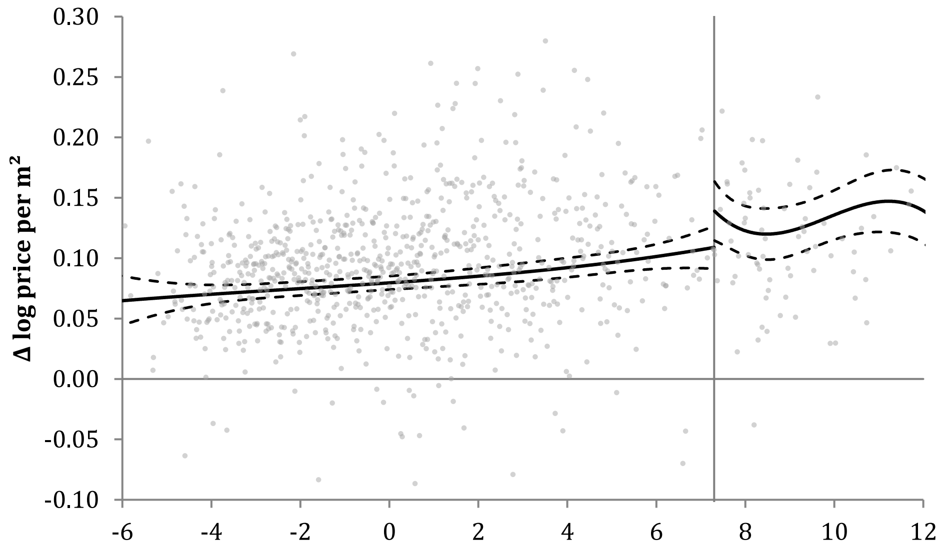


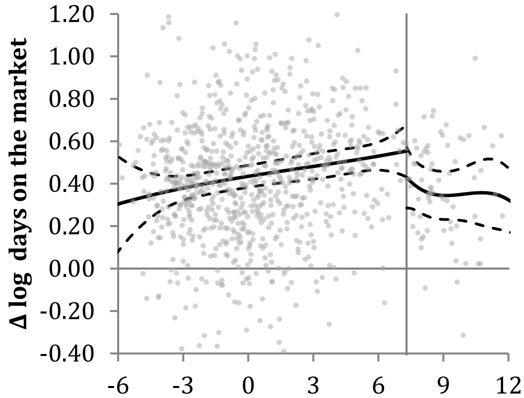
FIGURE 3 — CHANGES IN HOUSE PRICES AROUND THE THRESHOLD

Notes: We estimate weighted regressions of the change in log prices on year fixed effects, separate third-order polynomials of the z-score on both sides of the threshold, as well as a dummy indicating the change in treatment status. We instrument the latter with a dummy indicating the change in the scoring rule. The weights are equal to the inverse of the number of observations in a neighbourhood. Each dot represents the conditional average for a given z-score. We exclude observations within 2.5km of a treated neighbourhood.

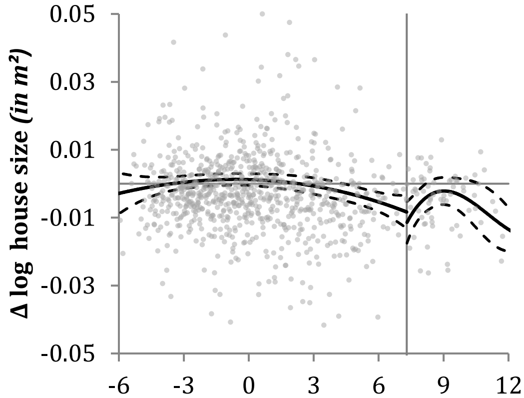
Appendix B.6 we investigate the effects on all demographics in more detail using a local linear approach. We also will test whether the KW-programme has influenced home-ownership shares. This may be important, as (part of) the treatment effect may be due to changes in home-ownership rates through selling of public housing.

To investigate whether we measure an effect of sorting or whether the treatment effect captures changes in the neighbourhoods' amenity levels, we will estimate specifications where we control for demographic characteristics and home-ownership shares and show that the treatment effects is essentially the same.[21] Furthermore, we investigate treatment heterogeneity in more detail in Section IV.C.

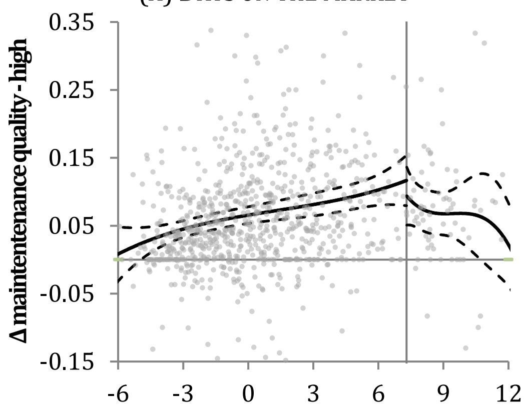
[21] We address potential endogeneity of demographic variables in Appendix C.2.



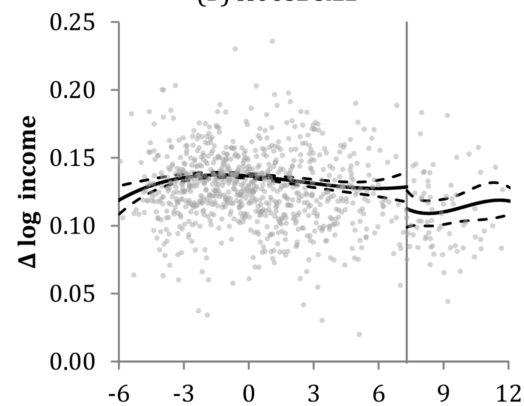
(A) Days on the market



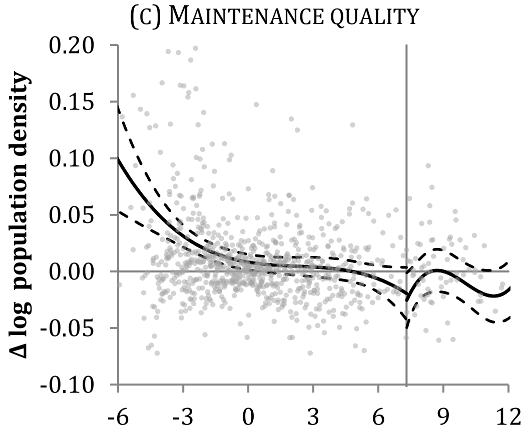
(B) House size



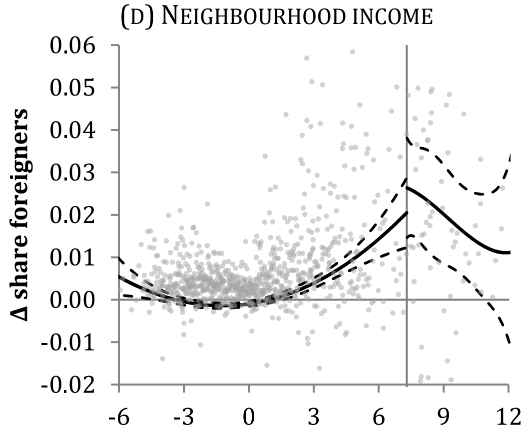
(C) Maintenance quality



(D) Neighbourhood income



(E) Population density



(F) Share foreigners

Figure 4 — Changes in covariates around the threshold

Notes: We estimate weighted regressions of the variable of interest on year fixed effects, separate third-order polynomials of the z-score on both sides of the threshold, as well as a dummy indicating the change in treatment status. We instrument the latter with a dummy indicating the change in the scoring rule. The weights are equal to the inverse of the number of observations in a neighbourhood. Each dot represents the conditional average for a given z-score. We exclude observations within 2.5km of a treated neighbourhood.

IV. Results

A. Baseline results

We analyse the price effect in the neighbourhood that received the KW-investment compared to the non-treated neighbourhoods. Table 2 reports the regression results.[22]

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 5.4 percent.[23] When we control for changes in housing attributes (column (2)), prices in targeted neighbourhoods have increased by 4.6 percent, relative to prices in other neighbourhoods. In column (3) we employ a sharp regression-discontinuity design by excluding non-complying neighbourhoods. We find an optimal bandwidth of 4.3, which implies that we only include about 15 percent of the observations. The price effect is 4.4 percent and similar to the previous specification.[24] 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 B5 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. The second stage results are in line with previous specifications. The result in column (4), Table 2, implies that prices in KW-neighbourhoods have increased by 4.2 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 by about 30 percent, this hardly has an impact on the price effect (4.4 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 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

[22] In all specifications, we cluster the standard errors at the neighbourhood level, because the treatment varies at the neighbourhood level. One may argue that treated neighbourhoods are often concentrated in space. We also have estimated standard errors while considering treated neighbourhoods that are adjacent to each other as one. This makes little difference for the estimated standard errors. The results are available upon request.

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

[24] One may argue that controls are not necessary in a valid RDD. 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 2 — 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	0.0526***	0.0452***	0.0426***	0.0412***	0.0430***	0.0332***
	(0.0123)	(0.0111)	(0.0119)	(0.0127)	(0.0125)	(0.0115)
Δ Size *(log)*		-0.862***	-0.880***	-0.886***	-0.879***	-0.863***
		(0.00582)	(0.0146)	(0.0153)	(0.0176)	(0.0193)
Δ Rooms *(log)*		0.00349***	0.00410***	0.00271*	0.00532***	0.00449**
		(0.000474)	(0.00146)	(0.00152)	(0.00170)	(0.00210)
Δ Maintenance quality – high		0.109***	0.100***	0.0957***	0.102***	0.0981***
		(0.00145)	(0.00302)	(0.00344)	(0.00339)	(0.00395)
Δ Central heating		0.0675***	0.0722***	0.0713***	0.0799***	0.0712***
		(0.00246)	(0.00472)	(0.00475)	(0.00526)	(0.00598)
Δ Listed building		0.0107+	0.00430	0.00210	-0.00703	-0.0170
		(0.00675)	(0.0157)	(0.0185)	(0.0121)	(0.0158)
Δ Neighbourhood income *(log)*						0.0690
						(0.0700)
Δ Population density *(log)*						0.00809
						(0.0794)
Δ Share foreigners						-1.085***
						(0.148)
Δ Share young people						0.00671
						(0.491)
Δ Share elderly people						-1.042***
						(0.285)
Δ Average household size						0.0796
						(0.113)
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	185,072	185,072	28,476	24,170	16,839	11,579
Number of clusters	3138	3138	257	176	285	184
R^2-within	0.365	0.526	0.543			
Kleibergen-Paap F-statistic				4797	9191	2592
Bandwidth h^*			4.312	3.229	4.600	3.419

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
+ Significant at the 0.15 level

neighbourhood demographics are influenced by the policy. We find evidence that KW-neighbourhoods have seen a relative decrease in neighbourhood income and an 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 changes in the demographics induced by the programme have caused the price changes, or whether the effect of the place-based investments is mainly due to a direct change in the quality of nearby public housing, we control for additional demographic variables in column (6), Table 2. More specifically, we include changes in population density, the share of foreigners, share of young (<25 years) and elderly people and the average household size and land use. 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 effect of place-based policies.[25] This also 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.[26]

In what follows, we will generalise our results in two directions. First, we investigate the steady state dynamics. Second, we pay attention to treatment heterogeneity.

B. Adjustment effects

We will now explicitly distinguish between short-run and long-run effects by allowing for adjustment effects. According to theory (see Appendix A.3), we expect that the price effect is permanent. Sales times are expected to become smaller over time and disappear in the long run once the market has reached a new steady state. This is given the assumption that search costs are proportional to house prices – but only in the long run. To examine the latter, we also analyse the effects on log days on the market. We estimate equation (4) and use the local linear approach without neighbourhood variables, which corresponds to the specification listed in column (4) in Table 2. We report the estimated coefficients in Table 3.[27]

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). Also the interaction term is positive, so that the price effect becomes somewhat

[25] We explore this conclusion further in Appendix C.2, where we instrument for potentially endogenous changes in neighbourhood characteristics.
[26] It may be that due to the programme home-ownership rates have changed so that part of the price effect may be attributed to an increase in home ownership. While we find some evidence in Appendix B.6 that home-ownership rates indeed have increased, we do not find evidence that the price effect is any different once we control for changes in home-ownership rates (see Appendix C.7).
[27] The bandwidth is optimised assuming that the interaction terms are exogenous. Given that the bandwidth is similar for the SRD and the FRD, we do not expect that this has any impact on the results.

TABLE 3 — REGRESSION RESULTS: ADJUSTMENT EFFECTS

	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	0.0203**	0.0281**		-0.315***	-0.326***	
	(0.00862)	(0.0124)		(0.0682)	(0.104)	
Δ (KW-investment × Years after investment)	0.00633***	0.000307		0.0366***	0.0446	
	(0.00222)	(0.00458)		(0.0119)	(0.0475)	
Δ (KW-investment × Years after investment)^2		0.000764			-0.00100	
		(0.000707)			(0.00567)	
Δ KW-investment × I(0.0-2.5 Years after investment)			0.0297***			-0.281***
			(0.0102)			(0.0650)
Δ KW-investment × I(2.5-5.0 Years after investment)			0.0416***			-0.194***
			(0.0123)			(0.0576)
Δ KW-investment × I(5.0-7.5 Years after investment)			0.0559***			-0.0605
			(0.0190)			(0.0677)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	24,170	24,199	24,478	57,651	57,651	56,097
Number of clusters	176	177	178	545	545	6.144
Kleibergen-Paap F-statistic	2413	1334	2108	5252	3370	22206
Bandwidth h^*	3.242	3.257	3.287	6.219	6.211	6.144

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
+ Significant at the 0.15 level

stronger over time. The specification predicts that after five years the price effect is 5.1 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 interaction effects. However, it is more insightful to test the joint significance of these coefficients over time. The results are presented in Figure C1 in Appendix C.1. After five years the price effect is 4.7 percent, while the immediate price effect is 2.8 percent. In column (3) we include interaction terms of the treatment variable and 2.5 year-interval dummies. The same pattern emerges: the price effect is increasing over time, but not so strongly (the p-value of 0.0393 indicates that the coefficients are significantly different at the five percent level). 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 Appendix

C.9). In any case, the results demonstrate that the price effect is permanent and that the price jumps once the policy was introduced.

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 3). The decrease in sales times is 27 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 12.3 percent. After 7.5 years, the effect is essentially zero and highly insignificant. The same holds if we include a second-order term in Column (5). Figure C1 in Appendix C.1 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 show that place-based investments have a permanent effect on house prices, whilst only a temporary effect on sales times 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 and provides us with information what is the long-term price effect.[28]

C. Heterogeneity in the treatment effect

In this subsection we investigate whether we can detect heterogeneity in the treatment effect. We report results in Table 4. As a first check, we interact the treatment effect with the deprivation rank. We normalise the rank to be between minus one and one, with zero being the average neighbourhood, minus one being the worst treated neighbourhood and one being the best treated neighbourhood (in terms of the z-score). The instrument is the change in the scoring rule and the rank if just the z-score was determined to select the neighbourhoods. In column (1) we observe that in the average treatment effect is 3.8 percent, which is close to the baseline estimate. The interaction effect is not statistically significant at conventional levels. For sales times, we observe a similar pattern (see column (5)): the instantaneous effect does not seem to be stronger in worse performing neighbourhood.

In column (2) we investigate whether the treatment effect is different for neighbourhoods with different demographic characteristics. To this end, we interact the treatment effect with the same demographics as reported in Table 2. To have a meaningful main effect, we subtract the locally weighted mean (based on the corresponding bandwidth) from the demographic variable of interest. These results indicate that place-based investments are much more

[28] 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 observe a permanent effect on sales time, in contrast to our results which show a temporary effect on sales time.

TABLE 4 — HETEROGENEITY IN THE ESTIMATED EFFECTS

	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
	Rank	*Demographic heterogeneity*	*Investment intensity*	*Rank*	*Demographic heterogeneity*	*Investment intensity*
Δ KW-investment	0.0375***	0.0172		-0.333***	0.0144	
	(0.0111)	(0.0133)		(0.0973)	(0.130)	
Δ (KW-investment × Years after investment)				0.0420**	0.0630***	
				(0.0207)	(0.0181)	
Δ (KW-investment × Deprivation rank)	0.0143			-0.0601		
	(0.0138)			(0.0875)		
Δ (KW-investment × Neighbourhood income *(log)*)		0.0341			-0.594**	
		(0.0404)			(0.245)	
Δ (KW-investment × Population density *(log)*)		0.0622***			-0.116⁺	
		(0.0148)			(0.0725)	
Δ (KW-investment × Share foreigners)		-0.0850			-0.856*	
		(0.0805)			(0.455)	
Δ (KW-investment × Share young people)		0.696**			4.194	
		(0.347)			(3.920)	
Δ (KW-investment × Share elderly people)		0.0829			3.063***	
		(0.162)			(0.979)	
Δ (KW-investment × Average household size)		-0.127⁺			-0.420	
		(0.0867)			(0.603)	
Δ (KW-investment × Investments per m²)			0.00583			-0.0195
			(0.00848)			(0.0488)
Δ (KW-investment × (Investments per m²)²)			0.00204			-0.0207*
			(0.00158)			(0.0106)
Δ (KW-investment × Years after investment × Investments per m²))						0.0137***
						(0.00444)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighbourhood characteristics (10)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	14,260	11,459	10,794	11,865	63,727	63,857
Number of clusters	241	179	168	192	2285	2295
R^2-within			0.532			0.056
Kleibergen-Paap F-statistic	703.8	169		387.9	283.8	
Bandwidth h	4.248	3.345	3.467	3.513	9.321	8.995

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
⁺ Significant at the 0.15 level

effective in dense areas. For example, when population density doubles, the treatment effect is 4.3 percentage points higher. We think this makes sense: as we are interested in the external effect of investments in public housing on surrounding properties, it critically matters how close the properties are to each other. Our finding that the effects are stronger in denser areas suggest that there is a strong spatial decay in spillovers, in line with Rossi-Hansberg et al. (2010). One may also notice that there seems to be a stronger effect in neighbourhoods with a higher share of young people. However, this effect is counteracted by a negative effect of household size. If we exclude household size, the effect of young people is highly statistically insignificant. In column (5) we find somewhat different effects for sales times, although the results are not always statistically strong. The immediate effect on sales times seems to be lower in areas with lower income and that are denser. Furthermore, the sales time effect seems to be substantially less strong in areas with a high share of elderly people.[29]

In column (3) of Table 4 we use ancillary data on the *direct spending by the national government per neighbourhood* (€ 250 million, about one quarter of total spending). Because our instrument is not informative on the level of spending, we employ a sharp regression-discontinuity design where we exclude non-complying neighbourhoods. We then include the spending per square metre of the treated neighbourhood area and the spending squared. The results suggest that the effect of spending is insignificant. However, when we plot the treatment effect as a function of spending (see Figure C2 in Appendix C.1), we find that neighbourhoods that have received more funding per square metre also have a higher treatment effect. For example, the treatment effect for the average spending (€ 2.11 per m^2 neighbourhood area) is 2.1 percent, similar to the baseline estimate albeit a bit lower. For the neighbourhood that received the most spending (€ 6.83 per m^2) we find a treatment effect of 13.4 percent. We note, however, that the confidence intervals are not small. The effects on sales times are similar: it seems that for areas that have received more funding, the instantaneous sales time effect is stronger. We add one caveat to these results: we only have information on the direct spending by the national government, while the total spending intensity (including the spending by housing associations) may not be strongly correlated to the direct spending, as we do not know whether the spending by housing associations is positively, or negatively, related to the direct spending by the national government.

V. Sensitivity analysis

We subject the baseline results to a wide range of additional robustness checks and ancillary regressions. In Appendix C we discuss all sensitivity analyses in detail. Here, we restrict ourselves to a summary.

[29] In Appendix C2 we test robustness of these results by adding the interactions with the demographic variables one by one. This leads to similar conclusions.

First, in Appendix C.1 we investigate whether the baseline results as reported in Table 2 hold for time on the market, while including a linear term of sales time and years after the investments. The results indicate a consistent and strong negative effect of the place-based investment on sales time, which becomes less pronounced over time in all specifications.

In Appendix C.2 we investigate whether the small decrease in the price effect when we control for changes in demographics and land use (see column (6), Table 2) is mainly due to changes in neighbourhood demographics or land use. We further instrument for potentially endogenous neighbourhood characteristics and show that the price effect is very similar. This is followed in Appendix C.3 by a sensitivity analysis for the heterogeneity in the treatment effect, leading to the same conclusion that place-based investments are more effective in denser areas.

Fourth, In Appendix C.4 we investigate whether we can detect effects on the private and public housing rental market. Using information on housing surveys from 2002, 2006, 2009, 2012 and 2015. Each wave consists of about 60 thousand respondents and is considered as a representative survey of the Dutch population. The surveys provide information on a wide range of housing characteristics including the rent paid and whether the property is rent-controlled (which is always the case for the public housing stock). We first focus on the effects of the (non-controlled) private rental stock. The preferred estimate seems to suggest that rents have increased by about 9 percent due to the KW-programme. In line with expectations, we do not find evidence of adjustment effects: the increase in private rents is immediate and permanent. A caveat here is that because the private rental market is such a small proportion of the housing market, the number of observations is rather low and the estimates are imprecise and only marginally significant. We repeat those analyses for the regulated rent where we do not find any evidence of the KW-programme having an effect on rents. Because those rents are rent-controlled and essentially do not relate to underlying characteristics of the property or neighbourhood, this result makes sense.

In Appendix C.5 we run a set of quasi-placebo experiments. We investigate whether we can detect price changes in neighbourhoods that were on a previous list of 340 neighbourhoods that had 'some' deprivation. Alternatively, we use a list of neighbourhoods that receive investments by another programme before the study period (so are in some way deprived) but do not receive KW-investments. A final placebo check involves the use of alternative deprivation scores to exploit the randomness in the determination of the z-score. We then investigate what happens if the 83 worst performing neighbourhoods would have been selected based on this alternative score. All these quasi-placebo regressions support 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.

Sixth, in Appendix C.6 we further investigate the issue of unobserved trends that may be correlated to the treatment. In particular, one may argue that the year of implementation is about at the peak of house prices. Mean reversion would then imply that prices of KW and non-KW neighbourhoods converge. As a check, we therefore exclude transactions around the peak of the housing market, between 2005 and 2011. Although this strongly reduces the number of observations, the coefficients is essentially unaffected despite the somewhat larger standard error. Even though we employ a RDD, and control for neighbourhood income, one might still 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. We continue by controlling 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. The treatment effects, however, are essentially unaffected.

Seventh, we examine in Appendix C.7 whether spatial spillovers of the investment programme are important. When allowing for spatial spillovers, we need 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 treated areas. Hence, we include the number of treated neighbourhoods within 500 metre rings of the property. The main effects are very similar to the baseline estimates, while spillovers are largely statistically insignificant.

In Appendix C.8 we investigate whether controlling for the share of home ownership and the share of private rental housing changes the results. This may be important for the interpretation of the result as the programme led to some (but small) increases in the share of owner-occupied housing (see Appendix B.6). However, using information on housing surveys for 2002, 2006, 2009 and 2012 we do not find any evidence that our results can be explained by changes in home ownership, as the treatment effect is essentially identical once we control for changes in the share of private rental and owner-occupied housing. Hence, it seems that the policy indeed had an effect through the improvement of public housing.

Ninth, we test robustness of our results with respect to the starting date of the investment in Appendix C.9. 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. However, the uncertainty on the exact starting date of the programme seems not to matter for our results.

Tenth, in Appendix C.10 we test for robustness of our results to assumptions with respect to the bandwidth of the local linear regression approach. This is followed by an investigation whether using the full sample, rather than repeated sales, influences our results. The coefficients are very similar to the baseline specification, if anything slightly higher. The results are reported in Appendix C.11.

Finally, we confirm in Appendix C.12 that our results are robust to the choice of identification strategy by employing a nonparametric propensity score matching method, rather than a regression-discontinuity approach.

VI. The KW-programme and the overall gains in property values

We aim to gain insight in the rate of return through the external effect of the revitalisation policy. We reiterate that we measure external effects because we focus on investments in the public housing stock on the prices of owner-occupied properties. Hence, we do not have estimates on the direct effect of the programme. Nevertheless, the direct effect are expected to be positive. Because we focus on the external effects, the calculated rate of return will serve as a severe underestimate of the total benefits of the programme.[30]

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. Furthermore, we gather data on the average house prices of all properties in each neighbourhood, including rental properties, which have slightly lower housing values than owner-occupied properties.[31] Table 5 reports the back-of-the-envelope calculations for different scenarios.[32] 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 effect is somewhat higher once we use the long-run estimate. The total benefits for home owners are about € 0.5 billion. The results indicate the gain-to-funding ratio is about 0.5 given the realised investments of € 1 billion.

To also include the benefits on (non-controlled) private renters, we use the average house prices of all properties. In the second calculation we assume that the effect on private renters is the same as for home owners. Because the share of owner-occupied housing is small in KW-neighbourhoods (only 24 percent), the benefits are now substantially larger: the gain-to-funding ratio is about 0.8.

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[30] 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 positive externalities are reduced in non-targeted areas (Rossi-Hansberg et al., 2010).

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

[32] 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 showed 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.

TABLE 5 — TOTAL BENEFITS OF THE KW-PROGRAMME

	Benefits per property owner *(in €)*		Total benefits *(in billion €)*	
Scenarios	*Baseline estimate*	*Long-run estimate*	*Baseline estimate*	*Long-run estimate*
1) Owner-occupied housing	5224	6347	0.481	0.585
2) Owner-occupied and private-rental housing →*Effect on private-rental equal to effect on owner-occupied housing*	5200	6318	0.824	1.001
3) Owner-occupied and private-rental housing	9715	12258	1.539	1.942
4) All housing →*Effect on public housing equal to effect on private-rental housing*	13102	16723	4.951	6.319

Notes: The estimated benefits are in 2007 prices. Information on number of housing units is based on Statistics Netherlands 2012.

It is maybe preferable to explicitly use the point estimates for the private rental market as shown and discussed in the sensitivity analysis. Because we find stronger effects for private rents than for the owner-occupied housing market (see Appendix C.4), the average benefits to property owners (including landlords) are 85 percent higher. This also implies gain-to-funding ratios that are above one, so the external effects of the KW-investment programme are larger than the supposed investments.

Homeowners living in the neighbourhood at the time of the investments directly have benefited from the programme due to the increase in house prices. Homeowners are usually higher-income households. The KW-programme was meant to help poor households and reduce inequality, so homeowners were not the intended beneficiaries of the programme. We show that the subsidy does not capitalise in controlled rents (see Appendix C.4), so public housing renters enjoy the positive neighbourhood effects that are caused by the programme without paying for it. To get a rough idea of the total benefits to renters in public housing, we assume an identical monetary effect on public rental housing as on private rental housing. The average benefits per property are then about € 13,000. This also implies that the gain-to-funding ratios are substantially larger and around 5, so that about two-thirds of the external benefits of the programme accrue to renters in public housing.[33] Note that public housing renters, besides the increase in neighbourhood quality, also have benefited directly from

[33] The total benefits in the fourth scenario are € 4.95 billion, while they are € 1.54 billion when we ignore public housing tenants. Hence, 69 percent of the total benefits accrue to renters in the public housing sector. We note that this estimate is given the assumption that the effect on public housing is equal to effect on private-rental housing, as measured in Appendix C.4.

improvements in their properties. In other words, the programme seems to have been effective in increasing the welfare of the poor.

The latter estimates probably serve as an upper bound of the external benefits of the programme, but we note that the estimates are still in line with Rossi-Hansberg et al. (2010), who also found substantial gain-to-funding ratios for an urban renewal project in Richmond, VA.

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 quality of the public housing stock. There is limited understanding to what extent such a place-based policy is effective and has external positive effects on nearby residents.

In the current paper we aim to estimate the external effects on nearby households in the owner-occupied market of a nationwide investment programme that improved the quality of public housing in the 80 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 on nearby property owners of the investment scheme. The programme has led to an increase in surrounding house prices of 3.5 percent and to temporary reductions in sales time that disappear after 7.5 years. We also find evidence for treatment heterogeneity: the effect is stronger in dense areas, likely because spillovers are more pronounced when properties are closer to each other. We calculated that the welfare benefits to property owners induced by the place-based policy programme are sizeable and at least half of the value of the expenditure on public housing. Moreover, public housing renters benefit from improvements in their properties and neighbourhood quality, while not paying a higher rent and are therefore better off. Hence, the programme has been effective in increasing the welfare of poor households.

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Online 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:

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

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.[34] 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:

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

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

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

(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 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 $p = \big((1-\psi)k + c\big)(1 + rs + 2s\phi)/\big(2rs(m + 2r + 4\phi)\big)$.[35] 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.

[35] See similarly Wheaton (1990).

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:

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

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:

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

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:

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

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.

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:

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

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

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

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 amenity level k, while according to (A12), sales times do not change.[37] 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.[38]

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[36] 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.

[37] 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.

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

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 w per household as:

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

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:

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

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

$$\frac{\mathrm{d}p}{\mathrm{d}k} = \frac{\mathrm{d}w}{\mathrm{d}k}\left(\frac{\bar{S}}{\bar{S}+v}\right) \approx \frac{\mathrm{d}w}{\mathrm{d}k}\left(1-\frac{v}{\bar{S}}\right). \tag{A18}$$

Hence, price changes are always smaller than welfare changes. 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.

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 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:

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

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):

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

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

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

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:

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

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:

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

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.[39] 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:

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

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

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.

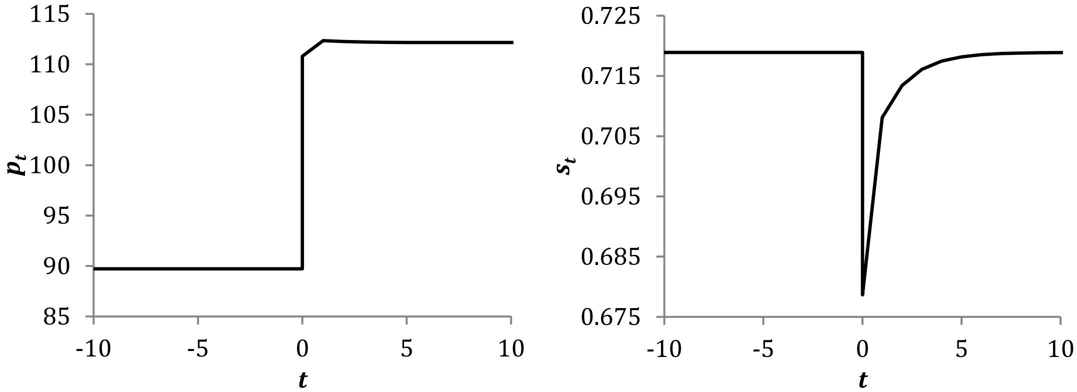
Panel A in Figure A1 shows the results for an unanticipated 25 percent increase in the amenity level. The long-run price increase is then exactly 25 percent. In the short run, prices jump almost immediately to the new steady state value after the amenity increase. Sales time immediately drops after the 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.

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

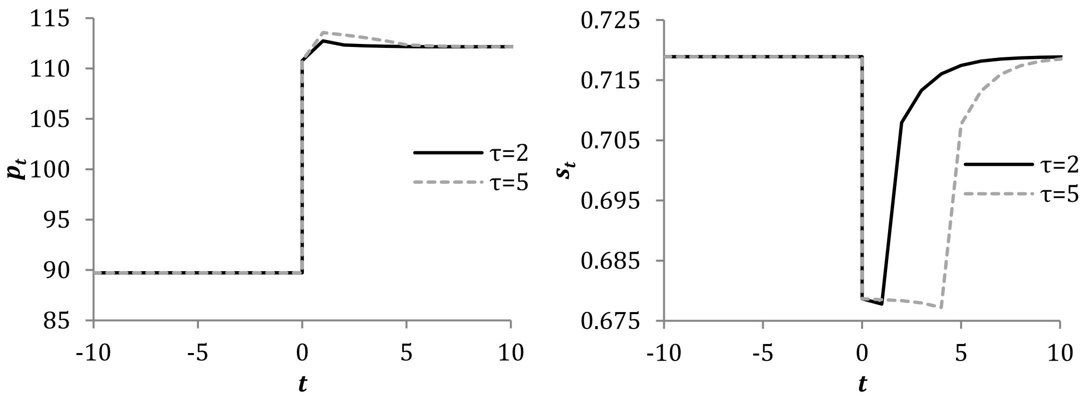
(iii) prices adjust quickly to the new steady-state value;

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

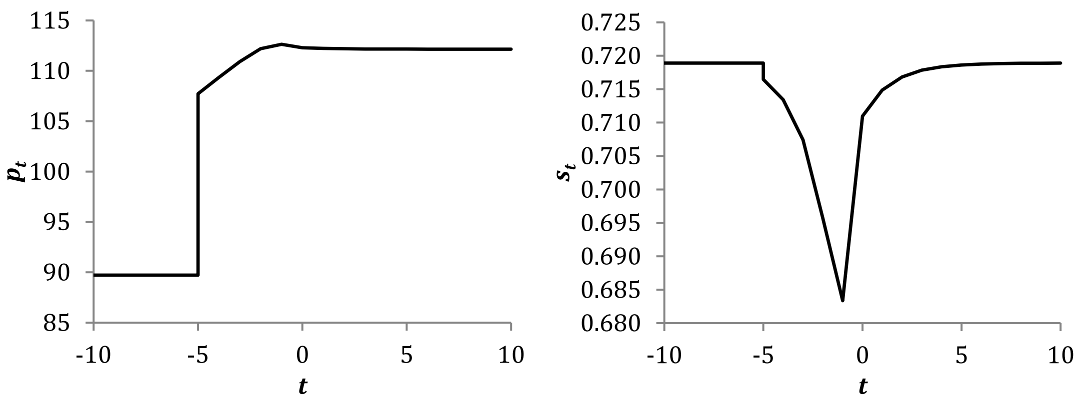
We do some sensitivity checks with respect to these two predictions. We assume that the time it takes for the search costs to become proportional to the amenity level is one year ($\tau = 1$). However, in practice it may take longer. Panel B in Figure A1 shows that there is some overshooting of prices 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 Panel C in Figure A1 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.



(A) BASELINE RESULTS



(B) RESULTS FOR THE SHORT-RUN WITH $\tau = 2$ AND $\tau = 5$



(C) RESULTS WITH ANTICIPATION EFFECTS

FIGURE A1 — SIMULATED PRICES AND SALES TIMES BEFORE AND AFTER TREAMENT

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 $m_t = \sqrt{e_t\, v_t/h_t^N}$.

Online Appendix B. Data and econometrics appendix

B.1 On z-scores and non-compliers

The deprivation z-score is the running variable in our regression-discontinuity design, as it determines whether a neighbourhood is eligible for treatment. In this data appendix we therefore describe in more detail how z-scores are determined and how neighbourhoods are selected. More details can be found in Brouwer and Willems (2007).

Three main issues discussed here are:

(i) What is a proper definition of a neighbourhood?

(ii) What aspects of a neighbourhood do contribute to deprivation and how can we measure those aspects?

(iii) What about non-complying neighbourhoods?

First, the definition of neighbourhoods is not obtained from Statistics Netherlands but based on postcode areas. In terms of size, postcode areas correspond to districts (*'wijken'*) in Statistics Netherlands. Postcode areas are used because they are not subject to (substantial) changes in their boundaries, while boundaries of neighbourhoods and districts in the definition used by Statistics Netherlands are subject to considerable changes over time, and are partly determined by municipalities. Moreover, a couple of datasets that are uses to calculate the z-score are only available at the postcode level. Importantly, all postcode areas with fewer than 100 inhabitants are excluded. Note that postcodes are fairly small: the average distance to the centroid of a neighbourhood is only 286 meter.

Second, what aspects should be included when construction a deprivation index and how can we measure them? There is a distinction made between four main categories: social deprivation, physical deprivation, social problems and physical problems. The variables that are included are listed in Table B1. For most indicators, data from Statistics Netherlands is used that is freely accessible. For the social problems and physical problems, Brouwer and Willems (2007) rely on the WoOn housing surveys, which we also use in this paper to estimate the effects of the KW-programme on rents. However, one should be aware that the number of respondents for each postcode area can be (very) small. Hence, this leads to a substantial variance in the estimated z-score. To reduce this variance, they rely on three waves: 1998, 2002 and 2006.

The first category – social deprivation – consists of income, employment and average educational level. For each variable they calculate the standardised z-score with unit standard deviation and zero mean. Then they take the mean of the z-scores for each indicator to get a z-score for social deprivation. A similar approach is followed for the second category – physical deprivation – using information on the housing stock. The last two categories – social and physical problems – are based on subjective perceptions of households living in a neighbourhood. Questions are asked about how they feel about vandalism, nuisance from

TABLE B1 — INDICATORS TO CALCULATE Z-SCORES

	Source	Year
Social deprivation		
Income	RIO, CBS	1998, 2002
Employment	RIO, CBS	1998, 2002
Education	Geo-marktprofiel	1998, 2002
Physical deprivation		
% Small properties (<60 m^2)	CBS, Syswov, CFV	1998, 2002, 2006
% Old properties (Construction year <1970)	CBS, Syswov, CFV	1998, 2002, 2006
% Cheap properties (< €125,000)	CBS, Syswov, CFV	1998, 2002, 2006
Social problems		
Vandalism – graffiti *(1-5 score)*	WoOn	1998, 2002, 2006
Vandalism – destruction *(1-5 score)*	WoOn	1998, 2002, 2006
Nuisance neighbours *(1-5 score)*	WoOn	1998, 2002, 2006
Nuisance residents *(1-5 score)*	WoOn	1998, 2002, 2006
Insecurity *(1-5 score)*	WoOn	1998, 2002, 2006
Physical problems		
Satisfaction property *(1-5 score)*	WoOn	1998, 2002, 2006
Satisfaction neighbourhood *(1-5 score)*	WoOn	1998, 2002, 2006
Inclination to move *(1-5 score)*	WoOn	1998, 2002, 2006
Noise pollution *(1-5 score)*	WoOn	1998, 2002, 2006
Air pollution *(1-5 score)*	WoOn	1998, 2002, 2006
Traffic nuisance *(1-5 score)*	WoOn	1998, 2002, 2006
Safety *(1-5 score)*	WoOn	1998, 2002, 2006

Notes: CBS = Statistics Netherlands, RIO = Regional Income Survey, Syswov = Housing stock data, CFV = Central Public Housing Fund, WoOn = Quadrennial housing survey

neighbours, pollution and inclination to move. Brouwer and Willems (2007) argue that those subjective feelings are important, although more objective data would have been preferable. However, at that time those data were not available. Each indicator is again standardised with mean zero and unit standard deviation. The mean of each z-score for each category is taken to get a z-score for social problems and physical problems.

To arrive at the final z-score, the z-score of each of the broad categories are added. In other words, the categories are weighted equally. Only neighbourhoods are considered that have a lower than average z-score for each category (hence: a z-score for each of the categories lower than zero).

The last issue that needs to be discussed is the final selection of neighbourhoods. We reiterate that it is clear from the above description that it was impossible to influence the z-score of a neighbourhood, which is supported by Figure 2: we do not find bunching around

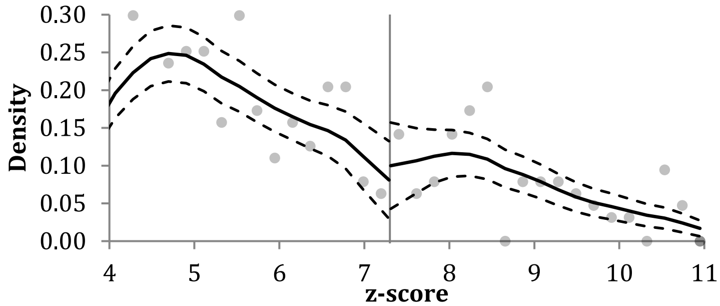


FIGURE B1 — MANIPULATION TEST FOR Z-SCORES WITH $h = 3.5$

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.

the threshold. We look at this more closely in Figure B1, where 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.

However, we find that twelve neighbourhoods were removed from the list, while two other neighbourhoods (in Amsterdam and Enschede) were added although they had z-scores below the threshold (respectively 6.84 and 5.00). The main reason is that some of the neighbourhoods with a sufficiently high z-scores did not had a lower than average z-score on each of the categories (four neighbourhoods). Eight other neighbourhoods, after discussion with local governments, were also removed. These were mainly downtown neighbourhoods for which the recorded nuisance is an innate feature of the neighbourhood because of e.g. retail and nightlife and entertainment activities. The local governments of Amsterdam and Enschede argued that one neighbourhood for which the z-score was sufficiently high (above 7.3) should be replaced by two neighbourhoods that were below the z-score because the latter neighbourhoods were known to experience more deprivation.

We re-emphasise here that non-complying neighbourhoods are not a threat to our identification strategy as long as we observe a jump in the probability to be treated at the threshold. This is strongly confirmed by Figure 1.

B.2 Other descriptive statistics for the housing sales data

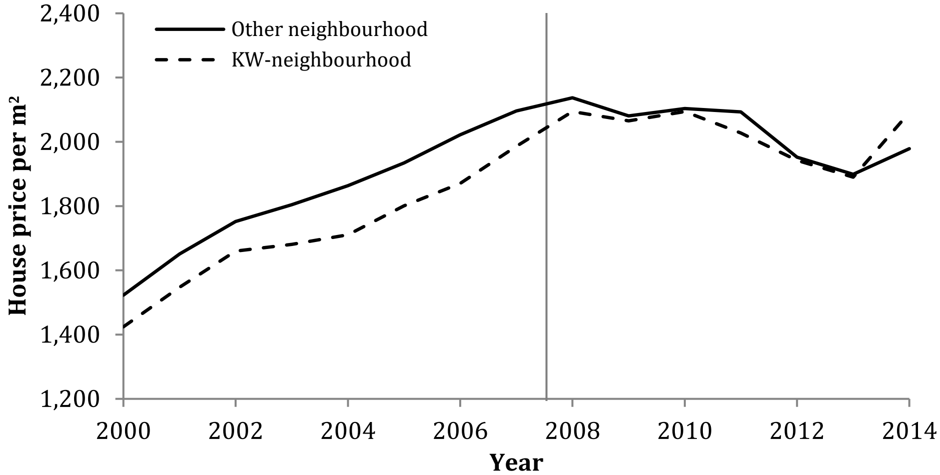
Table B2 reports the descriptive statistics for the repeated sales sample, which we discuss in more detail in Section III.C. In Figure B2 we plot the house price and sales time for KW and other neighbourhoods over time. Prices in KW-neighbourhoods were lower than in

TABLE B2 — DESCRIPTIVE STATISTICS FOR REPEATED SALES SAMPLE

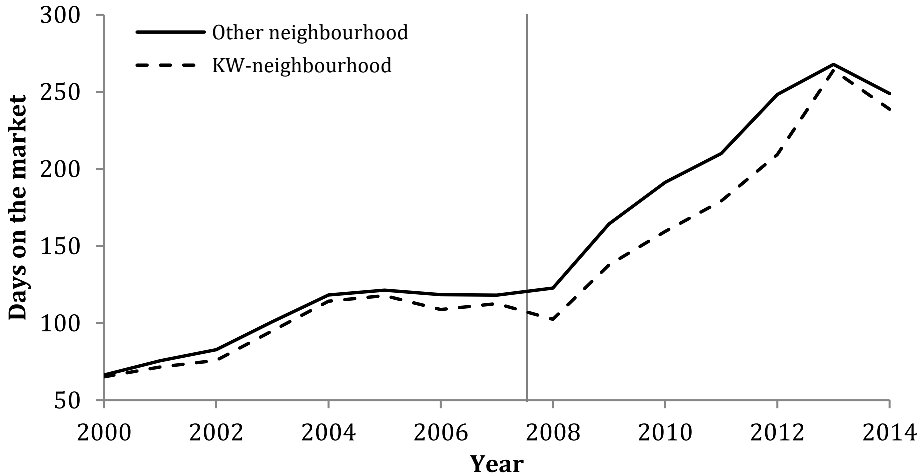
	Observations outside KW-neighbourhoods				Observations inside KW-neighbourhoods			
	μ	σ	Min	max	μ	σ	min	max
House price per m² *(in €)*	1,929	624.7	500	5,000	1,901	649.6	504.2	5,000
Days on the market	135.4	172.5	1	1,823	124.8	157.1	1	1,816
KW-investment received	0				0.427			
Deprivation z-score	0.530	2.890	-6.600	10.60	8.695	1.141	5	12.98
Size in m²	104.8	32.98	26	250	82.15	26.10	27	250
House type – apartment	0.412				0.823			
House type – terraced	0.321				0.131			
House type – semi-detached	0.212				0.0429			
House type – detached	0.0557				0.00280			
Garage	0.203				0.0485			
Garden	0.989				0.991			
Maintenance quality –good	0.905				0.873			
Central heating	0.928				0.875			
Listed	0.00560				0.00472			
Construction year <1945	0.257				0.365			
Construction year 1945-1960	0.0696				0.143			
Construction year 1961-1970	0.168				0.294			
Construction year 1971-1980	0.160				0.0378			
Construction year 1981-1990	0.146				0.0482			
Construction year 1991-2000	0.162				0.0814			
Construction year >2000	0.0380				0.0308			
Number of observations	461,489				20,353			

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 the reduction in the 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). Sales time for KW and non-KW neighbourhoods are pretty similar until 2007. After the investment, the sales time is much lower in KW-neighbourhoods than in other neighbourhoods. This difference seems to become somewhat smaller over time and disappears in 2013.

Table B3 reports the descriptive statistics for the full sample. We find that properties in KW-neighbourhoods are slightly 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 is that houses that are sold more than once tend to be somewhat smaller and more often come in the form of apartments. This is, most likely, because housing mobility in cities tends to be higher. Houses in cities are



HOUSE PRICE PER M^2



(B) DAYS ON THE MARKET

FIGURE B2 — HOUSE PRICES AND SALES TIMES INSIDE AND OUTSIDE KW-NEIGHBOURHOODS

also smaller and the share of apartments is higher.

Table B4 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 Appendix C.11). 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

TABLE B3 — 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 B4 — 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.

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.

B.3 Determining the bandwidth

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

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

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. $\widehat{m}_-^{(2)}$ and $\widehat{m}_+^{(2)}$ 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):

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

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.4 First-stage regression results

Table B5 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, indicating that results will be almost identical to a sharp regression-discontinuity design.

TABLE B5 — FIRST STAGE REGRESSION RESULTS
(Dependent variable: change in KW-investments)

	(1)	(2)	(3)
	FRD	FRD	FRD
Δ Score rule ($z > 7.30$)	0.977***	0.984***	0.969***
	(0.0141)	(0.0103)	(0.0190)
Control variables included (14)	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes
Number of observations	24,170	16,839	11,579
Number of clusters	176	285	184
First-stage R^2-within	0.961	0.961	0.953
Kleibergen-Paap F-statistic	4797	9191	2592
Bandwidth h^*	3.383	4.312	3.547

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
+ Significant at the 0.15 level

B.5 Data and descriptive statistics for rental housing

We gathered data on private and controlled rents based on the so-called *WoOn* housing surveys. We rely on five waves: 2002-2003, 2005-2006, 2008-2009, 2011-2012 and 2014-2015. Each wave consists of about 60 thousand respondents and is considered as a representative survey of the Dutch population. The surveys provide information on a wide range of housing characteristics, the (self-reported) rent, the size of the property, house type and whether the household has moved within the last two years. Because rents in the private sector can only be freely adjusted for new households, we focus on households that have moved within the last two years. The location is reported at the neighbourhood level. Because we do only know the location of a respondent at the neighbourhood level, we exclude all neighbourhoods that are adjacent to a treated neighbourhood.

We exclude properties that are constructed after 2006, which implies that we exclude 15 percent of the observations. In Table B6 we separately report descriptive statistics for non-controlled private rents and controlled rents, where the latter refer to public housing and controlled private rental observations. The average private rent is € 10 per square metre, while the controlled social rent is much lower (€ 6.2 per square metre).

In Figure B3 we report the trends of rents over the time, as well as the maximum rent threshold for units to become rent controlled. Importantly, we do not observe any decrease in rents after 2008, which is in stark contrast to the owner-occupied housing market.

TABLE B6 — DESCRIPTIVE STATISTICS FOR RENTAL HOUSING SAMPLE

	Observations in private rental sector				Observations in public housing sector			
	μ	σ	Min	max	μ	σ	min	max
Rent per m² *(in €)*	10.07	4.944	2.597	44.27	6.132	2.503	2.500	24.80
KW-investment received	0.0936	0.291	0	1	0.0863	0.281	0	1
Deprivation z-score	1.269	3.907	-6.230	12.98	2.142	3.846	-6.600	12.98
Size in m²	101.4	39.58	25	250	72.79	29.42	25	245
Rooms	3.840	1.299	1	12	3.274	1.179	1	20
House type – apartment	0.597	0.491	0	1	0.639	0.480	0	1
House type – terraced	0.207	0.405	0	1	0.215	0.411	0	1
House type – semi-detached	0.132	0.338	0	1	0.108	0.311	0	1
House type – detached	0.0506	0.219	0	1	0.0168	0.128	0	1
Floor	1.356	2.066	-1	20	1.346	1.948	-1	20
Number of stories building	3.475	3.106	1	42	3.188	2.641	1	66
Elevator	0.315	0.465	0	1	0.247	0.431	0	1
Garage	0.220	0.415	0	1	0.0597	0.237	0	1
Maintenance quality –good	0.724	0.447	0	1	0.675	0.468	0	1
Central heating	0.866	0.340	0	1	0.779	0.415	0	1
Construction year <1945	0.260	0.439	0	1	0.154	0.361	0	1
Construction year 1945-1960	0.0727	0.260	0	1	0.150	0.357	0	1
Construction year 1961-1970	0.143	0.350	0	1	0.254	0.435	0	1
Construction year 1971-1980	0.123	0.328	0	1	0.168	0.374	0	1
Construction year 1981-1990	0.116	0.321	0	1	0.144	0.351	0	1
Construction year 1991-2000	0.135	0.342	0	1	0.0819	0.274	0	1
Construction year >2000	0.150	0.357	0	1	0.0488	0.215	0	1
Number of observations	2,392				15,930			

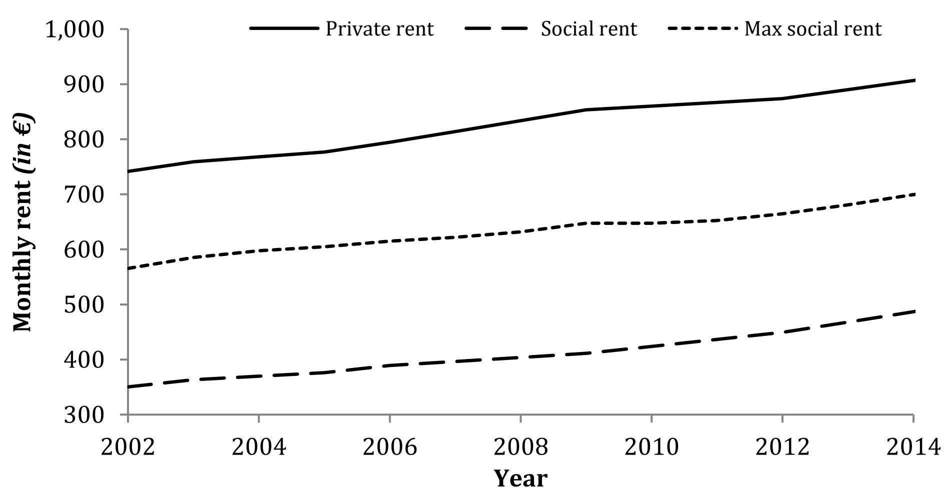


FIGURE B3 — PRIVATE AND RENT-CONTROLLED RENTS

The other descriptive statistics are comparable to the ones presented for the owner-occupied housing market except for house type. This likely is a result of different in house type definitions between the *NVM* data and *WoOn* surveys.

B.6 Changes in demographics and home ownership

In Table B7 we analyse the impact of the KW-investments on the demographic composition of KW-neighbourhoods. We analyse changes in income, population density, share of foreign population, share young people (<25 years), share elderly people (>65 years) and the average household size. The effects are estimated using the fuzzy-regression discontinuity design.

Column (1) shows that incomes have slightly decreased due to the policy investment (about 1.5 percent). This may be in line with Diamond and McQuade (2016), who show that the external effect of investments in public housing are mainly valued by poorer households. In column (2) we show that the KW-policy did not imply statistically significant changes in population density.

However, in column (3) 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 of 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 (4) 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 (column (5)). Also, it seems that the KW-policy has induced an increase in the average household size (column (6)). It seems that households are 0.036 persons larger than before (about one-tenth of a standard deviation).

Next, we investigate whether the KW-programme induced any changes in home-ownership shares. This may be important, as (part of) the treatment effect may be due to changes in home-ownership rates (through selling of public housing), rather than by improvements in the quality of public housing. We then use survey data from the WoOn surveys to test this in detail. The WoOn survey provide data for the years 2002, 2006, 2009, 2012 and 2015. We then employ a similar approach to estimate the treatment effect as in the previous specifications. Column (1) in Table B8 shows that the share of public housing indeed has decreased by 2.3 percentage point (about one-tenth of a standard deviation), so the effect seems rather small. The increase in the share of owner-occupied housing is of about the same magnitude (see column (3)).

TABLE B7 — REGRESSION RESULTS: CHANGES IN DEMOGRAPHICS

	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Dependent variable:	*Income* (log)	*Population density* (log)	*Share foreigners*	*Share young people*	*Share elderly people*	*Household size*
KW-investment	-0.0149**	-0.00406	0.0211***	-0.00442⁺	-0.0111***	0.0363***
	(0.00599)	(0.0103)	(0.00610)	(0.00280)	(0.00365)	(0.00921)
Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	6328	3766	3542	2240	2240	3920
Number of clusters	452	269	253	160	160	280
Kleibergen-Paap F-statistic	852.1	715	706	509	509	737
Bandwidth h^*	5.499	3.897	3.657	2.666	2.684	4.043

Notes: We exclude neighbourhoods adjacent to treated neighbourhoods. 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
⁺ Significant at the 0.15 level

TABLE B8 — REGRESSION RESULTS: CHANGES IN HOME OWNERSHIP

	(1)	(2)	(3)	(4)
	FRD	FRD	FRD	FRD
Dependent variable:	*Share public housing*	*Share public housing (weighted)*	*Share owner-occupied housing*	*Share owner-occupied housing (weighted)*
KW-investment	-0.0233*	-0.0281**	0.0196*	0.0337**
	(0.0126)	(0.0131)	(0.0119)	(0.0132)
Year fixed effects (14)	Yes	Yes	Yes	Yes
Neighbourhood fixed effects	Yes	Yes	Yes	Yes
Number of observations	2,355	2,450	1,725	1,682
Number of clusters	605	631	440	429
Kleibergen-Paap F-statistic	3234	3243	3125	3112
Bandwidth h^*	5.499	3.897	3.657	2.666

Notes: We exclude neighbourhoods adjacent to treated neighbourhoods. 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
⁺ Significant at the 0.15 level

One may argue that the survey data may not be representative when one is interested in the stock of housing. We therefore use so-called representation weights for each households. Those weights are used to scale up the individual observations to the full population at the neighbourhood level. The results in columns (2) and (4) in Table B8 show that this makes little difference. The effect on the share of public housing is very similar, while the effect on the share of owner-occupied housing seems to be slightly stronger.

Despite the changes in ownership rates being rather small, we will directly control for changes in the share of private rental and owner-occupied housing in Appendix C.7, to examine whether part of the treatment effect can be explained by induced changes in ownership rates. We do not find any evidence that this is the case.

Online Appendix C. Sensitivity analysis

C.1 Sales time – baseline results

In most empirical analyses, the effects of sales time are ignored. However, we hypothesised and showed that sales time effects may be present in the short run, because it takes time for the market to adjust to a new steady state. In this section we report the baseline results for sales times in Table C1, where we include a linear interaction effect of years after the investment with the treatment.

In column (1) we start again with a naïve regression of the change in the logarithm of days on the market and the effect of years after the investment on whether a property has experienced a change in the treatment status. This specification suggests that sales time has been reduced instantaneously with 23 percent due to the investment. In line with theoretical considerations, the effect becomes less pronounced over time: every year the effect decreases with 4.4 percentage points. If we control for housing attributes in column (2), the coefficients are essentially the same. In column (3) we employ the sharp regression-discontinuity design and exclude non-complying neighbourhoods (non-KW-neighbourhoods with a z-score above

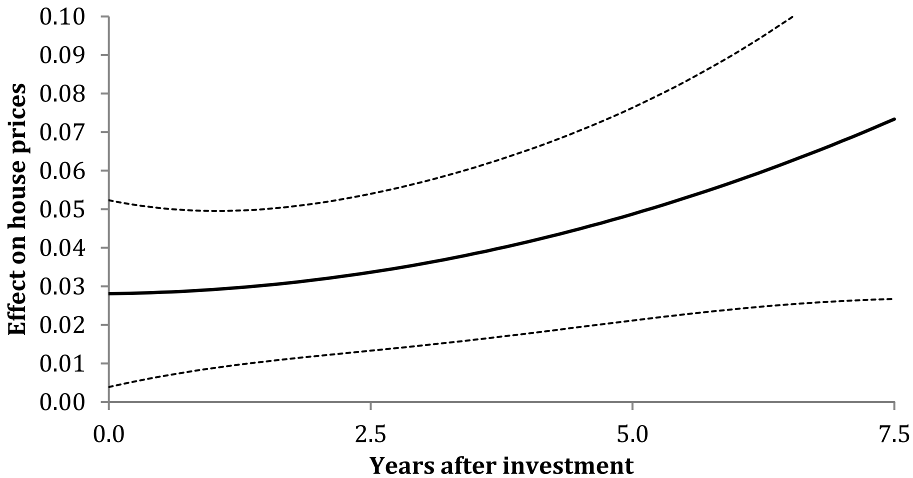
TABLE C1 — REGRESSION RESULTS: THE EFFECT OF PLACE-BASED POLICIES ON SALES TIME

(Dependent variable: change in log days on the market)

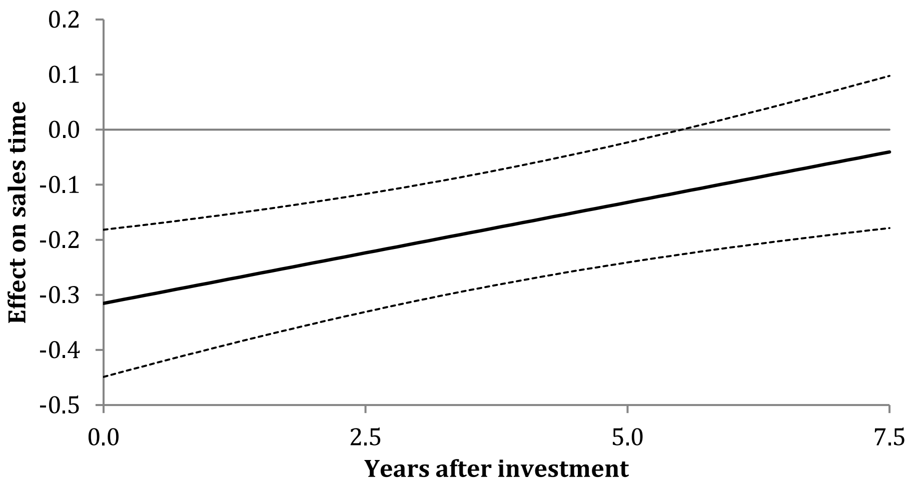
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	SRD	FRD	FRD	FRD
Δ KW-investment	-0.266***	-0.268***	-0.322***	-0.315***	-0.336***	-0.315***
	(0.0636)	(0.0633)	(0.0707)	(0.0682)	(0.0854)	(0.0855)
Δ (KW-investment × years after investment)	0.0434***	0.0439***	0.0414***	0.0366***	0.0351**	0.0440***
	(0.0109)	(0.0108)	(0.0123)	(0.0119)	(0.0168)	(0.0165)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighbourhood characteristics (10)	No	No	No	No	No	Yes
Δ Land use variables (4)	No	No	No	No	No	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	185,072	185,072	38,834	57,651	25,926	58,844
Number of clusters	3138	3138	355	545	525	2126
R^2-within	0.057	0.058	0.062			
Kleibergen-Paap F-statistic				5252	3994	3969
Bandwidth h^*			5.210	6.219	6.221	8.973

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
+ Significant at the 0.15 level



(A) EFFECT ON HOUSE PRICES



(B) EFFECT ON SALES TIME

FIGURE C1 — EFFECT ON 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.

the threshold and KW-neighbourhoods with a z-score below the threshold). The effect then becomes slightly stronger (the instantaneous effect is −27.5 percent, while the reduction in this effect is 4.2 percent per year). Next, we do not exclude neighbourhoods but use an instrumental variables approach instead, with the change in the scoring rule as the instrument. The fuzzy regression-discontinuity design leads to similar second stage results: column (4) in Table C1 suggests that the investment has led to an instantaneous 27 percent decrease in sales time, while the reduction in this effect is 3.7 percent per year. 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. Also if we control for changes in demographics, including income and population density, the results in column (6) confirm the previous results.

C.2 Controlling and instrumenting for neighbourhood characteristics

In the preferred specification in column (6) in Table 2 we control for neighbourhood and land use characteristics to show that the price effect is essentially unaltered – or at least is not statistically significantly lower – once we control for changes in demographic characteristics and land use. This strongly suggests that most of the price effect is due to an external effect of improved quality of public housing, rather than an indirect effect via a change in the composition of treated neighbourhoods.

In a standard hedonic regression, changes in neighbourhood demographics are usually endogenous. However, because of our research design, this should not be the case as changes in neighbourhood demographics close to the threshold should be (almost) identical in absence of the programme. Hence, observed changes in demographics are then likely induced by the KW-programme. If one is interested in the *total* effect of the programme, controlling for demographics may be an example of including 'bad controls' because these variables may absorb part of the effect one is interested in. However, if one is interested in the effect of the KW-investments, *conditional on induced sorting effects* (as is the case in column (6), Table 2), controlling for changes in neighbourhood demographics is a valid approach.

We explore this issue further in Table C2 where we first investigate whether the reduction in the effect of the KW-investment is mainly due to changes in land use or due to changes in demographic composition of the neighbourhood. In column (1) we show that the KW-investment implies a price increase of 3.8 percent if we do not control for changes in land use, which is 0.5 percent point lower than the baseline estimate. Column (2) shows that once excluding neighbourhood demographics, the coefficient is close to the baseline estimate. Hence, the effect of the KW-investment is unlikely to be explained by changes in land use. This is also confirmed when looking at the results for sales time in columns (5) and (6).

Gerritsen et al. (2017) provide some evidence that the share foreigners (in levels) is discrete at the z-score threshold. One may then suspect that the change in the share of foreigners is then endogenous if the change in the share foreigners is correlated to the level (see e.g. Card et al. 2008). We therefore aim to instrument for the change in the share of foreigners with the 'predicted' share of foreigners, using a Bartik (1991) shift-share approach. The instrument is the predicted level of foreigners in each neighbourhood in each year using the initial number of foreigners in the first year for which we have data (1998) and national changes in the share of foreigners. Hence, we use exogenous changes from the local

TABLE C2 — REGRESSION RESULTS: NEIGHBOURHOOD CONTROL VARIABLES AND SHIFT-SHARE INSTRUMENTS

	Panel 1: Δ Price per m² *(log)*					*Panel 2:* Δ Days on the market *(log)*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	FRD	FRD	2SLS	2SLS	2SLS	OLS	SRD	2SLS	2SLS	2SLS
Δ KW-investment	0.0375***	0.0411***	0.0377***	0.0449***	0.0428***	-0.292***	-0.353***	-0.290***	-0.346***	-0.293***
	(0.0116)	(0.0130)	(0.0112)	(0.0137)	(0.0140)	(0.0857)	(0.0864)	(0.0921)	(0.0888)	(0.0952)
Δ (KW-investment ×						0.0403**	0.0391**	0.0358**	0.0242+	0.0103
years after investment)						(0.0162)	(0.0172)	(0.0169)	(0.0168)	(0.0178)
Δ Neighbourhood income *(log)*	0.0513		0.0435	0.0362	0.0312	-0.0954		-0.426*	-0.120	-0.362+
	(0.0667)		(0.0677)	(0.0801)	(0.0740)	(0.172)		(0.234)	(0.190)	(0.226)
Δ Population density *(log)*	0.0393		0.0372	0.0958	0.100	-0.145+		-0.216	-0.417***	-0.390**
	(0.0783)		(0.0868)	(0.105)	(0.129)	(0.0992)		(0.153)	(0.158)	(0.180)
Δ Share foreigners	-1.067***		-1.151***	-0.903***	-1.043***	0.169		-1.658	-0.379	-3.508**
	(0.142)		(0.332)	(0.194)	(0.383)	(0.614)		(1.398)	(0.705)	(1.432)
Δ Share young people	0.106		0.238	0.311	0.317	0.154		0.617	-1.295	-0.257
	(0.475)		(0.529)	(0.629)	(0.595)	(0.874)		(1.252)	(1.005)	(1.230)
Δ Share elderly people	-0.797***		-0.809**	0.144	-0.208	-0.473		-0.515	-5.480***	-6.390***
	(0.283)		(0.350)	(0.698)	(0.930)	(0.551)		(0.689)	(1.669)	(1.899)
Δ Average household size	0.0656		0.0658	0.0626	0.0702	-0.161		-0.362	-0.457+	-0.859**
	(0.111)		(0.106)	(0.134)	(0.123)	(0.222)		(0.314)	(0.282)	(0.358)
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Δ Land use variables (4)	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Number of observations	12,501	13,029	12,714	9,573	10,676	69,294	22,544	45,600	60,219	50,718
Number of clusters	208	220	207	149	163	2443	448	1440	2164	1662
Kleibergen-Paap *F*-statistic	2539	6425	17.53	27.43	4.488	4128	3721	87.98	53.20	29.07
Bandwidth h^*	3.666	3.871	3.709	3.012	3.161	9.729	5.928	8.173	9.071	8.618

Notes: We exclude observations within 2.5 kilometres of targeted areas. In Columns (1)-(10) the change in KW-investment is instrumented with the change in the eligibility based on the scoring rule. In columns (3), (5), (8) and (10), we instrument the share of foreigners with the predicted share of foreigners based on the shift-share approach (see Bartik, 1991). Similarly, in columns (4), (5), (9) and (10), we instrument the share of elderly people with the predicted share of elderly people. 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
+ Significant at the 0.15 level

perspective in the growth of the share of foreigners to predict the local share of foreigners in each year. The results show that, although the Kleibergen-Paap *F*-statistic is much lower (because we now have two endogenous variables) it is still above the rule-of-thumb value of 10. What is more important, in columns (3), it is shown that the impact of the KW-programme is comparable to column (6) in in Table 2 and Table C1. Further, the impact of foreigners is very similar to the estimates where we do not instrument for the change in the share of foreigners, confirming that the change in the share of foreigners is unlikely to be endogenous. The coefficient suggests that a 10 percent increase in the share of foreigners (about one standard deviation) decreases house prices by 12 percent, which is substantial. The price effect of place-based investments becomes somewhat larger in magnitude and is comparable to the specification where we do not control for land use or neighbourhood demographics (see column (5), Table 2). This makes sense as the observed variation in share foreigners caused by the KW-programme is uncorrelated to the shift-share instrument.

When having a closer look at the results in column (6), Table 2, we also observe that a change in the share of elderly people has a substantial price effect. To be sure, we instrument for the change in the share of elderly people with the 'Bartik' instrument based on national changes in the age distribution. The effect of the share of elderly people now becomes statistically insignificant. The price effect of place-based investments in column (4), Table C2, is again comparable to the specification where we do not control for land use or neighbourhood demographics (see column (5), Table 2). This is also the case when we instrument both for the share of foreigners and share of elderly people in column (5) of Table C2. However, we should be careful in interpreting the latter result, as the Kleibergen-Paap *F*-statistic is rather low. These results are largely the same for sales times (see columns (8), (9) and (10)), although the effects becomes somewhat imprecise in the last column, likely because of the low first-stage *F*-statistic.

Hence, these additional robustness checks do not invalidate the conclusion that most of the price and sales time effects we obtain are due to the external effect of improved quality of public housing, rather than due to sorting of households of a specific type into treated neighbourhoods.

C.3 Heterogeneity

In this subsection we analyse treatment heterogeneity further by including interactions of demographic variables with the treatment dummy one by one. Table C3 reports the results for prices. In line with the baseline result as shown in Table 4, we do not find any effect of income in column (1). Hence, it is unlikely that the programme is correlated with gentrification. Moreover, in contrast to Diamond and McQuade (2016) we do not find evidence that only poorer households value external benefits of place-based investments in public housing. Column (2) confirms that KW-investments have been more effective in dense

TABLE C3 — HETEROGENEITY IN THE ESTIMATED EFFECTS, SENSITIVITY
(Dependent variable: change in log house price per square meter)

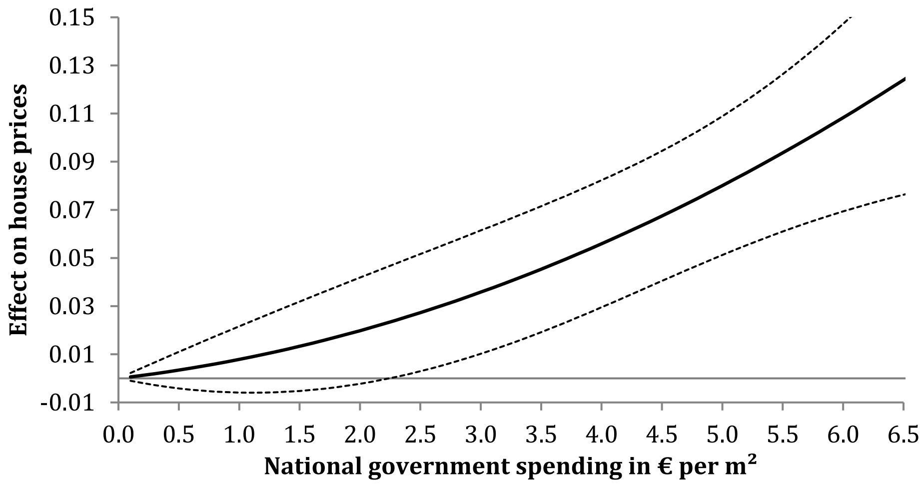
	(1)	(2)	(3)	(4)	(5)	(6)
	FRD	FRD	FRD	FRD	FRD	FRD
Δ KW-investment	0.0298**	0.0135	0.0325**	0.0347***	0.0278**	0.0275**
	(0.0149)	(0.0116)	(0.0141)	(0.0115)	(0.0114)	(0.0113)
Δ (KW-investment ×	-0.0286					
Neighbourhood income *(log)*)	(0.0495)					
Δ (KW-investment ×		0.0569***				
Population density *(log)*)		(0.0105)				
Δ (KW-investment ×			0.00692			
Share foreigners)			(0.0586)			
Δ (KW-investment ×				-0.365**		
Share young people)				(0.150)		
Δ (KW-investment ×					-0.260*	
Share elderly people)					(0.141)	
Δ (KW-investment ×						-0.0999***
Average household size)						(0.0328)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighbourhood characteristics (6)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Land use variables (4)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	185,072	185,072	38,834	57,651	25,926	58,844
Number of clusters	3138	3138	355	545	525	2126
R^2-within	0.057	0.058	0.062			
Kleibergen-Paap F-statistic				5252	3994	3969
Bandwidth h^*			5.210	6.219	6.221	8.973

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 and in parentheses.

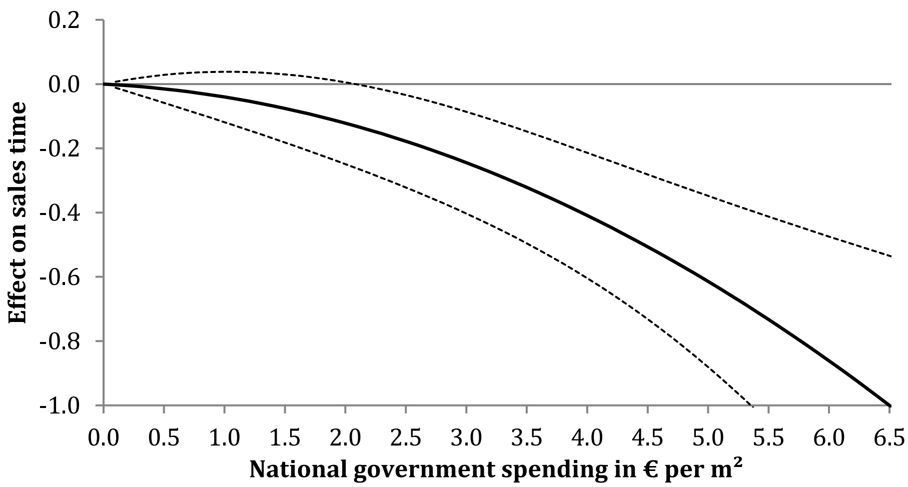
*** Significant at the 0.01 level
** Significant at the 0.05 level
* Significant at the 0.10 level
+ Significant at the 0.15 level

areas. The coefficient of the interaction term implies that doubling population density leads to an increase in the treatment effect of 3 percentage points, which is substantial. In a neighbourhood with an average density, we do not find a statistically significant positive effect. Again, because spillovers decay over space, we would expect to find stronger effects in denser areas because properties are located closer together.

For the other interactions with demographic variables (columns (3)-(6)), we do not find such important effects, although the coefficients for share young people, elderly people and



(A) EFFECT ON HOUSE PRICES



(B) INSTANTANEOUS EFFECT ON SALES TIME

FIGURE C2 — EFFECT ON HOUSE PRICES AND SALES TIME FOR DIFFERENT SPENDING INTENSITIES

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.

household size are statistically significant. This is likely because of a correlation with population density, as denser areas disproportionally attract younger people and smaller households.

Below we add the implied effect on prices and sales times based on columns (4) and (6) in Table 4 respectively where we investigate the effect of treatment intensity. We find that neighbourhoods which have received more funding per square metre also have a higher treatment effect. For example, the treatment effect for the average spending (€ 2.11 per m^2 neighbourhood area) is 2.1 percent (significant at the ten percent level), similar to the baseline estimate albeit a bit lower. For the neighbourhood that received the most spending (€ 6.83 per m^2 neighbourhood area) we find a treatment effect of 13.4 percent. We note, however, that the confidence intervals are quite large. The effects on sales times are similar: it seems that for areas that have received more funding, the instantaneous sales time effect is (much) stronger.

C.4 Analysis using housing rents

It would seem also natural to study non-regulated rental prices than housing values, as we are interested in amenity improvements. In contrast to housing prices, rents do not take into account expectations about potential future public investments in the targeted neighbourhood. House prices take into account the expectation that future public investments will be made again, the treatment effect may easily be overestimated. However, given a discontinuity regression setup, it is equally plausible that neighbourhoods that are just below the threshold have a higher probability of receiving future public housing investments, because they are 'the next in line'. In the latter case, the treatment effect will be underestimated. Whatever the sign of the bias, we believe that the bias will be small because programmes of this size are rare in the Netherlands: it is unlikely that homeowners expect more investments in public housing in the (far) future.

We already noted that about 90 percent of the rental housing stock refers to public housing and is rent-controlled, so that rents cannot freely adjust to a new situation. Given an owner-occupied housing stock of 60 percent, the private rental housing stock is small. Nevertheless, we gathered additional data on private and controlled rents based on the so-called *WoOn* housing surveys. We make use of five waves: 2002-2003, 2005-2006, 2008-2009, 2011-2012 and 2014-2015. Each wave consists of about 60 thousand respondents and is considered as a representative survey of the Dutch population. The surveys provide information on a wide range of housing characteristics, the (self-reported) rent, and whether the household has moved within the last two years. Because existing rental contracts are not easy to adjust in the Netherlands, we focus on people that have moved within the last two years. Furthermore, to exclude the possibility that we measure direct effects from the policy via properties that are constructed after 2007, we exclude all properties that are constructed in the treatment period. The location is reported at the neighbourhood level.[40] More information and descriptive statistics are provided in Appendix B.4.

We estimate fixed effects regressions of the form:

$$\log r_{\ell t} = \alpha k_{\ell t} + \beta x_{\ell t} + \upsilon_t + \phi_\ell + \epsilon_{\ell t}, \tag{3}$$

where $r_{\ell t}$ is the housing rent per square meter, $k_{\ell t}$ is the treatment variable that equals one

[40] Because we do only know the location of a respondent at the neighbourhood level, we exclude all neighbourhoods that are adjacent to treated neighbourhoods.

TABLE C4 — REGRESSION RESULTS: RENTAL HOUSING

(Dependent variable: log rent per square meter)

	Panel 1: Private rental sector					*Panel 2:* Public housing sector				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	SRD	FRD	FRD	FRD	OLS	SRD	FRD	FRD	FRD
Δ KW-investment	0.0695**	0.0684*	0.0888*	0.103**		0.00908	0.00538	0.00381	-0.0125	
	(0.0352)	(0.0411)	(0.0459)	(0.0494)		(0.0133)	(0.0148)	(0.0154)	(0.0166)	
Δ KW-investment × I(2008-2009)					0.0927+					-0.0239
					(0.0607)					(0.0178)
Δ KW-investment × I(2011-2012)					0.0697					-0.00125
					(0.0538)					(0.0236)
Δ KW-investment × I(2014-2015)					0.132**					0.00590
					(0.0555)					(0.0258)
Housing characteristics (17)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood characteristics (6)	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Land use variables (4)	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Year fixed effects (10)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,927	1,086	1,024	1,411	1,413	15,415	8,167	8,340	8,328	8,356
Number of clusters	478	250	234	342	343	1,686	516	526	525	528
R^2	0.563	0.644				0.560	0.509			
Kleibergen-Paap F-statistic			302.9	319.8	86.75			1823	1789	438.8
Bandwidth h^*		7.384	6.824	8.850	8.890		6.324	6.325	6.323	6.350

Notes: The instruments are Scoring rule for columns (3), (4), (8) and (9). In columns (4) and (8) we also include interactions of the scoring rule with a dummy for each survey wave. 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
+ Significant at the 0.15 level

when a property is in a treated neighbourhood after 2007, υ_t are year fixed effects and ϕ_ℓ are neighbourhood fixed effects. Again, we employ a fuzzy regression-discontinuity design based on the z-score to determine a causal effect of the treatment.

The main results are reported in Table C4. In the first five columns we focus on the effect of the policy on private rents. As a kind of placebo check, we also investigate what happens to rents in rent-controlled public housing in the next five columns of Table C4. If we would finda correlation, this might be a sign that there may be correlation with unobservable endowments (such as a change in the characteristics of the houses on offer).

Column (1) is a standard OLS with neighbourhood fixed effects. The coefficient indicates that there is a strong and positive effect of the place-based investment on rents; it appears that the rents have increased by 7.2 percent. The point estimate is a bit higher than the effect on house prices. Because of a much lower number treated observations (we observe 214 treated observations) than in the housing price sample, the confidence interval is much wider. Hence, the estimated effect on house prices falls well into the confidence interval. In column (2) we employ a sharp regression-discontinuity design by excluding non-complying neighbourhoods. We find an optimal bandwidth of 7.384, which implies that we exclude about 45 percent of the observations. The point estimate is then very similar and is marginally statistically significant. Column (3) displays the results of the fuzzy-regression discontinuity design where we instrument the treatment dummy with a dummy indicating whether a neighbourhood has a z-score of above 7.3 after 2007 (when the programme started). The coefficient is then slightly stronger: the KW-programme seems to have led to a rent increase of 9.2 percent.

In column (4) we further improve on these results by controlling for neighbourhood and land use characteristics, such as the average income, the population density and demographics. The effect of rents is very similar to the previous specification and even slightly stronger. In contrast to house prices, rents are (usually) not forward looking. We therefore expect that any amenity effect of place-based investments should capitalise directly and permanently into housing rents. This is indeed what we observe once we interact the treatment effect with a dummy indicating the year of the survey in column (5). Although the coefficients in the first two waves are just not statistically significant at conventional levels it seems that the policy indeed has had an immediate and permanent effect on housing rents.

In the second set of specifications we replace the dependent variable by the rents in the public housing sector, which are (strongly) rent-controlled and bear little relationship with underlying housing and neighbourhood characteristics (Van Ommeren and Van der Vlist, 2016). Hence, despite potential quality improvements, we do not expect rents to be changed significantly. That is what we observe in columns (6)-(10) in Table C4: there is no effect of the place-based investments on rents in the public housing sector. However, rents of public housing units are just very much unrelated to (changes in) underlying housing characteristics. In a way, this is good news for the relatively deprived households that rent in the public housing sector: they did not have to pay higher rents, but likely enjoy the benefits of internal improvements, as well as an improved quality of the neighbourhood.

C.5 Quasi-placebo experiments

We now conduct a series of quasi-'placebo' experiments using different classifications of deprived neighbourhoods used in the past, and differences in timing of programmes to test whether the effect we found is attributable to the KW-investment programme. Table C5 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 instantaneously 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 C5 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 small. 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

TABLE C5 — SENSITIVITY ANALYSIS: QUASI-PLACEBO EXPERIMENTS

	Panel 1: Δ Price per m² *(log)*			*Panel 2:* Δ Days on the market *(log)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Δ Winsemius neighbourhood	-0.00621			0.183***		
	(0.00558)			(0.0485)		
Δ (Winsemius neighbourhood × years after investment)				0.00113		
				(0.0101)		
Δ Kamp neighbourhood		0.000367			-0.00670	
		(0.00603)			(0.0718)	
Δ (Kamp neighbourhood × years after investment)					0.00393	
					(0.0146)	
Δ GSB neighbourhood			0.0131			-0.112
			(0.0105)			(0.124)
Δ (GSB neighbourhood × years after investment)						0.0274
						(0.0197)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	108,184	65,072	147,313	108,184	65,072	147,313
Number of clusters	2439	2560	2687	2439	2560	2687
R^2-within	0.531	0.430	0.528	0.059	0.063	0.056

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

neighbourhoods. One critique was that the z-scores were arbitrarily determined. For our last placebo check, we therefore use alternative so-called GSB-scores. GSB-scores were used in policies to determine deprived neighbourhoods in and around 1994 by the secretary of state at that time, Roger van Boxtel. These scores were updated in 2006 based on the *WoOn* housing survey. GSB-scores are between 0 and 10 and calculated for three categories: disintegration, depletion and nuisance. Similar to the calculation of the z-scores, we standardise those scores with mean zero and unit standard deviation and add them up to get the alternative z-scores. To illustrate that there is some arbitrariness in the calculation of the z-scores: there is a correlation of 0.505 of the GSB scores with the baseline z-scores. We then focus on the 83 neighbourhoods that would have been selected given these alternative scores. Similar to the previous placebo checks, we exclude observations in KW-neighbourhoods and observations within 2.5 kilometres of either a GSB or KW-neighbourhood. The results show that there is neither a price effect of being in a GSB-neighbourhood, nor an effect on sales times, suggesting that there is no general price trend that is correlated to an alternative proxy of deprivation.

TABLE C6 — SENSITIVITY ANALYSIS: UNOBSERVED 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	0.0412***	0.0430***	0.0453***	-0.316***	-0.277***	-0.293
	(0.0128)	(0.0116)	(0.0172)	(0.0678)	(0.0665)	(0.396)
Δ (KW-investment × years after investment)				0.0372***	0.0541***	0.0247
				(0.0118)	(0.0146)	(0.0598)
Distance to city centre *(log)*	-0.00157**			-0.000252		
	(0.000717)			(0.00124)		
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	20,736	18,044	10,137	60,073	37,938	12,233
Number of clusters	153	133	379	584	306	477
Kleibergen-Paap *F*-statistic	3545	621.3	20471	5344	586.5	2208
Bandwidth *h*	2.998	2.626	5.505	6.330	4.757	6.085

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

C.6 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 have 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. Although controlling for income does not change the results, it might be that neighbourhood income is an imperfect proxy for gentrification and ensuing amenities. In columns (1) and (4) of Table C6 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: a ten kilometre increase in distance to the city centre leads to a price decrease of 1.6 percent, so the effect is not large. For sales times the effect is statistically insignificant. Reassuringly, the treatment effects are essentially unaffected.

We investigate this issue further by including a third-order polynomial of distance to the city centre, implying that we control very flexibly for distance to the city centre. Another concern is that there are municipality-specific trends that play a role in explaining the positive price effects and negative sales time effects. Of course, when our identification strategy is valid, this should not make any difference. Indeed, when we include 455 municipality dummies in addition to a polynomial of distance to the city centre in columns (2) and (5), the results are essentially unchanged.

One may argue that the year of implementation is about at the peak of house prices. Mean reversion would then imply that prices of KW and non-KW neighbourhoods converge. As a first check we therefore exclude transactions three years before and three years after the peak, implying that we exclude transactions between 2005 and 2011. The results reported in columns (3) and (6) of Table C6 are very much in line with the baseline results for prices, despite the lower number of observations. For sales times, the point estimates are very similar, but imprecise because we exclude so many observations.

C.7 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 effects on house prices 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). When allowing for spatial spillovers we need 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 treated neighbourhood. So, we count the number of treated areas within 500 metre 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 to more than one KW-neighbourhood in column (1) of Table C7. It is shown that the treatment effects (within KW-neighbourhoods) are 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 column (2) we repeat the previous specification, but now using the fuzzy design, 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 C7 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 bandwidth 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.

TABLE C7 — SENSITIVITY ANALYSIS: SPATIAL SPILLOVERS
(Dependent variable: change in log house price per square meter)

	(1)	(2)	(3)
	SRD	FRD	FRD
Δ KW-investment	0.0375***	0.0351***	0.0318**
	(0.0112)	(0.0118)	(0.0151)
Δ Number of KW neighbourhoods, <0.5km	0.0147⁺	0.0123	-0.000438
	(0.00927)	(0.0113)	(0.00711)
Δ Number of KW neighbourhoods, 0.5-1.0km	0.00782	0.00504	0.000770
	(0.00850)	(0.0103)	(0.00523)
Δ Number of KW neighbourhoods, 1.0-1.5km	0.00148	-0.00210	0.0183***
	(0.0110)	(0.0133)	(0.00438)
Δ Number of KW neighbourhoods, 1.5-2.0km	0.00655	0.0135	0.0189***
	(0.0144)	(0.0167)	(0.00463)
Δ Number of KW neighbourhoods, 2.0-2.5km	0.00111	-0.00142	0.00404
	(0.00967)	(0.0110)	(0.00811)
Δ Housing characteristics (5)	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes
Number of observations	51,239	39,918	28,156
Number of clusters	500	355	204
R^2-within	0.537		
Kleibergen-Paap F-statistic		3582	193.8
Bandwidth h	5.260	4.059	2.521

Notes: In columns (2) and (3) we instrument the change in the treatment status with the change in the eligibility status. We construct similar instruments for the distance band variables by counting the change in the number of neighbourhoods in a specific band that have a z-score of at least 7.3. 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
⁺ Significant at the 0.15 level

C.8 Controlling for changes in home-ownership rates

In this subsection we test whether changes in home ownership affect the magnitude of the treatment effect. This is interesting because the KW-programme not only affects the quality of public housing but also may change the share of owner-occupied housing. Indeed, we find evidence in Appendix B.6 that the share of owner-occupied housing has slightly increased at the expense of public housing. In line with Diamond and McQuade (2016) this might have a positive effect on prices. We re-iterate that we rely on housing survey data from 2002, 2006, 2009 and 2012 to construct the share of public rental, private rental and owner-occupied housing. Hence, because we only include those years, the sample of housing transactions is substantially smaller.

TABLE C8 — SENSITIVITY ANALYSIS: CHANGES IN HOME OWNERSHIP

	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	0.0354***	0.0344***	0.0343***	-0.628**	-0.625**	-0.615**
	(0.0105)	(0.0105)	(0.0104)	(0.255)	(0.259)	(0.258)
Δ (KW-investment ×				0.119*	0.120*	0.117*
years after investment)				(0.0675)	(0.0685)	(0.0681)
Δ Share private rental housing		0.0591⁺	0.0804**		-0.429	-0.197
		(0.0385)	(0.0360)		(0.482)	(0.419)
Δ Share owner-occupied housing		0.0321	0.0290		-0.201	-0.166
		(0.0266)	(0.0253)		(0.283)	(0.244)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighbourhood characteristics (6)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Land use variables (4)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,784	1,781	1,781	1,784	1,781	1,781
Number of clusters	287	285	285	287	285	285
Kleibergen-Paap *F*-statistic	6333	7025	7230	76.11	77.43	78.63
Bandwidth *h*	5.301	5.149	5.199	6.557	5.652	6.017

Notes: We exclude observations within 2.5 kilometres of targeted areas and only keep observations in the years 2002, 2006, 2009 and 2012. 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

In columns (1) and (4) in Table C8 we first confirm that the results for house prices for this smaller sample are essentially identical to the baseline results. The results for sales time are somewhat stronger: the instantaneous effect seems to be about twice as strong, as well as the yearly adjustment effect. This may not be surprising, as it is hard to identify adjustment effects with only two years of observations (2009 and 2012) after the treatment. In columns (2) and (5) we control for the share of private rental and share owner-occupied housing. We find a small positive effect of the share of private rental housing on house prices. The coefficient of share owner-occupied housing also has the expected sign. Nevertheless, the effect of the KW-programme on house prices is almost identical to the previous specification. Hence, it seems that the treatment effect mainly captures improvements in quality, rather than changes in home ownership induced by the programme.

Because we rely on WoOn survey data to determine the share of owner-occupied and private rental housing, it may be that the sample is not representative for the housing stock. We therefore use representation weights provided in the survey, indicating how representative a household is for the neighbourhood's population. Using the weights, one should be able to obtain figures that are close to the actual situation. In columns (3) and (6) we control for the weighted share of private rental and share owner-occupied housing. Although the coefficient of share private rental housing is now statistically significant and positive at the five percent level, the treatment effects are again unaffected.

C.9 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 C9 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 2 and Table C1. Columns (2) and (5) take January 1, 2008 as a starting date. The effects are 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. This does not seem to bias our results.

C.10 RDD set-up

The baseline specifications use local linear estimation techniques, by only selecting neighbourhoods that have z-scores that are close to the threshold. 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 C10 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. 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:

TABLE C9 — 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	0.0358***	0.0370***	0.0424***	-0.333***	-0.381***	-0.377***
	(0.0109)	(0.0111)	(0.0116)	(0.0666)	(0.0643)	(0.0704)
Δ (KW-investment × years after investment)				0.0360***	0.0516***	0.0505***
				(0.0120)	(0.0129)	(0.0120)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	24,170	24,170	21,821	58,988	59,461	82,015
Number of clusters	176	176	201	559	573	1652
Kleibergen-Paap *F*-statistic	3215	2223	3149	3807	3259	4279
Bandwidth *h*	3.228	3.233	3.540	6.252	6.281	7.963

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
⁺ Significant at the 0.15 level

$$\Delta y_{\ell t} = \alpha \Delta k_{\ell t} + G(z_\ell) + \beta \Delta x_{\ell t} + \Delta v_t + \Delta \epsilon_{\ell t}, \quad (4)$$

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:

$$G(z_\ell) = \sum_{\wp=1}^{\mathcal{P}} \gamma_p^{+} (z_\ell - c)^{\wp} 1_{z_\ell \geq c} + \sum_{\wp=1}^{\mathcal{P}} \gamma_p^{-} (z_\ell - c)^{\wp} 1_{z_\ell < c}, \quad (5)$$

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 4.2 percent and the instantaneous sales time effect is −25 percent, while the yearly reduction in this effect is 4.1 percent.

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 2 and Table C1). Columns (2) and (3) in Table C10 show that the price effects are essentially unaffected when we vary the bandwidth. Also the sales time effects are very similar for different bandwidths (see columns (5) and (6)).

TABLE C10 — SENSITIVITY ANALYSIS: BANDWIDTH SELECTION

	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	0.0408***	0.0411***	0.0407***	-0.287***	-0.290***	-0.266***
	(0.00950)	(0.0143)	(0.0116)	(0.0585)	(0.0800)	(0.0648)
Δ (KW-investment × years after investment)				0.0408***	0.0236⁺	0.0439***
				(0.0135)	(0.0146)	(0.0111)
$G(z_\ell)$ 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	185,072	10,183	62,578	185,072	22,333	184,663
Number of clusters	3138	75	621	3138	160	3131
Kleibergen-Paap *F*-statistic	7942	627.1	12981	4323	2081	5819
Bandwidth *h*	∞	1.614	6.458	∞	3.110	12.439

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
⁺ Significant at the 0.15 level

C.11 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 B2 and Table B3 in Appendix B.2). 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 C11 reports the results.

In Columns (1) and (4) we regress respectively house price and sales time on whether the neighbourhood is treated, as well as a host of housing control variables (listed in Table B3 in Appendix B.2). The coefficients suggest a positive price effect of the programme of 5.9 percent. Sales times have been reduced instantaneously with 23 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 (−26 percent). In

TABLE C11 — 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	0.0577***	0.0426***	0.0385***	-0.262***	-0.297***	-0.313***
	(0.0112)	(0.0127)	(0.0114)	(0.0471)	(0.0488)	(0.0493)
KW-investment × years after investment				0.0273***	0.0279***	0.0282***
				(0.00658)	(0.00724)	(0.00784)
Housing characteristics (16)	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood characteristics (6)	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	1,393,040	138,966	138,955	1,393,040	345,337	347,103
Number of clusters	3671	198	198	3671	586	590
R^2	0.444			0.099		
Kleibergen-Paap F-statistic		4835	2495		8694	5941
Bandwidth h		3.176	3.178		6.176	6.198

Notes: We exclude observations within 2.5 kilometres of targeted areas. KW-investment is instrumented with the 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
+ Significant at the 0.15 level

Columns (3) and (6), Table C11, we also control for neighbourhood characteristics and changes in land use. The price effect is again slightly lower but similar to the baseline estimate (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.

C.12 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:

$$\text{Pr}(\ell = 1 \mid a_\ell) = \Phi\big(\Upsilon_\ell(a_\ell)\big), \tag{6}$$

where $\text{Pr}(\ell = 1 \mid a_\ell)$ 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 $\text{Pr}(\ell = 1 \mid a_\ell)$ 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 B4 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.[41] Table C12 reports the results.

[41] 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 C12 — 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	0.0421***	0.0436***	0.0348***	-0.329***	-0.210***	-0.125+
	(0.0109)	(0.0103)	(0.00942)	(0.0727)	(0.0779)	(0.0779)
Δ (KW-investment × years after investment)				0.0488***	0.0140	-0.0166
				(0.0177)	(0.0197)	(0.0204)
Δ Housing characteristics (5)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Neighbourhood characteristics (6)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Land use variables (4)	Yes	Yes	Yes	Yes	Yes	Yes
Δ Year fixed effects (14)	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	17,672	13,503	11,880	17,672	13,503	11,880
Number of clusters	144	115	97	144	115	97
R^2-within	0.536	0.535	0.522	0.070	0.075	0.078
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
+ Significant at the 0.15 level

Columns (1) and (4) use the set of control neighbourhoods based on Calipher matching. The price effect is then 4.3 percent, similar to baseline specifications. The instantaneous effect on sales times is somewhat larger and 28 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 for sales time we cannot detect a significant adjustment effect. The results suggest that the investments have led to a decrease in sales time of 18.9 percent. In Columns (3) and (6) we use nearest neighbour matching with replacement. This implies that we have only 38 control neighbourhoods. The price effect, however, is still very similar. The effect on sales times is lower and we cannot detect a significant adjustment effect. However, this is likely due to the higher variance in sales times. It is therefore that in the RDD the optimal bandwidth is somewhat larger when taking sales times as dependent variable.