EFFECTS OF DIVORCE ON CHILDREN'S EDUCATIONAL ATTAINMENT, AND WHETHER THEY ARE ACCOUNTED FOR BY MOVING HOUSE

Revised final report

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SUMMARY

This project has examined whether the long term "cost" of divorce in terms of the children's years of education, and the attainment of various educational milestones, has declined over time. To this end, it used data from the component of the International Social Science Survey Australia (IsssA) which has collected retrospective information on parents' marital situation and educational attainment (as well as control variables) from nationally representative samples of cross-sections of the population since 1984.

The bivariate results suggest that, instead of declining, the educational "cost" of divorce has increased over time (Table 1). There are signs that the rise may have levelled off in the most recent cohorts.

The rise is also found in the multivariate results, which suggest that the cost has risen, even net of other potentially confounding social changes. Importantly, the effect of divorce is focused relatively early in the educational career: it significantly reduces the chances of completing secondary school but, among those completing year 12 fails to have a statistically significant impact on the likelihood of completing a tertiary qualification.

The fact that divorce is becoming more damaging is partial evidence against the "selection hypothesis" that it is not divorce itself, but pre-existing adverse characteristics of the family of origin which cause both divorce and educational difficulties. In other words, divorce has been becoming less selective, so selectivity into divorce cannot account for an increasing educational "cost". As in much non-experimental work, there remain possibilities that variables omitted from the standard model account for the apparent effect of divorce on children's education.

The project also investigated the "selection hypothesis" from another angle, by modelling the risk of parental divorce, and then using the estimated prediction equation to create an adverse characteristics index (as a proxy for unmeasured adverse characteristics of the family of origin) for each respondent. This index was entered into the main models of education to see whether it would account for the divorce effect. The adverse characteristics index has a quite large statistically significant regression coefficient in the models of education, but the effect of divorce remains statistically significant when the adverse characteristics index is included in the models.

The project also examined the possibility that moving house, which is often attendant upon divorce, might account for the effect of divorce. The impact of moving house on educational attainment has been unexplored in Australia in previous systematic analysis of national surveys, so the work here is more exploratory, and some of the tests are more indirect because of the nature of the

existing data. Prior research has found a size of place of residence effect, with people from rural areas not going so far in education, and our results find this, too. The results also show that moving house into an urban area is not an educational advantage, and that moving house per se does not seem to be either an advantage nor a disadvantage.

Importantly, taking these various moving house effects into account in our key divorce equations leaves the estimated effect of divorce statistically significant.

INTRODUCTION

Objective

Two recent projects for FaCS – on "Effects of family and community on well-being" and "Neighbourhood and family effects on employment" both discovered different negative consequences of divorce for children, even after they had grown to adulthood. The former discovered a negative effect of parental divorce on their offspring's (decades later) subjective well being as adults[1]. The latter report found negative effects on employment, and, importantly, that those mostly could be attributed to the educational deficit associated with divorce. That finding makes important a closer examination of the effects of divorce on educational attainment.

This project will examine whether the deleterious effect of divorce on children's education is declining as social institutions have adapted to the huge rise in divorce since the Family Law Act of 1976. Moreover, the availability of benefits to single parents and improvements to the collection of child support should have ameliorated the financial stress associated with divorce. That, in turn should help the children invest in education.

On the other hand, it is possible that emerging parenting styles involve more persuasion and shaping of preferences than did older highly directive and often punitive styles. For example, in the directive style, a parent told the children to do their homework, and so it got done. In the persuasive style, parents encourage their children to take responsibility for their homework. The encouragement takes a lot of time (and tact). These newer styles may be more dependent on the "conspiracy of adults" of a pair of parents, with two voices chiming in more effectively. If so, then the deleterious effects of divorce may be increasing over time.

This report will also examine two important hypotheses about the selectivity of divorce. The divorce-selectivity hypotheses propose that there are no negative consequences of divorce per se. Instead, they hold that observed negative consequences of divorce can be attributed to variables omitted from the standard models of the impact of divorce on how much education children complete.

One hypothesis suggests that the apparently deleterious effect of divorce can be attributed to "adverse selection." In this view, parents who divorce were less successful parents anyway, and it is the less successful parenting that impairs education with the divorce having no additional negative effect. To the extent

[1] The respondents were a representative cross section at the full variety of ages present in the adult population. So for young adults, the parental divorce was relatively recent, whereas it was correspndingly longer ago for older adults.

that parental conflict interferes with investment in education, divorce by a highly conflicted pair of parents could enhance educational attainment.

A second key hypothesis proposes that a different omitted variable accounts for the apparently earlier departure from education of children of divorce. It derives from an important line of research in the US suggesting that the negative consequences of divorce are mainly attributable to the fact that families frequently move house in the wake of divorce. According to this line of reasoning, moving house is disruptive to children's education, but divorce has no separate effect of its own. Thus, the claim is that the children of divorce are more likely to move than others, and their education suffers from moving to the same extent as for other movers, but there is no additional deleterious impact of divorce. If so, this has important policy consequences for FaCS. Accordingly, one of the aims of the project is to assess whether moving house (net of other socioeconomic factors) reduces educational attainment in Australia.

This project, as agreed in the accepted proposal, has focused on the effects of divorce from a de jure marriage on children. This focus was proposed because the interest in changes over time requires us to use existing data which have, for decades, inquired into parental divorce as part of their inventory of retrospective family background questions. These data do not inquire into the circumstances of parents in de facto relationships. Several surveys now in the field in Australia inquire (or have recently inquired) into these arrangements for the current population, so contemporary delineations analysis of short-run changes in the effects of dissolution of de facto couples will be possible in complementary projects in the future.

In sum then, this project set out to (1) estimate the effect of divorce on how much education the children of divorce get, (2) assess whether that effect is changing over time, (3) evaluate the degree to which this apparent effect really represents adverse selection that can be accounted for by the characteristics of parents who divorce, (4) discover the degree to which the apparent effect of divorce really reflects deleterious effects of moving house (which is frequently initiated by divorce) rather than by divorce per se.

Motivation

The motivation for the project is to assist FaCS in better targeting policy to assist the children of divorce. This is important to FaCS's brief to support families and aid them in rearing children who will be financially autonomous. As noted above, earlier projects found that the negative effects of divorce on the later employment outcomes of children of divorce can be entirely accounted for by the negative impact of divorce on how much education children get. That means that if the children of divorce were enabled to invest as much in education as their peers from non-divorce families there would be no negative consequence of the divorce for their financial autonomy and success as adults.

If the impact of divorce is decreasing, the policy message is basically "steady as she goes". On the other hand, if the impact of divorce is steady or increasing, the message is that new policy developments are required in this area. The moving-house component is important because it will assist FaCS in (1) assessing the impact of labour market policies that encourage families to move to pursue work opportunities, and (2) assessing whether special support programs are needed for the children of divorce or whether programs should focus on integrating children who have moved house into their new school and neighbourhood. A recent report to DEST discovered no systematic analysis of nationally-representative sample survey data reporting on the consequences of moving house, so the information in this report should help fill an important gap in knowledge.

Data

This section covers several issues: (1) the data source for this report, (2) correcting for upward bias in educational measures (our outcome variables), (3) reliability of retrospective information on some topics where it is not already clearly established, and (4) measurement of the causal variables.

Data source

The data used in this report are from the International Social Science Survey/ Australia (IsssA) conducted by the International Survey Centre under the auspices of the Melbourne Institute of Applied Economic and Social Research at the University of Melbourne. The IsssA collects a variety of types of data, those used in the body of this report are simple pooled cross-sections of primary respondents selected at random from the electoral rolls. The IsssA also collects data on primary respondents' siblings, but those are not used in this report. We omitted them in order to enhance comparability with other projects FaCS might want to conduct and compare on this topic (for example using data from HILDA or from the FaCS longitudinal survey of children treated as a cross-section). The IsssA also collects panel data, but we do not use those in the body of the report as they are not pertinent to the issue at hand which is long-term consequences of childhood experiences for educational outcomes. We do use some of the panel data later in the "Reliability" subsection of this "Data" section to assess the reliability of some of our retrospective measures.

Population sampled

The population sampled by the IsssA consists of citizens of Australia who reside at the address which they have provided to the Electoral Office, who can read English sufficiently well to answer a self-completion questionnaire, and who are not too cognitively impaired to answer a self-completion questionnaire. For simplicity, we refer to this population as "Australians". The selection on citizenship should have little effect, since prior research shows that non-citizen

immigrants differ from citizen immigrants principally in their duration of residence, with few or no differences in issues that would be more relevant to this report, namely marital status and stratification characteristics (Evans 1988)

A note on sample size

The IsssA, unlike most social surveys, is based on a simple random sample. This is the optimal type of sample for most purposes, and the type of sample implicitly assumed by most statistical packages, so ordinary standard errors based on it are correct and do not require the inflating factors that cluster samples do. Simple random samples such as the IsssA are more efficient than the cluster samples used in almost all face-to-face surveys.

A reasonable rule of thumb for high quality cluster designs is that they are worth approximately two-thirds as much as simple random samples (NORC 1987: 435). Thus an IsssA sample of about 2,200 would provide as reliable information as a good cluster sample of around 3,300 cases.

The IsssA-Pool file has 19,058 cases with information on education.

Data collection procedures: IsssA

The IsssA surveys are from simple random samples of Australian citizens drawn by the Electoral Commission from the compulsory electoral roll, a public document. They are conducted by mail using a minor modification of Dillman's (1993) Total Response Method. Details on data collection and processing are in Appendix A: Data, below.

Representativeness

A very important feature of samples is their representativeness, for it is on this basis that one can make generalisations to the large population which is a key goal of most survey research. Indeed, modern survey research textbooks generally emphasise that completion rates/ response rates are only of interest because a very low completion rate may be a symptom of non-representativeness (e.g. Babbie 1995: 262). The representativeness of IsssA achieved samples has been clearly established in prior research (Bean 1991; Sikora 1997), and analyses using IsssA data appear regularly in the world's leading sociology journals.[2] The representativeness of the IsssA with regard to variables other than education is discussed in detail in Appendix A: Data, below.

Results for IsssA surveys conducted around the time of the 1991 Australian census show that the survey samples (1989-1993; 8234 cases) are representative of the population (Evans and Kelley 2002: Table 28.1).[3] There seem to be slightly conflicting results in assessing representativeness according to education: on

[2] For example: Evans, Kelley, and Kolosi (1992); Kelley and De Graaf (1997); Kelley and Evans (1993, 1995).

[3] More extensive comparisons show this as well (Bean 1991; Sikora 1997).

"age left school" measures, the IsssA and the Census match very closely, but there is somewhat more discrepancy on the qualifications.

Correcting for possible upward bias

The ABS provides information on educational qualifications and secondary school completion against which we can calibrate the IsssA data for the most recent cohorts. Such information for the earlier cohorts is provided in (Bean 1991; Sikora 1997; and Evans and Kelley 2002). In the ABS Yearbook 2000 section "Education and Training", subsection "Educational attainment", Table 10.16, information is provided on educational qualifications and, for persons lacking qualifications, on whether or not they completed secondary school. From this we can construct an estimate of the percent completing less than 12 years of education as the sum of the percentage of people without post-school qualifications who did not complete secondary school plus the percent who have a basic vocational qualification (and for whom no information on secondary school completion is available). In 1998, 39% of people aged 25 to 34 (and hence born 1964 to 1973) fall into this category. The IsssA data somewhat underestimate this category (as do nearly all non-ABS surveys), finding 35% in our cohorts born 1965 to 1974. The two numbers are not perfectly comparable, but the differences in definition seems likely to be fairly minor. That difference suggests there is likely to be a small downward bias of 4 percentage points in this category in the IsssA data. For convenience in the remainder of this report we will refer to those with less than 12 years of education has having "incomplete secondary". People with near-complete secondary and a skilled vocational qualification often have incomplete secondary but more than 12 years of education. We group them into the "12 or more years of education" group which, for convenience, we will sometimes call "secondary complete". There is a corresponding small upward bias in the percentage completing twelve or more years of education: the IsssA estimates 65% versus the ABS's 61%. Finally in terms of tertiary qualifications, the IsssA estimates 35%, whereas the ABS estimates 29%. Thus, the IsssA data for the most recent cohorts probably contain an upward bias in educational attainment, as has also been true for the earlier cohorts.

For the purposes of this analysis, the upward bias does not matter greatly, so long as it does not materially differ between people reared in intact families and people reared in divorced families. Unfortunately, ABS data do not normally collect retrospective information, so there is no ready benchmark. However, there is no particular reason to expect such a differential bias between the two groups, so it is reasonable to take as a working assumption that no such bias exists. Hence, the coefficient estimates concerning the difference can be expected to be unbiased, and the predicted values can be corrected for the upward bias. We will at several points in the analysis make predicted values – a simulation in which the values of other variables are fixed to particular values, and the regression model's coefficients are used with those fixed values and with specific

values of the variable of interest to generate average outcomes all else equal. For example, we will examine the probability of completing secondary school for those whose parents have divorced to the corresponding probability for those whose parents have not divorced, all else equal.

Reliability

Random measurement error is a great problem in estimation of effects in social science analysis since it leads to biased and inconsistent coefficient estimates. As a result it is important to use measures that are as reliable as possible. Prior research has established that most of the retrospective measures used in this analysis are highly reliable, but the reliability of the measures of parents' denomination and church attendance[4] has not, to our knowledge previously been assessed so it is worth reporting here.

The relevant parents' religious affiliation variables for this analysis is Catholic denomination, which very highly reliably reported. More specifically, in an IsssA panel contacting respondents in 1989/90 and 1995, the test-retest correlation between respondent's two reports of whether or not their parents were Catholic is 0.873 (where 0 would indicate no connection between the two reports and 1 would indicate an infallibly exact replication).

Parental church-going is also reported with high reliability. For the exact figure, we turn to an IsssA panel contacting the same respondents in 1989/90 and 1995, and asking them about their parents' church attendance on both occasions. The test-retest correlation between reports of parent's denomination in is 0.71 for fathers and 0.70 for mothers (where 0 would indicate no connection between the two reports and 1 would indicate an infallibly exact replication). The difference between fathers and mothers is not statistically significant.

Measurement

Dependent variable: Education

Respondent's education was ascertained by a series of questions on years of primary and secondary schooling and details on highest educational qualification. These were coded into the Australian Bureau of Statistics's 3 digit educational code and recoded into usual years of schooling. From this set of variables we make three dependent variables: (1) years of education, (2), percent with 12 or more years of education (for simplicity, hereafter often referred to as "percent with completed secondary school"), and (3) percent with a tertiary qualification.

[4] These variables are needed in the analysis of parents' divorce which is an important part of assessing the adverse selection hypothesis.

Focal Explanatory Variable: Parental Divorce

The key independent variable is whether or not a respondent's parents had divorced by the time respondent was age 14. In preliminary analyses, we also distinguished whether respondent subsequently lived with their natural mother (usual) or father (rare). This distinction made no appreciable difference , so for simplicity we omit it. A large majority of children of divorce reside with their mothers, so the case base for assessing the differential effect of living with a father rather than a mother is small. Hence, our null finding should be taken as provisional pending replication in future research on even larger samples.[5]

The contrast group is non-divorced families. These are mostly intact married couples, but they also include situations where a parent died and situations where a parent never married. This means that the observed contrast between divorced and non-divorced families probably understates the real contrast between divorced and intact families.

Size of place where family lived when respondent was born and when respondent was age 14 are defined as the natural log of the number of people living in that place. The answers are categorical bands of numbers of people "a Farm or property" (arbitrarily assigned a population size of 10), a "Village (under 1,000)"; "Town (to 20,000)"; 11.00 "Mid-sized city (to 100,000)"; "City (to 500,000)"; Metropolitan (500,000+)." *Change in size of place* is a simple interaction variable scored 1 if size of place at birth and at age 14 do not match, and otherwise scored zero.

International migration by age 14 is a derived variable made by comparing respondent's birthplace with the place where respondent lived at age 14. Those born outside Australia but living in Australia by age 14 are scored 1 and all others are scored zero.

State of residence at age 14 is represented by a set of dummy variables for Queensland, South Australia, Tasmania, Western Australia, and outside Australia, with the reference category being New South Wales and other (a very small number of cases from the ACT and the NT).

Interstate moves by age 14 are derived by comparing state of birth to state of residence at age 14. This is a dummy variable scored 1 for those who moved and zero for those who did not.

Moving house is a variable that is only available recently. The question reads "By age 14, had you moved house (to a different suburb, or further)..." and the

[5] Future studies should continue to assess whether the gender of the custodial parent has any impact, because it is possible that our null finding reflects a genuine effect concealed by a small number of cases. Moreover future research using larger case bases should also explore the further distinction whether the outcomes for children residing in shared arrangements differ from those who reside permanently with mothers and those who reside permanently with fathers.

answer categories are: “Never”, “Once”, “Twice”, “3 times”, “4 times”, “5 times”, “More”.

Note that none of the variables in this study captured the timing of moves, and it is possible that moving at different ages might have different impacts. If so, then we will observe a kind of averaged effect, pooling the effects of moves at different ages.

Control Variables and Supplementary Explanatory Variables

Parents' education. Mother's and father's education was measured by a single direct question with 8 or 9 categories (varying over time), and recoded into approximate years of schooling. For example, the categories for parents' education (recoded into mean years of schooling using data on the actual mean years of schooling for respondents in the corresponding categories) were: None= 0 years; Only a few years of primar=3.9 years; Finished primary=6 years; Left school about age 14 or 15 = 8.9 years; A little more than that=10.1 years; Finished secondary= 12 years; Some further study beyond that = 13.2 years; University or CAE graduate= 15.7 years.

Parents' education is defined as mother's education if only that is known; or father's education if only that is known; or the average of the two if both are known.

Father's Occupational Status / Social Class. The effects of father's occupational status on off spring's education and occupational status have of course been widely studied, most notably in the vast tradition of sociological research stemming from the Blau-Duncan paradigm (Blau and Duncan 1967; Featherman and Hauser 1978). The Blau-Duncan paradigm led to a flowering of research unprecedented in sociology. Robust findings about stratification emerged first for the US (Blau and Duncan 1967; Duncan, Featherman and Duncan 1972) and soon afterward for many other countries, including Britain and Australia in the western industrial world (Broom and Jones 1969); Poland and Hungary in Eastern Europe (Zagorski 1984); and developing and even tribal societies (Kelley 1978). A Kuhnian (1962) "normal science" of social stratification was the outcome. There are many available measures of occupational status, mostly highly correlated. Here we use the Worldwide Status Scores (Kelley 1990:350-56, 1992:23-34; Robinson and Kelley 1979). We do not include a measure of maternal occupation as it was not asked in most of the earlier surveys.[6] The potential effects of maternal employment are also of interest, but we were unable to include it in this exploratory analyses because of difficulties disentangling the causality of divorce and maternal employment (as discussed, below, in the “Models” subsection of the “Methods and Models” section.

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[6] This was mainly a prevalence issue. Recall that our respondents are a cross-section of the adult population, so in our surveys of the 1980s, only the young adults among our respondents had grown up during a time when maternal employment was rising, so a very large majority reported no maternal occupation when we asked it experimentally in the initial 1984/85 survey.. Future analyses, based on large samples of adults in the 21st century should, of course, take mother's occupation into account.

Mediterranean descent. This means having parents from Cyprus, Greece, Italy, Malta, Portugal, Spain, Former Yugoslavia, Slovenia, Croatia, Macedonia, Serbia & Montenegro, or Southern Europe (nfd).[7]

Methods and models

Methods

This report models the relationship between family structure during adolescence and educational outcomes using OLS and logistic regression methods which permit us to incorporate controls for a number of variables known to be associated with both educational attainment and parental divorce.

Data limitations inevitably make it impossible for any researcher to control for all theoretically relevant unmeasured, pre-existing differences. But our models of the effects of divorce include many variables as controls that provide direct measures of potential adverse selection processes. Moreover, results reported by Manski et al. (1992) make it abundantly clear that this may be an area where concerns over adverse selection may be exaggerated. In attempting to determine how the probability of secondary school completion would vary with family structure, if family structure were randomly assigned as in a controlled experiment, Manski and his colleagues show that not only are the errors in their structural and selection equations uncorrelated -- suggesting that family structure is indeed exogenous with respect to secondary school completion -- but the nonparametric bounds they estimate for the probability of secondary school completion have a high probability of including their parametric estimates. For our purposes, this means that not only is it reasonable to assume that family structure is exogenous and the functional form of our models is correct, but, perhaps more importantly, the parameters of parametric models of the kind estimated here are not seriously biased by parents' self-selection into divorce. As extra insurance against the adverse selection problem, the report estimates a logistic regression model predicting parental divorce from parental characteristics and develops from that a control variable that is an estimate of the force of adverse selection rather than of divorce itself. Divorce is normally very hard to predict (which is, in itself evidence against the adverse selection hypothesis), and our model is no exception (the OLS analogue of our logistic regression model yields an R-squared of just over 1 percent).

[7] Education was not widely available in many of the places from which Australia's Mediterranean migrants came, so it is possible that many actually had strong academic abilities despite their low educational attainments, and passed their talents and skills on to their children who went far in Australia's schools. Alternatively, one could argue that the intense familism of Mediterrnaean migrants fostered education. The mechanism could be elaborated in future qualitative research.

Another possibility that we investigate that could account for the observed deleterious effects of divorce on educational attainment is moving house. The methodological strategy that we use here is to enter variables representing different kinds of moves into our models of the effects of divorce to see whether moving has a negative impact and whether including the measures of moving in the models renders the divorce effects non-significant.

Models

All models include an implicit error term which, to save clutter, is not written out. Our models of divorce are given below. The proposal had anticipated also including measures of parental wealth and a measure of maternal employment, but they could not be included in the end. The measure of parental wealth proved too highly correlated with time, so we used an estimate of parental income instead. As to maternal employment, we became concerned that the estimated coefficient was too large to represent a genuine causal effect, and reflection suggested that the problem was likely to be that the causality was unclear, with reciprocal effects between the two being likely. When we removed maternal employment from the model, the effect of parental income suddenly became very large and statistically significant, so basically it was proxying maternal employment (since father's occupation which can be considered a measure of permanent income was already in the model), and would suffer the same problems of causal direction.

Divorce initial model:

Parental_divorce= f (Time, Gender, Parents' education, Fathers' occupation, ,
Urban or rural residence, Parent's_church_going, Parents_Catholic, Number of siblings,
Mediterranean_parents)

Divorce final model (eliminated variables are struck through):

Parental_divorce= f (Time, Gender, ~~Parents' education,~~ Fathers' occupation, ,
Urban or rural residence, Parent's_church_going, ~~Parents_Catholic~~, ~~Number of siblings~~,
Mediterranean_ethnicity)

Effect of divorce on education

Two important social changes need to be recognized by inclusion of interaction terms in our model. First, the effect of father's occupation and parents' education is declining in Australia as in many other nations (Ganzeboom et al. 1989). Second, the effect of gender first shrank and then reversed in that men used to get more education, then there was a cross-over, and now women get more (e.g. Marks 1992). Our preferred model caters for these changes by

including the corresponding interactions. The project was able to obtain satisfactory estimates of the model including these interactions with the OLS estimates for years of education, but for the logistic regression models we were only able to include time interactions for gender and divorce in the final models, because the estimates of the interactions of time with parents' education and father's occupation were rather unstable in sensitivity analyses, and in some versions rather destabilized the whole equation. The models (with initially planned but ultimately eliminated variables struck through) are:

Probability of completing 12 or more years of education
= f (Time, Gender, Parental_divorce, Time_x_Gender_Interaction, Time_x_Parental_Divorce_Interaction. Fathers' occupation , Size_of_place_of_residence (ln), Number of siblings, Books_in_the_home(ln), Mediterranean_ethnicity, ~~Time_x_Father's_Occupation, Time_x_Parents' Eduation)~~

Probability of completing a tertiary qualification
= f (Time, Gender, Parental_divorce, Time_x_Gender_Interaction, Time_x_Parental_Divorce_Interaction. Fathers' occupation , Size_of_place_of_residence (ln), Number of siblings, Books_in_the_home(ln), Mediterranean_ethnicity, ~~Time_x_Father's_Occupation, Time_x_Parents' Eduation)~~

Years of education completed
= f (Time, Gender, Parental_divorce, Time_x_Gender_Interaction, Time_x_Parental_Divorce_Interaction. Fathers' occupation , Size_of_place_of_residence (ln), Number of siblings, Books_in_the_home(ln), Mediterranean_ethnicity, Time_x_Father's_Occupation, Time_x_Parents' Eduation)

Promised moving model:

Education = f(Time, Time interactions with all relevant variables
Respondent's_gender, Bookishness_of_family, Parents' education,
Father's_occupation, Mother's_employment, Parents'_estimated_income,
Number_of_siblings, Urban_or_rural residence, State_of_residence,
National_unemployment_rate,
Moving house between states or categories of size of place or both)

Does the impact of divorce decline over time? Descriptive background

Combining the experience of all cohorts together, Australian children of divorce appear to do just a little worse than children from non-divorced families (Table 1, "Total"). But this modest difference is misleading because it ignores history: Divorce is common only in recent cohorts when educational levels are much higher than they were in the past.

Table 1. Description: Education by cohort and whether parents have divorced or not. Australia, cohorts born up to 1983 (secondary school completion) or up to 1975 (years of education and tertiary qualification). Corrected for upward bias in educational attainment.[c]

		Years of education		% 12 or more years of education		% Tertiary qualification		Number of cases [b]	
Year of birth	% with divorced parents	Non-divorced parents	Divorced parents	Non-divorced parents	Divorced parents	Non-divorced parents	Divorced parents	Non-divorced parents	Divorced parents
Up to 1924	4	8.6	9.5	16	24	2	3	2021	238
1925-34	5	9.3	8.9	19	15*	4	0	2131	249
1935-44	6	10.2	9.6*	28	22*	8	6	2702	331
1945-54	5	11.2	10.2*	39	23*	15	8*	4211	392
1955-64	7	11.7	10.9*	48	31*	17	9*	4058	448
1965+ [a]	10	12.7	11.9*	62	47*	29	26*	1971	306
Total	6	10.7	10.3*	36	29*	17	14	17094	1964

Notes:

[a] Years of education and % university completed for the birth cohorts in and after 1965 as a whole be downward biased in this descriptive table, because many of the younger people in this group who will complete university have not yet done so. Accordingly, the whole group is used in the computations for % who have completed secondary school and the cohorts are limited to 1965-1975 for the computations of years of education completed and % who have completed university.

[b] Note that the number of cases does not mirror the age structure, because these are pooled cross-sections beginning in 1984. Hence, people now in middle and old age have been at risk of being selected into the sample on numerous occasions, whereas the young have only been of eligible age for a short while.

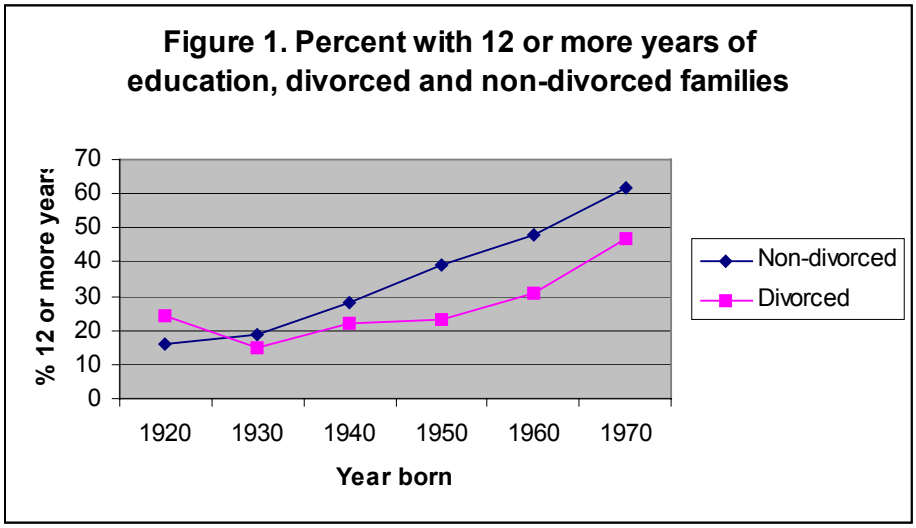
[c] Corrected for upward bias in educational attainment by subtracting one half year of education from thee estimated years of education, 4 percentage points from the estimates for the percentages having 12 or more years of education, and 6 percentage points from the estimates for the percentages having tertiary qualifications.

Strikingly, looking at educational attainments by birth cohort reveals that the size of the deficit increases over time (Table 1 above, and Figure 1, below, especially the percentage completing 12 or more years of education[8]). In early cohorts born before 1935, children from divorced families did about as well educationally as children from non-divorced families, with both groups completing roughly 9 years of education, on average. But in more recent cohorts, the offspring of divorced families have noticeably lower educational attainments, with a deficit of about eight-tenths of a year compared to people from non-

[8] The percentage point gap is a larger fraction of the percentage completing secondary school in the earlier cohorts.

divorced families. The percentage point difference was increasing through the cohorts of the 1960s.[9]

The difference between the two groups in the percentage having completed 12 or more years of education is shown in Figure 1.



This descriptive evidence is certainly not consistent with the hypothesis that the impact of divorce is declining or steady over time. But, based on them, one is not entitled uncritically to accept that the effect of divorce is increasing over time, because these descriptive results take no account of differences between the kinds of families who have divorced and those who have not. Hence they do not yet demonstrate that divorce – rather than something else about divorced families – harms children's education.

To assess whether this apparent increase in the damage done by divorce over cohorts is an artefact of other social changes, we need to turn to multivariate analysis. But before predicting the consequences of divorce, it is necessary to examine which families divorce, to examine the extent to which they were distinctive before divorce. That distinctiveness is then taken into account in our models predicting the consequences of divorce, to lessen the risk that divorce itself is not the problem, but merely a proxy for other factors that impair children's education.

[9] There may be a very small decrease in the very most recent cohorts. That will be taken up further in the multivariate models, below. The crucial point here is that the gap for the cohorts born in the 1950s, 1960s, and 1970s is much larger than it was for earlier cohorts.

Who divorces?

What kind of families divorce? The logistic regression analyses estimate that history plays an important role, even net of many other variables that have been changing over time (Table 2, Panel B, row 1, columns 2 and 6). This model includes time as a dichotomous indicator – whether respondent reached age 14 (the reference point for the parental divorce equation) before or after the Family Law Act was enacted. We have operationalised that as being born before or after 1962. This large and significant effect is very robust, and changes little with the inclusion or omission of other variables in the model (see Panel A, Table 2 and Appendix 1).

The model does not fit very well (fit statistics are in the table, but because of their familiarity the project also computed R-squared for these models estimated by OLS and found that they explain about 1 and one half percent of the variance). This does not mean that there is anything wrong with the estimates of the coefficients and effects in the table, but it does mean that there is probably a very large random component and possibly some unmeasured structured variables causing divorce. Because no other analysis has ever achieved a good fit of a divorce model, either in Australia (e.g. Kelley, Evans, and Headey 2001) or abroad (e.g. Morgan and Rindfuss 1985; Goldscheider and Waite 1991: Table 6.3), it seems likely that most of the unexplained variance is random.

Exploratory models also included other variables not included in the final model for various reasons (some discussed in the Models section), and some of these alternative models are included for perusal in Appendix 1, but one issue is worth mentioning here. If maternal employment is included in the model, it has a highly significant effect on divorce, but we have omitted it from the final model because of doubts about which way the causality goes – to what extent it is wife's employment that increases the risk of divorce and to what extent it is the loss of a husband (or its anticipation) that leads women to increase their workforce participation. We flag it here, because it is an issue that deserves future research attention.

For present purposes, one of the very convenient things about the findings is that two of the variables in the reduced model which appear to be among the most important determinants of divorce would not be expected to affect children's education directly, so the composite index based on the divorce equation will contain information independent of the main effects of the shared variables. The time effect mentioned above is one of these (for the effects of time on education have been ongoing, rather than the effect of time on divorce, which is basically a shift factor) at the time of the Family Law Act.

Table 2. Logistic regression analysis of the probability of parental divorce. The model of Panel A included maternal employment; it is excluded from the model of Panel B. The estimated in Panel B are used later to make a weighted index proxying family traits adverse to continuity in marriage. IsssA-Pool database.

Variable	Coef-ficient	Standard error	Wald	Signifi-cance	Exp(B)
Panel A:					
Born before 1962 (0 or 1)	-0.110	0.071	2	0.119	0.90
Gender (Male=1)	-0.156	0.054	8	0.004	0.86
Parents' education (years)	0.014	0.011	2	0.213	1.01
Father's occupation (0 to 100)	-0.001	0.001	1	0.235	1.00
Mother's work intensity index	1.091	0.070	242	0.000	2.98
Parents Catholic	-0.070	0.066	1	0.286	0.93
Parents' church-going (ln days/year)	-0.168	0.017	101	0.000	0.85
Constant	-2.280	0.130	307	0.000	
Panel B					
Born before 1962 (0 or 1)	-0.259	0.063	17	0.000	0.77
Gender (Male=1)	-0.149	0.049	9	0.002	0.86
Parents' education (years)	0.022	0.010	5	0.029	1.02
Father's occupation (0 to 100)	-0.002	0.001	4	0.034	1.00
Parents Catholic	-0.014	0.059	0	0.812	0.99
Parents' church-going (ln days/year)	-0.187	0.015	154	0.000	0.83
Constant	-1.821	0.115	253	0.000	
Chi-Square change					
Model 1	419.8	7			
Model 2	216.3	6			
Diff	203.5				

In particular, parental church attendance has a substantial effect (Table 2, Panel B, Row 4), showing that the more respondent's parents went to church, the less likely they were to divorce. The preliminary model (Panel A) also included a measure of whether parents were Catholic, because it seemed possible that the specific anti-divorce position of the Catholic church hierarchy could influence Catholics to divorce less than others. But in fact the results suggest that Catholics, at least over the period covered here were just as likely to divorce as were people of other faiths who were equally devout, all else equal. (Remember that church attendance is controlled in the model). It should be noted that it is possible that the negative association between divorce and church attendance comes about because divorced people were stigmatised at church and so stopped attending worship services. Panel data could help resolve this issue in future research.

Another factor that has quite a striking effect on divorce is having parents who migrated to Australia from the Mediterranean region (defined in the "Control variables" subsection of the Data section). Respondents growing up in such families were less likely than children from mixed or non-Mediterranean immigrant families to experience divorce.

Respondent's gender has a small association with parental divorce. Girls are a little more likely to report parental divorce than boys. A similar effect has also been observed in the United States (e.g. Morgan and Rindfuss 1985), so we are not inclined to dismiss it as chance.

Two other variables have statistically significant, but very small, coefficients. (1) Father's occupation (probably best understood here as representing permanent income) is associated with slightly lower divorce risk in this model, net of other factors. (2) For size of place of residence, the coefficient suggests that divorce risk increases slightly as city size increases.

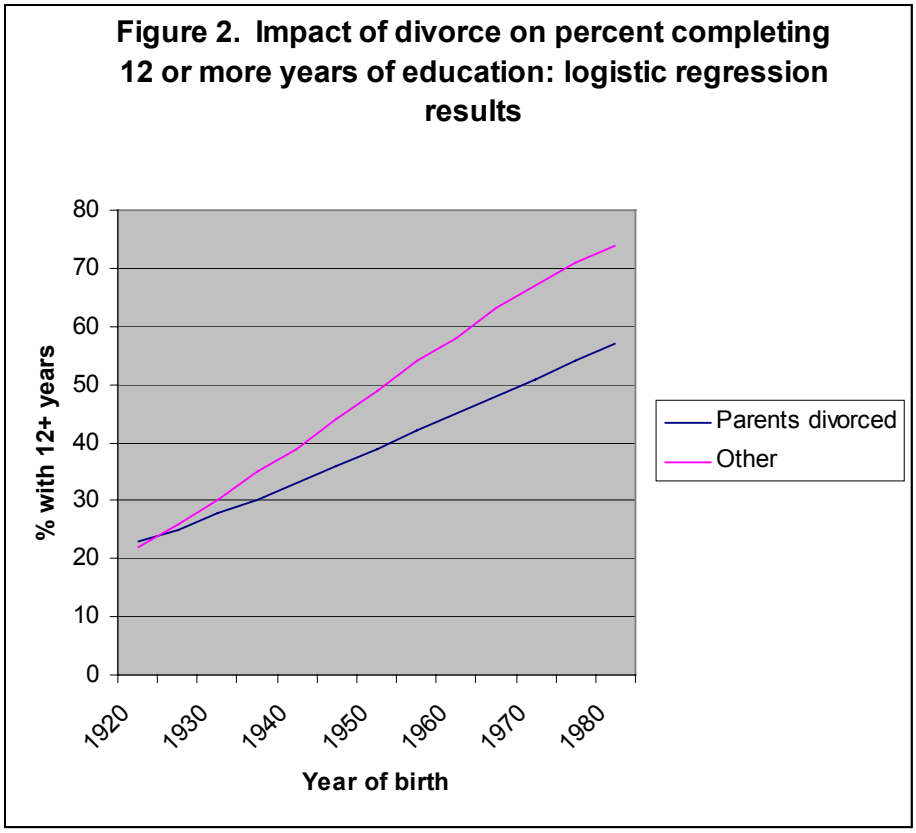
We use this reduced model to create a "selectivity" index of risk of divorce. Later, we enter this index into the model predicting education to see whether including it renders the impact of divorce non-significant. Should it do so, that would be evidence against divorce having an impact on children's educational attainment.

Impact of divorce on educational attainment: Multivariate results

Probability of completing twelve or more years of education

The analysis finds that respondents whose parents have divorced are less likely than others to complete twelve years of education, and that this effect is increasing over time, net of a range of other background variables normally included in models of educational attainment (Figure 2 and Table 3). There is a significant increase in model fit, with a reduction in deviance (-2 likelihood) of 8 with 1 degree of freedom when the time interaction is entered (Table 4).

Turning to the predicted values – a simulation that shows what expected probability people would have of completing twelve years of education if (1) they were average on all characteristics in the model other than the focal ones, and (2) if the "rules of the game" represented by the coefficients held, and (3) they had the specified combinations of birth year and parental divorce in Figure 2.



Notes: These are from a simulation that is based on the model in Panel A of Appendix Table 3. The simulation multiplies each coefficient in Panel A by its mean, except for divorce and time which are sequentially set to each of the years shown in the table for divorced and non-divorced families. These products are then summed and translated from logits back into years of education to obtain the predicted values (1) the probabilities that an otherwise average person (i.e. someone who has the sample's mean values on all other variables) from a divorce family would complete 12 or more years of education across this spann of years, and (2) the probabilities that an otherwise average person (i.e. someone who has the sample's mean values on all other variables) from a non-divorced family would complete 12 or more years of education across this span of years. the values shown in this figure are corrected for possible upward bias in education be deducting 4 percentage points from each value (see the "Data"section for details.

Table 3. Estimates of models of the probability of having 12 or more years of education. Logistic regression estimates.

Panel A: Preferred model. Time interactions with divorce and gender only.

Variable	Coef-ficient	Standard error	Wald	Signifi-cance	Exp(B)
Parents divorced by the time R was age 14 (0 or 1)	0.318	0.269	1	0.237	1.37
Parental divorce by year born interaction	-0.014	0.005	8	0.004	0.99
Year born (year-1900)	0.039	0.002	472	0.000	1.04
Gender (Male=1)	1.262	0.120	111	0.000	3.53
Gender by time interaction	-0.020	0.002	74	0.000	0.98
Parents' education (years)	0.135	0.008	278	0.000	1.15
Books in the home at age 14 (ln)	0.336	0.013	652	0.000	1.40
Father's occupation (0 to 100)	0.011	0.001	211	0.000	1.01
Siblings (#)	-0.107	0.010	119	0.000	0.90
Size of place (ln)	0.024	0.005	24	0.000	1.02
Parents Mediterranean migrants (0 or 1)	0.765	0.083	85	0.000	2.15
Constant	-5.411	0.131	1720	0.000	

Panel B: No time interaction with divorce, but does include time interactions with father's occupation and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	-0.432	0.074	34	0.000	0.65
Year born (year-1900)	0.050	0.005	105	0.000	1.05
Gender (Male=1)	1.262	0.121	110	0.000	3.53
Gender by time interaction	-0.020	0.002	73	0.000	0.98
Parents' education (years)	0.179	0.026	48	0.000	1.20
Parents' education by time interaction	-0.001	0.001	3	0.079	1.00
Books in the home at age 14 (ln)	0.334	0.013	643	0.000	1.40
Father's occupation (0 to 100)	0.015	0.002	40	0.000	1.02
Father's occupation by time interaction	-0.009	0.005	3	0.079	0.99
Siblings (#)	-0.107	0.010	119	0.000	0.90
Size of place (ln)	0.024	0.005	24	0.000	1.02
Parents Mediterranean migrants (0 or 1)	0.750	0.083	81	0.000	2.12
Constant	-5.931	0.254	546	0.000	

Panel C: Includes time interactions with divorce, gender, father's occupation, and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	0.288	0.271	1	0.287	1.33
Parental divorce by year born interaction	-0.014	0.005	8	0.006	0.99
Year born (year-1900)	0.050	0.005	105	0.000	1.05
Gender (Male=1)	1.279	0.121	112	0.000	3.59
Gender by time interaction	-0.020	0.002	75	0.000	0.98
Parents' education (years)	0.175	0.026	46	0.000	1.19
Parents' education by time interaction	-0.001	0.001	3	0.110	1.00
Books in the home at age 14 (ln)	0.335	0.013	645	0.000	1.40
Father's occupation (0 to 100)	0.015	0.002	41	0.000	1.02
Father's occupation by time interaction	-0.009	0.005	3	0.077	0.99
Siblings (#)	-0.106	0.010	117	0.000	0.90
Size of place (ln)	0.024	0.005	24	0.000	1.02
Parents Mediterranean migrants (0 or 1)	0.749	0.083	81	0.000	2.11
Constant	-5.948	0.254	548	0.000	

The predicted values in Figure 2 reveal that there was little or no difference between children from divorced and non-divorced families in the probability of completing twelve years of education for people born early in the last century, all else equal. The emerging difference had grown to a five percentage point gap between divorce and non-divorced families by the birth cohort (or "vintage") of 1935, to 10 percentage points by the birth cohort of 1950, and to 15 percentage points by the cohort of 1950. The gap since then is still growing, but more slowly, reaching 16 points for the cohort of 1960, and 17 for the cohorts of 1970 and 1975.[10]

If the growth of the difference has come to an end, that is an important point. One could obtain more precise estimate of the recent probabilities by pooling this dataset with the HILDA dataset to achieve a quite large sample size for these recent cohorts.[11]

Table 4. Fit statistics for Table 3.

	Deviance[a]	Chi-square	Degrees of freedom
Initial	25008.9		
Model A	20540.1	4468.812	11 df
Model B	20539.0	4469.865	12 df
Model C	20531.8	4477.14	13 df

Notes:[a] Deviance (-2 log likelihood)

Probability of acquiring a tertiary qualification

Our next topic is the influence of parental divorce on their offspring's chances of completing a tertiary qualification, net of other background influences likely to affect education. One could model the effect of divorce on the risk of acquiring a tertiary qualification either for the whole population over age 25, or only for those who have completed twelve or more years of education and hence who are the ones mainly eligible to enter courses leading to tertiary qualifications. The latter is a crisper comparison (because the former does not net out the effects of twelve years of education), but the former is closer to the way that the way that the issue is often presented, so it may be more familiar. In the event it does not make much difference, for the results are much the same either way, but,

[10] The finding of no difference between children of divorced and non-divorced families in years of education completed for cohorts born in the 1920s is interesting. We are inclined to interpret it as reflecting the predominance of older, more authority based parenting styles which could be implemented effectively by one parent. But it could also be that there are other changing features of the cultural climate that account for the fact. This is an issue which historical research could illuminate.

[11] One might particularly wish to investigate the introduction of FAS, secondary Austudy, Youth Allowance and, perhaps most importantly, the Child Support Scheme in this pooled dataset.

predictably, are somewhat clearer when the equation is estimated for those with twelve or more years of education, who better approximate the eligible ("at risk") population (Table 5) than when it is estimated for all over age 25 (Appendix Table 3).

The estimates in Table 5 are that the coefficients representing the effects of parental divorce on acquiring a tertiary qualification are not statistically

Table 5. Estimates of the effect of divorce on completing a tertiary qualification (with controls). Persons over age 25 who completed twelve or more years of education only (N=6205). Logistic regression estimates.

Panel A: Model A'. Time interaction gender only.

Variable	Coef-ficient	Standard error	Wald	Signifi-cance	Exp(B)
Parents divorced by the time R was age 14 (0 or 1)	-0.032	0.126	0	0.798	0.97
Year born (year-1900)	0.015	0.003	22	<0.005	1.01
Gender (Male=1)	1.345	0.208	42	<0.005	3.84
Gender by time interaction	-0.019	0.004	22	<0.005	0.98
Parents' education (years)	0.018	0.012	2	0.125	1.02
Books in the home at age 14 (ln)	0.132	0.021	39	<0.005	1.14
Father's occupation (0 to 100)	0.006	0.001	25	<0.005	1.01
Siblings (#)	-0.050	0.016	10	0.002	0.95
Size of place (ln)	0.032	0.008	16	<0.005	1.03
Parents Mediterranean migrants (0 or 1)	0.042	0.140	0	0.765	1.04
Constant	-2.396	0.216	123	<0.005	

Panel B: No time interaction with divorce, but does include time interactions with father's occupation and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	-0.042	0.126	0	0.741	0.96
Year born (year-1900)	-0.014	0.008	3	0.078	0.99
Gender (Male=1)	1.302	0.207	39	0.000	3.68
Gender by time interaction	-0.018	0.004	20	0.000	0.98
Parents' education (years)	-0.130	0.039	11	0.001	0.88
Parents' education by time interaction	0.003	0.001	15	0.000	1.00
Books in the home at age 14 (ln)	0.134	0.021	40	0.000	1.14
Father's occupation (0 to 100)	0.008	0.004	4	0.054	1.01
Father's occupation by time interaction	-0.004	0.008	0	0.634	1.00
Siblings (#)	-0.052	0.016	11	0.001	0.95
Size of place (ln)	0.031	0.008	15	0.000	1.03
Parents Mediterranean migrants (0 or 1)	0.112	0.141	1	0.428	1.12
Constant	-1.026	0.403	7	0.011	

Panel C: Includes time interactions with divorce, gender, father's occupation, and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	-0.345	0.463	1	0.456	0.71
Parental divorce by year born interaction	0.006	0.009	0	0.495	1.01
Year born (year-1900)	-0.014	0.008	3	0.073	0.99
Gender (Male=1)	1.295	0.208	39	0.000	3.65
Gender by time interaction	-0.018	0.004	20	0.000	0.98
Parents' education (years)	-0.129	0.039	11	0.001	0.88
Parents' education by time interaction	0.003	0.001	15	0.000	1.00
Books in the home at age 14 (ln)	0.134	0.021	40	0.000	1.14
Father's occupation (0 to 100)	0.008	0.004	4	0.053	1.01
Father's occupation by time interaction	-0.004	0.008	0	0.634	1.00
Siblings (#)	-0.052	0.016	11	0.001	0.95
Size of place (ln)	0.031	0.008	15	0.000	1.03
Parents Mediterranean migrants (0 or 1)	0.115	0.141	1	0.418	1.12
Constant	-1.015	0.403	6	0.012	

significant whether or not one includes the time interaction in a variety of specifications (model A, not shown, is like model A' but includes the time interaction. The fit statistics are shown in Table 6.

Thus, in the various alternative models examined here, the effect of parental

Table 6. Fit statistics for Table 5.

	Deviance[a]	Chi-square	Degrees of freedom
Initial	8321.4		
Model A'	8072.2	249.2	10
Model A	8071.5	249.9	11
Model B	8055.3	266.1	12
Model C	8054.9	266.5	13

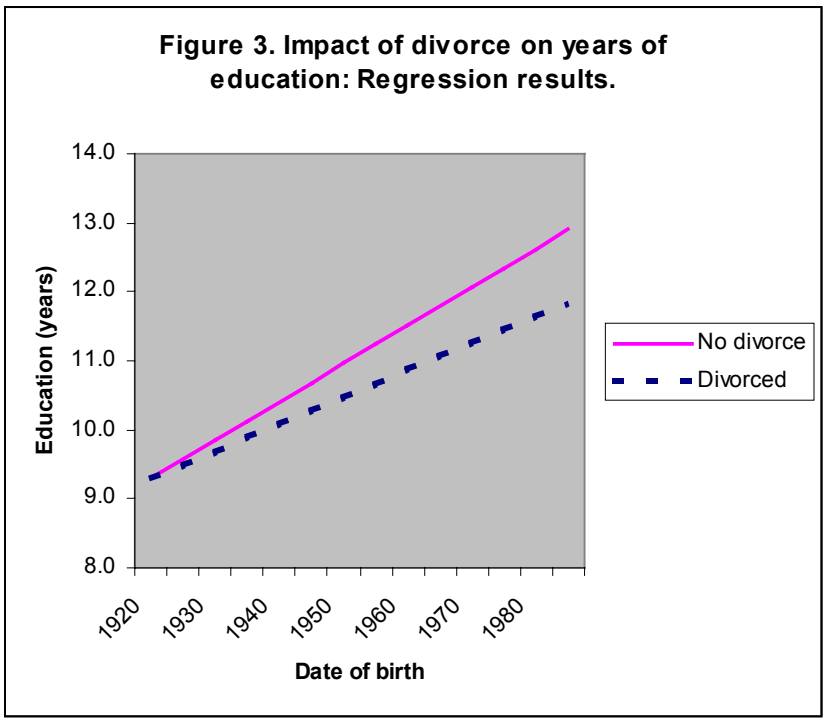
Notes:[a] Deviance (-2 log likelihood)

divorce and the interaction of parental divorce with time do not contribute to model fit and fail standard tests of statistical significance.[12] It is reasonable to interpret these results as indicating that, on the available evidence, parental divorce does not reduce the chances of completing a tertiary qualification among those who have completed twelve or more years of education.

Years of education

As one would expect from the analysis of secondary school completion, there is a clear connection between time, divorce, and years of education in these models (Figure 3 and Table 7). To show how the effect grows over time, it may be useful to turn to the results of a simulation generating predicted values of years of education from the estimates of our models with all other variables set to their means. This yields a view of what average years of education would have been for people from divorced and non-divorced families by year of birth, had they had the average values for the sample as a whole, and had the "translations" of all the family background characteristics into education represented by the estimated coefficients in the model held.

[12] Note that the interaction with time is based on the implicit hypothesis that selectivity into divorce should be decreasing over time as divorce has become readily available and socially acceptable. It seems likely that divorces in earlier times were much more selective on unmeasured variables such as adverse personality characteristics and substance abuse. However, the fact that the time interaction is not statistically significant suggests that either this is incorrect or that it is irrelevant ot educational attainment. Remember that the selectivity does not refer to socio-economic status which is controlled in the model explicitly.



According to the simulation, among the cohorts born around 1920 there was no difference in years of education completed between people raised in divorced and non-divorced families (Figure 3). By the cohorts born around 1940, people from divorced families were getting about 0.3 of a year less education, on average, than otherwise similar people from non-divorced families. That gap had widened to 0.6 of a year by the cohorts born around 1960, and to about 1.0 years by the cohorts born around 1980.

Table 7. Estimates of models of years of education, persons over age 25 only. OLS estimates. R-squared for all models=0.33.

Panel A: Time interactions with divorce and gender only.

Variable	Coef-ficient	Standard error	Standar-dised	t-value	Signifi-cance
Parents divorced by the time R was age 14 (0 or 1)	0.340	0.299	0.03	1.14	0.2557
Parental divorce by year born interaction	-0.016	0.006	-0.06	-2.67	0.0076
(Joint F for parental divorce and its interaction= 15.4 with 2 df (p<.001)					
Year born (year-1900)	0.062	0.002	0.30	31.76	<.0005
Gender (Male=1)	1.133	0.124	0.19	9.10	<.0005
Gender by time interaction	-0.014	0.003	-0.11	-5.15	<.0005
Parents' education (years)	0.168	0.009	0.15	18.76	<.0005
Books in the home at age 14 (ln)	0.448	0.014	0.24	32.36	<.0005
Father's occupation (0 to 100)	0.016	0.001	0.13	16.85	<.0005
Siblings (#)	-0.166	0.011	-0.10	-15.60	<.0005
Size of place (ln)	0.048	0.006	0.06	8.76	<.0005
Parents Mediterranean migrants (0 or 1)	-0.110	0.098	-0.01	-1.13	0.2582
Constant	4.342	0.127		34.06	<.0005

Panel B: No time interaction with divorce, but does include time interactions with father's occupation and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	-0.430	0.087	-0.03	-4.93	<.0005
Year born (year-1900)	0.100	0.005	0.48	21.07	<.0005
Gender (Male=1)	1.111	0.124	0.18	8.95	<.0005
Gender by time interaction	-0.013	0.003	-0.10	-4.96	<.0005
Parents' education (years)	0.316	0.025	0.28	12.64	<.0005
Parents' education by time interaction	-0.003	0.001	-0.21	-6.23	<.0005
Books in the home at age 14 (ln)	0.446	0.014	0.24	32.27	<.0005
Father's occupation (0 to 100)	0.027	0.003	0.22	9.93	<.0005
Father's occupation by time interaction	-0.025	0.006	-0.11	-4.30	<.0005
Siblings (#)	-0.161	0.011	-0.10	-15.22	<.0005
Size of place (ln)	0.047	0.006	0.06	8.60	<.0005
Parents Mediterranean migrants (0 or 1)	-0.140	0.097	-0.01	-1.44	0.1498
Constant	2.709	0.226		11.97	<.0005

Panel C: Includes time interactions with divorce, gender, father's occupation, and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	0.203	0.299	0	0.679	0.4969
Parental divorce by year born interaction	-0.013	0.006	0	-2.214	0.0268
(Joint F for parental divorce and its interaction= 15.4 with 2 df (p<.001)					
Year born (year-1900)	0.100	0.005	0	21.116	<.0005
Gender (Male=1)	1.119	0.124	0	9.011	<.0005
Gender by time interaction	-0.014	0.003	0	-5.013	<.0005
Parents' education (years)	0.313	0.025	0	12.497	<.0005
Parents' education by time interaction	-0.003	0.001	0	-6.086	<.0005
Books in the home at age 14 (ln)	0.446	0.014	0	32.292	<.0005
Father's occupation (0 to 100)	0.027	0.003	0	9.937	<.0005
Father's occupation by time interaction	-0.025	0.006	0	-4.322	<.0005
Siblings (#)	-0.160	0.011	0	-15.129	<.0005
Size of place (ln)	0.047	0.006	0	8.588	<.0005
Parents Mediterranean migrants (0 or 1)	-0.142	0.097	0	-1.455	0.1456
Constant	2.697	0.226	548	11.917	<.0005

Does adverse selection account for the effect of divorce?

The possibility remains that the apparent negative effect of divorce on educational attainment found in our models is more apparent than real, because it is pre-existing unmeasured characteristics of families who divorce that both increase the risk of divorce and produce reduced educational attainments. Even without the analysis to follow, it is worth mentioning that this does not seem tremendously likely to be so for several reasons. (1) Divorce is very difficult to predict, so it is unlikely to be highly selective. (2) Divorce has become more common, so it is likely that it has, if anything, become less selective on unmeasured characteristics, so the impact ought to be decreasing rather than increasing. (3) Many characteristics that influence educational attainment are controlled in the model. (4) Other research has shown that parental conflict, one of the key candidate selection variables, does not, in fact, affect educational attainments.

Nonetheless, it is worth examining the adverse selection hypothesis here. To this end, this project constructed an "adverse selection" proxy variable from the model of divorce presented above in the section" Who Divorces?". Basically, the coefficient estimates from that model give a set of weights that are applied to each respondent's characteristics to generate individual-level estimates of the degree to which respondent's family was adversely selected. To assess whether this measure accounts for the effect of divorce on children's education, we include it in the models presented above, and examine how it changes their behaviour.

To assess the impact of this adverse selection indicator, Table 8, Section 1 revisits the estimates of the coefficient representing the effects of divorce on offspring's education in Tables 3 and 7, and compares them to the estimates from those models augmented by the adverse selection indicator. Fit statistics for the original and augmented models are given in Table 9. Missing data differ slightly from the estimates in Table 3; so estimates of the original and augmented models on exactly the same cases are given in the Appendix.

Table 8 shows that the parameter estimates of the effects of divorce on the probability of completing 12 or more years of education differ little by whether or not the index of adverse selection is included in the model. Table 9 shows how the fit develops beginning with a baseline model that includes neither the adverse selection index nor parental divorce, then enters the adverse selection index in the next step and assesses the fit, and then enters parental divorce. The results clearly show that both adverse selection and parental divorce make separate, statistically significant contributions to fit.

Table 8. Estimates of models of the probability of completing twelve or more years of education and estimates of models of years of education, with and without adverse selection proxy variable.

Section 1: Dependent variable= probability of completing 12 or more years of education.					
Model A (Table 3):	Coef-ficient	Standard error	Wald	Signifi-cance	Exp(B)
Parents divorced by the time R was age 14 (0 or 1)	0.318	0.269	1.4	0.237	1.37
Parental divorce by year born interaction	-0.014	0.005	8.3	0.004	0.99
Model A plus adverse selection indicator					
Parents divorced by the time R was age 14 (0 or 1)	0.321	0.274	1.4	0.242	1.38
Parental divorce by year born interaction	-0.013	0.005	6.2	0.013	0.99

Section 2: Dependent variable: Years of education completed	Metric regression coefficient	Standard error	Standar-dized coefficient	T	Sig T
Model C (Table 7):					
Parents divorced by the time R was age 14 (0 or 1)	0.205	0.299	0.015	0.684	0.494
Parental divorce by year born interaction	-0.014	0.006	-0.050	-2.213	0.027
Model C plus adverse selection indicator					
Parents divorced by the time R was age 14 (0 or 1)	0.217	0.298	0.016	0.727	0.468
Parental divorce by year born interaction	-0.012	0.006	-0.044	-1.966	0.049

Table 8 also presents estimates of the effects of divorce on years of education completed, and here again comparing the parameter estimates shows that they remain much the same regardless of whether the adverse selection index is included. Similarly, the tests on changes in F-values with the inclusion of the adverse selection index and the divorce measures shows that the divorce measures make a statistically significant contribution to fit, above and beyond

that contributed by the adverse selection index (Table 9). Note that the adverse selection index has a quite notable linkage to educational attainment – both in regards to the probability of completing twelve or more years of education and in regards to the number of years of education completed – as shown by the fit statistics (Table 9) and the standardised regression coefficient (Appendix Table 6). The point is that divorce per se, in these models, has a statistically significant impact above and beyond that of the adverse selection index, and, moreover, that inclusion of the adverse selection index has little impact on the magnitude of the impact of divorce as measured by the coefficient estimates.

Table 9. Fit statistics for Table 8.

	Deviance[a]	Chi-square	Degrees of freedom
Pr (get 12 or more years of education), based on Model A, Table 3			
A1. Model A (Alternative 1): without parental divorce, or interaction of divorce and time, or adverse selection index	20406.6	4468.8	9 df
A2. Model A (alternative 2): A1 plus adverse selection	**20225.9**	**4216.9**	10 df
A3. Model A (alternative 3): A (alternative 2) plus divorce indicators	**20198.6**	**4189.6**	11
Years of education, based on Model C, Table 7	F-model	F-Change	Degrees of freedom
C1. Model C (Alternative 1): without parental divorce, or interaction of divorce and time, or adverse selection index	711.5		11
C2. Model C (alternative 2): C1 plus adverse selection	671.4	**153.9**	12
A3. Model A (alternative 3): A (alternative 2) plus divorce indicators	577.5	**9.7**	14

Thus, on the evidence available here, adverse family characteristics associated with divorce as proxied by our adverse selection index have a strong linkage to education, but divorce per se has a statistically significant additional impact.

Next this report addresses the question of whether moving house accounts for the effect of divorce on educational attainments.

Does moving house during childhood have deleterious effects on children's educational attainment?

There has been an argument, mainly in the US literature, that moving house during childhood has a deleterious impact on educational attainment, and that this impact entirely accounts for the deleterious effect of divorce on children's education. As the impact of moving house on educational attainment has never previously been studied in a nationwide representative sample in Australia, this section experiments with a variety of alternative models. There are constraints about what variables are available in existing data, although, presumably in several decades the FAC's Longitudinal Study of Australian Children will be able to provide definitive results.

To begin, let us examine the effects of changes in size of place – moving from a large city to a small one, and so on. Recall that in the models discussed thus far, size of place has a positive effect on education (as is also true in other models and other surveys in Australia). To set the stage, a comparison of rows 1 and 2 of Table 9 shows the significant gain in fit associated with place of residence at age 14 in the preferred Model A of Table 3, predicting completion of twelve or more years of education. The next model, whose fit statistics are summarised in row 3, adds in size of place where the family was living when respondent was born (see also the parameter estimates are in Appendix Table 7, Panel A). The estimate of the coefficient is non-significant, suggesting that information about the place where one started one's life does not make a statistical contribution to the explanation of the probability of completing twelve or more years of education net of size of place of residence at age 14. A further model, in Row 4, explores whether a measure of change in size of place (the difference between size of place at age 14 and size of place at birth) has a statistically significant effect. The fit statistics show that entering this variable does not significantly improve the fit of the model. Thus it seems reasonable to say that in these models, moving among different size of place categories between birth and age 14 does not adversely affect education, all else equal.

The next question is whether taking such moves into account renders the effect of parental divorce on children's education non-significant. Row 5 of Table 9 shows that omitting parental divorce and the interaction of parental divorce and time from Model 4 of this table significantly reduces the fit. Thus, the evidence here is that moving house from larger to smaller (or smaller to larger) sized places does not account for the impact of divorce on education.

Turning to the chances of completing a tertiary qualification, the results show that the larger the size of place of residence at age 14, the greater the chances of completing a tertiary qualification (Table 9, row 7, and Appendix Table 9). Here

Table 9. Impact of size of place of residence at age 14 and of moving between size of place of residence categories on educational attainment. Significant differences at p<.001 are in boldface.

Dependent variable: Completion of 12 or more years of education	Chi-square	Change in chi-square	df
1. Baseline (Model A of Table 3 but omitting size of place of residence at age 14)	2964.34		10
2. Adds size of place of residence at age 14 to Model 1 of this table		**15.867**	1
3. Adds size of place of birth to Model 2 of this table		0.13	1
4. Adds interaction of size of place at birth and at age 14 to Model 3 of this table		1.474	1
5. Removes divorce measures from Model 4 of this table		**-9.202**	2
Dependent variable: Completion of tertiary qualification			
6. Baseline: Model A of Table 5, but omitting size of place of residence at age 14	149.87		9
7. Adds size of place of residence at age 14 to Model 6 of this table		10.464	1
8. Adds size of place of birth to Model 7 of this table		0.239	1
9. Adds interaction of size of place at birth and at age 14 to Model 8 of this table		1.183	1
10. Removes divorce measures from Model 9 of this table	na	na	na
Dependent variable: Years of education completed	**Model F**	**F of this variable**	**df in this test**
11. Baseline: Model C, Table 9, but omitting size of place of residence at age 14	394.03		
12. Adds size of place of residence at age 14 to Model 11 of this table		**50.41**	1
13. Adds size of place of birth to Model 12 of this table		0.07	1
14. Adds interaction of size of place at birth and at age 14 to Model 13 of this table		1.81	1
15. Removes divorce measures from Model 14 of this table		**9.87**	2

neither size of place of residence of the family at the time of respondent's birth, nor the difference between size of place of residence at age 14 and at birth had a statistically significant impact on obtaining a tertiary qualification (rows 8 and 9). Because the earlier analysis of Table 5 indicates that parental divorce does not impact the probability of obtaining a tertiary qualification among those who complete 12 or more years of education, there are no tests in Row 10 of whether the impact of divorce is accounted for by movement. Note that these are logistic regression models estimated only for people who completed 12 or more years of education.

Finally, the bottom panel of Table 9 presents the F-statistics associated with the OLS models of years of education (unlike the previous panel, these are estimated

for all respondents). Here, too, there is a statistically significant improvement in fit associated with size of place of residence at age 14 (Row 12). By contrast, there is no significant improvement in fit with size of place of residence at birth(Row 13), nor with the difference between size of place of residence at birth and at age 14 (Row 14). The results in Row 15 show that in this model the variables representing parental divorce and the interaction of parental divorce and year born jointly have a statistically significant impact net of the location and moving variables on years of education completed.

Thus, there are two conclusions to be drawn from this evidence. (1) Moving house between size of place categories does not have a negative effect on educational attainment. (2) Moving house between size of place categories does not account for the negative effects of divorce on education.

Next, this report addresses the issues of international and interstate moves.

Turning first to international migration, we added international migration to the preferred Model A of Table 3 of completing twelve or more years of education. This yields a significant improvement to fit of the logistic regression model. Substantively, people who moved to Australia from abroad as children are significantly more likely than otherwise comparable native born Australians to complete twelve or more years of education, in this model (Table 10, Row 2). But this movement effect clearly does not account for the negative effect of parental divorce on children's educational attainment. Removal of the parental divorce effects from the augmented Model A worsens the fit as indicated by the significant change in chi-square (Table 10, Row 3).

Among those who have completed twelve or more years of education, the effect of migrating into Australia on obtaining a tertiary qualification is probably positive as well. Adding the immigration by age 14 dummy variables into the preferred Model A of Table 5 yields an improvement in fit that is significant at the .01 level, but not the .001 level (Table 10, Row 7). The estimates of Table 5 revealed nonsignificant impact of parental divorce on the chances of obtaining a tertiary qualification among people who have completed twelve or more years of education. Accordingly, there is no point in testing whether international migration accounts for what probably does not exist.

Adding international migration by age 14 into the preferred Model C of Table 7 predicting years of education completed significantly improves the fit of the OLS regression model (Table 10, Row 12). This effect, however, does not account for the effect of parental divorce on children's education: removing the parental divorce measures from the preferred Model C of Table 7 augmented by the international migration measure significantly worsens the fit (Table 10, Row 13).

Table 10. Impact of international and interstate moves on educational attainment. Significant differences at p<.001 are in boldface, p<.01 are in italics.

Dependent variable: Completion of 12 or more years of education	Chi-square	Change in chi-square	df
1. Baseline: Model A of Table 3	4468.812		11
2. Adds immigration by age 14 to baseline		**136.08**	1
3. Removes divorce measures from Model 2 of this table		**-229.00**	2
4. Adds state of residence at age 14 to baseline		**78.10**	5
5. Adds interstate moves to Model 4 of this table		0.35	1
6. Removes divorce measures from Model 5 of this table		**-41.38**	2
Dependent variable: Completion of tertiary qualification			
6. Baseline: Model A of Table 5	249.9		
7. Adds immigration by age 14 to baseline		*8.04*	1
8. Adds state of residence at age 14 to baseline		11.57	5
9. Adds interstate moves to Model 4 of this table		0.70	1

Note: The effect of parental divorce is not statistically significant in Model A of Table 5, so there is no "divorce effect" to account for and there would be little point in testing for whether its removal from models 7 or 9 decreases fit.

Dependent variable: Years of education completed	Model F	F of this variable	df in this test
10. Baseline: Model C, Table 7	562.92		
11. Adds immigration by age 14 to baseline		**46.09**	1
12. Removes divorce measures from Model 11 of this table		**15.99**	1
13. Adds state of residence at age 14 to baseline		**8.59**	5
14. Adds interstate moves to Model 13 of this table		0.44	1
15. Removes divorce measures from Model 14 of this table		**15.07**	2

Thus, in these models, international migration by age 14 has a significant effect on the probability of completing twelve or more years of education, net of an array of family socioeconomic and background characteristics. Moreover, it also significantly enhances the probability of obtaining a tertiary qualification (among those with twelve or more years of education) and the number of years of education completed. In sum, this form of moving house appears to be related to children's education. However, this effect is additional to that of divorce rather than explaining it.

Next, this report addresses the question of the impact of interstate moves on educational attainment.

First, note that adding state of residence at age 14 to the preferred model significantly improves the fit of preferred Model A of Table 3 predicting the probability of completing twelve years of education using logistic regression (Table 10, Row 4). It also improves the fit of preferred Model C of Table 7 predicting the number of years of education completed using OLS regression (Table 10, Row 14). The evidence about its impact on the fit of Model A of Table 5 predicting the probability of obtaining a tertiary qualification (among people who have completed twelve or more years of education) is more ambiguous, being significant at the 0.05 level, but not the 0.01 level.

Second, at least in these models, it seems that interstate moves do not have a significant impact on educational attainment, all else equal. Adding the measure of interstate moves to the preferred models of (1) the probability of completing twelve or more years of education, (2) the probability of obtaining a tertiary qualification (among those who have completed twelve years or more of education) and (3) the number of years of education completed yields nonsignificant changes to fit in all three cases (Table 10, Rows 5, 9, and 15).

Third, the state of residence at age 14 effects do not account for the effects of parental divorce on their offspring's educational attainment. When the parental divorce indicators are removed from the augmented models including state of residence at age 14 and interstate moves as well as the variables in the original preferred model of completing twelve or more years of education and the original preferred model of the number of years of education completed, the fit is significantly worse (Table 10, Rows 6 and 16).

Thus, in these models, state of residence influences educational outcomes, but interstate moves do not. The models do not detect any significant negative impact of interstate moves, and such moves do not account for the negative impact of divorce.

Next, this report presents results from the 2000/2001 IsssA survey which includes an explicit question on the number of times that the respondent had moved house to another suburb or further by the age of 14.

The results show that including the number of times a respondent moved house to another suburb or further by the age of 18 in the preferred Model A of Table 3 predicting completion of twelve or more years of education does not make a statistically significant impact on the fit of the model (Table 11, Row 2). However, with just 1581 cases available, this should be taken as a tentative rather than a definitive finding. This is especially true for the logistic regression analysis which is not as powerful a method as OLS.

Table 11. Impact of the number of moves by age 14 on educational attainment. Significant differences at p<.01 are in italics.

Dependent variable: Completion of 12 or more years of education	Chi-square	Change in chi-square	df
1. Baseline: Model A of Table 3	436.743		11
2. Adds number of moves to another suburb or further by age 14 to baseline		1.47	1
3. Removes divorce measures from Model 2 of this table		3.22	2
Dependent variable: Completion of tertiary qualification			
4. Baseline: Model A of Table 5	47.4		
5. Adds number of moves to another suburb or further by age 14 to baseline		0.05	1

Note: The effect of parental divorce is not statistically significant in Model A of Table 5, so there is no "divorce effect" to account for and there would be little point in testing for whether its removal from models 7 or 9 decreases fit.

Dependent variable: Years of education completed	Model F	F of this variable	df in this test
6. Baseline: Model C, Table 7	42.29		
7. Adds number of moves to another suburb or further by age 14 to baseline		3.48	1
8. Removes divorce measures from Model 7 of this table		*9.48*	2

Nor does the number of moves by age 14 have a statistically significant impact on the qualification of a tertiary qualification. As shown in Table 11, Row 5, adding the number of moves by age 14 into the preferred Model A of Table 5 does not lead to a statistically significant improvement in fit.

Finally, including the number of moves by age 14 into the preferred Model C of Table 7 does not significantly improve the fit (Table 11, Row 7), although it is a closer call. The coefficient is negative and close enough to statistical significance that the "no effect" conclusion should be treated as tentative rather than definitive. This result should be reassessed in larger samples as future editions of the IsssA or replicate questions in other surveys become available.

Thus, the finding of an increasing impact of divorce on children's education is found in both the logistic regression analysis of the probability of completing twelve or more years of education and in the OLS estimates of years of education completed.[13] It is not sensitive to model specification across the range of alternative models estimated in this report. The OLS models reported here do not detect the end of the widening of the gap that was found in the logistic regression models of the probability of acquiring 12 or more years of education Preliminary estimates of models that also included quadratics and quadratic interactions were very unstable, probably because more cases in the most recent cohorts are needed for stable estimates. A possible future project pooling these data with the HILDA data would provide a large number of cases for the more recent cohorts and thus would provide more definitive estimates. Models of the effect of divorce on the probability of gaining a tertiary qualification find all non-significant effects in a variety of model specifications. This suggests that the events or situations that bring about deleterious effects of divorce on education are concentrated before the usual ages of tertiary study, with the ages before 18 being a good place to explore these issues.

This report also examined the adverse selection hypothesis which claims that divorce itself really has no effect on children's education, but rather that it is pre-existing adverse characteristics of the family of origin that lead the parents to divorce and lead to less education than would be expected on the basis of family socioeconomic position. This issue was addressed by developing an index of adverse characteristics based on the coefficients from a logistic regression equation modelling parental divorce, and entering this index into the models of education. This index has a substantial negative statistical link to education, even net of a wide array of family background characteristics such as socioeconomic status, but inclusion of it in the models does not render the effect of divorce non-significant. At least with the measurements and models used here, it seems that adverse family characteristics do lead to less education than would be expected on the basis of family socioeconomic characteristics, but that effect is largely additional to, rather than accounting for, the effect of divorce.

Another hypothesis that has been put forward is that moving house accounts for the apparently deleterious effect of divorce on children's education. To obtain the historical depth needed for this project's focus on changes over time, the project mostly uses existing data reaching back to retrospective data collected in 1984 that have limited information on moving house. These data indicate that the size of the place of residence of the family when the respondent was age 14

[13] This discussion of an increasing difference refers to the absolute difference between those from divorced and non-divorced families in the percentage completing secondary school and in the years of school completed. Other perspectives on change, such as the size of the gap relative to the expected level could yield other interpretations.

(sometimes called urbanicity) has a positive association with educational attainment, even net of the family's socioeconomic position. However, moving from a residence at one size of place at birth to a residence in a different size of place at age 14 (for example from a village of about 1000 to a metropolis of over 500000) has no further impact on education. Similarly, international and interstate migrants do not appear to experience reduced educational attainments compared to their non-moving peers.

In the case of both size of place of residence and state of residence at age 14, on this evidence there are opportunity differences according to one's residence in adolescence. However, there does not appear to be any negative effect of shifting residence on educational attainment.

Finally, we turn to the most recent data which include explicit measure of the number of times respondents moved house during childhood (so this measure includes the local moves which would have been missed by the earlier surveys). When included in this report's models of educational attainment, these measures, too, suggest that moving house does not reduce educational attainments, all else equal, in Australia. Note that this result needs to be treated as tentative, because it is based on fewer cases.

All in all, the evidence available here suggests that moving house does not have deleterious consequences for education, all else equal, and that moving house does not account for the negative statistical association between education and parental divorce. The result that, at least in these models and with the data to hand, moving house does not have a significant negative impact on children's educational attainment suggests that welfare policies aimed to encourage workless parents to move to places where there are more jobs are unlikely to have unintended harmful consequences for children's education. Our results reflect the average effect across all persons, but there could be large interactions. For example, the impact of moving house could vary with the reasons for moving and the families attitudes towards moving.

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APPENDIX A: DATA

Data Sources

The analyses in this project are largely based on data from the International Social Science Surveys Australia (IsssA). The IsssA regularly collects extensive and detailed survey data on large, representative national samples of Australians, beginning in 1984 and repeated most years since then.[14] There are now over 26,000 cases and many hundreds of variables. Some of the analyses also include aggregate data at the postcode-level from the ABS which we shall discuss below.

The IsssA

The IsssA surveys' particular strengths are that they offer:

- Individual level data on a very large number of variables simultaneously, facilitating multivariate analysis and enabling one explicitly to control for many sources of selectivity.
- Extensive measurement of public policy preferences, attitudes, and values, based on carefully pretested multiple-item scales for more reliable measurement.
- Extensive information on family background and on current labour force involvement.
- Cross-national comparisons on many variables, allowing one to discover what is unique to Australia; what is common to culturally similar nations such as Britain and the USA; and what holds for industrial nations generally.
- Historical depth, with many items appearing regularly since 1984.
- Panel components with some measures available for the same respondents at several points in time.

[14] The first survey, then called the National Social Science Survey, was supported primarily by the Australian Research Grants Committee and research funds kindly provided by Don Aitkin, now vice-chancellor of the University of Canberra. Most, but not all, subsequent surveys through 1997 were mainly supported by the Research School of Social Sciences at the Australian National University. The IsssA's home is in the International Survey Centre which is now core-sponsored by the Melbourne Institute of Applied Economic and Social Research, the University of Melbourne, being designed as an omnibus the survey episodically includes modules sponsored by other organisations. Merging all the surveys into a pooled, user-friendly file with consistent variable definitions was sponsored by an ARC-Research Infrastructure and Equipment grant to the Melbourne Institute.

Population sampled

The population sampled by the IsssA consists of citizens of Australia who reside at the address which they have provided to the Electoral Office, who can read English sufficiently well to answer a self-completion questionnaire, and who are not too cognitively impaired to answer a self-completion questionnaire. For simplicity, we refer to this population as "Australians". The selection on citizenship should have little effect, since prior research shows that non-citizen immigrants differ from citizen immigrants principally in their duration of residence, with few or no differences in issues that would be more relevant to this report, namely marital status and stratification characteristics (Evans 1988).

A note on sample size

The IsssA, unlike most social surveys, is based on a simple random sample. This is the optimal type of sample for most purposes, and the type of sample implicitly assumed by most statistical packages, so ordinary standard errors based on it are correct and do not require the inflating factors that cluster samples do. Simple random samples such as the IsssA are more efficient than the cluster samples used in almost all face-to-face surveys.[15]

A reasonable rule of thumb for high quality cluster designs is that they are worth approximately two-thirds as much as simple random samples (NORC 1987: 435). Thus an IsssA sample of about 2,200 would provide as reliable information as a good cluster sample of around 3,300 cases.

Data collection procedures: IsssA

The IsssA surveys are from simple random samples of Australian citizens[16] drawn by the Electoral Commission from the compulsory electoral roll, a public document.[17] They are conducted by mail[18] using a minor modification of Dillman's (1993) Total Response Method. First, a personally-addressed preliminary letter announces the survey; offers a free telephone contact number for queries; and provides information on how to decline to participate[19]. Then the

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[15] Travel costs make simple random samples unaffordable for most face-to-face surveys.

[16] For the exact definition, see the section on "Population sampled", above.

[17] Most of the early surveys are repeated cross-sections (ie new samples drawn each time) but a few are panels (re-contacting previous respondents). Our current design is a permanent panel, augmented with some fresh respondents in each wave.

[18] The first survey was mainly face-to-face interviews, with only the most rural quarter of the sample contacted by mail. Comparison of the face-to-face interviews with mail samples suggests that there are no systematic differences (Bean 1991), and similar results have been reported for the US (Goyder 1985). Mail surveys may be better than face-to-face or telephone surveys for sensitive issues, such as income, since there is no interviewer to create embarrassment (e.g. Babbie 1995: 272). Moreover, non-governmental surveys are more likely to detect participation in the gray economy and income derived from it. Probably the chief drawback to postal questionnaires is that because they are self-administered they are not suitable for questions requiring complex skip patterns (e.g. Babbie 1995: 272).

[19] For our surveys of 1984-85 through 1996-97, we did not use a preliminary letter, but rather a cover letter. The transition to a preliminary letter was at the suggestion of Malcolm Mearns, principal of Datacol Research on the grounds that it would be likely to boost response rates and that it would make refusals

survey booklet itself arrives in the post about two weeks later (together with its pre-paid return envelope and a further cover letter). These average around 64 pages, ranging from 32 to 84 pages, are attractively laid out, and are printed in black and white. The covers feature a map of Australia and are usually glossy white, with the map in a colour that varies from year to year[20]. For non-respondents, this is typically followed by four follow-up mailings, two with fresh copies of the questionnaire, over a 6 to 12 month period.[21]

The data entry process is too elaborate to cover in detail here, because it changes over time[22], but it is worthwhile giving a sketch of current practices. Because the IsssA relies almost entirely on closed-ended questions (because of their superior analytic properties), data processing is relatively straightforward. Upon receipt, the answers from the survey booklets are entered into a specialised computer program that flags out-of-range codes (usually keypunching errors), and has column location checks at the end of every page to guard against the keypuncher missing a question and thus punching answers to subsequent questions in the wrong fields, a problem sometimes known as "off-column" errors. Double answers (respondent circles two adjacent answer categories) are randomly assigned to one or the other answer (with special arrangements for a few unusual items). Experienced coders work with an automated occupation-coding program to transform open ended occupation questions into ABS 4-digit occupational codes. Experienced coders also convert open ended questions on industry and educational qualifications into standard ABS codes. Throughout the data entry process, coders and data-entry personnel flag all confusing or unclear cases which are subsequently dealt with in problem-resolution sessions with experts. All personnel are carefully trained and supervised[23] to maintain high standards of data-quality. With these procedures, we estimate that the data entry errors are substantially less than one per thousand questions (based on a sample of questionnaires that were entered twice, with different personnel performing the two entries).

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cheaper (because the preliminary letter costs only ordinary letter postage, and people who refuse at that stage are excised from the mailing list before the higher cost mailing of the questionnaire). Research is now in progress systematically to evaluate the impact of the preliminary letter.

[20] Our assessment indicates that colour makes no difference to response rates, but varying the colour helps to keep track of multi-year surveys and was an important mnemonic device for questionnaire designers trying to locate questions from earlier survey booklets. The electronic age has made the last issue less relevant, so if one were starting a survey today, one might well prefer to choose one permanent "signature" colour-scheme.

[21] In some years we have experimented with telephone follow-ups and various other alternatives for the last contact, which proved neither demonstrably better nor demonstrably worse than standard practice. .

[22] The data from the first survey were coded and entered by Reark Research, the data from the 1986-87 and 1987-88 surveys were coded by research assistants at the Australian National University and entered by data processing personnel at the Australian National University's (former) Data Processing Unit, and the data from subsequent surveys are coded and entered by personnel at Datacol Research. Datacol Research also provides the foundational SPSS locating, identifying, and labelling variables.

[23] Including random checks.

Non-response bias in surveys

Representativeness

The representativeness of the IsssA surveys in terms of education is covered in detail in the text. This section assesses the representativeness with respect to other variables. Note that sample representativeness matters, because only on the basis of a representative sample can one can make generalisations to the population as a whole. The representativeness of IsssA achieved samples in general has been clearly established in prior research (Bean 1991; Sikora 1997), and analyses using IsssA data appear regularly in the world's leading sociology journals.[24]

Here, we take two approaches to the issue of representativeness (also sometimes known as survey response bias): (1) comparisons of IsssA survey results with the Australian Census, and (2) comparisons of prompt respondents with tardy respondents (who would have been non-respondents if not for our extensive follow-up procedures).

Results for IsssA surveys conducted around the time of the 1991 Australian census show that the survey samples (1989-1993; 8234 cases) are representative of the population (Table A1).[25] Similar comparisons with the 2001 Census will be conducted in due course.

The IsssAs around 1990 do find 7 per cent more people employed than does the nearby Census (65 per cent versus 58 per cent). There are two possible sources of this difference, one being that the Census is "true" and the IsssA unrepresentative, the other being that people engaged in grey and black employment are more ready to report it to a non-governmental survey. With the information to hand, there is no solid evidence for preferring one of the interpretations to the other.

As noted in the text, on "age left school" the IsssA data and ABS data closely agree (see table below), but the IsssA seems slightly to underrepresent those who have not completed year twelve, and to overrepresent those who have achieved some qualification (details in the text, above).

[24] For example: Evans, Kelley, and Kolosi (1992); Kelley and De Graaf (1997); Kelley and Evans (1993, 1995).

[25] More extensive comparisons show this as well (Bean 1991; Sikora 1997).

Table A1: Comparison of IsssA surveys with the census.

	Census 1991	IsssA 1989-93
Gender		
Male	*49%*	*51%*
Female	*51%*	*49%*
Age Groups		
18 - 24	*15%*	*11%*
25 - 34	*22%*	*21%*
35 - 44	*21%*	*23%*
45 - 54	*15%*	*17%*
45 - 64	*12%*	*14%*
65+	*16%*	*14%*
Education: Age Left School		
Under 15/none	*18%*	*18%*
15	*24%*	*23%*
16	*22%*	*23%*
17	*19%*	*21%*
18	*11%*	*11%*
19 and over	*7%*	*3%*
Employment Status		
Employed	*58%*	*65%*
Unemployed	*7%*	*2%*
Not in labour force	*36%*	*33%*
Occupation of employed persons		
Managers & admin	*14%*	*13%*
Professionals	*14%*	*19%*
Para-professionals	*8%*	*11%*
Tradespersons	*14%*	*12%*
Clerks	*16%*	*16%*
Sales, service	*13%*	*12%*
Plant & mchn operators	*8%*	*7%*
Labourers	*13%*	*10%*

Another line of insight into the representativeness question comes from comparisons of prompt respondents, who complete and return their questionnaires shortly after receiving them, with tardy respondents (who would be non-respondents had they not been contacted on multiple occasions). Note that our preliminary letter invites sample members to refuse if they do not wish to participate, and we do not re-contact anybody who refuses. So the non-respondents are people who have not indicated a desire not to participate. An analysis of characteristics of non-respondents compared to respondents is given below in the section on “Non-response”.

Survey non-response

Completion rates

Among the data quality issues that concern survey researchers are completion rates, because of the possibility that non-respondents may differ systematically from respondents, yielding an unrepresentative achieved sample, and thereby violating the assumptions that justify generalization from a sample to a population (e.g. Donald 1960; Brownlee 1975; Miller 1991: 145-155; Babbie 1995: 262). Completion rates (defined as completions divided by eligibles, where eligibles = refusals plus completions]) range between 60 and 65 per cent on IsssA surveys. Potential respondents are defined as "eligible" if they are currently living at the address given in the electoral roll, able to read English, and not seriously ill. The main uncertainty has to do with the addresses, a proportion of which are out of date, erroneous, or unoccupied and so ineligible. Following van

Dijk, Mayhew and Killias (1990) we define as ineligible addresses from which we have heard nothing after 5 or 6 contacts. The IsssA completion rate compares favourably with recent experience in Australia, the USA, and many other industrial nations. For example, the well-regarded International Crime Victim Survey averaged 41 per cent in 14 nations using a similar definition (van Dijk, Mayhew and Killias 1990).[26]

However, diligent pursuit of non-respondents is expensive. In the IsssA, as in other mail surveys (Dillman 1993), the great majority of the completions come within a month or two of entering the field[27]. But then things begin to get expensive. The IsssA typically sends a second questionnaire (expensive both in printing and in postage), followed by another reminder letter, followed by a third questionnaire, and often a final desperation contact of some sort. All this obtains relatively few responses. Much of the follow-up mail goes to "bad" addresses, mostly because the person we are seeking has moved house. So, much is spent, for little gain. At a rough guess, we spend two or three times more per completed questionnaire at this stage than at the first stage.

But is all this is worthwhile? Since the budget is fixed, an attractive alternative is to draw a bigger sample in the first stage, but then cut the pursuit of non-respondents short, dropping the third questionnaire (and possibly even the second). That would produce a larger sample within the same budget – of course, bigger samples are unequivocally better. The danger is that the "difficult" respondents who initially refuse our requests to participate and only complete the questionnaire months later are different from the "good" respondents who answer right away. So by giving up on those who initially refuse, we might get an un-representative sample. That would be unequivocally bad.

So a key question is whether "good" (and inexpensive) respondents differ systematically from "difficult" (and expensive) respondents and, by extension, from non-respondents (who are presumably like "difficult" respondents, but even more extreme). Good arguments can easily be made on both sides of this question,[28] but in the end the question is an empirical one, and is an important tool in the assessment of sample representativeness (Babbie 1995).

Are those who initially fail to complete the questionnaire, eventually answering only after many reminders, in fact different from "good" respondents? The

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[26] The ICVS is an appropriate benchmark because it offers the same definition of response rate in all the countries taking part, whereas in many other international surveys each country defines the response rate in a way that is customary for them, so the reports are not comparable.

[27] For example, the University of Hawaii estimates that on its impressive panoply of student surveys, 40 percent of responses are returned within two weeks of receipt (Babbie 1995: 280).

[28] It might be that poorly educated respondents find our lengthy questionnaire daunting; or that the rich have no time for it; or that housewives find the focus on work uninteresting; or that right-wingers find it intrusive, or left-wingers find it threatening. Alternatively, it could be that none of these matter – that filling out a questionnaire depends on random things (such as happening to have some free time that week) or on things uncorrelated with the variables we are interested in (such as mood, personality, or co-operativeness).

logistic regression analysis in Table A2 suggests that, in the main, they are not. At a simple descriptive level, nothing we have measured is strongly correlated with initially not answering the survey (column 1), a finding confirmed by the logistic regression (columns 2 and 3).[29] Demographic differences are minimal; status and political differences even smaller; and attitudinal differences negligible. There is only one statistically significant difference: younger people are very slightly more likely initially not to complete the questionnaire, all else equal.

Table A2. Analysis of non-response. Panel 1: Respondents who initially refused to complete the survey but eventually answered after many reminders (=1, all others=0). Panel 2: Item non-response (=number of individual questions not answered). Correlations (r), logistic regression coefficients (b), standardized partial regression coefficients estimated by OLS (beta), and significance tests. Australia IsssA 1994-95. N=1503.

	1. Initially refused to answer survey			2. Item non-response		
	r	logistic b	Wald ^{2}	r	OLS beta	t
Male	-0.034	ns	0.45	-0.10	-0.07	-2.24
Age	-0.123	-0.02	12.91	0.11	ns	1.63
Parents' party	-0.014	ns	2.04	-0.04	ns	-1.12
Born in Australia	-0.026	ns	1.51	-0.05	ns	-1.35
Urban	0.012	ns	0.23	-0.01	ns	0.47
Education	0.017	ns	0.91	-0.13	-0.07	-2.08
Family income	0.001	ns	0.72	-0.07	ns	-0.40
Liberal or National Party	-0.013	ns	1.34	0.02	ns	1.33
In labor force	0.031	ns	0.01	-0.11	ns	-1.11
Catholic	0.023	ns	0.23	0.01	ns	-0.20
Christian belief (scale)	0.056	ns	1.23	0.04	ns	0.42
Pro-union (scale)	-0.003	ns	2.11	-0.01	ns	0.55
Govt regulate business (scale)	0.072	ns	0.99	0.00	ns	-0.43
Knowledge of science	-0.001	ns	1.04	-0.11	ns	-1.21
For genetically engineered food	0.001	ns	1.32	-0.08	ns	-1.59
Govt pay more on superannuation	0.036	ns	2.10	0.06	ns	1.23
Initially refused to answer survey	--	--	--	0.03	ns	1.20

ns – Not significantly different from zero, p<.05, two-tailed.

Item non-response

Item non-response is also a long-standing concern for survey analysts (Hyman 1972; Sudman and Bradburn 1974). Respondents' typically do not answer all the questions in a survey, and the concern is that those skipping an item are

[29] There are also statistical ways of getting some empirical leverage even on permanent non-response and adjusting for any resulting biases in the multivariate analysis (see Heckman 1979 and the literature flowing from that). But the cure often seems more dangerous than the disease, so conventional wisdom has generally turned against such corrections – a view with which we concur.

systematically different from those who do answer. On a few topics (for example, income) 10 percent or more may not answer, although generally item non-response tends to be closer to 5 percent in IsssA surveys. There is a large statistical literature how to handle item non-response, with implications that turn largely on how distinctive the non-responders actually are (e.g. Joreskog and Sorbom 1988, chapter 1: 12-17; Little 1992:1229-31). If they are very different, serious difficulties can arise in the analysis; conversely, if item non-response is more or less random with respect to the variables of interest, it is relatively easy to deal with.

So again it is an empirical question: how distinctive are those who do not answer particular questions? To get some insight on this, we selected some widely used items and counted how many each respondent failed to answer. A typical count, for eight demographic and background items in the 1994-95 IsssA is:[30]

No missing data, answered all	*74%*
Missed 1 question	*21%*
Missed 2 questions	*4%*
Missed 3 questions	*1%*
Missed 4 questions	*0.4%*
Missed 5 questions	*0.1%*
Missed 6 or more questions	*0%*
	100%(1503 cases)

Thus, most people answered all these questions but 21 per cent skipped one, four per cent skipped two and a handful skipped more. We made similar counts for other sorts of questions, with similar results.

Who, then, are not answering? In all, there are no substantial differences between those who skip questions and those who do not,[31] at least for the variables we have measured (see Table !!!, panel 2):

The tendency not to answer is not strongly correlated with anything we have measured (first column of panel 2). Most of the correlations are near zero.

Multivariate analysis suggests that there are, however, a couple of significant, but small, differences (see the second and third columns of panel 2). Men are a little less likely than women to skip questions, and the well-educated less likely than the poorly educated. Both differences are small, with a standardised effect of only -.07. Interestingly, there is no relation between skipping questions and being a tardy respondent – that is, no statistically significant link between item non-response and survey nearly-non-response. Instead, we suspect that doing a survey involves a two-stage decision process: first one decides whether or not to do the survey, then after that and quite independently, whether or not to answer each particular question.

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[30] Sex, age, parents' political party, place of birth, urban residence, education, family income, and party preference.

[31] Results on earlier IsssA data are similar Bean 1991

There seems to be a general tendency toward skipping questions in a survey, although not a strong one (Table A3). For example, those who tended to skip background items also tended to skip political attitude questions (r=.32), questions on science (r=.21), attitudes toward retirement income provisions (r=.31) and religious matters (r=.19). All these links are clear, but only moderately strong.

In some analyses, we have estimated the effects of item nonresponse using a variation of Heckman's (1979) method suggested by Kelley and Evans (1993:118-20) which uses nonresponse on related questions elsewhere in the questionnaire to give an independent indicator of the underlying propensity not to respond. However, our experience thus far is that these adjustments rarely make any practical difference.

In practice, we therefore generally use the pairwise present method for missing data, without any further adjustment. It is statistically preferable to the usual simple alternatives (Joreskog and Sorbom 1988, chapter 1: 12-17; Little 1992:1229-31).

Table A3. Correlations between initial refusal to complete the survey and non-response to particular items in the survey. Australia IsssA 1994-95. N=1503.

		Item non-response on:				
	Initially refused to complete the survey	Background items	Political attitudes	Attitudes to science	Attitudes toward retirement	Religious issues
Background items	0.06	1.00	0.32	0.21	0.31	0.19
Political attitudes	0.01	0.32	1.00	0.15	0.20	0.16
Attitudes to science	0.02	0.21	0.15	1.00	0.40	0.07
Attitudes to retirement	0.03	0.31	0.20	0.40	1.00	0.10
Religious issues	-0.03	0.19	0.16	0.07	0.10	1.00

General matters of question design

In general, scale types and formats matter little to the psychometric quality of questionnaire items, so long as the substance of the question is clear and respondents can tell which end is high and which is low (Sheatsley 1983; Milkovich and Wigdor 1991: 3), although the reliability of ratings drops if there are under 3 answer categories or more than 9 answer categories (Milkovich and Wigdor 1991: 3). As a result, 5 to 7 answer categories are often treated as ideal, although one may need to vary this for specific purposes, such as replication. Some degree of balance of topics is ideal to maintain respondent concentration (Sheatsley 1983). Comparisons of survey data with formal records indicate that

factual questions tend to obtain more accurate answers when the questions are clear and not terribly complex (Dykema and Schaeffer 2000), so the IsssA routinely assesses new factual questions qualitatively in terms of respondents' experience of their clarity and complexity.

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APPENDIX B

In this appendix, mnemonics are sometimes used to reference variables briefly. the mnemonics and definitions are:

PNTDV14X=Parents divorced by the time R was age 14 (0 or 1)

PDVRXBRN= Parental divorce by year born interaction

Born00M = Date of birth- 1900.

MALEM = Gender (reference=0=female, 1=male)

MALEXBRN= Gender by time interaction

PNTEDYRM=Parents' education (years)

LNBOOKM= Books in the home at age 14 (ln)

FASTATM=Father's occupation (0 to 100)

NSIBSM=Siblings (#)

LNURB14Q=Size of place where resident at age 14(ln)

PNT_MED= Parents Mediterranean migrants (0 or 1)

ADVERSE= Adverse selection index

LNURBBOQ= Size of place where resident at birth (ln)

SIZZ014Q= [Size of place where resident at age 14(ln)- Size of place where resident at birth (ln)]

Alternative models of divorce

Appendix Table 1. Alternative estimates of divorce.

Variable	Coef-ficient	St. errror	Wald	Signi-ficance	Exp(B)
Panel A					
Born before 1962 (0 or 1)	-0.371	0.080	21.2	0.000	0.69
Gender (Male=1)	-0.216	0.068	10.2	0.001	0.81
Parents' education (years)	0.029	0.014	4.1	0.042	1.03
Father's occupation (0 to 100)	-0.001	0.002	0.2	0.670	1.00
Mother's work intensity index	1.556	0.085	333.9	0.000	4.74
Parents Catholic	-0.177	0.084	4.4	0.035	0.84
Parents' church-going (ln days/year)	-0.206	0.021	95.6	0.000	0.81
Constant	-2.868	0.160	321.2	0.000	
Panel B					
Born before 1962 (0 or 1)	-0.389	0.081	23.3	0.000	0.68
Gender (Male=1)	-0.210	0.068	9.6	0.002	0.81
Parents' education (years)M	0.032	0.014	5.0	0.026	1.03
Father's occupation (0 to 100)	-0.001	0.002	0.6	0.429	1.00
Mother's work intensity	1.584	0.087	329.4	0.000	4.87
Parents Catholic	-0.215	0.085	6.4	0.011	0.81
Parents church-going (ln days/year)	-0.204	0.021	93.1	0.000	0.82
Siblings (#)	0.065	0.019	11.9	0.001	1.07
Size of place (ln)	0.026	0.010	6.2	0.013	1.03
Constant	-3.341	0.203	269.7	0.000	
Panel C					
Born before 1962 (0 or 1)	-0.382	0.081	22.4	0.000	0.68
Gender (Male=1)	-0.203	0.068	9.0	0.003	0.82
Parents' education (years)M	0.036	0.014	6.3	0.012	1.04
Father's occupation (0 to 100)	0.000	0.002	0.0	0.910	1.00
Mother's work intensity$	1.607	0.089	326.8	0.000	4.99
Parents Catholic	-0.209	0.085	6.1	0.014	0.81
Parents church-going (ln days/year)	-0.207	0.021	95.7	0.000	0.81
Siblings (#)	0.065	0.019	12.0	0.001	1.07
Size of place (ln)	0.024	0.010	5.5	0.019	1.02
Parents in top income quartile	-0.363	0.093	15.2	0.000	0.70
Parents in bottom income quartile	-0.242	0.102	5.6	0.018	0.79
Constant	-3.320	0.211	247.1	0.000	

Appendix Table 1 continued

Panel D

Born before 1962 (0 or 1)	-0.525	0.102	26.6	0.000	0.5918
Gender (Male=1)	-0.145	0.083	3.0	0.083	0.8654
Parents' education (years)M	0.064	0.019	11.4	0.001	1.0661
Father's occupation (0 to 100)	-0.002	0.002	0.9	0.336	0.9983
Parents Catholic	0.000	0.105	0.0	1.000	0.9999
Parents church-going (ln days/year)	-0.280	0.027	104.9	0.000	0.7555
Siblings (#)	0.043	0.023	3.4	0.066	1.0437
Lived in metropolitan area at 14	0.135	0.088	2.4	0.125	1.1439
Parents income estimate (ln)	0.219	0.101	4.7	0.031	1.2442
Constant	-5.233	1.063	24.2	0.000	

Panel E

Born before 1962 (0 or 1)	-0.527	0.102	26.8	0.000	0.5906
Gender (Male=1)	-0.139	0.083	2.8	0.095	0.8699
Parents' education (years)M	0.065	0.019	11.6	0.001	1.067
Father's occupation (0 to 100)	-0.003	0.002	2.2	0.140	0.9973
Parents Catholic	-0.018	0.105	0.0	0.865	0.9823
Parents church-going (ln days/year)	-0.275	0.027	100.4	0.000	0.7597
Siblings (#)	0.048	0.023	4.2	0.041	1.0488
Size of place (ln)	0.039	0.013	9.6	0.002	1.0401
Parents income estimate (ln)	0.203	0.101	4.0	0.045	1.2252
Constant	-5.445	1.065	26.1	0.000	

Panel F

Born before 1962 (0 or 1)	-0.307	0.104	8.8	0.003	0.7358
Gender (Male=1)	-0.140	0.084	2.8	0.096	0.8692
Parents' education (years)M	0.072	0.019	14.2	0.000	1.0742
Father's occupation (0 to 100)	-0.001	0.002	0.1	0.718	0.9993
Mother's work intensity$	1.477	0.114	169.5	0.000	4.3817
Parents Catholic	-0.101	0.106	0.9	0.338	0.9038
Parents church-going (ln days/year)	-0.250	0.028	81.3	0.000	0.7788
Siblings (#)	0.093	0.024	15.2	0.000	1.097
Size of place (ln)	0.028	0.013	4.8	0.029	1.0286
Parents income estimate (ln)	-0.131	0.105	1.5	0.214	0.8775
Constant	-2.672	1.086	6.0	0.014	

Appendix Table 1, continued

Panel G					
Born before 1962 (0 or 1)	-0.307	0.104	8.8	0.003	0.7358
Gender (Male=1)	-0.140	0.084	2.8	0.096	0.8692
Parents' education (years)M	0.072	0.019	14.2	0.000	1.0742
Father's occupation (0 to 100)	-0.001	0.002	0.1	0.718	0.9993
Mother's work intensity$	1.477	0.114	169.5	0.000	4.3817
Parents Catholic	-0.101	0.106	0.9	0.338	0.9038
Parents church-going (ln days/year)	-0.250	0.028	81.3	0.000	0.7788
Siblings (#)	0.093	0.024	15.2	0.000	1.097
Size of place (ln)	0.028	0.013	4.8	0.029	1.0286
Parents income estimate (ln)	-0.131	0.105	1.5	0.214	0.8775
Constant	-2.672	1.086	6.0	0.014	
Panel H					
Born before 1962 (0 or 1)	-0.358	0.105	11.7	0.001	0.6993
Gender (Male=1)	-0.138	0.084	2.7	0.103	0.8715
Parents' education (years)M	0.053	0.020	7.3	0.007	1.0548
Father's occupation (0 to 100)	-0.001	0.002	0.2	0.664	0.9992
Mother's work intensity$	1.520	0.114	177.7	0.000	4.5709
Parents Catholic	-0.034	0.106	0.1	0.751	0.9669
Parents church-going (ln days/year)	-0.238	0.028	74.1	0.000	0.7879
Siblings (#)	0.089	0.024	13.9	0.000	1.0928
Size of place (ln)	0.031	0.013	5.7	0.017	1.0316
Parents income estimate (ln)	-0.135	0.105	1.6	0.201	0.874
Parents Mediterranean migrants (0 or 1)	-1.205	0.317	14.4	0.000	0.2997
Constant	-2.438	1.091	5.0	0.025	

Fit

Appendix Table 2. Fit: Baseline deviance minus model deviance, where deviance = -2 log likelihood.

	df	Deviance
Panel A	7	575
Panel B	9	592
Panel C	11	611
Panel D	9	193
Panel E	9	200
Panel F	10	366
Panel G	10	366
Panel H	10	387

Alternative models of the probability of obtaining a tertiary qualification

Appendix Table 3. Estimates of the effect of divorce on completing a tertiary qualification (with controls). All persons over age 25. Logistic regression estimates.

Panel A: Preferred model A'. Time interactions with divorce and gender only.

Variable	Coef-ficient	Standard error	Wald	Signifi-cance	Exp(B)
Parents divorced by the time R was age 14 (0 or 1)	-0.236	0.106	5	0.026	0.79
Year born (year-1900)	0.036	0.003	172	<.005	1.04
Gender (Male=1)	1.763	0.176	101	<.005	5.83
Gender by time interaction	-0.026	0.004	54	<.005	0.97
Parents' education (years)	0.089	0.010	72	<.005	1.09
Books in the home at age 14 (ln)	0.339	0.018	348	<.005	1.40
Father's occupation (0 to 100)	0.011	0.001	125	<.005	1.01
Siblings (#)	-0.111	0.014	66	<.005	0.89
Size of place (ln)	0.045	0.007	43	<.005	1.05
Parents Mediterranean migrants (0 or 1)	0.465	0.120	15	<.005	1.59
Constant	-6.466	0.188	1188	<.005	

Panel B: No time interaction with divorce, but does include time interactions with father's occupation and parent's education.

Parents divorced by the time R was age 14 (0 or 1)	-0.240	0.106	5	0.024	0.79
Year born (year-1900)	0.039	0.007	33	<.005	1.04
Gender (Male=1)	1.772	0.176	101	<.005	5.88
Gender by time interaction	-0.026	0.004	54	<.005	0.97
Parents' education (years)	0.074	0.034	5	0.031	1.08
Parents' education by time interaction	0.000	0.001	0	0.647	1.00
Books in the home at age 14 (ln)	0.338	0.018	344	<.005	1.40
Father's occupation (0 to 100)	0.017	0.003	26	<.005	1.02
Father's occupation by time interaction	-0.012	0.007	3	0.066	0.99
Siblings (#)	-0.110	0.014	65	<.005	0.90
Size of place (ln)	0.045	0.007	43	<.005	1.05
Parents Mediterranean migrants (0 or 1)	0.459	0.121	14	<.005	1.58
Constant	-6.607	0.351	354	<.005	

Panel C: Includes time interactions with divorce, gender, father's occupation, and parents' education.

Parents divorced by the time R was age 14 (0 or 1)	-0.045	0.411	0	0.913	0.96
Parental divorce by year born interaction	-0.004	0.008	0	0.624	1.00
Year born (year-1900)	0.039	0.007	33	0.000	1.04
Gender (Male=1)	1.777	0.177	101	0.000	5.91
Gender by time interaction	-0.026	0.004	54	0.000	0.97
Parents' education (years)	0.073	0.034	5	0.033	1.08
Parents' education by time interaction	0.000	0.001	0	0.630	1.00
Books in the home at age 14 (ln)	0.338	0.018	345	0.000	1.40
Father's occupation (0 to 100)	0.017	0.003	26	0.000	1.02
Father's occupation by time interaction	-0.012	0.007	3	0.064	0.99
Siblings (#)	-0.110	0.014	65	0.000	0.90
Size of place (ln)	0.045	0.007	43	0.000	1.05
Parents Mediterranean migrants (0 or 1)	0.458	0.121	14	0.000	1.58
Constant	-6.613	0.352	354	0.000	

Fit: probability of completing a tertiary qualification

Appendix Table 4. Fit statistics for Appendix table 3.

	Deviance[a]	Chi-square	Degrees of freedom
Initial	14686.2		
Model A'	12640.5	2045.8	10
Model A	12640.3	2046.0	11
Model B	12637.0	2049.2	12
Model C	12636.8	2049.5	13

Notes:[a] Deviance (-2 log likelihood)

Alternative models of the probability of completing twelve or more years of education, with and without adverse selection index included.

Appendix Table 5 presents the parameter estimates from the logistic regression models associated with text Table 8, and Table 6 presents the parameter estimates from the OLS regression models of text Table 8.

Appendix Table 5. Alternative models of probability of completing twelve or more years of education, Section A = baseline, Section B= Section A + adverse selection index, Section C= Section B+ parental divorce + parental divorce + time interaction.

Section A: Baseline Variable	Coef-ficient	Standard error	Wald	df	Signifi-cance	R	Exp(B)
BORN00M	0.0373	0.0017	456.3602	1	0	0.1353	1.038
MALEM	1.2536	0.1202	108.8503	1	0	0.0656	3.5028
MALEXBRN	-0.0199	0.0024	71.9085	1	0	-0.0531	0.9802
PNTEDYRM	0.1337	0.0081	269.9504	1	0	0.1039	1.1431
LNBOOKM	0.3401	0.0132	665.6401	1	0	0.1636	1.4051
FASTATM	0.0115	0.0008	213.8612	1	0	0.0924	1.0116
NSIBSM	-0.1086	0.0099	121.4616	1	0	-0.0694	0.8971
LNURB14Q	0.023	0.0049	22.1766	1	0	0.0285	1.0233
PNT_MED	0.7909	0.0827	91.4273	1	0	0.06	2.2054
Constant	-5.3657	0.1294	1718.823	1	0		
Section B: Adds adverse selection indicator							
BORN00M	0.0444	0.0019	568.0911	1	0	0.1666	1.0454
MALEM	1.2692	0.1223	107.6617	1	0	0.072	3.558
MALEXBRN	-0.0217	0.0024	82.4637	1	0	-0.0628	0.9785
PNTEDYRM	0.1349	0.0082	272.8061	1	0	0.1152	1.1444
LNBOOKM	0.3314	0.0133	625.1872	1	0	0.1748	1.393
FASTATM	0.0097	0.0008	145.1441	1	0	0.0838	1.0097
NSIBSM	-0.1197	0.01	143.9582	1	0	-0.0834	0.8872
LNURB14Q	0.2739	0.0194	199.3072	1	0	0.0983	1.315
PNT_MED	0.1353	0.0956	2.0031	1	0.157	0.0004	1.1449
ADVERSE	-0.5062	0.0378	178.9385	1	0	-0.0931	0.6028
Constant	-7.412	0.2045	1313.708	1	0		
Section C: Adds parental divorce and interaction of divorce and time							
BORN00M	0.0455	0.0019	572.3467	1	0	0.1679	1.0465
MALEM	1.2826	0.1227	109.3538	1	0	0.0729	3.6062
MALEXBRN	-0.022	0.0024	84.0941	1	0	-0.0637	0.9783
PNTEDYRM	0.1364	0.0082	277.9543	1	0	0.1168	1.1461
LNBOOKM	0.3281	0.0133	608.8732	1	0	0.1732	1.3883
FASTATM	0.0097	0.0008	145.721	1	0	0.0843	1.0097
NSIBSM	-0.1184	0.01	140.5676	1	0	-0.0828	0.8883
LNURB14Q	0.2658	0.0195	186.3084	1	0	0.0955	1.3044
PNT_MED	0.1402	0.0957	2.1469	1	0.1429	0.0027	1.1505
ADVERSE	-0.4881	0.038	164.8243	1	0	-0.0897	0.6138
PNTDV14X	0.3209	0.2743	1.3693	1	0.2419	0	1.3784
PDVRXBRN	-0.0125	0.005	6.2039	1	0.0127	-0.0144	0.9876
Constant	-7.3898	0.2057	1290.826	1	0		

Appendix Table 6. Alternative models of years of education completed, Section A = baseline, Section B= Section A + adverse selection index, Section C= Section B+ parental divorce + parental divorce + time interaction.

Section A: Baseline

Variable	Metric coef-ficient	Standard error	Standardised coef-ficient	T	Sig T
BORN00M	0.0998	0.004786	0.48	20.844	<.0005
MALEM	1.1018	0.125108	0.18	8.807	<.0005
MALEXBR	-0.0130	0.002712	-0.10	-4.803	<.0005
PNTEDYR	0.3159	0.02523	0.28	12.522	<.0005
PEDXBRN	-0.0034	5.41E-04	-0.22	-6.259	<.0005
LNBOOKM	0.4513	0.013848	0.24	32.588	<.0005
FASTATM	0.0264	0.002701	0.21	9.790	<.0005
FOCCXBR	-0.0244	0.005881	-0.10	-4.141	<.0005
NSIBSM	-0.1603	0.010657	-0.10	-15.039	<.0005
LNURB14(	0.0459	0.005544	0.06	8.282	<.0005
PNT_MED	-0.1164	0.097651	-0.01	-1.192	0.2332
(Constant)	2.7069	0.227936		11.876	<.0005

Section B: Adds adverse selection indicator

BORN00M	0.1017	0.004765	0.49	21.346	<.0005
MALEM	1.0605	0.124547	0.17	8.515	<.0005
MALEXBR	-0.0141	0.0027	-0.11	-5.208	<.0005
PNTEDYR	0.3057	0.025122	0.27	12.168	<.0005
PEDXBRN	-0.0031	5.39E-04	-0.20	-5.815	<.0005
LNBOOKM	0.4350	0.013844	0.23	31.418	<.0005
FASTATM	0.0247	0.002692	0.20	9.170	<.0005
FOCCXBR	-0.0256	0.005854	-0.11	-4.372	<.0005
NSIBSM	-0.1677	0.010622	-0.11	-15.790	<.0005
LNURB14(	0.3278	0.023382	0.41	14.020	<.0005
PNT_MED	-0.8753	0.114825	-0.06	-7.622	<.0005
ADVERSE	-0.5658	0.045609	-0.37	-12.406	<.0005
(Constant)	0.6917	0.278994		2.479	0.0132

Section C: Adds parental divorce and interaction of divorce and time

PNTDV14)	0.2166	0.298115	0.02	0.727	0.4675
PDVRXBR	-0.0119	0.006077	-0.04	-1.966	0.0493
BORN00M	0.1022	0.004764	0.49	21.453	<.0005
MALEM	1.0639	0.124526	0.17	8.544	<.0005
MALEXBR	-0.0142	0.0027	-0.11	-5.244	<.0005
PNTEDYR	0.3038	0.025147	0.27	12.083	<.0005
PEDXBRN	-0.0031	5.39E-04	-0.20	-5.671	<.0005
LNBOOKM	0.4307	0.013884	0.23	31.023	<.0005
FASTATM	0.0249	0.002691	0.20	9.250	<.0005
FOCCXBR	-0.0261	0.005852	-0.11	-4.462	<.0005
NSIBSM	-0.1672	0.010624	-0.11	-15.738	<.0005
LNURB14(	0.3205	0.02343	0.40	13.678	<.0005
PNT_MED	-0.8713	0.114766	-0.06	-7.592	<.0005
ADVERSE	-0.5494	0.045744	-0.35	-12.010	<.0005
(Constant)	0.7459	0.279244		2.671	0.0076

Alternative models of the effects of moving house.

The first set of models examines whether size of place of birth adds to the explanation of education, and whether the change in the size of place of birth adds to the explanation of education. The first set of models concern secondary school completion.

Appendix Table 7. Alternative models of probability of completing twelve or more years of education, Model 3 adds size of place of birth to preferred Model A of Table 3. Model 4 adds change in size of place to Model 3. Models 1 and 2 of text Table 8 are only of interest for the fit statistics and so are not shown here.

Variable	Coef-ficient	Standard error	Wald	df	Signifi-cance	R	Exp(B)
Model 3: adds size of place at birth							
PNTDV14X	0.3829	0.3145	1.5	1	0.2233	0.000	1.467
PDVRXBRN	-0.0143	0.0057	6.4	1	0.0116	-0.017	0.986
BORN00M	0.0416	0.0021	388.2	1	<.0005	0.163	1.043
MALEM	1.3941	0.1424	95.9	1	<.0005	0.080	4.032
MALEXBRN	-0.0229	0.0028	68.5	1	<.0005	-0.068	0.977
PNTEDYRM	0.1234	0.0095	169.3	1	<.0005	0.107	1.131
LNBOOKM	0.3511	0.0158	495.9	1	<.0005	0.184	1.421
FASTATM	0.0132	0.0009	193.8	1	<.0005	0.115	1.013
NSIBSM	-0.1002	0.0121	68.5	1	<.0005	-0.068	0.905
LNURB14Q	0.0235	0.0082	8.2	1	0.0041	0.021	1.024
PNT_MED	0.8366	0.0948	77.9	1	<.0005	0.072	2.309
LNURBBOQ	0.0026	0.0081	0.1	1	0.7445	0.000	1.003
Constant	-5.5586	0.157	1254.2	1	<.0005		
Model 4: Adds indicator of change in size of place							
PNTDV14X	0.2729	0.3342	0.7	1	0.4141	0.000	1.314
PDVRXBRN	-0.0126	0.0059	4.5	1	0.0342	-0.014	0.988
BORN00M	0.0416	0.0022	358.9	1	<.0005	0.165	1.043
MALEM	1.3965	0.1498	86.9	1	<.0005	0.080	4.041
MALEXBRN	-0.0226	0.0029	60.9	1	<.0005	-0.067	0.978
PNTEDYRM	0.1234	0.0098	158.9	1	<.0005	0.109	1.131
LNBOOKM	0.3466	0.0166	436.5	1	<.0005	0.182	1.414
FASTATM	0.0129	0.001	166.0	1	<.0005	0.112	1.013
NSIBSM	-0.1049	0.0131	64.4	1	<.0005	-0.069	0.900
PNT_MED	0.8940	0.1018	77.1	1	<.0005	0.076	2.445
LNURB14Q	0.0307	0.0109	7.9	1	0.0049	0.021	1.031
LNURBBOQ	-0.0055	0.0107	0.3	1	0.6079	0.000	0.995
SIZZ014Q	-4.10E-08	3.42E-08	1.5	1	0.2249	0.000	1.000
Constant	-5.4959	0.1636	1129.2	1	<.0005		

Appendix Table 8, repeats this analysis changing the dependent variable to acquiring a tertiary qualification.

Appendix Table 8. Alternative models of completing a tertiary qualification, Model 3 adds size of place of birth to preferred Model A of Table 5. Model 4 adds change in size of place to Model 3. Models 1 and 2 of Text table 8 are only of interest for fit statistics and are not shown here.

Variable	Coef-ficient	Standard error	Wald	df	Signifi-cance	R	Exp(B)
Model 3: adds size of place at birth							
PNTDV14X	-0.032	0.146	0.0	1	0.8283	0.000	0.97
BORN00M	0.015	0.004	17.0	1	<.0005	0.052	1.02
MALEM	1.306	0.249	27.6	1	<.0005	0.068	3.69
MALEXBRN	-0.019	0.005	15.2	1	0.0001	-0.049	0.98
PNTEDYRM	0.021	0.014	2.4	1	0.1225	0.008	1.02
LNBOOKM	0.156	0.026	36.4	1	<.0005	0.079	1.17
FASTATM	0.005	0.001	10.7	1	0.0011	0.039	1.00
NSIBSM	-0.026	0.020	1.7	1	0.1967	0.000	0.97
PNT_MED	0.175	0.159	1.2	1	0.2708	0.000	1.19
LNURB14Q	0.035	0.013	7.5	1	0.0062	0.031	1.04
LNURBBOQ	-0.006	0.012	0.2	1	0.6254	0.000	0.99
Constant	-2.521	0.261	93.5	1	<.0005		
Model 4: Adds indicator of change in size of place							
PNTDV14X	-0.029	0.146	0.0	1	0.8425	0.000	0.97
BORN00M	0.015	0.004	16.9	1	0	0.052	1.02
MALEM	1.305	0.249	27.6	1	0	0.068	3.69
MALEXBRN	-0.019	0.005	15.2	1	0.0001	-0.049	0.98
PNTEDYRM	0.021	0.014	2.4	1	0.1216	0.008	1.02
LNBOOKM	0.157	0.026	36.7	1	0	0.079	1.17
FASTATM	0.005	0.001	10.5	1	0.0012	0.039	1.00
NSIBSM	-0.026	0.020	1.7	1	0.1928	0.000	0.97
PNT_MED	0.181	0.159	1.3	1	0.2549	0.000	1.20
LNURB14Q	0.049	0.018	7.4	1	0.0067	0.031	1.05
LNURBBOQ	-0.019	0.017	1.2	1	0.2683	0.000	0.98
SIZZ014Q	-5.50E-08	5.061E-08	1.2	1	0.2773	0.000	1.00
Constant	-2.529	0.261	93.9	1	0		

In these models, size of place at age 14 has a significant positive coefficient indicating that it is associated with an increase in the probability of attaining a tertiary qualification, but, aside from that, neither the family's size of place of residence at birth nor the difference between size of place at birth and at 14 has a significant effect.

The results for years of education are in Appendix Table 9.

Appendix Table 9. Alternative models of completing a tertiary qualification, Model 13 adds size of place of birth to preferred Model A of Table 5. Model 14 adds change in size of place to Model 13. Models 11 and 12 of text Table 9 are only of interest for fit statistics and are not shown here

Model 13: adds size of place at birth

Variable	Metric regression coefficient	Standard error	Standar-dised regression coefficient	T	Signifi-cance
PNTDV14X	0.0517	0.3792	0.004	0.136	0.8916
PDVRXBRN	-0.0104	0.0074	-0.041	-1.393	0.1638
BORN00M	0.1032	0.0059	0.498	17.596	<.0005
MALEM	1.2434	0.1610	0.205	7.722	<.0005
MALEXBRN	-0.0167	0.0034	-0.134	-4.918	<.0005
PNTEDYRM	0.2935	0.0310	0.270	9.476	<.0005
PEDXBRN	-0.0031	0.0006	-0.206	-4.782	<.0005
LNBOOKM	0.4556	0.0175	0.244	26.102	<.0005
FASTATM	0.0320	0.0035	0.257	9.106	<.0005
FOCCXBRN	-0.0354	0.0075	-0.154	-4.755	<.0005
NSIBSM	-0.1510	0.0142	-0.088	-10.602	<.0005
LNURB14Q	0.0472	0.0095	0.060	4.96	<.0005
PNT_MED	0.0828	0.1193	0.006	0.694	0.4876
LNURBBOQ	0.0024	0.0095	0.003	0.255	0.7985
(Constant)	2.6013	0.2852		9.122	<.0005
Model 14: Adds indicator of change in size of place					
PNTDV14X	0.0552	0.3792	0.004	0.146	0.8842
PDVRXBRN	-0.0104	0.0074	-0.041	-1.392	0.1640
BORN00M	0.1033	0.0059	0.499	17.606	<.0005
MALEM	1.2435	0.1610	0.205	7.723	<.0005
MALEXBRN	-0.0167	0.0034	-0.134	-4.917	<.0005
PNTEDYRM	0.2936	0.0310	0.270	9.482	<.0005
PEDXBRN	-0.0031	0.0006	-0.206	-4.784	<.0005
LNBOOKM	0.4559	0.0175	0.244	26.117	<.0005
FASTATM	0.0321	0.0035	0.258	9.114	<.0005
FOCCXBRN	-0.0356	0.0075	-0.155	-4.772	<.0005
NSIBSM	-0.1510	0.0142	-0.088	-10.603	<.0005
LNURB14Q	0.0578	0.0124	0.073	4.668	<.0005
PNT_MED	0.0868	0.1193	0.006	0.728	0.4669
LNURBBOQ	-0.0080	0.0123	-0.010	-0.655	0.5124
SIZZ014Q	-5.44E-08	4.05E-08	-0.015	-1.344	0.1791
(Constant)	2.5958	0.2852		9.102	<.0005

[End]