Place, Peers, and the Teenage Years: Long-Run Neighborhood Effects in Australia†

By Nathan Deutscher*

I use variation in the age at which children move to show that where an Australian child grows up has a causal effect on their adult income, education, marriage, and fertility. In doing so, I replicate the findings of Chetty and Hendren (2018a) in a country with less inequality, more social mobility, and different institutions. Across all outcomes, place typically matters most during the teenage years. Finally, I provide suggestive evidence of peer effects using cross-cohort variation in the peers of permanent postcode residents: those born into a richer cohort for their postcode tend to end up with higher incomes themselves. (JEL D63, J13, J62, R23, Z13)

A new wave of studies highlights that where a child grows up influences their adult income, education, and social outcomes (Chetty and Hendren 2018a; Chetty, Hendren, and Katz 2016; Chyn 2018; Damm and Dustmann 2014). Previously, correlations in the outcomes of neighboring children sat alongside more tenuous evidence for an underlying causal relationship. Yet for policymakers, knowing when and why place matters can be just as important as knowing if place matters. This paper contributes to this literature by studying place effects in a new setting and across childhood, and by beginning to examine potential transmission mechanisms.

I find that where an Australian child grows up has a causal effect on their adult outcomes, but that place typically matters most during the teenage years. In doing so, I replicate the approach of Chetty and Hendren (2018a) by establishing causal effects of exposure to place in a country with less inequality, more social mobility, and different institutions. I extend their work, highlighting the relative importance of the teenage years.

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years, using the longer childhood histories available in de-identified Australian intergenerational tax data. Finally, I provide suggestive evidence of long-lasting peer effects, which provide both a potential mechanism for causal place effects and a potential explanation for the sensitivity of the teenage years.

I begin with the empirical framework introduced in Chetty and Hendren (2018a). I use variation in the age at which children move within Australia to identify the causal effect of exposure to place. This methodology avoids the selection bias inherent in simple comparisons between those choosing to live in different places or move between them. The key identifying assumption is that selection effects do not vary with the age at which children move. While this is a strong assumption, the main results hold under a variety of validation exercises also presented in Chetty and Hendren (2018a). Most notably, the outcomes of those moving converge to those of their destination in a way that reflects cohort- and gender-specific differences, and differences in not just the average but also the distribution of outcomes. To explain the results, any omitted variable would need to be correlated with the age at which children move and very specific features of the outcomes later realized by those in the destination.

The ability to observe children moving from infancy onward, and hence detect when place matters most, is the key advantage of the Australian data. Before age 11, the expected income ranks of children who move converge to those of the permanent residents of their destination at a rate of around 1.1 percent for each year spent in the destination. After age 11, this rate of convergence is around 4.2 percent. There is some evidence of heterogeneity in this pattern of exposure effects, with no significant difference in these rates of convergence between early and late childhood when looking only at those born into the second and third quintiles of the parent income distribution. This is consistent with place effects that reflect a range of underlying mechanisms, which matter to differing extents for children from different backgrounds.

The tendency for place to matter most during the teenage years has implications for the interpretation and use of the estimates in the work of Chetty and Hendren (2018a, b). As the authors noted, their estimates, by necessity, relied on incomplete child histories, which captured moves from age 9 onward. My findings suggest that naively extrapolating back from their estimates could overstate the effect of place. In the Australian setting, a child who moved at birth to a place with 1 percentile point higher incomes for permanent residents would receive around 70 percent of that effect themselves. Extrapolating back the exposure effects observed from age 9 onward yields a higher (biased) estimate of around 100 percent. On the other hand, the causal effects of specific places identified in Chetty and Hendren (2018b) could possibly capture the period in which location matters most—providing more comfort that similar effects would be seen with complete childhood histories.

Place also appears to matter—and, on average, it matters most in the teenage years—for a range of other outcomes. I examine university

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1 Chetty and Hendren (2018a) use outcomes at age 24 for the estimation of the average causal effects of place, which limited them to moves from age 9. They use outcomes at age 26 for the estimation of the causal effects of specific places, which limited them to moves from age 11. For earlier outcomes, they captured moves at earlier ages.
attendance, marriage, fertility, and living in a major urban area as alternative outcomes. These outcomes are of interest in their own right, as they reflect substantial differences in peoples’ lives. They also address concerns that differences in incomes may simply reflect differences in local price levels, and therefore may not reflect a difference in underlying well-being. While exposure to place matters for all these outcomes, what differs is the implied extent to which observed differences across places are causal. Almost all of the difference (over 90 percent) in propensity to live in a major urban area appears to reflect the effect of place itself rather than a propensity that follows children when they move. Conversely, most (but not all) of the differences between places in university attendance, marriage, and fertility appear to reflect the characteristics of those who live there.

Finally, despite Australia having less geographic variation in intergenerational mobility, this paper suggests causal differences that are economically meaningful. For example, if a place with poor outcomes (tenth percentile by region) for children born into the twenty-fifth percentile of the parent income distribution could be improved to mimic a place with typical outcomes (fiftieth percentile), then children would receive around an additional AUS$2,800 (US$2,000) a year in total household income at age 24. Naturally, knowing how much of this may be amenable to policy requires a deeper understanding of the underlying causal mechanisms.

The pronounced sensitivity of the teenage years complements the existing human capital literature, which has long identified the importance of early childhood (e.g., Case, Fertig, and Paxson 2005; Cunha and Heckman 2007; Currie and Almond 2011). For example, shocks to fetal health and targeted policy interventions in early childhood have been found to have long-lasting consequences. This paper finds that the causal effects of the places considered here are often most potent later in childhood—perhaps reflecting differences between these mechanisms versus those most frequently examined in prior literature. The paper concludes by examining one such mechanism that may result in a causal effect of place and explains the importance of adolescence.

One reason place may matter is because it influences who you grow up with: your peers and role models. To provide speculative evidence for such mechanisms, I depart slightly from the earlier exposure effects design. I turn from those who move between neighborhoods to those who are permanent residents of a given neighborhood, but who are exposed to a different set of peers by virtue of their birth cohort. This strategy begins to open up the “black box” of the earlier place effects—why do permanent residents of some places in some years do better than others? This approach is also closer in spirit and method to the existing peer effects literature, which frequently exploits cross-cohort variation.

I identify positive and statistically significant peer effects among permanent childhood residents of Australian postcodes. I do this using idiosyncratic cross-cohort variation in peer parental income rank—those who happen to be born into a particularly richer (poorer) cohort for their postcode tend to do better (worse). More specifically, a 10 percentile rank point increase in the mean parent income rank of an individual’s peers is associated with a 0.17–0.26 percentile point increase in their own household income rank at age 24. These results hold with a variety of different specifications to control for time-varying postcode characteristics. Finally, the peer
effects appear to be strongest in more densely populated postcodes and driven by same-sex peers, which lends weight to the view that the effects reflect social interactions between peers. Given the large psychological literature on the increasing importance of peers during the teenage years (Brown and Larson 2009), such peer effects may play a role in explaining the observed pattern of exposure effects. While I find that simple controls for peers cannot explain the effects of place observed earlier, this may reflect the limitations of working with indirect measures of peers.

Finally, this paper validates and extends not just the findings but also the research design of Chetty and Hendren (2018a). I show that their approach can be applied in Australia—a country with less than a tenth of the population of the United States and less geographic variation in intergenerational mobility (Deutscher and Mazumder 2019). I am also able to replicate many of their validation exercises. To assist with this, I exploit moves within large cities, apply more data-driven selection criteria, and adjust standard errors to reflect the use of generated regressors. The approach may thus be able to yield fresh insights from further applications in a wide variety of settings (and indeed, this approach is being applied elsewhere; for example, Laliberté 2018 uses this general design to study school and neighborhood effects in Montreal).

I. Related Literature

Many studies have documented correlations in the economic outcomes of children who grow up in the same neighborhood (Black and Devereux 2011, Sharkey and Faber 2014). However, the extent to which these correlations reflect underlying causal mechanisms has remained a much more open question. Early experimental studies typically found little evidence of neighborhoods affecting economic outcomes (Katz, Kling, and Liebman 2001; Oreopoulos 2003; Ludwig et al. 2008). However, a new wave of studies—ones that use novel research designs and sources of variation, and larger, more mature datasets—has started to change that picture (Chetty and Hendren 2018a; Chetty, Hendren, and Katz 2016; Chyn 2018; Damm and Dustmann 2014).

Chetty and Hendren (2018a) use a quasi-experimental approach applied to administrative data in the United States to identify causal place effects.\(^2\) Crucially, these effects vary according to the time exposed to the location. A separate paper revisits the Moving to Opportunity (MTO) experiment and also finds that location matters according to time exposed, with beneficial outcomes for those moving to a lower-poverty neighborhood before the teenage years (Chetty, Hendren, and Katz 2016).

A recurring theme in Chetty and Hendren (2018a) and Chetty, Hendren, and Katz (2016) is that these exposure effects appear relatively constant—the outcomes of children who move to a better neighborhood improve linearly in proportion to the amount of time they spend growing up there. However, as Chetty and Hendren note, the observed linearity is based on children moving at ages 9

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\(^2\)Chetty and Hendren (2018a) use variation in the age at which children move locations to identify the effects of exposure to place. Perhaps the most closely related precursor is Aaronson (1998), who uses variation in the age at which siblings move locations to identify potential neighborhood effects in survey data.
and above, and extrapolating from this is “a strong assumption that should be evaluated in future work.” Depending on how their estimates are extrapolated, a child moving at birth to a new location in the United States could be expected to pick up anywhere from 56 percent to 92 percent of the difference in permanent residents’ expected outcomes between their origin and destination. Furthermore, their causal estimates for specific places are based on moves made from age 11 onward, and thus potentially miss differences in the causal effects of places in early childhood.

There is no reason to expect that each year of childhood in a better location will be equally important. Indeed, the literature most closely related in methodology generally finds this is not the case. Age at migration to a country often matters most during the teenage years, with negligible or marginal gains to English language proficiency, years of schooling, earnings, height, and cognitive skills from an extra year in the destination country prior to adolescence (e.g., Bleakley and Chin 2004; van den Berg et al. 2014; Basu 2018). Exposure to place within a country can also matter more within the teenage years for outcomes such as teen parenthood (Wodtke 2013). Indeed, such patterns are also apparent in Chetty and Hendren (2018a) when college attendance and teen birth are the outcomes considered.

This paper adds to the literature by showing that exposure to place, at least on the scale considered here, tends to matter most during the teenage years. Note that this need not contradict the large literature on the sensitivity of early childhood (e.g., Case, Fertig, and Paxson 2005; Cunha and Heckman 2007; Currie and Almond 2011). It may simply be that the large neighborhoods examined here mask substantial variation in what matters in the early years, while exhibiting variation in what matters most during the teenage years. The shocks entailed by moving may simply differ from those examined in the literature on early childhood.

So why might place matter? One explanation for a causal role of place is the influence of peers. Peer relationships become more salient and complex during adolescence (Brown and Larson 2009), and they potentially explain part of the sensitivity of the teenage years. Peer effects have received a lot of attention in the education literature (Sacerdote 2011), reflecting both their potential role in school settings and the availability of data with clearly identified cohorts. School-based studies have also gone beyond student achievement to broader behaviors with potentially long-lasting consequences, including the decision to drop out (e.g., Gaviria and Raphael 2001) and involvement in criminal activities (e.g., Billings, Deming, and Schönberg 2017).

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3 See Chetty and Hendren (2018a, 1,110, 1160).
4 This is dependent on whether their 4 percent annual rate of convergence is applied only to the 14 years that they observe it—from age 9 to 23 years—or to the full 23 years.
5 I occasionally refer to locations with higher predicted total household income ranks as having “better” outcomes or as “better” locations. This is a convenient shorthand and is not intended as a normative statement, as many features of place beyond those considered here will be relevant for individual wellbeing.
6 This is more typically framed in terms of there being a critical (preteen) age beyond which one’s ability to catch up to those born in the country is progressively more limited.
7 Chetty and Hendren (2018a) note on teen birth: “the gradient is especially steep [exposure effects are especially high] between ages 13 and 18, suggesting that a child’s neighborhood environment during adolescence may play a particularly important role in determining teen birth outcomes” (1,157). This paper suggests the same could be said for broader economic outcomes.
8 Workplaces are another setting where peer groups may be credibly identified (e.g., Cornelissen, Dustmann, and Schönberg 2017).
Explicit studies of long-run effects are uncommon, but one notable exception is Black, Devereux, and Salvanes (2013), where cross-cohort variation is used to study the causal effects of teenage peers on long-run outcomes in Norway.\(^9\) I similarly use cross-cohort variation to provide suggestive evidence for lasting peer effects in Australia.

II. Data

I use de-identified Australian Taxation Office (ATO) intergenerational data drawn from federal income tax returns from the 1991 to 2015 financial years. Parent-child links primarily come from linking children to adults living at the same address when the child registers for a Tax File Number (TFN), which is a unique personal identifier issued by the federal government. The links are also informed by a short period in which children were claimed on tax returns. Children are linked to a “primary parent,” the adult considered most likely to be a parent based on shared address, shared last name, and age at the time of the child’s birth.\(^10\) Children are also linked to the primary parent’s first reported spouse over the period 1991–2015.\(^11\) More details on the construction of the data are in online Appendix E.\(^12\)

A. Sample Definitions

I initially restrict attention to a sample of those who (i) were born in Australia between the 1978 and 1991 financial years, and (ii) remained residents of Australia through 2015. Earlier cohorts cannot be reliably restricted to those born in Australia and later cohorts are too young to observe at age 24, the main outcome measure in this paper. The last step—excluding those who emigrate at some point—drops 4.8 percent of the sample, with a slight skew to those from higher-income families. Australia’s emigration rates are relatively low, presumably reflecting its distance from other large, wealthy, and culturally similar countries.\(^13\)

\(^9\) Black, Devereux, and Salvanes (2013) find girls’ outcomes benefit from having a higher proportion of female peers, while boys’ outcomes are harmed. They find little evidence that the age or education of peers’ mothers matters, while the earnings of peers’ fathers matters for boys. This study differs in ways that make it difficult to directly compare the results. Most notably, they define peers as those who were in the ninth grade in the same year and school—a more direct, but also much narrower, definition.

\(^10\) Intergenerational links based on shared residential addresses are behind the Canadian data used in a number of influential studies (Corak and Heisz 1999; Oreopoulos 2003; Corak and Piraino 2011). However, the Australian data has the notable advantage of drawing on earlier registration addresses and additional information such as claims for children on tax returns. The data most closely resembles that used by Chetty et al. (2014) in quality and coverage.

\(^11\) This is the case when the youngest child in the family is no older than 24 at the time the parent reported the spouse. Questions on spouse details have always appeared on tax returns over this period, but answers have not always been required. Linking to the earliest reported spouse rather than the spouse at either a fixed point in time or in childhood attempts to cater for the resulting underreporting of spouses. Nonetheless, a restriction based on the age of the youngest child is used to avoid linking to spouses who were not present during childhood.

\(^12\) The ATO staff constructed the intergenerational data. The author wrote code, which was submitted to the ATO to be run internally on the resulting datasets to produce the results presented in this paper.

\(^13\) For example, the proportion of those born in Australia, in their thirties around the year 2000, and living in another country was 4 percent. This equals the median among OECD countries and is lower than all other English-speaking countries, with the exception of the United States (OECD 2019).
The resulting families closely mirror population benchmarks for family structure, median parental age at birth, and family size (Table 1). There are slightly more children in coupled families and fewer in lone-mother families, likely reflecting repartnering of lone mothers that is captured in our panel but not in the population cross section. For the 1978–1991 birth cohorts considered here, I have parent links for around 3.1 million children, or 92 percent of those in the data.14

**Exposure Effects Samples.**—To investigate the effect of exposure to place, I first split the full sample in two based on residential history—creating permanent resident and mover subsamples. I use Statistical Area 4 (SA4), as defined by the Australian Bureau of Statistics (ABS), as my unit of geography. There are 107 SA4s covering Australia, typically with populations that range from 100,000 to 500,000. The choice of SA4 is motivated by the methodology; these units strike the best balance between having a large permanent resident population (and thus more precise predicted outcomes for those who move) and having a large population of moves (and thus a larger sample).15 Permanent residents are those whose primary parent files from only one SA4 from 1991 through to the year the child turned 35. Movers

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14 This is on par with the 91 percent link rate achieved by Chetty et al. (2014) for the 1980–1988 cohorts used in Chetty and Hendren (2018a). See online Appendix Table 1 in Chetty et al. (2014).

15 The next unit up in the main structure of the Australian Statistical Geography Standard is the state/territory level—Australia has only 8 of these and only 27 percent of the moves in the sample cross state/territory lines. The next unit down in the structure is the Statistical Area 3 level, with populations that range from 30,000 to 130,000—too small to generate precise predicted outcomes based on permanent residents.
are those whose primary parents file from multiple SA4s. I focus on those moving once (with parents filing from two distinct SA4s).

I limit the influence of measurement error in the presence and timing of moves in three ways. First, I consider only moves between SA4s where the primary parent filed in both for at least two years. Second, I consider only moves where the parent’s first year filing from the destination immediately follows their last year filing from the origin—this ensures the child’s age at move is known to within a year. Third, I restrict attention to those moving at least 15 kilometers, thus dropping short moves that just happen to cross SA4 boundaries.16

**Peer Effects Sample.**—To investigate peer effects, I begin again with the full sample, but now restrict attention to those growing up in the same postcode. I create a set of permanent postcode residents whose primary parent files from only one postcode from 1991 through to the year the child turned 20. Large neighborhoods are no longer required by the methodology. This is closer to the common conception of a neighborhood and the scale on which social interactions take place. It also more closely resembles the existing literature, where peer effects are commonly examined in the context of relatively small groups (e.g., workplaces and school grades).

### B. Variable Definitions

Individual income is defined as total pretax income.17 In years that an individual has filed a tax return, this is their reported total income or loss. In years that an individual has not filed a tax return, it is the sum of individual salary and wages reported by employers through annual pay-as-you-go (PAYG) payment summaries, and taxable government allowances, benefits, and pensions. This income measure includes labor and capital income, and taxable government payments such as unemployment and study benefits; it is prior to any tax deductions or offsets. Income variables are measured in 2015 Australian dollars, adjusted for inflation using the headline consumer price index published by the Australian Bureau of Statistics (2017). In all analyses, individuals with strictly negative parent or child adult total pretax family income are dropped, as negative income is typically associated with high wealth and hence a poor indicator of actual economic well-being.

**Parent Income.**—Parent income is the sum of the income of the identified parent(s) in each year, averaged from 1991 to 2001 inclusive. This window is chosen to strike a balance between potential attenuation biases arising from measuring parental income over too short a period versus too late in life when income is typically more volatile (Mazumder 2005).

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16 In Chetty and Hendren (2018a) only moves of 100 miles or more are considered. A less restrictive condition is used here as Australia’s population is highly concentrated in a small number of major cities—around one-third of Australians live in Sydney or Melbourne. Moves within these cities are thus of interest to policy makers and empirically important. In the unrestricted mover subsample, 44.3 percent of moves are within the same city while 26.3 percent are within Sydney or Melbourne.

17 Individual income is clearly identified in Australian tax returns, as the individual is the primary unit of taxation. While family-based income tests exist for some taxes and family benefits, income is reported on an individual basis.
Child Income and Other Outcomes.—Child adult family income is the income of the child and their most recently reported spouse. I follow Chetty and Hendren (2018a) in measuring child income at age 24 or, more precisely, in the financial year in which they turn 24. The intent here is not to proxy for a child’s lifetime income, but rather to gauge whether, how, and why place might have a causal effect on later outcomes. As such, measuring outcomes at this age is not as obviously problematic as it would be in more typical intergenerational mobility studies, where lifecycle bias has received significant attention (for a recent examination, see Nybom and Stuhler 2017). Nonetheless, key results are also examined with outcomes measured at later ages for a subset of individuals.

I also explore a range of other child outcomes. As a proxy for university attendance, I use whether an individual has a debt under the federal Higher Education Loan Program (HELP) at age 24. These income-contingent loans are on highly favorable terms and are taken up by the vast majority of students attending university. I also use residence in one of Australia’s capital cities at age 24 as an outcome variable. These capitals are the dominant cities in their state or territory and are growing in population share, reflecting their desirability as a place to live. Finally, I use whether an individual has a spouse and dependent children in 2015. This latter variable is only captured for all individuals in the last few years of the sample, which means number of children at a given age cannot be calculated.

Location.—In constructing the de-identified file, parents were assigned to the residential postcode listed on their tax return in each year. For nonfiling parents, the postcode was recorded as missing. Postcodes were interpolated across periods of missing observations—if a parent listed the same postcode on either side of such a gap they were assumed to have been in the one location the whole time.

C. Sample Comparisons and Summary Statistics

Table 2 presents key features of the sample compared with Chetty and Hendren (2018a). The key advantages of the Australian data are the longer panel of tax returns and longer span of birth cohorts linked to parents. This allows outcomes to be observed for children moving from age 2 (rather than from age 9). Despite the longer panel, sample sizes are significantly smaller, reflecting Australia’s population. The samples of permanent residents and 1-time movers are 1,683,800 and 313,900.

The ABS postcode correspondence assigns postcodes to areas roughly in proportion to population. A postcode may sit entirely within an area or be split across multiple areas. Postcodes are assigned to the area containing the largest split. Around 80 percent of postcodes sit within the one SA4. While some individuals will be misallocated, this misallocation is consistent by postcode and hence simply a caveat to the definition of the geographical units under consideration.
A challenge to identifying causal effects of place in Australia is that these effects are likely smaller than in the United States. Australia is a relatively centralized federation, with the federal government controlling the individual tax and transfer systems and exerting significant influence on health and education policy. Perhaps as a result, there is less variation in outcomes across Australian regions. Panel C in Table 2 shows the distribution across regions of the expected household income rank of a child born into the twenty-fifth percentile of the income distribution. This is the “absolute mobility” measure reported by Chetty et al. (2014). There is less regional dispersion in these expected ranks in Australia—the difference between the regions at the ninetieth versus the tenth percentile is only 8 rank points in Australia versus 14.7 in the United States.

Finally, the geographic units used in this paper are smaller on average and much more uniform in size (panel D). This reflects the differing treatment of major cities. In the United States, major cities such as New York and Chicago are represented by a single commuting zone, whereas they are represented by several SA4s in Australia. In the Australian context of a smaller and more concentrated population, this is an advantage, as within-city variation can be exploited. However, smaller geographic units also limit one’s ability to conduct some validation exercises.

In online Appendix Table B.1, summary statistics are presented for the permanent resident and 1-time mover samples. Moving children tend to be from slightly higher-income families and are more often from coupled families; they also tend to end up with higher incomes themselves. However, these differences are small and
mask substantial variation. In online Appendix Table B.2, summary statistics are presented for the origin and destination of the 1-time movers—there is a slight tendency to move to places with lower parent income ranks, fewer permanent residents, and worse predicted outcomes for children.

III. Empirical Framework

Estimation of Causal Place Effects

I use the identification strategy introduced in Chetty and Hendren (2018a). This strategy has essentially two steps. In the first step, we use the permanent residents of a place to generate predicted outcomes for those who grow up there. The resulting predicted outcomes will reflect the combination of any causal effect of the place itself and differences in the families who live there. In the second step, we examine those who move. Specifically, we estimate the extent to which moving a year earlier shifts expected outcomes from those predicted for the child in their origin toward those predicted in their destination. I discuss below the circumstances in which this can be viewed as a causal estimate of the effect of exposure to place. A fuller and more formal introduction to the identification strategy can be found in Chetty and Hendren (2018a).

Predicted Outcomes of Place.—In the first step, I use the sample of permanent residents to generate predicted outcomes for those growing up in a place. To do this, I estimate the relationship between parent and child household income ranks \( p_i \) and \( y_i \) in each SA4 \( l \) and birth cohort \( s \). The following parsimonious linear specification is used:

\[
y_i = \alpha_{ls} + \beta_{ls} p_i + \varepsilon_i
\]

where \( l \) and \( s \) denote the child’s SA4 location and financial year of birth cohort, respectively. Rank-based measures are less sensitive to attenuation and lifecycle bias—a point made in a number of studies, but particularly clear in the lifetime income histories available in Swedish data (Nybom and Stuhler 2017). In many countries, the linear specification is also a reasonable approximation to the relationship between parent and child household income ranks (Bratberg et al. 2017). To illustrate the suitability of the specification, online Appendix Figure A.1 plots the mean child household income rank at age 24 by parent household income decile for the 18 largest SA4 birth cohorts in 1978. Both the general linearity of the relationship and relatively small deviations from the national relationship are striking. The fitted model from equation (1) then generates two predicted ranks for each child in the sample of movers—denoted \( \tilde{y}_{ops} \) and \( \tilde{y}_{dps} \)—based on two counterfactuals, namely that they were permanent residents of their origin and destination, respectively.

20In the United States, the moving families have higher mean but lower median incomes, and their children end up with lower mean and median incomes. See Table 1 of Chetty and Hendren (2018a).
These predictions are particular not only to the places the child is moving between, but to the child’s birth cohort and parent income rank.

Expected Outcomes of Those Who Move.—In the second step, I use the sample of movers to examine if those moving at earlier ages pick up more of the difference in their predicted outcomes between their origin and destination. I use the same specifications as Chetty and Hendren (2018a). Specifically, I estimate a child’s eventual household income rank as a function of their predicted origin outcome \((y_{pos} - \bar{y}_{pos})\) and the “shock” to this coming from their destination \((\Delta_{odps} = y_{dps} - \bar{y}_{ops})\), interacted with their age at move \(m\). In the most general model, I estimate

\[
y_i = \alpha_{qosm} + \sum_{s=1978}^{1990} I(s_i = s)(\kappa_s \Delta_{odps}) + \sum_{m=2}^{34} I(m_i = m)(b_m \Delta_{odps}) + \varepsilon_i,
\]

where \(\alpha_{qosm}\) is a set of fixed effects for the child’s parent income decile \(q\), origin \(o\), cohort \(s\), and age at move \(m\). The coefficients \(b_m\) capture the expected increase in rank associated with moving at age \(m\) to a destination with a 1 percentile rank higher predicted outcome than the origin for permanent residents. The coefficients \(\kappa_s\) allow for measurement error arising from potential mismeasurement of the child’s origin—some children may have been born somewhere other than their parent’s first recorded location. This is more of a concern with the earlier cohorts.\(^{21}\) Similar estimates of \(b_m\) are obtained from a more parsimonious and less computationally burdensome model:

\[
y_i = \sum_{s=1978}^{1990} I(s_i = s)(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}) + \sum_{m=2}^{34} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p)
\]

\[
+ \sum_{s=1978}^{1990} I(s_i = s)(\kappa_s \Delta_{odps}) + \sum_{m=2}^{34} I(m_i = m)(b_m \Delta_{odps}) + \varepsilon_i,
\]

where we can think of the first sum as capturing origin effects, the second disruption effects, the third the cohort controls, and the fourth the coefficients of interest.

The \(b_m\) coefficients capture both the causal effect of moving at age \(m\) \((\beta_m)\) and a selection effect \((\delta_m)\). The selection effect captures the idea that parents who move to better or worse places may have other attributes, not adequately reflected in their income ranks, that affect child outcomes. For example, perhaps parents moving to places with better predicted outcomes for their children have higher wealth, job, or relationship security. To identify causal effects, I follow Chetty and Hendren (2018a) in making the additional assumption that selection effects do not vary with a child’s age at move \((\delta_m = \delta \text{ for all } m)\). While this assumption is strong, it is less restrictive than it may at first appear. For example, it allows for parents to select in to or out of moves on the basis of the difference in predicted outcomes between places (is the place better or not) or the age of their child (is it a good time to move or not). We assume away mechanisms that relate the two of these; for example, a tendency

\(^{21}\) Since the data begin in 1991, we have the full childhood location for only the most recent cohort.
for parents with better (worse) unobservables to move earlier to places with better (worse) outcomes. This is a more involved selection mechanism, and one that may be less likely for the large moves considered here. Past Australian work suggests that by restricting to moves of more than 15 km, I lose most moves motivated by personal and family reasons or a desire for better housing or a better neighborhood, but retain most moves triggered by work or study opportunities (Wilkins, Warren, and Hahn 2009). Nonetheless, in Section IVC, I discuss whether violations of the identifying assumption may bias the estimated effects of exposure to place.

With the assumption that selection effects do not vary with a child’s age at move, the causal effect of exposure to place can be identified. First, the selection effect $\delta$ can be identified from $b_m$, where $m$ is greater than the age at which the outcome is measured. This is because the causal effect must be zero if the move happens after the outcome is observed. Second, the causal effect $\beta_m$ of moving at age $m$ can be identified by subtracting the selection effect $\delta$ from $b_m$. The causal effect of an additional year of exposure at age $m$ can be identified as $\gamma_m := b_m - b_{m-1}$. If these exposure effects are a constant $\gamma_m$ over age groups $m \in M$, then we can further parametrize equation (3) as:

$$y_i = \sum_{s=1978}^{1991} I(s_i = s) (\alpha_s + \alpha_s \bar{y}_{i,pos}) + \sum_{m=2}^{34} I(m_i = m) \left( \zeta_m^1 + \zeta_m^2 p_i \right)$$

$$+ \sum_{s=1978}^{1990} I(s_i = s) (\kappa_s \Delta_{odps}) + \sum_{m \in M} \left( \delta_m + \gamma_m e_m \right) \Delta_{odps} + \varepsilon_i,$$

where $e_m$ is the number of years in age group $m$ that the child was exposed to the destination. I will mostly use the age groups $\{2, \ldots, 11\}, \{12, \ldots, 24\}$, and $\{25, \ldots, 34\}$.22

Finally, the above models fall into the general class of two-step estimation, where some regressors in the model of interest (one of equations (2)–(4)) are generated from an earlier model (equation (1)). A resulting econometric concern is that not accounting for the uncertainty in the generated regressors can lead to inconsistent standard errors (Pagan 1984). I address this in two ways. First, I use the estimated standard error on the $\Delta_{odps}$ terms to restrict the sample to moves with more precisely estimated regressors. Specifically, I consider moves where the standard error on $\Delta_{odps}$ is less than 2 percentile rank points. This is a more direct means of ensuring precision than the population restriction used in Chetty and Hendren (2018a), where the baseline analysis considers only commuting zones with populations of 250,000 or more in 2000. Second, Murphy and Topel (1985) provide a formula for calculating asymptotically correct standard errors in fairly general settings, and these Murphy-Topel standard errors are presented where possible throughout this paper.23 Online Appendix D describes the calculation of Murphy-Topel standard errors in

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22 For the post-outcome age group, we should have $\delta_m = \delta$ identifying the selection effect. I set $\delta_m = \delta_m'$ for the pre-outcome age groups, allowing only the exposure effect $\gamma_m$ to vary by age.

23 Bootstrapping standard errors was another option but was not computationally feasible. Murphy-Topel standard errors cannot be calculated where the model of interest has regressors from multiple earlier models. In this case naive standard errors are reported—this is mentioned in the table notes where applicable. The differences between naive and Murphy-Topel standard errors in the regressions tend to be relatively small in this paper.
more detail, and shows key findings to be robust to varying the precision-based sample restriction.

IV. The Causal Effect of Exposure to Place

I now turn to the results. In this section, I discuss the estimated causal effects of exposure to place in Australia, including the heightened importance of place in the teenage years. I begin with the baseline results before turning to other outcome variables beyond income, heterogeneity in the results, and a range of validation exercises.

Figure 1 shows the estimated coefficients $b_m$ from equation (3). These represent the expected boost to an individual’s household income rank at age 24 associated with a move at age $m$ to a place with 1 percentile rank point higher outcomes for permanent residents. Three patterns are of interest: the evidence of selection effects, positive exposure effects, and the more pronounced sensitivity of the teenage years.

First, there is evidence of positive but age-invariant selection into moves to better places. Children do better at age 24 even if their parents only later move to a place with better outcomes. This selection effect appears invariant to the child’s age at move: if I fit a line to the estimated regression coefficients $b_m$ for $m \in \{25, \ldots, 34\}$, I get a slope coefficient of $-0.00097$ (SE 0.0085). The mean selection effect is 0.308, slightly higher than the 0.226 found for the United States in Chetty and Hendren (2018a). This is consistent with Australia’s lower regional variation in mobility relative to the United States (see Deutscher and Mazumder 2019), reflecting less of a causal role of place.

Second, there is evidence of positive exposure effects—the expected benefits of a move to a place with better outcomes are greater the earlier one moves. Third, the exposure effects ($\gamma_m = b_m - b_{m+1}$) appear to vary systematically by age, with higher exposure effects in the teenage years. In total, a child whose parent moves at their birth would be expected to pick up about 70 percent of the difference in predicted outcomes between their origin and destination.

The implied causal effects of place are economically meaningful, despite the reduced variation across Australia. For example, if we consider total household income at age 24 for a child born to parents at the twenty-fifth percentile of the income distribution, then the difference between a region in the bottom decile and the typical (median) region translates to roughly AU$4,000 a year (US$2,800). If we treat 70 percent as causal and a potential feature of the childhood environment that could be changed, then we are left with around AU$2,800 a year (US$2,000).

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24 Selection effects are not considered for $m = 24$ as the child’s outcome is potentially affected by moves at this age. Outcomes are measured in the income year the child turns 24. Age at move is determined by how old the child turns in the income year for which the primary parent files a return from a new location. While the parent will file the return after that income year, the move may well have occurred during it and affected child outcomes.

25 The fact that most of the differences in outcomes between places in Australia appear to be causal does not imply that place explains a large portion of the variation in outcomes between individuals. As already noted, the geographic variation in outcomes in Australia is relatively modest. It may be the Danish finding (Bingley, Cappellari, and Tatsiramos 2016) that neighborhoods account for a large share of the variance in permanent earnings between siblings early in the working life, but a negligible share beyond age 30 would also replicate here, though this is beyond the scope of this paper.
Understanding the underlying mechanisms behind the causal effect of place is critical to knowing if and how such gaps could be closed. In online Appendix Table B.3, I provide exposure effect estimates and model fit statistics for several versions of the parametrized model in equation (4). In column 1, I assume constant exposure effects as in Chetty and Hendren (2018a). In columns 2–8 I allow a kink, with exposure effects that are allowed to vary between early and late childhood. Consistent with the visual impression left by Figure 1, the best fitting parametrized model allows a kink at age 11.26 An additional year in a place with 1 percentile rank point higher outcomes is associated with a gain at age 24 of 0.011 (SE 0.007) rank points before age 11 and 0.042 (SE 0.003) rank points after age 11; the $p$-value on a test of equivalence of the early and late childhood exposure effects is 0.00068. This is the baseline model for the analysis that follows.

Finally, the sensitivity of the teenage years is not confined to place exposure effects—the fixed costs associated with moving also appear higher in these years.27 Online Appendix Figure A.2 shows the expected household income rank at age 24 of

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26 This model is preferred to the other potential kink points on all the model selection criteria. It is also preferred to a model with constant exposure effects on all but the BIC, which places the highest penalty on the additional parameter.

27 Costs are “fixed” in the sense that they do not vary with the difference in predicted outcomes between the origin and destination ($\Delta_{odps}$).
a child born in 1991, with parent income rank of 50, and moving at age \( m \) between an origin and destination where their predicted outcome is also an income rank of 50 (implying \( \Delta_{odps} = 0 \)). Positive selection effects are apparent—children exceed their predicted ranks at age 24 even if their parents only move later—but these also appear age-invariant.\(^{28}\) If these selection effects can also be extrapolated back then the chart suggests a fixed cost of moving between places. These fixed costs are generally higher during the teenage years; in fact, they are highest at age 17, when students typically enter their final year of schooling and prepare for university entrance exams.

It is important to note that the implied fixed costs of moving are relatively large, and may well outweigh the benefits. If we subtract the assumed selection effects, then in the sample of moves in the data, only 15.6 percent of moves (23.9 percent of moves to places with better outcomes) carry an expected benefit to the child based on their age at move \( m \), parent rank \( p \) and the difference in outcomes of permanent residents \( \Delta_{odps} \). This discussion is intended as illustrative only, as I have not presented arguments in favor of a causal interpretation of these fixed costs.\(^{29}\) Rather, it is simply a useful caveat to bear in mind when considering any potential policy responses. The regressions here do not imply that moving to a better place is invariably associated with better expected outcomes. In addition, general equilibrium effects may become relevant with larger flows of people—for example, as constraints on supply in high-quality schools or demand in strong local labor markets begin to bind.

A. Other Outcome Variables

There are good reasons to look beyond income to other outcomes when exploring the causal effect of place.\(^{30}\) Income is far from the only determinant of individual wellbeing. Further, if observed differences in income simply reflect compensation for differences in the cost of living across Australia, then they may be of no consequence to the individual. In contrast, differences in educational attainment and family formation are more difficult to explain away as inconsequential. Finally, other outcome measures may be less subject than income to a transient component—such as local labor market cycles—and may thus bear a stronger relation to lifelong outcomes.

In Figure 2, I explore the effect of exposure to place on other outcomes—namely having attended university by age 24, having a spouse, having dependent children in 2015, and living in a major urban area at age 24. Across all four measures the same general patterns are observed. There is a positive selection effect that appears relatively age-invariant from the mid-20s.\(^{31}\) Prior to this, an additional year spent in

\(^{28}\) This positive selection into moving is consistent with online Appendix Table B.1, where movers were found to come from slightly higher-income families, on average.

\(^{29}\) The fixed costs of moving are not without precedent. In an altogether different setting, Chetty, Hendren, and Katz (2016) find slightly negative effects for those moving in adolescence as part of the Moving to Opportunity experiment (but positive effects for those moving when young), which they suggest may reflect disruption effects.

\(^{30}\) I thank the editors and an anonymous referee for stressing the value of this line of inquiry.

\(^{31}\) An exception to this is major urban residence where there is a very modest slope after age 24. For example, a child living in a major urban area might foreshadow or influence a later move by their parents—a reverse causality story. Note, however, that this was not apparent in Figure 1 when looking at income.
a destination with higher rates of university attendance, marriage, fertility, or major urban residence tends to lift an individual’s own expected outcomes. This effect is most pronounced in the teenage years.

A useful observation from Figure 2 is that the extent to which observed differences between places are considered causal—and hence closed by moving to a location at birth—differs across the outcome variables. For example, over 90 percent of the gap between places in the propensity of children to live in a major urban center by age 24 is closed for those moving at birth. This is perhaps unsurprising; there is good reason to expect a strong connection between where an individual grows up and where they end up living later in life. In Page and Solon (2003b) this is proposed as a potential mechanism behind neighborhood effects—with most of the correlation in the adult earnings of neighboring boys in the United States explained by the large earnings premium in urban areas and the high correlation between

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**Figure 2. Place Exposure Effect Estimates for Other Outcomes in Adulthood**

Notes: Estimated coefficients $b_m$ from equation (3), with 95 percent confidence intervals, estimated for other adult outcomes, namely having attended university by age 24, living in an urban area at age 24, having a spouse, and number of dependent children in 2015. Lowess curves through the resulting estimates are shown with a bandwidth of 0.5. The $b_m$ capture the expected boost to an individual’s probability of the given outcome (or expected number of children) from moving at age $m$ to a place with 1 percentage point higher expected probability of the outcome (or one more expected child). They are estimated by regressing the given outcomes $y_i$ of those whose parents move once in their childhood on the interaction of their age at parent move $m$ with $\Delta_{odps} = y_{dps} - y_{ops}$—the difference between the expected outcomes for permanent residents of the same parent percentile rank $p$ and cohort $s$ in the destination $d$ versus the origin $o$. Controls capture: cohort and origin effects (via indicators for cohort and their interactions with predicted outcomes for permanent residents of the origin); disruption effects (via indicators for age at move and their interaction with parental rank); and indicators for cohort interacted with $\Delta_{odps}$ to capture potential mismeasurement of the origin.
childhood and adult urban residence.\footnote{32, 33} In contrast, around 30 percent of the gap between places in the university attendance, marriage, and fertility rates is closed by moving at birth. As a point of comparison, recall that around 70 percent of the gap between places in income ranks is closed by moving at birth.\footnote{34} These differences are not surprising; indeed, they are informative. For marriage and fertility, the findings arguably accord with general intuition about the extent to which these outcomes reflect something children take with them when they move (e.g., family fertility norms) or are exposed to in a place. For university attendance, the reduced causal role of place—which is not as apparent in the United States (Chetty and Hendren 2018a)—is consistent with a variety of explanations, including a greater role in the Australian setting for regional income differences that reflect local price levels or labor market conditions rather than the acquisition of human capital.

The ability to measure early childhood exposure effects is the primary advantage of the Australian data, and the finding of smaller place effects in these years is a key contribution of this paper. It also seems unlikely that this finding will be unique to Australia. As noted earlier, the age-at-migration literature has generally found additional years of exposure to the host country matter most in the teenage years for skills such as language acquisition. Further, there are hints in Chetty and Hendren (2018a) that a similar pattern may be observed in the United States as more data becomes available. For example, as the authors themselves have noted, the teenage years seem more important than earlier years in determining teen births and college attendance.\footnote{35} Indeed, in more recent work, Chetty et al. (2018) note smaller average exposure effects at earlier ages for those moving across US census tracts, and suggest this is consistent with the relative importance of adolescence apparent in the Australian setting.

\section*{B. Heterogeneity}

The sensitivity of the teenage years is not driven by any particular subpopulation, though there are important heterogeneities. In Table 3, exposure effects are estimated separately for various subpopulations. Across all subpopulations, the exposure effects are larger in late childhood, though to varying extents. Late-childhood exposure effects are significantly larger for boys than for girls—0.049 (SE 0.005) versus 0.032 (SE 0.005). Moves by individuals from poorer families are nearest to having constant exposure effects, though the teenage years still appear to be more sensitive. To dig deeper into this, Figure 3 presents the estimated exposure effects for early and late childhood by parent income quintile. For the bottom and top two

\footnote{32 The same has been found for neighboring girls (Page and Solon 2003a).}

\footnote{33 An earlier version of this paper investigated the potential for a similar explanation here, but was unable to precisely estimate the extent to which local labor markets could explain the causal effects of place (Deutscher 2018).}

\footnote{34 Chetty and Hendren (2018a) also find smaller exposure effects for marriage rates and (teen) births, but find exposure effects for college attendance that are only slightly smaller than those estimated for income ranks. Chetty and Hendren (2018a) examine college attendance, marriage, teen births, and teen employment as their other outcomes. The differences across outcomes in the proportion of the gap between places that is closed by moving at birth is not as visually apparent in their paper, as their Figure VIII has different y-axes.}

\footnote{35 In Figures IV and V, the coefficients $b_m$ for $m \in \{9, \ldots, 12\}$ do not generally display as strong a linear relationship as the later years. These coefficients are also not as precisely estimated given that they are based on only the later cohorts for which moves at this age can be observed (Chetty and Hendren 2018a).}
quintiles, an additional year in a destination closes around 5 percent of the gap between origin and destination predicted incomes ranks after age 11, and has no effect beforehand (though both are imprecisely estimated for the bottom quintile).

In contrast, exposure effects for the second and third quintiles are similar. These heterogeneities are important caveats to the results. The place effects estimated in this framework will capture the individual effects of many underlying mechanisms—schools, peers, role models, local labor markets—using variation across regions between which children move at different ages. These may have differing effects at different ages and across different subpopulations. In particular, exposure to place matters relatively equally in early and late childhood for children from lower middle-income families, a group that is large and of interest to policy makers.

C. Validation Exercises

One caveat to these results is the strong underlying assumption that selection into moves to better or worse places is age invariant. There are grounds to doubt this assumption, both intuitively and based on the data. Online Appendix Figure A.3 shows that moves in late childhood are much less common and slightly skewed toward lower-income and sole-parent families. While parent income rank is controlled for in the regressions, differences in other background traits may lead to biased estimates of the causal effect of place.

In online Appendix C, I replicate and discuss in detail the full suite of validation exercises conducted in Chetty and Hendren (2018a). The results generally support a causal interpretation of my baseline estimates. Using a more general set of controls or a later age of observation does not alter the conclusions above. Adding family fixed

### Table 3—Exposure Effect Estimates: By Population Subgroup

<table>
<thead>
<tr>
<th>Gender</th>
<th>Parent</th>
<th>Destination</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male (2)</td>
<td>Poorer (4)</td>
<td>Worse (6)</td>
</tr>
<tr>
<td>Female (3)</td>
<td>Richer (5)</td>
<td>Better (7)</td>
</tr>
<tr>
<td>Baseline (1)</td>
<td>Male (2)</td>
<td>Female (3)</td>
</tr>
<tr>
<td>Early</td>
<td>Poorer (4)</td>
<td>Worse (6)</td>
</tr>
<tr>
<td>0.011</td>
<td>0.011</td>
<td>0.011</td>
</tr>
<tr>
<td>(0.007)</td>
<td>(0.009)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Late</td>
<td>Poorer (4)</td>
<td>Worse (6)</td>
</tr>
<tr>
<td>0.042</td>
<td>0.049</td>
<td>0.032</td>
</tr>
<tr>
<td>(0.003)</td>
<td>(0.005)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>Observations</td>
<td>264,500</td>
<td>135,100</td>
</tr>
</tbody>
</table>

Notes: Estimates of the exposure effects $\gamma_m$ from equation (4) for early ($m \in \{2, \ldots, 11\}$) and late ($m \in \{12, \ldots, 24\}$) exposure for the full sample and various subpopulations. These represent the expected boost to an individual’s household income rank associated with an additional year at this stage of life in a destination with 1 percentile rank higher expected outcomes for permanent residents. They are estimated by regressing the adult ranks $y_i$ of those whose parents move once in their childhood on the interaction of their time exposed to the destination at each life stage with $\Delta_{odps} = y_{dps} - y_{ops}$—the difference between the expected outcomes for permanent residents of the same parent percentile rank $p$ and cohort $s$ in the destination $d$ versus the origin $o$. Controls capture: cohort and origin effects (via indicators for cohort and their interactions with predicted outcomes for permanent residents of the origin); disruption effects (via indicators for age at move and their interaction with parental rank); and indicators for cohort interacted with $\Delta_{odps}$ to capture potential mismeasurement of the origin. The subpopulations considered are, in order: males and females; those with parental income rank $\leq 50$ (or not); and those with $\Delta_{odps} < 0$ (or not). Murphy-Topel standard errors are in parentheses.
effects results in only a very modest reduction in the estimated exposure effects, and
the general sensitivity of the teenage years remains apparent. This rules out the results
being driven by selection based on fixed family unobservables. Siblings who differ in
their exposure to place also differ in their subsequent outcomes; those who move at a
younger age are expected to close more of the gap between their origin and destination.
Finally, a series of placebo tests illustrate that the outcomes of those who move
converge to those of the permanent residents of their destination in a very sharp
sense that is difficult to reconcile with alternative explanations. First, the outcomes
of those who move converge to the outcomes of permanent residents of the same
parent percentile rank of the same birth and cohort in the destination versus the origin.
Controls capture: cohort and origin effects (via indicators for cohort and their interactions
with predicted outcomes for permanent residents of the same parent percentile rank
and cohort); disruption effects (via indicators for age at move and their interaction with parental
rank); and indicators for cohort interacted with the expected outcomes of those
who move to capture potential mismeasurement of the origin.

Figure 3. Early and Late Childhood Exposure Effect Estimates: By Income Quintile

Notes: Estimates of the exposure effects $\gamma_{\infty}$ from equation (4) for early ($m \in \{2, \ldots, 11\}$) and late ($m \in \{12, \ldots, 24\}$) exposure by parent income quintile (with 95 percent confidence intervals). These represent the expected boost to an individual’s household income rank associated with an additional year at this stage of life in a
destination with 1 percentile rank higher expected outcomes for permanent residents. They are estimated by regressing the adult ranks $y_i$ of those whose parents move once in their childhood on the interaction of their time exposed to
the destination at each life stage with $\delta_{odps} = \bar{y}_{odps} - \bar{y}_{ops}$—the difference between the expected outcomes for permanent residents of the same parent percentile rank $p$ and cohort $s$ in the destination $d$ versus the origin $o$. Controls capture: cohort and origin effects (via indicators for cohort and their interactions with predicted outcomes for permanent residents of the origin); disruption effects (via indicators for age at move and their interaction with parental
rank); and indicators for cohort interacted with $\delta_{odps}$ to capture potential mismeasurement of the origin.
Many of the potential concerns with the empirical framework in this paper and with Chetty and Hendren (2018a) reflect a worry that families will select in to or out of moves. To be a concern, such a mechanism would need to be correlated with factors that matter for child outcomes. It would also need to be correlated with child age at move and the differences in predicted outcomes between the origin and the destination. This is certainly plausible—for example, perhaps more aspirational families move earlier to better neighborhoods. However, the sharp way in which movers’ outcomes converge to those of permanent residents lifts the bar yet again for such a selection mechanism. Such a mechanism would need to replicate the features of place that are specific to individual birth cohorts, genders, and aspects of the child-income distribution—noting that the move often predates the realization of these outcomes by several years. While it is easy to imagine that parents are able to identify locations that may lead to generally better or worse outcomes, their ability to select based on birth cohort, gender, and distributional outcomes seems more suspect. The idea that place may have a causal effect after all and that it is identified with minimal bias in these estimates seems more plausible.

An important caveat to the results is the fact that the exposure effects for moves in the top decile of relative postcode outflows are significantly attenuated, though very imprecisely estimated—something not seen in Chetty and Hendren (2018a). The idea behind this exercise is that these moves seem more likely to have been forced—due to mass layoffs or natural disasters—rather than chosen. This is a potential caveat to the external validity of the results, but would be consistent with the finding of Chyn (2018) that treatment effects and mechanisms may fundamentally differ between those choosing to move versus those forced to move.

In sum, the tests provide good evidence in support of a causal interpretation of the estimates in the Australian setting. The original validation exercises in Chetty and Hendren (2018a) largely replicate in an entirely different setting. While the validation tests provide some comfort that a causal effect of place has been identified, they provide little guidance as to what may drive that effect. The following section provides suggestive evidence of a role for broadly conceived peer effects.

V. Might Peers Matter?

One reason it matters where a child grows up is because it determines who a child grows up with. To investigate peer effects I turn from those who move between neighborhoods to those who are permanent residents of a given neighborhood but are exposed to a different set of peers by virtue of their birth cohort. This strategy begins to open up the “black box” of the earlier place effects—why do permanent residents of some places in some years do better than others?

A. Empirical Strategy

I exploit variation in the mean parent income ranks of birth cohorts within a postcode. While ideally a more direct measure of peers would be available, these
geographic and temporal boundaries are reasonable and the best available in the data.\textsuperscript{36} I begin with a standard intergenerational regression of child household income rank at age 24 ($y_i$) on parent household income rank ($p_i$):

\begin{equation}
  y_i = \alpha + \beta p_i + \eta c_{zs(i)} + \zeta X_{zs(i)} + \varepsilon_{izs},
\end{equation}

but now include $c_{zs(i)}$ as a measure of peer background—the mean parent income rank of those in postcode $z$ and birth cohort $s$, excluding individual $i$. A set of postcode-varying and time-varying controls is included in $X_{zs(i)}$; the choice of these is critical and discussed in some detail below. The key identifying assumption is $E[\varepsilon_{izs}|c_{zs(i)}, p_i, X_{zs(i)}] = 0$; in essence, we wish to be able to treat peer background as exogenous, conditional on own parent income rank and the controls in $X_{zs(i)}$.

There are numerous threats to identification. Selection issues are perhaps most salient. Parents raising their children in high-income postcodes almost certainly differ from other parents, potentially in ways that benefit their children’s outcomes. Any omitted variables are just as problematic—for example, cyclical effects that happen to be correlated with peer background and eventual outcomes.

A variety of approaches exist to isolate plausibly exogenous variation in peer background. This includes drawing on policies that provide quasi-random variation in peer groups but also the use of rich sets of controls. A common approach in the education literature is to allow for cohort and school fixed effects, and school-specific linear trends (e.g., Hoxby 2000). The idea is to isolate idiosyncratic differences in peer-group composition due to, say, differences in birth timing. Here, this translates to controlling for cohort and postcode fixed effects, and postcode-specific linear trends:

\begin{equation}
  X_{zs(i)} := a_s + a_z + b_z s.
\end{equation}

This controls for any tendency to sort based on postcode characteristics that are fixed or have a linear trend, and any cyclical factors. It has the drawback of being computationally intensive and missing nonlinear time trends, which may be important over a 14-year period. A more parsimonious approach is to include the moving average of peer background as a control (this is a robustness exercise in Black, Devereux, and Salvanes 2013). To implement this I set:

\begin{equation}
  X_{zs(i)} := \tilde{c}_{zs(i)},
\end{equation}

where $\tilde{c}_{zs(i)}$ is the seven-year moving average of the measure of peer background $c_{zs(i)}$.\textsuperscript{37} This controls for any tendency to sort based on the local average peer background.

\textsuperscript{36}Postcodes are the smallest reliably recorded geography in the tax data and are typically modest in population and area. Financial year of birth is readily available in the tax data, but also aligns well with school entry cut offs over the period in question. Calculations using the Household Income and Labor Dynamics in Australia (HILDA) survey suggest that around 60 percent of the relevant cohorts who shared a financial year of birth would have also shared the same grade cohort in their final year of school, with no year-length window performing noticeably better at maximizing the share in the same grade cohort.

\textsuperscript{37}As noted later, the results are not driven by the choice of window width.
In both approaches, it is differences over time within postcodes rather than differences between postcodes that provides the identifying variation. If an individual just happens to be born in a richer-than-average or poorer-than-average birth cohort for their postcode, do they end up with a higher or lower income themselves? In small populations, as found in postcodes, idiosyncratic differences in birth timing will generate these plausibly exogenous differences between peer groups. In the Australian data, the mean size of these postcode birth-cohort peer groups is a modest 135 children, and there is meaningful and relatively symmetric variation within postcodes in peer background.

B. Estimated Peer Effects

In Table 4, I display the estimated peer effects—the coefficients on peer parent rank \( c_{23(i)} \) from OLS regressions of equation (5). Across a range of specifications the coefficient on peer parent rank is positive and statistically significant. An increase of 10 percentile rank points in the peer parent rank is estimated to increase a child’s expected household income rank at age 24 by between 0.17 and 0.26 percentile rank points. The influence of peers is between an eighth to a fifth of the influence of parents at age 24.

The regressions in Table 4 cover a variety of ways to isolate idiosyncratic variation in peers. In column 1, I use the moving average approach (as in equation (7)). In column 2, I instead use cohort and postcode fixed effects and a postcode-specific linear trend (as in equation (6)), which yields a slightly smaller coefficient. In column 3, I include additional time-varying postcode controls to the specification in column 2, capturing local cyclical and structural trends in income, education, and welfare reliance.\(^{38}\) These controls are individually significant, but see a modestly higher estimate of the peer effect. The estimates from all three specifications are positive and statistically significant, and within each other’s confidence intervals. I use the moving average approach over a seven-year window through the remainder of this section. This specification has a simple interpretation—the effect of a particularly poor or rich cohort relative to those on either side—and is also computationally simple.

The baseline results are robust to the use of richer controls and later ages of observation, and under placebo tests. First, I find positive and statistically significant peer effects when combining the moving average and postcode controls (online Appendix Table B.4). Slightly smaller effects are observed for shorter window widths, which would be consistent with individuals having some peers drawn from the neighboring birth cohorts.\(^{39}\) The coefficients remain similar with family fixed
effects, though statistical significance is lost. Second, the peer effects do not appear to fade with time; rather, they tend to increase with the age at which outcomes are observed (online Appendix Figure A.4). Finally, I conduct a placebo test, looking for any apparent effect of the peers born in the years on either side of an individual’s own cohort.40 The largest and only statistically significant estimate is on an individual’s own cohort, suggesting the results are not driven by inadequate controls for trends within postcodes (online Appendix Figure A.5).

Why Might Peers Matter?—The peer effects identified here may reflect many underlying mechanisms. These include any influence of the peers and their parents arising from social interactions. But they may also include nonsocial externalities—for example, if individuals benefit when their peers’ parents lobby for better teachers or more resources for their cohort. In this section, I provide suggestive evidence that social mechanisms underlie the observed results.

First, the estimated peer effects are stronger in postcodes where social interactions seem more likely.41 In Table 5, I reestimate the peer effects for postcodes on either side of the median postcode population density and geographic size. The peer effects are largest in the more densely populated postcodes, with a coefficient on peer background of 0.052 (SE 0.018) versus 0.015 (SE 0.013). Similarly, the effects

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Table 4—Parent and Peer Influences on Household Income Rank at Age 24

<table>
<thead>
<tr>
<th>Specification</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Parent rank</td>
<td>0.128</td>
<td>0.129</td>
<td>0.129</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>Peer parent rank</td>
<td>0.026</td>
<td>0.017</td>
<td>0.020</td>
</tr>
<tr>
<td>(0.011)</td>
<td>(0.007)</td>
<td>(0.007)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Coefficients from equation (5)—the regression of a child’s household income rank at age 24 on their parent household income rank, their peers’ mean parent rank, and a set of control variables. These controls are either: a seven-year moving average of peer mean parent rank (column 1); postcode and cohort fixed effects and a postcode linear trend (column 2); postcode and cohort fixed effects, a postcode linear trend and the postcode’s mean government benefits paid, higher education loan debt, salary and wages, and total income for each individual with a tax liability in the year of observation (column 3). Peers are defined by postcode and financial year of birth and exclude the individual in question. The moving average of the peer mean parent rank is taken across adjacent birth cohorts in each postcode. A peer’s primary parent must have been a permanent resident of the postcode—not filing from outside it—from 1991 to the year in which the child turned 20. Robust standard errors, clustered by postcode, are in parentheses.
are largest in the geographically smaller postcodes.\footnote{42} This is consistent with the results being driven by social interactions that are more likely or more intense when individuals are in close proximity. In contrast, similar effects are seen when looking at peer groups with more or less income diversity than is typical. There are plausible forces that could push this in either direction—nonlinear peer effects might make the results more pronounced in diverse postcodes, while homogeneity might increase social interactions.

Second, I reestimate equation (5) separately by sex and with separate controls for female and male peers. While the average parent income rank of girls and boys in a postcode will be very similar, whether a birth cohort has particularly rich or poor parents of girls and parents of boys relative to the surrounding years are two separate questions. As a result, the identifying variation in peer background will differ when measured separately for female and male peers. From Table 6, it appears that for both sexes, the rank of their same-sex peers’ parents matters most. This result rules out otherwise plausible explanations for the peer effects. A simple “boy/girl next door” explanation, whereby peers matter because some individuals marry their peers, would require the opposite-sex peers to matter most. Further, it appears less likely that nonsocial externalities drive the results, as they would then have to operate in a sex-specific manner only—for example, richer parents lobbying for teachers, resources, or opportunities that specifically benefit their child’s sex.

The fact that the association between peer background and an individual’s later outcomes is most pronounced in settings and peer groups where social interactions appear more likely lends credence to the idea that such interactions may drive the results. Nonetheless, data limitations prevent a range of other useful exercises, such as testing whether the results are driven by attendance at the same school or are

\footnote{42}There is a close mechanical connection between population density and geographic size, so I look at both simply to show the distinction does not matter for the results.
more pronounced for peers from the same ethnic group (as observed by Billings, Deming, and Ross 2019, and Damm and Dustmann 2014 when looking at social interactions and youth crime, respectively). As such, the case for a social mechanism is suggestive rather than clear-cut.

Ideally, I would be able to test whether peers matter most during the teenage years, and would be able to nest them in the earlier model of exposure-to-place effects. For the former, I examined how estimates varied if the definition of the peer group was expanded to include those moving into a postcode cohort before the cohort hit the teenage years. However, insufficient variation between the competing peer group measures made it impossible to draw firm conclusions. For the latter, it is straightforward to include controls for the peer shock that movers experience.43 However, these fail to explain the patterns observed—the late childhood exposure-to-place effect remains steady at 0.04, while the exposure-to-peers effects are small and statistically insignificant. It could be that peers do not, in fact, explain any of the causal effects of place examined earlier in the paper. But another possibility is that these simple controls fail to capture the true peers of movers, or their influence on movers’ outcomes, any better than the predicted outcomes of the permanent residents.44

43 Specifically, I construct controls for the mean parent income rank of the permanent residents of the origin (̂p₀os) and the difference between this and the same for the destination (Δₜds = ̂pₜds − ̂p₀ds). I then reestimate equation (4), including controls for ̂p₀os and Δ₀ds that are identical to those for ̂yₜos and Δ₀ds. This allows for an exposure-to-peers effect. Whether these measures are based on the origin and destination SA4 or on the origin and destination postcodes does not materially change the results.

44 In theory, the predicted outcomes of the permanent residents include the influence of the full set of peers that lived in that location, even if only temporarily. The controls for peers included here are based only on the permanent residents. I explored a time-varying measure of peers, but this is made difficult by the gaps in the location histories inherent in the tax data.
In the absence of direct measures of peer relationships, their influence on movers remains embedded in the exposure-to-place effect observed here.

VI. Conclusion

A growing body of evidence suggests that where a child grows up has a causal effect on their later life outcomes (Chetty and Hendren 2018a; Chetty, Hendren, and Katz 2016; Chyn 2018; Damm and Dustmann 2014). I add to this literature, demonstrating that where an Australian child grows up has a causal effect on their adult income, education, marriage, and fertility. The effect of exposure to place is typically largest during the teenage years, which may help narrow the search for underlying mechanisms. One potential set of mechanisms is a range of peer effects, which may be more potent in the teenage years, and I find suggestive evidence that this channel is relevant.

I began by replicating the empirical approach of Chetty and Hendren (2018a), where the causal effect of exposure to place is identified by using variation in the age at which children move locations. I establish broadly similar causal effects of exposure to place in the Australian setting—a country with less inequality, more social mobility, and different institutions. The outcomes of children who move converge to those of the children raised in the destination, reflecting their time exposed to the new environment. Those moving earlier to a place with better outcomes tend to do better. A suite of validation exercises introduced in Chetty and Hendren (2018a) also replicate in Australia, and suggest this pattern of results is unlikely to reflect an underlying selection mechanism.

An important advantage of the Australian data is the ability to observe how the importance of place may vary across childhood. Across all adult outcomes—income, education, marriage, and fertility—exposure-to-place effects are generally largest during the teenage years, with smaller effects apparent in early childhood. This is consistent with age-at-migration studies, which find that the benefits to language acquisition from migrating a year earlier are generally largest in adolescence as well. It is also consistent with the findings of Chetty and Hendren (2018a) for earlier life outcomes in the United States, such as teenage births and college attendance, and more recent evidence based on moves between census tracts (Chetty et al. 2018). As such, it seems unlikely that this finding is particular to Australia. This finding does not suggest that early childhood is unimportant. What matters in early childhood may often be factors—such as the family or more localized environmental influences—where most variation is within rather than between the large neighborhoods examined here. Finally, there is evidence of important heterogeneity in the results, with early and late childhood seemingly equally important for those born into the second and third income quintiles.

Many potential mechanisms could drive a causal effect of place—for example, the quality of local health services, schools, and labor markets. Another possibility is that it matters where a child grows up because it determines who they grow up with—the peers and role models they find in their local community. I find suggestive evidence for such effects among permanent residents of Australian postcodes, using cross-cohort variation in peer parental incomes. In short, those who happen to
be born into a richer cohort for their postcode tend to end up with higher incomes themselves. Given the psychological literature on the heightened importance of peer relationships in adolescence (Brown and Larson 2009), these may provide an explanation for some of the influence of place. While simple controls for peers fail to explain the exposure-to-place effects, this may reflect a need for better data on actual peer relationships.

The idea that where a child grows up within a given country has a sizeable causal influence on their later life outcomes runs counter to many conceptions of equal opportunity. However, any policy maker seeking to redress such inequities needs a clearer understanding not just of whether place matters, but also when and why. This paper adds to a growing literature on the causal effects of place, establishing such place effects in Australia, the importance of the teenage years, and the potential for peers to explain some of the observed patterns.

REFERENCES


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