Public Housing in Place: Announcement Effects of New Builds in More and Less Expensive Neighborhoods*

Syed Hasan†, Iqbal A. Syed†, and Anastasia Klimova‡

†*School of Economics and Finance, Massey University, New Zealand*
‡*Economics Discipline Group, University of Technology Sydney, Australia*

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Abstract: This study investigates the differential effect of the announcement of new public housing locations on property prices in relatively more expensive and less expensive neighborhoods, taking advantage of a single unanticipated government announcement of new builds in multiple locations in the Australian Capital Territory. We introduce a new method of controlling for the quality difference of properties to be used alongside the difference-in-differences method. We find the announcement dampens property prices in more expensive host neighborhoods but not in less expensive ones. Our findings will help policymakers make important decisions around equity and efficiency in the provision of public housing.

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1 Introduction

For decades governments in many countries have had a policy of constructing housing using public funds in order to provide shelter for low income households under subsidised arrangements (Nourse, 1963). With rising levels of housing unaffordability and increasing inequality in income levels in many major cities across the world, this supply-based public housing policy is expected to continue to play an important role in providing access to housing for people who are priced out of the private housing market (Glaeser and Gyourko, 2018). In this situation it is important that policy makers have the information they need to assess the social costs and benefits of public housing projects, enabling them to put appropriate and efficient policies in place with the goal of attaining social cohesion and housing equity.

This paper investigates the benefits and costs of public housing to host neighborhoods—neighborhoods where new public housing is to be located. Public housing, once constructed, remains a feature of the host neighborhood (Glaeser and Quigley, 2009). Whether the local and potential residents view this as a positive or negative feature can be seen in changes in their preferences towards living in that neighborhood. A positive view would be reflected in willingness to pay higher prices for houses in the neighborhood, and a negative view would be reflected in willingness to accept a lower sale price when moving out of the neighborhood (see, e.g. Bayer et al., 2007; Ellen et al., 2007; Diamond, 2016). Hence, one way to examine the impact of new public housing is to compare changes in the capitalized value of properties in host neighborhoods with changes in adjacent neighborhoods before and after an announcement that new public housing will be built.

More specifically, this paper examines whether the impact of the *announcement* of new public housing on property prices differs significantly between host neighborhoods that are more expensive than surrounding neighborhoods and host neighborhoods that are less expensive than surrounding neighborhoods. The paper argues that a differential impact of non-trivial magnitude should be taken into consideration when choosing locations for the equitable and efficient provision of new public housing. This is in line with Diamond and McQuade (2019), a seminal paper which examines the differential impact of LIHTC on property prices across neighborhoods with differing incomes.

The announcement examined in our paper occurred on 15 March 2017, outlining the construction of new public housing in five locations in Canberra, the capital city of Australia, located in the Australian Capital Territory (ACT). It was reported in the *Canberra Times* on the same day as follows (Lawson, 2017):

> "The ACT government has unveiled sites for 141 new public housing townhouses and apartments in five suburbs in Canberras south.
>
> The sites, most of which will have about 30 homes, are on land zoned for community facilities, a zone the government says allows supportive housing.
>
> The sites are in Monash, Holder, Chapman, Mawson and Wright. Most of the developments are groups of single-storey, two-bedroom townhouses, except at Stapylton Street, Holder, where the plan includes apartments."

This was not well received by many local residents, who were surprised by the announcement. On 5 May 2017, an article in the *Canberra Times* reported fierce criticism with regard to the location of the new public housing from the locals in some of the host neighborhoods (Burgess, 2017a). Some host neighborhood residents marched 15 kilometres to the ACT Legislative Assembly on 6 May 2017 to deliver a petition protesting these unexpected proposed public housing developments. While the residents complained that local amenities and public transport were not sufficient to support the new public housing tenants, the Chief Minister of the ACT government dismissed the residents concerns as thinly veiled NIMBY-ism (see, Burgess, 2017b,c). By the end of 2018 (where our data ends), the projects were still on foot although construction work had not yet begun.

It should be noted that the timing of the announcement and the ACT government's choice of locations for the public housing would have been non-random. The premise of our analysis is that substantial uncertainty exists with regard to the exact timing of the announcement and the exact locations where the public housing will be constructed. Similar identifying assumptions have been made in studies examining the impact of public housing on property prices; e.g. Schwartz et al. (2006) and Diamond and McQuade (2019). Diamond and Mcquade, when examining the impact of LIHTC construction on surrounding property prices, argue that there are many idiosyncratic factors that determine when exactly the public housing will be constructed and what the exact location of this public housing would be within a broader region. In the context of the ACT government's announcement, these idiosyncratic factors include: what policy mandate the political party in government has; the availability of funding; the macroeconomic condition of the country; the demography of the city, including how many people meet the eligibility criteria for public housing; and the state of housing and rental markets. All these factors make it difficult for a resident of a particular neighborhood to anticipate with any degree of certainty whether and when a public housing project will be constructed in their neighborhood. Hence, the public housing announcement examined in this paper was a shock to the property owners, and this allows us to examine the impact of the announcement on neighboring property prices within the framework of quasi-experimental analysis.

Furthermore, of the five neighborhoods where the public housing will be located, three are relatively more expensive and two are relatively less expensive than all other neighborhoods in the ACT. A neighbourhood is classed as more (less) expensive if its average property price is higher (lower) than the remaining neighborhoods in the ACT. Irrespective of whether the ACT government's decision to spread the locations across a mix of more and less expensive neighborhoods is deliberate or not, it creates an ideal situation for the purpose of our study, that is, to examine the differential impact of a public housing announcement on property prices, depending on whether the public housing is located in a relatively more or less expensive neighborhood. We set up difference-in-differences (DiD) models where in one such model we have the treatment group (host neighborhoods) more expensive than the control group (remaining ACT neighborhoods), and in another model we have the treatment group less expensive than the control group. Hence, we leverage the relative price levels across the selected sample of neighborhoods and examine whether there is any significant difference in the impact of public housing on the property prices of the more and less expensive host neighborhoods. The results do show that there is a significant difference between the impact of the public housing announcement on the property prices of host neighborhoods that are relatively more expensive and the property prices of host neighborhoods that are relatively less expensive.

Previous studies examining the impact of public housing that used a DiD method include Ellen et al. (2001), Schwartz et al. (2006), Goujard (2011) and Diamond and McQuade (2019). As can be seen in these studies, the challenges in identifying this announcement impact are: (1) separating this impact on property prices from the impacts of myriad other factors, such as location, general inflationary effects, and the structural attributes of properties, and obtaining an accurate set of counterfactual prices for surrounding properties as if there had been no public housing announcement; and (2) averting potential endogeneity bias in the regression estimate measuring this impact, such bias may occur if the timing of the announcement and the location of the public housing had been anticipated by the residents of the host neighbourhoods or if the local governments decisions were correlated with any unobserved factors in property prices. The paper addresses these challenges by: leveraging the attributes of the chosen locations that are conducive to the purpose of the study, using an actual transaction-based property price data set containing important price-determining characteristics of properties, and devising an appropriate new methodology for estimating the impact.

Within this broad empirical strategy built on DiD, we estimate the public housing impact using three different approaches, each setting out a different way of controlling for quality differences between properties sold in the pre- and post-announcement periods and between treatment and control neighbourhood properties. Our preferred approach employs a two-stage procedure. In the first stage, the quality adjustment stage, we create a balanced panel of properties for pre- and post-announcement periods using hedonic imputations of property prices. We achieve this by imputing a set of counterfactual prices for the pre-announcement properties had they been sold in the post-announcement period and, in a similar way, by imputing another set of counterfactual prices for the post-announcement properties had they been sold in the pre-announcement period. In the second stage, we estimate the difference-in-differences (DiD) model, where we use these imputed prices as the dependant variable in the regression model. This implies that the DiD model compares the prices of the same set of properties between the pre- and post-announcement periods, allowing the estimated DiD model to identify the impact of public housing without the result being tainted by quality change effects. To the best of our knowledge, this has not been tried before in the context of a quasi-experimental framework of analysis.[1]

There are only a few studies that have investigated the *differential* impact of public housing on surrounding property prices depending on the existing situation of the host neighborhood. Notable among these studies are Baum-Snow and Marion (2009), Goujard (2011) and Diamond and McQuade (2019). While our study belongs beside these few studies, it distinguishes itself from these studies in a number of ways and therefore complements the findings in the literature. First, our paper examines the impact of the *announcement* that public housing is to be constructed rather than examining the impact of *actual constructed* public housing over a long period of time. The duration of the post-announcement period in our paper is only one year, capturing the price effect of immediate reactions to public housing being nearby, such as panic sales. This is important because, for example, any abrupt fall in prices due to new public housing construction

[1] This hedonic adjustment method, however, has been researched extensively in the context of constructing price indexes for properties (Eurostat, 2013; Hill and Syed, 2016), and is often regarded as the most flexible way of disentangling quality changes from price changes in goods (Hill, 2013; Eurostat, 2013).

may lead to large consumption disruption and mortgage default, given that properties are in general highly leveraged assets. Baum-Snow and Marion (2009), Goujard (2011) and Diamond and McQuade (2019) all examine the impact on neighborhoods across decades.[2] Second, we categorize neighborhoods according to their pre-announcement property prices, whereas Goujard (2011) and Diamond and McQuade (2019) categorize neighborhoods according to the income of the inhabitants, and Baum-Snow and Marion (2009) categorizes them according to whether they are gentrifying or not. Although these categorizations may be correlated, they each investigate a different aspect of the differential impact of public housing on neighborhoods.

Third, the paper undertakes quality adjustment that is flexible, letting the implicit values of hedonic characteristics vary across pre- and post-announcement periods and control and treatment group properties, and creating counterfactual sets of prices for both pre- and post-announcement property sales; this kind of technique has not been used before in the context of quasi-framework analysis and, hence, contributes by introducing the use of a new methodology in this context. Fourth, the set-up of our empirical analysis—concurrent public housing, of similar size and structure, announced in five locations by the same state government in a relatively confined region, using actual transaction price data, being able to rank the locations in terms of most to least expensive neighborhoods, and using a flexible DiD hedonic method—puts this research in a distinctive place in the literature and contributes to a pure identification of the heterogeneity of the impact of public housing on nearby property prices. Fifth, most studies on the impact of public housing on property prices have been carried out in either US or European markets; very little has been undertaken in other regions, including Australia, hence our paper introduces the impact of public housing on property prices in an altogether different region.

Our results show that there is significant heterogeneity in the impact of public housing on surrounding property prices, depending on whether the public housing is located in a more expensive or a less expensive neighborhood compared to the remaining ACT neighborhoods (where the public housing will not be constructed). This finding of a differential effect of public housing on property prices between more expensive and less expensive neighborhoods is robust to

[2]The impact of public housing on property prices may be observed over many yearsin the immediate post-announcement period, after the commencement of construction, and after completion, with exact timing of the impact difficult to disentangle (Schwartz et al., 2006). A large part of this impact happens in the post-announcement stage because of existing perceptions of public housing (informed by personal experience, hearsay, media, political spin) and the immediate capitalization of expected changes in property prices (Poterba, 1984). The local or potential residents may be risk averse, leading them to buy or sell immediately rather than wait and see, given the fear that waiting may lead to higher prices for buyers and lower prices for sellers (Glaeser and Quigley, 2009).

our different model specifications. This finding is in concordance with Diamond and McQuade (2019), who report that public housing built in relatively higher income neighborhoods has a dampening effect on surrounding property prices, while public housing built in relatively lower income neighborhoods has an accelerating effect on surrounding property prices.

Our results show that the announcement that new public housing is to be located in a relatively more expensive neighborhood has a large impact on the nearby property prices—a fall of 7.0% in the year after the announcement. The impact is none to negligible on nearby property prices when it is announced that the public housing is to be located in a relatively less expensive neighborhood. The large fall in property prices in the more expensive neighborhoods will result in significant losses for property owners, leading to large consumption disruption and increasing the likelihood of defaulting on mortgage loans (Mian et al., 2013), and a significant reduction in property taxes collected by local/state (in this case ACT) government, potentially resulting in a significant efficiency loss to the local economy. This efficiency loss could be avoided by locating the public housing in relatively less expensive neighborhoods, but that would not satisfy equity considerations as less expensive neighbourhoods can also be expected to have fewer local amenities. Hence there is a trade-off between efficiency and equity—social planners who choose to maximize efficiency for a given level of equity will therefore locate public housing in relatively less expensive neighborhoods, and those who choose to maximize equity for a given level of efficiency will select relatively more expensive neighborhoods. Given that the objective of informed social planners is to maximize social welfare at the local level, our analysis, which looks at the differential impact of public housing location, will help these policymakers achieve their desired balance between equity and efficiency.

2 Literature on the effects of public housing location

An important aspect of public housing policy is that it is place based (Glaeser and Quigley, 2009; Koster and van Ommeren, 2019). Public housing projects are fixed to locations, implying that the benefit or detrimental effect of a public housing project largely depends on the amenities the host neighborhood offers to the residents of the public housing and the external effects that the public housing and its residents generate for the host neighborhood (Diamond and McQuade, 2019). The residents of the new public housing benefit from the positive amenities of the host location, which may include proximity to schools, playgrounds and train stations (Gibbons et al., 2014; Albouy et al., 2020; Breunig et al., 2019), and are disadvantaged by the negative features of the same host location, which may include prevailing pollution, traffic congestion and criminal activities (Kling et al., 2005; Ihlanfeldt and Mayock, 2010; Klimova and Lee, 2014).

Conversely, public housing and its residents may generate positive or negative external effects in the host location. Positive external effects could include the new building improving the facade of a location suffering from unsightly ageing buildings (Schwartz et al., 2006), or the income level of the local residents rising as a result of increased economic activity in the neighborhood (Diamond and McQuade, 2019). Negative external effects may include overcrowding of the host neighborhood and increases in crime rates (Gibbons, 2004; Aliprantis and Hartley, 2015) and a crowding out effect on private rental property development in the host neighborhood (Sinai and Waldfogel, 2005; Eriksen and Rosenthal, 2010). Hence, the extent to which new public housing generates social benefits depends not only on the obvious benefit of putting a roof over peoples heads, but also on how much benefit new public housing residents receive from the local neighborhood and how much benefit the new housing and new residents generate for the neighborhood.[3]

The external effects that public housing generates for the host location are found to be conditional on the particular features of the location. For example, public housing in a location with high crime levels may draw the new residents into criminal activities (Glaeser et al., 1996), while public housing in low density regions may reduce criminal activity by increasing street traffic (Schwartz et al., 2006). Baum-Snow and Marion (2009) report that new LIHTC units significantly crowd out nearby new rental construction in gentrifying neighborhoods, but do not have such an effect in stable and declining neighborhoods. The same type of public housing may generate different external effects depending on the location—benefits in one location and negative effects in another. For this reason it is important that policy makers consider the specifics of each location carefully when planning new public housing projects, and our paper provides important analysis to this effect.

There has been a modest amount of research examining the impact of public housing on different aspects of particular neighborhoods, including the impact on the prices of properties in

[3]In related literature, a number of studies report that property prices are impacted positively by the presence of amenities such as transport infrastructure, schools, parks, playgrounds, trees and vegetation (Efthymiou and Antoniou, 2013; Haisken-DeNew et al., 2018; Breunig et al., 2019; Pandit et al., 2013; Panduro and Veie, 2013; Polyakov et al., 2015) and negatively by disamenities such as crime and airport noise (Gibbons, 2004; Ihlanfeldt and Mayock, 2010; Klimova and Lee, 2014; Pennington et al., 1990; Affuso et al., 2019).

these neighborhoods. For a detailed review of these papers, see de Souza Briggs et al. (1999); Nguyen (2005); Schwartz et al. (2006). The findings with respect to whether these impacts are positive, negative or none on surrounding property prices are mixed. Nguyen (2005), reviewing 17 studies, reports that the impact of affordable housing on nearby property prices is negligible if this housing is located in healthy and vibrant neighborhoods and is negative when this housing is located in disadvantaged and declining neighborhoods. Schwartz et al. (2006), on the other hand, find significant and sustained external benefits arising from public housing in New York City, and that these benefits increase with the size of the project. The studies that report that new public housing has a positive impact on neighboring property prices include Nourse (1963); Rabiega et al. (1984); the studies that report new public housing has a negative effect include Goetz et al. (1996) and Lee et al. (1999); and studies finding negligible or no effect include de Souza Briggs et al. (1999) and Ellen et al. (2007). There are a variety of factors that might have impacted the findings in these studies. These include compatibility between the public housing and the host neighborhood (Nguyen, 2005), the methodology and data used in the studies (Schwartz et al., 2006), and the type of public housing, i.e. whether the public housing is high concentration, including high-rise buildings or not (Cummings and Landis, 1993; Aliprantis and Hartley, 2015). It is difficult to compare and contrast the findings across such diverse studies—which use a range of data, methodologies, empirical settings, types of public housing, and features of the host locations—in order to come to a definitive conclusion on the direction of the impact of public housing on nearby property prices.[4]

In order to obtain a like with like comparison of the impact of public housing on nearby property prices across different types of host locations, these different locations should all be examined in the same study. This will allow us to examine the *differential* impact in a controlled setting—using the same theory and empirical strategy, same data set, same definitions and empirical setting (such as, what constitutes neighboring properties, and the empirical hedonic models used). Only a few studies have done this, notably Diamond and McQuade (2019), who undertook a comprehensive study examining the impact of LIHTC public housing on the property prices

[4] A number of studies examine the effect of knowledge, rumors or the announcement of different events (not, however, of the construction of public housing). For example: Bauer et al. (2017) examine the effect of the 2011 Fukushima Daiichi nuclear power plant disaster on the prices of properties near nuclear power plants in Germany; Haisken-DeNew et al. (2018) examine the effect on property prices in Melbourne (Australia) of the release of information on the quality of schools; Hyun and Milcheva (2019) examine the impact of the announcement of urban development on property prices in Seoul (South Korea); and Kiel and McClain (1995) examine the impact of a rumor of undesirable land use on property prices in Massachusetts (USA).

of the surrounding neighborhoods in 129 counties in the United States from 1987 to 2008. They report that the construction of public housing in low income neighborhoods results in the property prices of the surrounding neighborhoods rising, and that the construction of public housing in high income neighborhoods results in the property prices of the surrounding neighborhoods falling. Baum-Snow and Marion (2009) show that the positive amenity effect of new LIHTC in declining and stable neighborhoods results in higher property prices and that there is little or no such effect in gentrifying neighborhoods where they find no effect on property prices. Goujard (2011) finds that newly constructed public housing in low income neighborhoods has a positive impact on surrounding property prices and finds there is no discernible impact on surrounding property prices with the renovation and refurbishment of old public housing in high income neighborhoods. While our paper belongs beside these three papers in that it examines the differential impact of public housing builds at different locations, we distinguish ourselves in a number of ways that will be important for policy decision making around the choice of locations for public housing.

3 Methodology

In order to estimate the impact of the unanticipated announcement of new public housing on property prices, we use the difference-in-differences (DiD) method in conjunction with hedonic quality adjustment. In our analysis, the treatment group is comprised of properties that are located in the neighborhoods of the Australian Capital Territory (ACT) where the public housing will be constructed, and the control group is comprised of properties that are located in the neighborhoods of the ACT where public housing will not be constructed. Our DiD approach compares the price changes of the treatment group properties with the control group properties before and after the public housing announcement. To begin with, let us consider the following DiD model:

$$Y_{ist} = \delta_0 + \delta_1 Treatment_s + \delta_2 Post_t + \delta_3 Treatment_s \times Post_t + \varepsilon_{ist}, \tag{1}$$

where Y_{ist} is the logarithm of the price of property i located in neighborhood s for $s = 1, \ldots, S$, at time t for $t = 0, 1$. $Post_t$ is an indicator variable that takes the value of 1 for periods after the public housing announcement and 0 otherwise. $Treatment_s$ is an indicator variable that takes the value of 1 if the property is located in a treatment neighborhood and 0 otherwise.

In the pre-announcement period, there will be a difference between the prices of the control and treatment properties for two reasons. First, the treatment group and the control group properties are located in different neighborhoods and, since location captures important price determining features of properties, this difference in location will result in a price difference between the two groups. This price difference is measured by δ_1 in equation 1. Second, properties are highly heterogeneous assets, i.e. each property is somewhat different from other properties in terms of physical attributes (e.g. floor area, no. of bedrooms), and in terms of locational factors within its own neighborhood (e.g. distance to local schools, parks and train stations). This quality difference will also contribute to the price difference between treatment group and control group properties, however, this price difference is not measured in equation 1. Let us denote the price difference by θ_1. Combining these two we obtain our *first difference*, $\psi_1 = \delta_1 + \theta_1$.

Now, turning to the post-announcement period, the impact of the public housing announcement on the prices of the treatment group properties will result in a change in the price difference (from the pre-announcement difference) between the treatment and control group properties. Let us suppose that this impact of public housing on the prices of treatment properties is μ. Another factor that will contribute to this change is that the quality of transacted properties, in terms of physical attributes and within-neighborhood locational factors, can also be expected to change between the two periods. Let us denote the price difference due to this quality difference by θ_2. The combination of these two changes results in a new total price difference between control and treatment group properties, and this is our *second difference*, denoted by $\psi_2 = \mu + \theta_2 + \delta_1$. If we now take the difference between the second difference and the first difference, we obtain: $\delta_3 = \mu + (\theta_2 - \theta_1)$. It can be shown that an estimate of δ_3 can be obtained from the coefficient of the $Treatment \times Post$ interaction term in the DiD equation 1.

However, our interest is not in δ_3 in equation 1 because its estimate mixes up the measures of the announcement effect (μ) and the quality change effect ($\theta_2 - \theta_1$). Our interest is in the announcement effect alone. Identifying this announcement effect on the treatment properties depends on the crucial assumption that the quality difference between the treatment and control group properties would have remained unchanged over the sample period in the absence of the public housing announcement. This assumption would allow the DiD method to cancel out the control and treatment property quality difference between the pre- and post-announcement periods. However, this assumption is unlikely to hold true with respect to the equation 1 when applied to property prices. This is because in each period (e.g. year) only a fraction of the stock of properties is transacted, resulting in large fluctuations in the quality of sold properties across periods (Hill, 2013; Eurostat, 2013).[5]

In order to control for quality difference due to physical attributes and within-neighborhood locational factors, we follow three approaches based on hedonic regression methodology. In the first approach, we extend the DiD equation 1 in the following way:

$$Y_{ist} = \delta_0 + \delta_1 Treatment_s + \delta_2 Post_t + \delta_3 Treatment_s \times Post_t + \beta Z_{ist} + \epsilon_{ist}, \qquad (2)$$

where, Z refers to the vector of physical and locational characteristics of properties. Hence, δ_3 provides a measure of the change in the property prices due to the announcement effect after controlling for the changes in Z. While this approach is simple and easy to implement, a problem is that the approach restricts the estimated coefficients to remaining fixed for the whole sample, so it does not allow the implicit values of the physical attributes to vary between control and treatment groups and between pre- and post-announcement samples of properties. In other words, this omits the potential interactions between Z and neighborhoods and the interactions between Z and periods, leading to a mis-specification of the DiD model in equation 2. These omitted interactive variables may be correlated with the DiD variables (such as, the *Treatment* $\times$ *Post* interaction term), resulting in biased estimates of the DiD coefficients.

Our second approach involves two regression estimations. In the first stage, we estimate hedonic regressions separately for control group and treatment group property prices. The characteristics included in the hedonic regressions are the same as in Z, i.e. the characteristics that are included in the first approach (equation 2). We calculate the estimated residuals—the difference between the actual and predicted prices—from each of these two hedonic regressions. Using these estimated residuals as our dependant variable, we estimate the DiD equation as specified in equation 1. Since these estimated residuals reflect the property prices that are free of any differences in the quality effect on properties, δ_3 measures the announcement effect on property

[5]The quality changes of the properties, for both treatment and control groups, may remain unobserved. If the unobserved effects on property prices remain unchanged between the pre- and post-announcement periods, the difference-in-differences method will allow for the cancellation of the control and treatment property quality difference between the pre- and post-announcement periods. For a discussion on how DiD approaches address problems arising out of unobserved heterogeneity, see Parmeter and Pope (2013).

prices controlled for quality changes. A shortcoming of this approach, however, is that the public announcement will impact the implicit values of some of the attributes of properties (e.g. lot size). These effects, which should constitute part of the announcement effect, are taken out when calculating the estimated residuals. Therefore, this approach based on estimated residuals is likely to understate the impact of public housing on property prices.[6]

Our third approach addresses the problems outlined in the first two approaches. This approach involves imputing a post-announcement period price for each property transacted in the pre-announcement period and, similarly, imputing a pre-announcement period price for each property transacted in the post-announcement period. In this way, all the properties that were sold in the pre-announcement period will have corresponding prices in the post-announcement period, and vice versa. This process constructs panel data of property transactions comprised of pre- and post-announcement periods, allowing us to compare prices of the same set of control group properties pre- and post-announcement and to compare prices of the same set of treatment group properties pre- and post-announcement. Therefore, by construction, these comparisons provide us with a measure of the announcement effect free of any quality changes of properties. Unlike the first approach, this approach is flexible as it allows the implicit values of the characteristics of properties to vary over time and differ between treatment and control group properties (Eurostat, 2013; Hill, 2013).

The third approach is implemented in two stages—the quality adjustment stage and the identification stage. In the quality adjustment stage, the first step involves running a separate hedonic regression for each of the following four samples of properties: pre-announcement control group properties, post-announcement control group properties, pre-announcement treatment group properties and post-announcement treatment group properties. In each of these hedonic regressions, the log of property prices is run on the location (using suburb dummies) and physical characteristics of the properties. Let us first introduce some notations for the purpose of illustrating the approach. Let $g \in (\zeta, \chi)$ denote whether a property is in the control group or the treatment group, where ζ refers to the control group and χ refers to the treatment group. Let $t \in (0, 1)$ denote whether a property was sold in the pre-announcement period or the post-announcement period, where 0 refers to the pre-announcement period and 1 refers to the post-announcement period. Let Z_{hgt} denote the vector of hedonic characteristics of property h, where property h

[6] A similar approach has been used recently in studies on property prices in other contexts (see, e.g. Breunig et al., 2019; Banzhaf and Mangum, 2019).

belongs to group g and was sold in period t. For example, $Z_{h\zeta\chi}$ refers to the characteristics of property h that are included in the hedonic regression using the sample of pre-announcement control group properties. $\hat{\beta}_{gt}$ denotes the estimated coefficients of the hedonic regression for group g and period t.

Once we have estimated the four hedonic regressions separately for $(\zeta, 0)$, $(\zeta, 1)$, $(\chi, 0)$ and $(\chi, 1)$, we move to the second step of quality adjustments where the prices of properties sold in one period are imputed from the hedonic model of another period. An imputed log price of a control group property h sold in period 0 can be obtained for period 1 by substituting the characteristics of property h into the hedonic model for period 1 as follows:

$$ln\, \widehat{p_{h\zeta 1}}(Z_{h\zeta 0}) = Z_{h\zeta 0}\, \widehat{\beta}_{\zeta 1}, \qquad h = 1, \ldots, H \tag{3}$$

We can also impute a price for control group property h sold in period 0 for the same period 0 using the following:

$$ln\, \widehat{p_{h\zeta 0}}(Z_{h\zeta 0}) = Z_{h\zeta 0}\, \widehat{\beta}_{\zeta 0}, \qquad h = 1, \ldots, H \tag{4}$$

An imputed price of treatment group property i sold in period 0 can be obtained for period 1 as follows:

$$ln\, \widehat{p_{i\chi 1}}(Z_{i\chi 0}) = Z_{i\chi 0}\, \widehat{\beta}_{\chi 1}, \qquad i = 1, \ldots, I \tag{5}$$

Similarly, we impute a price for treatment group property i sold in period 0 for the same period 0 using the following:

$$ln\, \widehat{p_{i\chi 0}}(Z_{i\chi 0}) = Z_{i\chi 0}\, \widehat{\beta}_{\chi 0}, \qquad i = 1, \ldots, I \tag{6}$$

Once the above imputations are conducted, we move to the second stage, the identification stage, of the method, where we compile these imputed prices in order to calculate the impact of the public housing announcement on property prices. We first calculate the difference between the pre-announcement imputed log prices of the treatment and control group properties. The log of this difference is obtained by taking the difference between the average of the imputed log prices obtained in equation 6 and the average of the imputed log prices obtained in equation 4 as follows:

$$
\begin{aligned}
\Delta ln\, P^{0}_{\chi,\zeta} &= \frac{1}{I}\sum_{i=1}^{I} ln\, \widehat{p_{i\chi 0}}(Z_{i\chi 0}) - \frac{1}{H}\sum_{h=1}^{H} ln\, \widehat{p_{h\zeta 0}}(Z_{h\zeta 0}) \\
&= ln \prod_{i=1}^{I} [\widehat{p_{i\chi 0}}(Z_{i\chi 0})]^{\frac{1}{I}} - ln \prod_{h=1}^{H} [\widehat{p_{h\zeta 0}}(Z_{h\zeta 0})]^{\frac{1}{H}} \qquad (7)
\end{aligned}
$$

We then calculate the difference between the post-announcement imputed log prices of the treatment and control group properties. The log of this difference is obtained by taking the difference between the average of the imputed log prices obtained in equation 5 and the average of the imputed log prices obtained in equation 3 as follows:

$$
\begin{aligned}
\Delta ln\, P^{1}_{\chi,\zeta} &= \frac{1}{I}\sum_{i=1}^{I} ln\, \widehat{p_{i\chi 1}}(Z_{i\chi 0}) - \frac{1}{H}\sum_{h=1}^{H} ln\, \widehat{p_{h\zeta 1}}(Z_{h\zeta 0}) \\
&= ln \prod_{i=1}^{I} [\widehat{p_{i\chi 1}}(Z_{i\chi 0})]^{\frac{1}{I}} - ln \prod_{h=1}^{H} [\widehat{p_{h\zeta 1}}(Z_{h\zeta 0})]^{\frac{1}{H}} \qquad (8)
\end{aligned}
$$

It is important to note that the first difference (equation 7) compares the pre-announcement imputed prices of the treatment and control group properties that were sold in the *pre-announcement* period, and the second difference (equation 8) compares the post-announcement imputed prices of the treatment and control group properties that were sold in the same *pre-announcement* period. In other words, both the pre-announcement and the post-announcement price differences are based on the imputed prices of the properties sold in the pre-announcement period. If we now take the difference between the price differences post-announcement (second difference) and pre-announcement (first difference), we obtain the following:

$$
\left[\Delta ln\, P^{1}_{\chi,\zeta} - \Delta ln\, P^{0}_{\chi,\zeta}\right]_{pre-sample} = ln \prod_{i=1}^{I} \left[\frac{\widehat{p_{i\chi 1}}(Z_{i\chi 0})}{\widehat{p_{i\chi 0}}(Z_{i\chi 0})}\right]^{\frac{1}{I}} - ln \prod_{h=1}^{H} \left[\frac{\widehat{p_{h\zeta 1}}(Z_{h\zeta 0})}{\widehat{p_{h\zeta 0}}(Z_{h\zeta 0})}\right]^{\frac{1}{H}} \qquad (9)
$$

Since we are comparing the same pre-announcement sample of properties across the two periods, the quality difference that existed between control and treatment groups pre-announcement remains the same post-announcement. Hence, equation 9 provides a measure of the price changes between pre- and post-announcement periods controlled for changes in the quality of the sold properties between these two periods. We now substitute equations 3–6 in equation 9 and obtain the following:

$$\left[\Delta ln\, P^{1}_{\chi,\zeta} - \Delta ln\, P^{0}_{\chi,\zeta}\right]_{pre-sample} = ln\,\left[\prod_{i=1}^{I} exp\left(Z_{i\chi 0}(\widehat{\beta}_{\chi 1} - \widehat{\beta}_{\chi 0})\right) \Big/ \prod_{h=1}^{H} exp\left(Z_{h\zeta 0}(\widehat{\beta}_{\zeta 1} - \widehat{\beta}_{\zeta 0})\right)\right] \quad (10)$$

This equation, subject to the fulfilment of the common trend assumption, provides a quality-adjusted measure of the impact of the public housing announcement on property prices and shows that the public housing announcement effect is reflected in the implicit prices of the hedonic characteristics of properties (i.e. the estimated hedonic coefficients). The impact is measured as the weighted average of the changes in the implicit prices of the hedonic characteristics of the treatment properties between the pre- and post-announcement periods $(\widehat{\beta}_{\chi 1} - \widehat{\beta}_{\chi 0})$ in relation to the changes in the implicit prices of the hedonic characteristics of the control properties between the pre- and post-announcement periods $(\widehat{\beta}_{\zeta 1} - \widehat{\beta}_{\zeta 0})$, where the weighting depends on the relative contribution of different hedonic characteristics to the prices of the properties transacted in the pre-announcement period.

An alternative way of implementing the second stage in order to estimate the impact of the public housing announcement is to estimate a fixed effect regression model of the following specification: $Y_{ist} = \delta_0 + \delta_1 Treatment_s + \delta_2 Post_t + \delta_3 Treatment_s \times Post_t + \varepsilon_{ist}$. Here, Y_{ist} refers to the imputed prices shown in equations 3–6, and ε_{ist} is the error term. The indicator variable *Treatment* takes the value of 0 for the prices in equations 3 and 4, and takes the value of 1 for the prices in equations 5 and 6. The indicator variable *Post* takes the value 0 for the prices in equations 4 and 6, and 1 for the prices in equations 3 and 5. The estimated coefficient of the *Treatment* × *Post* interaction term $\widehat{\delta}_3$ identifies the effect of the public housing announcement on property prices without being mixed-up with the quality changes effect, which is simply the difference-in-differences estimate shown in equation 10.[7]

While the above explanation of our third approach is based on the properties sold during the pre-announcement period, we can analogously apply this approach using the properties sold in the post-announcement period. In this case, the first step that involves running four hedonic regressions remains the same. In the second step, using the estimated coefficients obtained from

[7] The estimated coefficient $\widehat{\delta}_0$ is the average of the imputed log prices obtained in equation 4. The estimated coefficient $\widehat{\delta}_1$ is the pre-announcement log price difference $\Delta ln\, P^{0}_{\chi,\zeta}$ in equation 7. However, when we run a fixed-effect regression (instead of running an OLS regression with dummies), the *Treatment* variable is dropped out of the regression providing us with no estimate of δ_1. The estimated coefficient $\widehat{\delta}_2$ is the difference between the average of the imputed prices obtained in equation 3 and the average of the imputed prices obtained in equation 4.

the four hedonic regressions, we impute the prices of each property sold in the post-announcement period for the pre-announcement period. This provides us with four sets of imputed prices as follows:

$$
\begin{aligned}
ln\, \widehat{p_{j\zeta 0}}(Z_{j\zeta 1}) &= Z_{j\zeta 1}\, \widehat{\beta}_{\zeta 0}, & j = 1, \ldots, J & \qquad (11)\\
ln\, \widehat{p_{j\zeta 1}}(Z_{j\zeta 1}) &= Z_{j\zeta 1}\, \widehat{\beta}_{\zeta 1}, & j = 1, \ldots, J & \qquad (12)\\
ln\, \widehat{p_{k\chi 0}}(Z_{k\chi 1}) &= Z_{k\chi 1}\, \widehat{\beta}_{\chi 0}, & k = 1, \ldots, K & \qquad (13)\\
ln\, \widehat{p_{k\chi 1}}(Z_{k\chi 1}) &= Z_{k\chi 1}\, \widehat{\beta}_{\chi 1}, & k = 1, \ldots, K & \qquad (14)
\end{aligned}
$$

Equations 11 and 12 are the imputed log prices of post-announcement control group property j for pre- and post-announcement periods, respectively. Equations 13 and 14 are the imputed log prices of post-announcement treatment group property k for pre- and post-announcement periods, respectively. Using these imputed prices and following the same procedure as shown in equations 7–9, we can obtain the impact of the public housing announcement on property prices as follows:

$$
\begin{aligned}
\left[\Delta ln\, P^{1}_{\chi,\zeta} - \Delta ln\, P^{0}_{\chi,\zeta}\right]_{post-sample} &= \prod_{k=1}^{K} \left[\frac{\widehat{p_{k\chi 1}}(Z_{k\chi 1})}{\widehat{p_{k\chi 0}}(Z_{k\chi 1})}\right]^{\frac{1}{K}} \Bigg/ \prod_{j=1}^{J} \left[\frac{\widehat{p_{j\zeta 1}}(Z_{j\zeta 1})}{\widehat{p_{j\zeta 0}}(Z_{j\zeta 1})}\right]^{\frac{1}{J}} \\
&= ln \left[\prod_{k=1}^{K} exp\left(Z_{k\chi 1}(\widehat{\beta}_{\chi 1} - \widehat{\beta}_{\chi 0})\right) \Bigg/ \prod_{j=1}^{J} exp\left(Z_{j\zeta 1}(\widehat{\beta}_{\zeta 1} - \widehat{\beta}_{\zeta 0})\right)\right] \qquad (15)
\end{aligned}
$$

This measure of impact can be estimated by pooling the imputed prices in equations 11–14 and then using these imputed prices as the dependent variable in the fixed effect regression model of the specification shown in equation 1. In this equation, the estimated coefficient of the interaction term *Treatment* × *Post* identifies the impact of the public housing announcement on property prices controlled for quality changes in the sample properties.

This process provides us with two estimates of the impact of the public housing announcement, one that is based on the properties sold in the pre-announcement period and the other that uses the properties sold in the post-announcement period. We are indifferent when choosing between the estimates (equations 10 and 15), so provide both results side by side in the results section. A single estimate of the impact is obtained by taking the geometric mean of the two estimates.

$$\Delta ln\, P^1_{\chi,\zeta} - \Delta ln\, P^0_{\chi,\zeta} = \sqrt{\left[\Delta ln\, P^1_{\chi,\zeta} - \Delta ln\, P^0_{\chi,\zeta}\right]_{pre-sample} \times \left[\Delta ln\, P^1_{\chi,\zeta} - \Delta ln\, P^0_{\chi,\zeta}\right]_{post-sample}} \tag{16}$$

Furthermore, in order to assess the robustness of the results, we also provide the estimates obtained from the first two approaches in the empirical section. However, due to its methodological advantage, our discussion mostly focuses on the findings obtained from our third approach. It should be noted that the assumption involved in identifying the announcement effect—that the unobserved effects on the treatment and control group properties remain the same between the pre- and post-announcement periods—cannot be formally tested. This is because the counterfactual effects on treatment group property prices had there not been a public housing announcement are unknown. However, we conduct multiple placebo tests to examine the issue. Using our all three approches, we run placebo tests where false announcement dates are used, expecting no impact on the prices of treatment group properties if our identifying assumption is valid. These results are discussed in the results section.

4 Data

The paper uses property sales data consisting of actual transaction prices of properties sold in the Australian Capital Territory (ACT). While the data is obtained from Australian Property Monitors (APM), the original source of the data is the ACT Revenue Office.[8] The main analysis of the paper uses data consisting of 9,958 transactions of houses covering the two-year period from March 2016 to March 2018. This sample is obtained after excluding 1,273 observations that recorded extreme and implausible house prices (outside of AUD $10,000 - $5,000,000). As we know, the announcement of new public housing in multiple locations took place on 15 March 2017, hence our analysis uses transaction prices corresponding to one year before (4,932 observations) and one year after (5,026 observations) the announcement.

[8] APM is a private company having a considerable presence in Australia whose business focuses on providing property data and analytics to property professionals and real estate investors. APM, similar to other property data providers in Australia, supplements this original government-source data with additional characteristics information obtained from many sources, such as real estate agents, online sales advertisements, newspapers and property sales magazines. Most of the data set is publicly accessible through the Allhomes website (https://www.allhomes.com.au/ah/research).

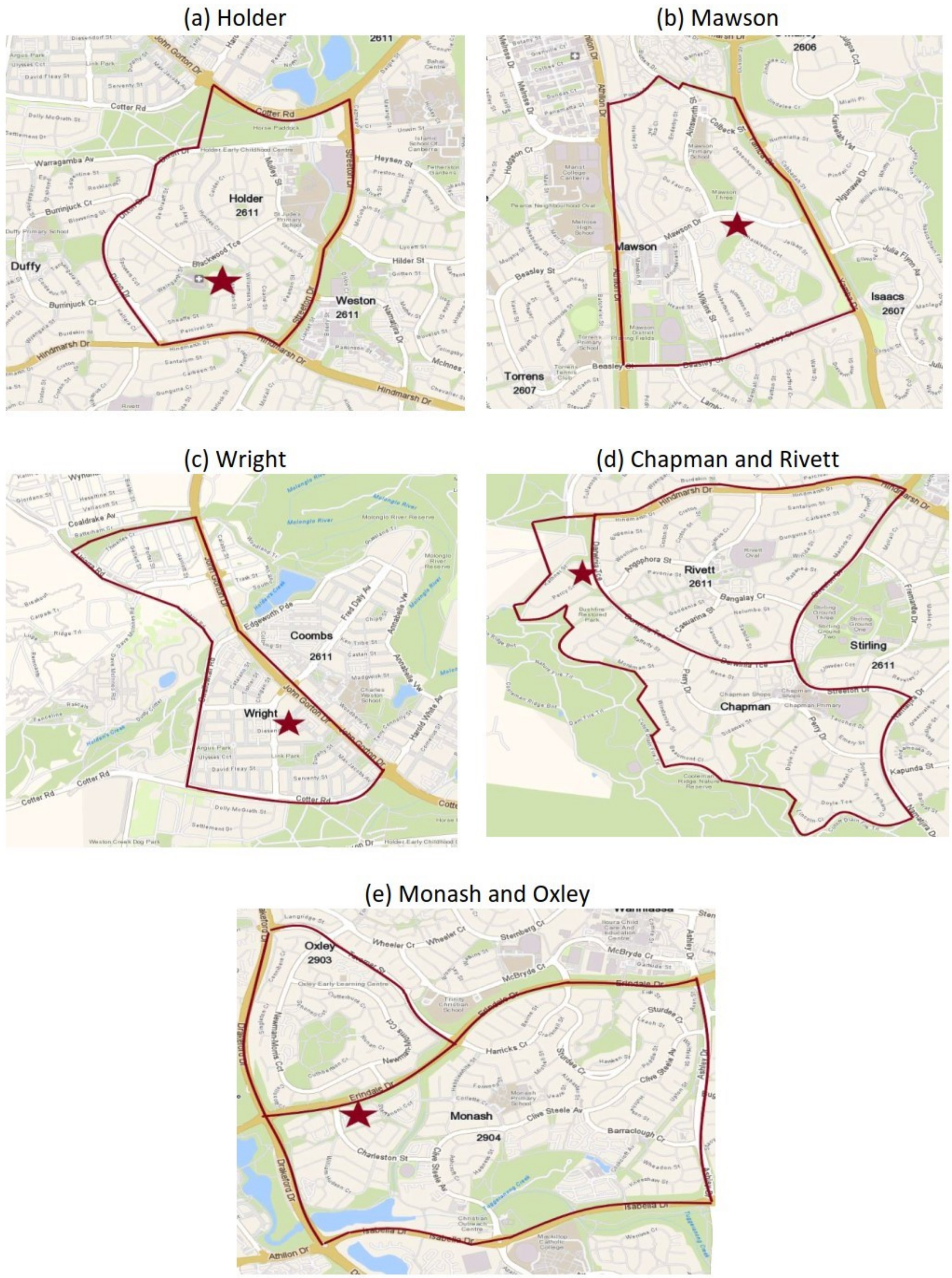

Figure 1: Announced public housing locations in treatment suburbs in the ACT (stars indicate locations of the announced public housing)

The five locations where the public housing will be constructed are in or in the close vicinity of the following suburbs: *Mawson*, *Holder*, *Monash*, *Chapman*, *Wright*, *Oxley* and *Rivett*. The five locations are indicated by the stars on the maps shown in Figure 1. In *Holder*, *Mawson* and *Wright*, the announced public housing locations are near the middle of the suburbs, surrounded by existing houses. The remaining two locations, in *Chapman* and *Monash*, are sited on the edges of these suburbs. The location on the edge of *Chapman* is just across the road from houses in *Rivett* and, similarly, the location on the edge of *Monash* is across a road and its grassed verges from houses in *Oxley*.[9] The property transactions in these seven suburbs—the five host suburbs and the two suburbs adjacent to two of the host suburbs—are included as the treatment group in our difference-in-differences (DiD) models. The property transactions that took place during our sample period in suburbs where the public housing will not be constructed (102 ACT suburbs, the remaining suburbs) are included in our DiD models as the control group. The number of observations in the treatment and control groups are 561 and 9,397, respectively.

Table 1 shows the mean and median prices of properties in the control and treatment groups for pre- and post-announcement periods. While the median price of the properties in the control suburbs increased by 7.6% between pre- and post-announcement periods, it only increased by 0.3% for the treatment suburbs. Similarly, the mean price of the properties in the control suburbs increased by 5.9% between pre- and post-announcement periods, it only increased by 1.6% for the treatment suburbs. One should, however, note that the quality of the sold properties might be different between the pre- and post-announcement periods and, in that case, the median and mean price changes would not reflect like with like changes in property prices between these comparison periods. For example, the public housing announcement might have resulted in the sale of high quality properties in the treatment suburbs, then the quality-adjusted price differences between control and treatment suburbs in the post-announcement period would be higher than the corresponding (quality unadjusted) median and mean price changes.

We now turn our attention to the seven treatment suburbs and rank these seven suburbs from most expensive to least expensive suburbs based on pre-announcement transaction prices adjusted for differences in the quality of properties. This ranking is obtained by running an hedonic regression of log(prices) on: quarterly time dummies; suburb dummies; lot size; the

[9] At the announced public housing locations, the border between *Chapman* and *Rivett* runs along Darwinia Terrace (a two-lane road), and the border between *Monash* and *Oxley* runs along Erindale Drive (also a two-lane road).

number of bedrooms, bathrooms and parking spaces; and the presence of a study, separate dining room, heating, air conditioning and/or ensuite. The regression includes 7 suburb dummies, each corresponding to a different *treatment* suburb, i.e. taking the value of 1 if the transaction corresponds to a particular treatment suburb, 0 otherwise. The base category for constructing these suburb dummies consists of pre-announcement transactions in all *control* suburbs. The estimated coefficients of the suburb dummies provide us with the ranking of the suburbs, with higher magnitudes implying more expensive, resulting in a higher rank, and lower magnitudes implying less expensive, resulting in a lower rank. We find that the ranking, as shown in Table 1, is as follows: *Wright*, *Chapman*, *Mason*, *Holder*, *Rivett*, *Oxley* and *Monash*.

Table 1: Prices of control and treatment group properties sold in pre- and post-announcement periods

Property location		Prices (AUD $ '000)			Quality-adjusted price differences†	
		Pre	Post	All	% of Base‡	Ranking
Control suburbs	Mean	598	633	616	-	-
	Median	576	620	600		
	N	[4,646]	[4,751]	[9,397]		
Treatment suburbs	Mean	640	650	645	-	-
	Median	628	630	630		
	N	[286]	[275]	[561]		
Wright	Mean	731	716	724	19.32***	1
	Median	787	780	782	(4.61)	
	N	[33]	[29]	[62]		
Chapman	Mean	738	757	745	18.85***	2
	Median	710	800	749	(2.62)	
	N	[44]	[31]	[75]		
Mawson	Mean	744	691	722	16.19***	3
	Median	735	681	713	(1.96)	
	N	[39]	[29]	[68]		
Holder	Mean	604	618	613	0.55	4
	Median	638	598	630	(3.02)	
	N	[30]	[50]	[80]		
Rivett	Mean	567	603	588	-3.89**	5
	Median	553	600	571	(1.96)	
	N	[48]	[64]	[112]		
Oxley	Mean	632	654	642	-5.88**	6
	Median	624	683	645	(2.72)	
	N	[16]	[14]	[30]		
Monash	Mean	552	616	580	-12.66***	7
	Median	543	608	569	(2.77)	
	N	[76]	[58]	[134]		
All suburbs	Mean	600	634	617	-	-
	Median	580	622	600		
	N	[4,932]	[5,026]	[9,958]		

Note: † These are obtained by running a hedonic regression of pre-announcement log(prices) of properties on suburb dummies, quarterly time dummies and physical attributes. The base category for the suburb dummies represents the control suburbs. ‡ The figures are the estimated coefficients of the suburb dummies and those in the parentheses are their estimated standard errors. The figures in the square brackets refer to the number of observations. *, ** and *** refer to the significance levels 10%, 5% and 1%, respectively.

Furthermore, we find that the estimated coefficients of *Wright*, *Chapman* and *Mason* are positive and are significantly different from zero at the 5% level, indicating that their property prices are on average more expensive than those of the control suburbs, the remaining ACT suburbs where public housing will not be constructed. This finding is consistent when we look at the simple mean and median prices where, although the ranking order of these three suburbs changes, all three suburbs still rank higher than the control suburbs. Moreover, the *magnitude* of the price differences between these three suburbs and the control suburbs, quality-adjusted or unadjusted, is also high, hence they clearly fall in the more expensive category. The estimated coefficients of *Rivett*, *Oxley* and *Monash* are negative and are significantly different from zero at the 5% level, indicating that the prices are on average less expensive than the control suburbs. The estimated coefficient of *Holder* is marginally positive and not significantly different from zero at the 5% level (p-value = 0.857). The mean and median prices of these four suburbs are either lower than or close to the mean and median prices of the control suburbs and, importantly, the prices are substantially lower than our three more expensive suburbs. For example, the difference in the median price of *Mawson*, which ranks lowest among the more expensive suburbs (rank: 3), and the median price of *Holder*, which ranks highest among the less expensive suburbs (rank: 4), is AUD$97,000 (this is 15.2% of *Holders* median price). Considering all this, we group these four suburbs in the less expensive category.

For the purpose of this study, it is ideal that we are able to group the treatment suburbs into more expensive and less expensive suburbs in comparison to the control suburbs, as this will allow us to examine the differential effect of a public housing *announcement* on property prices in host suburbs categorized into *relatively more and less expensive groups* in a quasi-experimental framework of analysis.[10] Only a few studies (e.g., Baum-Snow and Marion, 2009; Goujard, 2011; Diamond and McQuade, 2019) have examined this issue of the heterogeneous effect of public housing given the situation of the host location and none of them focused on the announcement effect and categorized into relatively more and less expensive host suburbs—hence our study plays an important complementary role within this scanty literature.

[10]When choosing locations, the ACT Government might have examined the price distribution across different suburbs and that distribution might have been one of the factors influencing their choices. This does not in any way invalidate the exogeneity assumption of the public announcement when analysing the differential effect of the announcement on property prices in the more and less expensive suburbs. This is because the ACT Governments prior knowledge would have no impact whatsoever on the individual buyers and sellers of properties in a particular suburb.

As discussed in the methodology section, it is important to be able to control for the changes in the quality of properties between pre- and post-announcement periods in order to obtain a pure measure of property price changes due to the announcement effect. Our data set, combined with the methodology used, will achieve this control for the following reasons. First, the vast majority of the ACT population resides in a planned city (Canberra, the capital of Australia), Canberra is largely populated by middle-income households (Robinson, 1973), and the ACT is administered by one body, the ACT Government (Banks and Brack, 2003)—all these factors contribute to a relative homogeneity of property quality, which works as a natural control to quality differences and concomitant heterogeneity of prices (Robinson, 1973; Banks and Brack, 2003). Second, location is an important price-determining characteristic of properties because location captures features related to amenities such as natural bushland, parks and playgrounds, pollution, traffic congestion and distances to places of interest. In the data, the suburbs in the treatment group are all located in the southern part of Canberra, implying that, in relation to the whole city, the properties in the treatment groups are in close proximity to each other, which controls for the external location-specific quality features. Third, the data set includes information on a number of physical attributes of properties—lot size, and the numbers of bedrooms, bathrooms and parking spaces, and the presence of a study, separate dining room, heating, air conditioning and/or ensuite. These are the physical characteristics that are typically included in hedonic regression models of property prices in order to control for quality differences (Eurostat, 2013; Hill, 2013) and, following this literature, we have used the same characteristics in our analysis.[11] Fourth, despite our above controls, there may be unobserved attributes of properties that result in quality differences between pre- and post-announcement properties. Provided these differences are the same between the control and treatment properties, the DiD method will control for the unobserved quality differences between pre- and post-announcement properties (Parmeter and Pope, 2013).

Furthermore, Cummings and Landis (1993) argue that the design and quality of public housing structures and the management of the housing are important factors affecting whether public

[11] The data set includes observations where the values corresponding to some characteristics are missing. These missing values are handled using dummy variables in the regression models; a dummy variable corresponding to a particular characteristic takes the value of 0 if the value is missing, 1 otherwise (Schafer and Graham, 2002; Breunig et al., 2019). In addition, we estimate different variants of hedonic models where we explore different ways of including the characteristics of properties. This includes estimating hedonic models with and without including missing values in the hedonic regressions. These variations do not make any qualitative difference with respect to our primary interest, i.e. the estimated DiD coefficients. The results obtained from these additional regressions are provided in the online appendix to this paper.

housing is viewed as a positive or negative feature of a neighbourhood. Our five public housing projects are similar in size and will be commissioned, administered and managed by the same authority means it is plausible to view these five public housing projects as similar in quality. All these controls—Canberra being a homogeneous city, treatment suburbs being located in relative proximity to each other and around the same distance from the center of the city, the announced public housing are of similar quality and our use of hedonic and DiD methodologies—will allow us to identify the impact of the public housing announcement without being mixed-up with other factors influencing the property prices during the same period.

In addition to the data used for our main analysis, we employed data with the same geographical coverage (the ACT) but going back for more years, starting from January 2010. This extra data was used to demonstrate the validity of the common-trend assumption in our DiD analysis. A series of placebo tests was conducted using this data, applying the artificial treatment to the years prior to the actual announcement. This data include transactions where prices range from AUD $10,000 - $5,000,000 (similar to the main data set), and contain 40,110 observations.

5 Results

This section discusses the results obtained from applying the three methodologies to our data. In these models, our primary interest is the DiD coefficient corresponding to the interaction term between the post-announcement period and the treatment group properties, i.e. $Treatment \times Post$.

5.1 Cross tables of property prices

Before we delve into the model results, let us construct cross tables of the mean of log prices in order to understand whether these simple measures provide an indication of what results we may expect to obtain when applying our three methods to the data (see Table 2). We construct such cross tables of the mean of log prices for treatment group properties sold in the pre- and post-announcement periods in: (a) more expensive suburbs, (b) less expensive suburbs, and (c) all treatment suburbs (that is, in both more and less expensive suburbs). The table also includes the mean of log prices of control group properties sold in the pre- and post-announcement periods. The first two rows in the cross tables show the two periods—post- and pre-announcement—and the first two columns in the tables show the two property groups—treatment and control. In each of the cross tables, the last row shows the difference in the mean of log prices of properties between the pre- and post-announcement periods, and the last column shows the difference in the mean of log prices between the treatment and control properties. The difference in differences in log prices is shown at the bottom right of each of the table panels. The figures in parentheses are standard errors, and square brackets are the number of observations.

Table 2: Cross tables of the mean of log prices by property group (control and treatment groups) and period (pre- and post-announcement periods)

	Treatment (1)	Control (2)	Difference (3)
a. More expensive suburbs			
Pre-announcement	13.487	13.262	0.225***
	(0.022)	(0.004)	(0.027)
	[116]	[4,646]	[4,762]
Post-announcement	13.456	13.323	0.133***
	(0.029)	(0.004)	(0.029)
	[89]	[4,751]	[4,840]
Post – Pre	-0.031	0.061***	-0.092**
	(0.035)	(0.006)	(0.040)
	[205]	[9,397]	[9,602]
b. Less expensive suburbs			
Pre-announcement	13.237	13.262	-0.025
	(0.017)	(0.004)	(0.022)
	[170]	[4,646]	[4,816]
Post-announcement	13.313	13.323	-0.010
	(0.014)	(0.004)	(0.020)
	[186]	[4,751]	[4,937]
Post – Pre	0.076***	0.061***	0.015
	(0.022)	(0.006)	(0.030)
	[356]	[9,397]	[9,753]
c. All treatment suburbs			
Pre-announcement	13.338	13.262	0.077***
	(0.015)	(0.004)	(0.017)
	[286]	[4,646]	[4,932]
Post-announcement	13.360	13.323	0.037**
	(0.014)	(0.004)	(0.017)
	[275]	[4,751]	[5,026]
Post – Pre	0.021	0.061***	-0.040*
	(0.020)	(0.006)	(0.024)
	[561]	[9,397]	[9,958]

Note: 1. The figures in the square brackets refer to the number of observations. 2. The figures in parentheses refer to the standard errors of the estimates. 3. *, ** and *** refer to the significance levels 10%, 5% and 1%, respectively.

Focusing first on the results for the more expensive suburbs, we observe that the mean of log price of the treatment group properties in the pre-announcement period is 13.49, shown in the cell at the intersection of the first row and first column. The mean of the log price of the control group properties in the pre-announcement period is 13.26. By taking the difference of the above two mean (log) prices, we find that, in the pre-announcement period, the treatment group property prices are on average 22.5% higher than the control group property prices. Moving one row down, to the post-announcement period prices, we find that the property prices in the treatment group are on average 13.3% higher than the property prices in the control group. The difference between these two differences (22.5% and 13.3%) provides us with a measure of the impact of the public housing effect on nearby property prices and reveals that the property prices in the treatment suburbs, compared to the control suburbs, fell by 9.2% between the pre- and post-announcement periods. Turning now to the results for the less expensive suburbs (panel (b)) and following the same steps in our calculations, we find that the impact of the public housing announcement on nearby property prices is negligible at 1.5%, which is also statistically not significant. One should, however, be aware that the results might be tainted by the difference in property quality between the pre- and post-announcement periods, which is why we emphasize quality adjustments in our analysis.

5.2 Hedonic quality adjustments

The data set includes information on a number of physical characteristics of properties—lot size, number of bedrooms, number of bathrooms, number of parking spaces and whether or not the property has an ensuite, study, separate dining, heating, air conditioning and/or a garage. We can see that there are quality differences in the properties sold between the periods and the groups in relation to their characteristics, which supports the use of hedonic adjustments within the difference-in-differences framework for identifying the impact of the public housing announcement on property prices. We use this information in the data on these physical characteristics and the suburb where the properties are located in the hedonic regressions that we estimate in order to control for the quality of properties between the pre- and post-announcement periods.[12]

All these characteristics of properties are converted to or treated as categorical variables and entered in the hedonic part of our three methods as dummy variables. The lot size is divided into quintiles, and these five groups are entered into our models as categorical variables, with the category corresponding to the first quintile treated as the base category. The number of bedrooms ranges from 1 to 8, and the properties are grouped into four categories: 1-bedroom, 2-bedrooms, 3-bedrooms and 4 or above bedrooms. Around 33% of the properties have 3 bedrooms, and this

[12] Online Appendix Table A.1 provides the value of the physical characteristics of the properties across treatment and control groups and pre- and post-announcement periods.

group is treated in the model as the base category. The number of bathrooms ranges from 1 to 3, and the properties are grouped into the three available categories. Around 29% of the properties have 1 bathroom, and this group is treated in the model as the base category. The number of parking spaces in our data ranges from 1 to 3. Around 17% of properties have 1 parking space, and this is included in the model as the base category. The other variables, presence or not of ensuite, study, separate dining, heating, air conditioning, and/or garage are dichotomous variables, entered in the hedonic models as 1 if yes, 0 if no. For each of the physical attributes, we include a dummy variable to accommodate a missing variable, which takes the value of 1 if the value of the attribute is missing and 0 otherwise.[13] The location of properties in our models is defined by suburb; there are 109 ACT suburbs included in the data of which 7 are treatment suburbs and the rest are control suburbs.

5.3 Method 1 results

The results obtained using method 1 are shown in Table 3. The table consists of 6 columns, denoted by (1) to (6), of the regression results. The results reported in the first 2 columns correspond to more expensive suburbs, the next 2 columns correspond to less expensive suburbs and the last 2 columns correspond to all suburbs. Each of these 3 sets of results contains 2 regression results corresponding to the DiD model augmented by the hedonic variables as specified in equation (2), with the odd and even columns reporting, respectively, results without and with suburb dummies in the models. The adjusted R^2 shows that suburb fixed effects improve the explanatory power of the models substantially in all 3 sets of results (e.g. for the more expensive suburbs, the adjusted R^2 increases by 29 percentage points when the suburb dummies are included). All of the estimated coefficients of the physical attributes have signs in line with our expectations and around 90% of the estimated coefficients are significant at the 5% level.

Now we turn our attention to the main variable of interest—the $Treatment \times Post$ interaction term in the models with suburb fixed effects. For the more expensive suburbs, the estimated

[13] There are missing observations in the data where the values of one or more characteristics are missing. It is reasonable to assume that the missing data are randomly missing in the sense that the probability of a characteristic of an individual property being missing does not depend on the missing value itself or on the value of other features of the property. The original sources of the data are government agencies. The physical characteristics information is not important for these agencies and can go missing, both at the submission and data entry stage (see, Hill and Syed, 2016). One way to address this *missingness* is to exclude observations with missing characteristics, which would reduce the efficiency of but not bias the estimates (Schafer and Graham, 2002). However, in our case, given that the number of properties in treatment groups is already very low, we use the dummy variable technique instead.

Table 3: Regression results of DiD models augmented by hedonic characteristics of properties (method 1)

	More expensive suburbs		Less expensive suburbs		All suburbs	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.180***		-0.070*		0.032	
	(0.019)		(0.036)		(0.059)	
Post	0.065***	0.071***	0.065***	0.071***	0.065***	0.071***
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)
Treatment × Post	-0.057***	-0.063***	0.026	0.009	-0.022	-0.018
	(0.010)	(0.013)	(0.021)	(0.019)	(0.029)	(0.020)
Missing lot size	-0.054**	-0.092***	-0.056**	-0.094***	-0.057**	-0.093***
	(0.027)	(0.031)	(0.027)	(0.031)	(0.026)	(0.031)
Lot size 20^{th} to 40^{th} percentile	0.096***	0.089***	0.096***	0.089***	0.090***	0.089***
	(0.024)	(0.013)	(0.024)	(0.012)	(0.024)	(0.012)
Lot size 40^{th} to 60^{th} percentile	0.104***	0.123***	0.105***	0.124***	0.103***	0.124***
	(0.023)	(0.014)	(0.024)	(0.014)	(0.024)	(0.014)
Lot size 60^{th} to 80^{th} percentile	-0.068**	-0.095***	-0.074**	-0.101***	-0.068**	-0.096***
	(0.029)	(0.025)	(0.029)	(0.024)	(0.029)	(0.024)
Lot size above 80^{th} percentile	-0.100***	-0.139***	-0.091***	-0.132***	-0.098***	-0.140***
	(0.032)	(0.018)	(0.032)	(0.017)	(0.031)	(0.018)
Missing bedroom	-0.189***	-0.195***	-0.195***	-0.202***	-0.193***	-0.197***
	(0.018)	(0.015)	(0.018)	(0.014)	(0.018)	(0.014)
1 bedroom	-0.325***	-0.462***	-0.309***	-0.457***	-0.316***	-0.463***
	(0.062)	(0.039)	(0.065)	(0.042)	(0.061)	(0.039)
2 bedrooms	-0.131***	-0.182***	-0.130***	-0.181***	-0.130***	-0.180***
	(0.024)	(0.019)	(0.024)	(0.019)	(0.024)	(0.019)
4+ bedrooms	0.152***	0.154***	0.150***	0.152***	0.151***	0.151***
	(0.014)	(0.010)	(0.014)	(0.010)	(0.013)	(0.010)
2 bathrooms	0.107***	0.076***	0.107***	0.077***	0.108***	0.076***
	(0.012)	(0.007)	(0.012)	(0.007)	(0.012)	(0.007)
3+ bathrooms	0.201***	0.168***	0.204***	0.172***	0.205***	0.169***
	(0.016)	(0.012)	(0.016)	(0.012)	(0.015)	(0.011)
2 parking spaces	0.049***	0.053***	0.048***	0.053***	0.048***	0.052***
	(0.012)	(0.006)	(0.012)	(0.006)	(0.012)	(0.006)
3+ parking spaces	0.034***	0.053***	0.031***	0.051***	0.032***	0.052***
	(0.012)	(0.006)	(0.011)	(0.006)	(0.011)	(0.006)
Has study	0.052***	0.033***	0.051***	0.032***	0.051***	0.032***
	(0.007)	(0.006)	(0.007)	(0.005)	(0.007)	(0.005)
Has separate dining	0.012	0.015***	0.011	0.014***	0.009	0.015***
	(0.009)	(0.005)	(0.008)	(0.005)	(0.008)	(0.005)
Has heating	-0.012**	0.010***	-0.012**	0.010***	-0.012**	0.010***
	(0.006)	(0.004)	(0.006)	(0.004)	(0.005)	(0.004)
Has air conditioning	-0.024***	-0.002	-0.023***	-0.001	-0.023***	-0.001
	(0.007)	(0.003)	(0.007)	(0.003)	(0.007)	(0.003)
Has ensuite	-0.015**	-0.003	-0.016**	-0.003	-0.015**	-0.003
	(0.007)	(0.005)	(0.007)	(0.005)	(0.007)	(0.005)
Has garage	-0.028***	-0.003	-0.027***	-0.002	-0.027***	-0.003
	(0.008)	(0.005)	(0.008)	(0.005)	(0.007)	(0.005)
Constant	13.077***	12.920***	13.079***	12.923***	13.080***	12.922***
	(0.030)	(0.013)	(0.030)	(0.013)	(0.030)	(0.013)
Suburb FEs	No	Yes	No	Yes	No	Yes
Adjusted R^2	0.33	0.62	0.32	0.61	0.32	0.61
N	9,602	9,602	9,753	9,753	9,958	9,958

Note: 1. The regression models in columns (2), (4) and (6) include suburb fixed effects and the regression models in columns (1), (3) and (5) do not. 2. The figures in parentheses refer to the standard errors of the estimates. 3. *, ** and *** refer to the significance levels 10%, 5% and 1%, respectively.

coefficients of the interaction term are negative (6.3%) and significant at the 1% level, indicating that the public housing announcement resulted in a fall in nearby property prices. For the less expensive suburbs, the estimated coefficient is close to 0 (0.9%) and not statistically significant at the 5% level, implying there was no meaningful impact of the public housing announcement on nearby property prices. The overall effect—the effect on all treatment properties—is negative but is not statistically significant at the 5% level.

5.4 Method 2 results

Method 2 allows the estimated implicit values of the quality attributes of properties to vary between treatment and control group properties. This flexibility may result in lower variances of the estimated residuals and, hence, improve the precision of the estimated DiD coefficients. This method involves two stages of estimations. In the first stage, the hedonic regressions of the log of property prices are run. Two variations of these regressions are estimated: one includes both treatment and control properties in the first stage, while the other runs two first-stage regressions, separately for the treatment and the control group.[14]. In the second stage, the residuals obtained from these first regressions are pooled together to run a regression of these estimated residuals on the DiD variables. Table 4 provides the results when this method is applied separately to more expensive suburbs, less expensive suburbs and all suburbs. The results are again very similar to those obtained when using method 1 in which the impact of the public housing announcement on property prices is negative and significant for more expensive treatment suburbs, and negligible and insignificant for less expensive treatment suburbs.

Table 4: DiD regression results of estimated residuals (dependent variable) obtained from stage 1 hedonic regressions (stage 2, method 2)

	More expensive suburbs		Less expensive suburbs		All suburbs	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.031***	0.031***	-0.005	-0.006	0.009	0.011
	(0.006)	(0.005)	(0.007)	(0.005)	(0.009)	(0.009)
Post	0.069***	0.069***	0.069***	0.069***	0.069***	0.069***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treatment × Post	-0.059***	-0.059***	0.008	0.009	-0.017	-0.021
	(0.012)	(0.010)	(0.018)	(0.014)	(0.019)	(0.019)
Constant	-0.035***	-0.035***	-0.035***	-0.035***	-0.035***	-0.035***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.04	0.04	0.04	0.04	0.04	0.04
N	9,602	9,602	9,753	9,753	9,958	9,958

Note: 1. The table shows the results from stage 2. Stage 1 runs hedonic regressions of property prices and calculates the regression residuals. 2. The stage 2 results in columns (1), (3) and (5) use the estimated residuals obtained from stage 1 regressions that are run combining treatment and control proerties, and the results in columns (2), (4) and (6) use the estimated residuals that are obtained from the stage 1 regressions that are run seperately for treatment and control properties. 3. The figures in parentheses refer to the standard errors of the estimates. 4. *, ** and *** refer to the significance levels 10%, 5% and 1%, respectively.

[14]The first stage regression results are provided in online appendix Table A.2

5.5 Method 3 results

In method 3, quality adjustment is attained in the first stage of the two stage procedure through the creation of a balanced panel of properties using hedonic imputation of property prices. This is achieved by estimating hedonic models separately for 4 sets of transacted properties: pre-announcement treatment group properties, post-announcement treatment group properties, pre-announcement control group properties and post-announcement control group properties. All the estimated coefficients which are significant at the 5% level have signs in line with our expectations (around 90% of the estimated coefficients are significant at the 5% level).[15] In the second stage, the identification stage, we estimate the DiD models using the imputed prices (obtained from the first stage) as the dependent variable. Table 5 provides the results of the estimated DiD models. Panel (a) of the table is based on the properties that were sold in the pre-announcement period, and panel (b) relies on the properties that were sold in the post-announcement period. Columns 1, 2 and 3 show the impacts of public housing on more expensive suburbs, less expensive suburbs and all suburbs, respectively.

First, we focus on the results for the more expensive suburbs. In panel (a), which is based on pre-announcement properties, the estimated coefficient corresponding to the *Post* dummy indicates that on average property prices in the control suburbs increase by 7.0% between the pre- and post-announcement periods. Note that the treatment dummy is dropped out of these property fixed-effect regressions because each property, belonging to either the treatment or the control group, has two observations—one for the pre- and the other for the post-announcement period and, therefore, Table 5 does not show any estimates of treatment effects. The estimated coefficient corresponding to the interaction dummy $Treatment \times Post$ indicates that the increase in the prices of treatment properties between the pre- and post-announcement periods is 5.9% lower compared to the control properties. In other words, this means while the overall prices of properties went up in the post-announcement period, the prices of the treatment properties increase only by 1.1%, indicating that the dampening effect of the new public housing announcement on nearby property prices has been substantial. The results in panel (b), which is based on post-announcement properties, indicate that on average property prices in control suburbs increase by 7.1% between the pre- and post-announcement periods, while the property prices in treatment suburbs decrease by 1.0%, indicating a substantial dampening of negative 8.1%. The results in the two panels

[15] The results of these four first stage regressions are provided in online appendix Table A.3.

Table 5: DiD regression results of imputed prices (dependent variable) obtained from stage 1 hedonic regressions (stage 2, method 3)

	More expensive suburbs (1)	Less expensive suburbs (2)	All suburbs (3)
a. Using pre-announcement property characteristics			
Post	0.070***	0.070***	0.070***
	(0.004)	(0.004)	(0.004)
Treatment × Post	-0.059***	0.015	-0.015
	(0.014)	(0.019)	(0.022)
Constant	13.267***	13.261***	13.266***
	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.70	0.70	0.68
N	9,524	9,632	9,864
b. Using post-announcement property characteristics			
Post	0.071***	0.071***	0.071***
	(0.004)	(0.004)	(0.004)
Treatment × Post	-0.081***	0.001	-0.025
	(0.009)	(0.013)	(0.018)
Constant	13.256***	13.252***	13.255***
	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.67	0.67	0.65
N	9,680	9,874	10,052

Note: 1. Stage 1 of the method uses the estimated hedonic regressions in the previous table in order to impute a post-announcement price for each property sold in the pre-announcement period, a pre-announcement price for each property sold in the post-announcement period, and a price for each property for the actual period (pre- or post-announcement) in which it was sold. 2. The figures in parentheses refer to the standard errors of the estimates. 3. *, ** and *** refer to the significance levels 10%, 5% and 1%, respectively.

indicate that, for properties in the more expensive suburbs, the implicit prices for the property characteristics post-announcement have been affected negatively compared to the same property characteristics pre-announcement. The geometric mean of the two effects gives us the combined effect of the public housing announcement on more expensive suburbs—negative 7.0%.

Moving now to the less expensive suburbs, in panel (a) the estimated coefficient corresponding to $Post$ shows that property prices in the control suburbs increase by 7.0% between pre- and post-announcement periods. The estimated interactive coefficient $Treatment \times Post$ is small and not significantly different from zero at the 5% significance level, indicating that there has been no impact on the public housing announcement on nearby property prices. In panel (b), the estimated coefficient is again small and statistically insignificant. This indicates that, in contrast to the more expensive suburbs, implicit prices for pre- and post-announcement property characteristics in the less expensive suburbs are not affected in any systematic way by the announcement.

Overall, the results in Table 5 demonstrate that the differential impact of the public housing announcement depends on whether the public housing is to be located in more or in less expensive suburbs. This finding of the heterogeneous impact of public housing is in concordance with Diamond and McQuade (2019) who in their research observe public housing in high-income neighbourhoods had a dampening effect on surrounding property prices. However, while they report public housing in low-income neighbourhoods had an accelerating effect on surrounding property prices, we find that, based on the results we obtained from all three of our methodologies, the announcement of public housing to be built in less expensive neighbourhoods has negligible to no effect on nearby property prices.

5.6 Robustness checks

In order to check the robustness of the results, in addition to the three methods discussed above, we also estimate the impact of the announcement using different variations of our empirical models and different sample data. These results are provided in online appendix Table B.1–B.8. As we have discussed, it is important to undertake appropriate quality adjustments in order to identify the announcement effects. We estimate different variations of hedonic models that include the physical and locational attributes and treat missing observations in different ways. For example, when we categorize properties by number of bedrooms and bathrooms, we mix the categories around in our various hedonic models. In the case of lot size, our base model includes lot size in five categories but we also estimate our models where log(lot size) is included as a continuous variable (see online appendix Table B.1). With regard to controlling for locational factors, while our base model controls for suburbs, we also estimate hedonic models where no suburb dummies are included and where we include postcode dummies (postcodes cover a larger area than suburbs) instead of suburb dummies (see online appendix Table B.2).

With regard to addressing missingness, we use dummy variable techniques in our base models. We also check robustness by estimating the impact of the announcement on all treatment properties where we exclude properties with missing characteristics and we find similar results (see online appendix Table B.3). Furthermore, we also estimate the models using different sample data for the pre-announcement period in which, instead of using data from only one year prior to the announcement, we include data from the previous year as well (see online appendix Tables B.4 and B.5). With regard to the variations in control group properties, we use properties in non-host suburbs that are in the same postcode as the host suburbs where the public housing will be located (see online Table B.6). In order to check that our results are not impacted by the inclusion of the five ACT rural townships that are outside Canberra, we undertake further estimates using only suburbs within Canberra (see online appendix Table B.7).

With regard to the variations in treatment group properties, we restrict the treatment group to properties sharing the same street where the public housing will be located. Because of the small sample size of treatment group properties, we could carry out cross tabulation but could not use our econometric models for this robustness exercise. Further variations in our selection of treatment group properties are based on the distance of the properties from the public housing locations and include distance dummies as the $Treatment$ variables in the DiD model. While we find that the estimated coefficients of the treatment variables are not significant for most treatment variables, we generally find that the public housing impact on property prices reduces as distance increases (see online appendix Table B.8).

All these robustness checks provide strong support for the results that we find from using our base models—that the public housing announcement has a differential impact on nearby property prices, with a large negative impact when the host neighborhood are relatively more expensive and none to a negligible effect when the host neighborhood are relatively less expensive neighborhood.

5.7 Validation of the common trend assumption

It is important to note that the DiD estimate in our models can identify the average treatment effect (ATE) of the announcement only if the common trend assumption holds. We conduct multiple placebo tests to examine the identifying assumption where we carry out the same exercise we conducted in our base models but with *false announcement dates*. The first such test that we conduct assumes that the announcement was on 15 March 2016 (i.e. one year prior to the actual announcement) and uses properties sold between 15 March 2015 and 14 March 2016 as the pre-announcement sample and properties sold between 15 March 2016 and 14 March 2017 as the post-announcement sample. Table 6 provides the results obtained for these placebo tests from our three models. The results show that 11 out of the 12 estimated $Treatment \times Post$ DiD coefficients are not significant at the 5% level. Such results provide strong evidence of the validity of the common trend assumption in our data and the models we apply to both the more expensive and the less expensive treatment group properties.

Table 6: Placebo test results of our DiD models based on a false announcement date

	More expensive suburbs (1)	Less expensive suburbs (2)	All suburbs (3)
a. Method 1			
Post (placebo)	0.053***	0.053***	0.053***
	(0.005)	(0.005)	(0.005)
Treatment × post (placebo)	0.001	-0.029	-0.018
	(0.015)	(0.028)	(0.021)
Adjusted R^2	0.61	0.61	0.61
N	9,638	9,766	9,980
b. Method 2			
Post (placebo)	0.052***	0.052***	0.052***
	(0.004)	(0.004)	(0.004)
Treatment × post (placebo)	-0.012**	-0.029	-0.021
	(0.006)	(0.027)	(0.019)
Adjusted R^2	0.02	0.02	0.02
N	9,638	9,766	9,980
Method 3			
c. Using pre-announcement property characteristics			
Post (placebo)	0.053***	0.053***	0.053***
	(0.004)	(0.004)	(0.004)
Treatment × Post (placebo)	-0.003	-0.023	-0.016
	(0.009)	(0.032)	(0.022)
Constant	13.236***	13.231***	13.236***
	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.54	0.51	0.51
N	9,752	9,900	10,096
d. Using post-announcement property characteristics			
Post (placebo)	0.057***	0.057***	0.057***
	(0.006)	(0.006)	(0.006)
Treatment × Post (placebo)	-0.027	-0.037	-0.033*
	(0.018)	(0.026)	(0.019)
Constant	13.211***	13.205***	13.211***
	(0.003)	(0.003)	(0.003)
Adjusted R^2	0.40	0.40	0.39
N	9,524	9,632	9,864

Note: 1. These placebo tests are based on a false public housing announcement in March 2016, i.e. one year before the actual announcement. 2. The figures in parentheses refer to the standard errors of the estimates. 3. *, ** and *** refer to the significance levels 10%, 5% and 1%, respectively.

We further investigate the validity of the common trend assumption using our model 3 applied to data covering a longer period, 2010–2018. This investigation relies on seven different false public housing announcement dates and one actual announcement date, one for each of the years covered by the data. These placebo tests require us to estimate hedonic regressions separately for control and treatment suburbs for each of these eight years, and then imputing the prices of the properties sold in the year prior to the actual announcement and in the year after the actual announcement, hence coinciding with the pre- and post-announcement periods of our base models. Altogether these tests provide us with 48 estimates of *Treatment* × *Post* DiD coefficients, of which 42 estimates are based on the false announcement dates (placebos) and 6 estimates are based on the actual announcement date (see online appendix Tables B.4–B.5). Of these 42 estimates for placebos, 37 are found to be not statistically significant at the 5% level. The 5 estimates which are found to be significant are scattered across the false treatment dates and the more expensive, less expensive and all treatment suburbs. Hence, all these investigations indicate that the common trend assumption in our analysis is valid and that the estimated DiD coefficients in our base models measure the average treatment effects of the announcement of new public housing on nearby property prices.

6 Policy implications and conclusion

This paper investigates the differential effects of a public housing announcement on relatively more and less expensive host neighborhoods. We undertake this investigation using a quasi-experimental framework of analysis, taking advantage of (1) a single unanticipated government announcement of the construction of new similarly sized public housing in five locations in the Australian Capital Territory (ACT) on 15 March 2017, and (2) the fact that these host locations include both more and less expensive suburbs in relation to the remaining suburbs in the ACT. In order to identify the impact of the public housing announcement, we use the difference-in-differences (DiD) method in conjunction with hedonic quality adjustment and a data set of actual house sales in all ACT suburbs from one year before the announcement (March 2016) to one year after (March 2018). We apply three DiD methods to our data, where these methods differ in terms of how we control for quality changes of properties between the pre- and post-announcement periods. In all three methods, our primary interest is the interactive $Treatment \times Post$ DiD coefficient, which identifies the impact of the public housing announcement on nearby property prices. We find significant heterogeneity in the impact of the public housing announcement, which depends on whether the public housing is to be located in relatively more expensive or relatively less expensive suburbs.

The results from our most preferred model show that where the host suburb is relatively more expensive than the remaining ACT suburbs, the dampening effect of the public housing announcement on nearby property prices is 7.0% in the first year after the announcement. Given that the total stock of property in those suburbs was 9,116 and the median price was AUD$628,000 in 2016 (see Table 1), this implies a total fall in property value in these suburbs of more than

AUD$400 million.[16] Since property is a highly leveraged asset, this reduction is substantially higher on the equity of the property owners. Using the average loan-to-value ratio of 50% for Australia (Kohler and Hobday, 2019), this implies that the public housing announcement resulted in a nearly 14% fall in the equity of property owners within one year after the announcement. The fall in equity will be even more substantial for new home buyers whose loan-to-value ratio is typically much higher (around 80%) than the national average. This large fall in property values will also result in the reduction of property tax collected by the ACT government. If, on the other hand, all the public housing had been located in suburbs that are less expensive than the remaining ACT suburbs, property owners in the neighborhood would not experience any impact on their property prices, hence the increased likelihood of defaults on mortgages resulting from a fall in property prices would not eventuate, and the amount of property tax collected by the ACT would be unaffected.

However, as discussed earlier, in addition to providing affordable shelter, public housing provides residents with benefits depending on the existing features of the host location; positive amenities such as schools and playgrounds will increase the benefit to new public housing residents (Gibbons et al., 2014; Albouy et al., 2020; Breunig et al., 2019) while negative features such as crime and overcrowding will have a detrimental effect on the residents (Gibbons, 2004; Sinai and Waldfogel, 2005; Eriksen and Rosenthal, 2010; Aliprantis and Hartley, 2015). These positive and negative location-based features also play an important role in determining the prices of properties across different suburbs, so the more expensive suburbs tend to have more amenities than the less expensive suburbs (see, Bayer et al., 2007; Ellen et al., 2007; Diamond, 2016). This means if the public housing is located in the relatively less expensive suburbs, the benefit to the future public housing residents will be lower than if the public housing is located in the relatively more expensive suburbs.

A question that plays an important role in policy discussions in welfare and regional economics is how to attain a balance between efficiency and equity (Alexiadis, 2018). Policymakers may consider building public housing in the relatively less expensive suburbs because, ceteris paribus, this will maximize the combined market value of properties in the region. However, public housing in less expensive suburbs may result in economic and racial segregation (Verdugo and Toma, 2018). Hence, there is a trade-off between efficiency and equity—it may be more efficient

[16] The data on the stock of properties is obtained from the 2016 Census Data collected by the Australian Bureau of Statistics (ABS).

for public housing to be located in less expensive suburbs but more equitable for public housing to be located in more expensive suburbs. Social planners who choose to maximize efficiency at a given level of equity will therefore locate public housing in relatively less expensive suburbs, and those who choose to maximize equity will select more expensive suburbs (Kleinhans, 2004; Galster, 2007). Our analysis, looking at the differential impact of locating public housing in more and less expensive suburbs, will help policymakers to make informed decisions about public housing locations in order to achieve their desired balance between the two objectives of equity and efficiency.

In the case of the ACT, the host suburbs are located near each other in the southern part of the city, each roughly the same distance from the city centre, hence, the price difference due to any external amenities (i.e. amenities outside these treatment suburbs) would be negligible. Furthermore, Canberra is a relatively homogeneous city—the difference in the value of properties across suburbs may be partly due to the presence or absence of amenities. This means if social planners identify which particular amenities are responsible for the observed price differences (e.g. the presence or absence of good public transport or a proper children's playground), they could cover the cost of providing these amenities with the savings achieved by avoiding a fall in property prices through not locating public housing in the more expensive suburbs. Hence, the public housing could be located in relatively less expensive suburbs on the grounds of efficiency when complemented by the provision of amenities these suburbs lack, and this would also reduce inequity, resulting in an outward shift of the social welfare function. Given the resulting potential benefit of new amenities to particular host locations, which includes a potential rise in property prices, this kind of well-informed choice by policymakers may result in less opposition to public housing by local residents and create a favorable environment for more public housing.

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