Neighbourhood Renewal and Housing Externalities

Professor Gavin Wood

RMIT University

Dr Melek Cigdem

RMIT University*

***Corresponding author:**

Melek Cigdem

melek.cigdem@rmit.edu.au

Abstract:

In this paper we implement a robust quasi-experimental methodological design to evaluate Victoria's Neighbourhood Renewal (NR) program. NR's main objective is to tackle disadvantage in neighbourhood areas with concentrations of public housing. This paper draws on housing attributes recorded in the Victoria-General's Transactions and Valuations datasets and applies propensity score techniques combined with a difference-in-difference estimator to measure the impact of seven NR program sites. These quantitative techniques are used to arrive at financial measures of the non-shelter benefits generated by the renewal programs. Our findings suggest that four NR sites generate a housing externality benefit of $394m, and an increase in total stamp duty revenues of $9.2m. Benefit/cost measures indicate a positive multiple with average externality gains of $3.04 for every dollar invested over a nearly 10 year period, 2002 – 2011.

Keywords:

Neighbourhood Renewal, propensity score method

I. *Introduction*

Neighbourhoods that host large concentrations of disadvantaged households with few employment opportunities are associated with widespread health problems, family breakdown, lower educational achievement, drug abuse, and associated crime and social stigma (Klein 2004). In the Australian context these spatial concentrations of poverty related problems have been linked with public housing estates that increasingly house those assessed by state housing authorities to be in acute housing need. In response Australian state governments have intervened by injecting a substantial amount of public funds to regenerate public housing concentrated in disadvantaged neighbourhoods (Hughes 2004). Victoria's Neighbourhood Renewal program (NR), which we evaluate in this paper, is typical; it was introduced by the state government in 2001 as a place-based response focusing on the regeneration of marginalised communities. It aims to tackle disadvantage in neighbourhoods with concentrations of public housing, a feature that it shares in common with the similar US neighbourhood renewal program HOPE VI (see Zielenbach, Voith and Mariano, 2010)[1].

Research studies evaluating Australian renewal programs have generally relied on qualitative research methods (Shield, Graham & Taket 2011; Kelaher, Warr & Tacticos 2010; Klein 2004; Department of Human Services 2005, 2008). There are virtually no quantitative studies using modelling techniques, or modern quasi-experimental methods[2]. Randolph and Judd (2006, p98) put these major gaps down to the 'complete absence of any national policy interest in estate renewal outcomes over much of the last decade'. The result has been a

[1] The Hope VI Federal Program was introduced in 1992 and provided competitive grants to housing authorities for the revitalization of severely distressed public housing developments. Between 1993 and 2010 it issued 254 grants- valued at over $6.1 billion - to 132 public housing authorities (Levy, 2012). Levy (2012) overviews the HOPE VI program

[2] An exception to the qualitative bias in evaluation studies is Groenhart (2007) who reports findings from a cost benefit analyses on urban renewal centred around public housing using a before and after type methodology.

significant lag in the development and application of evaluation methodologies as compared to other developed countries (Randolph & Judd 2006).

In the USA, on the other hand, there has been extensive quantitative research that generates financial measures of the impact of urban revitalisation policies[3]. All but one (Newell 2010) of the studies we have reviewed find that place-based housing investment interventions have positive impacts as detected by land and house price premiums. Two papers, those by Rossi-Hansberg, Sarte and Owens III (2010) and Galster et al. (2004b), use variants of a quasi-experimental approach to estimate impacts from the same neighbourhood renewal program in Richmond, USA. Both conclude that the program is effective. Rossi-Hansberg, Sarte and Owens III (2010) estimate that over a six-year period, one dollar invested in the programme generated between $2 and $6 increments in land value. Both Ellen et al. (2002) and Ellen & Voicu (2006) report positive findings for New York urban revitalisation programs. With one exception there are no studies of this type in other countries; the exception is Ki and Jayantha's (2010) examination of neighbourhood renewal in Kowloon, Hong Kong, which again detects positive impacts. The overwhelmingly US based evidence base could yield findings that are location specific given differences in housing institutions and market features. For example, US housing markets have pronounced ethnic and racial segmentation (McDonald and McMillen, pp191 – 195), 'fault lines' that are not an important feature of Australian housing markets. These differences in context could result in different findings when replicated in Australia.

Our research approach uses difference-in-differences modelling combined with a Propensity Score Matching method to estimate price premium measures of housing externalities attributable to NR interventions. We draw on the Victorian Valuer-General's data base of property transactions to construct house price profiles before and after the introduction of neighbourhood renewal programs. These profiles are compared with those from a control

[3] See Nourse 1963; Schafer 1972; Santiago et al. 2001; Ellen et al. 2002; Galster et al. 2004b ; 2006; Bair & Fitzgerald 2005; Ellen & Voicu 2006; Schwartz et al. 2006; Castells 2010; Ki et al, 2010 ; Newell 2010; Rossi-Hansberg, Sarte and Owens III (2010); and Zielenbach et al. 2010. .

group of properties that have been selected using the propensity score weighting method. There are two key outputs; one is a benefit to cost ratio for each neighbourhood renewal area sampled. There is a second potentially important contribution. State governments add stamp duty to the prices paid by home buyers and investors, and levy land taxes on the land occupied by rental dwellings, while local governments raise property taxes on all unimproved capital values. Tax revenues will therefore increase in the areas positively impacted by neighbourhood renewal. We employ price premium estimates in NR areas to measure any additional stamp duty revenues.

The paper proceeds as follows: in section 2 we provide an overview of urban renewal studies. Section 3 describes the methodological approach and econometric challenges in evaluating neighbourhood renewal programs. Section 4 describes the data sources and sample design, and presents the modelling techniques used to undertake the empirical analysis. Section 5 presents key findings and their implications. A concluding section provides a summary of our main findings.

II. Literature Review

Neighbourhood renewal studies have in the past two decades progressed from simple before and after study designs (Semenza, et. al 2006; Santiago et. al 2001), to the use of quasi-experimental estimators (Schwartz et al, 2006; Castells, 2010; Galster et al. 2006; Rossi-Hansberg et al. 2010). Critics of naïve before and after intervention comparisons argue that they can attribute improvements in outcomes to neighbourhood renewal, when in fact they are due to metropolitan and economy-wide changes that have benefited all areas. The quasi-experimental method addresses this limitation by a study design containing control sites that are adjacent to NR sites, and would therefore be exposed to the same metropolitan and economy wide influences as the NR sites. Before and after intervention outcome indicators are then contrasted in treatment and control areas.

A difference-in-differences hedonic price model specification is employed by Schwartz et al. (2006) to estimate the housing externalities resulting from publicly-subsidised investments in the renovation of New York apartment blocks between the years 1987 and 2000. The authors define the treatment group as properties that are sold within 2000 feet of a renovated apartment block (housing investment site). Castells (2010) employed similar methods to estimate the extent to which Baltimore's three completed HOPE VI redevelopment sites had positive external effects on nearby property values. Using geographically coded property sales and structural characteristics data for Baltimore City between the years 1990 to 2006, Castells examines the extent to which price levels in the area immediately surrounding HOPE VI sites (micro-neighbourhoods) deviate from price levels outside the sites but in the same neighbourhood (macro-neighbourhoods). In creating micro-neighbourhoods, Castells uses a single ring around each HOPE VI project site, with the ring distance varying with the size of the project.

Galster et al. (2006) argue that studies of this kind are flawed because they compare micro- and macro-neighbourhood house price levels before and after the intervention, but do not control for the trend change in prices. In the pre-intervention period prices could have been increasing faster in the treatment-neighbourhoods, and a continuation of this trend would generate a relative increase in their price level even in the absence of HOPE VI interventions.

Galster et al. (2006) tackle this problem by employing an adjusted interrupted time series (AITS) method. It allows for divergent pre-intervention price trends in treatment- and control-neighbourhoods in their measurement of the external benefits generated by Richmond, Virginia's Neighbourhoods in Bloom (NiB) revitalisation program[4.] To measure outcomes of

[4] The NiB program was first introduced in 1998 in Richmond, VA and set out to revitalise selected high-poverty neighbourhoods in Richmond. The aim of the program was to essentially improve existing owner-occupied dwellings, rehabilitate blighted properties and develop new housing that would foster mixed-income homeownership environments. The AITS method was first introduced in Galster, Tatian and Smith (1999).

the NiB program, the authors use home sale prices[5] within revitalisation areas and compare them with home sale prices in control areas defined as census tracts not exposed to NiB programmes, but with 1990 median sale values below $69 000.

The design of control groups in these studies is crude. They assume that properties belonging to the same or similar neighbourhoods, but outside the boundaries of a neighbourhood renewal site, are equally likely to have been exposed to the treatment (neighbourhood renewal). But intra - and inter- neighbourhood heterogeneity is typically present, so one cannot assume that houses outside the NR boundaries but within the same neighbourhood act as a satisfactory control. We address this problem by applying the propensity score method to construct a more suitable control design for the evaluation of Victoria's NR programme.

III. Methodological Approach

Estimating the effect of Victoria's NR scheme on nearby property and land values poses three main econometric challenges. The first of these challenges relates to the non-random selection of areas designated for Neighbourhood Renewal. Areas that were selected for intervention exhibited high concentrations of poverty based on a range of indicators of disadvantage including area-specific unemployment rates, crime levels, and the proportion of persons receiving disability pensions (Department of Human Services, 2009). Given the strict set of selection criteria for NR intervention, it is unlikely that surrounding neighbourhoods will share the same property and neighbourhood characteristics as those within the boundaries of NR sites. Thus, appropriate steps must be taken to ensure that properties within areas designated for NR and those lying outside the NR site boundaries are as similar as possible in all observable characteristics. A second challenge is omitted-variable bias. This problem may occur when important neighbourhood- or property-level variables are excluded from the model and their effects captured by other explanatory

[5] Authors justify the use of sales data on the basis that 'home sales prices are well known to capitalise many changes in the underlying desirability of neighbourhoods, and thus represent a powerful summary measure of neighbourhood trajectory' (p.462).

variables whose effects are over or under-estimated as a result. A third issue is choice of geographic scale when defining the treatment. Research studies typically choose treatment boundaries that define areas over which urban renewal programs impact; but this choice is essentially arbitrary. For instance, in estimating the effect of a housing program on property values, Santiago et al. (2001) defines the treatment group as all properties within 2000 feet of a housing investment site. To detect whether impacts vary, the authors create a series of 'neighbourhoods' centred on the housing sites, each comprising one of several concentric rings: 0–500 feet, 501–1001 feet, and 1001–2000 feet from the site. But practice varies across studies and so no firm guidelines can be gleaned from the literature on this important decision.[6]

We address the first two challenges through our careful selection of a control group and in our application of the propensity score technique. The propensity score approach is used to estimate of the conditional probability that any property or land parcel is 'exposed' to neighbourhood renewal on the basis of their observable pre-treatment covariates. Originally developed by Rosenbaum and Rosen (1983), the propensity score allows the matching of treatment and control groups using a single index, which is convenient when the subjects (in this case dwellings) can vary across many characteristics, making it impracticable to match on each individual characteristic. Importantly, it eliminates any bias that may arise as a result of differences in the baseline covariates of NR-designated and non-designated areas. However, its success in eliminating all ~~this~~ bias hinges on the key assumption that **all variables** that are relevant to treatment assingmenet and outcome are included in the propensity score model estimator. ~~that affect treatment assignment and outcome are accounted for in the model used to estimate the propensity score.~~ While the dataset at

Commented [SW1]: substitute "within the same neighbourhood as the NR site" - see my comment below.

[6] Schwartz et al. (2006) and Zielenbach et al. (2010) also confine the treatment group to properties within a 2000 foot radius. However, in areas where there were nearby public housing sites, Zielenbach et al. (2010) reduce the size of the treatment areas 'so as to minimize the influence of the non-selected sites'. Bair and Fitzgerald (2005) define treatment areas surrounding HOPE VI project sites as those properties that had longitude and latitude coordinates within a radius of 1.5 miles (7920 feet) of the centre point of the HOPE VI site. Ki and Jayantha (2010), on the other hand, confine treatment areas to properties within 750 metres (2460 feet) of the centre of the redevelopment area. Castells (2010) alternates between 1500 feet and 2000 feet rings from each project site, depending on the size of the project. Ellen et al. (2002) defines treatment areas as properties within 500 feet of program sites.

our disposal includes a rich array of property-level information and measures of their distance from key amenities, ~~is~~ it does not provide measures of other key area-level information such as those used by the DHS to select NR-designated areas (i.e. area-level crime, poverty or the mean income of residents). Thus, relying solely on the propensity score to deal with baseline differences between the treatment and control groups is not sufficient in our case. We deal with this data limitation by firstly imposing a geographic criterion on properties to be selected in the control sample. Specifically, we restrict the control sample to those properties that are within 1500ft to 3000ft of the boundaries of an NR site (discussed in more detail in section IV(a)). The motivation for imposing this criterion is based on the assumption that areas immediately adjacent to an NR site will share similar levels of education, unemployment, average income, crime rates and other socio-economic and demographic profiles as well as broader uniform market conditions. In restricting the geographic scale of the control group, we essentially eliminate these inter-neighbourhood differences that could confound comparisons between otherwise similar properties. Next, we estimate a propensity score to account for any property-level differences between the treatment and restricted control group. Using a probit model, we estimate the conditional probability that any property or land parcel within the relevant baseline control sample is 'exposed' to neighbourhood renewal. The estimates are the predicted values (the propensity score) from a probit model with selection into the treatment as the dependent variable. To safeguard against the misspecification of the propensity score model estimator, we also conduct a difference-in-difference regression model; this results in a 'doubly-robust' estimate (Shadish, et al., 2002). In combining the two methods, we are allowing for the effect estimator to be robust to misspecification of one of the two models (Funk, et.al. 2011). In the case where both models are correctly specified, a double-robust estimator also can offer greater precision (Emsley, et.al. 2008).

The propensity score approach has been invoked in a number of recent urban economics papers to form treatment and control samples for the estimation of hedonic house price models. The implementation of propensity score matching varies; in Gibbons and Machin (2005) a kernel-based matching algorithm is deployed; Eichholtz, et.al. (2013), McMillen and McDonald (2002) and Davis (2011) use propensity scores to weight each observation in treatment and control samples (propensity score weighting method); while Alberini (2007) includes propensity scores as a regressor in hedonic model specifications.

In our study we follow Eichholtz, et.al. (2013), McMillen and McDonald (2002) and Davis (2011) and apply inverse probability of treatment weighting (IPTW) using the propensity score. The IPTW method ensures that the mean values of the property and neighbourhood characteristics (used in the probit model) in the control sample are equal to those in the treatment sample. By creating a synthetic sample where the control and treatment samples have equal mean values on the characteristics driving treatment selection, we are essentially mimicking randomized control samples which ensure that the distribution of the covariates are independent of NR assignment (Austin, 2011). The IPTW method therefore strengthens the credibility of the control sample's price profile as a counterfactual. An attractive feature of using IPTW over alternative propensity score matching specifications is that the former utilises all available data while the latter only uses a matched subset as would be used with say a nearest neighbour algorithm. Also, Hahn (1998), Robins, Mark, and Newey (1992) and Heckman, Ichimura, and Todd (1998) demonstrate that while conditioning on the true propensity removes all bias resulting from differences in the covariates between the treatment and control groups, it does not not necessarily lead to an efficient estimator. Hirano, Imbens and Ridder (2003) prove that inverse probability weights, on the other hand, do provide more effciient estimates.

The steps taken to carry out the empirical analysis can be summarised as follows: Firstly, we estimate a probit regression model with the dichotomous treatment variable (indicating whether the property transaction is within the boundaries of an NR area) as the dependent variable, and observable property and neighbourhood characteristics that could affect both selection into the treatment group and the outcome (i.e. property price premiums) of interest

(Heinrich et al. 2010). Second, we use the parameter estimates from the first stage probit regression model to generate the predicted probability of a property being exposed to NR conditional on its property and neighbourhood characteristics, i.e. the propensity score. Third, we assign weights to each of the observations in treatment and control samples, those in the treatment assigned a weight of 1, and those in the control assigned the inverse of the propensity score, i.e. $\frac{p_i}{1-p_i}$,, where p_i denotes the propensity score for property *i* (Hirano el. al., 2003; Nichols, 2008). Fourth, a series of balancing checks are performed to compare the treatment and control samples weighted by the inverse probability weights. Fifth, we estimate a weighted difference-in-differences hedonic regression model which includes all covariates used in the probit model as well as additional property and neighbourhood characteristics relevant to property values. This approach is sometimes referred to in the literature as the 'doubly robust estimator' (Robins et.al. 1995) because it offers consistent estimates of treatment effects, provided the outcome regression model or propensity score model is correctly specified (Emsley et. al., 2008). Results for the balancing diagnostic tests along with empirical estimations are presented in the next section.

IV. Econometric Estimations

(i) Data Sources and Sample Design

The analysis exploits two separate housing datasets that were obtained from the Office of the Victorian Valuer-General (VG): they are (1) the Victoria Property Valuations dataset; and (2) the Victoria Property transactions dataset. Supplied in a confidentialised format[7], the two datasets provide us with detailed property-level information on sales prices as well as neighbourhood and property characteristics that span a period of more than 20 years. The Property Valuations database is the main source for information on property-level housing, locational and neighbourhood characteristics as at 2008, while the Property Transactions

[7] This database was originally developed under AHURI project titled 'Planning reform, land release and the supply of housing', by Goodman, R., Buxton, M., Chhetri, P., Taylor, E. and Wood, G. 2010 to analyse land use planning policies. We are grateful to Elizabeth Taylor who was responsible for the original design and creation of the merged dataset.

database contains sales information on every sold property in metropolitan Melbourne from years 1990 to 2011. We merged the two datasets to create a single dataset that matches every sold property's sales information (such as price) with property characteristics like number of bedrooms, age of building and land and floor area, location in relation to principal and major activity centres (areas designated by planning authorities as focal points for employment growth, transport nodes and urban amenities), and planning regulations such as zoning and overlay areas[8]. Overlay areas apply to parts of a municipality that contain land or building characteristics which require an additional layer of planning control. Examples are heritage overlays which apply to areas containing clusters of historical buildings. Table A1 in the Appendix summarises the key variables contained in the final merged dataset along with their definitions and unit of measurement.

A critical step in the data construction phase was to enhance the housing dataset so that it could identify all properties located in the immediate vicinity of an NR site. To be able to do this, we first identified the street-level location and boundaries of each neighbourhood renewal site in metropolitan Melbourne.[9] Across metropolitan Melbourne 11 projects have been progressively launched since 2001. Among the metropolitan sites four are left out of the sample frame because they contain 100% public housing[10] or could not be analysed because NR was initiated late in the study timeframe, leaving too few post-treatment years for robust estimation of impacts. This leaves a sample frame covering 7 neighbourhood renewal sites, just under 50% of the state government's NR programme. Table 1 presents a list of the sample frame NR sites, the number of property transactions and the year NR commenced in each site. Next, we used GIS tools to delineate the boundary of each neighbourhood renewal site to identify all property transactions lying within the boundaries of

[8] The overlay boundaries are identified using VicMap database 2010 version.

[9] We would like to thank Olwyn Redshaw and Mark O'Driscoll from the Victorian Department of Human Services for their assistance.

[10] The four sites are those in Collingwood, Fitzroy, Atherton Gardens and Flemington. Percentage figures on the extent of public housing stock within NR sites were supplied by the Department of Human Services. We are grateful to Moy Lam and Dianne Hill for their assistance.

neighbourhood renewal sites; these properties form a 'treatment' sample containing transactions in privately owned housing units *directly* exposed to the neighbourhood improvements and upgrades executed in their immediate vicinity. Because there are only transactions in privately owned dwellings and vacant lots, the estimates of price premiums due to NR are based solely on the sales records of privately owned properties. Private properties are not eligible for renovation under NR spending programmes (only public housing is eligible), and so any price premiums will reflect housing externalities associated with NR. In arriving at estimates of aggregate non-shelter benefits we assume private properties not sold over the sample period benefit to the same degree.

To construct control groups, we created separate control samples for each NR site comprising all properties lying between 1500 feet and 3000 feet of the boundaries of individual neighbourhood renewal sites. The 1500ft exclusion zone aims to eliminate (from the control sample) properties outside the boundaries of NR sites that could be 'contaminated' by spillover effects from NR programmes. In omitting these properties however, we run the risk of underestimating the benefits of the NR intervention should the spillover effects extend beyond 1500ft from an NR site. The final dataset therefore comprises two groups of properties: the treatment group, which includes all vacant land and housing transactions located within the boundaries of an NR site; and a control sample, composed of transactions in land and properties located within 1500 to 3000 feet of an NR site. Properties with extreme property values were trimmed at the 1st and 99th percentile along with observations with missing or invalid characteristic values[11]. The final sample data is summarised in Table 1 which presents descriptive statistics on the treatment and control samples. Unsurprisingly, Table 1 shows that mean and median property values were generally lower for properties located within an NR site boundary compared to those located further out yet still within the same neighbourhood. . Treatment and control sample sizes

[11] This resulted in a loss of 17 transaction records for the Hastings site which contains the fewest number of property transactions over the sample period, and a loss of 144 transactions within the Doveton-Eumemmerring site which contains the largest number of property transactions.

vary from a minimum of 1,787 records to a maximum of 8,847. Sample sizes in the treatment and control groups of each NR site are roughly equal.

TABLE 1

Number of transactions within NR sites

NR Site	*Year of NR Program Commencement*	*Summary Statistics*					
		Within NR site boundary (Treatment Group)			*Within 1500-3000 feet of NR site (Control Group)*		
		Mean Price	*Median Price*	*Count*	*Mean Price*	*Median Price*	*Count*
Braybrook (Braybrook and Maidstone)	2002	$207,214	$180,775	2,462	$211,644	$175,000	3,146
Ashburton (Ashburton, Ashwood and Chadstone)	2003	$302,636	$252,375	2,108	$387,102	$318,000	2,841
Broadmeadows		$170,805	$150,000	1,205	$197,522	$167,000	1,688
Werribee (Heathdale)		$146,986	$119,000	3,750	$150,965	$126,000	5,097
Doveton-Eumemmerring		$144,398	$115,000	3,695	$189,732	$150,000	3,527
West Heidelberg	2006	$171,682	$130,300	1,133	$258,639	$215,000	1,998
Hastings		$138,853	$117,500	757	$162,344	$120,000	1,030

(ii) Propensity Score Weighting and Doubly-Robust Estimator

In the first stage of the empirical analysis we run a separate probit regression model for each NR site to obtain a propensity score for every observation in the treatment and control samples. The probit regression model can be expressed as follows:

$$prob(DTreatment = 1|X) = \phi(\beta_0 + \beta_1 X + \varepsilon) \quad (1)$$

where $DTreatment$ is the probability of a property being assigned to an NR designated area given its characteristics; ϕ represents the normal cumulative distribution and X includes all relevant regressors that could affect both selection into the treatment group and the outcome (i.e. property price premiums) of interest (Heinrich, et.al. 2010). This includes property-level characteristics like number of bedrooms, floor area, age of building and land area. Next, we take the predicted values from the probit regression model to estimate the propensity score of each property transaction. The propensity score estimates were used to reweight properties in the control sample by the relative odds $\frac{p_i}{1-p_i}$.If the probit model specification is correct, weighting will ensure that difference in differences will provide an efficient estimate of the effect of NR on the treatment sample (Davis, 2010; Nichols, 2008). To verify the success of the weighting scheme in equalizing the mean characteristics in the treatment and control samples, we carried out a standardized bias test and a pseudo-R2 test and a test of joint significance to compare the treatment sample with the weighted and unweighted (raw) control samples12. A standardised bias[13] test indicates the reduction in bias that is attributable to reweighting. The Pseudo-R2, on the other hand, indicates how well the covariates in the probit model explain the probability of being in an NR area (Sianesi, 2004). Log-likelihood tests can also be used to test the joint significance of the models before and after reweighting the sample; ideally, the log-likelihood ratio test estimates should be significantly different from zero in the treatment and unweighted control sample, and should reduce to insignificance in the treatment and reweighted samples (Caliendo and Kopeiniig, 2008. While no firm guidelines can be gleaned from the literature on the maximum acceptable bias for a sample to be considered as balanced, Caliendo and Kopeinig (2008)

[12] See Austin (2011) for a detailed explanation of balancing diagnostics to test the performance of different propensity score specifications.

[13] The standardized bias is measured by first estimating the standardized residual which is the difference in means between the treatment and unweighted control sample for variable x divided by the standard deviation of the pooled sample. The standardized residual is then recalculated for each variable, only this time the numerator is the difference in means in variable x between the treated and reweighted control group while the denominator remains as the standard deviation for the pooled sample. Differences in the standardized residuals between the weighted and unweighted samples for each variable offer a measure of the percentage reduction in bias due to inverse probability weighting.

suggest that, as general rule of thumb, a bias reduction below 5% is regarded as sufficient (p.48). Another symptom of a good propensity score specification is the removal of any systematic differences in the distribution of the covariaties as a result of reweighting. When this is the case, pseudo-R2 should be lower in the propensity score reweighted sample as compared to the unweighted sample. Summary statistics of the overall covariate balance as indicated by the absolute standardized bias and the pseudo-R2 tests are presented for each NR sites in Table 2[14]. Test results from the standardized bias test suggest strong improvements in the reweighted control sample, despite there being large initial biases in the raw sample. Propensity score reweighting achieves considerable reductions in the absolute standardized bias scores, the means of which lie below between 1.2 and 4.3% across all NR sites in the reweighted sample. Similarly, test scores from the Pseudo R^2 approach zero in all cases after reweighting and the likelihood-ratios are significant across all NR sites on the unweighted samples but become insignificant in the reweighted samples, suggesting little systematic variation in the distribution of the covariates between the treatment and reweighted control samples. Moreover, log-likelihood estimates are consistently significant in the unweighted sample, and consistently insignificant in the reweighted samples.

[14] Covariate statistics for the standardized bias test are available from the authors upon request.

TABLE 2

Summary Statistics for Absolute Standardised Bias Test and Pseudo R^2 Test for the

Raw and Inverse Probability Treatment Weighted Samples

	Maidstone		*Ashburton*		*Broadmeadows*		*Werribee*		*Doveton*		*West Heidelberg*		*Hastings*	
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted
(a) Absolute Standardised Bias														
Mean Bias	29.42	2.31	29.45	1.20	31.57	2.94	10.94	3.06	9.67	2.73	28.77	2.03	35.38	4.29
Median Bias	17.03	1.54	17.24	1.29	26.12	2.99	9.44	3.21	7.35	1.97	29.85	1.68	37.18	4.63
SD of Bias	28.29	1.98	23.20	1.00	26.36	1.65	6.31	2.03	7.55	2.52	12.90	2.00	16.11	2.74
Minimum Bias	1.39	0.40	3.47	0.00	5.93	0.29	5.33	0.67	2.71	0.14	1.20	0.07	12.35	0.09
Maximum Bias	73.23	6.09	68.16	3.61	79.50	5.11	23.57	6.04	22.52	6.13	45.41	5.90	62.28	8.76
Explanatory Variables	8	8	11	11	8	8	8	8	6	6	11	11	10	10
(b) Pseudo R2 Balance Test Statistics														
Pseudo R^2	0.145	0.001	0.308	0.000	0.222	0.003	0.026	0.002	0.017	0.001	0.170	0.003	0.156	0.003
LR Chi^2	1111.43	6.58	2081.11	2.85	868.49	11.34	83.28	5.86	169.81	10.31	696.91	8.68	379.38	7.00

P-value	0.000	0.583	0.000	0.993	0.000	0.183	0.000	0.662	0.000	0.112	0.000	0.652	0.000	0.725

To arrive at an estimate of NR effect, we run a difference-in-differences regression model and apply population weights to reweight the treatment and control groups by the inverse probability weights. The difference-in-difference model is performed separately for each NR site, and regresses the log of house price on neighbourhood characteristics, property-level characteristics, year/quarter time dummy variables, a time trend, postcode dummy variables, and dummy variables representing properties within the boundary of an NR site. Specifically, this hedonic regression takes the following form:

$$Y_{it} = \Sigma\beta_t S_{it} + \Sigma\delta_k C_i + \Sigma\gamma_t Q_{it} + \lambda DTreatment_i + \phi DTreatment_i * DPost_i + \varepsilon_{it} \quad (2)$$

(2)

where Y_{it} is the log of the per unit sales price of property *i* in period *t*; S_{it} is a vector of property-related characteristics and includes number of bedrooms, size of the land parcel and age of the building, C_{ik} is a group of time invariant structural and locational characteristics and Q_{it} represents a set of annual dummy variables with year 1990 representing the base period. Variable $DTreatment_{it}$ is a dummy variable identifying property transactions that are located within the boundaries of a neighbourhood renewal site that was introduced in year *t*. Variable $DPost_{it}$ is a dummy variable that flags transactions that had taken place after the NR scheme was introduced in year *t*. The intercept is α, and the coefficient γ_t measures deviations from trend in any one year. The coefficient on $DTreatment_{it}$, λ, measures the location effect that is not due to the introduction of NR. The parameter of particular interest is ϕ, the effect of the interaction term *DTreatment**Dpost* : ϕ estimates the change in property values due to the NR scheme[15]. Finally, ε_{it} is an error term that is assumed to be normally distributed with an expected value equal to zero, and

[15] Strictly speaking the coefficient Φ is not the simple difference in differences estimator that is obtained from an OLS regression without controls, but it has a similar interpretation (see Wooldridge, 2001). In another variant of this model specification, we allow the difference in differences (or average treatment effect) coefficient (ϕ) to vary across calendar years in the post-treatment era. Estimates from this model are available from the authors upon request.

constant variance[16]. Robust standard errors are reported that account for heteroskedasticity.

V. Empirical Findings

Table 4 lists the coefficient estimates for key variables as obtained from the difference-in-difference model specification in equation 2. The substantive table content is presented in columns 2 and 3; our variable *DTreatment* confirms that pre-intervention house prices within the boundaries of targeted NR projects were on average below those of properties in the control sample in all but one (Maidstone) of the NRs. The variable *DTreatment*Post* suggests that NR is the source of a statistically significant price premium favouring private housing transactions within the boundaries of four NR areas. In three NR projects (Broadmeadows, Hastings and Werribee) there are no statistically significant differences in housing prices as compared to the control sample.

The final two columns report sample numbers and a 'goodness of fit' R^2 statistic that can range between zero and one. Sample numbers are invariably healthy, with only one NR area dipping below 2,000 transactions (Hastings). With annual time dummies and a wide range of controls for property, neighbourhood and amenity characteristics, high R^2 goodness of fit statistics are achieved[17].

Table 5 translates the price premium coefficients into a percentage increase[18] (column 3), and uses the percentage price premium as the basis for computing an aggregate measure of externality (non-shelter) benefits within the boundary of each NR area. It is arrived at by selecting each post-NR intervention private housing transaction and calculating the product

[16] We use sampling weights in the difference-in-differences regression to reweight the control sample using the pweight command in Stata.

[17] A large number of control variables are added to the regression model specification that includes the use of calendar time variables to captures trend increases in house prices and deviations from trend as advocated by Galster et.al 2004 (the adjusted interrupted Time Series Model). Coefficient estimates and annual time dummy estimates are available from the authors upon request.

[18] The percentage impact estimates for binary variables (when the dependent variable is a natural logarithm) are calculated from (e^{β} - 1), where β is the estimated coefficient (see Halvorsen and Palmquist, 1980).

of the transaction price and percentage price premium. This dollar figure is then expressed at 2011 prices by indexing using the CPI and summed over all transactions. It is assumed that price premiums are uniform across the NR private housing stock; the inverse of private housing transactions as a proportion of the total private housing stock is then employed to aggregate across the entire private housing stock. The results are the estimates in column 5, table 5. In the NR areas where price premiums are found to be positive and statistically significant externality benefits sum to $394m at 2011 prices. Unsurprisingly aggregate benefits tend to be larger where the private housing stock is large, and the average price of properties is higher...

TABLE 4

Key findings from DID Specification-Weighted Regression Adjustment Method

		Key Variables			
Neighbourhood Renewal Area	*Cohort*	*DTreatment*	*DTreatment* Post*	*No. of obs.*	*R-squared*
Doveton-Eumemmerring	2003	-.323 (.020)***	.136 (.012)***	7,170	0.8223
Ashburton	2003	-.231 (.022)***	.121 (.024)***	4,949	0.9046
West Heidelberg	2006	-.268 (.037)***	.094 (.031)***	3,092	0.8248
Maidstone	2002	.001 (.090)	.042 (.014)***	5,608	0.8829
Hastings	2006	-.187 (.043)***	.036 (.029)	1,787	0.8540
Broadmeadows	2003	-.020 (.093)	.029 (.019)	2,875	0.9068
Werribee (Heathdale)	2003	-.055 (.025)**	.009 (0.028)	8,809	0.7803

Note: Standard errors in parentheses * denotes coefficient statistically significant at 10%, two-tailed test; ** denotes coefficient statistically significant at 5%, two-tailed test; *** denotes coefficient statistically significant at 1% level, two-tailed test.

TABLE 5

Price premiums and aggregate benefits

Variables	*Cohort*	*Price premium (%)*	*Total private housing stock within NR site(units)*[1]	*Aggregate Benefit (2011 prices) $m*	*Average price post intervention (2011)*	*% of public housing*
Ashburton	2003	13%	2,892	205.4	$430,230	20
Doveton/ Eummemmerring	2003	15%	3,810	104.9	$200,692	13
Maidstone	2002	4%	2,906	47.1	$273,025	21
West Heidelberg	2006	10%	673	36.7	$316,803	49

Note: 1 The total number of housing units within each NR area has been identified using the Vicmap Address data set and Mapinfo (see section 3); from the total we have subtracted the number of public housing units, a percentage figure we obtain from the Department of Human Services. We are grateful to Moy Lam for assistance in this regard.

The price premiums attributable to NR will generate additional stamp duty revenues. This potentially important return to government revenues is estimated in table 6; it is arrived at by again selecting each post-NR intervention private housing transaction and converting the prices at sale dates to 2011 values using the CPI. The 2011 Victorian state government stamp duty schedule is applied to estimate stamp duty liabilities[19]. A hypothetical stamp duty is estimated by subtracting the NR price premium to generate the counterfactual sale price in the absence of NR. The difference between the two stamp duty estimates is our measure of additional stamp duty revenue. These calculations indicate that total stamp duty revenues increase by $9.2m.

TABLE 6

Stamp Duty Gains

Variables	*Cohort*	*No of transactions*	*Stamp duty revenue with*	*Stamp Duty revenue in*	*Increase in Stamp duty*

[19] The identity of buyers is unknown and so concessions to first home buyers and the higher rates applicable to investor purchases cannot be taken into account.

		in the NR housing stock (post NR period)	*NR ($m) (1)*	*absence of NR ($m) (2)*	*revenue due to NR ($m) (1)-(2)*
Doveton/ Eummemmerring	2003	1141	$22.0	$17.1	$4.9
Ashburton	2003	671	$25.9	$22.9	$3.0
Maidstone	2002	861	$23.3	$22.5	$0.8
West Heidelberg	2006	230	$5.8	$5.3	$0.5

Benefit Cost Analysis

The Victorian State Government's Department of Human Services has released expenditure budgets for each NR area[20]. Each NR budget lists total budget spending in each calendar year since its introduction. In the NR areas where we find positive price premiums total programme expenditure varies from a high of $57m in Maidstone to a low of $10 million in West Heidelberg. There is a relatively small programme in Heidelberg that did not start until 2006, and hence some of its expenditure budget has yet to be spent. These figures differ from the historic cost numbers because we have converted outlays in each financial year to 2011 prices[21] and therefore ensure that both externality benefits and expenditures are expressed at the same year's price level. There are two caveats with respect to the cost figures. Firstly, the outlays include all spending on public housing units including items for activities such as routine maintenance that would have been incurred in the absence of NR action plans. Secondly, there are other service delivery agencies (e.g. health, education) that could have invested in NR programmes/areas to generate synergies from the integration of services, an important component of the strategy. Estimates of these cost outlays by other agencies are not available.

Table 7 below summarises the results from a benefit cost appraisal. We first consider those NR programmes with statistically significant price premiums. For these four NR areas we estimate benefit-to-cost ratios that range from a high of $14 for each dollar spent in

[20] We are grateful to Moy Lam and Dianne Hill for their assistance in providing these figures.

[21] For example, in Maidstone/Braybrook the historic cost measure produces a total expenditure of $47.8m, but when converted to 2011 prices this is equivalent to $57.0m.

Ashburton,, to just under $1 per one dollar spent in Maidstone. Ashburton happens to be distinctive because of a relatively light emphasis on capital spending (67% of total), and a relatively heavy focus on employment and community infrastructure services (grants) at 18% of the total expenditure budget. In these four NR areas we estimate that every dollar spent is responsible for the generation of 4.2 dollars of housing externality benefits. But there are three NR areas where housing externality gains are not detected. When we add these sites and their costs into the benefit-cost equation our measure remains a positive multiple with externality gains of $3.04 for every dollar invested in NR areas over a nearly 10 year period 2002 – 2011. Rossi-Hansberg, Sarte and Owens (2010) estimate that neighbourhood revitalisation programmes in Richmond, Virginia generate housing externality gains that range between $2 and $6 per dollar invested in the program over a 6 year period. Our own estimates are also in this range.

TABLE 7

Benefit-Cost Analysis; Results

NR Site	Cohort	Aggregate Benefits (2011 prices) $m	Total Expenditure1 (2011 prices) $m	Benefit/ Cost ratio	Capital Spending as % of all Outlays
Ashburton	2003	205.4	11.9	$14.2	67%
Doveton	2003	104.9	57.0	$8.8	66%
Maidstone	2002	47.1	14.4	$0.8	94%
West Heidelberg	2006	36.7	10.2	$3.6	68%
Sub-Total	*NR*	*394.2*	*93.7*	*$4.2*	*74%*
Werribee	2003	No statistically significant benefits detected	11.8	$0	59%
Broadmeadows	2003	No statistically significant benefits detected	16.3	$0	73%
Hastings	2006	No statistically significant benefits detected	8.0	$0	68%
Grand Total	***NR***	***394.2***	***129.8***	***$3.04***	***71%***

VI. Concluding Comments

This paper designs and implements a quasi-experimental methodology for the evaluation of urban renewal programs. The approach rests on the key insight that if neighbourhood renewal reverses negative externalities such as crime and vandalism, these benefits will generate house price premiums on private property transactions within the boundaries of neighbourhood renewal areas. We estimate these price premiums using a study design that compares private housing market transactions in neighbourhood renewal areas, with a control group of transactions that is formed using propensity score matching techniques. We believe this is the first Australian study to apply these kinds of quasi-experimental techniques in a housing and urban context. It is also the first Australian study to quantify the possible returns to state governments on the revenue side of their budgets.

We find that NR generates statistically significant price premiums within the boundaries of four (out of seven) NR areas. In the NR areas where price premiums are significantly positive, housing externality benefits sum to $394m (at 2011 prices). Unsurprisingly, benefits tend to be bigger where the private housing stock within the boundaries of NR areas is larger (e.g. Ashburton). The Ashburton NR is also distinctive because of a relatively light emphasis on capital spending (67% of total), and a relatively heavy focus on employment and community infrastructure services (grants) at 18% of the total expenditure budget.

In the NR areas with positive price premiums total programme expenditure varies from a high of $57m to a low of $10m (at 2011 prices). Across the four NR areas expenditure outlays to 2010-'11 total $94m (at 2011 prices). We therefore estimate that every dollar spent generates $4.2 of housing externality benefits. But there are three NR areas where housing externality gains are not statistically significant. When we add these sites and their costs into the benefit-cost equation externality gains are lower at $3.04 for every dollar invested over a nearly 10 year period 2002 – 2011. A *part* of the housing externality gains accrue to government as a result of additional revenues from taxes and charges such as stamp duties, land taxes and property rates. We are able to estimate the additional stamp duty revenues; these calculations indicate that total stamp duty revenues increase by $9.2m (at 2011 prices), a modest offset to NR budget outlays. It is common for advocates of neighbourhood renewal to emphasize an equity rationale for place based interventions, and their role in closing the gap between severely disadvantaged communities and other more favorably placed communities. But this overlooks an efficiency rationale. The housing externality gain estimates presented here are efficiency gains. Neighbourhood renewal can if successful help reverse negative housing externalities that cause the misallocation of resources because of under investment in assets and activities adversely affected by negative externalities. Our estimates suggest that neighbourhood renewal can succeed in that task.

Acknowledgement and disclaimer. *The Property Valuations (2008) dataset and the Property Transactions dataset used in this paper were supplied by the Office of the Valuer-General, Victoria (VGV). VGV is a division within the Department of Sustainability and Environment. It is the state's independent authority on property valuations, and oversees government property valuations and council rating valuations. The findings and views reported in this paper are those of the authors and should not be attributed to VGV. The authors are also grateful to the journal referees for very helpful comments on an earlier draft. The usual disclaimer applies.*

REFERENCES

Alberini, A. 2007, 'Determinants and effects on property values of participation in voluntary clean-up programs: the case of Colorado'. *Contemporary Economic Policy* 25, no. 3: 415-432.

Austin PC. An Introduction to Propensity Score Methods for Reducing the Effects of Confounding in Observational Studies. *Multivariate Behavioral Research*. 2011;46(3):399-424. doi:10.1080/00273171.2011.568786.

Bair, E & Fitzgerald, JM 2005, 'Hedonic estimation and policy significance of the impact of HOPE VI on neighborhood property values', *Review of Policy Research*, vol. 22, no. 6, pp.771–786.

Caliendo, M & Kopeinig, S 2008, 'Some practical guidance for the implementation of propensity score matching', Journal of Economic Surveys, vol. 22, no. 1, pp.31–72.

Castells, N 2010, 'HOPE VI neighborhood spillover effects in Baltimore', Cityscape: A *Journal of Policy Development and Research*, vol. 12, no. 1, pp.65–97.

Davis, L.W. 2011. The effect of power plants on local housing values and rents. *Review of Economics and Statistics,* 93(4):1391–1402.

Dehejia, R. and Wahba, S. 1999, 'Causal Effects in Non-Experimental Studies: Re-Evaluating the Evaluation of Training Programs,' *Journal of the American Statistical Association*, Volume 94, No. 448, pp. 1053-1062.

Dehejia, R. and Wahba, S. 2002. Propensity Score-Matching Methods for Nonexperimental Causal Studies. *The Review of Economics and Statistics,* Vol.84, No.1: 151-161.

Department of Human Services. 2002. *Neighbourhood Renewal- Growing Victoria Together*. Office of Housing, Department of Human Services, Victoria.

Department of Human Services, Victoria 2005, *Neighbourhood renewal: Evaluation report 2005*, Neighbourhood Renewal Unit, Department of Human Services, Victoria.

Department of Human Services, Victoria 2008, *Neighbourhood renewal: Evaluation report 2008,* Neighbourhood Renewal Unit, Department of Human Services, Victoria.

Department of Human Services (2009) Creating a Fairer Victoria: Minister for Housing's statement. Melbourne: Department of Human Services. At: http://www.neighbourhoodrenewal.vic.gov.au, last accessed 14 May 2012.

Eichholtz, Piet & Kok, Nils & Quigley, John M., 2013. 'The Economics of Green Building,', *Review of Economics and Statistics,* 95(1): 50-63.

Ellen, IG, Schill, MH, Schwartz, AE & Susin, S 2002, Building homes, reviving neighbourhoods: Spillovers from subsidised construction of owner-occupied housing in New York City, *Journal of Housing Research*, no. 12, pp.185–206.

Ellen, IG & Voicu, I. 2006, 'Nonprofit housing and neighborhood spillovers', *Journal of Policy Analysis and Management*, vol. 25, no. 1, pp.31–52.

Emsley RA, Lunt M, Pickles A, Dunn G. 2008, 'Implementing double-robust estimators of causal effects'. *The Stata Journal*, 8(3), 334-353.

Funk MJ, Westreich D, Wiesen C, Stürmer T, Brookhart MA, Davidian M. 2011, 'Doubly robust estimation of causal effects', *American Journal of Epidemiology*, Vol. 173(7):761-7.

Galster, G., Tatian, P., and Accordino, J. 2006, Targeting investments for neighbourhood revitalization. *Journal of the American Planning Association 724: 457-474*

Galster, G, Temkin, K, Walker, C & Sawyer, N. 2004, Measuring the impacts of community Development initiatives: A new application of the adjusted interrupted time-series model, *Evaluation Review,* vol. 28, no. 6, pp.502–538.

Gibbons, S. and Machin, S. 2005, Valuing rail access using transport innovations, *Journal of Urban Economics*, 57(1): 148-169

Goodman, R, Buxton, M, Chhetri, P, Taylor, E, & Wood, G 2010, Planning and the characteristics of housing supply in Melbourne, AHURI Final Report No. 157. *Australian Housing and Urban Research Institute*, Melbourne.

Groenhart, L. E., 2007, 'Economic Modelling of Public Housing Estate Renewal, European Network of Public Housing Estate Renewal, *ENHR International Conference*, 25-28 June, Rotterdam.

Hahn, J. (1998): "On the Role of the Propensity Score in Efficient Semiparametric Estimation of Average Treatment Effects," *Econometrica*, 66, 315-331.

Heckman, J., H. Ichimura, AND P. Todd (1997): "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program," *Review of Economic Studies*, 64, 605-654.

Heinrich, C., Maffioli, A., & Vázquez, G. 2010. 'A primer for applying propensity-score matching. Impact-Evaluation Guidelines', Technical Notes No. IDB-TN-161. *Office of*

Strategic Planning and Development Effectiveness, Inter-American Development Bank.http://idbdocs.iadb.org/wsdocs/getdocument.aspx?docnum=35320229

Hirano, K., Imbens, G. & Ridder, G. (2003). Efficient estimation of average treatment effects using the estimated propensity score. *Econometrica*. 71, 1161-89.

Hughes, M. 2004, Community economic development and public housing estates, Shelter New South Wales, Sydney.

Kelaher, M. Warr, DJW & Tacticos, T, 2010, 'Evaluating health impacts: Results from the neighbourhood renewal strategy in Victoria, Australia', *Health and Place,* no. 16, pp.861–867.

Ki, CO & Jayantha, WM. 2010, 'The effects of urban redevelopment on neighbourhood housing prices', *International Journal of Urban Sciences*, vol. 14, no. 3, pp 276–294.

Klein, H. 2004. 'Neighbourhood renewal: Revitalising disadvantaged communities in Victoria', *Public Administration Today,* September-November, pp.20–29.

McDonald, J. F. and McMillen, D. P. (2011), *Urban Economics and Real Estate: Theory and Policy*. New Jersey, Wiley, Second Edition.

McMillen, D.P & McDonald, J.F., 2002. 'Land Values In A Newly Zoned City', *Review of Economics and Statistics*, MIT Press, vol. 84(1), pp. 62-72.

Newell, T. A. 2010. 'Development and neighborhood revitalization: The effects of residential investment on property values in Durham, NC', *The Michigan Journal of Business*, Vol. 3, no. 2: 97-120.

Nichols, A. (2008) Erratum and discussion of propensity-score reweighting. *The Stata Journal* 8(4).

Nourse, Hugh. 1963. 'The effect of public housing on property values in St. Louis', *Land Economics* 39 (4): 433-441.Randolph, B. & Judd, B. 2006, 'The Role of Qualitative Analysis in the Evaluation of Community Renewal Programs'. *Urban Policy and Research,* Vol 24, No 1. pp 97-114.

Robins, J., S. Mark, and W. Newey (1992): "Estimating Exposure Effects by Modeling the Expectation of Exposure Conditional on Confounders," *Biometrics*, 48, 479-495.

Robins, J. M., Rotnitzky, A., and Zhao, L. P. (1995). "Analysis of semiparametric regression models for repeated outcomes in the presence of missing data", *Journal of the American Statistical Association* 90, 106– 121.

Rosenbaum, PR & Rubin, DB. 1983, 'The central role of the propensity score in observational studies for causal effects', *Biometrika,* vol. 70, no. 1, pp.41–55.

Rossi-Hansberg, E., Sarte, P-D. and Owens, R. 2010, Housing Externalities *Journal of Political Economy,* Vol. 118, no. 3: 485-535.

Santiago, AM, Galster, G & Tatian, P. 2001, 'Assessing the property value impacts of the dispersed subsidised housing program in Denver', *Journal of Policy Analysis and Management,* vol. 20, no. 1, pp.65–88.

Schafer, Robert. 1972. 'The effect of BMIR housing on property values', *Land Economics* 48 (3): 282-286.

Schwartz, AE, Ellen, IG, Voicu, I & Schill, MH. 2006, 'The external effects of place-based subsidised housing', Regional Science and Urban Economics, no. 26, pp.679–707.

Semenza, J.C., March, T. and Bontempo, B.D. 2007, 'Community-initiated Urban Development: An Ecological Intervention', *Journal of Urban Health*, Vol. 84 (1), pp. 8-20.

Shadish, W. R., Cook, T. D., & Campbell, D. T. 2002, *Experimental and quasi-experimental design for generalized causal inference*. Boston: Houghton-Mifflin.

Shield, M, Graham, M & Taket, A. 2011, 'Neighbourhood renewal: An effective way to address social inclusion', *Journal of Social Inclusion*, vol. 2, no. 2, pp. 4-18.

Sianesi, B. (2004), 'An evaluation of the Swedish system of active labour market programmes in the 1990s', *Review of Economics and Statistics*, 86, 133-155.

Zielenbach, S, Voith, R & Mariano, M. 2010, 'Estimating the local economic impacts of HOPE VI', *Housing Policy Debate*, Vol. 20, no. 3, pp.485–522.

Appendix

TABLE A1

List of Variables contained in the merged housing dataset

Variable name	Definition	Measurement
Sales Price	Continuous variable indicating log of the sales price of land plot or property;	Nominal Dollars
Number of Bedrooms (log)	Continuous variable indicating number of bedrooms contained in each sold property	Log of Number of Bedrooms variable
ICSEA score	Index of Community Socio-Educational Advantage (ICSEA), a continuous variable representing the socioeconomic profile of secondary school's catchment area[22]	Linear value
Age of Building (log)	Continuous variable indicating the age of the building in years	Log of Age of Building variable
Distance to CBD (log)	Continuous variable indicating distance from property *i* to the CBD;	Log of distance to the CBD in km;
Distance to train station (log)	Continuous variable indicating distance from property *i* to the nearest train station;	Log of distance to nearest train station in km;
Distance to activity centre (log)	Continuous variable indicating distance from property *i* to the nearest principal or major activity centre	Log of distance to nearest activity centre in km;
Distance to primary school (log)	Continuous variable indicating distance from property *i* to the nearest state primary school;	Log of distance to nearest primary school in km;
Distance to secondary school (log)	Continuous variable indicating distance from property *i* to the nearest state secondary school;	Log of distance to nearest secondary school in km;
Land size (squared metres) (log)	Continuous variable indicating the area of land plot;	Log of the size of the land plot in square metres;
Rural zone dummy	Dummy variable indicating properties located in area that is zoned for rural development;	Equal to 1 if the property is in an area zoned as residential, zero otherwise; (omitted category);
Residential zone	Dummy variable indicating properties located in area that is zoned for residential	Equal to 1 if the property is in an area

[22] Each property transaction is located in relation to its nearest public secondary school and is assumed to belong to the catchment area of its nearest school. It is then matched with the corresponding ICSEA index value.

dummy	development;	zoned as residential, zero otherwise;
Industrial zone dummy	Dummy variable indicating properties located in area that is zoned for industrial development;	Equal to 1 if the property is in an area zoned as industrial, zero otherwise;
Business zone dummy	Dummy variable indicating properties located in area that is zoned for commercial/business development;	Equal to 1 if the property is in an area zoned as commercial/business, zero otherwise;
Other zone dummy	Dummy variable indicating properties in an area that is zoned for other land uses (e.g. public use zone, comprehensive development zone etc.)	Equal to 1 if the property is in an area zoned as comprehensive development zone, road zone, public park and recreation zone and special use zone, zero otherwise;
Environmental significance overlay dummy	Dummy variable indicating properties with environmental significance;	Equal to 1 if land is in area regarded as environmentally significant, zero otherwise;
Land subject to inundation overlay dummy	Dummy variable indicating property in an area prone to flooding;	Equal to 1 if land is in flood area, zero otherwise;
Heritage overlay dummy	Dummy variable indicating areas regarded as places of natural, historical or cultural significance;	Equal to 1 if land is in heritage area, zero otherwise;