# Place-based policies and the housing market: Prices, time on the market and welfare[[1]](#footnote-1)\*

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*This version: 08 April 2016*

**SUMMARY** ― We study the economic effects of place-based policies in the housing market and show that in a theoretical model with search frictions, these policies not only increase house prices but also temporarily reduce sales times. Nevertheless, price changes can be shown to be good measures of welfare changes. We investigate the effect of a place-based programme aimed to improve public housing by investing in 83 impoverished neighbourhoods throughout the Netherlands. We combine a first-difference approach with a regression-discontinuity design taking into account that a neighbourhood’s treatment probability is endogenous. Dutch place-based policies, on average, appears to increase house prices of owner-occupied properties with 3.5 percent and temporarily reduce sales times with 20 percent (about one month). The sales time effect dissipates within five years, while the price effect is permanent. The long-run welfare benefits induced by the programme are sizeable and about half of the value of the investments.

*JEL-code* ― R30, R33

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

## Introduction

In many countries *place-based policies* have been developed that make large public investments in poor neighbourhoods. Economists are not necessarily in favour of these policies. It has been argued that governments should help people, rather than places, and “not bribe people to live in unattractive places” (Glaeser, 2011). However, if nonmarket interactions are important, then this may justify place-based policies. For example, through local spillovers, a neighbourhood participation programme may decrease negative externalities (Rossi-Hansberg et al., 2010). In the literature, there has been ample attention paid to the effectiveness of place-based labour market programmes (see e.g. Neumark and Kolko, 2010; Mayer et al., 2012; Busso et al., 2013; Kline and Moretti, 2013, and Neumark and Simpson, 2015 for an overview). However, the effects of place-based policies on the housing markets are hardly researched. There are few studies that confirm that place-based investments have led to higher house prices (Ioannides, 2003; Schwartz et al., 2006; Rossi-Hansberg et al., 2010). This does not imply, however, that place-based policies are always effective. For example, a number of studies, including Briggs (1999), Lee et al. (1999), Santiago et al. (2001) and Ahlfeldt et al. (2013), find no statistically significant, or even small negative, effects of place-based policies that subsidise housing.

Most of these empirical studies focus on a specific programme with a small number of neighbourhoods in a specific city. Hence, these studies be may subject to sampling error leading to spurious results and may have limited external validity. Furthermore, because neighbourhood selection is endogenous – only the worst performing neighbourhoods receive subsidies – the estimates of the benefits of place-based policies are not always causal. The studies also focus exclusively on house prices, in line with spatial equilibrium models that measure welfare gains of local policies through changes in land prices. This approach is particularly attractive when assuming absentee landowners and frictionless markets.[[4]](#footnote-4) Adjustment effects that may occur after the investment has taken place are usually ignored and it is typically assumed that price effects are immediate and permanent. These studies assume away the presence of search frictions and may therefore overlook several essential features of owner-occupied markets, including the fact that it takes time to sell a house and therefore it takes time for a market to adjust to a new steady state (Harding et al., 2003; Genesove and Mayer, 2001; Merlo and Ortalo-Magné, 2004).[[5]](#footnote-5)

In this paper, we analyse the effects of place-based policies on house prices and sales times. Our contribution is threefold. First, we set up a theoretical model including housing search and matching. We show that place-based policies increase house prices (in the short and long run). By contrast, these policies reduce sales times *temporarily* (in the short run), but not in the long run. This is useful as a consistency test: if one does not find a temporary effect of place-based policies on sales times, then this will put doubts on the causality of an effect on prices.[[6]](#footnote-6) Information on the effects on sales time are also indicative how much time it takes before the market returns to a steady state, so that we can identify the long-run price change, and to what extent reductions in sales time are beneficial to incumbent homeowners. The long-run benefits of the policy then translate into higher house prices, as in a model without search frictions. In a model with search frictions, house prices are not necessarily one-to-one related to welfare measures. We demonstrate, however, that the percentage price effect can be interpreted as a percentage welfare effect. When the market is in spatial equilibrium, absolute changes in prices are underestimates of absolute welfare changes, but when vacancy rates are low as is usually the case in empirical data, these will be very similar.

The second contribution of the paper relates to the scale of the programme we evaluate. We evaluate changes in local amenity levels due to a large-scale nationwide urban revitalisation programme in the Netherlands, starting in 2007. In this so-called *krachtwijken-*programme (henceforth: KW investment scheme), 83 neighbourhoods were selected for revitalisation with funding from the national government.[[7]](#footnote-7) The government and (not for profit) public housing associations announced to invest about € 2.75 billion in these neighbourhoods, on average about € 3.5 thousand per household in receiving neighbourhoods. However, in the end only € 1 billion was invested (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). A vast majority of the investment was spent on improving of the public housing stock (about 30 percent of the Dutch housing market is public housing). The remainder was used for investments in green spaces, social empowerment programs and the conversion of public to private housing (Wittebrood and Permentier, 2011). The private housing stock, to which our data refer, was not improved by the program. We utilise a nationwide dataset with information on (privately-owned) house transactions from 2000 to 2014, including the house price and sales time. We use a first-differences estimation strategy based on thousands of repeated sales observations. In essence, we compare changes in house prices, as well as sales times, between *many* targeted and non-targeted neighbourhoods. Hence, the results of our study are likely to have external validity and neighbourhood sampling error is eliminated

The third, but important, contribution of the paper is to the identification of causal effects of place-based policies. In contrast to the bulk of the existing literature, we take into account that areas targeted by place-based policies are not randomly chosen, but are *explicitly* chosen because of undesirable characteristics. We employ a fuzzy regression-discontinuity design (FRD) by using information on an eligibility criterion to receive investments.[[8]](#footnote-8) This criterion is dependent on so-called deprivation scores, calculated by the national government for the whole of the Netherlands. Although the neighbourhoods with the highest deprivation scores were not always chosen, there is a discrete and substantial jump in the probability to become selected when the deprivation score exceeds a certain threshold (the jump is about 0.75 percentage points).[[9]](#footnote-9)

We find that due to investments (mainly in public housing), house prices increased by about 3.5 percent, which is in line with previous studies (see, e.g., Rossi-Hansberg et al., 2010). We also find that the effect on sales time effect is strong as sales times are reduced temporarily with 15-20 percent (about a month). The latter result indicates that selling time and matching is a non-negligible feature of the housing market. We show that the sales time effect is temporary and disappears after five years. The empirical results survive remarkably unaltered when we extensively check for robustness, for example by testing for spatial spillovers, conducting quasi-placebo experiments and using propensity score matching rather than a FRD. A counterfactual analysis indicates that the benefits to homeowners induced by the programme are about half of the value of the investments. We also show that, despite the strong effect of the programme on sales times, the sales time effect contributes very little (less than 5 percent) to the monetary benefits of incumbent home owners.

The remainder of the paper is organised as follows. In Section II we discuss why an effect of amenity changes has only short-term effects on sales times, while the price effect should be permanent. In Section III we discuss the features of the KW-investment scheme, the data and the econometric framework. Section IV turns to the empirical results, which is followed by a counterfactual analysis in Section V. We subject the baseline results to an extensive sensitivity analysis in Section VI and Section VII concludes.

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

### A house price bargaining model

What are the effects of place-based investments on the housing market when search and matching are important? We follow Wheaton (1990) and assume a neighbourhood with a given housing supply of 2and a certain number of households here housing supply exceeds the number of households in period . The neighbourhood contains two types of housing. The supply of each type of housing is exactly half of total housing supply and equal to . Households have a preference for one housing type.

In each period *t*, households change exogenously their preference regarding the type of housing with probability (e.g. due to birth of a child). We will then distinguish between three types of households. Households will be matched if they occupy the preferred housing type, otherwise they will be mismatched. The number of matched households who own one property is denoted by. The number of mismatched households is denoted by . When households are mismatched, they will search for the other type of housing by incurring search costs. The search costs depend on the effort level , which will be endogenously chosen. After finding and moving into a new house, a household is matched but then possessesalso *a second vacant property* which it aims to sell. The number of households with two houses are denoted by . Matched households receive a utility flow of from living at a certain location, where is the amenity level.

We then make two additional assumptions. First, we assume that the mismatched utility flow is less than, but proportional, to the utility flow when they are matched ( with . This is a common assumption in the search literature and implies that the difference between the mismatched and the matched utility flows is proportional to the matched utility flow.[[10]](#footnote-10)

Second, we assume that search costs are an increasing function of search effort and proportional to the amenity level in the previous period, denoted by . Hence, we define search costs as , where is some function of search effort and , . The proportionality assumption has a range of justifications. For example, in most housing markets, real estate agents charge fees that are proportional to housing prices (which are proportional to the difference between the mismatched and matched utility flows in our model). Moreover, search time costs will increase approximately proportionally with household income of searching households.[[11]](#footnote-11) The assumption that the costs depend on the amenity level in the *previous* period implies that that search costs adjust to changes in the amenity value with a delay, for example because real estate agent fees need time to adjust.

In each period, the household enjoys the amenity and pays mortgage costs , where is the interest rate. Note that households pay mortgage costs equal to in each period. When a household owns two houses, it pays . Mismatched households also incur search costs. Households take into account that they may change state (e.g. by selling their house or finding a new house) and the present values also incorporate information on *future* changes in the states (by means of , ,). The lifetime utilities – i.e. the present values of utility of each state – (matched , owning two houses , mismatched ) are given by:

|  |  |
| --- | --- |
| (1) |  |
| (2) |  |
| (3) | ,  |

where is the interest rate. is the expected sales time of a house, which is given by where is the number of vacant units for each type and the number of mismatched households. denotes the probability of a mismatched household to find a house, given by . House prices are endogenously determined when a household owning two houses and a mismatched household are matched, by splitting the surplus of the match.

Let us now assume that a household may move into this neighbourhood. The household does not know in which state it will enter the market and it only knows the probabilities associated with each state. The expected utility to locate in this particular neighbourhood is then:

|  |  |
| --- | --- |
| (4) |  |

where the first term denotes the probability of being matched multiplied with the amenity level in the neighbourhood. The second term is the probability of being mismatched multiplied with the share of the amenity level and the search costs. The third term captures the expected mortgage costs. A household may also locate somewhere else and gain utility , which we normalise to zero. In a spatial equilibrium it should then hold that:

|  |  |
| --- | --- |
| (5) |  |

This equation implies that the house price in a neighbourhood depends positively on the amenity level and on search frictions via search costs , the number of mismatched households and the number of vacancies . Given , and , in steady state, the number of households in the neighbourhood will adjust in such a way that the above condition holds.[[12]](#footnote-12)

### Steady-state effects of place-based investments

We first focus on the effect of place-based investments in the long run by assuming steady-state, so we will drop subscript . Given equations (1), (2) and (3) and the house price is given by:

|  |  |
| --- | --- |
| (6) |  |

Given that place-based investments lead to an increase in , it is clear that this will induce an increase in prices, conditional on the level of and that are endogenously determined.[[13]](#footnote-13)

Will place-based investments also have an effect on sales times given the above-stated assumptions? The answer appears to be no. Using (7), the first-order condition for privately optimal search effort is given by , which in steady state simplifies to:

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| --- | --- |
| (7) |  |

which does not depend on the level of Hence, the chosen search effort is *not* a function of the amenity level, because the marginal benefits and costs of search are both proportional to the amenity level. 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, and *(ii)* the expected sales time will not be affected by these place-based investments.

### Out of steady-state effects of place-based investments

We will now examine the effect of place-based investments on prices and sales time out of the steady state. Given the investment, the market will need time to adjust to a new steady state. In the short-run, search effort, matching rates and sales times may deviate from the long-run steady state. We solve the system of equations (1), (2) and (3), taking into account future changes in present values of utility. The price of a property is then given by:

|  |  |
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| (8) |  |

with The first part of this equation is comparable (but not identical) to (6). The second part is incorporating the future changes in the present values of each state. The first-order condition for privately optimal search out of steady state is given by:

|  |  |
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| (9) |  |

Hence, optimal search effort (in depends on the present utility values of being matched and mismatched in the next period (in . Recall that the matching rate and the sales time are a function of the number of households in each state, which vary over time. The number of households in a state depends on the matching rate, sales time and the probability of changes in housing preferences . It is straightforward to show that the number of households in each state can be written as (see also Wheaton, 1990):

|  |  |
| --- | --- |
| (10) |  |
| (11) |  |
| (12) |  |

which provides a stable model of changes in household type as well as residential moving. We numerically solve this out-of-steady-state model using a procedure that we describe in more detail in the Appendix. We choose (reasonable) parameter values for , , , , , and functional forms for and in line with the literature and assume that the matching

Figure — Prices and sales times in the short-run

*Notes:* We assume for , for , , , , , , , and .

function is Cobb-Douglas. Figure 1 shows the results for an unanticipated 25 percent increase in the amenity level (at .[[14]](#footnote-14) 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. There is some overshooting, but the magnitude of this overshooting is almost negligible. Sales time immediately drops after the amenity increase with about 4 percent and *slowly* adjusts to the former steady-state value.

Hence, these numerical results yield two additional testable empirical predictions given an increase in the amenity level: *(i)* prices adjust quickly to the new steady state value and *(ii)* sales time drop in the short run, while this effect disappears in the long run.

### Welfare and prices

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 welfare changes of these investments.[[15]](#footnote-15) We show that in the long run, so when search effort does not change, house price changes due to changes in are accurate measures of welfare changes. Following Wheaton (1990), we define welfare per household as:

|  |  |
| --- | --- |
| (13) |  |

In the long run, is proportional to . It is then straightforward to see that:

|  |  |
| --- | --- |
| (14) |  |

Then:

|  |  |
| --- | --- |
| (15) |  |

The above holds because and are independent of in the long-run, so that the partial derivative is identical to the full derivative of log welfare with respect to .

We also may take the log of the price equation and then determine the percentage change to a change in . Given (6), we have:

|  |  |
| --- | --- |
| (16) |  |

It can be easily seen that:

|  |  |
| --- | --- |
| (17) |  |

which holds because , and are independent of in the long-run. Hence, *percentage* price changes are an exact measure of *percentage* welfare changes in the long-run.

It is also to know whether absolute price changes are equal to absolute change in welfare. From equations (5) and **Error! Reference source not found.**, given that the market is initially in spatial equilibrium, it can be seen that . Hence, when the observed vacancy rate in our data is sufficiently small – which is usually the case in empirical data – absolute changes in welfare are very similar to absolute changes in prices. If vacancy rates are high, price changes are always an underestimate of welfare changes.

## Empirical framework and data

### The urban revitalisation programme

There is ample empirical evidence that households with low incomes and associated social problems are disproportionally concentrated in certain urban neighbourhoods. For example, many US inner cities contain large concentrations of low-income households and score low on almost every measure capturing social dysfunction (Mills and Lubuele, 1997; Glaeser et al., 2008). In the Netherlands, we observe a similar but less extreme pattern.[[16]](#footnote-16) About 70 percent of the most deprived neighbourhoods are located in the four largest cities of the Netherlands (Amsterdam, Rotterdam, The Hague and Utrecht). The share of public housing is much higher in these neighbourhoods than in other parts of the Netherlands.[[17]](#footnote-17) The gap between these poor neighbourhoods and other neighbourhoods in terms of unemployment, crime rates and income, has widened in the last decade. Therefore, in 2007, a substantial national investment programme was launched by the Dutch secretary of state, who was responsible for housing and labour: € 216 million was planned to be invested in the 83 worst performing postcode areas, which we refer to as neighbourhoods (The Court of Audit, 2010). The investment fund was used to assist municipalities in restructuring and revitalisation of neighbourhoods. On 14 September 2007 the secretary of state agreed with large public housing associations that they would invest another € 2.5 billion in the selected neighbourhoods over a course of ten years (in total about € 3500 per household residing in these neighbourhoods) (The Court of Audit, 2010). We consider this date as the start of the investment programme, but we will check for robustness of the assumed date later on. Although the exact monetary value of the investment is unknown, experts estimate that eventually about one billion Euros has been invested in these neighbourhoods between 2007 and 2012 (Permentier et al., 2013). Apart from physical restructuring of public rental housing and sale of public housing, the investments were also targeted at poor households directly through empowerment programs (Department of Housing, Spatial Planning and the Environment, 2007; Wittebrood and Permentier, 2011).

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

The selection of the KW neighbourhoods was based on the deprivation score which were known to be disadvantaged (Permentier et al., 2013). The idea was to target neighbourhoods with a z-score of at least 7.30. However, twelve neighbourhoods were removed from the list after discussions with local governments, while two other neighbourhoods (in Amsterdam and Enschede) were added although they had z-scores below the threshold (respectively 6.84 and 5.00).[[18]](#footnote-18) Table 1 shows that targeted KW neighbourhoods have scores that are about two standard deviations above the Dutch average for the different categories. The overall average score for these neighbourhoods is 8.94, more than 3.5 times the standard deviation above the Dutch average. In Figure 2 we plot the selection of neighbourhoods as function of the z-score. While controlling for a flexible function of the z-score, it is shown that there is a substantial discrete jump in the probability to become selected when . For example, a neighbourhood with a z-score of 7.29 has a probability of 0.055 to be included, whereas for a neighbourhood with a z-score of 7.30 this probability is 0.803. We also investigate whether the cumulative distribution of the z-scores is discontinuous around the threshold value, which would be a problem if we employ a regression-discontinuity design. However, it appears that the distribution function is continuous around the threshold point suggesting that municipalities could not manipulate the z-score (see Figure A2 in the Appendix).

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| --- |
|  Table — Deprivation scores for neighbourhoods |
|  | All neighbourhoods |  | KWneighbourhoods |  | Non-KWNeighbourhoods |
|  |  |  |  |  |  |  |  |  |
| 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, seeBrouwer and Willems (2007).  |

Figure — The z-score and selection

*Notes:* This is a regression of the assignment of a neighbourhood on the scoring rule dummy and a flexible function of the z-score. The number of observations is 4016.

### Data

Our analysis is based upon a house transactions dataset from the NVM (Dutch Association of Real Estate Agents). It contains information on about 80 percent of all transactions between 2000 and 2014, so roughly seven years before and after the investment took place.[[19]](#footnote-19) For 1,796,542 transactions, we know the transaction price, asking price, the sales time (in days on the market), the exact location, and a wide range of house attributes such as size (in square meters), type of house, number of rooms and construction year.[[20]](#footnote-20) On average, properties in our sample are sold 1.29 times in our study period. In the analysis, we focus on a repeated sales sample, so properties that are sold at least twice, leaving us with 434,033 transactions.[[21]](#footnote-21)

In Table 2, descriptives are reported for observations outside and inside (targeted) KW neighbourhoods. About 3.8 percent of the observations in the repeated sales sample is in a targeted KW neighbourhood whereas 1.6 percent in this sample is in a KW- neighbourhood in the post-investment period. It appears that the price per square metre in non-KW neighbourhoods is 3.5 percent higher than in KW neighbourhoods. The difference seems fairly small, but is explained by the observation that most deprived neighbourhoods are located in urban, rather than rural, areas, where prices are generally higher. Properties in KW neighbourhoods tend to have a lower quality: they are more often apartments, are older, have less often central heating and are of a lower maintenance quality. Also, 34 percent of the properties in these areas have been constructed between 1961 and 1970, a building period which is in the Netherlands associated with low building quality. Table A1 in the Appendix 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.[[22]](#footnote-22)

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|  Table — Descriptive statistics for repeated sales sample |
|  | Observations outside KW neighbourhoods |  | Observations inside KW neighbourhoods |  |
|  |  |  |  |  |  |  |  |  |  |
|  |  |  |  |  |  |  |  |  |  |
| House priceper m² *(in €)* | 1,910 | 597.0 | 500 | 5,000 |  | 1,846 | 601.1 | 504.2 | 4,972 |
| Days on the market | 136.2 | 173.5 | 1 | 1,823 |  | 126.8 | 159.4 | 1 | 1,816 |
| KW investment received | 0 |  |  |  |  | 0.418 |  |  |  |
| Deprivation z-score | 0.431 | 2.829 | -6.600 | 10.60 |  | 8.684 | 1.181 | 5 | 12.98 |
| Size in m² | 105.1 | 32.87 | 26 | 250 |  | 83.44 | 26.14 | 27 | 250 |
| House type – apartment | 0.406 |  |  |  |  | 0.803 |  |  |  |
| House type – terraced | 0.324 |  |  |  |  | 0.145 |  |  |  |
| House type – semi-detached | 0.213 |  |  |  |  | 0.0492 |  |  |  |
| House type – detached | 0.0567 |  |  |  |  | 0.00335 |  |  |  |
| Garage | 0.205 |  |  |  |  | 0.0557 |  |  |  |
| Garden | 0.988 |  |  |  |  | 0.989 |  |  |  |
| Maintenance quality –good  | 0.909 |  |  |  |  | 0.874 |  |  |  |
| Central heating | 0.932 |  |  |  |  | 0.886 |  |  |  |
| Listed | 0.00497 |  |  |  |  | 0.00508 |  |  |  |
| Construction year <1945 | 0.226 |  |  |  |  | 0.293 |  |  |  |
| Construction year 1945-1960 | 0.0710 |  |  |  |  | 0.143 |  |  |  |
| Construction year 1961-1970 | 0.177 |  |  |  |  | 0.344 |  |  |  |
| Construction year 1971-1980 | 0.166 |  |  |  |  | 0.0432 |  |  |  |
| Construction year 1981-1990 | 0.152 |  |  |  |  | 0.0515 |  |  |  |
| Construction year 1991-2000 | 0.167 |  |  |  |  | 0.0894 |  |  |  |
| Construction year >2000 | 0.0408 |  |  |  |  | 0.0358 |  |  |  |
|  |  |  |  |  |  |  |  |  |  |
| *Notes:* The number of observations outside KW neighbourhoods is 417,307 and inside KW neighbourhoods 16,726. Note that the house type variables, garage, garden, and construction year are time-invariant, so they will drop in the first-differences equations. |

In Figure 3 we plot the house price and the sales time for KW and other neighbourhoods over time. In Figure 3a, it is confirmed that prices in KW neighbourhoods were lower than in other neighbourhoods, but this price gap is substantially reduced after 2007, while from 2009 onwards house prices seem almost identical. Although suggestive, one may not conclude that this reduction in price gap is due to the investment programme, because it ignores that other factors may play a role (e.g. gentrification, disproportionate construction of new houses). In Figure 3b, it is 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. Although this difference seems to become somewhat smaller over time and disappears in 2013.

(a) House price per m²

(b) Days on the market

Figure — House prices and sales time inside and outside KW neighbourhoods

### Econometric framework and identification

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

|  |  |
| --- | --- |
| (18) |  |

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

|  |  |
| --- | --- |
| (19) |  |

The above specification ignores the possibility of spatial spillovers. However, houses close to a targeted area may also experience changes in because positive effects are likely to decay over space (Rossi-Hansberg et al., 2010). We therefore exclude observations within two and a half kilometres of a targeted neighbourhood.[[23]](#footnote-23) When estimating (19), the crucial identifying assumption for consistent estimation of is that unobserved trends are uncorrelated with the change in treatment . This assumption may be problematic, e.g. because of demographic trends such as gentrification. We therefore need to find neighbourhoods that are almost identical to the KW neighbourhoods but are not targeted by the investment scheme.

An identification strategy which comes close to random sampling is a regression-discontinuity design (RDD), implying that we compare the change in the outcome variable close to the threshold, as outlined in the previous section. We therefore combine first-differencing with a RDD based on the deprivation score of the neighbourhood.[[24]](#footnote-24) This approach approximately provides the causal effect of the investment if neighbourhoods are not able to manipulate the score. The latter seems plausible because the deprivation score was a function of 18 indicators that are very difficult to influence in the short run (including subjective feelings about the neighbourhood, level of education and housing stock). What is more important, the investment programme was announced in 2007, based on data from 2006 and 2002. It is therefore highly unlikely that local governments anticipated the exact selection criteria.

In principle, to avoid any bias, one would prefer to only include observations that are *at* the z-score threshold, so . However, this would lead to a few number of observations and therefore to large standard errors. Hence, we estimate (19) using a weighted regression, which can be interpreted as a local linear (LL) regression approach, where observation close the threshold receive a higher weight (Hahn et al., 2001). This implies:

|  |  |
| --- | --- |
| (20) |  |

where denotes the kernel function. We use a uniform kernel:

|  |  |
| --- | --- |
| (21) |  |

where is the bandwidth that indicates how many observations are included on both sides of the threshold. The estimated parameters are usually sensitive to the choice of the bandwidth. We use the approach proposed by Imbens and Kalyanaraman (2012), who show that the optimal bandwidth can be estimated as:

|  |  |
| --- | --- |
| (22) |  |

where the constant and is the number of observations. and are the conditional variances of given on both sides of the threshold (indicated with ‘‘ and ‘’), denotes the estimated density of at . and are estimates of the second derivatives of a function of the z-score. and are estimated regularisation terms that correct for potential error in the estimation of the curvature of on both sides of the threshold.

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

|  |  |
| --- | --- |
| (23) |  |

where the indicates first-stage coefficients and is the parameter of interest. Here, equals one when and when a property is sold before and after the investment. In Figure 1, it was shown that was highly statistically significant at the neighbourhood level. The coefficient was about 0.75; note that when we had a sharp RDD, must have been equal to one. In the second stage we then insert (and calculate standard errors taking into account that is estimated):

|  |  |
| --- | --- |
| (24) |  |

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

|  |  |
| --- | --- |
| (25) |  |

where and . and denote the conditional covariance of the treatment and dependent variable at on both sides of the threshold. We note that, as in previous applications, equation (25) leads to very similar bandwidths as (22).

Note that a fuzzy RDD only 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 would be similar to the estimation procedure where we include all neighbourhoods (see equation (19)), this would suggest that the local average treatment effect at the threshold is equal to the average treatment effect.

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

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

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

|  |  |
| --- | --- |
| (26) |  |

where indicates the immediate effect and are parameters that capture adjustment effects. The above equation indicates that we have endogenous variables. The instruments are then changes in the scoring rule dummy and the change in the interaction of the scoring rule and the ’th polynomial of years after the investment.

## Results

### Baseline results – house prices

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

We start with a naïve regression of the change in house price on the change in the investment status. The coefficient in Column (1) shows that investments seem to have generated a positive effect on prices of 4.5 percent.[[25]](#footnote-25) When we control for changes in housing attributes (Column (2)), prices in targeted neighbourhoods have increased with 3.8 percent, relative to prices in other neighbourhoods. In Column (3) we employ a sharp regression-discontinuity design by controlling for the z-score and excluding non KW neighbourhoods with a z-score above the threshold and KW neighbourhoods with a z-score below the threshold. Using equation (22) we find an optimal bandwidth of 5.13, which implies that we only include about 25 percent of the observations. The price effect is 3.4 percent and somewhat lower than in previous specifications. 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 , 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 A3 in the Appendix). In all the specifications, having a z-score above the threshold is a very strong instrument of being treated (), with a

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| --- |
|  Table — 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 |
|  |  |  |  |  |  |  |
| ∆ KW investment | 0.0441\*\*\* | 0.0372\*\*\* | 0.0338\*\*\* | 0.0329\*\*\* | 0.0358\*\*\* | 0.0334\*\*\* |
|  | (0.0114) | (0.0104) | (0.0117) | (0.0122) | (0.0122) | (0.0118) |
| Δ Size *(log)* |  | -0.877\*\*\* | -0.885\*\*\* | -0.889\*\*\* | -0.887\*\*\* | -0.876\*\*\* |
|  |  | (0.00586) | (0.0138) | (0.0139) | (0.0177) | (0.0194) |
| Δ Rooms *(log)* |  | 0.00296\*\*\* | 0.00362\*\* | 0.00297\* | 0.00515\*\*\* | 0.00389\* |
|  |  | (0.000475) | (0.00157) | (0.00156) | (0.00195) | (0.00204) |
| Δ Maintenance quality – good  |  | 0.106\*\*\* | 0.0978\*\*\* | 0.0940\*\*\* | 0.0990\*\*\* | 0.0958\*\*\* |
|  |  | (0.00151) | (0.00334) | (0.00351) | (0.00378) | (0.00408) |
| Δ Central heating |  | 0.0648\*\*\* | 0.0676\*\*\* | 0.0688\*\*\* | 0.0804\*\*\* | 0.0738\*\*\* |
|  |  | (0.00250) | (0.00501) | (0.00508) | (0.00636) | (0.00687) |
| Δ Listed building |  | 0.00239 | 0.0107 | 0.00855 | 0.000337 | -0.00890 |
|  |  | (0.00805) | (0.0163) | (0.0188) | (0.0187) | (0.0203) |
| ∆ Population density *(log)* |  |  |  |  |  | 0.0635 |
|  |  |  |  |  |  | (0.0815) |
| ∆ Share foreigners |  |  |  |  |  | -1.045\*\*\* |
|  |  |  |  |  |  | (0.154) |
| ∆ Share young people |  |  |  |  |  | 0.213 |
|  |  |  |  |  |  | (0.485) |
| ∆ Share elderly people |  |  |  |  |  | -0.703\*\* |
|  |  |  |  |  |  | (0.293) |
| ∆ Average household size |  |  |  |  |  | 0.0609 |
|  |  |  |  |  |  | (0.114) |
|  |  |  |  |  |  |  |
| ∆ Year fixed effects (14) | Yes | Yes | Yes | Yes | Yes | Yes |
| ∆ Land use variables (4)  | No | No | No | No | No | Yes |
|  |  |  |  |  |  |  |
| Number of observations | 169,664 | 169,664 | 24,353 | 22,589 | 12,766 | 10,484 |
| *R*²-within | 0.375 | 0.538 | 0.549 |  |  |  |
| Kleibergen-Paap *F*-statistic |  |  |  | 5444 | 8063 | 2571 |
| Bandwidth  |  |  | 4.099 | 3.383 | 4.312 | 3.547 |
| *Notes:* We exclude observations within 2.5 kilometres of targeted areas. In Column (3) we exclude non-targeted neighbourhoods with a z-score above 7.3 and targeted neighbourhood with a z-score below 7.3. In Columns (4)-(6) the change in KW investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level and in parentheses.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

coefficient close to one: houses in neighbourhoods that are in a neighbourhood with have an approximately 98 percent higher probability to become treated.[[26]](#footnote-26) The second stage results are in line with previous specifications. The result in Column (4), Table 3, implies that prices in KW neighbourhoods have increased with 3.3 percent due to the investment programme. In Column (5) we explore the robustness of the findings further by removing the observations that are referring to transactions that both occur before or after the treatment date. While this reduces the sample size with about 50 percent, this hardly has an impact on the price effect (3.6 percent).

The final column (7) 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 number of houses increases due to the place-based policy, age composition of the households may change. These indirect effects may partly explain the effects on prices. To test this, we control for additional demographic variables. These variables are potentially endogenous. For example, higher prices imply that it is more attractive to construct houses, leading to a higher population density. Although we do not claim causal effects of the neighbourhood controls, it is informative to see to what extent the coefficients related to the place-based investments are influenced by inclusion of these additional controls. More specifically, we include the changes in population density, the share of foreigners, share of young (<25 years) and elderly people (>65 years) and the average household size and land use.[[27]](#footnote-27) Increases in population density are associated with price increases. However, we do not think this effect can be interpreted as a causal effect, because neighbourhoods with positive price changes may also experience an increase in the construction of housing leading to a higher population density. Furthermore, the share of foreigners is associated with price decreases. More importantly, the coefficient of interest is hardly affected by inclusion of these controls (3.4 percent), which suggests that sorting on observable neighbour characteristics is not a main determinant of the statistically significant effect of place-based policies. This seems to suggest that the effect of the place-based investments is mainly due to a direct change in the quality of public housing.

### Baseline results – sales time

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

In Column (1) we start again with a naïve regression of the change in the logarithm of days on the market on whether a property has experienced a change in the treatment status. This specification suggests that the sales time has been reduced with 8.1 percent due to the investment. If we control for housing attributes in Column (2), the coefficient is essentially

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| --- |
| Table — 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 |
|  |  |  |  |  |  |  |
| ∆ KW investment | -0.0843\* | -0.0843\* | -0.150\*\*\* | -0.149\*\*\* | -0.193\*\*\* | -0.161\*\*\* |
|  | (0.0473) | (0.0470) | (0.0529) | (0.0501) | (0.0533) | (0.0562) |
| Δ Size *(log)* |  | 0.198\*\*\* | 0.165 | 0.161 | 0.0996 | 0.0885 |
|  |  | (0.0658) | (0.144) | (0.103) | (0.167) | (0.130) |
| Δ Rooms *(log)* |  | -0.0271\*\*\* | -0.0352\*\*\* | -0.0382\*\*\* | -0.0383\*\*\* | -0.0262\*\* |
|  |  | (0.00535) | (0.0134) | (0.00966) | (0.0149) | (0.0113) |
| Δ Maintenance quality – good  |  | 0.0642\*\*\* | 0.0535\*\* | 0.0453\*\* | 0.0531\* | 0.0871\*\*\* |
|  |  | (0.0126) | (0.0260) | (0.0183) | (0.0298) | (0.0238) |
| Δ Central heating |  | -0.0538\*\*\* | -0.103\*\*\* | -0.103\*\*\* | -0.137\*\*\* | -0.112\*\*\* |
|  |  | (0.0162) | (0.0317) | (0.0238) | (0.0375) | (0.0307) |
| Δ Listed building |  | 0.0186 | 0.0535 | 0.0596 | 0.111 | 0.0724 |
|  |  | (0.0554) | (0.0842) | (0.0673) | (0.0893) | (0.0830) |
| ∆ Population density *(log)* |  |  |  |  |  | -0.113 |
|  |  |  |  |  |  | (0.140) |
| ∆ Share foreigners |  |  |  |  |  | 0.564 |
|  |  |  |  |  |  | (0.671) |
| ∆ Share young people |  |  |  |  |  | -1.682 |
|  |  |  |  |  |  | (1.145) |
| ∆ Share elderly people |  |  |  |  |  | -0.362 |
|  |  |  |  |  |  | (0.731) |
| ∆ Average household size |  |  |  |  |  | -0.118 |
|  |  |  |  |  |  | (0.316) |
|  |  |  |  |  |  |  |
| ∆ Year fixed effects (14) | Yes | Yes | Yes | Yes | Yes | Yes |
| ∆ Land use variables (4)  | No | No | No | No | No | Yes |
|  |  |  |  |  |  |  |
| Number of observations | 169,664 | 169,664 | 34,569 | 64,324 | 22,447 | 36,905 |
| *R*²-within | 0.057 | 0.057 | 0.060 |  |  |  |
| Kleibergen-Paap *F*-statistic |  |  |  | 16228 | 14819 | 9660 |
| Bandwidth  |  |  | 5.153 | 6.950 | 6.147 | 7.645 |
| *Notes:* We exclude observations within 2.5 kilometres of targeted areas. In Column (3) we exclude non-targeted neighbourhoods with a z-score above 7.3 and targeted neighbourhood with a z-score below 7.3. In Columns (4)-(6) the change in KW investment is instrumented with the change in the eligibility based on the scoring rule. Standard errors are clustered at the neighbourhood level and in parentheses.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

the same. In Column (3) we employ the sharp regression-discontinuity design and exclude non KW neighbourhoods with a z-score above the threshold and KW neighbourhoods with a z-score below the threshold. The effect then becomes somewhat stronger ( percent). Next, we do not exclude neighbourhoods but use an instrumental variable approach instead, with the change in the scoring rule as the instrument. Note that the first stage results are almost identical to the price regressions (see Table A3 in the Appendix). The fuzzy regression-discontinuity design leads to similar second stage results: Column (4) in Table 4 suggests that the investment has led to a 13.8 percent decrease in sales time. The optimal bandwidth is somewhat larger than in the price regressions, possibly because of a greater variance of the dependent variable. In Column (5) we only include observations for which transactions occur before and after the treatment date leading to similar results: the place-based investment seems to have reduced sales times with 17.6 percent. This effect is very similar ( percent) if we control for changes in demographics in Column (6).

### Adjustment effects

We will now explicitly distinguish between short-run and long-run effects by allowing for adjustment effects. We estimate equation (26) and use the local linear approach without neighbourhood variables, which corresponds to the specification listed in column (4) in Table 3 and Table 4. We report the estimated coefficients in Table 5.[[28]](#footnote-28) Recall that according to theory, we expect that the price effect is immediate and permanent. On the other hand, sales times are expected to become smaller over time and disappear in the long run.

In column (1) we include a linear interaction term of the treatment status with the time after the investment (measured in years). It is shown that there is an immediate price effect (2.0 percent). The linear interaction term is positive, but small and only marginally statistically significant. The specification predicts that after five years the price effect is 4.0 percent (and statistically significant at the one percent level), which is similar to the baseline estimate. Column (2) includes also a second-order term leading to statistically insignificant coefficients. However, it is more insightful to test the joint significance of these coefficients over time. The results are presented in Figure 4a. This leads to very similar results: after five years the price effect is 3.8 percent, while the immediate price effect is 2.2 percent. In column (3) we include interaction terms of the treatment variable and 2.5 years interval dummies. The same pattern emerges: the price effect is increasing over time, but not so strongly and the price coefficients are only marginally statistically significantly different from each other (*p­*-value). The price effect in the first 2.5 years might also a bit lower because of uncertainty about the exact starting date of the programme (an issue which we discuss in more detail in Section VI.F).

Let us now investigate the adjustment effects of sales times after the announcement of the investment programme. It seems that the sales time effect is immediate and substantial (see Column (4), Table 5). The decrease in sales times is 22.4 percent, which is on average about a month reduction in sales times. The effect of sales times tends to become less pronounced over time. After five years, the effect is 9.6 percent and only marginally statistically

|  |
| --- |
| Table — 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 |
|  |  |  |  |  |  |  |  |
| ∆ KW investment | 0.0199\*\* | 0.0215\* |  |  | -0.275\*\*\* | -0.257\*\*\* |  |
|  | (0.00892) | (0.0119) |  |  | (0.0676) | (0.0975) |  |
| ∆ (KW investment ×  | 0.00393\* | 0.00265 |  |  | 0.0364\*\*\* | 0.0222 |  |
|  years after investment) | (0.00230) | (0.00471) |  |  | (0.0134) | (0.0467) |  |
| ∆ (KW investment ×  |  | 0.000170 |  |  |  | 0.00174 |  |
|  years after investment)² |  | (0.000704) |  |  |  | (0.00540) |  |
| ∆ KW investment × (0.0-2.5 |  |  | 0.0251\*\* |  |  |  | -0.235\*\*\* |
|  years after investment) |  |  | (0.00998) |  |  |  | (0.0631) |
| ∆ KW investment × (2.5-5.0 |  |  | 0.0381\*\*\* |  |  |  | -0.150\*\* |
|  years after investment) |  |  | (0.0120) |  |  |  | (0.0602) |
| ∆ KW investment × (5.0-7.5 |  |  | 0.0406\*\* |  |  |  | -0.0231 |
|  years after investment) |  |  | (0.0184) |  |  |  | (0.0698) |
|  |  |  |  |  |  |  |  |
| ∆ Housing characteristics (5) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 22,589 | 22,607 | 22,589 |  | 61,950 | 60,837 | 63,643 |
| Kleibergen-Paap *F*-statistic | 2717 | 1537 | 2065 |  | 6359 | 3570 | 31324 |
| Bandwidth *h* | 3.385 | 3.408 | 3.393 |  | 6.780 | 6.749 | 6.884 |
| *Notes:* The instruments are ∆ 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 (*p­*-value). After 7.5 years, the effect is essentially zero. The same holds if we include a second-order term in Column (5). Figure 4b 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 is essentially zero after five years. Hence, these outcomes confirm the theoretical predictions listed in Section II that place-based investments have a permanent effect on house prices, whilst only a temporary effect on sales time effect because the market has to adjust to a new steady state. The results for sales time give us also more confidence in the results for house prices. Recall that house prices and sales time tend to be negatively correlated. Let us suppose now that our house price results are completely spurious due to omitted variables. In that case, one would also expect to observe a permanent effect on sales time, in contrast to our results which show a temporary effect on sales time.

(a) Effect of house prices

(b) Effect of sales time

Figure — Effect of house prices and sales time after the investment

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

## Counterfactual analysis

We aim to gain insight in the rate of return of the *externa*l effect of the revitalisation policy using a counterfactual analysis. We reiterate that we measure external effects because we focus on investments in the public housing stock on the prices and sales times of owner-occupied properties. Expenditures through the KW programme were financed from additional and external sources and were not part of the municipal budget or the budget of housing associations. In contrast, when expenditures are e.g. raised by limiting expenses in

|  |
| --- |
|  Table — Counterfactual analysis: benefits of the programme |
|  |  | Benefits per household *(in €)* |  | Total benefits *(in billion €)* |
|  |  | *Owner-occupied* | *All properties* |  | *Owner-occupied*  | *All properties* |
| *Price effect* |  |  |  |  |
| Baseline estimate |  | 5223 | 5063 |  | 0.481 | 1.939 |
| Long-run estimate |  | 6345 | 6151 |  | 0.585 | 2.355 |
|  |  |  |  |  |  |  |
| *Welfare effect* |  |  |  |  |  |  |
| Baseline estimate |  | 5438 | 5396 |  | 0.501 | 2.066 |
| Long-run estimate |  | 6607 | 6555 |  | 0.609 | 2.510 |
|  |  |  |  |  |  |  |
| *Notes:* The estimated benefits are in 2007 prices. The data on the number of housing units are from 2012 and obtained from Statistics Netherlands. To obtain the welfare estimates we use a vacancy rate of 3.96 percent for owner occupied housing and 6.2 percent for all properties, based on data from Statistics Netherlands. |

other neighbourhoods, this may imply that externalities are negative in non-targeted areas (Rossi-Hansberg et al., 2010). One should be very careful in interpreting the results as an overall measure of general equilibrium welfare benefits of the investment programme, but we consider them as partial equilibrium results.

We use additional data on the number of housing units from Statistics Netherlands. We estimate the benefits and costs in 2007 prices, by deflating house prices by the consumer price index, obtained from Statistics Netherlands. We assume that the average price is constant across the study period, so . To estimate the average price for owner-occupied housing in each neighbourhood, we take the average of deflated prices of all transactions in our study period. In the Netherlands, for the large majority of rental houses the rents are controlled. One may argue that even when the subsidy does not capitalise in controlled rents, renters still enjoy the positive external effects that are caused by the programme. To include these social benefits, we assume an identical percentage effect for the remainder of the market. Furthermore, we gather data on the average house prices of all properties, including rental properties, which are somewhat lower than the price for owner-occupied properties.[[29]](#footnote-29) Because the share of owner-occupied housing is small in KW-neighbourhoods (only 24 percent), the benefits are likely substantially larger. We interpret the results for all housing units as an upper-bound estimate. Table 6 reports the back-of-the-envelope calculations.

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

The total benefits for home owners are about € 0.5 billion. The results indicate the gain-to-funding ratio is about 0.5 *if* the realised investments are indeed € 1 billion. To also include the social benefits on renters, we use the average house prices of all properties. Because the share of owner-occupied housing is small in KW-neighbourhoods (only 24 percent), the benefits are now substantially larger. The results suggest a gain-to-funding ratio of 1.9, in line with Rossi-Hansberg et al. (2010). This might be accidental, because the programmes are different in many aspects. Welfare effects of the programme are again somewhat higher. When we take into account the effect on welfare on the total housing stock, the maximum gain-to-funding ratio is 2.5. However, the latter estimate is probably an overestimate when the external price effects on rental housing are less pronounced than in the owner-occupied market.

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

## Sensitivity analysis

### Introduction

In this sensitivity analysis, we subject the baseline results to a wide range of robustness checks. First, we test for potential spatial spillovers of the investment programme. Second, we will conduct a series of quasi-‘placebo’ experiments based on previous investment programmes selecting different neighbourhoods. Third, we will test robustness of our results to assumptions with respect to the bandwidth of the local linear regression approach. Fourth, we will inspect whether our results are robust to the identification strategy by employing a nonparametric propensity score matching method, rather than a regression-discontinuity approach. Fifth, we will test robustness of our results with respect to the starting date of the investment. Sixth, we employ a RDD based on price *level* differences between KW and other neighbourhoods using the full dataset. Finally, we investigate whether using the full sample, rather than repeated sales influences our results. We consider the specification in column (4) in Table 3 and Table 4 as the baseline specification because we identify the effect using all available data and we do not include potentially endogenous neighbourhood attributes.

### Spatial spillovers

Although we do not consider it as the main purpose of this paper to investigate the spatial decay of spatial externalities (as in Rossi-Hansberg et al., 2010, where more detailed information is available on the exact location of investments), we investigate in Table 7 whether there are spatial spillovers by including observations close to KW neighbourhoods.

In columns (1) and (4) we include observations within 2.5 kilometres of a KW neighbourhood, and create a dummy variable indicating whether there is a change in treatment status of adjacent neighbourhoods. The price and sales time effect have both the expected effects and a similar magnitude compared to the baseline estimates. However, the price effect and sales time effect seems to be more important in adjacent neighbourhoods (respectively 7.5 and percent). However, for sales times the effect is not statistically significantly different (*p­*-value). Also the price effect is only marginally statistically significantly different from the main treatment effect (*p­*-value). Nevertheless, it may seem not so obvious that the price and sales time effects are a bit stronger just outside KW neighbourhoods. One explanation is that the investment programme clearly points out which neighbourhoods were considered as deprived, which may have led to stigmatised property values and sales times (see McCluskey and Rausser, 2003).[[30]](#footnote-30) If the spatial spillover effect of stigmatisation is smaller than the spillover effects of the investment, then neighbourhoods adjacent to KW neighbourhoods may benefit the most from the investment.

A similar observation can be made by looking at the results from the specifications in Columns (2) and (5), where we include a dummy indicating whether the property is within 2.5 kilometre of a treated area. The instrument is a dummy indicating whether a neighbourhood is within 2.5 kilometre of a neighbourhood with a z-score of at least 7.3 after the starting date of the programme. The results indicate a profound price and sales time effect for the treated areas and for the areas within 2.5 kilometre of a treated area.

In columns (3) and (6) we improve on these specifications by including 500 metre distance band dummies that indicate whether observations are within a certain distance of KW neighbourhoods after the start of the programme. The instruments are then distance band dummies capturing the distance to neighbourhoods with a z-score of at least 7.3 after the starting date of the programme. The results show that the effect in KW neighbourhoods

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| Table — Sensitivity analysis: spatial spillovers |
|  | *Panel 1:* Δ Price per m² *(log)* |  | *Panel 2:* Δ Days on the market *(log)* |
|  | (1) | (2) | (3) |  | (4) | (5) | (6) |
|  | 2SLS-LL | 2SLS-LL | 2SLS-LL |  | 2SLS-LL | 2SLS-LL | 2SLS-LL |
|  |  |  |  |  |  |  |  |
| ∆ KW investment | 0.0361\*\*\* | 0.0420\*\*\* | 0.0410\*\*\* |  | -0.147\*\*\* | -0.153\*\*\* | -0.148\*\*\* |
|  | (0.0134) | (0.0130) | (0.0129) |  | (0.0534) | (0.0524) | (0.0530) |
| ∆ Adjacent to KW investment | 0.0722\*\*\* |  |  |  | -0.166\*\*\* |  |  |
|  | (0.0158) |  |  |  | (0.0348) |  |  |
| ∆ KW investment, <2.5km |  | 0.0561\*\*\* |  |  |  | -0.102\*\*\* |  |
|  |  | (0.00896) |  |  |  | (0.0265) |  |
| ∆ KW investment, <0.5km |  |  | 0.0723\*\*\* |  |  |  | -0.189\*\*\* |
|  |  |  | (0.0110) |  |  |  | (0.0428) |
| ∆ KW investment, 0.5-1.0km |  |  | 0.0790\*\*\* |  |  |  | -0.176\*\*\* |
|  |  |  | (0.0179) |  |  |  | (0.0443) |
| ∆ KW investment, 1.0-1.5km |  |  | 0.0541\*\*\* |  |  |  | -0.105\*\*\* |
|  |  |  | (0.0145) |  |  |  | (0.0401) |
| ∆ KW investment, 1.5-2.0km |  |  | 0.0186 |  |  |  | 0.0516 |
|  |  |  | (0.0174) |  |  |  | (0.0647) |
| ∆ KW investment, 2.0-2.5km |  |  | -0.0117 |  |  |  | 0.0680 |
|  |  |  | (0.0111) |  |  |  | (0.0680) |
|  |  |  |  |  |  |  |  |
| ∆ Housing characteristics (5) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 41,765 | 41,765 | 41,765 |  | 106,822 | 106,918 | 106,918 |
| Kleibergen-Paap *F*-statistic | 68.02 | 113.9 | 16.66 |  | 191.5 | 154.8 | 41.74 |
| Bandwidth *h* | 3.384 | 3.833 | 3.833 |  | 6.945 | 6.950 | 6.950 |
| *Notes:* The instruments are ∆ Score rule and ∆ Score rule in adjacent neighbourhood in columns (1) and (4), and ∆ Score rule and ∆ Score rule, <2.5km in columns (2) and (5). In columns (3) and (6) we include ∆ Score rule, ∆ Score rule, <0.5km, Score rule, 0.5-1.0km, Score rule, 1.0-1.5km, Score rule, 1.5-2.0km and Score rule, 2.0-2.5km as instruments.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

for house prices is somewhat stronger within one kilometre of KW neighbourhoods than in KW neighbourhoods itself, while the price effect becomes statistically insignificant beyond 1500 meters (Column (3), Table 7). However, we note that price effects close to treated areas are never statistically significantly *higher* at the five percent level. For sales times, the picture is similar: sales times have been reduced a bit more close to KW neighbourhoods (Column (6)). For example, within 500 meters of a KW neighbourhood, the sales time has been reduced with 7.2 percent. After 1500 meters, the effect is becomes statistically insignificant. But again, the sales time effect is never statistically significantly stronger than the main effect.

### Quasi-placebo experiments

We also conduct a series of quasi-‘placebo’ experiments using different classifications used in the past of deprived neighbourhoods and differences in timing of programmes to test whether the effect we found is attributable to the KW-investment programme. Table 8 reports the results.

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

In 2003 the Dutch secretary of state, Henk Kamp, published another list of the most deprived neighbourhoods in the Netherlands, which received some funding at that time (the size of the programme was however an order of magnitude smaller). There was substantial overlap (about 57 percent of the observations that are in a KW neighbourhood are also in a ‘Kamp’-neighbourhood). Neighbourhoods that are a ‘Kamp’-neighbourhood but not a KW neighbourhood are a feasible ‘placebo’-group. We therefore treat these neighbourhoods as if they are KW neighbourhoods and received funds in 2007 and exclude the observations in and close to (within 2.5 kilometres) of a KW neighbourhood and before 2003. Again, we also exclude observations within 2.5 kilometres of a ‘Kamp’-neighbourhood to avoid biases due to spatial spillovers. Columns (2) and (5) in Table 8 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 errors of the estimate is smaller than in the previous specifications. This supports the conclusion that our results indeed are driven by the KW-investment and not by other investments or a general price trend in deprived neighbourhoods.

The last quasi-placebo experiment relies on another definition of deprived neighbourhoods. There was a substantial controversy around the selection of the 83 deprived neighbourhoods. One critique was that most of these neighbourhoods were located in the suburbs of the largest cities in the Netherlands. By the end of 2009 26 additional neighbourhoods were selected that received some funding from 2010 onwards. These so-

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|  Table — 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 |
|  |  |  |  |  |  |  |  |
| ∆ Winsemius neighbourhood | -0.00702 |  |  |  | 0.192\*\*\* |  |  |
|  | (0.00564) |  |  |  | (0.0339) |  |  |
| ∆ Kamp neighbourhood |  | 0.00199 |  |  |  | 0.00266 |  |
|  |  | (0.00638) |  |  |  | (0.0400) | 0.00980 |
| ∆ KW-plus neighbourhood |  |  | -0.0125 |  |  |  | (0.0882) |
|  |  |  | (0.00784) |  |  |  |  |
|  |  |  |  |  |  |  |  |
| ∆ Housing characteristics (5) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 100,248 | 59,945 | 82,722 |  | 100,248 | 59,945 | 82,722 |
| *R*²-within | 0.545 | 0.444 | 0.460 |  | 0.060 | 0.063 | 0.038 |
| *Notes:* Standard errors are clustered at the neighbourhood level.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

called KW-plus neighbourhoods might also be considered as a valid placebo group. We therefore again treat these neighbourhoods as if they are KW neighbourhoods and exclude the observations in and close to (within 2.5 kilometres) of KW and KW-plus neighbourhoods and exclude transactions after 2009. The results in Columns (3) and (6) suggest that there is no meaningful price effect and sales time effect in these neighbourhoods before 2010, which again point to the conclusion that there seem no specific trends that are correlated with the KW programme.

### RDD set-up

The baseline specifications use local linear estimation techniques, by only selecting neighbourhoods that have z-scores that are close to a threshold, based on a bandwidth. To guide the bandwidth choice , 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 9 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

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|  Table — 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) |
|  | 2SLS-LL | 2SLS-LL | 2SLS-LL |  | 2SLS-LL | 2SLS-LL | 2SLS-LL |
|  |  |  |  |  |  |  |  |
| ∆ KW investment | 0.0363\*\*\* | 0.0344\*\* | 0.0350\*\*\* |  | -0.166\*\*\* | -0.187\*\*\* | -0.0825\* |
|  | (0.00904) | (0.0137) | (0.0110) |  | (0.0531) | (0.0609) | (0.0483) |
|  |  |  |  |  |  |  |  |
|  included | Yes | No | No |  | Yes | No | No |
| ∆ Housing characteristics (5) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 169,664 | 8,912 | 61,831 |  | 169,664 | 22,744 | 169,664 |
| Kleibergen-Paap *F*-statistic | 8581 | 594.9 | 15993 |  | 8581 | 5515 | 18727 |
| Bandwidth *h* | ∞ | 1.692 | 6.767 |  | ∞ | 3.475 | 13.900 |
| *Notes:* We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

nonparametric control function of the z-score to (19). The idea is that is the only determinant of the treatment status, implying that will capture any correlation between and . Hence:

|  |  |
| --- | --- |
| (27) |  |

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

|  |  |
| --- | --- |
| (28) |  |

where and and are additional parameters to be estimated. Columns (1) and (4) indicate that this procedure leads to very similar results. The price effect is 3.7 percent and the sales time effect is 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 3 and Table 4). Columns (2) and (3) show that the price effect is essentially unaffected when we vary the bandwidth. The sales time effect is also similar once we select a bandwidth that is half the size of the optimal bandwidth. When we double the bandwidth in column (6) the sales time effect is somewhat lower. However, the effect is not statistically significantly lower compared to the baseline estimate.

### 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:

|  |  |
| --- | --- |
| (29) |  |

where is the probability that a neighbourhood is selected, is the cumulative distribution function of the normal distribution and is a nonparametric function of attributes . 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 on depends on the location of the neighbourhood. The kernel weights for are equal to , where is a vector capturing the kilometre distance between the centroid of 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 A2 in the Appendix reports the means and standard deviations at the neighbourhood

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|  Table — 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) |
|  | OLS | OLS | OLS |  | OLS | OLS | OLS |
|  |  |  |  |  |  |  |  |
| ∆ KW investment | 0.0426\*\*\* | 0.0500\*\*\* | 0.0407\*\*\* |  | -0.204\*\*\* | -0.208\*\*\* | -0.190\* |
|  | (0.0118) | (0.0101) | (0.0105) |  | (0.0570) | (0.0719) | (0.0965) |
|  |  |  |  |  |  |  |  |
| ∆ Housing characteristics (5) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 15,295 | 11,385 | 9,851 |  | 15,295 | 11,385 | 9,851 |
| *R*²-within | 0.519 | 0.507 | 0.487 |  | 0.063 | 0.066 | 0.067 |
| Matching method | Calipher | NN no repl. | NN repl. |  | Calipher | 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 |

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.[[31]](#footnote-31) Table 10 reports the results.

Columns (1) and (4) use the set of control neighbourhoods based on calipher matching. The price effect is then 4.4 percent, similar to baseline specifications. The effect on sales times is somewhat larger and 18.5 percent. In Columns (2) and (5) we use nearest neighbour matching without replacement. It can be seen that the price effect of place-based policies is again similar to the baseline specification, while the effect of sales time is again somewhat larger in magnitude. The results suggest that the investments have led to a decrease in sales time of 18.8 percent, which is still in the same order of magnitude as our baseline estimates. In Columns (3) and (6) we use nearest neighbour matching with replacement. This implies that we have only 38 control neighbourhoods. The price effect, however, is still very similar. The effect on sales times also similar but only marginally statistically significant due to large standard errors.

|  |
| --- |
| Table — 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) |
|  | 2SLS-LL | 2SLS-LL | 2SLS-LL |  | 2SLS-LL | 2SLS-LL | 2SLS-LL |
|  |  |  |  |  |  |  |  |
| ∆ KW investment | 0.0325\*\*\* | 0.0330\*\*\* | 0.0393\*\*\* |  | -0.186\*\*\* | -0.172\*\*\* | -0.165\*\*\* |
|  | (0.0110) | (0.0111) | (0.0123) |  | (0.0509) | (0.0507) | (0.0571) |
|  |  |  |  |  |  |  |  |
| ∆ Housing characteristics (5) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 22,589 | 22,562 | 15,795 |  | 64,150 | 64,810 | 89,742 |
| Kleibergen-Paap *F*-statistic | 3245 | 2256 | 2047 |  | 9129 | 9062 | 14025 |
| Bandwidth *h* | 3.382 | 3.380 | 3.047 |  | 6.949 | 6.973 | 8.551 |
| *Notes:* We exclude observations within 2.5 kilometres of targeted areas in Columns (1), (2), (4) and (5). Standard errors are clustered at the neighbourhood level.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

### 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 11 we take the official announcement as alternative starting date. It is shown that the effect on house prices and sales times is very similar to the specifications reported in Column (4) in Table 3 and Table 4. Columns (2) and (5) take January 1, 2008 as a starting date. The effects are very again very similar. In Columns (3) and (6) we just avoid the problem by excluding transactions that took place in 2007. The price and sales time effects are again very comparable to the baseline estimates. Hence, although the exact starting date of the programme is somewhat unclear, it does not seem to bias our results.

### RDD in levels

In principle, when the set-up of the regression-discontinuity design is valid, we may also compare price differences in levels after the treatment has taken place. This set-up requires stronger identifying assumptions because all time-invariant *and* time-varying unobservable factors should be uncorrelated to the treatment around the cut-off. When we identify the effect based on changes, then only time-varying unobservables should be uncorrelated to the treatment around the cut-off. Moreover, because many (unobservable) factors that influence prices are omitted, the approach using variation in price and sales time *levels* may be quite inefficient. While keeping these limitations in mind, we take such an approach and report results in Table 12.

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

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

### Full sample

We have used repeated sales and first-differencing to estimate the effects of interest. However, one may argue that repeated sales are a non-random sample of the full sample of houses. For example, it might be that the most attractive houses are sold less often, because people have fewer incentives to move. We showed that there are hardly structural differences between the full sample and the repeated sales sample (see Table 2 and Table A1

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| --- |
| Table — Sensitivity analysis: RDD in levels |
|  | *Panel 1:* Price per m² *(log)* |  | *Panel 2:* Days on the market *(log)* |
|  | (1) | (2) | (3) |  | (4) | (5) | (6) |
|  | SRD | FRD | FRD |  | SRD | FRD | FRD |
|  |  |  |  |  |  |  |  |
| KW investment | 0.0367 | 0.0203 | 0.0360 |  | -0.165\*\*\* | -0.150\*\*\* | -0.0809\* |
|  | (0.0480) | (0.0539) | (0.0516) |  | (0.0370) | (0.0374) | (0.0462) |
|  |  |  |  |  |  |  |  |
| Housing characteristics (16) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| Neighbourhood characteristics (5) | No | No | Yes |  | No | No | Yes |
| Land use variables (4) | No | No | Yes |  | No | No | Yes |
| Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 21,171 | 22,156 | 24,705 |  | 180,993 | 119,614 | 194,491 |
| *R*² | 0.352 |  |  |  | 0.060 |  |  |
| Kleibergen-Paap *F*-statistic |  | 60.54 | 41.96 |  |  | 4111 | 443 |
| Bandwidth *h* | 1.162 | 1.158 | 1.244 |  | 6.667 | 5.330 | 6.856 |
| *Notes:* We only include observations after the treatment started. We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

|  |
| --- |
| Table — Sensitivity analysis: full sample |
|  | *Panel 1:* Price per m² *(log)* |  | *Panel 2:* Days on the market *(log)* |
|  | (1) | (2) | (3) |  | (4) | (5) | (6) |
|  | OLS | FRD | FRD |  | OLS | FRD | FRD |
|  |  |  |  |  |  |  |  |
| KW investment | 0.0575\*\*\* | 0.0429\*\*\* | 0.0385\*\*\* |  | -0.163\*\*\* | -0.198\*\*\* | -0.225\*\*\* |
|  | (0.0112) | (0.0127) | (0.0114) |  | (0.0376) | (0.0396) | (0.0404) |
|  |  |  |  |  |  |  |  |
| Housing characteristics (16) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| Neighbourhood characteristics (5) | No | No | Yes |  | No | No | Yes |
| Land use variables (4) | No | No | Yes |  | No | No | Yes |
| Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| PC6 fixed effects  | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 1,393,246 | 140,932 | 140,921 |  | 1,393,246 | 343,395 | 336,314 |
| *R*² | 0.444 |  |  |  | 0.099 |  |  |
| Kleibergen-Paap *F*-statistic |  | 4989 | 2579 |  |  | 16268 | 7849 |
| Bandwidth *h* |  | 3.228 | 3.225 |  |  | 6.164 | 6.137 |
| *Notes:* We exclude observations within 2.5 kilometres of targeted areas. Standard errors are clustered at the neighbourhood level.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

in the Appendix). 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 13 reports the results.

In Columns (1) and (4) we simply regress respectively house price and sales time on whether the neighbourhood is treated and a host of housing control variables (listed in Table A1 in the Appendix). The coefficients suggest a positive price effect of the programme of 5.9 percent. Sales times have been reduced with 15.0 percent. In Columns (2) and (4) we employ the fuzzy regression-discontinuity design. The price effect is then somewhat lower (4.4 percent), while the sales time effect is somewhat stronger ( percent). In Columns (3) and (6), Table 13, we also control for neighbourhood characteristics and changes in land use. The price effect is again slightly lower but similar (3.4 percent). The investment programme has reduced sales times with 20.1 percent. In general, we may conclude that the results using the full sample are very similar to the baseline results.

## Conclusions

In many countries, governments invest in deprived neighbourhoods to narrow income disparities within cities and fight social problems. There is limited understanding to what extent place-based policies may be effective in improving neighbourhood quality. One justification for place-based policies is that they may reduce negative externalities in the housing market. In the current paper, we examine whether place-based policies that aim to improve public housing in certain neighbourhoods improves the attractiveness of these neighbourhoods for households in the owner-occupied market. We set up a theoretical model including housing search and matching, in which we analyse the effects of place-based investments on house prices and sales times. We show that place-based investments capitalise into house prices, and temporarily reduce sales times. Given that the market is in spatial equilibrium, we show that price changes are good measures of welfare changes.

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

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## Appendix

### A.1 Theory: solving the model

We first solve the steady state before and after the investments to determine the long-run effects of changes in the amenity level of the neighbourhood. We determine the exogenous parameters , , , , and . To find an optimum, the costs of search should be convex in search effort and the matching rate should be concave in search effort. We then assume:

|  |  |
| --- | --- |
| (A1) |  and  |

To solve the model, we first pick a starting value for and calculate the starting values for , and . Then we determine the present values for each state and calculate the optimal level of search effort using equation (7). We then update , , , , , and the present values. We iterate this procedure until search effort converges.

To determine the short-run effect of changes in the amenity level, we use the steady state in before the amenity change and use equations (10), (11) and (12) to determine the number of households in each state in each period. Because the optimal search effort, and therefore the house price, depend on future values of being in each state we first calculate initial values for , and using the steady state values for and . We then use these values to determine , , , , and in each period. We repeat this whole process for all time periods and update , and in each iteration until converges.

Figure A — Prices and sales times in the short-run with anticipation effects

*Notes:* We assume for , for , , , , , , , and .

### A.2 Theory: anticipation effects

It may be the case that place-based investments that increase the amenity level in a neighbourhood are announced before the investments take place. Prices and sales times are then expected to adjust before the actual investments take place because the utility of households in each state is dependent on future values. In Figure A1 we show the results. Indeed, prices jump once the announcement is made (5 years before the actual treatment takes place). The immediate drop in sales time is small, and then sales times decrease until . After that, sales time return to the steady-state value.

### A.3 Other descriptive statistics

Table A1 reports the descriptive statistics for the full sample. The descriptives of the full sample seem to suggest that houses inside KW neighbourhoods are somewhat more expensive than properties located outside the treated areas. Again, this is mainly because the targeted areas are disproportionally located in larger cities. The selling time of properties in the full sample is somewhat higher (about 20 percent) than properties in the repeated sales sample. Another difference between the full sample and repeated sales sample is that houses tend to be somewhat smaller and more often apartments in the latter sample. This is, most likely, because housing mobility in cities tends to be higher. Houses in cities are also smaller and the share of apartments is higher.

 In Figure A2 we show that the cumulative distribution of z-scores. An assumption of the regression-discontinuity design is that the running variable is continuous around the threshold (McCrary, 2008). When we zoom in on the neighbourhoods around the threshold (right panel), the figure strongly suggests that the distribution of z-scores around the threshold is continuous.

|  |
| --- |
| Table A — Descriptive statistics for full sample |
|  | Observations outside KW neighbourhoods |  | Observations inside KW neighbourhoods |  |
|  |  |  |  |  |  |  |  |  |  |
|  |  |  |  |  |  |  |  |  |  |
| House priceper 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 –good  | 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.  |

Figure A2 — Cumulative distribution of z-scores

|  |
| --- |
| Table A — Descriptive statistics for propensity score matching |
|  | KW neighbourhoods |  | Control neighbourhoods |
|  |  |  | Calipher matching,  | Nearest neighbour matching without replacement | Nearest neighbour matching with replacement |
|  |  |  |  |  |  |  |  |  |  |
| Population density *(ha2)* | 9,081.000 | 5,171.000 |  | 5,601.000 | 4,352.000 | 5,965.000 | 4,233.000 | 6,804.000 | 4,476.000 |
| Income | 10,965.000 | 1,050.000 |  | 11,866.000 | 1,166.000 | 11,634.000 | 1,188.000 | 11,670.000 | 1,263.000 |
| Median construction year | 1,950.000 | 24.000 |  | 1,947.000 | 90.000 | 1,957.000 | 21.000 | 1,953.000 | 22.000 |
| 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. |

Table A2 reports the means and standard deviations at the neighbourhood level for the KW neighbourhoods and three different sets of control neighbourhoods using the propensity score matching method (see Section VI.E). It appears that the control neighbourhoods are relatively similar to the KW neighbourhoods in most neighbourhood attributes. There are two notable differences between the targeted and control neighbourhoods. The first is that population density is about a third lower in the control neighbourhoods. Indeed, targeted areas are on average located in larger cities. Furthermore, the share of foreigners is much lower in control neighbourhoods. We note that the propensity scores of neighbourhoods that are neither targeted nor control neighbourhoods are very close to zero, suggesting that the method performs reasonably well.

### A.4 First-stage regression results

Table A3 report the first-stage regression results, where we regress the change in the KW-investment status on the change in the scoring rule (the scoring rule is zero before the programme was launched). The coefficient related to the scoring rule is close to one, but significantly lower than one (at the five percent level) in all specifications. When the coefficient would be equal to one, the specifications would be identical to a sharp regression-discontinuity design.

|  |
| --- |
| Table A — First stage regression results*(Dependent variable: change in KW-investments)* |
|  | *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 |
|  |  |  |  |  |  |  |  |
| ∆ Score rule  | 0.979\*\*\* | 0.982\*\*\* | 0.970\*\*\* |  | 0.989\*\*\* | 0.988\*\*\* | 0.985\*\*\* |
|  | (0.0133) | (0.0109) | (0.0191) |  | (0.00776) | (0.00811) | (0.0100) |
|  |  |  |  |  |  |  |  |
| Control variables included (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
| ∆ Year fixed effects (14) | Yes | Yes | Yes |  | Yes | Yes | Yes |
|  |  |  |  |  |  |  |  |
| Number of observations | 22,589 | 12,766 | 10,484 |  | 64,324 | 22,447 | 36,905 |
| First-stage *R*²-within | 0.957 | 0.956 | 0.951 |  | 0.965 | 0.963 | 0.966 |
| Kleibergen-Paap *F*-statistic | 5444 | 8063 | 2571 |  | 16228 | 14819 | 9660 |
| Bandwidth  | 3.383 | 4.312 | 3.547 |  | 6.950 | 6.147 | 7.645 |
| *Notes:* Standard errors are clustered at the neighbourhood level.  \*\*\* Significant at the 0.01 level \*\* Significant at the 0.05 level \* Significant at the 0.10 level |

1. \* This work has benefited from a VENI research grant from the Netherlands Organisation for Scientific Research. We thank NVM, ABF Research and Statistics Netherlands for providing data. We thank Felipe Carozzi, Jens Suedekum, Maximilian von Ehrlich, the seminar audiences at the University of Tokyo, IDE-JETRO, Düsseldorf Institute for Competition Economics, the IEB Urban Economics Workshop in Barcelona, NARSC conference in Washington, Tinbergen Institute Amsterdam, and the ERSA conference in Palermo for constructive comments. [↑](#footnote-ref-1)
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4. Conditional on the housing stock, house prices (which reflect building costs as well as land prices) can then be interpreted as land prices. [↑](#footnote-ref-4)
5. This is in sharp contrast to the labour market literature where search frictions are at the core of the debate and have been extensively analysed for more than two decades (e.g. Pissarides, 1987; 1994). [↑](#footnote-ref-5)
6. This is particularly true, because changes over time in house prices and sales time are strongly negatively correlated (see Koster and Van Ommeren, 2016). If the results of the hedonic price model indicate a permanent effect of place-based policies, but these results are spurious due to omitted variable bias, then one would expect to find a permanent effect of place-based policies on sales time as well. [↑](#footnote-ref-6)
7. The scheme was also known as *aandachtswijken*-scheme or *Vogelaarwijken*-scheme. [↑](#footnote-ref-7)
8. Hence, we allow house price *trends* to be neighbourhood-specific, so trends in neighbourhood unobserved variables are allowed to be correlated with the selection of targeted neighbourhoods. [↑](#footnote-ref-8)
9. To further control for potential biases, we control for housing attributes, a flexible function of the deprivation score, and include additional fixed effects that capture unobserved trends. [↑](#footnote-ref-9)
10. This assumption is also in line with the empirical hedonic price literature in which it is usually assumed that *the logarithm* of the house price (rather than the house price in levels) is a linear function of housing and neighbourhood attributes [↑](#footnote-ref-10)
11. Notice further that expenditure is strongly increasing in household income (although the elasticity is usually thought to be less than one). [↑](#footnote-ref-11)
12. Equivalently, one may assume that housing supply $S$ changes in response to changes in the house price. However, for the practical implications of this model, this does not matter. [↑](#footnote-ref-12)
13. Koster and van Ommeren (2016) show that this result also holds in the equilibrium given less restrictive assumptions, and when $c$ and $s$ endogenously change to an increase in $k$. [↑](#footnote-ref-13)
14. We consider the case where the change in the amenity level is announced a few years before implementation in the Appendix. [↑](#footnote-ref-14)
15. Welfare calculations for the short run are less useful, because these investments have a long time span. [↑](#footnote-ref-15)
16. Due to substantial benefit transfers, differences in Dutch household income are less pronounced than in the US. [↑](#footnote-ref-16)
17. Public housing is common in the Netherlands: about 35 percent of residences are public housing, which is by far the highest in Europe. [↑](#footnote-ref-17)
18. 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. [↑](#footnote-ref-18)
19. In the (large) cities we focus on, the NVM has a more dominant position, so the 80 percent is likely an underestimate. The figure may be as high as 90 percent. [↑](#footnote-ref-19)
20. 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 25m2 or larger than 250m2. We drop a few properties that are sold more than five times in our study period or more than twice in one year and are listed for more than five years on the market or were listed zero days on the market. Based on the distribution, we also drop observations for which the percentage transaction to asking price is below 70 or above 110 percent. These selections do not influence the results. [↑](#footnote-ref-20)
21. Using repeated sales may imply a selection problem, because certain house types may be sold less often. In Section VI.G, we check whether our results are robust with respect to this selection. [↑](#footnote-ref-21)
22. 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. [↑](#footnote-ref-22)
23. In the sensitivity analysis (Section VI.B), we investigate whether there are spatial spillovers of the investment. [↑](#footnote-ref-23)
24. One may also estimate a cross-sectional RDD by comparing treated neighbourhoods with non-treated neighbourhoods after the treatment has taken place. We think that the latter set-up requires stronger identifying assumptions because all time-invariant *and* time-varying unobservable factors should be uncorrelated to the treatment, rather than time-varying unobservables only. Nevertheless, if the RDD set-up is valid, this should not affect the consistency of the parameters. However, because many (unobservable) factors that influence prices and sales times are omitted, the approach may be quite inefficient. Indeed, in Section VI.G, we show that point estimates are similar to the baseline estimate, but the confidence intervals are substantially wider. [↑](#footnote-ref-24)
25. The marginal effect is calculated as $e^{\hat{α}}-1$. [↑](#footnote-ref-25)
26. Note that the jump in probability to become treated is higher than recorded in Figure 2, because neighbourhoods are not of equal size (in terms of the number of housing units). [↑](#footnote-ref-26)
27. We include variables related to changes in land use using data from Statistics Netherlands for 2000, 2003, 2006, 2008 and 2010. We match each transaction year to the nearest preceding year of the land use data. This may lead to some bias, but as the average time difference between transactions of the same property is almost four years, we expect that the bias is limited. We then calculate the share of land used for housing, commercial activities, infrastructure and open space for each neighbourhood. [↑](#footnote-ref-27)
28. The bandwidth is optimised assuming that the interaction terms are exogenous variables. Given that the bandwidth is very similar for the SRD and the FRD, we do not expect that this has any impact on the results. [↑](#footnote-ref-28)
29. We ignore that house owners can deduct their interest mortgage payments from their income, so prices may somewhat exceed house prices compared to an unregulated market. [↑](#footnote-ref-29)
30. The list of targeted neighbourhoods was initially not made public because the Secretary of State was afraid of a negative stigmatisation effect. Nevertheless, in late 2008, the secretary of state was forced to disclose the list with the ranking under pressure of the press. [↑](#footnote-ref-30)
31. 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. [↑](#footnote-ref-31)