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Neighbourhood, school zoning and the housing market: Evidence from New South Wales*

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Abstract

This paper investigates the impact of primary school zoning on housing prices in Australia. Using comprehensive data on both schools and housing transactions in New South Wales along with the combination of boundary and regression discontinuity design techniques, we find that the price of houses located in high-performing side of primary school zone boundaries is, on average, about 2.7% to 3.3% higher than that of similar houses located in low-performing side of these boundaries. This finding provides not only an insight into the price elasticity of demand for high quality education, but also has important policy implications as it highlights the need to address the potential educational inequalities associated with school zoning in Australia.

Key words: School Zoning, House Prices, Boundary Discontinuity Design, Regression Discontinuity Design, Price Premium.

JEL/MS classification: R31; I24; I28; C14; C21.

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1 Introduction

Admission into public primary schools in most Australian states is generally contingent on a student's residential address, and New South Wales (NSW) is no exception. Each government school within NSW has a designated intake zone and students residing within this area are guaranteed admission. Although government schools may accept enrolments from outside their designated zone, a school's enrolment capacity is generally fulfilled by enrolling only those students who reside in the school's catchment area. This is especially the case for high-performing public schools.

With limited admission into high-performing public schools, the capitalization of school quality into the price of houses in high-performing school zones is inevitable. The willingness of parents to pay a premium on property to secure their child's enrolment into a high-performing school is a widely recognised phenomenon. Many studies in the United States (US) and United Kingdom (UK) have attempted to quantify this premium by, for example, coupling the boundary fixed effects (BFE) analysis with other estimation strategies. In this paper, we focus on New South Wales (Australia) and investigate the extent to which primary school zoning impacts housing prices.

The BFE method was introduced by Black (1999) in this context to address concerns arising from the fact that better schools tend to be located in affluent neighbourhoods, and students drawn from these 'privileged' socioeconomic backgrounds generally have higher academic achievement. A key assumption of the BFE strategy is that houses located near school attendance boundaries and sufficiently close to one another share the same neighbourhood characteristics, therefore once house characteristics have been controlled for, any difference in house prices across boundaries is attributable to school quality (Black, 1999). Restricting the sample to housing sales located within 250 metres of an attendance boundary, Black (1999) finds that a 5% increase in primary school test scores (approximately one standard deviation) leads to a 2.5% increase in house prices.

Since the seminal work of Black (1999), a number of studies have coupled the BFE technique with additional controls relating to socio-economic characteristics, such as household income. In doing so, studies like Kane et al. (2005) have established a greater fall in the estimated impact of school test scores on housing prices compared to Black (1999). Fack and Grenet (2010) improve the BFE estimation strategy by incorporating it into a matching framework under which identical properties across school admission boundaries are matched. In addition to Fack and Grenet's (2010) matching framework, Gibbons et al. (2013) also propose to control for spatial trends (i.e.

the distance between two houses on opposing sides of a school attendance boundary) in the BFE model.

Fack and Grenet (2010) differs from much of the literature in that they explore the extent to which housing prices react to the quality of education offered by neighbouring public and private secondary schools. They establish that a one-standard deviation increase in public school performance raises house prices by 1.4% to 2.4% in Paris (France). Their most remarkable finding is that increased access to private schools tend to mitigate the impact of public school performance on housing prices. Unlike Fack and Grenet (2010), the focus of many existing studies has been solely on public primary school performance and attendance zones, thus ignoring school choice and the variation in secondary school assignments. As a result, the estimates on the capitalization of public school performance on housing prices could be biased downwards in these studies, particularly when households on either side of the boundary do not share a secondary school. This highlights an avenue for future research whereby, adopting the BFE approach, one would simultaneously examine the primary and secondary public school attendance zoning effect on house prices, upon controlling for access to private schooling.

With the exception of Davidoff and Leigh (2007), this topic is yet to be formally addressed in Australia using appropriate recent econometric techniques. To the best of our knowledge, we are not aware of an Australian study that investigates rigorously the relationship between primary school zoning and housing prices. Using NSW as a case study, our study fills this gap by estimating the extent to which primary school zoning impacts housing prices. We focus on primary school zoning for two reasons predominantly. Firstly, it is well-established that educational attainment in a child's early years are positively correlated with academic and economic success later in life (Gibbons and Machin, 2003). And secondly, receiving a high quality primary education is often crucial for children to gain admission into selective secondary schools. For these reasons, parents are likely to invest more into their child's primary education, with the expectation to get into a good quality secondary school. As such, parents are likely to pay a premium on house prices to be located in a high-performing primary school zone. Our goal is to quantify this premium.

To identify this premium, we couple the boundary discontinuity design (BDD) strategy of Black (1999) with the regression discontinuity design (RDD) framework. The RDD has been widely used in empirical studies and is considered the most credible non-experimental strategy within the casual inference framework (Calonico et al., 2018). Its reliance on weak non-parametric identifying assumptions enables flexible and robust estimation and inference for local treatment effects (Calonico et al., 2018). The BDD is a special case of RDD. Embedding the BDD into the data selection procedure, we consider only those housing sales close to and on either side of a school attendance boundary, upon maintaining the BFE key assumption (i.e., houses located near school attendance boundaries and sufficiently close to one another share the same neighbourhood characteristics). Although maintaining this assumption addresses a number of endogeneity concerns, it does not address the possibility of high-income households sorting into high-performing school zones. Such sorting may contribute to higher average house prices in performing school zones since high-income households are more likely to invest in property improvements. Due to data limitation, we could not formally address this selection problem. However, we have controlled for neighbourhood characteristics (including the median high and low suburbs' income) throughout all our estimations.

Our joint BDD & RDD identification strategy provides clearly a useful insight on the capitalization of school performance into house prices in Australia in general, and New South Wales in particular. We find that on average the price of a house located in a high-performing primary school zone is approximately 2.7% to 3.3% higher than a similar house in a lower-performing zone. To enable comparison with the existing literature, Table 1.1 below summarises the estimated effects of school quality on house price from seven prior studies. All the estimates measure the effect of a one-standard deviation increase in school quality on house prices, thus enabling for a direct comparison across studies.

Table 1.1: Studies Estimating the Effect of School Quality on House Prices

Study	**Effect (%)**	**Schooling Level**	**Sample**
Australia			
Davidoff and Leigh (2008)	3.5	Secondary	Australian Capital Territory
UK			
Gibbons & Machin (2003)	3 to 10	Primary	UK
Gibbons et al (2013)	3	Primary	UK
USA			
Black (1999)	2.5	Elementary	Boston
Kane et al. (2005)	10	Elementary	North Carolina
Bayer et al. (2007)	2.4	Middle	San Francisco Bay Area
France			
Fack & Grenet (2010)	1.4 to 2.4	Secondary	Paris

Note: The RD effect is measured as the effect of a 1-s.d. increase in school performance on house prices.

The remainder of this paper is as follows. Section 2 describes the extraction process of data and the construction of key variables. Section 3 outlines the empirical strategy employed. Section 4 presents the results, while Section 5 provides diagnostic checks validating the identification strategy employed. Finally, Section 6 concludes.

2 Data

The data set used was provided by *Australian Property Monitors* (APM); a leading property intelligence platform that has delivered comprehensive property data and analytics across Australia since 1989. This extensive data set contains the sale price of all properties sold in New South Wales between January 2014 and March 2019. Property characteristics are also included within APM's sales transaction data, enabling the construction and use of appropriate control variables within the empirical analysis. Such characteristics include the *property type*, *area size*, *number of bedrooms*, *bathrooms and parking spaces*, and a broad range of other property features. These features include whether the property has an *air conditioner*, *alarm*, *balcony*, *barbeque*, *courtyard*, *ensuite*, *family room*, *fireplace*, *garage*, *heating*, *internal laundry*, *locked garage*, *polished timber floor*, *pool*, *rumpus room*, *separate dining*, *spa*, *study room*, *sunroom*, and *walk-in wardrobe*. Each of these variables, aside from area size, number of bedrooms, bathrooms and parking spaces, are dummy variables and thus, are not appropriate for our the regression discontinuity design analysis that usually relies on continuity in the control variables.

To enable within-sample comparison, apartments and units were excluded from the analysis. While townhouses are included within the sample, their area size was excluded since the land area of a townhouse complex is often reported as the area size of an individual townhouse. Furthermore, housing sales exceeding $6 million were excluded from the sample because these sales often relate to large commercial parcels of land.

The APM data set also provides the precise location of each household. With this information neighbourhood characteristics could be controlled for through suburb clustering and additional controls. Suburb boundaries as well as the median household income of each suburb was obtained from the *Australian Bureau of Statistics* (ABS) 2016 census data. Due to the ABS census design, the median household income was recorded as an income range. For example, the median household income in Bondi; an affluent Sydney suburb, lies between a lower bound of AU$156,000 and an upper bound of AU$181,999. As such, both the lower and upper bounds of median suburb income were included in our analysis. The availability of geographical coordinates also allowed for the implementation of Black's (1999) boundary fixed effects method, thus enabling us to control for neighbourhood specific effects in our empirical strategy, as detailed later on.

Primary school catchment zones and data on New South Wales primary schools, including their geographical coordinates, was obtained from the NSW Department of Education. The data pertaining to NSW primary schools was filtered down to include only those public schools located in *metropolitan NSW*. Again, to enable within sample comparison, only comprehensive, non-selective, co-educational, kinder to year 6 public primary schools without an attached pre-school or intensive English centre, operating within ordinary schooling hours were included in the sample.

To identify low- and high-performing schools, researchers generally adopt measures such as the proportion of students who reach the target level of attainment in standardized testing. Australia's standardized testing, first introduced in 2008, is known as the National Assessment Program – Literacy and Numeracy (NAPLAN). However, with recent concerns over NAPLAN testings ability to adequately rank schools (see e.g. Thompson and Cook, 2014; Wu, 2015; Rose et al., 2018), we avoid using NAPLAN as schools' performance measure. The challenge is then wich measure should be used to rank schools? Many Australian parents are familiar with the activities of Better Education, a free online community that provides a platform for linking parents to schools. Aside from government platforms, Better Education is the most popular school directory and education information website in Australia. It provides "state overall scores" of schools and their related ranking. With the absence of any consensual measure of performance in Australia, we hypothesize that most parents assess school performance using the resources provided by Better Education. As such, we use their ranking as our performance measure of primary schools in NSW. For the public primary schools in our sample, state overall scores range from 68 to 100. Schools with a score that is strictly above 90 were deemed high-performing in our baseline analysis, while the remaining were treated as lower-performing. A sensitivity analysis shows that increasing this threshold to 95 leads to similar conclusions as those of the baseline 90 cut-off point.

In order to implement the boundary discontinuity design (BDD), data was extracted from boundaries shared by high-performing and lower-performing school zones, as illustrated in Figure 2.1. The school on the left-hand side (LHS) of this figure, represented by a red circle with "0", is in a lower-performing school zone, while the school on the right-hand side (RHS) , represented by a red circle with "1", is in a high-performing school zone. Both share the boundary of interest, represented by the dark line. Housing sales (represented by orange circles) within 200, 400 and 600 metres radius of the boundary were extracted to form our sample. The choice of this radius was guided by previous studies, see e.g. Table 2.1 from applied studies where 153 and 610 metres were used as the minimum and maximum distances to the nearest boundary respectively. As mentioned in the introduction, the key assumption of the boundary fixed effect (BFE) strategy is that houses located near the boundaries and sufficiently close to one another share the same neighbourhood characteristics. So, houses located farther away on either side of the boundary are more likely to be dissimilar in those characteristics than houses close to the boundary. For example, Black (1999) restricted the sample to housing sales located within 250 metres of the boundary. Here, we vary the radius from 200, 400 and 600 metres to check the sensitivity of our results to the BFE key assumption. In Figure 2.1, houses located on the LHS of the boundary are part of the control group, while those located on the RHS of the boundary are in the treated group. Hereinafter, we refer to the 200 metre, 400 metre, 600 metre radius as '**Band 1**,' '**Band 2**', and '**Band 3**' respectively.

Figure 2.1: Data extraction

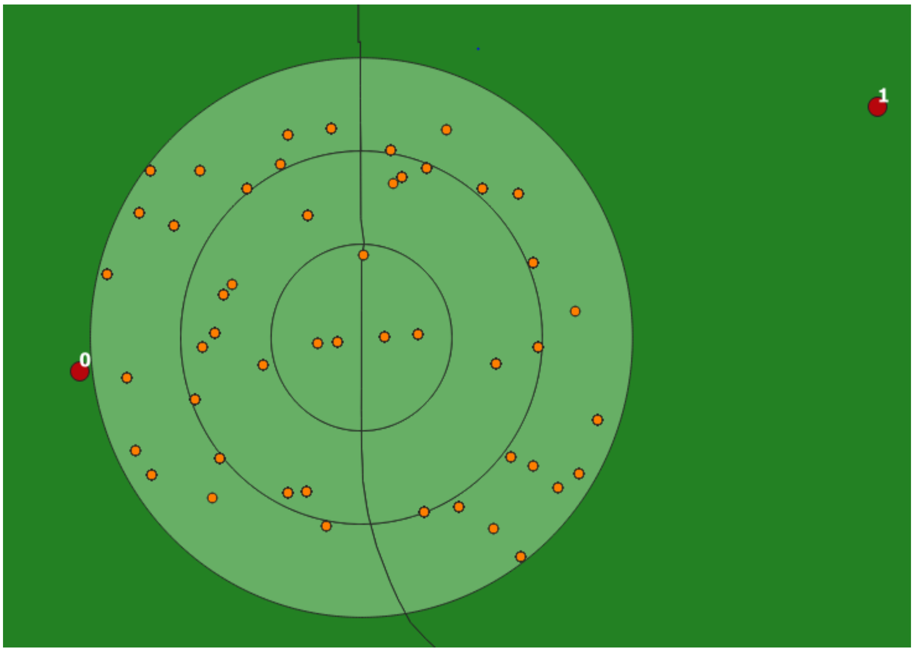


Table 2.1: Distance to nearest boundary– previous literature

Study	Max dist. to boundary (metres)
Ries and Somerville (2010)	250, 350, 500
Chiodo et al. (2010)	160
Dhar and Ross (2010)	305, 458, 610
Fack and Grenet (2010)	250, 300, 350
Dougherty et al.(2009)	245
Gibbons et al. (2009)	500*, 735*
Davidoff and Leigh (2008)	200, 500, 600
Bayer et al. (2007)	322
Kane et al. (2006)	153, 305, 610
Gibbons and Machin (2003)	250
Kane et al. (2003)	153, 305, 610
Black (1999)	242, 322, 564

Note: * indicates the average distance to the boundary and nearest matched property.

Summary statistics of the key variables used in the RDD analysis for the 200 metre radius (i.e., **Band 1**) are provided in Table 2.2. There are 2801 houses included in the sample for this band, covering 70 shared boundaries between lower-performing and high-performing school zones. The average log house price in this sample is about AU$13.91 (corresponding to an average house price of around AU$1.1 million) with a standard deviation of AU$0.398 (i.e., AU$530,000). The average land area within **Band 1** is approximately 557 square metres, with a standard deviation of 200 square metres. The average number of bedrooms in this band is about 3.6 bedrooms, with a standard deviation of 0.960. This average lies above the 3.1 bedroom average of the whole of NSW estimated by the Australian Bureau of Statistics (2018) between 2017 and 2018. On average, **Band 1** has 1.830 bathrooms and 1.871 parking spaces, with standard deviations of 0.785 and 0.941, respectively. The average lower bound of the log median suburb income is AU$11.603 (i.e., AU$109,425), with a standard deviation of AU$0.184 (i.e., AU$22,106).

Meanwhile, the average upper bound of the log median suburb income is about AU$11.871 (i.e., AU$143,057), with a standard deviation of AU$0.182 (i.e., AU$28,556). As such, the dispersion of the distribution in both income bounds are quite similar in **Band 1**.

Summary statistics of the complete list of variables (including the dummies) in **Bands 1 to 3** are presented in Tables A.1, A.3, and A.5 in Appendix A.

Table 2.2: Summary statistics – **Band 1**

	Mean	**Median**	**Std. Deviation**	**Min.**	**Max.**	**Obs.**
Log House Price	13.909	13.889	0.398	12.206	15.556	2801
Land Area	557.105	556	200.057	114	1183	2228
Bedrooms	3.455	3	0.960	1	9	2507
Baths	1.830	2	0.785	1	7	2487
Parking	1.871	2	0.941	1	12	2331
Median Suburb Income (LB)	11.603	11.552	0.184	11.082	12.112	2801
Median Suburb Income (UB)	11.794	11.775	0.182	11.264	12.245	2801

Median Income (LB) - the lower bound of median suburb income.

Median Income (UB) - the upper bound of median suburb income.

3 Empirical Strategy

We begin with the model specification in Section 3.1. Section 3.2 describes the estimation strategy.

3.1 Model specification

We consider the sharp regression discontinuity (RD) design setting (see Cattaneo et al., 2018). The unit of observation is '*house*' and the *outcome* (dependent) variable of interest is (log)-price of the house. Each house is located in a school zone, and schools are ranked through their overall state score– those above a certain known threshold $\bar{R}$ are considered 'high-performing,' while those below this threshold are considered 'lower-performing.' The treatment variable, T, is binary indicating whether a given house is located in a high-performing school zone, i.e., houses located in high-performing school zones are considered treated, while those in lower-performing school zones are considered not treated (control group). As such, the school overall state score represents the running variable in this setting, i.e., the treatment is determined by this score (Thistlethwaite and Campbell, 1960).

Let $\mathscr{Z}_1, \mathscr{Z}_2, \ldots, \mathscr{Z}_J$ denote the school zones for some finite integer J, and R_j be the overall state score of school j located in zone $\mathscr{Z}_j$. Also, let $\mathcal{A}_j$ denote the set of houses located in zone Z_j (i.e., we have $\underset{1\leq j\leq J}{\cup}\mathcal{A}_j = \{1, 2, \ldots, n\}$ and $\mathcal{A}_j \cap \mathcal{A}_k = \emptyset$ for all $j \neq k$), and $R_{ij} \equiv R_j$ be the common value of the running variable for all $i \in \mathcal{A}_j$. Then, for any $j \in \{1, 2, \ldots, J\}$, the treatment assignment rule for a given house $i \in \{1, 2, \ldots, n\}$ takes the form:

$$T_{ij} \equiv T_i(\mathcal{A}_j) = \mathbb{1}(R_{ij} \geq \bar{R}) = \begin{cases} 1 & \text{if } R_{ij} \geq \bar{R} \\ 0 & \text{if } R_{ij} < \bar{R}, \end{cases} \tag{3.1}$$

where $\mathbb{1}(\cdot)$ is the indicator function. This treatment assignment rule implies that the knowledge of the house location automatically determines whether it is in the treatment group or the control group; a key defining feature of any RDD– *the probability of treatment assignment as a function of the score changes discontinuously at the cut-off point.*

We are interested in identifying the causal treatment effect, i.e the effect of school zoning on house prices, within the potential-outcomes framework commonly used in the literature (see e.g. Heckman and Vytlacil, 2007; Imbens and Wooldridge, 2009). For this purpose, let $ln(p_{ijt}^{(1)})$ denote the potential outcome (i.e. log-price) of house i located in school zone j at time (in year) t under the treatment condition, and $ln(p_{ijt}^{(0)})$ its potential outcome without treatment (control group). The treatment effect is obtained by comparing the potential outcomes, $ln(p_{ijt}^{(1)})$ and $ln(p_{ijt}^{(0)})$. Note that even if house i is supposed to have both $ln(p_{ijt}^{(1)})$ and $ln(p_{ist}^{(0)})$, only one of these outcomes is observed in practice. For example, if house i were located in a high-performing school zone, one will only observe $ln(p_{ijt}^{(1)})$ and $ln(p_{ijt}^{(0)})$ will remain latent (unobserved). Similarly, if house i were located in a lower-performing school zone, one will observe $ln(p_{ijt}^{(0)})$ and $ln(p_{ijt}^{(1)})$ will be latent. In this setting, the observed outcome (log-price) of house i is expressed as:

$$ln(p_{ijt}) = (1 - T_{ij})ln(p_{ijt}^{(0)}) + T_{ij}ln(p_{ijt}^{(1)}) = \begin{cases} ln(p_{ijt}^{(0)}) & \text{if } R_{ij} < \bar{R} \\ ln(p_{ijt}^{(1)}) & \text{if } R_{ij} \geq \bar{R}. \end{cases} \tag{3.2}$$

Suppose that we observe a random sample $\{(p_{ijt}, T_{ij}, R_{ij}, X_{ijt})' : i = 1, \ldots, n; j = 1, \ldots, J; t = 1, \ldots, \mathcal{T}_0\}$, whereby X_{ijt} includes the observed house and neighbourhood (zones) characteristics. We are interested in the Sharp RD treatment effect, which is given under mild continuity conditions, by the estimand

$$\tau_{SRD} \quad = \quad \mathbb{E}\big[ln(p_{ijt}^{(1)}) - ln(p_{ijt}^{(0)}) \mid R_{ij} = \bar{R}\big] \tag{3.3}$$

that is often referred to as the average treatment effect at the cut-off, $\mathbb{E}[.]$ denotes the expectation with respect to a relevant probability measure $\mathbb{P}$. This parameter captures the treatment effect for units (houses) with score values $R_{ij} = \bar{R}$, i.e., it answers the question what would be the average log-price change for units with score level $R_{ij} = \bar{R}$ if we switched their status from control to treated? As such, τ_{SRD} is, by construction local in nature, i.e., it is not informative about the treatment effects at other levels of the score R_{ij} in the absence of additional assumptions. Also, Sharp RDD implies that all units (houses) with $R_{ij} = \bar{R}$ are treated, so τ_{SRD} is a local RD average treatment effect on the treated. Hahn et al. (2001) show that units with very similar values of the score but on opposite sides of the cut-off can be compared. In particular, among other conditions, if $\mathbb{E}\big[ln(p_{ijt}^{(1)}) \mid R_{ij} = R\big]$ and $\mathbb{E}\big[ln(p_{ijt}^{(0)}) \mid R_{ij} = R\big]$, seen as functions of R, are continuous at $R = \bar{R}$, then in the Sharp RDD considered above, it is the case that

$$\tau_{SRD} \quad = \quad \lim_{R \downarrow \bar{R}} \mathbb{E}\big[ln(p_{ijt}^{(1)}) \mid R_i = R\big] - \lim_{R \uparrow \bar{R}} \mathbb{E}\big[ln(p_{ijt}^{(0)}) \mid R_i = R\big], \tag{3.4}$$

which is nonparametrically identifiable; Hahn et al. (2001); Imbens (2004); McCrary (2008); Lee and Lemieux (2010).

In this study, we hypothesize that $\tau_{SRD} > 0$. Indeed, each public school in NSW has a designated intake zone and students residing within this zone are guaranteed admission. Therefore, one expects that parents seeking to guarantee a place for their child in a high-performing public school will pay a premium to secure a house within that high-performing school intake zone. As such, a house located in a high-performing school zone will, on average, have a higher price compared with a similar house in a lower-performing school zone.

In identifying this causal treatment effect, it is important to minimize any bias that may result from the unobserved (or confounding) house characteristics possibly correlated with school performance. To enable the controlling for these confounding factors, we have adopted the boundary discontinuity design (BDD) technique, focusing exclusively on the set of sales that take place in close proximity to, but on either side of, school attendance boundaries (Black, 1999). The boundaries of interest are those shared by high-performing (treated group) and lower-performing (control group) school zones. Therefore, our identification strategy is that houses close to, but on opposite sides of a common boundary share similar unobserved neighbourhood characteristics, i.e., these unobserved characteristics vary continuously at the boundary. The difficulty in assessing the validity of this strategy is that it is not possible to directly test for *continuity in the unobservable* characteristics. However, one can test for *continuity in the observed* characteristics of houses, and *continuity in the latter* suggests that the unobserved characteristics are also relatively unchanging (Cattaneo et al., 2018). If the observed characteristics were continuous and predetermined (i.e. determined before treatment assignment), then the difference in house prices are directly attributable to treatment. Continuity in observed covariates implies that the treatment effect on these covariates is zero at the cut-off. If this were the case, there should be no systematic difference between houses located near the boundaries between high-performing and lower-performing zones. Assessing this continuity assumption on covariates forms the basis of our first validity test in Section 5.1.

3.2 Estimation method

As discussed in Section 2, in the practical implementation of our joint RDD & BDD strategy, we use the assigned baseline cut-off point value of 91 (which is slightly above 90). Houses located in school zones with an overall state score equal to or greater than 91 are in the treated group, while those located in school zones with a score less than 91 form the control group. Given this information, we proceed with the graphical and statistical analyses.

Firstly, we explore evidence of a discontinuity at the cut-off through a preliminary analysis of the RD plots, as is often the case in most RDD applications. Using a global polynomial fit of order 4, the RD plots provide a smooth approximation of the overall shape of the estimated regression function for both the treated and control groups. Conceptually, RD plots approximate the estimated regression function by bins upon finding the mean of observations falling within each bin of the running variable (i.e., the score in this study), and plotting the average outcome in each bin against its mid-point. Combining all these bins allows one to visualise the overall shape of regression functions and provide information about the local behaviour of the data, thus enabling one to observe both the RD treatment effect and the variability of the data around the cut-off (Cattaneo et al., 2018). Due to the nature of our data set, all RD plots are run in quantile-spaced (QS) bins. QS bins are most appropriate as they contain approximately the same number of observations and are directly comparable in terms of data variability.

Secondly, we proceed by running clustered, naive and covariate adjusted regressions after this preliminary analysis of plots. The covariate adjusted regressions are run with the 6 key covariates, as well as all the 26 covariates. Although many of the 26 covariates violate the continuity assumption, the RD treatment effect estimate is qualitatively the same as that of the baseline 6 key covariates case, thus indicating some form of robustness of our results to the continuity assumption. In each case, a local linear RD treatment effect is estimated using uniform kernel weights with optimal bandwidth selected using the mean square error (MSE) criterion. As highlighted by (Cattaneo et al., 2018), unlike the global polynomial RD estimation, the local RD estimation avoids over-fitting the data, are less sensitive to outliers, and provide a good approximation of the RD treatment effect at boundary points. Furthermore, although higher order polynomials generally improve the accuracy of the approximation, they often deteriorate the efficiency of the estimate due to increased variability. We select the optimal bandwidths of equal length on each side of the cut-of using the MSE criterion based on the standard error produced in the naive estimation (i.e., the estimation no covariate). In Section 5.3, we check the sensitivity of the estimates to the bandwidth choice by selecting the optimal bandwidth using a criterion based on coverage errors rate (CER), and the results are qualitatively similar to those presented here. A uniform kernel was used to give equal weight to all observations within the interval, but the estimates are not generally sensitive to the choice of kernel (Cattaneo et al., 2018). Finally, given that the data was clustered in suburbs, employing variance estimators that are robust to the clustered nature of the data was necessary.

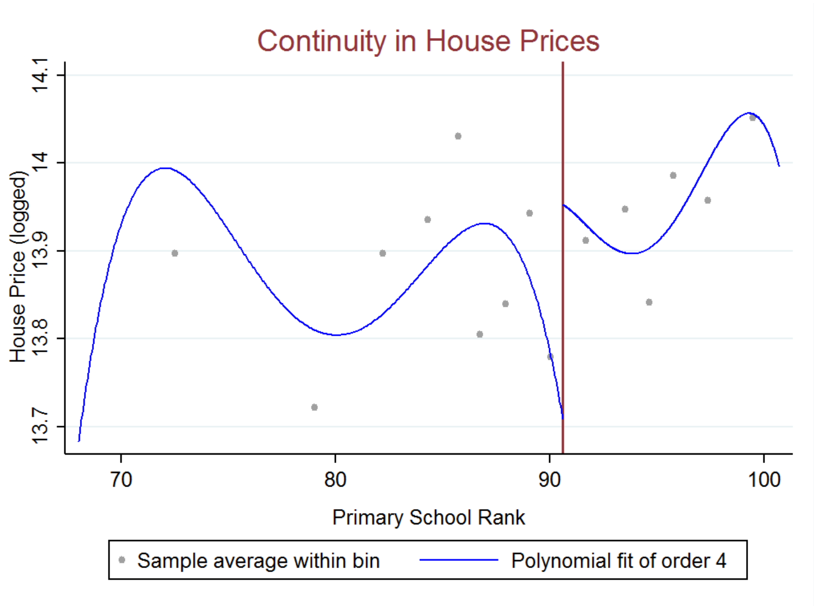
4 Results

We first present the main results for the 200, 400, and 600 metre bands respectively in Section 4.1. Section 4.2 extends to an assigned cut-off score of 95.

4.1 Main results

We begin with the the 200 metre band (**Band 1**). The RD plot of the outcome variable (log house price) for this band is shown in Figure 4.1. The LHS of the figure shows the plot for the control group, while the plot in the RHS is for the treated group. The figure provides preliminary evidence of a discontinuity in housing prices at the 91 cut-off points.

Figure 4.1: Continuity– **Band 1**



The RD estimate, i.e., the systematic difference in log housing prices between high-performing and lower-performing school zones is presented in Table 4.1. The first row presents the RD estimate with no control variables (Naive). The second row shows the estimate with 6 key control variables; *land area*, *bedrooms*, *bathrooms*, *parking*, *upper median income bound* and *lower median income bound*. Finally, the third row presents the RD estimate with 26 control variables, all of which are listed in Table A.2 in the Appendix. Column 3 shows the optimal Bandwidth selected by the MSE criterion. Columns 4 and 5 present the RD estimate and its standard error respectively. Columns 6 and 7 present the robust, bias corrected p-value and the 95% confidence interval of the RD estimate, respectively. Finally, column 8 presents the total number of effective observations used in the estimation.

Table 4.1: RD estimates– **Band 1**

					Robust Inference			
	Controls	MSE-Optimal Bandwidth	RD Estimate	Std. Error	P-Value	95% CI		Obs.
Naive	0	1.346	0.443***	0.111	0.000	0.283	0.793	353
Covariate Adjusted (1)	6	1.318	0.421***	0.151	0.003	0.167	0.813	210
Covariate Adjusted (2)	26	1.398	0.344***	0.122	0.002	0.153	0.715	229

* 10% significance level, ** 5% significance level, *** 1% significance level.
Covariate Adjusted (1): *land area*, *bedrooms*, *bathrooms*, *parking*, *lower median income bound*, and *upper median income bound*.
Covariate Adjusted (2): See Table A.2 in the appendix for the list of covariates.

With a positive treatment effect ranging from 0.344 to 0.443, each of the regressions outlined in Table 4.1 shows significance of the treatment at the 1% nominal level. First, excluding all control variables, the naive estimation generates a treatment effect of 0.443. Second, once 6 covariates are controlled for (land area, number of bedrooms, bathrooms, parking spaces and, upper and lower median income bounds), the RD estimate fell to 0.421, but is still significant at the 1% nominal level. The difference in point estimates between the two sets of regressions is relatively small, but the 95% confidence interval for the covariate-adjusted local polynomial RD estimation is wider. This may be due to a fall in the number of observations (353 to 210 observations) or, it may suggest that the inclusion of covariates was less efficient– i.e, it did not add to the accuracy of the estimation (Cattaneo et al., 2018). Finally, including all 26 control variables lowers the RD estimate compared to the naive and 6 covariate estimations. The RD estimate stands now at 0.344, and is significant at the 1% nominal level. The associated confidence bound is narrower than that with 6 covariates, however, given the uncertainty surrounding the APM's data collection process[1], it is possible that the estimate with 6 covariates is more reliable. In the data, the minimum and maximum logged house price in *Band 1* for lower-performing zone sample is AU$12.874 (i.e., AU$390,038.21 in level approximately) and AU$15.520 (i.e., AU$5,498,577.61 in level approximately), respectively. If houses within lower-performing school zones had instead relocated to the high-performing side of the boundary, an estimated average treatment effect of 0.421 (6 covariates case) translates to a housing price jump of 2.7% to 3.3%.[2]

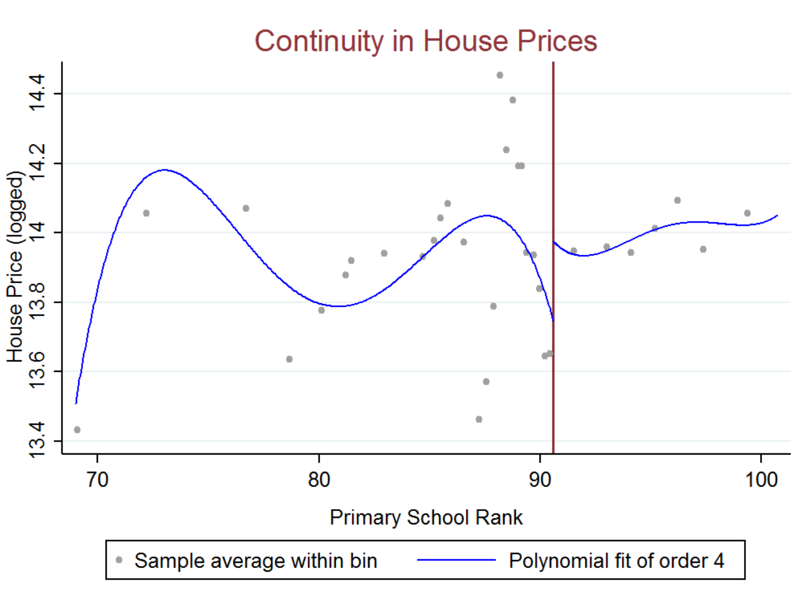
We now expand this analysis to include houses within 400 and 600 metres (*Band 2* and *Band 3*, respectively) of school attendance boundaries. With a radius of just 200 metres, *Band 1* ensures that neighbourhood characteristics on either side of the boundaries are relatively similar, supporting the boundary fixed effects (BFE) assumption of Black (1999). Although this assumption becomes less credible as we move further from the boundary, expanding the sample coverage area to *Band 2* and *Band 3* allows us to exploit more data, potentially enabling us to check the robustness of our results to the violation of the BFE assumption.

Considering *Band 2* (i.e., a 400 metre radius) first, the RD plot of housing prices for both the control (LHS of the figure) and treated (RHS of the figure) groups is shown in Figure 4.2. It illustrates a systematic difference between the price of houses located in lower-performing school zones and those located in high-performing school zones.

[1]The APM data set does not distinguish between a scenario where a property's features have not been recorded and a scenario where a property does not have a feature. For example, a property may have an air conditioner but it has not been recorded or, a property does not have an air conditioner - in both instances the APM data set reports this feature as missing.

[2]These percentages are obtained as $\frac{0.421}{15.520} \times 100 = 2.7\%$ and $\frac{0.421}{12.874} \times 100 = 3.3\%$.

Figure 4.2: Continuity– *Band 2*



Looking at this figure, the size of the discontinuity appears relatively small compared to that in Figure 4.1 (the 200 metre radius). Precisely, the RD estimates for **Band 2** are presented in Table 4.2 below.

Table 4.2: RD estimates– **Band 2**

	Controls	MSE-Optimal Bandwidth	RD Estimate	Std. Error	Robust Inference P-Value	Robust Inference 95% CI		Obs.
Naive	0	1.241	0.205**	0.111	0.039	0.023	0.920	554
Covariate Adjusted (1)	6	1.405	0.468**	0.174	0.012	0.118	0.944	489
Covariate Adjusted (2)	26	1.610	0.414**	0.166	0.040	0.020	0.880	634

* 10% significance level, ** 5% significance level, *** 1% significance level.
Covariate Adjusted (1): *land area*, *bedrooms*, *bathrooms*, *parking*, *lower median income bound*, and *upper median income bound*.
Covariate Adjusted (2): See Table A.4 in the appendix for the list of covariates.

As seen from the table, the RD estimates are positive and significant at the 5% nominal level. The naive estimation (i.e., when no covariate is accounted for) generates a treatment effect of 0.205. Once the 6 baseline covariates are controlled for, the estimated treatment effect increases to 0.468 and is significant at 2% (the p-value is 0.012). Finally, controlling for all 26 covariates results in an estimated RD treatment effect of 0.414 and is significant at 4% (the p-value is 0.040). Again, if we focus on the key 6 covariate case, the estimated treatment effect in **Band 2** is quite close to that in **Band 1** (see Table 4.1). Specifically, the RD estimate in **Band 2** is about 0.047 higher than that in **Band 1**. Note, however, that the significance of the estimated treatment effect has faded marginally in Table 4.2 (**Band 2**) compared with that in Table 4.1 (**Band 1**), due probably to decreased estimate precision (i.e., increased standard error estimates). These results indicate clearly that the treatment effect does not disappear as one moves from **Band 1** to **Band 2**, i.e., an additional 200 metres from the boundary. This finding aligns with Davidoff and Leigh (2007) which finds a significant change in the price of houses located within 200 and 500 metres of a school attendance boundary. Again, the minimum and maximum logged house price in the subsample of houses on the lower-performing side of the boundaries for **Band 2** are AU$12.931 (approximately AU$412,916.22) and AU$15.538 (approximately AU$5,598,448.14), respectively. Therefore, a treatment effect estimate of 0.468 translates to a housing price jump of 3% to 3.6%, from lower-performing school zone to high-performing school zone.

Let us now expand the analysis to *Band 3* (i.e., a 600 metre radius). Figure 4.3 below shows the RD plot of housing prices for both the control and treated groups. As seen, evidence of a discontinuity in housing prices at the cut-off is apparent. Again, this discontinuity appears smaller than that observed in **Band 1** (see Figure 4.1).

Figure 4.3: Continuity– **Band 3**

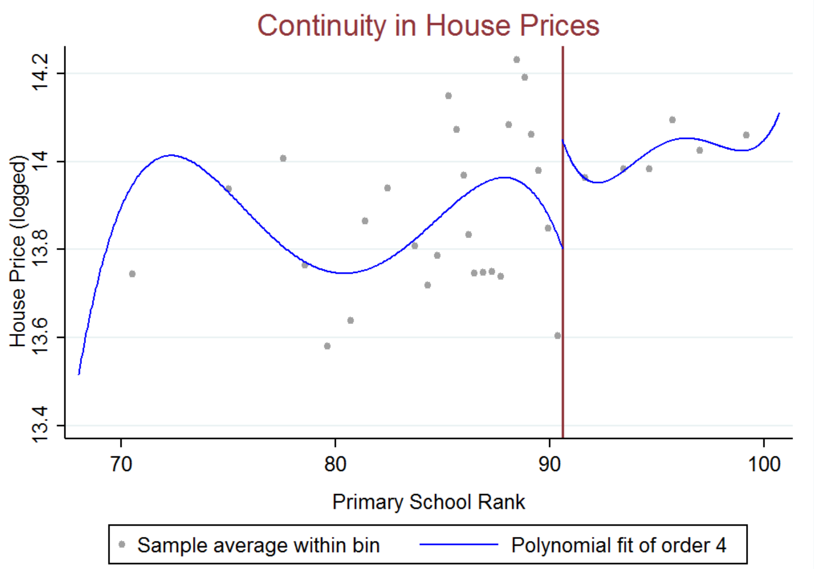


Table 4.3 below reports the associated RD estimates. The naive model (model without covariates) predicts an average treatment effect of 0.407, which is significant at the 1% nominal level. When 6 key covariates are controlled for, the estimated treatment effect increases to 0.413, and is significant at the 1% nominal level. However, this increase is only 0.008 with regards to that of the baseline naive model. Notably, the 95% confidence interval associated with the covariate adjusted estimation is narrower than that of the naive model. This generally implies that the covariates were successfully included within the estimation– i.e., they add to the accuracy of the estimation. The minimum and maximum logged house price in the subsample of

houses on the lower-performing side of the boundaries for **Band 3** are AU$12.502 (approximately AU$268,874.50) and AU$15.586 (approximately AU$5,873,727.51), respectively. Therefore, if a house within a lower-performing school zone had instead relocated to the high-performing side of the boundary, an estimated average treatment effect of 0.413 (i.e., the 6 key covariate case) translates to a house price jump of 2.6% to 3.3% in **Band 3**. This price range is slightly wider than the estimated price jump in **Band 1**. Both **Band 1** and **Band 3** estimate a maximum price jump of about 3.3% but **Band 3** predicts a minimum price jump about 0.1% lower than **Band 1**. This may suggest that as the distance from the boundary increases beyond 600 metres, the treatment effect begins to decline. Again, it is important to recall that the boundary fixed effects assumption is less credible as houses located further away from the boundary are included within the sample. As such, the estimated treatment effect obtained from **Band 3** controls for the unobserved neighbourhood characteristics to a lesser extent compared with that from **Band 1**.

Table 4.3: RD estimates– **Band 3**

	Controls	MSE-Optimal Bandwidth	RD Estimate	Std. Error	Robust Inference P-Value	Robust Inference 95% CI		Obs.
Naive	0	1.589	0.407***	0.143	0.002	0.181	0.789	1799
Covariate Adjusted (1)	6	1.277	0.413***	0.081	0.000	0.233	0.623	891
Covariate Adjusted (2)	26	1.172	0.398***	0.089	0.000	0.222	0.609	801

* 10% significance level, ** 5% significance level, *** 1% significance level.
Covariate Adjusted (1): *land area*, *bedrooms*, *bathrooms*, *parking*, *lower median income bound*, and *upper median income bound*.
Covariate Adjusted (2): See Table A.6 in the appendix for the list of covariates.

4.2 Increasing the assigned cut-off

To test the sensitivity of the results to the assigned cut-off score of 91, a secondary data set was constructed using a cut-off score of 95. Schools with an overall state score strictly less than 95 now form the lower-performing school zones subsample whilst schools scoring equal to or greater than 95 form that of the high-performing school zones. The housing sales within **Band 1, 2, 3** now share 83 boundaries between high- and lower-performing school zones. This data was extracted to produce the average RD treatment effect at the 95 cut-off. The results for the model with the 6 key covariates for the pooled 83 boundaries is shown in Table 4.4.

Table 4.4: All 83 boundaries at 95 cut-off

	MSE-Optimal Bandwidth	RD Estimate	Std. Error	Robust Inference P-Value	Robust Inference 95% CI		Obs.
Band 1	1.216	-0.003	0.102	0.778	-0.191	0.255	526
Band 2	1.509	0.020	0.070	0.946	-0.140	0.150	1724
Band 3	1.320	-0.028	0.079	0.566	-0.211	0.116	2903

Note: 6 key covariates are used– *land area*, *bedrooms*, *bathrooms*, *parking*, *lower median income bound*, and *upper median income bound*.

Looking at the table, the estimated RD treatment effect is not significant even at the 10% nominal level for all three bands. This may suggest that there is no evidence of a treatment effect when a cut-off of 95 is applied. However, because the RD treatment effect was positive and significant a the cut-off of 91, it is likely that there are many boundaries shared by schools with an overall state score between 91-94 and those with an overall state score above 95 in the pooled data. Therefore, classifying those schools (scoring 91 to 94) as lower-performing, and comparing them with schools above 95, as done in Table 4.4, is absorbing any treatment effect that may exist. To avoid boundaries where high-performing schools are compared with one another, we exclude all boundaries which have a lower-performing score between 91 and 94. This leaves us with just 31 out of the 83 overall boundaries. With distinction, lower-performing school zones now consist of schools with an overall state score strictly less than 91, and high-performing schools zones consist of schools with an overall state score equal to or greater than 95. The assigned cut-off of 95 is unchanged. The RD estimates for the three are presented in Table 4.5 below.

Table 4.5: RD estimates with the reduced sample at 95 cut-off

	MSE-Optimal Bandwidth	RD Estimate	Std. Error	Robust Inference P-Value	Robust Inference 95% CI		Obs.
Band 1	476.194	0.490***	0.111	0.001	0.179	0.725	66
Band 2	679.430	0.365	0.092	0.205	-0.118	0.553	346
Band 3	206.872	0.366***	0.116	0.005	0.127	0.699	219

* 10% significance level, ** 5% significance level, *** 1% significance level.
Note: 6 key covariates are used– *land area*, *bedrooms*, *bathrooms*, *parking*, *lower median income bound*, and *upper median income bound*.

From Table 4.5, the RD estimates for **Bands 1 and 3** are positive and significant at the 1%

nominal level. The estimated RD treatment effect in **Bands 1**, although not significant, is large– around 0.365. This is likely due to the very large bandwidth selected in this case.

In **Band 1**, for the subsample of houses located in lower-performing school zones, the minimum and maximum logged house price is AU$13.028 (approximately AU$454,976.02 in level) and AU$15.520 (approximately AU$5,498,577.61), respectively. A treatment effect of 0.490 therefore translates to a house price jump of 3.2% to 3.8% if a **Band 1** house in a low-performing school zone were exposed to treatment. In **Band 2**'s lower-performing sample, the minimum and maximum logged house price is 12.931 and 15.538, respectively. A treatment effect of 0.365 therefore translates to a house price jump of 2.3 to 2.8% in Band 2. Similarly, in **Bands 2**, a treatment effect of 0.365 translates to a house price jump of 2.3% to 2.8% from control to treatment, while this price jump is from 2.3% to 3.1% in **Bands 3**. These results support our previous findings in Section 4.1.

4.3 Discussion and policy implications

The analysis from the previous sections suggests found evidence of a positive RD treatment effect whether a cut-off of 91 or 95 is applied, as summarised in Table 4.6 below. For each band, the table presents the range of the estimated RD treatment effect at both the 91 and 95 cut-off.

Table 4.6: Summary of RD treatment effect estimates

	Treatment Effect (%)	
	91 Cut-Off	95 Cut-Off
Band 1	[2.7 , 3.3]***	[3.2 , 3.8]***
Band 2	[3.0 , 3.6]**	[2.3 , 2.8]
Band 3	[2.6 , 3.3]***	[2.3 , 3.1]***

*10% significance level, **5% significance level, ***1% significance level.

Consider the 91 cut-off first, we see that in **Band 1** a house located in a high-performing school zone is, on average, 2.7% to 3.3% more expensive than a similar house in a lower-performing school zone. This estimated treatment effect ranges between 3% to 3.6% for **Band 2** and between 2.6% to 3.3% for **Band 3**. Looking now at the 95 cut-off, the results are quite similar to that found for the 91 cut-off. For **Band 1**, a house located in a high-performing school zone is, on average, 3.2% to 3.8% more expensive than a similar house in a lower-performing school zone.

For **Band 2** and **Band 2**, this estimated treatment effect ranges between 2.3% to 2.8% and 2.3% to 3.1%, respectively.

Clearly, these results indicate a systematic difference in housing prices between high-performing and lower-performing school zones. This finding provides an insight into the price elasticity of demand for high quality education. It further provides existing and future property owners with an understanding of property valuations. Such an understanding may be pivotal in the financial planning of new families who wish to enrol their children into a high-performing public school. The capitalization of school performance into house prices also has important policy implications. By restricting access to high-performing schools predominantly to those who can afford to live in-zone, students from lower income households are being priced out of high quality education. This has the capacity to exacerbate educational inequality and further segregate high- and low-income families as they continually sort into high-performing and lower-performing school zones, respectively.

If a disparity in household income between high-performing and lower-performing school zones persists, more Australian schools may become socioeconomically disadvantaged.[3] Additionally, the resources that contribute to students' academic success, such as teacher quality, school funding, parents' social capital, and student peer characteristics, may become more unequally distributed since low-income areas presumably offer fewer resources than high-income areas. The link between school resources and school socioeconomic standing is outlined in the Programme for International Student Assessment (PISA), recently conducted by the OECD in 2015. According to PISA, the level of teacher resources available in Australian disadvantaged schools was significantly lower than the OECD average in 2015. Teachers in disadvantaged Australian schools were generally less qualified, less experienced and had higher rates of absenteeism. Moreover, Australia was one of the few OECD countries in which the student-to-teacher ratio was poorer in disadvantaged than in advantaged schools. With a clear lack of resources in disadvantaged schools, it is unsurprising that socioeconomically disadvantaged students in Australia were achieving at a level approximately three years behind their socioeconomically advantaged peers in 2015 and, 12% of the variation in student performance in financial literacy was associated with socio-economic status (Thomson et al., 2017).

The PISA findings highlight the gap in educational achievement between disadvantaged and advantaged Australian schools and, potentially, high- and low-income school zones. This gap may be amplified by reducing low-income households, who cannot afford housing premiums in high-performing school zones and presumably offer limited resources, to lower-performing

[3] Disadvantaged schools are defined as those schools in which the average socio-economic background of students is below the national average.

school zones. Since education is highly associated with future social and economic outcomes, the widening of this gap has the potential to heighten the cycle of disadvantage. Putting student education aside, the sorting of high-income households into high-performing school zones may further exacerbate the cycle of disadvantage since high-performing school zones generally have favourable neighbourhood qualities. These qualities include; proximity to employment, shopping and recreational conveniences (Kane et al., 2005).

The drawbacks of school zoning could perhaps be mitigated by allowing for additional considerations, other than residential address, in the enrolment of students. Or, what if school zoning was abolished in Australia? If so, access to high quality education should not be limited by family wealth and, students from disadvantaged backgrounds should have equal access to high-performing public schools. If abandoning school zoning is implausible, the Australian government should ensure that all Australian public schools are receiving the financial resources necessary to improve the educational outcomes of students in lower-performing (hence lower-income) school zones. As it stands, the provision of public funding for government and non-government schools is a heavily debated topic in Australia. Although public school enrolments outnumber private school ones annually, Australian public school students gained just $155 of public funding over the 2007 to 2017 period, while private school students were given $1429 of public funds (Goss, 2019). Allocating a greater proportion of public funds to public schools, especially those located in low socioeconomic areas[4], will likely enhance the educational outcomes of students from low-income households. The allocation of additional funds to low-performing public schools, which likely comprise of students from low-income households, should increase access to the resources which contribute to student academic success.

Drawing from the Japanese education system, strategies other than funding can be used to mitigate the negative effects of school zoning in Australia. Although Japan spends less on education than many other developed countries, Japan ranks highly among its peers in providing equal education to high- and low-income students (OECD, 2016). According to the OECD's 2015 PISA report, only 9%, as opposed to 12% for Australia, of the variation in student performance in Japan is explained by students' socioeconomic background (OECD, 2016; Thomson et al., 2017). The success of Japan's education system can be attributed to a number of factors. Firstly, Japan's teachers are allocated to schools by the local education authority. Under this system, teachers periodically change schools throughout their career and are given incentives to teach in

[4] Australian Bureau of Statistics (2016) ranks areas in New South Wales according to relative socio-economic advantage and disadvantage. Within this data set each suburb in NSW is assigned a decile number. The lowest 10% of suburbs are given a decile number of 1 and so on, up to the highest 10% of suburbs which are given a decile number of 10. Suburbs with a decile number of 1 are the most disadvantaged relative to the other deciles. Using this, we identified 9 suburbs in low-performing school zones that have a decile number strictly less than 6. Therefore, in terms of their level of disadvantage, these 9 suburbs are in the lowest 50% of suburbs in NSW.

disadvantaged schools. This 'career-based' system ensures that all schools have access to effective teachers and maintain a balance of experienced and less-experienced teachers. Secondly, Japan's teachers are paid more than the OECD average and the profession has high barriers to entry.

Introducing a career-based system in Australia and higher barriers to entry could potentially prevent high concentrations of inexperienced and less-qualified teachers in disadvantaged or low-income schools, potentially easing the educational inequalities associated with school zoning.

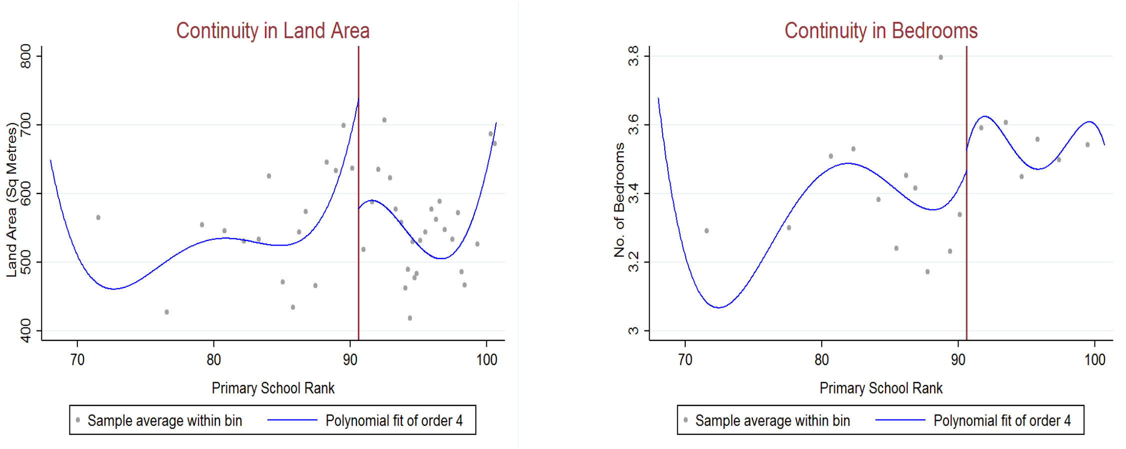
5 Falsification

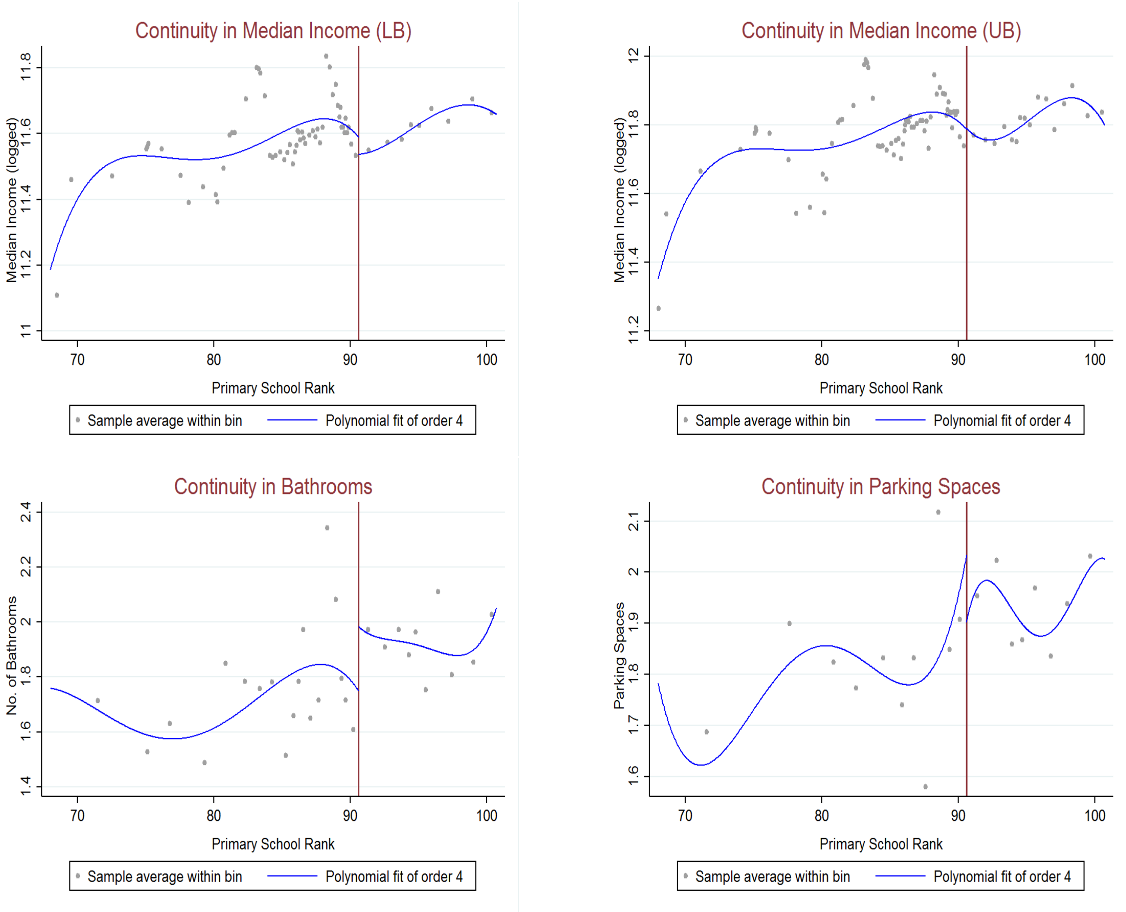
The section provides empirical tests validating the assumptions of the boundary and regression discontinuity design.

5.1 Continuity in predetermined covariates

The validity of any RD analysis relies on the discontinuity of the outcome variable (i.e. house prices) at the cut-off point. To ensure that this discontinuity is directly attributable to treatment, it is important that covariates which are correlated with the outcome of interest are continuous at the cut-off. To check the continuity of key covariates in our study, we present the RD plots of these key covariates. Figure 5.1 shows these plots for **Band 1**. The plots for **Band 2** and **Band 3** are presented in the Appendix in Figures A.1 and A.2 respectively.

Figure 5.1: Continuity in key covariates– **Band 1**





From these graphs, there is no obvious discontinuity in the upper median income bound at the cut-off. However, a discontinuity in the land area, number of bathrooms, parking spaces, the lower bound of median income, and in some sense the number of bedrooms, is apparent. We conduct statistical tests to confirm these facts.

A different curvature and overall shape of each covariate can be observed through Figure 5.1, implying that the estimated regression function for each covariate differs. As such, the optimal Bandwidth used to estimate continuity in covariates will differ for each variable and must be re-estimated in each case (Cattaneo et al., 2018). For this reason, we conduct statistical analysis for each covariate separately. We estimate a local linear RD treatment effect with uniform kernel weights and MSE-optimal Bandwidths. To shorten the presentation, here we present the results for the key covariates in Table 5.1 below. The RD treatment effect estimates for all 26 covariates are given in Table A.2 in the Appendix for **Band 1**, and in Tables A.4 & A.6 for **Band 2** and **Band 3**, respectively.

Table 5.1: Continuity of key covariates– **Band 1**

	RD Estimate	Robust P-Value
Land Area	-191.740	0.000***
Bedrooms	0.605	0.034**
Bathrooms	0.772	0.000***
Parking Spaces	0.079	0.822
Median Income (LB)	0.053	0.045**
Median Income (UB)	0.023	0.274

*10% significance level, **5% significance level, ***1% significance level.

As seen from the table, the RD estimate is strongly significant for land area and bathrooms (with robust p-values of 0.000). The estimate is also significant at the 5% nominal level for number of bedrooms and the lower bound of median income. Only the estimates of parking spaces and the upper bound of median income are not statistically significant - even at the 10% nominal level (p-value of 0.822 and 0.274 respectively). Therefore, there is strong evidence of a systematic difference at the cut-off in land area, number of bathrooms, number of bedrooms, and the lower bound of median income. This means that these covariates do not pass the continuity test at the cut-off. However, both parking spaces and the upper bound of median income are continuous at the cut-off. Continuity in these covariates at the cut-off may suggest that unobservable house and neighbourhood characteristics are also relatively unchanging.

These results are similar to Black (1999); Kane et al. (2005); Bayer et al. (2007), who also found systematic differences in house or neighbourhood characteristics on the high- and lower-scoring side of school boundaries. These differences likely reflect residential sorting whereby higher income households sort onto the performing side of the boundary. Nevertheless, each of the aforementioned studies included variables that were significantly different on opposing sides of school attendance boundaries because their omission could lead to upward bias (Black, 1999). Furthermore, although discontinuity in covariates typically may cast doubt on the assumptions underlying the RD design, it does not necessarily invalidate the approach (Imbens and Lemieux, 2008). For these reasons, we include all covariates in our analysis. It is also important to mention that the statistical significance of the RD treatment effect estimate is unchanged when these discontinuous covariates are left out of the analysis, and it persists even if no covariate is used (see the naive estimates in Tables 4.1-4.3).

5.2 Sensitivity to observations near the cut-off

Following Cattaneo et al. (2018), this falsification approach determines whether a systematic manipulation of the score value has taken place. Manipulation, if present, would generally involve the observations close to the cut-off. By excluding these observations and repeating the RD estimation, one can test for manipulation in the running variable (here the score). Furthermore, this approach also provides a test to assess the sensitivity of the results to the extrapolation inherent in local polynomial estimation (Cattaneo et al., 2018). Indeed, since the observations close to the cut-off are the most influential when fitting the local polynomials, excluding them would mean that one is testing not only for manipulation, but also for result sensitivity to the removal of observations closer to the cut-off.

To run this test, we exclude houses located in school zones with an overall state score of 91 and re-estimate the RD treatment effect. Both the control group and the 91 cut-off score remain unchanged, while the treatment group now consists of houses in school zones with scores strictly above 91. As observations near the boundaries have been eliminated, the continuity in covariates at the cut-off is no longer guaranteed, so we shall focus on the baseline naive RD treatment effect estimates. To shorten the presentation, we show the RDD estimate for **Band 1** but our results are qualitatively the same for the other bands. Table 5.2 below contains the result. The first row of the table reports the previous naive RDD estimates in Table 4.1, whereas the second row shows the new naive estimate. As seen, the RD treatment effect estimated with the new data is about 0.975 and is significant at 5% nominal level. Although this estimate is approximately double that of the original estimation (row on the bottom part of the table), both are positive and highly significant, rejecting the null hypothesis of no systematic difference in prices between high- and lower- performing school zones. Clearly, our findings in Section 4 remain qualitatively unchanged when observations close to the cut-off are excluded. As such, the results are not overly sensitive to observations near the cut-off.

Table 5.2: Testing for manipulation in the score– **Band 1**

	MSE-Optimal Bandwidth	RD Estimate	Std. Error	Robust Inference P-Value	95% CI		Obs.
Original	1.346	0.443***	0.111	0.000	0.283	0.793	353
Newly Adjusted	2.276	0.975**	0.415	0.021	0.165	2.008	470

Note: *10% significance level, **5% significance level, ***1% significance level.

5.3 Sensitivity to bandwidth choice

In this section, we analyse the sensitivity of our results to the bandwidth choice. Instead of removing observations near the cut-off, this test adds or removes observations at the endpoints. We perform the test with the key covariates and use **Band 1** housing sales. The results for **Band 2** and **Band 3** are qualitatively the same as those shown here, therefore are omitted to shorten the presentation. Table 5.3 below compares the RD estimate generated when using the mean squared errors (MSE) criterion with that obtained using the coverage error rates (CER) criterion. As seen from Table 5.3, the results are quite similar. Using the MSE-optimal bandwidth resulted in an RD treatment effect estimate of about 0.421, while the one from CER-optimal bandwidth is around 0.426. Both are significant at that 1% nominal level. This suggests that our results are not very sensitive to the choice of the bandwidth.

Table 5.3: Sensitivity to the bandwidth choice– **Band 1**

	Bandwidth	RD Estimate	Std. Error	Robust Inference P-Value	95% CI		Obs.
MSE Optimal Bandwidth	1.318	0.421***	0.151	0.003	0.167	0.813	210
CER Optimal Bandwidth	1.043	0.426***	0.120	0.000	0.226	0.722	159

* 10% significance level, ** 5% significance level, *** 1% significance level.
Note: 6 key covariate used– *land area*, *bedrooms*, *bathrooms*, *parking spaces*, *lower median income bound*, and *upper median income bound*.

6 Conclusion

In this paper, we use comprehensive data on housing transactions from January 2014 to March 2019 to investigate the existence of a house price differential between high-performing and lower-performing school zones in New South Wales (Australia). Using the RD treatment setting, we find that residing in a high-performing primary school zone results in paying, on average, a premium on housing of approximately 2.7% to 3.3% compared with residing in a lower-performing school zone. This result is robust to a number of sensitivity checks, and is in line with that of existing studies in the US and UK. Given the median house price of $870 000 in New South Wales in March 2019 (Australian Bureau of Statistics, 2019), our RD treatment effect estimate suggests that the price of a house located on the high-performing side of a school boundary is on average $23,490 to $28,710 higher than that of a similar house located on the lower-performing side of the boundary.

It is worth noting that school attendance boundaries provided to us by the New South Wales Department of Education date back to before January 2014. However, the construction of new schools and the expansion of existing schools between January 2014 and March 2019 may have caused a shift in school zoning. In addition, due to data limitation, we could not directly address issues related to sorting into neighbourhood, whereby high-income families tend to cluster around high-performing school zones. Moreover, although omitted variable bias has been relatively mitigated through the adoption of the boundary discontinuity design technique, it may not be ruled out completely. Despite these limitations, our findings provide home owners, parents and policy-makers with a useful insight into the capitalization of primary school performance into housing prices in Australia, and New South Wales in particular.

Appendices

A Descriptive Statistics

Table A.1: Summary statistics– **Band 1** (91 cut-off)

	Mean	Median	Std. Deviation	Min.	Max.	Obs.
Key Variables						
House Price	13.909	13.889	0.398	12.206	15.556	2801
Land Area	557.105	556	200.057	114	1183	2228
Bedrooms	3.455	3	0.960	1	9	2507
Baths	1.830	2	0.785	1	7	2487
Parking	1.871	2	0.941	1	12	2331
Median Suburb Income (LB)	11.603	11.552	0.184	11.082	12.112	2801
Median Suburb Income (UB)	11.794	11.775	0.182	11.264	12.245	2801
Dummy Variables						
Air Conditioning	0.431	0	0.495	0	1	2801
Alarm	0.132	0	0.339	0	1	2801
Balcony	0.187	0	0.390	0	1	2801
Barbeque	0.065	0	0.247	0	1	2801
Courtyard	0.134	0	0.340	0	1	2801
Ensuite	0.317	0	0.465	0	1	2801
Family Room	0.019	0	0.135	0	1	2801
Fireplace	0.122	0	0.327	0	1	2801
Garage	0.078	0	0.269	0	1	2801
Heating	0.124	0	0.330	0	1	2801
Internal Laundry	0.194	0	0.396	0	1	2801
Lockup Garage	0.187	0	0.390	0	1	2801
Polished Timber Floor	0.145	0	0.352	0	1	2801
Pool	0.056	0	0.229	0	1	2801
Rumpus Room	0.098	0	0.298	0	1	2801
Separate Dining	0.151	0	0.358	0	1	2801
Spa	0.041	0	0.198	0	1	2801
Study	0.218	0	0.413	0	1	2801
Sunroom	0.047	0	0.212	0	1	2801
Walk-in Wardrobe	0.084	0	0.277	0	1	2801

Table A.2: Covariate Check– **Band 1** (91 cut-off)

	RD Estimate	**Robust P-value**
Key Covariates		
Land Area	-191.740***	0.000
Bedrooms	0.605**	0.034
Bathrooms	0.772***	0.000
Parking Spaces	0.079	0.822
Median Income (LB)	0.053**	0.045
Median Income (UB)	0.023	0.274
Dummy Covariates		
Air Conditioning	0.605**	0.034
Alarm	0.772***	0.000
Balcony	0.079	0.822
Barbeque	0.053**	0.045
Courtyard	0.023	0.274
Ensuite	-0.320***	0.00
Family Room	0.048	0.544
Fireplace	-0.004	0.255
Garage	0.056	0.892
Heating	0.030	0.632
Internal Laundry	0.150	0.227
Lockup Garage	0.026	0.333
Polished Timber Floor	0.052	0.581
Pool	-0.021	0.780
Rumpus Room	-0.127	0.137
Separate Dining	-0.017	0.971
Spa	-0.042	0.529
Study	0.060	0.605
Sunroom	-0.022	0.508
Walk-in Wardrobe	-0.065	0.271

*10% significance level, **5% significance level,***1% significance level.

Table A.3: Summary statistics– **Band 2** (91 cut-off)

	Mean	**Median**	**Std. Deviation**	**Min.**	**Max.**	**Obs.**
Key Variables						
House Price	13.965	13.940	0.448	7.258	15.556	5089
Land Area	568.885	560	204.246	127	1195	4176
Bedrooms	3.566	3	0.999	1	12	4515
Baths	1.932	2	0.849	1	9	4492
Parking	1.928	2	0.909	1	12	4223
Median Suburb Income (LB)	11.640	11.552	0.197	11.082	12.245	5089
Median Suburb Income (UB)	11.823	11.775	0.188	11.264	12.363	5089
Dummy Variables						
Air Conditioning	0.446	0	0.497	0	1	5089
Alarm	0.006	0	0.075	0	1	5089
Balcony	0.204	0	0.403	0	1	5089
Barbeque	0.070	0	0.254	0	1	5089
Courtyard	0.128	0	0.334	0	1	5089
Ensuite	0.339	0	0.473	0	1	5089
Family Room	0.019	0	0.135	0	1	5089
Fireplace	0.120	0	0.325	0	1	5089
Garage	0.093	0	0.290	0	1	5089
Heating	0.130	0	0.336	0	1	5089
Internal Laundry	0.193	0	0.395	0	1	5089
Lockup Garage	0.189	0	0.391	0	1	5089
Polished Timber Floor	0.149	0	0.356	0	1	5089
Pool	0.071	0	0.256	0	1	5089
Rumpus Room	0.117	0	0.321	0	1	5089
Separate Dining	0.155	0	0.362	0	1	5089
Spa	0.046	0	0.210	0	1	5089
Study	0.231	0	0.422	0	1	5089
Sunroom	0.048	0	0.213	0	1	5089
Walk-in Wardrobe	0.103	0	0.303	0	1	5089

Figure A.1: Continuity in key covariates– **Band 2** (91 cut-off)

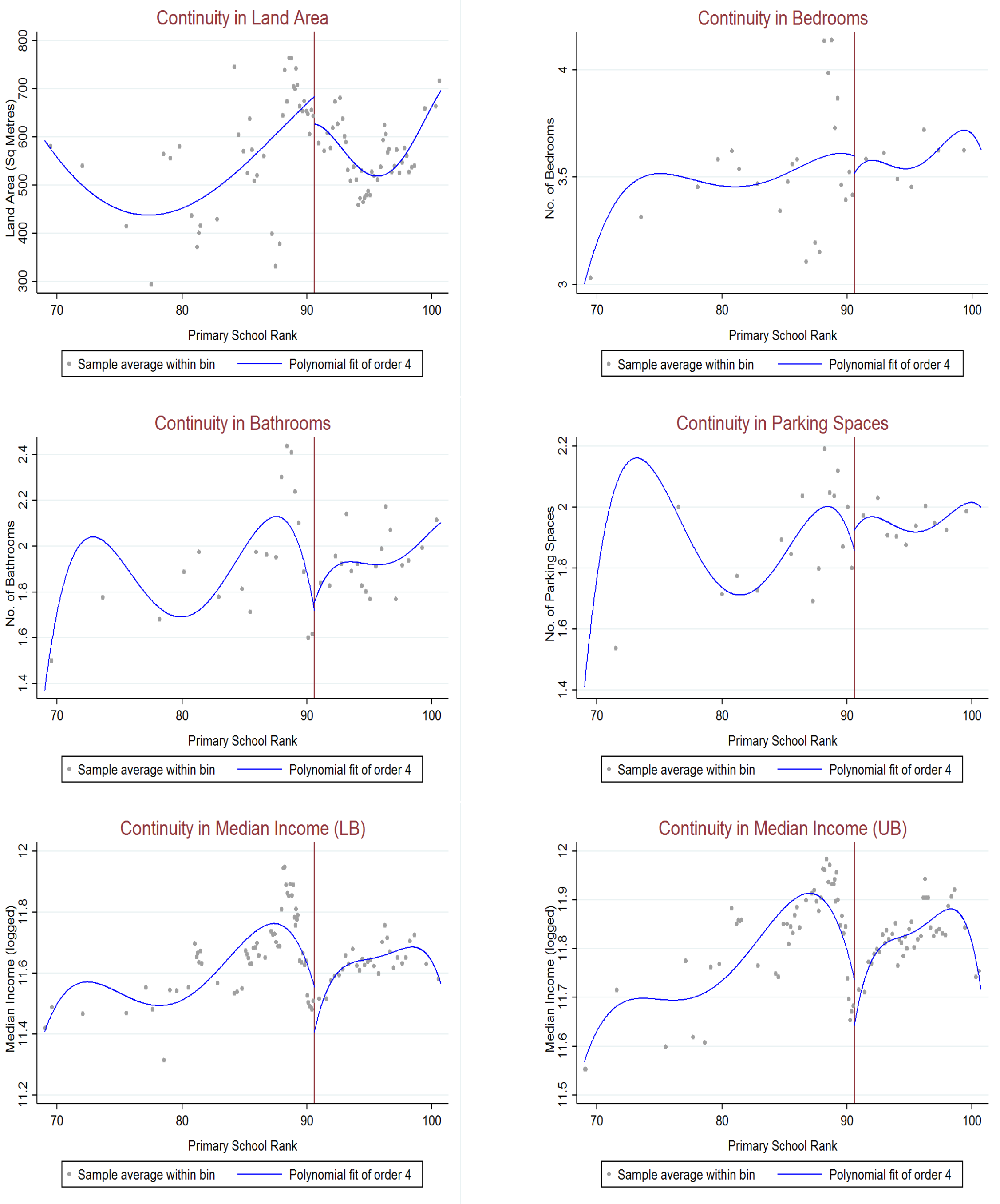


Table A.4: Covariate Check– **Band 2** (91 Cut-Off)

	RD Estimate	**Robust P-value**
Key Covariates		
Land Area	-33.093	0.333
Bedrooms	0.098	0.420
Bathrooms	0.315*	0.088
Parking Spaces	-0.085	0.802
Median Income (LB)	0.055	0.253
Median Income (UB)	0.030	0.308
Dummy Covariates		
Air Conditioning	-0.282***	0.005
Alarm	0.015	0.697
Balcony	-0.089*	0.095
Barbeque	-0.031	0.300
Courtyard	-0.023	0.660
Ensuite	0.073	0.571
Family Room	-0.085***	0.000
Fireplace	-0.003	0.880
Garage	-0.105*	0.054
Heating	-0.106**	0.049
Internal Laundry	0.190**	0.013
Lockup Garage	0.168**	0.023
Polished Timber Floor	-0.021	0.558
Pool	-0.067**	0.028
Rumpus Room	-0.070	0.172
Separate Dining	0.150***	0.005
Spa	0.010	0.696
Study	0.112*	0.071
Sunroom	0.020	0.575
Walk-in Wardrobe	0.102**	0.024

*10% significance level, **5% significance level,***1% significance level.

Table A.5: Summary Statistics– **Band 3** (91 Cut-Off)

	Mean	Median	Std. Deviation	Min.	Max.	Obs.
Key Variables						
Dwelling Price	13.944	13.914	0.430	7.258	15.586	12853
Median Suburb Income (LB)	11.617	11.552	0.193	10.621	12.245	12835
Median Suburb Income (UB)	11.802	11.775	0.186	10.859	12.363	12835
Land Area	562.647	557	197.054	82	1197	10569
Bedrooms	3.536	3	0.979	1	14	11478
Baths	1.882	2	0.834	1	9	11419
Parking	1.907	2	0.901	1	13	10743
Dummy Variables						
Air Conditioning	0.448	0	0.497	0	1	12853
Alarm	0.006	0	0.080	0	1	12853
Balcony	0.198	0	0.399	0	1	12853
Barbeque	0.069	0	0.253	0	1	12853
Courtyard	0.121	0	0.327	0	1	12853
Ensuite	0.319	0	0.466	0	1	12853
Family Room	0.020	0	0.141	0	1	12853
Fireplace	0.112	0	0.316	0	1	12853
Garage	0.096	0	0.295	0	1	12853
Heating	0.123	0	0.328	0	1	12853
Internal Laundry	0.190	0	0.392	0	1	12853
Lockup Garage	0.188	0	0.391	0	1	12853
Polished Timber Floor	0.149	0	0.356	0	1	12853
Pool	0.067	0	0.251	0	1	12853
Rumpus Room	0.116	0	0.320	0	1	12853
Separate Dining	0.157	0	0.364	0	1	12853
Spa	0.044	0	0.205	0	1	12853
Study	0.228	0	0.419	0	1	12853
Sunroom	0.047	0	0.211	0	1	12853
Walk-in Wardrobe	0.103	0	0.304	0	1	12853

Figure A.2: Continuity in key covariates– **Band 3** (91 cut-off)

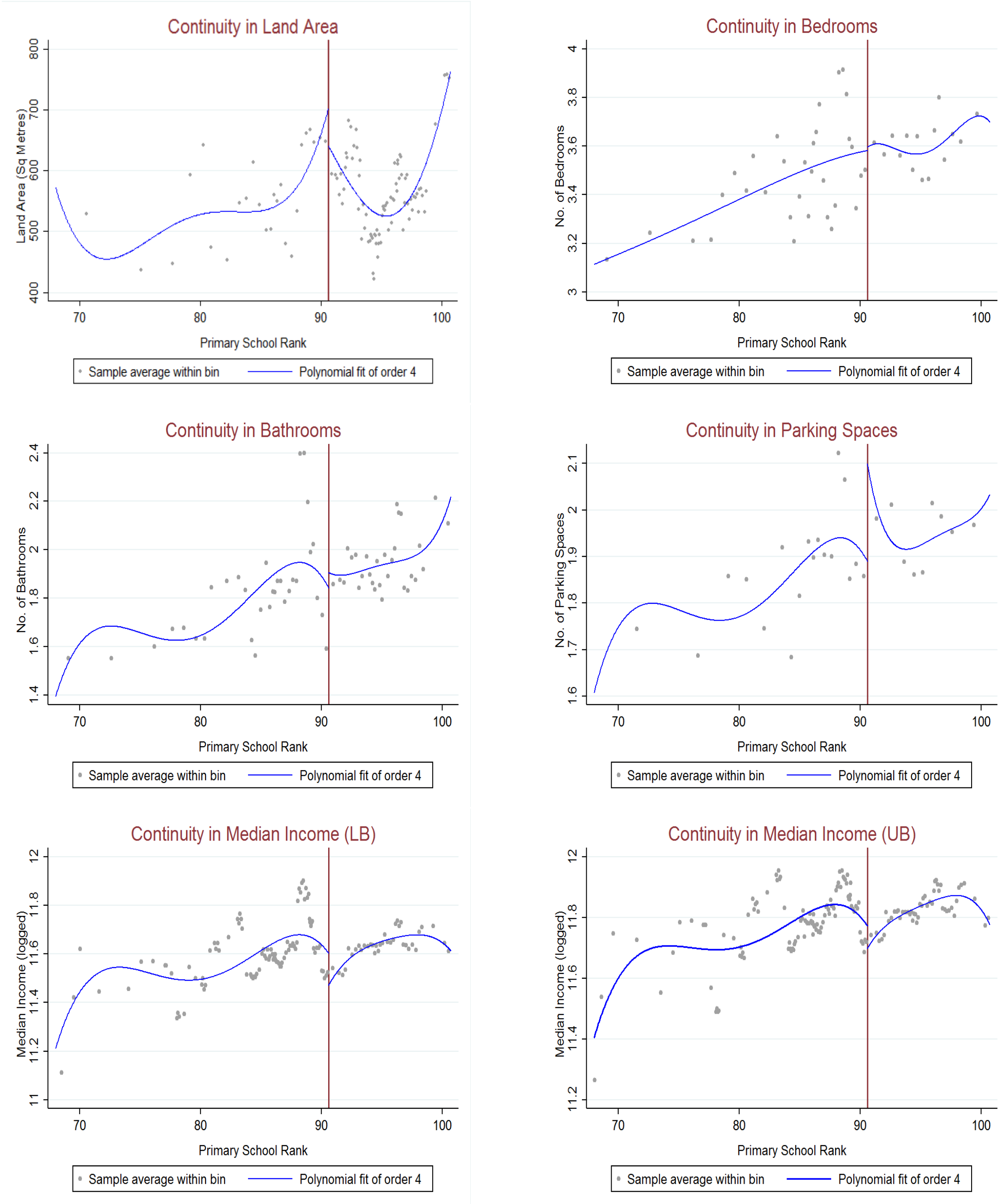


Table A.6: Covariate Check– **Band 3** (91 Cut-Off)

	RD Estimate	**Robust P-value**
Key Covariates		
Land Area	-106.350***	0.000
Bedrooms	0.200	0.101
Bathrooms	0.247***	0.007
Parking Spaces	0.160*	0.077
Median Income (LB)	0.091*	0.058
Median Income (UB)	0.100**	0.021
Dummy Covariates		
Air Conditioning	-0.174**	0.039
Alarm	0.014	0.503
Balcony	-0.013	0.959
Barbeque	0.008	0.834
Courtyard	-0.036	0.454
Ensuite	0.046	0.715
Family Room	-0.058***	0.000
Fireplace	-0.004	0.782
Garage	-0.059**	0.050
Heating	-0.049	0.099
Internal Laundry	-0.011	0.858
Lockup Garage	0.117**	0.024
Polished Timber Floor	0.006	0.776
Pool	0.004	0.524
Rumpus Room	-0.038	0.542
Separate Dining	0.146***	0.000
Spa	-0.035*	0.069
Study	0.015	0.606
Sunroom	0.031	0.164
Walk-in Wardrobe	-0.003	0.862

*10% significance level, **5% significance level,***1% significance level.

B RD estimates

Table B.1: RD Estimates– **Bands 1-3** (91 Cut-Off)

	Controls	MSE-Optimal Bandwidth	RD Estimate	Std. Err	Robust Inference P-value	95% CI		Obs.
Band 1								
Naive	0	1.346	0.443***	0.111	0.000	0.283	0.793	353
Covariate Adjusted (1)	6	1.318	0.421***	0.151	0.003	0.167	0.813	210
Covariate Adjusted (2)	26	1.398	0.344***	0.122	0.002	0.153	0.715	229
Band 2								
Naive	0	1.241	0.205**	0.111	0.039	0.023	0.920	554
Covariate Adjusted (1)	6	1.405	0.468**	0.174	0.012	0.118	0.944	489
Covariate Adjusted (2)	26	1.610	0.414**	0.166	0.040	0.020	0.880	634
Band 3								
Naive	0	1.589	0.407***	0.143	0.002	0.181	0.789	1799
Covariate Adjusted (1)	6	1.277	0.413***	0.081	0.000	0.233	0.623	891
Covariate Adjusted (2)	26	1.172	0.397***	0.089	0.000	0.222	0.609	801

*10% significance level, **5% significance level,***1% significance level.
Note 1: Covariate Adjusted (1) includes 6 control variables - land area, number of bedrooms, bathrooms, parking spaces and, upper and lower bounds of median income.
Note 2: Covaraite Adjusted (2) includes 26 control variables - see list of variables in Table A.06 above.

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