Place-based policies and the housing market: Search, bargaining and house prices

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SUMMARY — We study the economic effects of place-based policies on the housing market. We develop a two-neighbourhood search and bargaining model. The model shows that, given an amenity-improving place-based policy, house prices increase, whereas sales times, and therefore housing vacancies, decrease. We demonstrate that despite the presence of search frictions, it is reasonable to use price changes as an indication of welfare benefits. We use a Dutch nationwide dataset with repeated sales to test these predictions for policies that aim to improve 83 impoverished neighbourhoods. We combine a first-difference approach with a (fuzzy) regression-discontinuity design to take into account that a neighbourhood’s treatment probability is endogenous. House prices have increased with 2.5–4.0 percent. It is shown that the sales time is reduced with 16–26 days (15–25 percent). The presence of search frictions indicate that price changes are an underestimate of welfare changes.

JEL-code — R30, R33

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

I. Introduction

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

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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. Also, investments in deprived inner cities in the US seem to be ineffective, as the income disparities in these cities have increased in the last decades (Mills and Lubuele, 1997).

Most of these empirical studies focus 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. However, this assumes away the presence of search frictions and may therefore overlook several essential features of owned-housing markets, including the presence of housing vacancies and the fact that it takes time to sell a house (Harding et al., 2003; Genesove and Mayer, 2001; Merlo and Ortalo-Magné, 2004).

Our contribution is to study place-based policies that affect the well-being of local homeowners via the amenity level of a neighbourhood, when taking search frictions into account. Our starting point is that households aim to move residence. For example, the birth of a child may lead to changes in housing demand. Because households are unable to buy and sell at the same moment, some households possess two houses, of which one is vacant. Building on the seminal study of Wheaton (1990), we introduce a two-neighbourhood housing market model with endogenous buyer search effort, house prices, vacancies as well as sales times (or: time on the market). This model enables us to study the effects of place-based investments that increase the amenity level in one neighbourhood, so we relax the symmetry assumption. The place-based investment induces higher prices as well as shorter sales time, also compared to the non-treated neighbourhood where prices and sales time also might change. In the type of model analysed, the welfare effects of policies tend to be ambiguous, because welfare depends on whether the level of search effort is efficient in equilibrium. We show that households search not necessarily less than socially desired.

Assuming that housing search effort is reasonably close to the level which maximises social welfare, we show that place-based investments lead to social benefits and changes in housing prices induced by these investments are a reasonable measure of the effects on social welfare. We also show that price change due to the place-based investment will be an underestimate of welfare changes when search and bargaining are important.

In the empirical application, we analyse the effects of place-based policies on house prices and sales time. We evaluate changes in local amenity levels due to a large-scale nationwide

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1 Conditional on the housing stock, house prices (which reflect building costs as well as land prices) can then be interpreted as land prices.
2 This is a sharp contrast to the labour market literature where search frictions are at the core of the debate and have been analysed for more than two decades (e.g. Pissarides, 1987; 1994).
3 Housing models that incorporate search and bargaining (e.g. Wheaton, 1990; Taylor, 1999; Genesove and Han, 2012) are fundamentally non-spatial, which is essential to study place-based policies.
4 We improve on Wheaton (1990), which concludes erroneously that households always search less than socially desired. Our improvement is in line with a number of studies which show that previous welfare results determined for search models were incorrect (Pissarides, 1987; 2000).
urban revitalisation programme in the Netherlands, starting in 2007. In this so-called *krachtwijken*-programme (henceforth: KW investment scheme), 83 postcode areas were selected for revitalisation with funding from the national government. The government and (not for profit) public housing associations invest about 2.75 billion Euros in these areas from 2007 onwards, on average about 3.5 thousand Euros per household in receiving neighbourhoods. The main objectives of the programme are 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 investments is spent on restructuring of the public housing stock. The remainder is 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 restructured.

We utilise a nationwide dataset with information on (privately-owned) house transactions from 2001 to 2011, 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 have more external validity and neighbourhood sampling error is eliminated, which plagues interpretation of previous studies that include one or a couple of targeted neighbourhoods.

Importantly, 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 (RDD) by using information on an eligibility criterion to receive investments. This criterion is dependent on so-called deprivation scores, calculated by the national government for the whole 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). This fuzzy regression-discontinuity design identifies the local average treatment effect of the policy exactly at the threshold, but we show that the local average treatment effect is almost identical to baseline OLS estimates that may identify an average treatment effect.

We find that due to investments house prices increased by about 2.5-4.0 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 much stronger as it is reduced with 15-25 percent (16-26 days). The latter result indicates that search is a non-negligible feature of the housing market. The empirical results survive remarkably when we extensively check for robustness, for example by testing for spatial spillovers, conducting quasi-placebo experiments and using propensity

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5 The scheme was also known as aandachtswijken-scheme or Vogelaarwijken-scheme.
6 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.
7 To further control for potential biases, we control for housing attributes and a flexible function of the deprivation score and include fixed effects that capture unobserved trends.
score matching rather than a RDD. The results of a counterfactual analysis suggests that the welfare benefits of the inhabitants who were living in the treated neighbourhood before the investment are higher than indicated by the price increase.

The remainder of the paper is organised as follows. In Section II we present a house price bargaining model in which we analyse the effects of place-based policies on search behaviour and house prices. In Section III we discuss the features of the KW-investment scheme, the data and the econometric framework. Section IV turns to the results. We subject the baseline results to an extensive sensitivity analysis in Section V, which is followed by a counterfactual analysis in Section VI. Section VII concludes.

II. Place-based policies and the housing market: a house price bargaining model

A. Matching and sales time

We analyse a housing market with search frictions where households may move, but not freely, between neighbourhoods. We extend the seminal house price bargaining model by Wheaton (1990), which can be interpreted as a model with two symmetric neighbourhoods. We relax the symmetry assumption, which is essential to analyse investments in one of the neighbourhoods and analyse price and sales time differences due to neighbourhood-specific investments that increase the amenity level. Moreover, we improve on the welfare analysis.

Assume a city with a given number of home-owning households. The city consists of two neighbourhoods with the same number of houses.\(^8\) Households may own two houses. All houses, including vacant ones, are owned by a household implying that \(\bar{V}/\bar{S} < 0.5\) and \(2\bar{H} - \bar{S} > 0\) where \(\bar{H}, \bar{S}\) and \(\bar{V}\) denote the average number of households, houses and vacant houses per neighbourhood respectively, which are all assumed to be exogenous.

Households prefer to live in one of the two neighbourhoods. Let \(H_i\) denote the number of households who prefer to live in neighbourhood \(i\), where \(i = 1, 2\). Households who live in their preferred neighbourhood are matched, otherwise they are mismatched. Mismatched households search for housing in the other neighbourhood. Dual-ownership households have a house in both neighbourhoods, are therefore matched, but own a vacant house in the other neighbourhood.

Preferences for a neighbourhood change over time at an exogenous rate \(\beta.\(^9\) The change over time in \(H_i\) denoted by \(\dot{H}_i = \beta(\bar{H}_j - H_i)\) for \(i \neq j\). In what follows, \(j\) will always denote the other neighbourhood. The total endogenously determined number of households that have a preference to reside in \(i\) is given by \(H_i = H_i^M + H_i^D + H_i^S\), where \(H_i^M\) is the number of matched households with one house, \(H_i^D\) is the number of households possessing two houses, and \(H_i^S\) denotes the number of mismatched households.

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\(^8\) Introducing exogenous housing supply that differs between neighbourhoods is not fundamental here.

\(^9\) For example, the location of the job may be exogenously changed from one neighbourhood to the other due to a firm relocation (Mulalic et al., 2014). Changes over time are denoted with a dot.
By construction, the number of vacancies in a neighbourhood $V_i$ equals housing supply $\bar{S}$ minus the number of households living there. Because mismatched households $j$ live in neighbourhood $i$, the following holds:\(^{10}\)

\[
V_i = \bar{S} - H_i^D - H_i^M - H_i^S.
\]  

(1)

Each vacant house is owned by a household that lives in the other neighbourhood, so $H_i^D = V_j$ where $i \neq j$. Hence, the number of vacancies in neighbourhood $j$ is equal to the number of dual-ownership households living in neighbourhood $i$. Because the number of vacancies in the city is fixed and $H_i^D = V_j$ (where $i \neq j$), it follows that $\dot{H}_i^D + \dot{H}_j^D = \dot{V}_i + \dot{V}_j = 0$. Using (1) it follows that $H_i^D + H_j^D = 2 (\bar{S} - \bar{H})$.

Mismatched households search for houses in the other neighbourhood and given a contact with a vacancy in that neighbourhood, they find a house with probability one. Matching of mismatched households and vacant houses, and therefore the sales of vacant residences, occur with a Poisson process. The sales rate $q_i$ equals the number of matches, $M_i$, so the product of the number of mismatched households of type $i$ and $m_i$, the matching rate of a mismatched household $i$, divided by the number of vacancies in neighbourhood $i$:

\[
q_i = \frac{M_i}{V_i} = \frac{m_iH_i^S}{V_i}.
\]

(2)

Given the above assumptions, following Wheaton (1990), the first-order differential equations that indicate how households change type and move between neighbourhoods are given by:

\[
\dot{H}_i^S = -(m_i + \beta)H_i^S + \beta H_i^M,
\]

(3)

\[
\dot{H}_i^D = -(q_j + \beta)H_i^D + m_iH_i^S + \beta H_i^D,
\]

(4)

\[
\dot{H}_i^M = -\dot{H}_i^S - \dot{H}_i^D, \quad i \neq j
\]

(5)

which provides a stable model of changes in household type as well as residential moving.

We emphasise that the matching rate is given here, but will be endogenously determined by household search behaviour as well as the endogenously determined number of vacancies and mismatched households in both neighbourhoods, as discussed later on.

\[ \text{B. Steady-state} \]

We will now assume steady-state. Hence, $\dot{H}_i = H_j = 0$, and $H_i = H_j = \bar{H}$.\(^{11}\) Using (2) and (4) it holds that $q_j = q_i V_i / V_j + \beta (V_i - V_j) / V_j$. In our empirical application, we will focus on sales time difference between neighbourhoods, so we focus on the sales time ratio between neighbourhoods. Here, sales time $L_i$ is defined as $1/q_i$. Importantly, the latter equality

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\(^{10}\) In our notation, Wheaton (1990) states erroneously that $V_i = \bar{S} - H_i^D - H_i^M - H_i^S = \bar{S} - H_i$. Because he assumes symmetry later on, this implies that $H_i^S = H_i^S$, so this has no consequences for his analysis.

\(^{11}\) In addition, it can be shown that the number of households who move residence from one neighbourhood into the other must be equal to the number of households who move the other way round. It can be shown that $m_i H_i^S = m_i H_i^S + \beta (H_i^D - H_i^D)$, where the last term denotes the net number of dual ownership households who move from $i$ to $j$, not because they have bought a new house, but because they have changed preference.
implies that there is a positive relationship between the sales time and vacancy ratios \( \left( \frac{\partial (L_i/L_j)}{\partial (V_i/V_j)} = \frac{(1 + \beta L_i)}{(1 + \beta L_j)} > 0 \right) \). This a convenient result, because it implies that in our empirical application it is sufficient to use data on sales times, and we do not need data on vacancies.

Conditional on the matching rates \( m_i \) and \( m_j \) (determined in the next subsection), it is straightforward to obtain explicit expressions for the matching rates. It can be shown that:

\[
H_i^S = \frac{\beta (2\bar{H} - \bar{S})}{2\beta + m_i},
\]

which is positive. Given the latter equality, we obtain the intuitive result that the number of mismatched households in a neighbourhood depends negatively on the matching rate \( m_i \) in that neighbourhood (but does not depend on the matching rate of the other neighbourhood \( m_j \)).

The number of dual-ownership households in \( i \) can be written as:

\[
H_i^P = \bar{S} - \bar{H} + \frac{\beta (2\bar{H} - \bar{S})}{2\beta + m_j} - \frac{\beta (2\bar{H} - \bar{S})}{2\beta + m_i},
\]

which depends positively on \( m_i \) but negatively on \( m_j \). The latter is intuitive when it is understood that a higher matching rate \( m_j \) increases the probability that a vacant property in \( j \) will be sold. In other words, due to a higher matching rate in the other neighbourhood, households have a lower probability to possess two properties.

Similarly, the number of matched households in \( i \) can be written as:

\[
H_i^M = 2\bar{H} - \bar{S} - \frac{\beta (2\bar{H} - \bar{S})}{2\beta + m_j},
\]

which is an increasing function of the matching rate in the other neighbourhood \( m_j \). This result is intuitive, because a higher \( m_j \) decreases the number of households who live in \( i \) and own a vacant property in \( j \). Note that \( H_i^M \) does not depend on the matching rate \( m_i \). The above equation implies that because the vacancy rate cannot exceed the matching rate, \( H_i^S \) is also always less than 0.5.

The sales rate can be written as:

\[
q_i = \frac{\beta (2\bar{H} - \bar{S})m_i (2\beta + m_j)}{S(m_i (3\beta + m_j) + \beta (4\beta + m_j)) - H(4\beta (\beta + m_i) + m_i m_j)}.
\]

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12 Note that a positive relationship between sales time and vacancy levels is not self-evident. One of the main messages of Wheaton (1990) is that at the national level (in our model, city level), this relationship is non-monotonic, because a higher vacancy level goes together with a higher number of matches and therefore shorter sales times, but at low vacancy levels, there is a negative relationship when the market is ‘frozen’.

13 Note that when \( m_i \) approaches infinity, so search frictions in \( i \) are absent, the number of mismatched households of type \( i \) (who live in \( j \)) approaches zero.

14 When the matching rates for both neighbourhoods approach infinity (no search frictions in the city), the number of dual ownership households in a neighbourhood is equal to the average number of housing units minus the average number of households.
It is then easy to see that $\partial q_i / \partial m_i > 0$ and $\partial q_i / \partial m_j < 0$, so the sales rate depends positively on the matching rate in a neighbourhood, but negatively on the matching rate in the other neighbourhood.\textsuperscript{15} It follows that $\partial L_i / \partial m_i < 0$ and $\partial L_i / \partial m_j > 0$.

The ratio of vacancies and mismatched/searching households in a neighbourhood will play an important role in the model, as it determines the matching function. This ratio can be written as a function of parameters:

\begin{equation}
\frac{V_i}{H_i^S} = \frac{H_i^P}{H_i^S} = \frac{\beta \bar{S} + m_i(\bar{S} - \bar{H})}{\beta(2\bar{H} - \bar{S})} - \frac{2\beta + m_i}{2\beta + m_j},
\end{equation}

When we take the derivatives of $V_i/H_i^S$ with respect to $m_i$ and $m_j$ respectively, we have:

\begin{equation}
\frac{\partial (V_i/H_i^S)}{\partial m_i} = \frac{\bar{S} - \bar{H}}{\beta(2\bar{H} - \bar{S})} - \frac{1}{2\beta + m_j} \quad \text{and} \quad \frac{\partial (V_i/H_i^S)}{\partial m_j} = \frac{2\beta + m_i}{(2\beta + m_j)^2} > 0.
\end{equation}

It can be seen that the latter derivative is always positive, so when the matching rate in the other neighbourhood increases, sales time increases (see equation (2)). However, the effect of an increase of $m_i$ on this ratio is ambiguous.

### C. Search effort, matching and house prices

Until now, matching rates were given. We will now assume that matching rates are endogenously determined. Mismatched households choose the level of search effort $e_i$ to find a new house. Following the extensive theoretical and empirical literature on (labour market) matching (Pissarides, 2000), we assume the presence of an aggregate matching function, which depends on total search effort in the neighbourhood, $e_iH_i^S$, and $V_i$. So, $M_i = m(e_iH_i^S,V_i)$, where $m$ is increasing and concave in both arguments. The matching function $M_i$ is homogeneous of degree one, hence $m_i = m(e_i,V_i/H_i^S)$.\textsuperscript{16} It is important to recall that $V_i/H_i^S$ depends on $e_i$ through the matching rates in both neighbourhoods (see (11)), we may write $m_i = m(e_i)$.

The next step is to determine the level of search effort which is based on the households’ present discounted value of being in a certain state, which depends, among others, on house prices. We use standard Bellman techniques. In steady state, the value of a certain state can be written as the sum the utility flow of being in that state:

\begin{equation}
rU_i^M = A_i + \beta(U_i^S - U_i^M),
\end{equation}

\begin{equation}
rU_i^D = A_i + q_j(U_i^M - U_j^D + P_j) + \beta(U_j^P - U_j^D),
\end{equation}

\begin{equation}
rU_i^S = -c_i + \beta(U_i^M - U_i^S) + m_i(U_j^P - U_i^S - P_i), \quad i \neq j
\end{equation}

\textsuperscript{15} When the matching rate in a neighbourhood approaches infinity, the sales rate and corresponding sales time in this neighbourhood remains positive (and becomes a constant). We may also write $q_i\big|_{m_j=m_i} = m_i\beta(2\bar{H} - \bar{S})/((m_i + 2\beta)(\bar{S} - \bar{H}))$, which leads to $\beta(2\bar{H} - \bar{S})/(\bar{S} - \bar{H})$ when $m_i \to \infty$. This result is interesting, because it indicates that the stylised fact that we observe vacant houses and positive sales times does not necessarily implies that there are search frictions.

\textsuperscript{16} Wheaton (1990) assumes that $m_i = m_i(e_i,V_i/S_i)$, which does not depend on $H_i^S$.\textsuperscript{16}
where \(U_i^M, U_i^D, U_i^S\) are the present values of each state (matched, dual ownership, mismatched), \(A_i\) refers to the utility flow of the neighbourhood-specific amenity, \(r\) is the discount rate, \(c_i\) is the cost of search and \(P_i\) is the house price. Households are assumed to choose the level of search effort \(e_i\) to maximise \(U_i^S\).

In equation (14) it is assumed that mismatched households incur search costs as a function of search effort. \(c_i\) is a convex cost function of search effort \(e_i\): more search effort will increase the costs of search \((\partial c_i / \partial e_i > 0, \partial^2 c_i / \partial e_i^2 > 0)\) and \(c(0) = 0\). Note that \(m_i\) also depends on search effort \(e_i\) (and that searching households of type \(i\) live in neighbourhood \(j\)). Hence, given (12) and (13), only matched and dual-owners households enjoy the neighbourhood-specific amenity \(A_i\). However, this is not the case for searching households of type \(i\) who live in \(j\). We assume that the latter do not enjoy \(A_j\).\(^{17}\)

Our interest is in the marginal effect of search effort of a single mismatched household on the matching rate, conditional on the search behaviour of other mismatched households. We consider only symmetric equilibria where all households choose the same search effort level. Hence, the level of effort of the single mismatched household equals the effort of the other mismatched households. For each unit of individual search effort, the individual matching rate is the product of the individual search effort and the average number of matches per search effort (the aggregate number of matches divided by the aggregate search effort) (see Pissarides, 2000). Let \(\tilde{m}_i\) be the matching probability of an individual household and \(\tilde{e}_i\) the level of search effort for the individual household. Then:

\[
\frac{\partial \tilde{m}_i}{\partial \tilde{e}_i} \bigg|_{\tilde{e}_i = e_i} = \frac{M_i}{H_i^2 e_i} = \frac{m_i}{e_i} > \frac{\partial m_i}{\partial e_i},
\]

where the latter inequality follows because \(m_i\) is a concave function of search effort. Consequently, it follows that the marginal effect of search effort of a single mismatched household on its matching rate exceeds the marginal effect of search effort of all mismatched households on the matching rate. It also immediately follows that the second-order derivative with respect to search effort is equal to zero.

The marginal gain from an additional unit of search effort is equal to the marginal costs of that unit. Given equations (14) and (15), we have:

\[
\frac{\partial c_i}{\partial \tilde{e}_i} = \frac{\partial \tilde{m}_i}{\partial \tilde{e}_i} \bigg|_{\tilde{e}_i = e_i} (U_i^D - U_i^S - P_i),
\]

where \(U_i^D - U_i^S - P_i\) indicates the benefit of a match for searching households who aim to buy a residence.\(^{18}\)

\(^{17}\) If we would assume that unmatched households would receive a certain share of \(A_j\), this would lead to qualitatively the same conclusions.

\(^{18}\) In a partial equilibrium setting (where prices and behaviour of other households are fixed), it is straightforward to show that search effort depends negatively on the price in the neighbourhood one is searching, as it holds that \(\partial^2 U_i^S / \partial \tilde{e}_i \partial P_i = -(r + \beta)m_i) / (e_i(r + \beta + m_i)^2)\), which is smaller than zero. We will see in the next subsection that this property does not hold when house prices are endogenous.
In a general equilibrium setting, prices are endogenous. We will assume that buyers and sellers bargain about prices. We assume Nash-bargaining, where buyers get a fixed share \( \sigma \) of the total benefits of the match, which implies that \( U^D_i - U^S_i - P_i = \sigma (U^D_i - U^S_i + U^M_j - U^D_j) \) and hence \( P_i = \sigma (U^D_i - U^M_j) + (1 - \sigma) (U^D_i - U^S_i) \).

D. Place-based policies: comparative statics

We are interested in the marginal effect of place-based investment in neighbourhood \( i \). We assume that neighbourhoods are identical before treatment.\(^{19}\) We then investigate the effect of the investment, viz. an increase in the amenity level, denoted by \( v_i \). In the initial situation, \( A_i = A_j = A \). Given the investment, \( A_i = A + v_i \) and \( A_j = A \).\(^{20}\) Let us define \( w_i = r + 2\beta + \sigma m_i \). Given equations (12), (13), (14) and (16), the price in neighbourhood \( i \) is then given by as a function of structural parameters:\(^{21}\)

\[
P_i = \frac{[r + q_i(1 - \sigma) + \beta][(A + v_i + c_i)w_j + q_j(1 - \sigma)(2A + v_i + c_i + c_j)]}{r[w_iq_j(1 - \sigma) + w_jq_i(1 - \sigma) + w_jw_i]}. \tag{17}\]

Similarly, one may write the benefit of a match of a searching household as follows:

\[
U^D_i - U^S_i - P_i = \frac{\sigma[(A + v_i + c_j)w_j + q_j(1 - \sigma)(2A + v_i + c_i + c_j)]}{w_jw_i + q_iw_j(1 - \sigma) + q_jw_i(1 - \sigma)}. \tag{18}\]

Hence, the ratio of the benefit of a match of a searching household to the house price is equal to \( \sigma (r + q_i) (1 - \sigma) + \beta > 0 \).

The price in neighbourhood \( j \) is given by:

\[
P_j = \frac{[r + q_j(1 - \sigma) + \beta][(A + v_i + c_i)w_i + q_i(1 - \sigma)(2A + v_i + c_i + c_j)]}{r[w_iq_j(1 - \sigma) + w_jq_i(1 - \sigma) + w_jw_i]}. \tag{19}\]

Combining (16), (17), (18) and (19), we can write the ratio of prices in the neighbourhoods as a function of search effort in both neighbourhoods:

\[
\frac{P_i}{P_j} = \frac{\frac{\partial c_i}{\partial e_i} (\frac{m_i}{e_i})^{-1} (r + q_i) (1 - \sigma) + \beta}{\frac{\partial c_j}{\partial e_j} (\frac{m_j}{e_j})^{-1} (r + q_j) (1 - \sigma) + \beta}. \tag{20}\]

It appears then, by taking the marginal derivative of (20) with respect to \( e_i \) in the point where \( e_j = e_i \), that if \( e_i > e_j, m_i > m_j \), then \( q_i > q_j, L_i < L_j \) and hence \( P_i > P_j \). Consequently, (20) implies that, in equilibrium, households will always search more in neighbourhoods with higher prices.

\(^{19}\) This is line with our empirical investigation where we compare neighbourhoods with control neighbourhoods that are argued to be approximately identical. Wheaton (1990) derives comparative statics assuming symmetry combined with assumptions on the functional form of the matching function.

\(^{20}\) Equivalently, we may also consider a difference in the amenity level between neighbourhoods before the investments take place. For example, we may assume that \( A_{i,t-1} = A - v_i \) and \( A_{j,t-1} = A \) and that \( A_{i,t} = A_{j,t} = A \) in time period \( t \). We emphasise that the results that follow will be almost identical to the situation we consider here.

\(^{21}\) The full derivation can be received upon request.
We are interested in the marginal effect of $v_i$ and evaluate the effect when $v_i = 0$, implying that $e_i = e_i$. We will evaluate the effect of an investment on search effort and prices in both neighbourhoods, leading to the following proposition:

**Proposition 1**: Given a marginal increase in $v_i$, search effort in both neighbourhoods increase. The house price in neighbourhood $i$ will then exceed the house price in neighbourhood $j$ implying that search effort in neighbourhood $i$ exceeds search effort in neighbourhood $j$.

*Proof.* See Appendix A. □

Hence, place-based policies increase the price in the targeted neighbourhood relative to the price in the neighbourhood that is not targeted. Nevertheless, it is important to emphasise that also in the other neighbourhood, search effort increases because mismatched households who live in the neighbourhood where amenities are increased (but do not directly benefit from this increase), have an increased incentive to find a house in the other neighbourhood, because they realise that they can obtain a higher price for their current house.

Next, we show the impact of changes in search effort on matching rates in both neighbourhoods and consequently on sales times. The proposition is as follows:

**Proposition 2**: Given a marginal increase in $e_i$, the matching rate in neighbourhood $i$ increases and exceeds the matching rate in neighbourhood $j$. This implies that sales times in neighbourhood $i$ are shorter than in $j$.

*Proof.* See Appendix A. □

Because $v_i$ implies that $e_i > e_j$, this implies that $v_i$ lead to higher matching rates in $i$ compared to $j$. Because $m_i$ is positively associated with an increase in $v_i$ (via $e_i$) and $m_i > m_j$ when $v_i$ marginally increases, the relative sales time in $L_i$ decreases due to the place-based investment.

### E. Welfare and optimal search effort

In the current setup, search effort is subject to two types of externalities. When mismatched households increase their search effort they ignore that all other households who search are disadvantaged (a negative congestion externality), whereas all households who aim to sell their house are advantaged (a positive congestion externality) as the ratio of $e_i H_i^S$ to $V_i$ changes. So, we will contribute to the housing market search literature by showing that for a specific value of $\sigma$, search will be efficient.

Similar to Wheaton (1990), we define welfare as:

---

22 The empirical interpretation for this assumption is that we compare two identical neighbourhoods before the policy became relevant. Our empirical identification strategy claims that we compare (almost) identical neighbourhoods before treatment.
\[ W = \sum_{t=1}^{2} \int e^{-\delta t} (A_i (\bar{H} - H_t^i) - c_i H_t^i) \, dt, \]

where \( H_t^i \) starts in \( t = 0 \) and moves according to the system of differential equations (3), (4) and (5). We determine the optimal search level by taking the derivative of \( W \) with respect to search effort in location \( i \) in the current steady-state. Using techniques discussed by Diamond (1980), we may write:

\[
\frac{dW}{de_i} = -\frac{H_t^i \, \partial c_i}{r \, \partial e_i} - \frac{H_t^i \, \partial m_i}{r \, \partial e_i} \times [A + v_i + c_i \quad 0 \quad A + c_j \quad 0] \times [HH]^{-1} \times \begin{bmatrix} -1 \\ r \end{bmatrix},
\]

with

\[
[HH] = \begin{bmatrix} r & 0 & 0 & 0 \\ 0 & r & 0 & 0 \\ 0 & 0 & r & 0 \\ 0 & 0 & 0 & r \end{bmatrix} - \begin{bmatrix} -m_i - \beta & 0 & -\beta & -\beta \\ m_i & -\beta & -m_j & \beta \\ -\beta & -\beta & -m_j - \beta & 0 \\ -m_i & \beta & m_j & -\beta \end{bmatrix}.\]

Equation (22) then gives:

\[
\frac{dW}{de_i} = \frac{H_t^i}{r} \left( -\frac{\partial c_i}{\partial e_i} + \frac{\partial m_i}{\partial e_i} \right) (A + v_i + c_i) \frac{A + c_j}{r + 2\beta + m_i} - \frac{\partial c_i}{\partial e_i} = 0.
\]

We are particularly interested in optimal search effort level when \( v_i = 0 \). Given the first-order condition that (24) is equal to 0, the optimal search effort condition can be written as:

\[
\frac{\partial m_i}{\partial e_i} \frac{A + c_i}{r + 2\beta + m_i} - \frac{\partial c_i}{\partial e_i} = 0.
\]

This result is intuitive. The first term is the marginal benefit of search, which equals the marginal increase in the matching rate multiplied with the benefit of a match. The instantaneous benefit equals \( A \) plus the savings of not searching \( c_i \), which is amortised at a rate \( r + 2\beta + m_i \). This rate exceeds the interest rate \( r \) with \( 2\beta \), because the match has no value any more to society when the household changes preferences. The term \( m_i \) captures the possibility that a match (with another seller) may occur in the future. Using (16) and (18), the private search effort condition in symmetry is equal to:

\[
\frac{m_i}{e_i} \frac{\sigma(A + c_i)}{r + 2\beta + \sigma m_i} - \frac{\partial c_i}{\partial e_i} = 0.
\]

This expression is very similar but notice that the buyer receives certain share \( \sigma \) of the benefit of a match (now or in the future). Recall that \( m_i/e_i > \partial m_i/\partial e_i \). So, when \( \sigma = 1 \), it can be immediately observed that search is not efficient and search effort exceeds optimal search effort. This result is intuitive because given \( \sigma = 1 \), households do not benefit from selling their house, so there is no positive congestion externality. For \( \sigma = 0 \), exactly the opposite

---

23 When \( v_i > 0 \), so when the market is not symmetrical, it can be shown that there is no value of \( \sigma \) for which search is efficient (because \( \sigma \) must be region-specific for welfare to be maximised in both neighbourhoods).

24 According to Wheaton (1990) search effort is welfare optimal when \( \sigma = 1 \), but otherwise too low. However, his result was erroneously based on search effort of a representative household, rather than the search effort of a household which takes the search effort of the other households as given.
holds. Search effort is less than optimal, because households do not benefit from buying a house and will not search. It is now intuitive that for a specific value of $\sigma$ between zero and one, search is efficient. More specifically, search effort is optimal when:

$$\sigma^* = \frac{\psi(r + 2\beta)}{1 - \psi} + r + 2\beta,$$

where $\psi = (\partial m_i/\partial e_i)(e_i/m_i)$ refers to the elasticity of the household matching rate with respect to search effort, which is always between zero and one given the properties of the matching technology. Because the elasticity is always between zero and one, it appears indeed that there is always a $\sigma^*$ between zero and one for which holds that search effort maximises welfare. Note further that this condition implies that if $m_i$ approaches infinity, $\sigma^*$ approaches zero. The latter is a common assumption in the urban economics literature that assumes competitive land markets where landowners get the full surplus from selling land for housing.

F. Welfare: empirical considerations

In the empirical application, we will demonstrate place-based induced changes in prices (as well as in sales times). We aim to know to what extent these changes in prices reflect changes in welfare and whether information about changes in sales times is helpful to calculate the impact of place-based policies on welfare.

In the standard competitive model, changes in house prices are perfect indicators of changes in welfare (and information about changes in sales time is redundant). In a model with search imperfections, this result does not hold, but we will show that under certain conditions relative changes in the neighbourhood price differences approximately reflect relative changes in welfare even in a search setting.

Assume that the investment per household living in region $i$ is equal to $v_i$. The number of households is equal to $\bar{H}$. Consequently, total investment is equal to $v_i \bar{H}$. Hence, we aim to know the effect of this investment on welfare, which can be written as the marginal effect of investment on total welfare per household living in region $i$.

$$\frac{dW}{d(v_i\bar{H})} = \frac{dW/\bar{H}}{dv_i}.$$

Let us first calculate the overall effect of the investment on welfare:

$$\frac{dW/\bar{H}}{dv_i} = \frac{\partial W/\bar{H}}{\partial v_i} + \frac{\partial W/\bar{H}}{\partial e_i} \frac{\partial e_i}{\partial v_i} + \frac{\partial W/\bar{H}}{\partial e_j} \frac{\partial e_j}{\partial v_i}.$$

The first assumption we make is that search effort is approximately at its welfare-optimal level. We believe that this is a restrictive, but not unreasonable assumption given that we have shown that such a level may exist. It follows then that:

---

25 One may label the above condition as the housing market Hosios condition, because a similar condition has been derived for search in the labour market by Hosios (1990).

26 Importantly, in numerical simulations in Appendix B, we have examined to what extent, the level of welfare depends on the difference between the optimal share $\sigma^*$ and other values of $\sigma$. Given a range of
where the last step holds for marginal changes in $v_i$ with respect to the initial situation (see Diamond, 1980). Because the number of mismatched households is always less than $0.5\bar{H}$, it follows that:

$$\frac{1}{2r} \leq \frac{dW/H}{dv_i} \leq \frac{1}{r}.$$ (31)

This is a convenient result, because it determines a upper and lower value for the overall effect of the place-based investment on welfare.

Now we will focus on the overall effect of the place-based investment on the price differences. [We are now working on a proof to show that] $^{27}$

$$\frac{d(P_i - P_j)}{dv_i} \leq \frac{1}{2r}. $$ (32)

This result is convenient because it shows that there is an upper value to the increase in prices. It is now immediate clear that:

$$\frac{d(P_i - P_j)}{dv_i} \leq \frac{dW/H}{dv_i}. $$ (33)

Consequently, and importantly, it follows that the welfare increase due to the place-based investment is at least equal to the observed (increase in) price differences between neighbourhoods that follows from this investment. The reason is that due to bargaining, part of the price effect in neighbourhood $i$ dissipates to the other neighbourhood.

It can be shown that the equality in (32) only holds when $\beta$ approaches infinity. $^{28}$ For this case, it is intuitive that the marginal welfare as well as the price difference increase are exactly $1/2r$, because both the selling as well as the buying household benefit exactly half of the time of the type from the investment (because the sales time remains positive, it means that households never sell before changing of type). It can also be shown that the difference between the welfare increase and the price difference increase is the largest when $\beta$ approaches zero. In this case, the price increase is minimal (also in absolute value), because the buying household realises that the expected sales time of selling its current home is extensive, that it hardly gives any benefit of buying another property.

---

$^{27}$ Note that when $v_i = 0$, the following hold: $d(P_i - P_j)/dv_i = (1/P_i)(d(P_i/P_j)/dv_i)$.

$^{28}$ In Figure B2, Appendix B, we report the effect of $v_i$ on relative and absolute prices. It is shown that $P_i$ and $P_j$ are non-monotonic functions of $\beta$, but that the relative price increase is a monotonically increasing function of $\beta$. 

parameter assumptions, the optimal share turns out to be much smaller and around 0.1, but the differences in welfare are very small (less than one percent).
III. Empirical framework and data

A. The urban revitalisation programme

There is ample empirical evidence that households with low incomes and associated social problems are disproportionately 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. About 70 percent of the most deprived neighbourhoods are located in the four largest cities of the Netherlands (Amsterdam, Rotterdam, The Hague and Utrecht). The share of public housing is much higher in these neighbourhoods than in other parts of the Netherlands. The gap between 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 invested in the 40 worst performing districts in the Netherlands, covering 83 postcode areas (The Court of Audit, 2010). In what follows, we refer to postcode areas as neighbourhoods. 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 living in the neighbourhoods) (The Court of Audit, 2010). We consider this date as the start of the investment programme, but we will check for robustness later on. Although we do not know the exact monetary value of the investment, at least 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 based on 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

29 Due to substantial benefit transfers, differences in Dutch household income are less pronounced than in the US.
30 Public housing is common in the Netherlands, about 35 percent of Dutch residences are public housing, which is by far the highest in Europe.
The overall z-score is the sum of the standardised scores of four categories, the standard deviation of the z-score is much larger than one, but the average score for The Netherlands is zero.

Table 1 shows that targeted KW-neighbourhoods have scores that are at least one standard deviation above the Dutch average for the different categories. The overall average score for these neighbourhoods is 8.94. The selection of the KW-neighbourhoods was based on the deprivation score using an initial list of 340 postcodes 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, four neighbourhoods were not selected because they were not on the initial list, although their score was above 7.30. In addition, eight neighbourhoods were excluded 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). 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 $z \geq 7.30$. For example, a neighbourhood with a z-score of 7.29 has 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 may be shown that the distribution function is continuous around the threshold point (see Figure C1 in Appendix C).

### Table 1 — Deprivation scores for neighbourhoods

<table>
<thead>
<tr>
<th></th>
<th>All neighbourhoods</th>
<th>KW neighbourhoods</th>
<th>Non-KW, on initial list</th>
<th>Non-KW, not on initial list</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\mu$</td>
<td>$\sigma$</td>
<td>$\mu$</td>
<td>$\sigma$</td>
</tr>
<tr>
<td>Social deprivation</td>
<td>0.000</td>
<td>0.654</td>
<td>1.167</td>
<td>0.322</td>
</tr>
<tr>
<td>Physical deprivation</td>
<td>0.000</td>
<td>0.611</td>
<td>2.070</td>
<td>0.660</td>
</tr>
<tr>
<td>Social problems</td>
<td>0.000</td>
<td>0.924</td>
<td>2.612</td>
<td>1.053</td>
</tr>
<tr>
<td>Physical problems</td>
<td>0.000</td>
<td>0.950</td>
<td>3.087</td>
<td>0.976</td>
</tr>
<tr>
<td>Overall</td>
<td>0.000</td>
<td>2.414</td>
<td>8.935</td>
<td>1.340</td>
</tr>
</tbody>
</table>

Number of neighbourhoods: 4016, 83, 257, 3676.

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 social housing stock. Social problems consists of five indicators: two vandalism indicators, two nuisance-from-neighbours indicators, and one indicator relates to feelings of insecurity. Physical problems includes seven indicators: house and living environment satisfaction, the inclination to move, and indicators relating to noise and air pollution, traffic intensity and traffic safety. For details, see Brouwer and Willems (2007).

31 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 postcode areas/neighbourhoods are fairly small: the average distance to the centroid of a neighbourhood is only 286 meter.
Notes: This is a regression of assignment of neighbourhood on the scoring rule dummy and a flexible function of the z-score. The number of observations is 4,016.

B. Data

Our analysis is based upon a house transactions dataset from the NVM (Dutch Association of Real Estate Agents). It contains information on about 80 percent of all transactions between 2001 and 2011.\textsuperscript{32} For 1,394,856 transactions, we know the transaction 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.\textsuperscript{33} On average, properties in our sample are sold 1.24 times in our study period. In the analysis, we focus on properties that are sold at least twice, leaving us with 271,039 transactions.\textsuperscript{34}

In Table 2, descriptives are reported for observations outside and inside (targeted) KW-neighbourhoods. About 4.5 percent of the observations in the repeated sales sample is in a targeted KW-neighbourhood and 1.5 percent of the total observations is in a KW-neighbourhood area in the post-investment period. It appears that the price in non-KW neighbourhoods is about 4 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 higher. Properties in KW-neighbourhoods tend to have a lower quality: they are older, have a lower share central heating and are less well maintained. Also, about 33 percent of the properties in these areas have been constructed between 1961 and 1970, a building period which is in the Netherlands

\textsuperscript{32} 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.

\textsuperscript{33} 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 25m\textsuperscript{2} or larger than 250m\textsuperscript{2}. We drop a few properties that are sold more than four times in our study period and are listed for more than two years on the market or were listed zero days on the market.

\textsuperscript{34} Using repeat sales may imply a selection problem, because certain house types may be sold less often. In Section V.E we therefore check whether our results are robust when using the full sample.
associated with low building quality. Table C1 in Appendix C 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 of properties and the sample of houses that are sold at least twice. 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. We also investigate the (cross-sectional) relationship between sales time and house prices. In line with the theoretical model, there is a negative correlation between sales times and house prices. This relationship is of course not causal, as there is likely reverse causation between house prices and sales time.\textsuperscript{35} Genesove and Mayer (1997) for example show that properties with higher loan-to-value ratios are listed at higher asking prices and therefore have a longer sales time.

<table>
<thead>
<tr>
<th>TABLE 2 — DESCRIPTIVE STATISTICS FOR REPEATED SALES SAMPLE</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Observations outside KW-neighbourhoods</strong></td>
</tr>
<tr>
<td><strong>Observations inside KW-neighbourhoods</strong></td>
</tr>
<tr>
<td><strong>µ</strong></td>
</tr>
<tr>
<td>House price per m(^2) (in €)</td>
</tr>
<tr>
<td>Days on the market</td>
</tr>
<tr>
<td>KW investment received</td>
</tr>
<tr>
<td>Deprivation z-score</td>
</tr>
<tr>
<td>Size in m(^2)</td>
</tr>
<tr>
<td>Rooms</td>
</tr>
<tr>
<td>Maintenance quality – good</td>
</tr>
<tr>
<td>Central heating</td>
</tr>
<tr>
<td>Listed</td>
</tr>
<tr>
<td>House type – apartment</td>
</tr>
<tr>
<td>House type – terraced</td>
</tr>
<tr>
<td>House type – semi-detached</td>
</tr>
<tr>
<td>House type – detached</td>
</tr>
<tr>
<td>Garage</td>
</tr>
<tr>
<td>Garden</td>
</tr>
<tr>
<td>Construction year &lt;1945</td>
</tr>
<tr>
<td>Construction year 1945-1960</td>
</tr>
<tr>
<td>Construction year 1961-1970</td>
</tr>
<tr>
<td>Construction year 1971-1980</td>
</tr>
<tr>
<td>Construction year 1981-1990</td>
</tr>
<tr>
<td>Construction year 1991-2000</td>
</tr>
<tr>
<td>Construction year &gt;2000</td>
</tr>
</tbody>
</table>

Notes: The number of observations outside KW-neighbourhoods is 271,039 and inside KW-neighbourhoods 12,352. Note that the house type variables, garage, garden, and construction year are time-invariant, so they will drop in the first-differences equations.

\textsuperscript{35} See Figure C2 in Appendix C for more details. One may argue that this correlation is due to housing attributes that may explain why certain houses are sold quicker than others (see Taylor, 1999). We therefore include property fixed effects, but the negative correlation is very similar: a house that is sold immediately is about 5 percent more expensive than a house that is sold after a year.
In Figure 3 we plot the house price and the sales time for other and KW-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 in 2011 the house prices seem to be almost identical. Although suggestive, it is too soon to conclude that this is due to the investment programme, because here we do not control for other factors that 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, it is still there in 2011.

![Figure 3: House Prices and Sales Time](image-url)
C. Econometric framework and identification

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

$$y_{t\ell} = \alpha k_{t\ell} + v_t + \epsilon_{t\ell},$$

where $\alpha$ is the parameter to be estimated and $\epsilon_{t\ell}$ is 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 $\alpha$. However, only deprived neighbourhoods are selected, which implies a correlation between $\epsilon_{t\ell}$ and $k_{t\ell}$. We therefore employ a first-difference approach, where the change in the outcome variable, $\Delta y_{t\ell}$, is regressed on the change in the investment, $\Delta k_{t\ell}$. $\Delta k_{t\ell}$ equals one when we observe a property located in a targeted area before and after the investment 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 housing variables $x_{t\ell}$ implying:

$$\Delta y_{t\ell} = \alpha \Delta k_{t\ell} + \beta \Delta x_{t\ell} + \Delta v_t + \Delta \epsilon_{t\ell}.$$ 

In the theoretical model, we assumed that the impact of place-based policies is only local, excluding the possibility of spatial spillovers. Houses close to a targeted area may also experience changes in $y_{t\ell}$ because positive effects are likely to decay over space (Rossi-Hansberg et al., 2010). We therefore exclude observations within a kilometre of a targeted neighbourhood. When estimating (35), the crucial identifying assumption for consistent estimation of $\alpha$ is that unobserved trends are uncorrelated with the change in treatment $\Delta k_{t\ell}$. This assumption may be problematic, e.g. because of demographic trends, such as in gentrification. We therefore need to find neighbourhoods that are almost identical to each other but are not both 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 use a RDD based on the deprivation score of the neighbourhood. 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

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36 In the sensitivity analysis (Section V.B), we investigate whether there are spatial spillovers of the investment.
investment programme was announced in 2007, based on data from 2006 and 2002. It is 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 (\( z = 7.30 \)). However, this would lead to a few number of observations and therefore to large standard errors. Following Van der Klaauw (2002), we also include neighbourhoods away from the threshold and add a nonparametric control function \( G(\cdot) \) of the z-score to (35), which is continuous at the boundary threshold. The idea is that \( z_\ell \) is the only determinant of the treatment status, implying that \( G(\cdot) \) will capture any correlation between \( \Delta k_{\ell t} \) and \( \Delta \epsilon_{\ell t} \). Hence:

\[
\Delta y_{\ell t} = \alpha \Delta k_{\ell t} + G(z_\ell) + \beta \Delta x_{\ell t} + \Delta u_\ell + \Delta \epsilon_{\ell t},
\]

As suggested by Trochim (1984), we use a conventional power series approximation of \( G(z_\ell) = \sum_{p=1}^{p} y_p z_\ell^p \), where \( p = 5 \).

Although neighbourhoods could not manipulate the 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 RDD potentially invalid, as it assumes a one-to-one relationship between the assignment and the z-score. A simple solution is to remove these neighbourhoods from the analysis altogether. However, it is preferable to employ a fuzzy RDD, because the neighbourhoods that were removed may be a non-random selection of eligible neighbourhoods. A fuzzy RDD 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:

\[
\Delta k_{\ell t} = \bar{\alpha} \Delta s_{\ell t} + \bar{G}(z_\ell) + \bar{\beta} \Delta x_{\ell t} + \Delta u_\ell + \Delta \bar{\epsilon}_{\ell t},
\]

where the \( ~ \) indicates first-stage coefficients and \( \bar{\alpha} \) is the parameter of interest and \( \bar{\epsilon}_{\ell t} \) is a random error term. Here, \( \Delta s_{\ell t} \) equals one when \( z \geq 7.30 \) and when a property is sold before and after the investment. In the second stage we then insert \( \Delta \bar{k}_{\ell t} \) (and calculate standard errors taking into account that \( \Delta \bar{k}_{\ell t} \) is estimated). In Figure 1, it was shown that \( \bar{\alpha} \) was highly statistically significant at the neighbourhood level. The coefficient was about 0.75; note that when we had a sharp RDD, \( \bar{\alpha} \) must have been equal to one.

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 \( \alpha \) would be similar to the estimation procedure where we include all neighbourhoods (equations (35) and (36)), this would suggest that the local average treatment effect at the threshold is equal to the average treatment effect.

A concern that arises when estimating equation (36) is that there is a spatial trend in the dependent variable that is correlated with the decision to invest in certain neighbourhoods. For example, it has been recorded that Dutch middle class households in targeted neighbourhoods tend to move to the suburbs and are replaced by relatively poor non-
western migrants (Wittebrood and Permentier, 2011). To further control for this unobserved spatial trends, we add postcode fixed effects $\xi_t$. If equation (36) is correctly specified, adding postcode fixed effects will not have an impact on the magnitude of $\alpha$, although it may impact its standard errors. Because $z_t$ does not vary over time within postcode areas, $G(z_t)$ is collinear with $\xi_t$ and will drop from the equation. The equation to be estimated then becomes:

$$\Delta y_{tt} = \alpha \Delta k_{tt} + \beta \Delta x_{tt} + \xi_t + \Delta u_t + \Delta \epsilon_{tt}. \quad (38)$$

In the above equation, it seems reasonable to assume that observations close to the threshold are more informative about the causal effect we aim to identify. Hence, we also estimate (38) 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 entails the following (second-stage) specification:

$$\left(\hat{\alpha}, \hat{\beta}, \hat{\xi}_t, \hat{\epsilon}_t\right) = \arg\min_{\alpha, \beta, \xi_t, \epsilon_t} \sum_{i=1}^{n} w_i \left(\Delta y_{tt} - \alpha \Delta k_{tt} - \beta \Delta x_{tt} - \xi_t - \epsilon_t\right)^2, \quad (39)$$

with $w_i = 1_{|z_i - 7.3| < h}$, implying that we use a uniform weighting function and let $h$ denote the bandwidth. If (for a small $h$) this semiparametric approach delivers similar estimates, it seems reasonable to assume that we have controlled for any systematic correlation between the treatment status $\Delta k_{tt}$ and unobserved shocks $\Delta \epsilon_{tt}$. We choose $h$ based on visual inspection of the data to trade-off statistical precision and potential bias. Note that, because we use first-differencing and include postcode fixed effects, the bias is thought to be reasonably small. We come back to the issue of bandwidth selection in the sensitivity analysis.

IV. Results

A. Baseline results – house prices

Our theoretical model predicts a positive price effect due to an increase in amenities. Table 3 reports the results. 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 had a positive effect on prices of 3.6 percent.\footnote{The marginal effect is calculated as $e^\alpha - 1$.} When we control for changes in housing attributes (Column (2)), prices in targeted neighbourhoods have increased with 3.2 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. The price effect is 2.6 percent and similar to previous specifications. It should be noted that controlling flexibly for the z-score does not seem to matter. Rather than excluding neighbourhoods that are not in accordance
## Table 3 — Regression results: the effect of place-based policies on house prices

(Independent variable: change in log house price per square meter)

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
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<tr>
<td></td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS-LL</td>
</tr>
<tr>
<td>∆ KW investment</td>
<td>0.0349***</td>
<td>0.0319***</td>
<td>0.0257***</td>
<td>0.0316***</td>
<td>0.0357***</td>
<td>0.0396***</td>
<td>0.0326**</td>
</tr>
<tr>
<td></td>
<td>(0.00983)</td>
<td>(0.00941)</td>
<td>(0.00958)</td>
<td>(0.0101)</td>
<td>(0.0112)</td>
<td>(0.0109)</td>
<td>(0.0138)</td>
</tr>
<tr>
<td>∆ Size (log)</td>
<td>-0.876***</td>
<td>-0.876***</td>
<td>-0.876***</td>
<td>-0.883***</td>
<td>-0.899***</td>
<td>-0.919***</td>
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</tr>
<tr>
<td></td>
<td>(0.00704)</td>
<td>(0.00696)</td>
<td>(0.00696)</td>
<td>(0.0118)</td>
<td>(0.0170)</td>
<td>(0.0199)</td>
<td></td>
</tr>
<tr>
<td>∆ Rooms (log)</td>
<td>0.00192***</td>
<td>0.00232***</td>
<td>0.00237***</td>
<td>0.00305**</td>
<td>0.00271*</td>
<td>0.00512**</td>
<td>0.000565</td>
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<td></td>
<td>(0.000565)</td>
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<td>(0.000577)</td>
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<td>(0.00140)</td>
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<td>∆ Maintenance quality – good</td>
<td>0.101***</td>
<td>0.100***</td>
<td>0.100***</td>
<td>0.0923***</td>
<td>0.0904***</td>
<td>0.0883***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00168)</td>
<td>(0.00170)</td>
<td>(0.00170)</td>
<td>(0.00356)</td>
<td>(0.00317)</td>
<td>(0.00413)</td>
<td></td>
</tr>
<tr>
<td>∆ Central heating</td>
<td>0.0680***</td>
<td>0.0665***</td>
<td>0.0663***</td>
<td>0.0632***</td>
<td>0.0531***</td>
<td>0.0479***</td>
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</tr>
<tr>
<td></td>
<td>(0.00243)</td>
<td>(0.00238)</td>
<td>(0.00237)</td>
<td>(0.00411)</td>
<td>(0.00391)</td>
<td>(0.00564)</td>
<td></td>
</tr>
<tr>
<td>∆ Listed building</td>
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<td>0.00550</td>
<td>0.00490</td>
<td>-0.0122</td>
<td>-0.0117</td>
<td>-0.0114</td>
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<td>(0.00644)</td>
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<td>(0.00639)</td>
<td>(0.0124)</td>
<td>(0.00788)</td>
<td>(0.00879)</td>
<td></td>
</tr>
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<td>G(z_r) included</td>
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<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>∆ Year fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighbourhood fixed effects</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
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<td>129,949</td>
<td>129,345</td>
<td>129,949</td>
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<td>9,544</td>
</tr>
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<td>R² - within</td>
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<td>0.401</td>
<td>0.405</td>
<td>726.7</td>
<td>679.2</td>
<td>766.4</td>
<td>576.3</td>
</tr>
<tr>
<td>Kleibergen-Paap F-statistic</td>
<td>∞</td>
<td>∞</td>
<td>∞</td>
<td>726.7</td>
<td>679.2</td>
<td>766.4</td>
<td>576.3</td>
</tr>
<tr>
<td>Bandwidth h</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>2.00</td>
</tr>
</tbody>
</table>

Notes: We exclude observations within one kilometre of targeted areas. In Columns (4)-(7) the change in KW investment is instrumented with the change in the eligibility based on the scoring rule. 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 Column (6) we exclude neighbourhoods that were not on the initial list. Column (7) only includes neighbourhoods with 5.3 < z < 9.3. 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

with the scoring rule, 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 D1 in Appendix D). In all the specifications, the scoring rule is a very strong instrument of being treated ($F > 500$), with a coefficient close to one: houses in neighbourhoods that are above the scoring rule have an approximately 90 percent higher probability to become treated. The second stage results are in line with previous specifications. The results in Column (4), Table 3, implies that prices in KW-neighbourhoods have increased with 3.2 percent due to the investment programme. In Column (5) we explore the robustness of the findings further by removing the observations that were not on the initial list and therefore had no chance to receive treatment. This implies that we focus on 340 neighbourhoods with at least some deprivation (the average z-score in the selected 2016-2019 period).

Note that the jump in probability to become treated is higher than recorded in Figure 1, because neighbourhoods are not of equal size.
sample is 5.81, compared to 0.91 in the initial sample). Although this reduces the number of observations by 85 percent, the coefficient is very similar: the price effect of the investment is 3.6 percent. Note that the standard errors are hardly influenced by the selection. In Column (6) we include neighbourhood fixed effects to control for unobserved trends. Then, the coefficient becomes slightly higher (4.0 percent). In Column (7), we employ the local linear approach and only include observations that are within two points of the scoring rule, which implies that we select 160 neighbourhoods (so, $h = 2$). The price effect is 3.3 percent and still statistically significant at the five percent level. Hence, these results suggest a pronounced price effect due to place-based investments that have increased the amenity levels in targeted neighbourhoods. However, without knowing whether search frictions are important, we cannot say much about welfare effects of the place-based investment.

B. Baseline results – sales time

The other prediction of the theoretical model is that place-based policies should reduce the sales time of houses. The presence of an effect of place-based policies on sales time also indicates that bargaining and search are important. 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 substantially reduced with 13.2 percent (about 18 days) due to the investment. If we control for housing attributes in Column (2), the coefficient is essentially the same. In Column (3) we employ the sharp regression-discontinuity design and exclude non KW-neighbourhoods with a z-score above the threshold and KW-neighbourhoods with a z-score below the threshold. The coefficient is then somewhat stronger (20.1 percent). Next, we do not exclude neighbourhoods but use an instrumental variable approach instead, with the change in the scoring rule as an instrument. Note that the first stage results are identical to the price regressions (see Table D1 in Appendix D). The fuzzy regression-discontinuity design leads to very similar second stage results: Column (4) in Table 4 suggests that the investment has led to 20.4 percent decrease in sales time. In Column (5) we only include 340 neighbourhoods with some deprivation that were on the initial list. The coefficient is very similar to the previous specifications. In Column (6) we include neighbourhood fixed effects to control for unobserved trends at the neighbourhood level. The coefficient is now slightly lower (16.3 percent), but still statistically significant at the one percent level (although the standard error increases somewhat). Column (7) only includes neighbourhoods 'close' to the threshold score, by assuming $h = 2$. The coefficient is of a similar order of magnitude compared to the previous specifications: place-bases investments have reduced the sales time with 15 percent.

Hence, the results seem to suggest a substantial and pronounced effect of the place-based investment on sales times, which suggests that spatial equilibrium models that infer (general) welfare changes from price changes are likely underestimates of welfare changes, because they ignore welfare effects of search frictions and bargaining.

—23—
### Table 4 — Regression results: the effect of place-based policies on sales times

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

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>OLS</td>
<td>OLS</td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS-L1</td>
</tr>
<tr>
<td>Δ KW investment</td>
<td>-0.143***</td>
<td>-0.142***</td>
<td>-0.223***</td>
<td>-0.227***</td>
<td>-0.213***</td>
<td>-0.178***</td>
<td>-0.164**</td>
</tr>
<tr>
<td></td>
<td>(0.0501)</td>
<td>(0.0498)</td>
<td>(0.0522)</td>
<td>(0.0533)</td>
<td>(0.0597)</td>
<td>(0.0630)</td>
<td>(0.0755)</td>
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<tr>
<td>Δ Size (log)</td>
<td>0.214***</td>
<td>0.214***</td>
<td>0.217***</td>
<td>0.0607</td>
<td>0.0898</td>
<td>0.0105</td>
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</tr>
<tr>
<td></td>
<td>(0.0813)</td>
<td>(0.0812)</td>
<td>(0.0811)</td>
<td>(0.204)</td>
<td>(0.200)</td>
<td>(0.272)</td>
<td></td>
</tr>
<tr>
<td>Δ Rooms (log)</td>
<td>-0.0300***</td>
<td>-0.0276***</td>
<td>-0.0279***</td>
<td>-0.0455***</td>
<td>-0.0434***</td>
<td>-0.0350</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00595)</td>
<td>(0.00593)</td>
<td>(0.00592)</td>
<td>(0.0170)</td>
<td>(0.0167)</td>
<td>(0.0234)</td>
<td></td>
</tr>
<tr>
<td>Δ Maintenance quality – good</td>
<td>0.0494***</td>
<td>0.0435***</td>
<td>0.0436***</td>
<td>0.0716**</td>
<td>0.0460</td>
<td>0.0824*</td>
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</tr>
<tr>
<td></td>
<td>(0.0150)</td>
<td>(0.0149)</td>
<td>(0.0148)</td>
<td>(0.0323)</td>
<td>(0.0317)</td>
<td>(0.0434)</td>
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</tr>
<tr>
<td>Δ Central heating</td>
<td>-0.0948***</td>
<td>-0.103***</td>
<td>-0.103***</td>
<td>-0.133***</td>
<td>-0.0991***</td>
<td>-0.157***</td>
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</tr>
<tr>
<td></td>
<td>(0.0175)</td>
<td>(0.0175)</td>
<td>(0.0174)</td>
<td>(0.0376)</td>
<td>(0.0362)</td>
<td>(0.0518)</td>
<td></td>
</tr>
<tr>
<td>Δ Listed building</td>
<td>0.0367</td>
<td>0.0259</td>
<td>0.0335</td>
<td>0.0640</td>
<td>0.0897</td>
<td>0.137</td>
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<tr>
<td></td>
<td>(0.0614)</td>
<td>(0.0624)</td>
<td>(0.0614)</td>
<td>(0.122)</td>
<td>(0.136)</td>
<td>(0.180)</td>
<td></td>
</tr>
</tbody>
</table>

**G(z_r) included**
- No
- Yes

**Δ Year fixed effects**
- No
- Yes

**Neighbourhood fixed effects**
- No
- Yes

| Number of observations      | 129,949 | 129,949 | 129,345 | 129,949 | 20,055   | 20,055   | 9,544   |
| R²-within                   | 0.040   | 0.041   | 0.042   |         |          |          |         |
| Kleibergen-Paap F-statistic|         |         |         | 726.7   | 679.2    | 766.4    | 576.3   |
| Bandwidth h                 | ∞       | ∞       | ∞       |          |          |          | 2.00    |

*Notes: We exclude observations within one kilometre of targeted areas. In Columns (4)-(7) the change in KW investment is instrumented with the change in the eligibility based on the scoring rule. 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 Column (6) we exclude neighbourhoods that were not on the initial list. Column (7) only includes neighbourhoods with 5.3 < z < 9.3. 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

### V. Sensitivity analysis

#### A. Introduction

In this sensitivity analysis, we subject the baseline results to a wide range of robustness checks. First, we test for adjustment effects and inspect the magnitude of potential spatial spillovers of the investment programme. Second, we will investigate whether the effect of place-based policies is mainly direct (via an increased quality of the building stock) or indirect (via sorting mechanisms) by controlling for demographic variables and neighbourhood-specific trends. Third, we will conduct a series of quasi-‘placebo’ experiments based on previous programmes selecting different neighbourhoods. Fourth, we investigate whether using the full sample, rather than repeated sales influences our results. Fifth, we will inspect whether our results are robust to a propensity score matching method, rather than a regression-discontinuity approach. Sixth, we will test robustness of our results to different
bandwidths of the local linear regression approach. Finally, we will test robustness of our results with respect to the start date of the investment. We consider the specifications in Columns (7) in Tables 3 and 4 as the baseline specifications, as the latter specification provides the most persuasive evidence of a causal effect of the investment programme.

B. Adjustment effects and spatial spillovers

First, we test the robustness of the presence of adjustment effects after the considered start date of the investment programme, as one may argue that the market needs time to reach a new steady state. Table 5 reports the results.

### Table 5 — Sensitivity Analysis: Adjustment Effects and Spatial Spillovers

<table>
<thead>
<tr>
<th></th>
<th>Panel 1: Δ Price per m² (log)</th>
<th>Panel 2: Δ Days on the market (log)</th>
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</thead>
<tbody>
<tr>
<td></td>
<td>(1) 2SLS-LL</td>
<td>OLS-LL</td>
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<tr>
<td>Δ KW investment</td>
<td>0.0326**</td>
<td>0.0242*</td>
</tr>
<tr>
<td></td>
<td>(0.0127)</td>
<td>(0.0146)</td>
</tr>
<tr>
<td>Δ Years after investment × KW investment</td>
<td>-2.87e-05</td>
<td>0.0106</td>
</tr>
<tr>
<td></td>
<td>(0.00327)</td>
<td>(0.0248)</td>
</tr>
<tr>
<td>Δ (Years after investment)² × KW investment</td>
<td>-0.00165</td>
<td>0.227</td>
</tr>
<tr>
<td></td>
<td>(0.0110)</td>
<td>(0.150)</td>
</tr>
<tr>
<td>Δ (Years after investment)³ × KW investment</td>
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<td>-0.0269</td>
</tr>
<tr>
<td></td>
<td>(0.00141)</td>
<td>(0.0198)</td>
</tr>
<tr>
<td>Δ KW investment &lt;0.25km</td>
<td>0.0495***</td>
<td>(0.0154)</td>
</tr>
<tr>
<td>Δ KW investment 0.25-0.5km</td>
<td>0.0461***</td>
<td>(0.0151)</td>
</tr>
<tr>
<td>Δ KW investment 0.50-1.0km</td>
<td>0.0169</td>
<td>(0.0133)</td>
</tr>
</tbody>
</table>

Control variables included | Yes | Yes | Yes | Yes | Yes | Yes |
Δ Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
Neighbourhood fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
Number of observations | 9,544 | 9,125 | 12,653 | 9,544 | 9,125 | 12,653 |
\( R^2 \) | 0.484 | 0.484 | 0.868 | 0.086 | 0.868 |
Kleibergen-Paap F-statistic | 186.1 | 15.50 | 186.1 | 15.50 |
Bandwidth \( h \) | 2.00 | 2.00 | 2.00 | 2.00 |

Notes: We exclude observations within one kilometre of targeted areas in Columns (1), (2), (4) and (5). We only include neighbourhoods with \( 5.3 < z < 9.3 \). 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
Columns (1) and (3) represent regressions for log prices and log sales time respectively, where we include a linear interaction of the change in years after the investment and the treatment status. For prices, the results do not suggest that there is an adjustment effect. The effect of sales time seems to become somewhat weaker over time. The coefficients imply that the sales time effect has reduced to 5.5 percent after three years. However, one may argue that potential adjustments effects are nonlinear. We therefore include a third-order polynomial of the years after the investment interacted with the treatment status. Because this implies a nonparametric function of years after the investment, we cannot use standard instrumental variables techniques. Hence, we use the sharp RDD and exclude neighbourhoods that were not selected although their score was above 7.3 and neighbourhoods that were targeted that had a score below 7.3. It is shown that both for house prices and sales times, all coefficients for prices are now statistically insignificant (Column (2), Table 5). However, it is more insightful to test the joint significance of the coefficients over time. Figure C1 in Appendix C provides only weak evidence of adjustment effects. The price effect immediately after the investment is about 1.7 and increases to 3.5 percent after 2 years. However, the differences are not statistically significantly different from each other. The effect of sales times tends to become less pronounced over time, but the confidence interval is too large to draw strong conclusions. Moreover, it seems that after three years there is still an effect of sales time, although it is not statistically significant at the five percent.

Although it is not 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 Column (3) whether there are spatial spillovers. This implies that we also include observations within one kilometre of a KW-neighbourhood, and include 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 to neighbourhoods with a z-score of at least 7.3 after the start date of the programme. The results show that the effect in KW-neighbourhoods for house prices is somewhat stronger within 500 meters of KW neighbourhoods than in KW neighbourhoods itself, while the price effect becomes statistically insignificant beyond 500 meters (Column (3), Table 5). For sales times, the picture is similar: sales times have been reduced more strongly close to KW neighbourhoods (Column (6)). For example, within 500 meters of a KW neighbourhood, the sales time has been reduced with about 18 percent. After 500 meters, the effect becomes statistically insignificant. It may seem not so obvious that the price and sales time effects are 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). 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.
Nevertheless, note that the coefficients related to observations in and outside but within 500 meter of KW-neighbourhoods are not statistically significantly different.

C. Sorting

Place-based policies may increase the amenity level, but may also influence the composition of the population. For example, because the share of public housing decreases, the average neighbourhood income may increase and the share of foreigners may decrease. These indirect effects may partly explain the positive price effects and decrease in sales times. To test this we gather additional data on demographics in the neighbourhoods. 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, we think that it is informative to see if the coefficients related to the place-based investments are in any way influenced by inclusion of these potentially endogenous variables. Table 6 reports the results.

<table>
<thead>
<tr>
<th>Table 6 — Sensitivity Analysis: Sorting</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel 1: Δ Price per m² (log)</strong></td>
</tr>
<tr>
<td>(1) 2SLS-LL  (2) 2SLS-LL  (3) 2SLS-LL</td>
</tr>
<tr>
<td>Δ KW investment</td>
</tr>
<tr>
<td>(0.0126)</td>
</tr>
<tr>
<td>Δ Population density (log)</td>
</tr>
<tr>
<td>(0.0930)</td>
</tr>
<tr>
<td>Δ Share foreigners</td>
</tr>
<tr>
<td>(0.636)</td>
</tr>
<tr>
<td>Δ Share non-western foreigners</td>
</tr>
<tr>
<td>(0.676)</td>
</tr>
<tr>
<td>Δ Share young people</td>
</tr>
<tr>
<td>(0.546)</td>
</tr>
<tr>
<td>Δ Share elderly people</td>
</tr>
<tr>
<td>(0.494)</td>
</tr>
<tr>
<td>Δ Average household size</td>
</tr>
<tr>
<td>(0.138)</td>
</tr>
<tr>
<td>Land use variables included</td>
</tr>
<tr>
<td>Control variables included</td>
</tr>
<tr>
<td>Δ Year fixed effects</td>
</tr>
<tr>
<td>Neighbourhood fixed effects</td>
</tr>
<tr>
<td>Neighbourhood-specific time trends</td>
</tr>
<tr>
<td>Number of observations</td>
</tr>
<tr>
<td>Kleibergen-Paap F-statistic</td>
</tr>
<tr>
<td>Bandwidth h</td>
</tr>
</tbody>
</table>

Notes: We exclude observations within one kilometre of targeted areas and in neighbourhoods that were not on the initial list. We only include neighbourhoods with 5.3 < z < 9.3. Standard errors are clustered at the neighbourhood level.

*** Significant at the 0.01 level
**  Significant at the 0.05 level
*   Significant at the 0.10 level
In Columns (1) and (4) we include population density, the share of foreigners, share of non-western foreigners, share of young (<25 years) and elderly people (>65 years) and the average household size in the regressions related to prices and sales time respectively. The coefficients are almost unaffected by inclusion of these controls, which suggests that sorting on observable neighbour characteristics is not a main determinant of the statistically significant effect of place-based policies. Most of the neighbourhood variables are statistically insignificant. A notable exception is population density. Changes in population density are associated with price increases and shorter selling times. 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. Also, neighbourhoods that receive an influx of young people have relatively shorter selling times. Young people may be more flexible in their housing choices than older people, and they may have lower search costs, leading to a lower selling time (Cheshire et al., 2014).

In Columns (2) and (4) we also include variables related to changes in land use, using data from Statistics Netherlands for 2000, 2003, 2006 and 2008. 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 infrastructure, industrial activities, open space, activities and residences for each neighbourhood. It appears that inclusion of the land use variables leads to almost the same coefficients for prices and sales time.

One may argue that sorting on unobservables is much more important than sorting on the few observable demographic characteristics we included in the previous specifications. Although inclusion of neighbourhood fixed effects should partly deal with this, in Columns (3) and (6) we choose an even more flexible approach, by including neighbourhoods-specific linear time trends. This implies that neighbourhoods-specific price and sales time trends may increase or decrease more than proportional over time. It can be immediately seen that this does not influence the results. The effect of place-based policies have increased prices with 3.2 effect and decreased sales times with 17.9 effect. Hence, the results seem to suggests that the effect of the place-based investments is mainly direct and is not much correlated with sorting or general trends in prices or sales times.

D. Quasi-placebo tests
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 7 reports the results.
The first placebo-experiment uses a list of 340 deprived neighbourhoods published by the Dutch secretary of state Pieter Winsemius in 2006, of which the 83 neighbourhoods were selected in the end. We therefore treat the non-treated neighbourhoods as if they are KW-neighbourhoods and received funds in 2007 and exclude the observations in and close to (within one kilometre) 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 one kilometre of a neighbourhood on the Winsemius list. Columns (1) and (4) highlight that there is no general trend in prices or sales times in deprived neighbourhoods that were not targeted. In any case, in the preferred specifications, we focus on neighbourhoods that were on the Winsemius list only, so also if we would find a certain trend effect, this would not be a problem.

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 one kilometre) of a KW-neighbourhood. Again, we also exclude observations within one kilometre of a ‘Kamp’-neighbourhood to avoid biases due to spatial spillovers. Columns (2) and (5) in Table 7 show that the coefficients for house prices and sales time are highly statistically insignificant. This result is particularly convincing for house prices, where the standard 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 differences in timing. We then assume that KW neighbourhoods have received funding in 2003 (the 'Kamp'-programme) rather than 2007. This implies that we assume that the KW-programme took place in 2003 rather than in 2003. If there are, again, general trends in these neighbourhoods, we would find positive price effects and negative effects on sales times. We then exclude observations that took place after the official investment date (September 14, 2007). The results in Columns (3) and (6) suggest that there is no meaningful price effect and sales time effect, which confirms that there seem not to be trends that are correlated with the KW-programme.

### E. Full sample

We have used repeated sales and first-differencing to estimate the effects of interest. However, one may argue that repeated sales are a non-random sample of the full sample of houses. For example, it might be that the most attractive houses are sold less often, because people have fewer incentives to move. We showed that there are hardly structural differences between the full sample and the repeated sales sample (see Table 2 and Table B1 in Appendix C). 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 8 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 B1, Appendix C). The coefficients suggest a positive price effect of the programme of 4.5 percent. Sales times have been reduced with 17.8 percent. In Columns (2) and (4) we only select neighbourhoods that were on the initial list of 340 neighbourhoods and include flexible neighbourhood-specific time trends. The price effect is then even somewhat larger (5.8 percent). In Columns (3) and (6), Table 8, we only focus on observations in neighbourhoods that are close to the threshold. The price effect is hardly affected by this selection and is still above 5.5 percent. The investment programme has reduced sales times with 18.2 percent. We then may conclude that the results using the full sample are very similar to the baseline results, although somewhat larger in magnitude. Hence, if anything, our initial estimates are conservative.
### TABLE 8 — Sensitivity analysis: full sample

<table>
<thead>
<tr>
<th>Panel 1: Price per m² (log)</th>
<th>Panel 2: Days on the market (log)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
</tr>
<tr>
<td>KW investment</td>
<td>0.0437*** (0.0104)</td>
</tr>
<tr>
<td>Control variables included</td>
<td>Yes</td>
</tr>
<tr>
<td>Year fixed effects</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighbourhood-specific time trends</td>
<td>No</td>
</tr>
<tr>
<td>PC6 fixed effects</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>1,231,106</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.819</td>
</tr>
<tr>
<td>Kleibergen-Paap F-statistic</td>
<td>451.2</td>
</tr>
<tr>
<td>Bandwidth $h$</td>
<td>$\infty$</td>
</tr>
</tbody>
</table>

Notes: We exclude observations within one kilometre of targeted areas. In Column (2), (3), (5) and (6) we exclude neighbourhoods that were not on the initial list. We only include neighbourhoods with 5.3 < z < 9.3 in Columns (3) and (6). 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

#### F. Propensity score matching

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

\[
Pr(\ell = 1 \mid a_{c}) = \Phi(Y_{c}(a_{c})),
\]
where $\Pr(\ell = 1 \mid a_{\ell})$ is the probability that a neighbourhood $\ell$ is selected, $\Phi(\cdot)$ is the cumulative distribution function of the normal distribution and $Y_{\ell}(\cdot)$ is a nonparametric function of attributes $a_{\ell}$. 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. So, the impact of $a_{\ell}$ on $\Pr(\ell = 1 \mid a_{\ell})$ depends on the location of the neighbourhood. The kernel weights for $\ell$ are equal to $\omega_{\ell} = 1/d_{\ell}$, where $d_{\ell}$ is a vector capturing the kilometre distance between the centroid of $\ell$ 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 caliper 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 C3 in Appendix C presents the means and standard deviations at the neighbourhood level for the KW-neighbourhoods and three different sets of control neighbourhoods. It appears that the control neighbourhoods are relatively similar to the KW-neighbourhoods in most neighbourhood attributes. Table 9 reports the results.

Columns (1) and (4) use the set of control neighbourhoods based on caliper matching. The price effect is then 4.0 percent, similar to baseline specifications. The effect on sales times is somewhat lower and only marginally statistically significant. However, the standard error is quite large, so the estimate is not statistically significantly different from the baseline estimate. In Columns (2) and (5) we use nearest neighbour matching without replacement. It can be seen that the effects of place-based policies are similar to the baseline specification, although the effect of sales time is again somewhat smaller in magnitude. The results suggest that the investments have led to a decrease in sales time of 12.5 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 is not statistically significant at conventional levels. However, because we only select 38 control neighbourhoods, this seems mainly an issue of precision, because the point estimate is very similar to previous specifications.

---

39 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 points lower. We note that the propensity scores of non-control neighbourhoods are very close to zero, suggesting that our model performs reasonably well.
TABLE 9 — SENSITIVITY ANALYSIS: PROPENSITY SCORE MATCHING

<table>
<thead>
<tr>
<th>Panel 1: Δ Price per m² (log)</th>
<th>Panel 2: Δ Days on the market (log)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td></td>
<td>OLS</td>
</tr>
<tr>
<td>Δ KW investment</td>
<td>0.0390***</td>
</tr>
<tr>
<td></td>
<td>(0.00961)</td>
</tr>
<tr>
<td>Control variables included</td>
<td>Yes</td>
</tr>
<tr>
<td>Δ Year fixed effects</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighbourhood fixed effects</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>12,484</td>
</tr>
<tr>
<td>$R^2$-within</td>
<td>0.486</td>
</tr>
<tr>
<td>Matching method</td>
<td>Calipher</td>
</tr>
<tr>
<td>Control neighbourhoods</td>
<td>116</td>
</tr>
</tbody>
</table>

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

G. Bandwidth selection

The baseline specifications use local linear estimation techniques, by only selecting neighbourhoods that have z-scores that are within two points within the threshold score of 7.3, implying that $h = 2$. This bandwidth choice is obviously somewhat arbitrary. To guide the bandwidth choice $h$, we may use a cross-validation procedure. We are aware that cross-validation have asymptotic properties that imply extremely slow rates of convergence, which may lead to noisy measures of the optimal bandwidth (Ludwig and Miller, 2005; Hastie et al., 2009). We assume that the bandwidth is the same on both sides of the threshold to avoid the disadvantage of having additional noise in estimating the optimal value from a smaller sample (Imbens and Lemieux, 2008). The cross-validation criterion is then:

$$ CV_{ΔY}(h) = \frac{1}{N} \sum_{n=1}^{N} (Δy_{nt} - Δ\tilde{y}_{nt}(h))^2, $$

Because we employ a fuzzy regression-discontinuity design, we also estimate a first stage. Therefore, to avoid asymptotic biases, we use the smallest bandwidth selected by the cross-validation criterions of both stages. Hence, $h_{CV}^* = \min.CV_{δk}(h),.CV_{ΔY}(h)$. It appears that $CV_{ΔY}(h)$ provides very noisy measures for $h < 1$, so we assume that $h \geq 1$. Because we have multiple dependent variables and we wish to use the same samples for the regressions of price and days on the market, we first take the average of $h_{CV}^*$ for all $y$, and alternatively take the minimum and maximum, respectively. Table 10 reports the results.
In Columns (1) and (4) we take the average of the optimal bandwidth of the price and sales time regressions, leading to $h_{CV}^* = 1.89$, which is very close to the bandwidth used in the preceding analyses. It is therefore not too surprising that the coefficients are almost the same as the baseline specifications in Table 3 and 4. In Columns (2) and (5) we take the minimum of the two optimal bandwidths, implying $h_{CV}^* = 1.46$. The price effect is still there and highly statistically significant. The effect of the programme on sales times is still negative, but very imprecisely estimated, due to the low number of observations. In any case, sales time is a much more noisy variable than prices (e.g. because household may already have found buyers before they put their house on the market), so this is not too surprising. If we take the maximum, $h_{CV}^* = 2.30$. The price effect is then 3.1 percent and the sales time effect $-16.1$ percent, similar to the baseline results.

H. **Start date of investment**

The exact start 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 start 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 start 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 the start date. It is shown that the effect on house prices and sales times
are very similar to the specifications reported in Column (6) in Tables 3 and 4. Columns (2) and (5) take January 1, 2008 as a start date. The coefficient of the sales time regression is then somewhat lower, suggesting that there was already a decrease in sales time before the first of January. The price effect is very similar. In Columns (3) and (6) we just avoid the problem by excluding transactions that took place in 2007. Although the coefficient related to prices is only marginally statistically significant, the effect is very similar to previous specifications. Also the sales time effect is very comparable. Hence, although the exact start date of the programme is somewhat unclear, it does not seem to bias our results.

**Table 11 — Sensitivity analysis: start date of investment**

<table>
<thead>
<tr>
<th></th>
<th>Panel 1: Δ Price per m² (log)</th>
<th>Panel 2: Δ Days on the market (log)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Δ KW investment</td>
<td>0.0345** (0.0139)</td>
<td>0.0331** (0.0142)</td>
</tr>
<tr>
<td>Control variables included</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Δ Year fixed effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighbourhood fixed effects</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>9,544</td>
<td>9,544</td>
</tr>
<tr>
<td>Kleibergen-Paap F-statistic</td>
<td>497.8</td>
<td>452.1</td>
</tr>
<tr>
<td>Bandwidth $h$</td>
<td>2.00</td>
<td>2.00</td>
</tr>
</tbody>
</table>

Notes: We exclude observations within one kilometre 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

VI.  **Counterfactual analysis and welfare**

[INCOMPLETE, TO BE UPDATED].

We aim to gain insight in the rate of return of the external effect of the revitalisation policy. We emphasise that expenditures were financed from additional and external sources and were not part of the municipal budget or the budget of housing associations. When this is not the case, and expenditures were e.g. raised by limiting expenses in other neighbourhoods, this may imply that externalities are negative in non-targeted areas (Rossi-Hansberg et al., 2010). In any case, one should be very careful in interpreting the numbers as an overall measure of the benefits of the investment programme, but we consider them as suggestive. We use data on the number of households in each neighbourhood, as well as data on the share owner-occupied housing from respectively Statistics Netherlands and a housing survey (WoOn Survey) from 2006. The median house price (2007 prices) is obtained from the NVM data.
### Table 12 — Counterfactual Analysis

<table>
<thead>
<tr>
<th></th>
<th>All neighbourhoods</th>
<th>KW neighbourhoods</th>
<th>Non-treated neighbourhoods</th>
</tr>
</thead>
<tbody>
<tr>
<td>Housing units</td>
<td>17,123,176</td>
<td>808,877</td>
<td>16,314,296</td>
</tr>
<tr>
<td>Median house price</td>
<td>€ 253,375</td>
<td>€ 153,340</td>
<td>€ 255,486</td>
</tr>
<tr>
<td>Share owner-occupied</td>
<td>0.666</td>
<td>0.225</td>
<td>0.675</td>
</tr>
<tr>
<td>No spillovers, only owner-occupied</td>
<td>€ 853,721,280</td>
<td>€ 853,721,280</td>
<td>€ 0</td>
</tr>
<tr>
<td>No spillovers, all properties</td>
<td>€ 3,040,906,766</td>
<td>€ 3,040,906,766</td>
<td>€ 0</td>
</tr>
<tr>
<td>Spillovers, only owner-occupied</td>
<td>€ 3,125,677,699</td>
<td>€ 853,721,280</td>
<td>€ 2,271,956,419</td>
</tr>
<tr>
<td>Spillovers, all properties</td>
<td>€ 7,857,548,976</td>
<td>€ 3,040,906,766</td>
<td>€ 4,816,642,210</td>
</tr>
<tr>
<td>Number of neighbourhoods</td>
<td>4016</td>
<td>83</td>
<td>3933</td>
</tr>
</tbody>
</table>

Notes: The number of housing units is calculated as the number of households plus a fixed vacancy rate of 4.7 percent (Statistics Netherlands).

A conservative estimate of the gains of the investment programme is to assume that the price effect only capitalises in owner-occupied housing, using the argument that about 85 percent of the rental market is rent-controlled. We then calculate the benefits by multiplying the median price with the estimate of our baseline Specification (6) (see Table 3), the number of housing units and the share owner-occupied housing in each neighbourhoods. We then sum up the benefits for each neighbourhood type. It then appears that the gain-to-funding ratio is 0.853 if the investment is one billion euro (see Table 6). However, one may argue that, while the subsidy does not capitalise in actual rents, renters still enjoy the positive external effects that are caused by the programme. To include these social benefits, we have to estimate the market value of rental housing. It appears that the rental housing value is 67.9 and 69.3 percent of the median house price in respectively Amsterdam and Rotterdam (see Van Ommeren and Koopman, 2011; Van Ommeren and Van der Vlist, 2014)). Based on these figures, we assume that the median value of rental housing is 68.5 percent of the median house price in each neighbourhood. Because the share of rental housing is large in KW-neighbourhoods (only 23 percent), the benefits are now substantially larger. The results suggest a gain-to-funding ratio of 3.040, which is very much in line with Rossi-Hansberg et al. (2010).

One may argue that this is still an underestimate because we ignore any spatial spillovers. Specification (2) in Table 5 suggests that there are price effects outside the targeted areas, but not beyond 500 meters. We then calculate the share of each postcode within 500 meter of a KW-neighbourhood and assume a spillover price effect of 4.8 percent (see Section V.B). If we ignore any price effects on rental housing, the gain-to-funding ratio is 3.126. Note that almost 75 percent of the benefits can be assigned to houses outside targeted neighbourhoods. If we assume that the price effect also capitalises in rental housing, the benefits are more than 7 billion, implying a gain-to-funding ratio of 7.858.

---

40 This is under the assumption that there are no tax frictions in the housing market. Of course, this is not true, because in the Netherlands house owners can deduct their costs from their taxes.
VII. Conclusions
In many countries, governments invest in deprived neighbourhoods to narrow income disparities within cities and fight social problems. There is limited understanding what the effects of place-based policies are if bargaining and search processes are taken into account. We develop a housing price bargaining model in the spirit of Wheaton (1990) and show that investments capitalise in house prices, but this effect is much smaller than in a model without bargaining. Moreover, we show that place-based policies have a permanent effect on sales time in steady state. The presence of such an effect indicates that bargaining and search processes are important in the housing market and should be taken into account in empirical studies.

We test these predictions of the effects of place-based policies on the housing market using a nationwide investment programme that aims to restructure and revitalise the poorest neighbourhoods in the Netherlands. A rich repeat sales dataset on house sales in the period 2001-2011 is used. We combine a first-differences approach with a (fuzzy) regression-discontinuity design based on a jump in the probability to be treated. The probability to become treated is dependent on neighbourhood-specific deprivation score. We find compelling evidence for the presence of positive effects of the investment scheme. In line with our model, public investments have led to an increase in house prices of about 2.5-4 percent, which is very similar in magnitude compared to previous findings by Rossi-Hansberg et al. (2010). It is also shown that the sales time has been reduced with about 10 days (15 percent). The latter effect suggests that standard spatial equilibrium models that infer welfare changes from price changes are likely to provide incorrect estimates, because search frictions are ignored, but are relevant in practice. More specifically, under the condition that search is approximately at its welfare optimising level, price changes may lead to underestimates of welfare changes.

References


Appendix A. Proofs

A.1 Proof of Proposition 1

We first prove that in a partial equilibrium $\partial e_i / \partial u_i > 0$, $\partial e_j / \partial u_i > 0$ and $\partial e_i / \partial u_i > \partial e_j / \partial u_i$. Then we will show that $P_i / P_j$. The first step is to define the first-order conditions for maximisation. Implicit differentiation of (16) yields:

$$\frac{\partial \tilde{e}_i}{\partial u_i} = \frac{m_i \partial (U^p_i - U^s_i - P_i)}{e_i} \frac{\partial (U^p_i - U^s_i - P_i)}{\partial e_i} \bigg|_{e_i = \tilde{e}_i}$$

Note that $U^p_i$ and $P_i$ are given for an individual household. Because the average effect is equal to the marginal effect (see (15)), it holds that $\frac{\partial e_t}{\partial u_i} \bigg|_{e_t = e_i} = 0$. The last term in the denominator is equal to zero, so that:

$$\frac{\partial \tilde{e}_i}{\partial u_i} = \frac{m_i \partial (U^p_i - U^s_i - P_i)}{e_i} \frac{\partial (U^p_i - U^s_i - P_i)}{\partial e_i} \bigg|_{e_i = \tilde{e}_i}.$$

Using (18), we write the above equation as:

$$\frac{\partial \tilde{e}_i}{\partial u_i} = \frac{m_i \sigma (1 - \sigma) q_j + w_j}{e_i w_j w_i + q_i w_j + q_j w_i}.$$

It can be immediately seen that $\frac{\partial \tilde{e}_i}{\partial u_i} > 0$. Similarly, it may be shown that:

$$\frac{\partial \tilde{e}_j}{\partial u_i} = \frac{m_j \sigma (1 - \sigma) q_i}{e_j w_j w_i + q_i w_j + q_j w_i}.$$

Note that in the point where $u_i = 0$, it is true that in a partial equilibrium $\frac{\partial \tilde{e}_i}{\partial u_i} > \frac{\partial \tilde{e}_j}{\partial u_i}$ because:

$$\frac{\partial \tilde{e}_i}{\partial u_i} = 1 + \frac{w_i}{(1 - \sigma) q_i},$$

where $w_i$ is positive.

The next step is to focus on the effect of $u_i$ on house prices. Let us define $P_i = P(v_i, e_i, e_j)$ and $P_j = P(v_i, e_i, e_j)$. We then start from the observation that:

$$\frac{d(P_i/P_j)}{du_i} = \frac{\partial (P_i/P_j)}{\partial e_i} + \frac{\partial (P_i/P_j)}{\partial e_j} \frac{\partial e_i}{\partial u_i} + \frac{\partial (P_i/P_j)}{\partial e_j} \frac{\partial e_j}{\partial u_i}$$

where $\partial (P_i/P_j)/\partial e_i$ is the derivative conditional on $e_j$ and $u_i$, but taking into account the effects of $e_i$ through $m_i, q_i, c_i, m_j, q_j, c_j$. It is straightforward to show that on the right-hand side, the first term is positive (see equations (17) and (19)), the second term is always positive, whereas the third term is negative. However, when $e_j = e_i$, (20) implies that
\[ \frac{\partial (P_i/P_j)}{\partial e_i} = - \frac{\partial (P_i/P_j)}{\partial e_j}. \] Furthermore, we know from (46) that \( \frac{\partial e_i}{\partial v_i} > \frac{\partial e_j}{\partial v_i} \) so that the second term exceeds the third term.

Finally, equation (20) implies that search effort is always higher in \( i \) than in \( j \) due to the investment because \( \frac{d (P_i/P_j)}{dv_i} > 0 \). □

### A.2 Proof of Proposition 2

We use implicit function theory to prove \( \frac{\partial m_i}{\partial e_i} |_{e_j = e_i} > 0 \). Let us start with \( m_i - m(e_i, V_i/H_i^S) = 0 \). According to Cramer’s rule, \( \frac{\partial m_i}{\partial e_i} = \frac{\det(Z_i)}{\det(Z)} \), where matrix \( Z \) is denoted by:

\[
Z = \left( \begin{array}{cc}
1 - \frac{\partial m}{\partial (V_i/H_i^S)} & \frac{\partial m}{\partial (V_i/H_i^S)}
\end{array} \right) - \left( \begin{array}{cc}
\frac{\partial m}{\partial (V_i/H_i^S)} & \frac{\partial m}{\partial (V_i/H_i^S)}
\end{array} \right).
\]

and \( Z_i \) as:

\[
Z_i = \left( \begin{array}{cc}
\frac{\partial m}{\partial e_i} & \frac{\partial m}{\partial (V_i/H_i^S)}
\end{array} \right).
\]

Let us define:

\[
\frac{V_i}{H_i^S} = \frac{\bar{S}}{(2H - \bar{S})} - \frac{2\beta}{2\beta + m_j} + \frac{\partial (V_i/H_i^S)}{\partial m_i} m_i = k_0 + k_1 m_i.
\]

Because \( k_0 > 0 \), it is true that \( V_i/H_i^S > k_1 m_i \). Due to concavity, the following inequality holds:

\[
\frac{\partial m}{\partial (V_i/H_i^S)} \leq \frac{m_i}{V_j/H_j^S}.
\]

Given that the economy is in symmetry \( e_i = e_j, m_i = m_j \) and \( V_i/H_i^S = V_j/H_j^S \). Hence,

\[
\frac{\det(Z_i)}{\frac{\partial m}{\partial e_i}} = \frac{\partial m}{\partial e_i} \left( 1 - \frac{\partial m}{\partial (V_i/H_i^S)} \right) \geq \frac{\partial m}{\partial e_i} \left( 1 - \frac{m_i}{V_i/H_i^S} \frac{\partial (V_i/H_i^S)}{\partial m_i} \right).
\]

Using (50), we may write the last equation as:

\[
\frac{\partial m}{\partial e_i} \left( 1 - \frac{k_1 m_i}{k_0 + k_1 m_i} \right),
\]

which is greater than zero. Hence, \( \frac{\det(Z_i)}{\frac{\partial m}{\partial e_i}} > 0 \).

The next step is to determine that \( \det(Z) > 0 \). We write \( \frac{\partial (V_i/H_i^S)}{\partial m_i} |_{e_j = e_i} = k_2 - \frac{\partial (V_i/H_i^S)}{\partial m_j} |_{e_j = e_i} \), with \( k_2 = (\bar{S} - H)/\beta (2H - \bar{S}) \). Then,

\[
\det(Z) |_{e_j = e_i} = \left( 1 - \frac{\partial m}{\partial (V_i/H_i^S)} \right)^2 - \left( \frac{\partial m}{\partial (V_i/H_i^S)} \frac{\partial (V_i/H_i^S)}{\partial m_j} \right)^2
\]

\[
= \left( 1 - k_2 \frac{\partial m}{\partial (V_i/H_i^S)} + \frac{\partial m}{\partial (V_i/H_i^S)} \frac{\partial (V_i/H_i^S)}{\partial m_j} \right)^2 - \left( \frac{\partial m}{\partial (V_i/H_i^S)} \frac{\partial (V_i/H_i^S)}{\partial m_j} \right)^2.
\]
\[ \det(Z)_{e_j=e_i} \text{ is positive when } 1 - k_2 \frac{\partial m}{\partial (V_i/H_i^s)} > 0. \text{ Due to concavity, it must hold that} \\
1 - \frac{\partial m}{\partial (V_i/H_i^s)} k_2 < 1 - k_2 \frac{m_i}{(V_i/H_i^s)}. \text{ Using (11), we write:} \\
(55) \quad 1 - \frac{k_2 m_i}{(V_i/H_i^s)} = 1 - \frac{k_2 m_i}{(2H - \bar{S}) - 2\beta + m_i + k_2 m_i}.
\]

Because \( m_i = m_j \), the second term of the denominator is equal to one. Hence, the denominator is positive because \( \bar{S}/(2H - \bar{S}) > 1 \). As a result, the second term in equation (55) is always less than one and \( \det(Z)_{e_j=e_i} > 0 \). Hence, \( \det m_i/\partial e_i|_{e_j=e_i} = \det(Z)_{e_j=e_i}/\det(Z)_{e_j=e_i} > 0 \).

The next step is to show that \( \partial(\frac{m_i}{m_j})/\partial e_i > 0 \). Again we evaluate this derivative in the point \( e_j = e_i \). This implies that \( \partial m_j/\partial e_j|_{e_j=e_i} = \partial m_i/\partial e_i|_{e_j=e_i} \). We again use Cramer's rule, where \( Z_j \) is given by:
\[
(56) \quad Z_j = \begin{pmatrix} 0 & -\frac{\partial m}{\partial (V_i/H_i^s)} & \frac{\partial (V_i/H_i^s)}{\partial m_i} \\ \frac{\partial m}{\partial e_j} & 1 & -\frac{\partial m}{\partial (V_i/H_i^s)} & \frac{\partial (V_i/H_i^s)}{\partial m_j} \end{pmatrix}.
\]

Given that the economy is in symmetry \( e_i = e_j, m_i = m_j \) and \( V_i/H_i^s = V_j/H_j^s \). Hence,
\[
(57) \quad \det(Z_j)|_{e_j=e_i} = \frac{\partial m}{\partial e_j} \frac{\partial (V_i/H_i^s)}{\partial m_i} \frac{\partial (V_i/H_i^s)}{\partial m_j}.
\]

Hence, it must hold that:
\[
(58) \quad 1 - \frac{\partial m}{\partial (V_i/H_i^s)} \frac{\partial (V_i/H_i^s)}{\partial m_i} \geq \frac{\partial m}{\partial (V_i/H_i^s)} \frac{\partial (V_i/H_i^s)}{\partial m_j}.
\]

Using (10) and (11), we have:
\[
(59) \quad 1 - \frac{\partial m}{\partial (V_i/H_i^s)} \left( \frac{\bar{S} - H}{\beta(2H - \bar{S})} \right) \geq 0.
\]

It must hold that the elasticity \( \left( \frac{\partial m}{\partial (V_i/H_i^s)} \right) \frac{(V_i/H_i^s)/m_i} \leq 1 \), so that we must prove that \( (\bar{S} - H)/(\beta(2H - \bar{S})) - (V_i/H_i^s)/m_i \leq 0 \). Given (10), we have:
\[
(60) \quad -\frac{1}{m_i} \left( \frac{\bar{S}}{2H - \bar{S}} - 1 \right) \leq 0.
\]

Because \( 2\bar{H} - \bar{S} < \bar{S} \), \( \det(Z_j)|_{e_j=e_i} < \det(Z_j)|_{e_j=e_i} \) and the effect of search effort \( e_i \) on \( m_i \) exceeds the impact on \( m_j \).

The final step is to investigate the effect on sales times, given that \( m_i > m_j \). Given equation (9), we have:
\[
(61) \quad L_i = \frac{m_j(2\beta + m_i)}{m_i(2\beta + m_j)} \left( \bar{H}(m_i m_i + 4\beta(\beta + m_i)) - \bar{S}(m_i m_j + \beta(4\beta + 3m_i + m_j)) \right)
\]

\[
L_j = \frac{m_j(2\beta + m_i)}{m_i(2\beta + m_j)} \left( \bar{H}(m_i m_j + 4\beta(\beta + m_j)) - \bar{S}(m_i m_j + \beta(4\beta + 3m_i + m_j)) \right).
\]
We take the derivative of this ratio with respect to \( m_i \) and evaluate the derivative when \( e_j = e_i \), which implies that \( m_j = m_i \). Then:

\[
\frac{\partial (L_i/L_j)}{\partial m_i} \bigg|_{e_j = e_i} = -\frac{2\beta (m_i \bar{H} + 2\beta (\bar{S} - \bar{H}))}{m_i (m_i + 2\beta)^2 (\bar{S} - \bar{H})}.
\]

Because \( \bar{S} > \bar{H} \), \( L_i < L_j \) for a marginal increase in \( m_i \). □

**Appendix B. Numerical simulations**

**B.1 The effects of an improved amenity level**

We may also analyse price, sales times differences and welfare differences numerically. The model is solved using an iterative two-step procedure.\(^{41}\) We assume that \( \bar{S} = 1000 \) and \( \bar{H} = 900 \), which implies a vacancy rate of 10 percent. We assume a (concave) constant returns to scale Cobb-Douglas matching function \( m_i = e^\omega_i (V_i/H_i^S)^{1-\omega} \forall i \), with \( \omega = 0.50 \), and a convex cost function \( c_i = \chi e_i^2 \forall i \), with \( \chi = 0.5 \). We furthermore assume that sellers and buyers have equal bargaining power (\( \sigma = 0.5 \)) and the interest rate is 2.5 percent (\( r = 0.025 \)). The initial amenity \( A \) level is set to 10. We analyse the outcomes when the market is in steady state. We are interested in the effect of a marginal increase in the amenity level in \( i \), so we assume \( v_i = 1 \). We analyse the impact for different intensities of people to move (\( \beta \)), the magnitude of search costs (\( \chi \)) and the bargaining power of sellers and buyers (\( \sigma \)).

Figure 1 reports price (left y-axis) and sales time differences (right y-axis). We also plot the change in the welfare per household with respect to the situation where \( v_i = 0 \) (right y-axis), which corresponds to equation (29). It is shown that when \( \beta \to 0 \) (households do not move), the price and sales time difference becomes negligible.\(^{42}\) However, when households move more often, the effects of place-based policies become more pronounced and should lead to higher prices and lower sales times in the targeted neighbourhood. It is shown that the price differences approaches \( 1/2r \) when \( \beta \to \infty \). We also estimate welfare using equation (21). It is apparent that the welfare difference approaches the maximum \( 1/r \), whereas when \( \beta \to \infty \) the welfare difference converges to \( 1/2r \). The last observation we can make is that if we observe larger sales time differences, the price estimate is closer to the welfare difference due to the placed-based policy.

\(^{41}\) First, conditional on an initial (arbitrary) choice of values for number of household variables \( H_i^S, H_i^F, H_i^M, H_i^H \), we solve for house prices, optimal search effort in both areas, using equations (12) to (16). Given optimal search effort, we determine \( m_i, m_j, q_i \) and \( q_j \). Given these values, we calculate the number of households in each state using equations (3) to (5). We continue this process until the model converges.

\(^{42}\) One may argue that in the empirical section we do not measure \( P_i - P_j \), but \( P_i - P_{10} \). However, we note that the numerical results in this appendix would be qualitatively the same if we would plot \( P_i - P_{10} \) instead of \( P_i - P_j \). \( P_i - P_{10} \) is also always lower than \( 1/2r \) and is almost one-to-one correlated to \( P_i - P_j \).
Next, we analyse the differential impact of an increase in \( v_i \) for different values of \( \chi \). When \( \chi \) is higher, we expect that search costs are more important and therefore that the impact of search frictions on welfare will also be more pronounced. Figure 2 presents the results. It is apparent that without search costs, the price difference is small and the sales time difference is also close to zero.\(^{43}\) However, when \( \chi \) becomes large and approaches infinity, \( P_i - P_j \rightarrow 1/2r \). The welfare increase due to an investment is the largest when \( \chi \rightarrow 0 \) and becomes smaller when search costs are more important. When \( \chi \rightarrow \infty \), \( P_i - P_j = \mathcal{W} - \mathcal{W}_0 = 1/2r \). So, again note that when sales time differences are 'large', the price difference is likely to be closer to the welfare estimate.

\(^{43}\) Note that the relative price difference is also small when \( \chi \rightarrow 0 \) and is maximised when \( \chi \rightarrow \infty \).
Figure 3 reports results for different values of $\sigma$. Recall that when $\sigma = 1$, buyers have all bargaining power. It is shown that price differences are higher for lower values of $\sigma$, possibly because sellers do not take into account search costs of buyers. When $\sigma \to 0$, $P_i - P_j \to 1/2r$. The sales time difference then becomes very large. The welfare difference is hardly related to $\sigma$. Only when $\sigma \to 0$, $\mathcal{W} - \mathcal{W}_0 = 1/2r$. This again confirms that when the sales time difference is larger, the price increase is closer to the welfare increase due to the places-based policy.

![Figure B3 - Welfare, Price and Sales Time Differences in Neighbourhood $i$ for Different $\chi$](image)

**Figure B3 — Welfare, Price and Sales Time Differences in Neighbourhood $i$ for Different $\chi$**

Notes: We assume that $S = 1000$, $H = 900$, $m_i = e_i^\omega (V_i / H_i^\chi)^{1-\omega} \forall i$, $e_i = \chi e_i^\omega \forall i$,

$r = 0.025$, $\omega = 0.5$, $\beta = 0.2$, $\chi = 0.5$, $A = 10$ and $v_i = 1$.

### B.2 Optimal search effort

In Section II.E we argued that the bargaining power parameter $\sigma$ that maximises welfare should always be between one and zero. In Figure B4 we report the welfare levels for different values of $\sigma$. When $v_i = 0$, the welfare level $\mathcal{W}_0$ is the highest when $\sigma^* = 0.18$. Also if we analyse the welfare levels when $v_i = 1$, it appears that $\sigma^* = 0.18$. That $\sigma^*$ is hardly affected by the place-based policy is in line with Figure B3, which showed that the welfare difference due to the policy is hardly related to changes in $\sigma$. 

---

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Figure B4 — Optimal Search Effort

Notes: We assume that $\tilde{S} = 1000, \bar{H} = 900, m_t = e_t^{\alpha}(V_t/H_t^{\beta})^{1-\omega} \forall i, c_t = \chi e_t^{\gamma} \forall i, , r = 0.025, \omega = 0.50, \beta = 0.20, \chi = 0.50, A = 10$ and $\nu_i = 1$.

Appendix C. Other descriptive statistics

Table C1 — Descriptive statistics for full sample

<table>
<thead>
<tr>
<th>Observations outside KW-neighbourhoods</th>
<th>Observations inside KW-neighbourhoods</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\mu$</td>
</tr>
<tr>
<td>House price per m² (in €)</td>
<td>1,980</td>
</tr>
<tr>
<td>Days on the market</td>
<td>137</td>
</tr>
<tr>
<td>KW investment received</td>
<td>0.000</td>
</tr>
<tr>
<td>In KW neighbourhood</td>
<td>0.000</td>
</tr>
<tr>
<td>Deprivation z-score</td>
<td>0.382</td>
</tr>
<tr>
<td>Size in m²</td>
<td>117</td>
</tr>
<tr>
<td>Rooms</td>
<td>4.33</td>
</tr>
<tr>
<td>Maintenance quality – good</td>
<td>0.867</td>
</tr>
<tr>
<td>Central heating</td>
<td>0.914</td>
</tr>
<tr>
<td>Listed</td>
<td>0.006</td>
</tr>
<tr>
<td>House type – apartment</td>
<td>0.297</td>
</tr>
<tr>
<td>House type – terraced</td>
<td>0.315</td>
</tr>
<tr>
<td>House type – semi-detached</td>
<td>0.268</td>
</tr>
<tr>
<td>House type – detached</td>
<td>0.120</td>
</tr>
<tr>
<td>Garage</td>
<td>0.308</td>
</tr>
<tr>
<td>Garden</td>
<td>0.620</td>
</tr>
<tr>
<td>Construction year &lt;1945</td>
<td>0.244</td>
</tr>
<tr>
<td>Construction year 1945-1960</td>
<td>0.073</td>
</tr>
<tr>
<td>Construction year 1961-1970</td>
<td>0.153</td>
</tr>
<tr>
<td>Construction year 1971-1980</td>
<td>0.167</td>
</tr>
<tr>
<td>Construction year 1981-1990</td>
<td>0.142</td>
</tr>
<tr>
<td>Construction year 1991-2000</td>
<td>0.156</td>
</tr>
<tr>
<td>Construction year &gt;2000</td>
<td>0.065</td>
</tr>
</tbody>
</table>

Notes: The number of observations outside KW-neighbourhoods is 1,348,963 and inside KW-neighbourhoods 20,857.
Notes: The continuous black line is obtained by a nonparametric regression of price per square meter on days on the market, while controlling for the time trend. The dashed line is obtained by the same regression including house fixed effects.

Table C3 — Propensity score matching sample

<table>
<thead>
<tr>
<th></th>
<th>KW-neighbourhoods</th>
<th>Control neighbourhoods</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\mu$</td>
<td>$\sigma$</td>
</tr>
<tr>
<td>Population density (ha$^2$)</td>
<td>9,081</td>
<td>5,171</td>
</tr>
<tr>
<td>Income</td>
<td>10,965</td>
<td>1,050</td>
</tr>
<tr>
<td>Median construction year</td>
<td>1,950</td>
<td>24</td>
</tr>
<tr>
<td>Share owner-occupied housing</td>
<td>0.459</td>
<td>0.180</td>
</tr>
<tr>
<td>Share foreigner</td>
<td>0.333</td>
<td>0.044</td>
</tr>
<tr>
<td>Share young</td>
<td>0.123</td>
<td>0.050</td>
</tr>
<tr>
<td>Share elderly</td>
<td>0.170</td>
<td>0.158</td>
</tr>
<tr>
<td>Share open space</td>
<td>0.224</td>
<td>0.038</td>
</tr>
<tr>
<td>Share social allowance</td>
<td>0.367</td>
<td>0.059</td>
</tr>
<tr>
<td>Share unemployed</td>
<td>0.471</td>
<td>0.047</td>
</tr>
</tbody>
</table>
### Appendix D. Other regression results

**Table D1 — First stage regression results**  
*(Dependent variable: change in log house price per square meter)*

<table>
<thead>
<tr>
<th></th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS</td>
<td>2SLS-LL</td>
</tr>
<tr>
<td>Δ Score rule ($z_t \geq 7.3$)</td>
<td>0.917***</td>
<td>0.919***</td>
<td>0.924***</td>
<td>0.921***</td>
</tr>
<tr>
<td></td>
<td>(0.0340)</td>
<td>(0.0353)</td>
<td>(0.0334)</td>
<td>(0.0384)</td>
</tr>
<tr>
<td>Δ Size (log)</td>
<td>0.00319</td>
<td>0.0176</td>
<td>-0.000689</td>
<td>-0.00209</td>
</tr>
<tr>
<td></td>
<td>(0.00247)</td>
<td>(0.0138)</td>
<td>(0.00622)</td>
<td>(0.0121)</td>
</tr>
<tr>
<td>Δ Rooms (log)</td>
<td>-0.000296</td>
<td>-0.00271*</td>
<td>0.000823</td>
<td>-0.000117</td>
</tr>
<tr>
<td></td>
<td>(0.000185)</td>
<td>(0.00154)</td>
<td>(0.000861)</td>
<td>(0.00116)</td>
</tr>
<tr>
<td>Δ Maintenance – good</td>
<td>0.000360</td>
<td>0.000660</td>
<td>0.00202</td>
<td>0.00186</td>
</tr>
<tr>
<td></td>
<td>(0.000466)</td>
<td>(0.00215)</td>
<td>(0.000861)</td>
<td>(0.00116)</td>
</tr>
<tr>
<td>Δ Central heating</td>
<td>0.000117</td>
<td>0.000201</td>
<td>0.00137</td>
<td>0.00149</td>
</tr>
<tr>
<td></td>
<td>(0.000753)</td>
<td>(0.00309)</td>
<td>(0.00126)</td>
<td>(0.00211)</td>
</tr>
<tr>
<td>Δ Listed building</td>
<td>0.00173</td>
<td>0.00722</td>
<td>0.00897</td>
<td>0.00657</td>
</tr>
<tr>
<td></td>
<td>(0.00503)</td>
<td>(0.0222)</td>
<td>(0.0122)</td>
<td>(0.0177)</td>
</tr>
<tr>
<td>G ($z_t$) included</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Δ Year fixed effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Neighbourhood fixed effects</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Number of observations</td>
<td>129,949</td>
<td>20,055</td>
<td>20,055</td>
<td>9,544</td>
</tr>
<tr>
<td>$R^2$-within</td>
<td>0.900</td>
<td>0.891</td>
<td>0.912</td>
<td>0.917</td>
</tr>
<tr>
<td>Kleibergen-Paap F-statistic</td>
<td>726.7</td>
<td>679.2</td>
<td>766.4</td>
<td>576.3</td>
</tr>
<tr>
<td>Bandwidth $h$</td>
<td>$\infty$</td>
<td>$\infty$</td>
<td>$\infty$</td>
<td>2.00</td>
</tr>
</tbody>
</table>

**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
Figure D1 — Effect of house prices and sales time after the investment.