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Estimating the Direct and Indirect Effects of
Compulsory Schooling on Well-Being in Australia**

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ABSTRACT

The Marginal Income Effect of Education on Happiness: Estimating the Direct and Indirect Effects of Compulsory Schooling on Well-Being in Australia^{*}

Many economists and educators favour public support for education on the premise that education improves the overall well-being of citizens. However, little is known about the causal pathways through which education shapes people's subjective well-being (SWB). This paper explores the direct and indirect well-being effects of extra schooling induced through compulsory schooling laws in Australia. We find the net effect of schooling on later SWB to be positive, though this effect is larger and statistically more robust for men than for women. We then show that the compulsory schooling effect on male's SWB is indirect and is mediated through income.

JEL Classification: I20, I32, C36

Keywords: schooling, indirect effect, well-being, mental health, windfall income, HILDA survey

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Many economists and educators favour public support for education on the premise that education improves the overall well-being of citizens. However, little is known about the causal pathways through which more education shapes people's overall subjective well-being (SWB). While some studies of the effect of compulsory schooling on people's future mental health and well-being – primarily in the United Kingdom's context – confirm the presence of a causal relationship, they do not systematically explain the mediating pathways by which the effect is transmitted. Consider, for example, the study by Oreopoulos (2007) in which a change to the minimum school leaving age law in the UK has been shown to significantly increase future overall life satisfaction of people who were affected by that law. What this study does not reveal, however, is how much of this positive effect is direct and how much is indirectly channelled through potential mediators of education such as income and health? These are difficult questions, but they seem important for policy makers to understand, especially if we assume that their broad goal is the maximisation of people's well-being.

This paper focuses on a particular indirect effect of education on SWB – an increase in the lifetime financial returns induced by a change in the compulsory schooling law – and attempts empirically to estimate this causal chain. Using the different timing of education laws across the states of Australia as instruments for years of education, and shocks in personal income from bequest, inheritance, severance pay, and other irregular income (such as lottery wins) as instruments for total real personal income, we estimate the direct and indirect effects of education on three measures of SWB – life satisfaction, mental health, and financial satisfaction – for working-age adults in Australia. We initially find a positive causal effect of staying an extra year in school on mental health, but not on the other two SWB measures. Further, this effect is only statistically significant for men. However, we later demonstrate that this causal effect of compulsory schooling on males' SWB is not direct.

Rather, we find evidence to suggest that most of the well-being effect of education reforms in Australia is mediated causally through income.

The paper is structured as follows. Section 1 briefly reviews the literature on education, mental health, and other measures of SWB. Section 2 describes the Australian data we use. An empirical model for causal analysis with respect to education and income is outlined in Section 3. Section 4 reports the results. Section 5 concludes.

1. Education, Mental Health, and Well-being

While it is theoretically and empirically well-established that there is a significant pecuniary benefit to acquiring human capital, previous studies on the link between education and SWB have produced mixed results. Using highest education qualification dummies as control variables in cross-section SWB equations, many scholars have found a positive and statistically significant association between education and self-rated happiness and life satisfaction across different international data sets and time periods (see, for example, Easterlin, 2001; Graham and Pettinato, 2002; Blanchflower and Oswald, 2004; Ferrer-i-Carbonell, 2005;). Yet there have also been other studies that have documented either a negative or a statistically insignificant effect of education on happiness, job satisfaction, and different measures of mental health (e.g., Clark and Oswald, 1996; Clark, 2003; Flouri, 2004).

One explanation for these mixed findings is that schooling may be correlated with unobserved characteristics of individuals, such as personality traits, intelligence, aspirations and motivation, which also jointly determine how individuals evaluate their mental health and happiness (Dolan *et al.*, 2008; Oreopoulos and Salvanes, 2011). Moreover, the

coefficient on education is likely to depend on what else is being included in the estimation model. For example, highly educated individuals are, relative to less educated individuals, likely to be healthier and earn higher income, while at the same time have significantly higher aspirations (Stutzer, 2004), are more likely to feel rushed for time (Oreopoulos and Salvanes, 2011), have greater responsibilities in the workplace, and spend more time engaging in relatively stressful activities such as commuting and paid employment. By controlling for these so-called ‘bad controls’ (or variables that are themselves outcomes of education) in SWB regressions, we are likely to either underestimate the effect of education on mental health and well-being when income and/or health are fully controlled for, or overestimate it when commuting time and working hours are fully controlled for.¹

In an attempt to establish causality, Oreopoulos (2007) and Oreopoulos and Salvanes (2011) explore the impact of changes to compulsory schooling laws in the United Kingdom on overall life satisfaction. Using Eurobarometer data from 1973 to 1998, the authors show that an increase in the minimum school leaving age from 15 to 16 increases mean overall life satisfaction, measured in adulthood, by approximately 0.048 of a point (*S.E.*=0.010) on an 11-point (0 to 10) satisfaction with life scale. They also find the estimated coefficient falls by less than half, to 0.035 (*S.E.*=0.012), when conditioning on individual income. In a separate study, Chevalier and Feinstein (2006) use an instrumental variables (IV) approach to estimate the effect of education on a measure of depression among individuals in the UK’s National Child Development Study. The authors find that education significantly reduces the risk of adult depression; on average, having a secondary education qualification reduces the risk of depression at age 42 by approximately 5 to 7 percentage points. However, the positive effect of education on SWB could not be replicated in a more recent study by Banks and Mazzonna (2012). Using the 1947 change to the minimum school leaving age in England from 14 to 15

¹ For a discussion of ‘bad controls’, see Angrist and Pischke (2008).

as an IV for education, the authors find a large and significant effect of the school reform on males' memory and executive functioning at older ages. Nonetheless, they do not find any statistical evidence of a positive effect of education on the CASP-19, a measure of quality of life that comprises four domains (control, autonomy, self-realisation and pleasure) that has been widely used by psychologists.

Evidence of a positive and statistically well-determined effect of compulsory schooling on life satisfaction is important in the recent human capital literature (for a review see Oreopoulos and Salvanes, 2011). Yet like all previous reduced-form estimates, the coefficient on compulsory schooling in a life satisfaction equation only demonstrates that schooling affects individuals' evaluations of their life in a positive and statistically significant manner. To the extent that there is a large pecuniary return to education, we would ideally want to be able to estimate the indirect effect that schooling has on SWB through income and include that in the total effect of education.

Previous research in economics has focused primarily on the estimation of financial returns to education without ever attempting to link those returns to mental health and well-being. For example, while studies have successfully utilised changes to compulsory schooling laws in the United States and in the United Kingdom to estimate the causal effects of education on wages (e.g., Angrist and Krueger, 1991; Harmon and Walker, 1995; Oreopoulos, 2006), there have been no attempts to estimate how much these effects indirectly cause a change in individual SWB outcomes. The reason for this is simple: estimating causal indirect effects is extremely difficult (Pischke, 2009; Bullock *et al.*, 2010; Green *et al.*, 2010). For instance, in order to establish how much income mediates the causal relationships between schooling and life satisfaction, one needs both education to be randomly distributed across the population, and income to be independent of unmeasured factors that affect life satisfaction. If randomised education shifts income, and randomised income shifts life

satisfaction, then in principle (and assuming that the causal effects of education and income are the same across observations) the direct and indirect effects of education on life satisfaction via income can be estimated free of bias. However, what this kind of “double experiments” approach implies is that at least two sets of IVs or natural experiments (or one of each), with one influencing education but not income directly, and the other influencing income but not life satisfaction directly, are required to be present in the same data set.

Following the framework set out in Figure 1, the current study utilises changes to compulsory schooling laws and data on windfall income in Australia to estimate the direct and indirect effects of schooling on self-assessed life satisfaction, mental health, and financial satisfaction. The key assumptions used in the estimation of the causal mediation effect of schooling are: (i) changes to compulsory schooling laws affect individual level of schooling directly, but are otherwise uncorrelated with total personal income; and (ii) a shock in windfall income affects total income of the respondent directly, but is otherwise uncorrelated with the level of education and other unobserved measures of SWB.

2. Data

The data used in this analysis come from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, a longitudinal survey that has been tracking members of a nationally-representative sample of Australian households since 2001. A total of 7,682 households participated in wave 1, providing an initial sample of 19,914 persons (Wooden *et al.*, 2002). The members of these participating households form the basis of the panel pursued in subsequent annual survey waves. Interviews are conducted with all adults (defined as persons aged 15 years or older) who are members of the original sample, as well as any other adults who, in later waves, are residing with an original sample member. Annual re-interview

rates (the proportion of respondents from one wave who are successfully interviewed the next) are reasonably high, rising from 87% in wave 2 to over 96% by wave 9 (see Watson and Wooden, 2012).

Alongside socio-economic questions typically asked in standard household surveys, the HILDA Survey regularly collects data on a number of SWB measures. Most notably, as part of the personal interview component of the survey, respondents are asked to rate, on a 0 to 10 bipolar scale, their satisfaction with eight aspects of life – the home in which they live, their employment opportunities, their financial situation, how safe they feel, the extent to which they feel part of their local community, their health, the neighbourhood in which they live, and the amount of free time they have. This is then followed by a question about overall life satisfaction. The question reads: “All things considered, how satisfied are you with your life? Again, pick a number between 0 and 10 to indicate how satisfied you are.” A visual aid is used in the administration of these questions, which involves a pictorial representation of the scale with the extreme points labelled “totally dissatisfied” and “totally satisfied”.

In addition, as part of a separate self-completion questionnaire given to all persons interviewed, the Short Form (SF36) Health Survey is administered. Described in more detail by Ware *et al.* (2000), the SF36 is a survey of generic health concepts that has been extensively tested and used around the world. It comprises 36 items that can then be used to construct multi-item scales measuring eight different health concepts. One of those sub-scales is the Mental Health Inventory (MHI-5), a measure of psychological well-being that has proven to be an effective screening instrument for identifying persons with mental health problems in large populations (e.g., Rumpf *et al.*, 2001; Hoeymans *et al.*, 2004). It comprises five items that assess frequency (on a 6-point scale) of symptoms of anxiety and mood disturbance over the 4-week period preceding interview. The response options range from “all of the time” to “none of the time”, with all response options fully labelled. Like all SF36

sub-scales, raw scores on each item are summed and then standardised so that the scale values range from 0 to 100. Relatively low scores are indicative of a poor mental health state. While the MHI-5 is measuring something distinctly different from overall life satisfaction, the two measures are quite strongly correlated, with the wave specific Pearson correlations ranging from 0.44 to 0.49.

For the purpose of the current study, our measures of SWB are: (i) mental health (MHI-5); (ii) overall life satisfaction; and (iii) financial satisfaction. The rationale for choosing financial satisfaction as a potential subjective outcome is because income is being considered here as a potential mediator of schooling, and is expected to have a direct effect on financial satisfaction, even if it does not have a large impact on overall life satisfaction. Further, financial satisfaction is an interesting subjective outcome in its own right. The measure has been used many times in economics as an important component of one's domain satisfactions (e.g., Schwarze, 2003; Easterlin, 2006).

Following Leigh and Ryan (2008), estimates of years of education are derived from respondents' highest educational attainment. Thus a respondent reporting having completed secondary school (Year 12) is assumed to have completed 12 years of education, a person completing an ordinary university degree is assumed to have completed 15 years of education, and so on. As is conventional, we are not measuring actual years spent in education (which would vary with the time with which qualifications are completed, the number of qualifications obtained, and time spent studying that did not lead to a qualification) but instead the time typically taken to obtain the highest qualification reported.

Also following Leigh and Ryan (2008), we allow the variation in schooling to be instrumented by the interaction between the within-state variation in compulsory school

leaving age and the birth year of the respondent.² The compulsory schooling age by birth cohort for each of the Australian states and territories and how it has changed over time is depicted in Appendix A.

In addition, the HILDA Survey also asks individuals to report the amount of their personal income by source (e.g., wages and salaries, government benefits, dividends, business ownership, etc.) for the preceding financial year (year ended 30 June). This includes amounts from irregular sources, or what we describe as windfall income. We focus our attention on particular types of windfall income; namely inheritances, bequests, redundancy or severance payments, and other irregular sources of payments such as income from lottery wins. About 4% of the entire sample (2,192 observations) reported receiving these types of windfall income at least once during the first ten years of the panel. Of these: 1,341 reported receiving it only once; 592 reported having received windfall income in two different years; and 259 reported having received windfall income three or more times throughout the panel. The average real windfall income across all individuals (with and without windfall income) in a given year is A\$1,029 per annum, with a large standard deviation of A\$13,792. There is also very little gender difference in mean windfall income. The average real annual windfall income is A\$1,135 for females and A\$999 for males. When we exclude all observations with zero windfall income in each of the surveyed years, the average real annual windfall income among those with positive windfall income is A\$33,980 for men and A\$35,060 for women.

The sample used in the analysis consists of all adults aged 22 to 65 who participated in any of the first ten survey waves and who responded to the life satisfaction, financial satisfaction, and mental health questions, recorded positive total annual personal income (regular plus irregular income), and completed secondary school in Australia. This leaves 62,780 observations (9,799 persons). Of these, 29,528 (4,732 persons) are males and 33,252

² See Leigh and Ryan (2005) for a detailed discussion of school leaving age policies in Australia.

(5,067 persons) are females. A potential drawback of the current HILDA Survey dataset is that we do not have information about the state in which the respondent attended school, and so we proxy this by the current state of residence. See Appendix B for a table of descriptive statistics for each of the variables used in our analysis.

3. Empirical Estimation Strategy

4.1. Establishing Causal Pathways

The current study contributes to the literature by attempting to establish the causal mechanisms of the schooling effects on mental health and well-being through income in Australia. To determine the direct and indirect effects of education on SWB, we are interested in estimating an equation of the following form:³

$$\text{SWB}_i = \alpha_0 + b\text{Educ}_i + c\text{Income}_i + \varepsilon_i, \quad i = 1, 2, \dots, N \quad (1)$$

Educ_i is the individual's total number of years of education; Income_i is log of real total personal income of individual i ; SWB_i is a measure of subjective well-being; and ε_i is the error term. Suppose that both Educ_i and Income_i are not randomly assigned. Also suppose that Educ_i causes Income_i . Then:

$$\text{Income}_i = \lambda_0 + a\text{Educ}_i + u_i \quad (2)$$

Now suppose that we have a variable Z_{1i} , which is a valid instrument for Educ_i in (2), and a variable Z_{2i} , which is a valid instrument for Income_i in (1). Then a system of instrumental variables (IV) regressions of SWB_i on Income_i and Educ_i with Z_{1i} as the instrument for Educ_i

³ For a summary of how to model causal mechanisms in empirical economics, see Pischke (2009).

and Z_{2i} as the instrument for Income_i identifies b and c . To illustrate this, consider the following reduced forms:

$$\text{Educ}_i = \gamma_0 + \gamma_1 Z_{1i} + \mu_i \quad (3)$$

$$\text{Income}_i = \Pi_0 + \Pi_1 Z_{1i} + \gamma_2 Z_{2i} + \eta_i \quad (4)$$

$$\text{SWB}_i = \Lambda_0 + \Lambda_1 Z_{1i} + \Lambda_2 Z_{2i} + v_i \quad (5)$$

The reduced forms have causal interpretations, where the coefficients are given by: $\Pi_0 = \lambda_0 + a\gamma_0$; $\Pi_1 = a\gamma_1$; $\Lambda_0 = \alpha_0 + a\gamma_0 + b(\lambda_0 + a\gamma_0)$; $\Lambda_1 = \gamma_1(ac + b)$; $\Lambda_2 = c\gamma_2$. Since Z_{1i} and Z_{2i} are independent by random assignment, and assuming that b and c are constant, then IV will identify b and c in (1). To calculate the indirect (or mediating) effect of Educ_i on SWB_i through Income_i , we estimate the following reduced form income equation:

$$\text{Income}_i = \zeta_0 + \zeta_1 Z_{1i} + \nu_i, \quad (2')$$

where $\zeta_0 = \Pi_0 = \lambda_0 + a\gamma_0$; $\zeta_1 = a\gamma_1$. This enables us to identify a in (2). The indirect effect can then be calculated simply as the product of a and c (Baron and Kenny, 1986).

3.2. Instrumenting for Years of Education

Previous research has used a wide range of variables to instrument for schooling. Included here are distance from home to college (Card, 1995), the introduction of restrictive compulsory schooling laws (Harmon and Walker, 1995), and regional spending on education in regions where the individual was a student (Berger and Leigh, 1989). In this paper, we follow Leigh and Ryan (2008) and use changes in the school leaving laws in Australia (which were introduced in different states at different times) interacted with the birth year of the

respondent to instrument for $Educ_i$. The rationale behind our decision to interact the two indicator variables together is that it allows the effect of compulsory schooling laws on years of education to operate differently for different birth cohorts.⁴ Provided the instruments strongly predict $Educ_i$ but not $Income_i$ and SWB_i directly, we can treat the predicted $Educ_i$ as randomly distributed among those who complied with the laws.

3.3. Instrumenting for income

The issue of income endogeneity when income is an explanatory variable is traditionally difficult to deal with. Studies in psychology have consistently shown that people who are extroverted and resilient are more likely to be happier with life, as well as more productive in the labour market (e.g., Judge *et al.*, 1997; Kivimaki *et al.*, 1997; Salgado, 1997). This is reflected in evidence that happier people earn more than others in general (see, for example, Graham *et al.*, 2004; DeNeve and Oswald, 2012), resulting in upward bias in the estimate of c . By contrast, income is likely to be highly correlated with work hours, higher aspirations for more incomes and status, time spent away from family and loved ones, and time spent commuting to and from work, as well as subject to the usual attenuation bias from mis-measurement, all of which could potentially bias the estimated income effect towards zero. What this implies is that *a priori* the direction of the overall bias on the estimate of c is unclear.

Understandably little has been done to address the income endogeneity issue in happiness research. Luttmer (2005), Pischke (2011) and Levinson (2012) use industry codes or industry wage differentials to instrument for family income in life satisfaction equations. Since industry wage differentials most likely reflect rents rather than differences in

⁴ See Leigh and Ryan (2008) for a full description of the changes in the school leaving law in Australia; e.g., the extent and timing of each law in each different state.

unobserved skills and worker sorting, their argument is that workers in a high wage and a low wage industry may not be very different in terms of unobserved characteristics but workers in high wage industries will tend to have higher wages, earnings, and family incomes.⁵ Very differently, Powdthavee (2010) instruments family income using the proportion of household members showing the interviewer their payslips. The rationale behind this instrument is that family income is likely to be more accurately measured when payslips are shown, thereby minimising measurement error bias, while there is no reason to believe the reference to a payslip should have any impact on reported life satisfaction. Overall, these studies have shown that IV estimates of the income effect are either similar to or slightly larger than those obtained using ordinary least squares (OLS).

In finding an appropriate approximation for Z_{2i} , the current study utilises the longitudinal property of the HILDA Survey data and uses within-person variations in windfall income to instrument for the total income of an individual. The identification strategy here is that while people with certain fixed characteristics may be more likely than others to receive windfall income, a positive within-person shock in the level of windfall income should not have a direct effect on SWB_i beyond its effects on total income. This is a plausible assumption, and receives support from other survey evidence showing that, holding income and other things constant, SWB_i does not increase *in the year* of a lottery win (Gardner and Oswald, 2007; Oswald and Winkelmann, 2008; Apouey and Clark, 2010).⁶ In other words, our exclusion restriction relies on the assumption (and statistical evidence) that there is no immediate *net* effect that runs from winning a lottery to people's mental health and well-being. More specifically, we allow Z_{2i} (that is, annual personal windfall income) to

⁵ Pischke and Schwandt (2012), however, report evidence that casts doubt on the exogeneity of industry affiliation as a valid instrument for income.

⁶ Although there is some evidence of a delaying effect of a lottery win on SWB of approximately two years.

vary by t and decompose it into its mean over the observation period and the deviation from that mean as follows:

$$Z_{2it} = \bar{Z}_{2i} + (Z_{2it} - \bar{Z}_{2i}) \quad (6)$$

Introduction of the within-person mean windfall income (\bar{Z}_{2i}) in both first-stage and second-stage regression equations helps to correct for the fact that people with some unobserved fixed traits are more likely than others to receive windfall income (Mundlak, 1978). Provided that $Z_{2it} - \bar{Z}_{2i}$ (the within-person deviation in annual windfall income) is exogenous, we can use it to predict income in the first-stage regression equation. Moreover, in order to get a better predicted income variable that is representative across people who were exposed to different compulsory schooling laws at different times, we also allow the effect of $Z_{2it} - \bar{Z}_{2i}$ on income to operate differently for different laws at different times. What this implies is that the IV for Income_i becomes $Z_{2it} - \bar{Z}_{2i}$ interacted with Z_{2i} (or the within-person change in annual windfall income \times compulsory schooling laws \times birth year).

All of the equations set out above can be estimated using the random effects estimator. However, given we would also want to account for clustering at both the state-birth year level and the individual level, it seems better to follow the empirical strategy outlined in Cameron *et al.* (2006) and allow for a non-nested two-way clustering by personal identification and by state \times birth year in all of our regressions.

Finally, the system of simultaneous equations that we can estimate to gauge the direct and indirect effects of schooling on SWB is given by

$$\text{SWB}_i = \alpha_0 + \beta_0 \text{Educ}_i + \gamma_0 \text{Income}_i + X_i' \lambda_0 + \varphi_0 \bar{w}_i + \varepsilon_{0i} \quad (7)$$

$$\text{Educ}_i = \alpha_1 + \delta_1 (\text{Compulsory schooling}_i \times \text{Birth year}_i) + X_i' \lambda_1 + \varphi_1 \bar{w}_i + \varepsilon_{1i} \quad (8)$$

$$\text{Income}_i = \alpha_2 + \lambda_2 (w_{it} \times \text{Compulsory schooling}_i \times \text{Birth year}_i) + X_i' \lambda_2 + \varphi_2 \bar{w}_i + \varepsilon_{2i} \quad (9)$$

where: X_i' represents a vector of exogenous control variables, including linear and quadratic age effects, year dummies, birth year dummies, and state dummies; \bar{w}_i denotes the within-person mean of yearly personal windfall income; and w_{it} is the yearly personal windfall income measured in period t . Following Devereux and Hart (2010), who conclude that the effect of compulsory schooling on wages in the UK is only positive for males but not for females, we estimate the direct and indirect effects of schooling on SWB by gender.

4. Results

Is there a causal link between schooling and SWB in Australia? To make a first pass at this, Table 1 estimates and reports simple linear regressions in which SWB is the dependent variable and years of education is the independent variable of interest. The regression equations are estimated using OLS with standard errors corrected for two-way clustering by personal identification and by state \times birth year. The exogenous control variables in these equations are respondent age, age-squared ($/100$), birth year dummies, state dummies, and year dummies.

Looking at the first panel of Table 1 (the first three numeric columns), in which schooling is assumed to be exogenous in the SWB equations, we can see that, for both men and women, years of education enters positively and statistically significantly at conventional confidence levels in mental health and financial satisfaction equations. On the other hand, the estimated relationship between education and overall life satisfaction is positive and statistically significant for men but not women. These positive associations are, however, likely to be confounded by omitted variables such as income, abilities, and personality traits.

We begin instrumenting for years of education with Compulsory School Law \times Birth Year as IVs in the second panel of Table 1 (the last three columns). While many of the individual interactions between Compulsory School Law and Birth Year are not statistically significant in both male and female samples, a test of the joint significance of the excluded variables produces a large F-statistic for both samples ($F_{(79, 298)} = 3894.95$ in the male sample and $F_{(77, 300)} = 312.58$ in the female sample), indicating that the IVs are jointly significant predictors of years of education.

Instrumenting for years of education leads to an increase in the size of the estimated schooling coefficient in the mental health equation for men but not for women; the coefficient on years of education increases almost three-fold from 0.7 to 1.8 in the mental health equation for men. However, an extra year of education is no longer statistically significantly associated with higher levels of financial satisfaction for men after instrumentation. The estimated local average treatment effect (LATE) of education on SWB is positive and statistically well-determined only in the mental health equation for men. By contrast, the LATE of education is negative and marginally significant in the life satisfaction equation, and statistically insignificantly different from zero in the mental health and financial satisfaction equations for women. Hence, what we have here is empirical evidence that changes in the compulsory schooling laws have a positive and statistically significant *net* effect on only one of the three measures of SWB for men and zero net effect on all three measures of SWB for women in Australia.

One interpretation of these results is that an extra year of education contributes very little to the overall well-being of people who complied with the law in the long-run. Our hypothesis, however, is that compulsory schooling affects SWB through different positive and negative channels, one of which is the positive effect it has on income, which may cancel each other out on aggregate. To test this hypothesis, we estimate an equation in which the

dependent variable is log of total real annual personal income and report the estimates for men and women in Table 2. Education appears to affect men's and women's income differently. Consistent with the differential effects of compulsory schooling on wages by gender found by Devereux and Hart (2010), the estimated LATE of schooling in Australia is higher for men than for women: the rates of return to personal income of one more year of schooling are estimated to be 15.5% for men and 10.9% for women.

Table 3 moves on to estimate the direct and indirect effects of schooling on SWB by gender. The first three columns estimate, for men and women, SWB equations in which schooling is exogenous (through instrumentation) but real total personal income is not. The last three columns then estimate SWB equations in which both schooling and real total personal income are simultaneously instrumented by their respective IVs. As described in the preceding section, the IV for real total personal income here is the shock in personal windfall income, w_{it} , interacted with the exogenous variations in compulsory schooling law and birth year. It is worth noting here that a test of the joint significance of the excluded variables produces a large F-statistic for both samples ($F_{(90, 298)} = 4998.11$ in the male sample and $F_{(90, 300)} = 51771.56$ in the female sample), indicating that the IVs are jointly highly significant predictors of income for both genders.

The first three columns of Table 3 show that there is a positive and statistically significant association between income and SWB in most cases for men and women. However, it is also worth noting that the magnitudes of the correlation are notably smaller in the female sample than in the male sample when the only control variables – other than the instrumented years of education – are the exogenous characteristics of the respondent that are age, age-squared, birth year, and state of residence. After instrumenting for income, we can see from the last three columns that income contributes to higher SWB in all specifications

for men but not for women. For example, a positive shock in income increases life satisfaction, mental health and financial satisfaction in the male sample. Women, on the other hand, only report a significantly higher level of financial satisfaction and a marginally higher level of mental health with instrumented income. In virtually all cases for men, we also find that it makes little differences to the size of the estimated income coefficient whether one assumes income to be exogenous or endogenous (and therefore requires instrumentation) in the SWB regression equations. The male sample's results are consistent with previous studies which have found IV estimates to be similar to OLS estimates, suggesting that most of the association between income and SWB is causal (Luttmer, 2005; Powdthavee, 2010; Pischke, 2011).

The positive and statistically well-determined LATE that we observed earlier for male's mental health in Table 1 continues to be statistically significantly different from zero when randomised income is included as a regressor, although there is a reduction in the magnitude of the effect from 1.841 to 1.135. What this implies is that education affects male's mental health both directly and indirectly through income. By contrast, most of the causal effect of education on male's life satisfaction and financial satisfaction, but not female's, is indirectly channeled through the effect of income.

In an attempt to determine the statistical significance of the estimated indirect effects, Table 4 uses the coefficients reported in Tables 2 and 3 to calculate the indirect effect of schooling on SWB through income. The standard errors for the indirect effect are calculated using the Sobel test (Preacher and Leonardelli, 2013), an appropriate method for testing the significance of the mediation effect given sufficiently large N (MacKinnon *et al.*, 2002).⁷

⁷ Unfortunately, given the unusually complex nature of our IV and multi-way clustering analysis (rather than the simultaneous equation modeling technique typically applied in the literature), we were unable to apply the bootstrapping method in order to generate the standard errors for our product coefficients here.

The indirect effect of changes in the compulsory schooling laws on life satisfaction, mental health, and financial satisfaction are given by the product of the years of education coefficient obtained in Table 2 and the income coefficients reported in Table 3. Assuming that the estimated effect of income is homogeneous across people within the same gender group who complied with the compulsory schooling laws, we find the estimated indirect effect of an extra year of education on life satisfaction to be positive and statistically well-determined for men but not for women: the product coefficients are 0.037 [*S.E.*=0.014] in the male sample and 0.006 [*S.E.*=0.006] in the female sample. A similar pattern is also observed for mental health: the estimated indirect effect of schooling on mental health is 0.471 [*S.E.*=0.163] for men and 0.082 [*S.E.*=0.079] for women. On the other hand, the indirect effect of schooling on financial satisfaction appears to be positive and statistically well-determined for both genders, though noticeably larger for males than females: the estimated indirect effect of schooling on financial satisfaction is 0.130 [*S.E.*=0.032] in the male sample and 0.043 [*S.E.*=0.027] in the female sample. Given the distribution of SWB, these are not small effects. They are equivalent to around 2%, 3%, and 6% of the standard deviations of male's life satisfaction, mental health, and financial satisfaction, respectively. These statistically significant indirect effects of education through income further strengthen our earlier conclusion that most of the causal effect of schooling on SWB for men is mediated almost completely through income.

One possible objection to our results is that the effect of income may not be constant across all individuals in the sample, especially those who were affected by the changes in the compulsory schooling laws. This is a fair objection as (i) different instruments with different compliers were used to estimate the simultaneous equations, and (ii) only a minority of people was affected by shocks in windfall income. What this implies is that it will be no

longer possible to decompose the LATE of education on SWB into direct and indirect effects if we cannot assume a constant average treatment effect (ATE) for income.

While it may not be possible to completely address this issue, we can nevertheless try to make an inference about the size of the ATE of income on SWB based on previous findings and our own estimates, and use that as a basis for our calculations of the indirect effects of schooling. Using different IVs and natural experiments to gauge the effect of income on SWB, scholars working in this area have reached the conclusions that (i) income causally affects SWB in a positive and statistically significant way, and (ii) the magnitude of the IV coefficient on income is either the same as or larger than the income coefficient obtained in the OLS estimation (Frijters *et al.*, 2004; Luttmer, 2005; Powdthavee, 2010; Li *et al.*, 2011; Pischke, 2011). Note that (ii) is also the same as what had been found earlier in our Table 3, thus suggesting that what had been found in terms of the LATE of income on SWB may already be close to mirroring the population ATE.

Table 5, as a further check, re-estimates Tables 2 and 3's specifications but on smaller sub-samples of relatively more homogenous groups of men and women who report positive personal windfall income, thus ignoring the majority of people who report zero income from bequest, inheritance, severance payment, lottery wins and other irregular sources in any given year. In other words, we now have a population in which *everyone* is affected by one instrument (a shock in windfall income), although not necessarily by the other instrument (compulsory schooling). As can be seen, the magnitudes of the estimated indirect effects on male's SWB are, with the exception of mental health, very similar to what had been previously observed in when using the full male sample (see Table 4). However, the same cannot be said for women, where all of the estimated indirect effects of education on SWB are now imprecisely estimated using the small sub-sample.

Finally, Table 6 explores whether the statistically insignificant indirect effects obtained in the female sample's regressions are due to the decision to use personal income and not equivalised household income as the mediator of schooling on SWB. One could imagine, for instance, that schooling affects personal and household income differently for females, and that it is household income and not personal income that is most relevant for SWB. In Table 5, the log of real total annual personal income is replaced by the log of equivalised real annual household income.⁸ The IV for equivalised household income, Z_{2i} , is the average shock of pooled windfall income, and \bar{Z}_{2i} is the within-household average of pooled windfall income.

Replacing personal income with equivalised household income makes little difference to the results for men. With equivalised household income as the mediator, the magnitudes of the indirect effects of education on male's life satisfaction, mental health, and financial satisfaction are, at 0.035, 0.318, and 0.117, respectively similar to the specification where personal income is used as the mediator. For women, however, the use of equivalised household does make more of a difference. The estimated LATE of schooling on equivalised household income is positive and statistically significant for women at 0.062 [S.E.=0.031]. The IV estimate of the income effect is now positive and statistically significant in all SWB equations for women, thus suggesting that it is the shared household income – rather than personal income – that contributes to higher levels of SWB for women. As a result, we now observe an estimated indirect effect of education that is positive and marginally significant at the 10% level in the life satisfaction equation, as well as continuing to observe a statistically significant indirect effect that runs from years of education to financial satisfaction. Hence,

⁸ The equivalised real annual household income is calculated using the following formula: real annual household income/(1 + 0.5*(number of adult household members-1) + 0.3*(number of children aged 0-4 in the household + number of children aged 5-9 in the household + number of children aged 10-14 in the household)).

we were able to marginally improve upon our results for women by substituting personal income with equivalised household income.

5. Conclusions

According to the traditional investment model, people invest in education in hopes of greater lifetime wealth and consumption in return. And while evidence of a significant financial return to schooling is well-established in the economic literature, we continue to know very little about the extent to which this effect might contribute to increases in the overall well-being of individuals.

In this paper, we empirically demonstrate that, for individuals who were affected by changes in the compulsory schooling laws in Australia, an extra year of education improves the SWB of these individuals primarily by raising incomes. In other words, with the exception of men's mental health, we find no evidence of an extra year of education causing higher SWB independently of income even when the causal effect of income is properly modeled in the estimation of SWB regression equations. We also find that men are significantly more affected by the indirect effect of compulsory schooling laws than women, although for both males and females an extra year of education statistically significantly enhances satisfaction with their overall financial situation – with the effect being larger for men than for women.

Why do we find the net effect of an extra year of education on life satisfaction and financial satisfaction to be statistically insignificant when we also find the effect of education on income and the effect of income on SWB to be significantly different from zero? The most likely reason is straightforward; changes in compulsory schooling laws do very little to

improve a person's life beyond its impact on his or her lifetime income. Alternatively, it might be argued that there are significant non-pecuniary benefits of schooling induced through compulsory schooling laws – e.g., better health and more job security – but these positive effects on SWB have been completely offset by other negative effects that typically come from having more years of education. This might include, for example, higher aspirations and more work-related stress (Stutzer, 2004; Shields *et al.*, 2009). Future research may need to extend from the current analysis and estimate a model of multiple indirect effects that would allow us to determine the relative importance of mediating factors of schooling – other than income – on overall well-being. Also, given that ‘years of education’ is not a particularly good measure of education, it seems important for future researchers to examine the direct and indirect effects of other measures of education such as school quality and degree attainment on SWB.

More generally, we believe that future researchers working in the area will benefit from a similar modeling strategy in their attempts to better understand the causal pathways through which schooling affects their outcome variables of interest.

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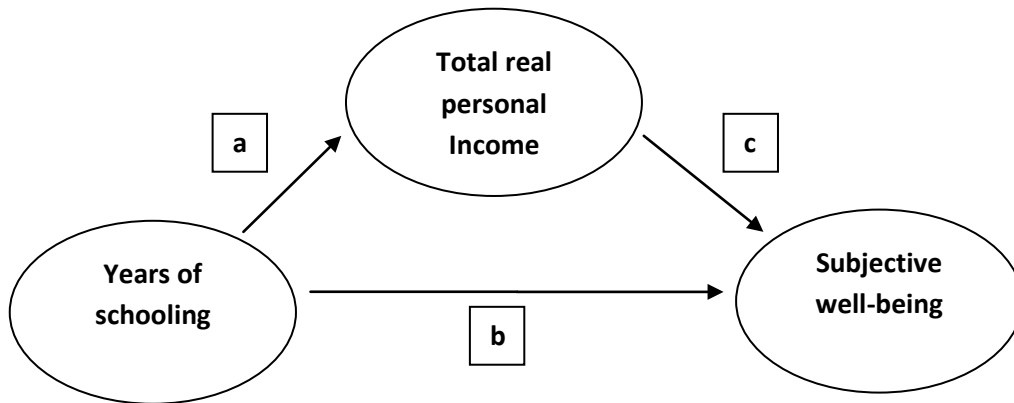
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Fig. 1: *Direct and Indirect effects of Schooling on Subjective Well-being*



Note: *A* represents the effect of schooling on total real personal income; *B* represents the effect of real total personal income on measures of subjective well-being; and *C* represents the direct effect of schooling on measures of subjective well-being (given earnings).

Table 1

*Reduced-form Regressions With and Without Compulsory School Laws as Instruments for
Years of Education*

	OLS Regressions			IV Regressions		
	LSAT	MH	FSAT	LSAT	MH	FSAT
A) Men (N = 29,629)						
Years of education	0.015* [0.008]	0.663*** [0.097]	0.168*** [0.012]	0.064 [0.052]	1.841*** [0.649]	0.156 [0.098]
B) Women(N = 33,404)						
Years of education	0.002 [0.007]	0.779*** [0.092]	0.156*** [0.011]	-0.100* [0.053]	-0.331 [0.777]	0.005 [0.117]

Notes: LSAT = Life Satisfaction; MH = Mental Health; FSAT = Financial Satisfaction. All equations include age, age-squared, birth year fixed effects, state fixed effects, and wave fixed effects. Instrumental variables for schooling = Compulsory School Law \times Birth Year (see Leigh and Ryan, 2008). All equations also allow for multiple clustering by (i) personal identification and (2) State \times Birth Year (Cameron *et al.*, 2008). The sample consists only of males and females with positive personal income.

* $<10\%$; ** $<5\%$; *** $<1\%$. Standard errors are in parentheses.

Table 2

Estimating the Causal Schooling Effect on Log of Real Total Annual Personal Income

	All men (N = 29,629)	All women (N = 33,404)
Years of education	0.155*** [0.033]	0.109** [0.043]

Notes: All equations include age, age-squared, birth year fixed effects, state fixed effects, and wave fixed effects. Instrumental variables for schooling = Compulsory School Law \times Birth Year.

<5%; *<1%. Standard errors are in parentheses.

Table 3

Estimating the Causal Schooling Effect on Mental Health and Well-being Through Income

A) Men	All men (N = 29,629)			All men (N = 29,629)		
	LSAT	MH	FSAT	LSAT	MH	FSAT
Log of real total personal income (assumed exogenous)	0.200*** [0.056]	2.422*** [0.633]	0.871*** [0.091]			
Log of real total personal income(IV)				0.239*** [0.072]	3.041*** [0.831]	0.841*** [0.106]
Years of education (IV)	0.012 [0.062]	1.121 [0.709]	-0.023 [0.101]	0.023 [0.049]	1.135** [0.555]	0.037 [0.078]
B) Women	All women (N = 33,404)			All women (N = 33,404)		
	LSAT	MH	FSAT	LSAT	MH	FSAT
Log of real total personal income (assumed exogenous)	0.033 [0.041]	1.690*** [0.519]	0.288*** [0.091]			
Log of real total personal income(IV)				0.056 [0.052]	0.750 [0.666]	0.394*** [0.087]
Years of education (IV)	-0.095 [0.058]	-0.555 [0.753]	-0.012 [0.132]	-0.070 [0.042]	-0.225 [0.603]	-0.003 [0.096]

Notes: See Table 2. Mean windfall income over the previous financial year is included as an additional control variable here. The instrumental variable for total real personal income is the shock in incomes received from bequest, inheritance, redundancy payment, lottery wins and other irregular incomes and its interaction with Compulsory School Law \times Birth Year to allow for the heterogeneous effects of windfall income on real total annual personal income by people who were exposed to different compulsory schooling laws.

<5%; *<1%. Standard errors are in parentheses.

Table 4*Indirect Effects of Schooling on Mental Health and Well-being Through Individual Income*

	All men (N = 29,629)			All women (N = 33,404)		
	LSAT	MH	FSAT	LSAT	MH	FSAT
Path coefficient <i>a</i> : <i>Educ</i> → <i>Income</i>	0.155*** [0.033]	0.155*** [0.033]	0.155*** [0.033]	0.109** [0.043]	0.109** [0.043]	0.109** [0.043]
Path coefficient <i>c</i> : <i>Income</i> → <i>SWB</i>	0.239*** [0.072]	3.041*** [0.831]	0.841*** [0.106]	0.056 [0.052]	0.750 [0.666]	0.394*** [0.087]
Calculating indirect effects						
Indirect effect of schooling $a \times c$	0.037*** [0.014]	0.471*** [0.163]	0.130*** [0.032]	0.006 [0.006]	0.082 [0.079]	0.043** [0.027]
S.D. of the dependent variable	1.481	16.520	2.211	1.484	17.350	2.300
Indirect effects as % of that S.D.	2.50%	2.85%	5.91%	0.40%	0.47%	1.87%

Notes: Standard errors of the indirect effect are calculated using the Sobel test and reported in parentheses (Preacher and Leonardelli, 2013).

<5%; *<1%.

Table 5*Indirect Effects of Schooling on Mental Health and Well-being for People with Positive Personal Annual Windfall Income*

	Men with positive personal windfall income (N = 999)			Women with positive personal windfall income (N = 948)		
	LSAT	MH	FSAT	LSAT	MH	FSAT
Path coefficient <i>a</i> : <i>Educ</i> → <i>Income</i>	0.150*** [0.043]	0.150*** [0.043]	0.150*** [0.043]	0.063 [0.050]	0.063 [0.050]	0.063 [0.050]
Path coefficient <i>c</i> : <i>Income</i> → <i>SWB</i>	0.302*** [0.097]	1.615 [1.078]	0.820*** [0.130]	0.047 [0.085]	-0.330 [0.954]	0.566*** [0.111]
Calculating indirect effects						
Indirect effect of schooling $a \times c$	0.045** [0.019]	0.242 [0.176]	0.123*** [0.040]	0.003 [0.006]	-0.021 [0.062]	0.036 [0.062]
S.D. of the dependent variable	1.484	15.790	2.167	1.452	16.724	2.331
Indirect effects as % of that S.D.	3.03%	1.53%	5.68%	0.21%	-0.13%	1.54%

Notes: Standard errors of the indirect effect are calculated using the Sobel test and reported in parentheses (Preacher and Leonardelli, 2013).

<5%; *<1%.

Table 6

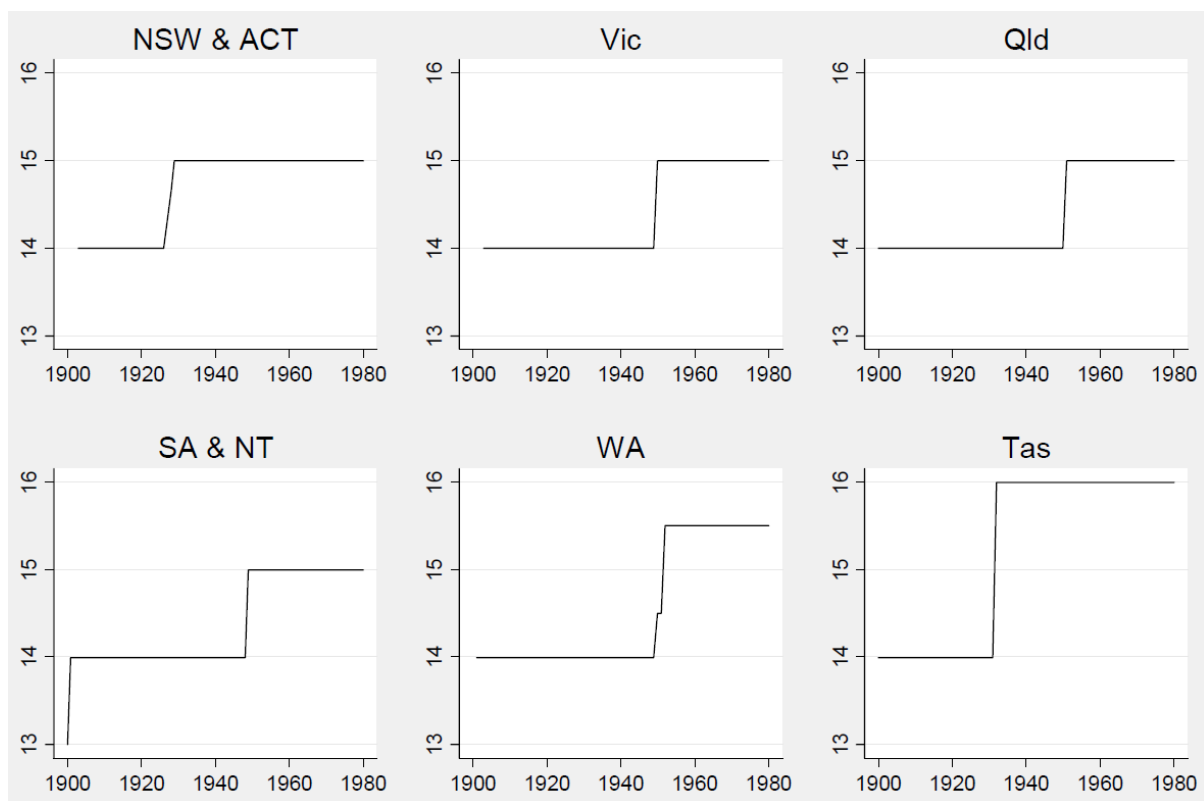
Indirect Effects of Schooling on Mental Health and Well-Being Through Equivalised Annual Household Income

	All men (N = 29,892)			All women (N = 33,948)		
	LSAT	MH	FSAT	LSAT	MH	FSAT
Path coefficient <i>a</i> : <i>Educ</i> → <i>Income</i>	0.124*** [0.027]	0.124*** [0.027]	0.124*** [0.027]	0.062** [0.031]	0.062** [0.031]	0.062** [0.031]
Path coefficient <i>c</i> : <i>Income</i> → <i>SWB</i>	0.280*** [0.078]	2.563*** [0.909]	0.940*** [0.120]	0.189*** [0.063]	1.620*** [0.625]	0.771*** [0.098]
Calculating indirect effects						
Indirect effect of schooling $a \times c$	0.035*** [0.012]	0.318** [0.132]	0.117*** [0.029]	0.012* [0.007]	0.100 [0.063]	0.048* [0.025]
S.D. of the dependent variable	1.483	16.519	2.216	1.510	17.353	2.316
Indirect effects as % of that S.D.	2.36%	1.93%	5.28%	0.79%	0.58%	2.07%

Notes: Standard errors of the indirect effect are calculated using the Sobel test and reported in parentheses (Preacher and Leonardelli, 2013).

*<10%; **<5%; ***<1%.

Appendix A: Compulsory School Leaving Age by Birth Year



Source: Leigh and Ryan (2005).

Appendix B: Summary of Descriptive Statistics

Variables	Men		Women	
	Mean	S.D.	Mean	S.D.
Years of education	12.451	2.309	12.372	2.439
Life satisfaction	7.754	1.463	7.876	1.484
Mental health (MHI-5)	75.358	16.541	73.291	17.350
Financial satisfaction	6.335	2.198	6.304	2.300
Ln (total real annual personal income)	10.285	0.863	9.701	0.998
Annual real windfall income/1000	1.741	22.927	1.287	15.016
Age	44.144	10.510	43.903	10.586
<i>N</i>	29,629		33,404	