THE ECONOMIC EFFECT OF GAINING A NEW QUALIFICATION IN LATER LIFE[*]

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Abstract

Pursuing educational qualifications later in life is an increasingly common phenomenon within OECD countries since technological change and automation continues to drive the evolution of skills needed in many professions. We focus on the causal impacts to economic returns of degrees completed later in life, where motivations and capabilities to acquire additional education may be distinct from education in early years. We find that completing and additional degree leads to more than $3000 (AUD, 2019) per year compared to those who do not complete additional study. For outcomes, treatment and controls we use the extremely rich and nationally representative longitudinal data from the Household Income and Labour Dynamics Australia survey is used for this work. To take full advantage of the complexity and richness of this data we use a Machine Learning (ML) based methodology to estimate the causal effect. We are also able to use ML to discover sources of heterogeneity in the effects of gaining additional qualifications, for example those younger than 45 years of age when obtaining additional qualifications tend to reap more benefits (as much as $50 per week more) than others.

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1 Introduction

Pursuing educational qualifications later in life is an increasingly common phenomenon within OECD countries (OECD, 2016). Technological change and automation continues to drive the evolution of skills needed in many professions, or to oust the human workforce in others. This is particularly true for middle-income workers performing routine tasks (Autor, Katz and Kearney, 2008, Acemoglu and Autor, 2011). Also at the lower end of the income-distribution, such as among welfare recipients, governments are increasingly trying to promote the idea of life-long learning.

This paper contributes to understanding one efficacy dimension of these policy and individual choices by estimating the causal effects on earnings and by focusing on mature-age students. We add to previous work on the returns to education for 'younger students'. Previous research points to positive and significant wage premiums for younger cohorts with more education, ranging between 5 and 13% (Angrist and Keueger, 1991, Harmon, Oosterbeek and Walker, 2003, Machin, 2006) or even higher than 15% as in the case of Harmon and Walker (1995). The wage returns to education may be more uncertain for older students as they face higher opportunity costs to study and need to navigate a more fragmented system in the postsecondary education setting.

We also add to the literature that investigates the economic returns for mature-age learners at community or training colleges (Jacobson, LaLonde and Sullivan, 2005, Chesters, 2015, Zeidenberg, Scott and Belfield, 2015, Polidano and Ryan, 2016, Xu and Trimble, 2016, Belfield and Bailey, 2017*a*, Dynarski, Jacob and Kreisman, 2016, 2018, Mountjoy, 2022). The evidence on the labour market returns to vocational and community college education is strong and positive, particularly for female students (Belfield and Bailey, 2017*a*, Zeidenberg, Scott and Belfield, 2015, Perales and Chesters, 2017). The results are even stronger once authors account for the different earnings-growth profiles of students and non-students before undertaking the degree (Dynarski, Jacob and Kreisman, 2016, 2018).

By focusing on the one institutional setting – the community or training college – the results of such studies may not be generalisable to the entire mature-age education market, such as to students who seek different degree types or who study at different institutions (Belfield and Bailey, 2017*b*, Mountjoy, 2022). We add to this literature by estimating the returns across all formal degree-types (post-graduate degrees, training certificates, diplomas etc), and spanning all subjects and institutions at which the study took place. This means we analyse the effects for a group of students with a larger span of demographic and socio-economic background characteristics. The broad remit of students that we analyse also allows our study to compliment studies that evaluate government-run training programs, which tend to enrol low-productivity workers (Ashenfelter, 1978, Ashenfelter and Card, 1985, Bloom, 1990, Leigh, 1990, Raaum and Torp, 2002, Jacobson, LaLonde and Sullivan, 2005, Card, Kluve and Weber, 2018, Knaus, Lechner and Strittmatter, 2022).

We contribute the first evidence in systematically identifying which groups of mature-age students tend to benefit more from further education. We also compliment previous studies that already find significant heterogeneity by degree-type, institutional setting, and by the background characteristics of the student (Blanden et al., 2012, Zeidenberg, Scott and Belfield, 2015, Polidano and Ryan, 2016, Dorsett, Lui and Weale, 2016, Xu and Trimble, 2016, Belfield and Bailey, 2017*a*, Perales and Chesters, 2017, Böckerman, Haapanen and Jepsen, 2019). A benefit of a systematic, data-driven approach to heterogeneity analysis is that it can reduce the risk of overlooking important sub-populations compared to less data-driven approaches (Athey and Imbens, 2017, Knaus, Lechner and Strittmatter, 2021).

A key challenge in estimating the causal returns to later-life education is that factors that enable mature-age learners to pursue and complete a qualification may also be precursors to later-life success. Moreover, the drivers of degree completion may be numerous and related to other variables in complex, unknown ways. We use a machine learning (ML) based methodology in this work since it allows us to intensively control for many confounding factors, as well as discover sources of treatment heterogeneity. ML algorithms also automatically discover nonlinear relationships that may be unknown to the researcher. For high-dimensional and complex datasets such as we use in this research, these methodological abilities are crucial in reducing bias from model mis-specification and confounding (e.g. selection into treatment), and reducing variance from correlation/collinearity.

We adapt ML tools for causal inference purposes. We recognise that, as with all statistical models, we make assumptions when we use ML techniques for causal inference, and these need to be tested. One key assumption is that the controls included in the ML models sufficiently account for selection into treatment. We propose to undertake a replication exercise where we compare the results of the ML model with that of baseline models, using Ordinary Least Squares (OLS) and Fixed Effects. We also contrast the selected control variables in the ML model with those that were manually selected in Chesters (2015), and comment on the potential biases from manual variable selection. We have chosen this published work because it uses the same data (HILDA) and examines the same topic.

The results show that an additional degree in later-life increases total future earnings by more than an average of $3,000 per year compared to those who do not complete any further study. We consistently estimate this causal effect using a selection-on-observables strategy based on T-learner, Doubly Robust and Bayesian models. The estimate is based on 19 years of detailed nationally representative Australian data from the Household Income and Labour Dynamics Australia (HILDA) survey. Two dimensions of these data are important. The first is that they contain a wealth of information about each respondent. For example, we begin with more than 3,400 variables per observation, including information about the respondents' demographic and socio-economic background, and on their attitudes and preferences. Access to this broad range of information means that by controlling for them, we can potentially proxy for unobservable differences between those who do and do not obtain a new qualification. Secondly, this dataset contains many variables that are highly correlated, so we require a systematic approach to reduce such information redundancy – something that ML models are adept at.

Our ML approach also identifies new sub-populations for which the treatment effects are different. We document that the starting homeloan amount and employment aspirations are significant factors related to the extent of gain from further study. We also find that the starting levels of and pre-study trends in personal and household income are hugely important. Age and mental health variables also account for variation in estimated effects. All of these variables are consistently selected as being significant for prediction out of the 3,400 features within the HILDA data. This selection is consistent across different ML models (which includes linear and non-linear model classes) and across numerous boostrap draws of the original sample.

Previous studies have found that individuals who seek a futher degree tend to have slower-growing earnings in the period before their study starts compared to similar individuals who do not seek further study (Jacobson, LaLonde and Sullivan, 2005, Dynarski, Jacob and Kreisman, 2016, 2018). By accounting for dynamic selection into obtaining a further degree, we can be confident that we compare the earnings paths of mature-age students to the paths of similar non-students who displayed the same earnings (and other) paths before study began. In this paper, we explicitly control for the trajectories of socio-economic and demographic circumstances before study starts. Standard fixed effects estimation would miss these dynamic confounders. We find that our ML estimates are significantly smaller than the size of the standard fixed effects results. We also estimate lower returns compared to Ordinary Least Squares (OLS) models. We document the additional confounder variables that we include in our models but are usually omitted from standard OLS specifications. These variables suggest there is significant selection into mature-age students who undertake a further degree.

We adapt ML models for the purpose of estimating causal effects. Standard off-the-shelf ML models are better suited to predictive purposes. When obtaining a prediction, off-the-shelf ML models can find generalisable patterns and minimise overfitting issues, though the use of cross-validation, because the true outcomes are observed. This means that we can optimize a goodness-of-fit criterion. Causal parameters, however, are not observed in the data, which means we cannot directly train and evaluate our models.

In this paper, we take the difference between the two optimal outcome models, which can achieve the optimum bias-variance trade-off point for the conditional average treatment effect. Specifically, we model the response surfaces for two conditional mean equations – one using the treatment observations and another using the control observations. We estimate these equations with ML methods such as the T-learner and Doubly Robust. Here, we employ both linear (LASSO and Ridge) and non-linear (Gradient Boosted Regression) model classes. We compare and evaluate their comparative performance using nested cross-validation. We then test the statistical significance of our causal parameters by examining the distribution of the estimates through bootstrapping. Last, we use a variety of Bayesian ML models following the formulation presented in Hahn, Murray and Carvalho (2020) that reduce effect estimation bias within the Bayesian paradigm. These models have several properties that may be desirable, such as the ability directly parameterise heterogeneous prognostic and treatment models.

2 Context: Higher education and Vocational study in Australia

Mature-age education in Australia is among the highest in the world. In 2014, Australia's participation in vocational education by those aged 25-64 was the highest among OECD countries. The tertiary education rate for those aged 30-64 was the second highest (Perales and Chesters, 2017). Mature-age Australians are increasingly enrolling in university or college to change employers, change careers, gain extra skills, improve their promotion prospects and earning capability or search for better work/life balance. Redundancy and unemployment have also been driving forces for individuals to return to education later in life (Coelli, Tabasso and Zakirova, 2012).

The increase in mature-age learners accessing higher education has in part been driven by government policy. In 2009, the Australian government adopted a national target of at least 40% of 25-34-year-olds having attained a qualification at bachelor level or above by 2025 (O'Shea, May and Stone, 2015). This was part of a policy that transitioned Australia to a demand-driven system (Universities Australia, 2020). The policy had a large effect on access to higher education, as it removed the cap on the number of university student places. By 2017, 39% of 25-34-year-olds had a bachelor's degree or higher (Caruso, 2018).

While the initial uptake of university places in the demand-driven system was strong, especially among mature-age students[1] (Universities Australia, 2019), growth in undergraduate enrolments slowed since 2012. In 2018, mature-age enrolments even dropped below the previous year. The 40+ age group showed the worst growth, receding by 10%, while the 25-29's and 30-39's showed growth of around -4% (Universities Australia, 2020). The decline of enrolments coincided with the freezing of the Commonwealth Grant Scheme (CGS) which capped funding at 2017 levels, effectively ending the demand-driven system (Universities Australia, 2020).

Access to Commonwealth Supported Places (CSPs) have since been limited to 2017 levels, with cap raises from 2020 subject to performance measures (Universities Australia, n.d.). As a proportion of the working age population, mature-age students also participated less in vocational education and training (VET) over the same period. It appears the introduction of the demand-driven system also increased VET participation between 2010 and 2012, before continuing its decline (Atkinson and Stanwick, 2016). Total VET enrolments since 2018 stabilised, with 2019 and 2020 enrolments slightly above 2018 levels[2] (NCVER DataBuilder, 2021). The impact of COVID-19 on 2021 enrolments is yet to be fully determined. So far, VET enrolments for the first half of 2021 are well above the previous 4 years across all age groups, with $\sim$1 million enrolments in 2021 compared to $\sim$870 thousand enrolments in 2017[3] (NCVER DataBuilder, 2021).

The cost of a bachelor's degree for domestic students in Australia is the sixth highest among OECD countries (Universities Australia, 2020). In 2018, the average annual cost of a bachelor's degree was around $5,000 in Australia, about half of the top 2 most expensive countries where it costs around $9,000 in the US and $12,000 in the UK[4]. VET and TAFE courses in Australia cost a minimum of $4,000 per year on average while postgraduate courses cost a minimum of $20,000 per year on average[5] (Studies in Australia, 2018).

[1] Between 2010 and 2012, growth in mature-age enrolments in undergraduate courses doubled for the 30-39 age group and tripled for the 40+ age group.

[2] Total VET enrolments 2016-2020.

[3] Government funded program enrolments Jan-June 2017-2021.

[4] Values are in US dollars.

[5] Values are in Australian dollars.

Mature-age students can cover the cost of further study themselves or they can receive support from the government. Students at university or approved higher education providers can access financial support from the Higher Education Loan Program (HELP) scheme, which provides income-contingent loans. This allows students to defer their tuition fees until their earnings reach the compulsory repayment threshold, upon which repayments are deducted from their pay throughout the year at a set rate. Postgraduate students can access the Commonwealth Supported Place (CSP) scheme, which subsidises tuition fees for those studying at public universities and some private higher education providers. However, most CSPs are for undergraduate study.

FEE-HELP is the HELP scheme available to full-fee paying students who don't qualify for a CSP i.e., post-graduate students. VET Students Loans (formerly VET FEE-HELP) are also part of the HELP scheme and are available to students undertaking vocational education and training (VET) courses outside of higher education (Universities Australia, 2020). CSPs and HELP loans are withdrawn from students who fail half of their subjects, assessed on a yearly or half-yearly basis depending on the level of study.[6]

3 Data

We use data from the Household Income and Labour Dynamics Australia (HILDA) survey. These data are rich, and we exploit the full set of background information on individuals (beginning with more than 3,400 variables per observation).

HILDA covers a long time span of 19 years, starting in 2001. We use the 2019 release. This means we observe respondents annually from 2001 to 2019.

3.1 Sample exclusions

Our main analysis sample contains respondents who were 25 years or above in 2001. This allows us to focus on individuals who obtain a further education – beyond that acquired in their previous degree.

Our main analysis focuses on measuring the impact of further education using wave 19 outcomes. Here, the feature inputs to the models are taken from the individuals in 2001. We delete any individuals who were 'currently studying' in 2001. This also ensures that our features, which are defined in 2001 are not contaminated by the impacts of studying but clearly precede the study spell of interest. These sample exclusions result in 7,359

[6] Yearly at bachelor level and per trimester for courses lower than bachelor level.

respondents being dropped because they are below the age of 25 in 2001 and a further 1,387 respondents being dropped because they were studying in 2001.

We then restrict the sample to those who are present in both 2001 and 2019. This ensures that we observe base characteristics and outcomes for every person in our analysis sample. This results in a further 5,727 respondents being dropped from the sample. Our analysis sample has 5,441 observations. More details of our main analysis sample and data can be found in the Online Appendix Document 1.[7]

3.2 Outcomes

We measure outcomes in 2019 across the groups of individuals who did and did not get re-educated. We use annual earnings to measure the economic returns to education. We also analyse outcomes related to the labour market such as employment, changes in earnings, changes in occupation, industry, and jobs.[8]

3.3 Treatment

We define further education as an individual who obtains a further degree in a formal, structured educational program. These programs must be delivered by a certified training, teaching or research institution. Thus, we do not analyse informal on-line degrees (such as Coursera degrees). We also do not consider on-the-job training as obtaining further education.

Our treatment variable is a binary variable that takes the value of 1 if an individual has obtained an additional degree anytime between wave 2 (2002) and wave 17 (2017). As we analyse outcomes in 2019, this means we calculate the average returns between 2 years and up to 17 after course completion. We delete any respondent who obtained a qualification after wave 17. This allows us to analyse outcomes at least two years after course completion.

[7] For sensitivity analysis, a second sample of respondents are examined. They are slightly younger when they began study, their feature values are taken in the two years before study began and their outcomes are measured four years after their study began. In this second sample, there are 1,814 individuals who started and completed a further educational degree, and 60,945 person-wave control observations who never completed a further degree. We detail our second approach in the Online Appendix Document 2.

[8] A second approach is to use outcomes measured four years after the start of a study spell. For sensitivity analysis, we repeat our main estimations using this second approach. Here, as many individuals in our dataset never started a further degree i.e. they are in our control group, we assign a time stamp to them for every year the control person theoretically could have started to study. We do this for every year from 2003 to 2019. This implies that control group individuals can be duplicated multiple times in the dataset. We then measure the control individuals' outcomes 4 years after their theoretical time stamp.

HILDA documents formal degree attainment in two ways. The first is to ask respondents, in every, wave what is their highest level of education. The second way is to ask respondents, in every wave, if they have acquired an additional educational degree since the last time they were interviewed.

We utilise both these questions to construct our measure of further education. Using the first question, we compare if the highest level of education in 2019 differs from that in 2001. If there has been an upgrade in educational qualification between these two years, we set the treatment indicator to be one and zero otherwise. This question, however, only captures upgrades in education; it fails to capture additional qualifications that are at the same level or below as the degree acquired previously by the respondent. We rely on the second survey question to fill this gap.

These two survey questions thus capture any additional qualification obtained from 2002 to 2017, inclusive. Additional qualifications refer to the following types of degrees: Trade certificates or apprenticeships; Teaching or nursing qualifications, Certificate I to IV, Associate degrees, Diplomas (2-year and 3-year fulltime), Graduate Certificates, Bachelor, Honours, Masters and Doctorate degrees.

3.4 Covariates/features

We define our covariates, or features as they are known in machine learning parlance, using 2001 as the base year. Since we delete any respondents who were currently studying in 2001, we ensure that all features were defined before a respondent begins further study.[9]

A unique approach to our feature selection strategy is that we use all the information available to us from the HILDA survey in 2001. This means that we have more than 3,400 raw variables per observation. Before using the features in a ML model, we delete any features that are identifiers or otherwise deemed irrelevant for explaining the outcome.

In order to reduce redundancy in this vast amount of information, we next apply a supervised Machine learning model to predict outcomes 5 years ahead of 2001 i.e., in 2006. We then select the top 100 variables that are most predictive of the outcome in 2006.[10] These variables are listed in Table 1.

[9] We also test the sensitivity of our results to using feature inputs that are taken from the individuals closer to the timing of their study, namely two years before study began. Here, we use both the year and the two years preceding the start of a study spell to define our features. This allows us to capture both level and growth values in the features.

[10] Confounders are features that both have an impact on the outcome and on the treatment. Chernozhukov et al. (2018) suggest including the union of features kept in the two structural equations (outcome on features and treatment on features). Here, we only include the features that predict the

3.5 Missing variables from the baseline model

As part of a replication exercise, we constrast the results from the ML model with published work using Ordinary Least Squares (OLS) and Fixed Effects models. We also contrast the features selected in the ML model with an approach that manually selects the variables as in the case of Chesters (2015). We call this the 'baseline' model.

As a descriptive exercise, Table 2 presents the features that were 'missed' by the baseline model. In the baseline model, we included features such as age, gender, state of residence, household weekly earnings, highest level of education attained, and current work schedule. This collection of variables have been informed by theory or previous empirical results.

The data-driven model identifies more salient variables compared to the baseline model. Additional variables include employment conditions such as work schedule, casual employment, firm size, tenure or years unemployed; financial measures such as weekly wage, investment income and mortgage debt; health measures such as limited vigorous activity and tobacco expenses; and work-life preferences related to working hours and child care.

We identify variables as missing from the baseline model if those variables explain the residual variation in the outcome. Specifically, we regress the residuals from the baseline models (without the treatment included) on the features included in the data-driven model and train a LASSO model to highlight the salient variables that were missed. The variables that are chosen are listed in Table 2. We also document how these variables are correlated to the outcome and to the treatment in order to give us a sense of the direction of the bias their omission may induce.

Most of the omitted variables bias the OLS estimates is upwards.[11] The upward bias is consistent with the ML-models estimating an economic returns on obtaining a new qualification that is significantly smaller than the returns from an OLS model or a Difference-in-Difference - Fixed Effects (DD-FE) model. In the DD-FE model, we use the same 5,441 individuals as the other methods but they are followed over two waves: 2001 and 2019 (i.e. there are 10,882 person-wave observations). We control for individual and wave fixed-effects.

Figure 10 displays the estimated returns from six different models. The first three bars show significantly higher returns based on the OLS (no controls), OLS (with controls) and the DD-FE models compared to the last three bars, which are based on the ML

outcome equation because including features that are only predictive of the treatment can erroneously pick up instrumental variables (see Pearl (2012) for a discussion of this issue).

[11] Exceptions include casual employment status, the presence of a past doctorate qualification, years unemployed, parental child care and dividend and business income.

models - Gradient Boosted Regression, Doubly Robust and Bayesian Causal Forest. We discuss these methods in more detail below.

It is important to highlight that our approach to identifying missing variables from the baseline model is a descriptive one. As previously mentioned, the ML algorithm randomly selects variables that are highly correlated thus we may have missed out on reporting the label of important variables omitted from the baseline model.

4 Descriptive Figures and Tables

We calculate the average returns to degree completion for mature-age students who completed degrees between 2002 and 2017. The window in which study and degree-completion took place is noticeably large. However, sample size limitations with our survey data mean that it is not feasible to run an ML analysis, disaggregated by the timing-of-completion.

In order to obtain some insights into the potential heterogeneity over time, we present a series of descriptive graphs in this section. Here, our aim is not to present any causal analysis but to describe which groups studied earlier in the time period (and thus had more time to accumulate returns). These graphs can also point to the potential different factors driving study across the time period, and different effects on earnings depending on how much time has elapsed since completion.

Figure 3 presents the distribution of degree completion over time. There is a steep decline in degree-completion proportions over time. This is likely to reflect the aging profile of HILDA survey respondents and that further study is disproportionately higher among the younger cohorts (25-44 year olds) (See Figure 4).

Over time, Figure 5 shows that the composition of degrees completed has shifted. Among those who completed a degree in later years, compared to those who completed a degree in the earlier period, a higher percentage completed a Certificate III or IV, Diploma or Advanced Diploma as opposed to a lower-level degree (Certificate I or II or below). In all years, the most frequently completed degrees are Cert 3 or 4, Associate degrees, Diplomas and Advanced Diplomas.

The predominance of Cert 3 or 4 degrees is common across gender. Although, Figure 6 shows the distribution of degrees is more heavily skewed towards these degrees for men then they are for women.

Figure 7 shows an increase in both average earnings and employment overtime between 2002 and 2017. Despite the upward trajectory, these outcomes show more volatility following 2008. This is likely to reflect the smaller samples in the later years of the survey. In our main analysis we average the returns over time as the samples within each year are inadequate to draw inference about heterogeneity across time.

5 Method

We aim to estimate the causal impact of obtaining a new qualification. Our empirical challenge is a missing data one in the sense that we do not observe the counterfactual outcome for each person – what would have their income been if they had/had not obtained a new qualification?

We use capitalisation to denote random variables, where $Y \in \mathbb{R}^+$ is the outcome variable, $T \in \{0, 1\}$ is the binary treatment indicator, and $X \in \mathcal{X}$ are the conditioning variables (which can be a mix of continuous or categorical in type). Small case is used to denote realisations of these random variables, e.g. y, t and x, and we may use a subscript for an individual realisation, e.g. y_i for individual i from a sample of size n.

Under the potential outcomes framework of Imbens and Rubin (2015), $Y(0)$ and $Y(1)$ denote the outcomes we would have observed if treatment were set to zero ($T = 0$) or one ($T = 1$), respectively. In reality, we only observe the potential outcome that corresponds to the realised treatment,

$$Y = T \cdot Y(1) + (1 - T) \cdot Y(0). \tag{1}$$

The missing data problem (or the lack of counterfactuals) is especially problematic when the treated group is different from the control group in ways that also affect outcomes. Such selection issues mean that we cannot simply take the difference in the average of the non-missing values of $Y(0)$ and $Y(1)$.

To address the missing data problem, we turn to a range of ML-based techniques. Standard ML tools are purposed to predict, but our aim is to estimate the causal parameter. These are different aims, and so we have to adapt the ML tools. We may potentially bias our causal parameter of interest if we were to use the off-the-shelf tools. For example, if we were to select the important confounders using an ML model to predict the outcome Y, then we may undervalue the importance of variables that are highly correlated to the treatment T but only weakly predictive of Y (Chernozhukov et al., 2018).

We approach filling the missing data indirectly with three types of ML models that have been specially adapted to causal inference. They are: the T-Learner, Doubly Robust and Bayesian models. For all our models, we require the following identification assumptions.

Identification assumptions

To interpret the estimated parameter as a causal relationship, the following assumptions are needed:

1. **Conditional independence** (or conditional ignorability/exogeneity or conditional unconfoundedness) Rubin (1980): $Y(0)$ and $Y(1)$ are independent of T conditional on X; i.e. $\{Y(0), Y(1)\} \perp T \mid X$.

This assumption requires that the treatment assignment is independent of the two potential outcomes. Practically, this amounts to assuming that components of the observable characteristics available in our data, or flexible combinations of them, can proxy for unobservable characteristics. Otherwise, unobservable confounding bias remains.

A benefit of using all the features the HILDA dataset has to offer is that we may minimise unobserved confounding effects. Specifically, we rely on the 3,400 features and complex interactions between them as well as flexible functional forms to proxy for components of this unobserved heterogeneity. For example, while we do not observe ability or aptitude directly, we may capture components of it with other measures that are observed in HILDA such as past educational attainment or the long list of income and other sources of income variables (see Table 1 for a list of the features).

The reader is likely to conceptualise other dimensions of unobserved heterogeneity that may not be captured in Table 1. There are two likely scenarios in this case. First, HILDA may not be exhaustive enough, even with its existing richness, to capture all dimensions of unobserved heterogeneity. As a result, our estimates may be biased.

Another potential scenario is that the source of unobserved heterogeneity in question (or some components of it) is still captured but modelled under the guise of another variable label. Variables that are highly correlated with each other are unlikely to be simultaneously included in the model. This is because the ML algorithm, in attempting to reduce the amount of information redundancy, may have randomly dropped one or more of those correlated variables.

2. **Stable Unit Treatment Value Assumption** (SUTVA) or counterfactual consistency: $Y = Y(0) + T(Y(1) - Y(0))$.

Assumption 2 ensures that there is no interference, no spill-over effects, and no hidden variation between treated and non-treated observations. SUTVA may be violated if individuals who complete further education influence the labour market outcomes of those who do not complete further education. For example, if the former group absorb resources that would otherwise be channelled to the latter group. Alternatively, the former group may be more competitive in the labour market and reduce the probability of promotions or job-finding for the latter group. As those who complete further education are a relatively small group, it is unlikely that these general equilibrium effects would occur.

3. **Overlap Assumption** or common support or positivity – no subpopulation defined by $X = x$ is entirely located in the treatment or control group, hence the treatment probability needs to be bounded away from zero and one.

The overlap is an important assumption because counterfactual extrapolation using the predictive models,

$$\mathbb{E}[Y|X{=}x, T{=}1] \approx \mu_1(x) \quad \text{and} \tag{2}$$

$$\mathbb{E}[Y|X{=}x, T{=}0] \approx \mu_0(x) \tag{3}$$

is likely to perform best for treatment and control subpopulations that have a large degree of overlap in $\mathcal{X}$. If the treatment and control groups had no common support in $\mathcal{X}$, we would be pushing our counterfactual estimators to predict into regions with no support in the training data, and therefore we would have no means by which to evaluate their performance.

This means the optimum bias-variance trade-off point for the conditional average treatment effect may not align with the optimum bias-variance trade-off point for the separate $\mu_1(x)$ and $\mu_0(x)$ models. Since, ultimately we are interested in the CATEs (as opposed to the predictive accuracy of the individual conditional mean functions), this can mean that we have biased CATEs.

4. **Exogeneity of covariates (features)** – the features included in the conditioning set are not affected by the treatment.

To ensure this, we define all of our features at a time point before any individual started studying. Specifically, we use the first wave of HILDA (in 2001) to define our features. We only look at those individuals who completed further education in 2002 onwards. Furthermore, we delete any individuals who were currently studying in 2001 to ensure the features cannot reflect downstream effects of current study.

With the strong ignorability and overlap assumptions in place, treatment effect estimation reduces to estimating two response surfaces – one for treatment and one for control.

5.1 T-Learner model

The first adaptation of ML models for causal estimation is the T-learner approach. We aim to measure the amount by which the response Y would differ between hypothetical worlds in which the treatment was set to $T = 1$ versus $T = 0$, and to estimate this across subpopulations defined by attributes X.

The T-learner is a two-step approach where the conditional mean functions defined in Equations (2) and (3) are estimated separately with any generic machine learning algorithm.

Machine learning methods are well suited to find generalizable predictive patterns, and we employ a range of model classes including linear (LASSO and Ridge) and non-linear (Gradient Boosted Regression). Once we obtain the two conditional mean functions, for each observation, we can predict the outcome under treatment and control by plugging each observation into both functions. Taking the difference between the two outcomes results in the Conditional Average Treatment Effect (CATE).

To show this, we define our parameter of interest, the CATE, which is formally defined as:

$$\tau(x) = \mathbb{E}[Y(1) - Y(0)|X{=}x] \tag{4}$$

which, with the assumptions above, is equivalent to taking the difference between two conditional mean functions $\mu_1(x) - \mu_0(x)$:

$$\begin{aligned}\tau(x) &= \mu_1(x) - \mu_0(x)\\ &\approx \mathbb{E}[Y|T{=}1, X{=}x] - \mathbb{E}[Y|T{=}0, X{=}x]\\ &= \mathbb{E}[Y(1) - Y(0)|X{=}x] \end{aligned} \tag{5}$$

In this estimation, we are not interested in the coefficients from regressing Y on X. What we require is a good approximation of the function $\tau(x)$, and hence good estimates from $\mu_1(x)$ and $\mu_0(x)$, which is within the perview of machine learning methods.

A benefit of our set-up is that when we take the difference between the two conditional mean functions, we coincidently find the optimum bias-variance trade-off point for the conditional average treatment effect. This means that we have an indirect way to obtain the best prediction of the CATE through two predictive equations, where we observe the true outcomes (and thus are able to regularise).

In practice, however, this indirect way of minimising the mean squared error for each separate function to proxy for the minimum mean squared error of the treatment effect can be problematic. See, for example, Künzel et al. (2019), Kennedy (2020) for settings when the T-learner is not the optimal choice. One potential estimation problem arises when there are fewer treated individuals than control individuals and the individual regression functions are non-smooth. In this instance the response surfaces can be difficult to estimate them in isolation, and the T-learner does not exploit the shared information between treatment and control observations. For example, if X relates to Y in the same fashion for treated and control observations the T-learner cannot utilise this information. As a result, the estimate μ_1 tends to over smooth the function; in contrast, the estimate μ_0 regularises to a lesser degree because there are more control observations. This means a naïve plug-in estimator of the CATE that simply takes the difference between $\mu_1 - \mu_0$ will be a poor and overly complex estimator of the true difference. It will tend to overstate the presence of heterogeneous treatment effects. We turn to other ML models to address this potential problem.

5.2 Doubly Robust model

The second approach is the Doubly Robust learner (DR-learner). It is similar to the T-learner in that it separately models the treatment and control surfaces, but it uses additional information from a propensity score model. In this case the propensity score model is a machine learning classifier that attempts to estimate the treatment assignment process,

$$\mathbb{E}[T{=}1|X{=}x] = \mathbb{P}(T{=}1|X{=}x) \approx \rho(x), \tag{6}$$

where $\rho(x)$ as a probabilistic machine learning classifier. This allows information about the students' background, and the nature and complexity of their situation that may have led them to pursue further education to be incorporated into the model. Thus, the doubly robust approach can improve upon the T-learner approach because it can reduce misspecification error either through a correctly specified propensity score model or through correctly specified outcome equations. Another feature of the Doubly Robust approach is that it places a higher weight on observations in the area where the relative count of treatment and control observations is more balanced (i.e. the area of overlap). This may allow better extrapolations of the predicted outcomes within the region of overlap. The ATE is estimated from three separate estimators,

$$\hat{ATE} = \frac{1}{n}\sum_{i=1}^{n}\left[\frac{t_i(y_i - \mu_1(x_i))}{\rho(x_i)} + \mu_1(x_i)\right] - \frac{1}{n}\sum_{i=1}^{n}\left[\frac{(1-t_i)(y_i - \mu_0(x_i))}{1-\rho(x_i)} + \mu_0(x_i)\right] \tag{7}$$

Previously, with the T-learner, we were just estimating $\mu_0(x)$ and $\mu_1(x)$. With the DR-learner, we augment $\mu_0(x)$ and $\mu_1(x)$. For example, for the treated observations, we augment $\mu_1(x)$ by multiplying the prediction error by the inverse propensity scores. This up-weights those who get treated but who are statistically similar to the control observations. We then apply this same augmentation to the $\mu_0(x)$ for the control observations.

5.3 Bayesian Models

The third approach is to use Bayesian models. We follow the general formulation presented by Hahn, Murray and Carvalho (2020) that suggests a predictive model of the following form,

$$\mathbb{E}[Y|X{=}x_i, T{=}t_i] \approx \mu_0(x_i, \rho(x_i)) + \tau(x_i)\cdot t_i, \tag{8}$$

where $\mathbb{E}[T = 1|X{=}x_i] \approx \rho(x_i)$ is the propensity score of individual i for the treatment. The component $\mu_0(x_i, \rho(x_i))$ is known as the 'prognostic' effect, and is the impact of the control variates, X, on the outcome without the treatment. Then we are left with $\tau(x_i)$, which is the individual treatment effect,

$$\begin{aligned}\mathbb{E}[Y|X{=}x_i, T{=}1] - \mathbb{E}[Y|X{=}x_i, T{=}0] &\approx [\mu_0(x_i, \rho(x_i)) + \tau(x_i)] - \mu_0(x_i, \rho(x_i)),\\ &= \tau(x_i).\end{aligned}$$

Average treatment effect is then just simply estimated as,

$$\hat{ATE} = \frac{1}{n}\sum_{i=1}^{n}\tau(x_i).$$

The advantage of this approach are manifold. From a Bayesian perspective, it allows us to place explicit and separate priors on the prognostic and treatment components of the models. For example, it may be sensible to expect the prognostic component to be flexible and strongly predictive of the outcome, while me may expect that the treatment component is relatively simple and small in magnitude (Hahn, Murray and Carvalho, 2020). Furthermore, this separation of model components and inclusion of the propensity score minimises bias in the form of regularisation induced confounding (RIC) which is discussed in more detain in (Hahn et al., 2018, Hahn, Murray and Carvalho, 2020). And finally, it is a very natural way to estimate heterogeneous treatment effects, since we can parameterise $\tau(x_i)$ directly as an additive effect on μ_0, rather than having to separately parameterise control and treatment surfaces.

We explore three different model classes for μ_0 and τ, the first is a linear model for both prognostic and treatment models, the next uses a Gaussian process (GP), and lastly we use Bayesian additive regression trees (BART). We detail these models in the following sections.

Hierarchical Linear Model

The first Bayesian model uses linear prognostic and treatment components from Equation (8),

$$\begin{aligned} y_i &\sim \mathcal{N}\big(\mu_0(x_i, \rho(x_i)) + \tau(x_i) \cdot t_i, \sigma^2\big) \quad \text{where,} \\ \mu_0(x_i, \rho(x_i)) &= w_0 + w_x^\top x_i + w_\rho \rho(x_i), \\ \tau(x_i) &= w_t + w_{tx}^\top x_i. \end{aligned}$$

We have used the following hierarchical priors,

$$\begin{aligned} \{\lambda_0, \lambda_x, \lambda_\rho\} &\sim \text{Uniform}(0, 100) \\ \{\lambda_t, \lambda_{tx}\} &\sim \text{Uniform}(0, 1000) \\ \sigma &\sim \text{HalfCauchy}(25) \\ w_0 &\sim \mathcal{N}(0, \lambda_0^2) \\ w_x &\sim \mathcal{N}(0, \lambda_x^2 \mathrm{I}_d) \\ w_\rho &\sim \mathcal{N}(0, \lambda_\rho^2) \\ w_t &\sim \mathcal{N}(0, \lambda_t^2) \\ w_{tx} &\sim \mathcal{N}(0, \lambda_{tx}^2 \mathrm{I}_d), \end{aligned}$$

where I_d is the identity matrix of dimension d, which is the number of control factors. The propensity score, $\rho(x_i)$, is obtained from a logistic regression model. We also tested a gradient boosted classifier (Friedman, 2001) for this using five-fold nested cross validation. It did not seem to be more performant than the logistic model on held-out log-loss score.

For model inference, we use the no U-turn MCMC sampler (Hoffman and Gelman, 2014) in the numpyro software package (Bingham et al., 2019, Phan, Pradhan and Jankowiak, 2019). The choice of an uniform improper and non-informative prior over the regression weight scales, λ_*, is motivated by the advice in Gelman (2006) where we desire a non-informative prior that admits large values. We choose a broader prior for the treatment component of the model to minimise bias as suggested by Hahn, Murray and Carvalho (2020). We first burn in the Markov chain for 30,000 samples, then draw 1000 samples from the posterior parameters to approximate the ATE,

$$A\hat{T}E = \frac{1}{Sn} \sum_{s=1}^{S} \sum_{i=1}^{n} \tau^{(s)}(x_i), \tag{9}$$

where (s) denotes a sample from the posterior parameters has been used to construct a random realisation of the treatment model component, and $S = 1000$.

Gaussian Process Regression

Gaussian process (GP) regression can be viewed as a non-linear generalisation of Bayesian linear regression that makes use of the kernel trick (Williams and Rasmussen, 2006, Bishop, 2006). Another way of understanding a GP is that is parameterises a distribution over functions (response surfaces) directly, rather than model weights as is the case with Bayesian linear regression.

Say we have the regression function, $\mathbb{E}[Y|X{=}x_i] = f(x_i)$, a Gaussian process models the covariance of $f(x)$ directly using a kernel function,

$$\begin{aligned} \mathbb{E}[f(x_i) \cdot f(x_j)] &= k(x_i, x_j) \qquad \text{or,} \\ \mathbb{E}[Y_i \cdot Y_j] &= k(x_i, x_j) + \sigma^2 \delta_{ij}, \end{aligned}$$

where δ_{ij} is a Kroneker delta, and is one iff $i = j$, otherwise zero. This formulation also assumes $\mathbb{E}[Y] = \mathbb{E}[f(x)] = 0$ for simplicity – and can be used directly if the outcomes are transformed to be zero mean, or we can model an additional mean function (see Williams and Rasmussen (2006) for details). The Gaussian process can be written as,

$$\mathbf{y} \sim \mathcal{N}(\mathbf{0}, \mathbf{K} + \sigma \mathbf{I}_n),$$

where $\mathbf{y} = [y_1, \ldots, y_i, \ldots, y_n]^\top$ is the vector of all outcome samples, $\mathbf{K}$ is the covariance matrix with elements $\mathbf{K}_{ij} = k(x_i, x_j)$, and $\mathbf{I}_n$ the n-dimensional identity matrix.

To implement the functional relationship in Equation (8) in a Gaussian process, we create the kernel function over $\langle x, t \rangle$ pairs,

$$k(\langle x_i, t_i \rangle, \langle x_j, t_j \rangle) = \sigma_{\mu_0}^2 k_{\mu_0}(\langle x_i, \rho(x_i) \rangle, \langle x_j, \rho(x_j) \rangle) + t_i t_j \cdot [\sigma_\tau^2 k_\tau(x_i, x_j) + \tau_0].$$

Here k_{μ_0} and k_τ are the prognostic and treatment kernels respectively, σ_{μ_0} and σ_τ allow us to scale the contribution of these kernels to the functional relationships learned, and τ_0 permits a constant treatment effect. This induces the functional relationship we want; $f(x_i, t_i) = \mu_0(x_i, \rho(x_i)) + \tau(x_i) \cdot t_i$. We use the same propensity model for $\rho(x_i)$ as the linear model previously.

We have chosen isotropic Matérn $\frac{3}{2}$ kernel functions for k_{μ_0} and k_τ,

$$k_{\nu=3/2}(x_i, x_j) = \left(1 + \frac{\sqrt{3}|x_i - x_j|}{l}\right) \exp\left(\frac{-\sqrt{3}|x_i - x_j|}{l}\right),$$

where l is the length scale parameter, and controls the width of the kernel function. Smaller length scales allow for more high-frequency variation in the resulting function $f(x_i)$. The Matérn kernel is a stationary and isotropic kernel, but does not have excessive smoothness assumptions on the functional forms it can learn – this kernel leads to the response surface being at least once differentiable (Williams and Rasmussen, 2006). A Gaussian process with this kernel can learn non-linear and interaction-style relationships between input features and the outcome. Our composite kernel is not necessarily stationary however, as we have included a non-stationary term, $t_i t_j$.

A-priori, we expect reasonably smooth variation $\mathbb{E}[y_i \cdot y_j]$ so we choose a long length-scale for the prognostic kernel function, $l_{\mu_0} = 10$, and an amplitude, $\sigma^2_{\mu_0} = 1$. We expect an even smoother relationship with less contribution for the treatment, and set the corresponding kernel parameters as; $l_\tau = 50$, $\sigma^2_\tau = 0.1$ and $\tau_0 = .001$. These parameters are then optimised using the maximum likelihood type-II procedure outlined in Section 5.4.1 of Williams and Rasmussen (2006).

The ATE is then approximated as,

$$A\hat{T}E = \frac{1}{Sn} \sum_{s=1}^{S} \sum_{i=1}^{n} f_*^{(s)}(x_i, 1) - f_*^{(s)}(x_i, 0),$$

where $f_*^{(s)}(x_i, t)$ are samples from the Gaussian process posterior predictive distribution[12] with kernel inputs $k_*(\langle x_i, t\rangle, \langle x_i, t\rangle)$, which is equivalent to sampling from the distribution over $\tau(\cdot)$. We use $S = 100$ samples.

[12] See Equations (2.22)-(2.24) of Williams and Rasmussen (2006).

Bayesian Causal Forests

The last Bayesian model we use is the Bayesian causal forest introduced In Hahn, Murray and Carvalho (2020). Broadly it models the prognostic and treatment components As Bayesian additive regression trees (BART),

$$\begin{aligned} y_i &\sim \mathcal{N}\big(\mu_0(x_i, \rho(x_i)) + \tau(x_i) \cdot t_i, \sigma^2\big) \quad \text{where,} \\ \mu_0(x_i, \rho(x_i)) &= \mathrm{BART}(x_i, \rho(x_i)), \\ \tau(x_i) &= \mathrm{BART}(x_i). \end{aligned}$$

We use the accelerated BART (XBART) implementation of this algorithm detailed in Krantsevich, He and Hahn (2022). BART (Chipman, George and McCulloch, 2010) has been shown to be an effective and easily applicable non-parametric regression technique that requires few assumptions in order to capture complex relationships that can otherwise confound effect estimation. We follow Hahn, Murray and Carvalho (2020) in our choice of BART priors,

$$\begin{gathered} \alpha_{\mu_0} = 0.95,\ \alpha_\tau = 0.25, \\ \beta_{\mu_0} = 2,\ \beta_\tau = 3. \end{gathered}$$

This choice prefers a more simple treatment effect model, $\tau(x_i)$, that is less likely to branch, and more likely to have shallower trees than the prognostic model. Similarly, we use 200 trees for the prognostic model, and 50 for the treatment. We take 500 burn-in sweeps, and then 2000 sweeps to estimate the posterior BART distributions.

ATE is estimated in the same way as for the linear model in Equation (9), but where the BART posterior is used for the treatment effect distribution.

5.4 Model selection and model evaluation

For the non-Bayesian models we separate the evaluation of the model class and estimation of the ATE and CATE parameters in two procedures. We evaluate the predictive capacity of each model class using nested cross-validation. The procedure is represented in Figure 1. Here, our aim is to compare the predictive performance of three model classes: LASSO, Ridge and Gradient Boosted Regression (GBR). Our second procedure is to estimate the ATE and CATE parameters. The procedure is represented in Figure 2. We use bootstrap sampling (with replacement) to generate uncertainty estimates for the parameters, which we obtain over several draws of the same model class, but with model parameter re-fitting.

Focusing on the first procedure, we apply nested cross-validation to evaluate which model class performs best. In a first step, as Figure 1 shows, we pre-process the full dataset (containing 3,400 variables) to generate a dataset with a smaller set of highly predictive features (containing 91 variables). We apply a supervised machine learning approach with a LASSO model to select our top 91 predictors of the outcome of interest using outcomes measured in 2006. Note that in our later estimations of the treatment effect, the outcome is measured in 2019. We implement this intermediary step in order to reduce the correlation between variables and eliminate redundant information.

We assume that the top 91[13] features that are most predictive of the outcome in 2006 correlate with the features that would be most predictive of the outcome in 2019. By choosing to apply this pseudo-supervised ML approach on the same outcome variable, but measured at a different time point, we obtain a good indication of the features that are useful for a model to perform well. Improved model performance here will also mean that the selected features are likely to represent the important confounders. We have chosen 2006 to ensure there is no overlap with 2019 outcomes to avoid overfitting issues with subsequent models.[14]

Using the top 91 predictors, we then apply nested cross validation to evaluate the predictive capacity of each model class (LASSO, Ridge, GBR). First, we split the data into train and test folds with an 80-20 split. Within the 80 percent train fold we perform 5-fold cross-validation in order to train and evaluate the performance of each configuration of hyperparameters. We do this separately for the outcome surface using the treated observations and the outcome surface using the control observations. From this, we select the models with the best mean predictive scores. We then evaluate the predictive performance of the selected model on the holdout test.

We repeat this process ten times (10-outer scores) for each model class. This allows us to evaluate the performance based on the mean and standard deviation of these scores. Note that thus far, we have not evaluated any particular configuration of the model, rather the performance of the model class on random (without replacement) subsets of data. The

[13] We were aiming for approximately 100 features, and 91 was the closest we could get the LASSO estimator to select by changing the value of its regularisation strength.

[14] We do not compromise predictive performance when we use the selected subset of features as opposed to the full set of features. For example, the predictive performance from a Gradient Boosted Tree model that predicts earnings in 2006, using 5-fold nested cross-validation, is statistically similar between models that use the 91 feature set and the full, 3,400 feature set (with Root Mean-Squared Errors (RMSEs) of 484.251 and 482.286, respectively). This is a neglible loss in predictive performance and is in fact smaller than the associated loss between the restricted and full feature sets from models predicting earnings in 2019 (RMSEs of 843.548 and 831.931, respectively).

nested cross validation procedure protects us against overfitting when reporting predictive performance, as the model selection and validation happens on different data.

Table 3 shows that the GBR is the best performing model class. It yields the highest out-of-sample R-squared and the lowest MSE. This is true for both the outcome surfaces separately.

As the DR-learner model relies on the same treatment and control outcome surfaces estimated in the T-learner, we do not repeat Table 3 for the DR results. A further component of the DR model, however, is the propensity score. Here, we implement a regularised logistic regression to predict the likelihood of being treated (to obtain a further degree). Specifically, we use cross validation to fit a Logistic regression and obtain the predictions from the original sample. The holdout performance of the fitted Logistic regression model yields an area under the ROC curve of 0.71.

Inference via bootstrapping

Once we have selected the best performing model class, we turn to the estimation of the parameters and parameter uncertainty. We use bootstrapped validation for generating uncertainty estimates. This captures the uncertainty arising from model hyperparameter selection in addition to that from estimating parameters of a fixed model from noisy, finite data.

A common approach to inference in the causal machine learning literature is to use sample splitting (Athey and Wager, 2019). Sample splitting ensures that the standard errors on the estimators are not underestimated because it avoids using the same data point to both select hyperparameters of the model and to estimate the parameters. The result of using common data for model selection and effect estimation is that our standard errors would suffer from pre-test bias since the model may suffer from overfitting.

Sample splitting is appropriate when the sample size is large. An issue with studies that rely on survey-based data is that sample sizes are often not large enough to perform sample splitting. For example, there is not enough data to split the dataset into train and test datasets such that each of these splits would cover all the common and uncommon values of the X-features that are observed in the full sample. If we were to use a training dataset that was insufficiently sized or non-representative, it would be difficult for the ML models to effectively map the X-features to the outcome surfaces, $\mu_1(x)$ and $\mu_0(x)$. There would also not be enough data in the test set to effectively estimate the parameters of the model configuration chosen in the train set. As a result, our estimate treatment effects are likely to have a large degree of uncertainty.

A suitable alternate procedure is to use bootstrapping. Bootstrap resampling allows us to estimate variation in the point model parameter estimates. In this way, we side-step the need to rely on the assumption of asymptotical normality, and it is more efficient than sample splitting to generate standard errors. In our bootstrapping procedure, we ensure that the standard errors reflect the sources of uncertainty stemming from both the selection of the model and the estimation of the model. As a result, we generate standard errors that avoid any potential pre-test issues.

As a first step, as shown in Figure 2, we obtain the 91 top predictors from the initial pre-processing of the full dataset. That is, we train a supervised machine learning LASSO model to extract which features best predict earnings in 2006.

As a second step, we train our models using the 91 top predictors on the first bootstrapped sample to select the best models for $\mu_1(x)$ and $\mu_0(x)$. Within this bootstrap sample, we divide the dataset into five folds and perform cross-validation to select the best model configuration. Similar to the cross-validation description above, our model configuration is trained on subsets of the data, and then evaluated on holdout samples. We modify the 5-fold cross validation to ensure bootstrap replicated training data does not simultaneously appear in the training and validation set. We perform this model selection step within the bootstrapping procedure to capture the uncertainty coming from the selection of hyperparameters. If we simply re-estimated the same model with a given set of hyperparameters in each bootstrap model then the uncertainty is only over the model parameters, and not the model choice (e.g. the GBR tree depth).

Third, and once we have these predicted outcome surfaces, $\mu_1(x)$ and $\mu_0(x)$, we are able to calculate the individual treatment effect, $\tau(x_i)$, for each person in the original sample (not the individuals from the bootstrap sample) by substituting the values of their features into the LASSO, Ridge or tree estimators for $\mu_1(x)$ and $\mu_0(x)$. We can obtain a sample mean, $\bar{\tau}^{(s)}$, by averaging over all the individual $\tau^{(s)}(x_i)$ in the bootstrap sample, s. We repeat this procedure over $S = 100$ bootstrap samples. This provides an empirical distribution of $\bar{\tau}$ and $\tau(x_i)$. The grand mean over the bootstrap sample means, $\bar{\tau}_G = \frac{1}{S}\sum_s \bar{\tau}^{(s)}$, will converge to the sample treatment effect mean. We use $\bar{\tau}_G$ as an estimate of the ATE, and $\frac{1}{S}\sum_s \tau^{(s)}(x_i)$ as an estimate of the individual CATE. The bootstrap resample is the same size as the original sample because the variation of the ATE depends on the size of the sample. Thus, to approximate this variation we need to use resamples of the same size.

These are boot-strapped sur-faces? So maybe we should denote them $\mu_0^{(s)}(x_i)$?

What does this mean?

To obtain confidence intervals for the ATE and CATE estimates we use standard empirical bootstrap confidence interval estimators (Efron and Tibshirani, 1986).

For the DR-learners, similar to the T-learner, we train $\mu_1(x)$ and $\mu_0(x)$ models across 100 bootstrap samples and weight these outcome surfaces by the Propensity Score Model, which is estimated using Logistic Regression (as described above).

Inference for the Bayesian models

The inference process for the Bayesian models a little different since the hyper-paramters of the models are either fixed or selected automatically by the learning algorithm (maximum likelihood type-II or MCMC). Bayesian inference procedures tend to afford some protection against over-fitting since they are parsimonious when choosing posterior distributions over model parameters that vary from their prior distributions, which induces a natural model complexity penalty[15]. As such, we use all the available data to learn the model posterior distributions, which we then sample from to form empirical estimates of the (C)ATE as outlined in the previous section.

6 Results

There are clear economic benefits to gaining an additional qualification in later life (25 years or older). The effects remain strong up to a decade-and-a-half after course completion. Table 4 displays a gain of approximately \$88-110 per week in gross earnings across the T-learner approaches. In 2019, this was roughly 7-8 percent of the average gross weekly earnings of \$1256.20 for all Australian employees (ABS, 2019; 6345.0 Wage Price Index, Australia).

The effect sizes from the GBR model are smaller than that of the two linear models. GBR better captures non-linearities. For example, age is likely to exhibit a highly non-linear relationship with earnings in 2019. Those who were aged 46 or above in 2001 will be aged 65 or above in 2019. This means they are more likely to have retired by 2019 compared to those who were aged below 46 in 2001. As a result, we may expect a shift down in earnings at age 46.

Age fixed-effects alone are unlikely to capture the differential age effects across other variables such as across different occupations, or by gender, and earnings. The linear ML models include age fixed effects. However, they do not include interactions between age and other variables whereas GBR does include them.

To illustrate how GBR adequately captures non-linearities we re-estimated our results focusing on those who were aged 25-45 in 2001. This is the same as interacting a binary

[15] This point can be understood more thoroughly by examining the evidence lower bound in variational Bayesian inference, see Chapter 10 of Bishop (2006).

variable (for age 25-45) with every other feature in the model. In Appendix Figure 13, we see that the results across the models are now more similar than when we use the full sample.

The Doubly Robust (DR) models estimate smaller effects compared to the T-learner models. Table 4 displays a gain of approximately \$62-69 per week in gross earnings across the DR approaches. The estimated effect sizes are statistically different from zero. The confidence intervals for the DR estimates also exclude the point estimates from the T-Learner approach.

One reason the DR approach differs from the T-learner approach is that the former uses additional information from the propensity score (i.e. we estimate machine learning models to gain a better understanding of the treatment assignment process, the students' background, and the nature and complexity of their situation that may have led them to pursue further education). Thus, the doubly robust approach can improve upon the T-learner approach because it can reduce misspecification error either through a correctly specified propensity score model or through correctly specified outcome equations. Another feature of the Doubly Robust approach is that it places a higher weight on observations in the area where the relative count of treatment and control observations is more balanced (i.e. the area of overlap). A benefit of this is that it can also provide better extrapolations of the predicted outcomes.

The Bayesian models estimate similar sized effects to the DR models for the most part. However, they tend to have more uncertainty associated with their estimates. They all remain significant with the 95% confidence intervals remaining above \$0. The hierarchical linear model and the Gaussian process both estimate a gain of approximately \$61-\$63 per week in gross earnings, with the Gaussian process being more certain in its estimate. Interestingly, the Gaussian process prefers a much smoother and smaller treatment effect component compared to its prognostic component – the treatment kernel length scale is long, and the kernel has a small amplitude and offset ($l_\tau = 243$, $\sigma^2_\tau = 0.0517^2$, and $\tau_0 = 0.0312^2$). Whereas the prognostic kernel parameters stay relatively close to their initial settings ($l_{\mu_0} = 16$, and $\sigma^2_{\mu_0} = 1.42^2$). The Bayesian causal forest estimates a slightly higher gain of \$84.50 per week in gross earnings, which is more inline with the GBR T-learner. This suggests that the tree ensemble methods may be able to more easily capture non-linear relationships than the other models.

Proportionate changes in earnings can be measured by taking the log of the earnings measures. In Appendix Figure 14, we see that the proportionate change in earnings was large at 50 percent. This is likely to be because of people entering the labour market as a result of the new qualification. We find that a new qualification increases the likelihood of employment by approximately 8 percent. See Figure 11.

As previously mentioned, the ML models estimate smaller returns than the returns estimated in DD-FE or cross-sectional models (OLS with and without controls) where features have been selected based on theory or previous empirical learnings. For example, the 'OLS Baseline model' uses the features in models estimated in Chesters (2015). The DD-FE eliminates all selection effects that are fixed over time. Figure 10 displays the estimated returns from six different approaches.

A potential reason for the smaller results estimated in the ML models is that the additional features included, as well as the non-linear specifications of the features, more effectively account for selection into treatment. The smaller results suggest individuals positively select into further study i.e. the characteristics that lead one to complete further study are positively correlated to future earnings. Once we control for this upward selection bias, we thus estimate smaller returns to further education.

The smaller estimated results relative to the DD-FE model are likely to stem from the inclusion of key time-varying variables such as the 'change in total gross income' in the ML models, as well as other non-linear specifications. For example, the ML models allow the treatment effects to vary in a highly flexible fashion across different parts of the feature distributions rather than making linear extrapolations.

This points to a benefit of using ML models, compared to conventional models, because they can more effectively identify confounders. We show evidence of the types of confounders missed in conventional models in Table 2, as well as the direction of the bias stemming from their omission.

In addition, we show evidence that models which allow for more flexible functional-form specifications lead to differences in the ATE. Within our ML models, the GBR tree ensemble tended to perform better (in terms of the nested cv results) compared to the linear-based models. The former yielded a slightly smaller ATE compared to the LASSO and Ridge results, for example, and they were also consistent with results from the Bayesian Causal Forest.

7 Sub-group analysis

Qualification advancements may not benefit individuals in the same way. In this section we analyse if there is heterogeneity in the treatment impacts. We use a data-driven approach to select the sub-groups.

Specifically, we identify the important variables for which we expect to see the largest changes in the treatment effects. This involves using a Permutation Importance procedure.

7.1 Permutation importance feature selection method

We use a permutation importance selection method (Breiman, 2001, Molnar, 2020) to evaluate the relative importance of individual features. Our aim here is to understand where the heterogeneous treatment effects are most pronounced. In other words, we aim to identify the sub-groups for which the treatment effects differ most significantly. In selecting the important features our objective is to understand how to partition the data by the treatment effects as opposed to predicting the outcomes themselves.

The permutation importance proceedure involves testing the performance of a model after permuting the order of samples of each individual feature, thereby keeping the underlying distribution of that feature intact but breaking the predicitve relationship learned by the model with that feature. The model performance we are interested in, as previously mentioned, is the one that maps the features to the individual treatment effects.

Following the approach described above, we compute the individual treatment effects. Note that we train the model on the bootstrapped sample but estimate the individual treatment effects using the feature values for individuals from the original sample. Thus, for every individual we have a distribution of values of their individual treatment effects.

After obtaining the individual treatment effects, we train another model that maps the features to the individual treatment effects. We use cross-validation to select our hyperparameters and obtain the optimal model.

Using the original data, we take a single column among the features and permute the order of the data and calculate a new set of individual treatment effects. We compare the new and original individual treatment effects (based on the permuted data and those from the non-permuted data) and calculate the Mean Squared Errors (MSE).

We repeat this for all the features, permuting them individually and evaluating how they change the prediction of the individual treatment effect target. Features that yield the largest MSEs are likely to be more important than those features with lower MSEs since permuting those features breaks the most informative predictive relationships.

We then repeat the above steps across all the bootstrap samples. Note that a different bootstrap sample will change the value of the individual treatment effects since we train different outcome surfaces for $\mu_0^{(s)}(x)$ and $\mu_1^{(s)}(x)$ for each bootstrap sample.

We embed the permutation importance selection method in a bootstrapping procedure in order to capture hyperparameter uncertainty. For example, a different 'tree depth' could be chosen between different bootstrap samples. This would affect the type of non-linear/interaction relationships that would be captured by the models, which in turn would affect which features turn out to be important.

Finally, we obtain an average MSE for each feature, averaged across all bootstrap samples. This average value allows us to rank the features by their importance. Again, those with the largest average MSE values are the most important. We can also evaluate the uncertainty of this estimate since we obtain a distribution of MSE values across the different bootstrap samples.

Figure 8 displays the top ten features (based on the permutation importance procedure described above) and a residual category for all the other features. The features that are most important are: weekly gross wages on the main job and income- or wealth-related variables. Together, this class of income/wealth variables accounts for 40% of the importance of all variables. We focus on these selected features since our Nested CV approach pointed to the better predictive performance of the GBR model over the linear models.

Other important features include those related to employment, including occupational status, employment expectations, and employment history. The demographic background of the individual, namely their age, is also important.

Figure 9 displays the distribution of the MSE values across the bootstrap samples for the GBR model. It displays the distributions for the top 3 features. The feature with the highest importance score: weekly gross wage in the main job. This suggests that in some of the bootstrap samples, where the MSE is larger, the individual treatment effects from the permuted data differ greatly from the original individual treatment effects.

The results from the T-learner model (using GBR) shows a similar story to the results from the permutation importance procedure using the DR model. Overall, as Appendix Figure 15 shows, income and employment-related variables are the most salient in explaining treatment effect heterogeneity.

Continuing to focus on the results from the Doubly Robust model, Figure 12 shows that there is heterogeneity in the treatment impacts. We have identified the features that were considered most important according to the permutation procedure. For each feature, we divide the sample into two groups. For continuous variables, we take the median value and divide the sample into those who are above and below this median value.

Weekly personal income has a large impact on the effect size. Those with below median income in 2001 derive more benefits than those with above median income, possibly because high income earners hit an earnings ceiling. Younger people in 2001 also derive more returns, as they may have had more time to accumulate returns. This result aligns with findings from previous studies (Polidano and Ryan, 2016, Dorsett, Lui and Weale, 2016, Perales and Chesters, 2017). Weekly personal income and age are likely to be highly correlated – with older individuals tending to earn a higher personal income. We cannot say which variable is the main driver of the heterogeneous treatment effects and there may also be interaction effects between them.

We also investigate if there are heterogeneous treatment effects according to commonly used variables in Figure 12. Females reap slightly higher returns compared to males although this is not statistically significant. Similar treatment effects apply to those with and without a resident children, although the effect sizes widen in favour of parents with older children in the household.

Acquiring an additional qualification may increase earnings through a number of potential mechanisms. We find evidence that, in Figure 11 for example, it increases the chance that individuals move from being unemployed or out of the labour force to being employed. The increase in employment is approximately 8 percentage points and is statistically significant. We also find evidence pointing to workers switching occupations or industries. This suggests that further education in later life can support the economic goals of a larger workforce as well as a more mobile one.

7.2 Sensitivity Analysis

For sensitivity analysis, we repeated the T-learner estimations using feature inputs values taken from individuals two years before they began study. Thus we examine if our main results are sensitive to changes in the mapping equations for the treatment and control outcome equations when features are measured closer to the event of study, compared to taking input values in 2001. We also measured outcomes four years after study began. This means that the timing between when the feature input values are measured, when a further degree commenced and was completed, as well as when the outcomes are measured, are all closer together. This necessarily leads us to estimate the short-term returns of obtaining a further degree.

Our results from the sensitivity analysis are similar to that of the main results. Specifically, the gains in gross earnings from a further degree in the sensitivity analysis are: \$74 per week (Ridge), \$117 per week (LASSO) and \$93 per week (GBR). The key take-away from these results is that the average treatment effects in the main analysis are not sensitive to whether our features use 2001 as the input year or use the two years before study.

Also, the main results are not sensitive to when outcomes are measured i.e. the returns measured four years after the start of a study spell are comparable to the returns averaged over 2 to 17 years after study completion. This may point to the fact that the returns to further study are accrued in the immediate years following the completion of the degree. It also suggests the returns may not atrophy over time, especially since the majority of people who did complete a degree in the main analysis did so in the earlier years of the survey (Figure 5). Unfortunately, our sample sizes are not sufficient to explore heterogeneity in treatment effects by the year of completion.

The importance of employment-related features such as earnings (individual and household), wages, and hours worked are reiterated in the sensitivity analysis using the panel structure of the data. Namely, when we define our outcomes 4 years after the start of a study spell and where we define features two years before study started, we also see similar results to that of the main results. However, in Figure 16, it is clear that the 'trend' or 'growth' in the values of features such as individual earnings, hours worked and household income are also important. This finding of dynamic selection is echoed in the literature (Jacobson, LaLonde and Sullivan, 2005, Dynarski, Jacob and Kreisman, 2016, 2018).

In Figure 16, the feature mental health is also picked. This result may reflect the fact that the timing of the measurement of features, treatment and outcomes are all closer together compared to the main results. This means that mental health is an important factor in explaining the heterogeneity in relatively 'short-term' treatment effects.

8 Conclusions

Using a machine learning based methodology and data from the rich and representative Household Income and Labour Dynamics Australia survey we have shown that completing an additional degree later in life can add \$60-80 (AUD, 2019) per week to an individual's gross earnings. This represents roughly 7-8 percent of the weekly gross earning for the average worker in Australia. Our machine learning methodology has also uncovered sources of heterogeneity in this effect.

Our methodology has allowed us to exploit the full set of background information on individuals from the HILDA survey, beginning with more than 3,400 variables, to control our analysis. We find that our automated feature selection method selects a set of controls/features that include those that have theoretical foundations and/or align with those chosen in past empirical studies. However, we also choose features that have been traditionally overlooked. These include variables such as household debt, wealth, housing, and geographic mobility variables. Other important predictors include the ages of both resident and non-resident children: non-resident children aged 15 or above matter and resident children aged 0-4 are important.

Qualification advancements do not benefit Australian workers in the same way: those with lower weekly earnings appear to benefit more from later-life study than those with higher earnings. One possible reason is that ceiling effects limit the potential returns from additional education. We also find that younger Australians (less than 45 years of age) benefit more than their older counterparts. Again, a ceiling effect phenomenon may apply since age is highly correlated to weekly earnings.

Acquiring an additional qualification may increase earnings through a number of potential mechanisms. We find evidence that it increases the chance that individuals move from being unemployed or out of the labour force to being employed. We also find evidence pointing to workers switching occupations or industries. This suggests that further education in later-life can support the economic goals of a larger workforce as well as a more mobile one.

9 Tables and Figures

Table 1: Summary Statistics

Variable label	Variable name	Mean	SD
Outcomes			
Annual Earnings individual in 2019	y_wscei	614.730	1044.717
Imputed wages			
Change in annual earnings between 2001 and 2019	y_dwscei	129.029	980.754
Treatment Indicators			
Highest level of educ changed between 2001 and 2017	reduhl	0.097	0.296
Extra degree attained in 2002 to 2017	reduﬂ	0.257	0.437
Extra degree Bachelor and/or above	bachab	0.072	0.259
Below bachelor	bbach	0.209	0.406
Technical degree	techdeg	0.151	0.358
Qualitative degree*	qualdeg	0.080	0.272
Covariates (features)			
Demographics			
Sex	hgsex	1.536	0.499
Section of State	hhsos	0.690	1.046
Age	hgage1	46.025	12.832
Age of youngest person in HH	hhyng	27.115	21.886
No. persons aged 0-4 years in HH	hh0_4	0.257	0.589

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Variable label	Variable name	Mean	SD
No. persons aged 10-14 years in HH	hh10_14	0.274	0.606
Age when first left home	fmagelh	21.502	11.230
Living circumstances	hgms	1.997	1.708
English fluency	hgeab	1.604	0.262
Unemployment rate in region	hhura	6.884	1.075
Education			
Highest year of school completed/attending	edhists	2.383	1.439
Bachelor degree (without honours) obtained	edqobd	0.211	0.330
Masters degree obtained	edqoms	0.041	0.160
Doctorate obtained	edqodc	0.011	0.085
No. qualifications unknown	edqunk	0.078	0.403
Employment			
Occupation	jbmo61	3.772	1.825
Years in paid work	ehtjbyr	21.963	11.907
Tenure with current employer	jbempt	8.505	7.369
Type of work schedule	jbmday	3.785	2.612
Current work schedule	jbmsch	2.255	1.819
Casual worker	jbcasab	1.797	0.291
Hours/week worked at home	jbmhrh	12.372	7.174
Hours/week travelling to and from work	lshrcom	3.052	3.716
Satisfaction with employment opportunities	losateo	6.693	2.557
Occupational status - current main job	jbmo6s	50.177	19.199

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Variable label	Variable name	Mean	SD
No. persons employed at place of work	jbmwpsz	3.746	1.961
Age intends to retire	rtiage1	345.709	230.208
Age retired/intends to retire	rtage	113.904	130.211
Prob. of losing job in next 12 months	jbmploj	15.196	35.018
Prob. of accepting similar/better job	jbmpgj	59.585	26.196
Looked for work in last 4 weeks	jsl4wk	1.272	0.411
Years unemployed and looking for work	ehtujyr	0.464	1.647
Hours per week worked in last job	ujljhru	34.990	6.922
Industry of last job	ujljin1	9.373	1.822
Work preferences			
Total hours per week would choose to work	jbprhr	34.378	6.407
Importance of work situation to your life	loimpew	6.854	2.908
Childcare			
Child looks after self	chu_sf	0.128	0.144
Uses child care while at work	cpno	1.257	0.139
Parent provides child care	cpu_me	0.434	0.151
Work-family balance			
Do fair share of looking after children	pashare	2.411	0.671
Miss out on home/family activities	pawkmfh	3.904	1.069
Working makes me a better parent	pawkbp	4.038	0.979
Family			
No. dependent children aged 5-9	hhd5_9	0.261	0.584

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Variable label	Variable name	Mean	SD
No. dependent children aged 10-14	hhd1014	0.269	0.604
No. non-resident children	tcnr	0.993	1.373
Sex of non-resident child	ncsex1	1.509	0.320
Likely to have a child in the future	icprob	1.188	0.374
Finances			
Owned a home previously	hspown	1.368	0.424
Amount outstanding on home loans	hsmgowe	96803.720	43547.610
Time until home loan paid off	hsmgfin	2011.858	4.157
Food expenses outside the home	xposml	36.982	42.522
SEIFA (level of economic resources)	hhec10	5.463	2.897
Taxes on total income	txtottp	7476.727	14035.510
Change in total gross income since 1 year ago	wslya	2231.465	1950.065
Had an incorporated business	bifinc	1.715	0.199
Had a non-LLC or unincorporated business	bifuinc	1.259	0.193
Income			
HH current weekly gross wages - all jobs	hiwscei	992.666	918.261
Current weekly gross wages - main job	wscme	468.062	556.185
HH financial year gross wages	hiwsfei	52472.490	49458.180
Financial year gross wages	wsfe	25463.770	30265.630
Financial year regular market income	tifmktp	30734.790	33618.860
Financial year disposable total income	tifditp	27477.160	22701.270
Imputation flag: current weekly gross wages - all jobs	wscef	0.070	0.256

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Variable label	Variable name	Mean	SD
Imputation flag: current weekly gross wages - other jobs	wscoef	0.044	0.205
Imputation flag: financial year gross wages	wsfef	0.071	0.256
Other sources of income			
Receive superannuation/annuity payments	oifsup	0.059	0.232
Receive redundancy and severance payments	oifrsv	0.002	0.038
Receive other irregular payment	oifirr	0.001	0.027
Receive government pensions or allowances	bncyth	0.004	0.027
Receive Disability Support Pension	bnfdsp	0.151	0.181
Receive other regular public payments	oifpub	0.000	0.019
Financial year regular private income	tifprin	77.299	1409.625
Financial year investments	oifinvp	1951.052	10569.050
Financial year dividends	oidvry	744.263	4651.593
Financial year interest	oiint	666.116	3448.494
Financial year regular private pensions	oifpp	967.101	5055.004
Financial year business income (loss)	bifn	185.652	3274.511
Financial year business income (profit)	bifip	2597.792	13649.410
Financial year irregular transfers from non-resident parents	oifnpt	35.067	1305.812
Financial year public transfers	bnfapt	2865.540	4717.042
Financial year government non-income support payments	bnfnis	1025.031	2237.987
HH financial year public transfers	hifapti	5542.675	7937.136
HH financial year business income	hibifip	4880.589	18393.360

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Variable label	Variable name	Mean	SD
Health			
Imputation flag: current weekly public transfers	bncapuf	0.044	0.204
Imputation flag: financial year investments	oifinf	0.124	0.330
Imputation flag: financial year dividends	oidvryf	0.079	0.270
Imputation flag: financial year rental income	oirntf	0.071	0.257
Imputation flag: financial year business income	biff	0.071	0.258
Health limits vigorous activities	gh3a	2.108	0.718
How much pain interfered with normal work	gh8	1.704	0.971
Health condition/disability developed last 12 months	helthyr	1.870	0.151
Tobacco expense in average week	lstbca	37.771	10.690
Housing			
Years at current address	hsyrcad	9.541	10.226
External condition of dwelling	docond	1.970	0.870
No dwelling security	dosecno	0.552	0.497
No. homes lived in last 10 years	mhn10yr	3.456	1.107
Moved to be near place of work	mhreawp	0.084	0.111
Moved because I was travelling	mhrearo	0.009	0.038
Attitudes			
Importance of religion	loimprl	4.612	3.483
Working mothers care more about work success	atwkwms	3.729	1.807
Mothers who don't need money shouldn't work	atwkmsw	3.951	1.982
Identifiers			

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Variable label	Variable name	Mean	SD
Family number person 02	hhfam02	NA	NA
Relationship to person 03	rg03	NA	NA
ID of other responder for HH Questionnaire	hhp2	NA	NA

*Definition of technical and qualitative degree: Technical: STEM, Architecture, Agriculture and Environment, Medicine, Other Health-related Studies and Nursing, Management and Commerce and Law. Non-technical: Education, Society and Culture (includes economics!), Creative Arts, and Food, Hospitality and Personal Services.

Table 2: ML variables omitted by OLS Baseline model

Variable label	Variable name	Relationship with re-education (redufl)	Relationship with outcome (y_wscei)	Bias direction in OLS models
Education				
Doctorate obtained	edqodc	-	+	-
Employment				
Tenure with current employer	jbempt	-	-	+
Current work schedule	jbmsch	-	-	+
Casual worker	jbcasab	-	+	-
Occupational status - current main job	jbmo6s	+	+	+
No. persons employed at place of work	jbmwpsz	+	+	+
Prob. of accepting similar/better job	jbmpgj	+	+	+
Years unemployed and looking for work	ehtujyr	+	-	-
Work-life balance				
Total hours per week would choose to work	jbprhr	+	+	+
Parent provides child care	cpu_me			-
Do fair share of looking after children	pashare	-	+	-
Miss out on home/family activities	pawkmfh	+	+	+
Income				
Current weekly gross wages - main job	wscme	+	+	+
Imputation flag: current weekly gross wages - all jobs	wscef	+	+	+

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Variable label	Variable name	Relationship with re-education (redufl)	Relationship with outcome (y_wscei)	Bias direction in OLS models
Change in total gross income since 1 year ago	wslya	+	+	+
Financial year investments	oifinvp	-	-	+
Financial year business income (profit)	bifip	-	-	+
Amount outstanding on home loans	hsmgowe	+	+	+
Imputation flag: financial year dividends	oidvryf	+	-	-
Imputation flag: financial year rental income	oirntf	+	+	+
Imputation flag: financial year business income	biff	+	-	-
Health				
Health limits vigorous activities	gh3a	+	+	+
Tobacco expense in average week	lstbca	-	-	+
Identifiers				
ID of other responder for HH Questionnaire	hhp2	-	-	+

Table 3: Nested CV Holdout Sample: Level Earnings

Model	Outcome surface	Negative MSE	NMSE Std	R-squared	R-squared Std	ATE	ATE_std
GBR	Treated	-886515	452077	0.22	0.06	68.2	28.4
	Control	-659056	107251	0.36	0.07		
LASSO	Treated	-955958	361911	0.15	0.09	94.1	14.5
	Control	-710521	178030	0.32	0.05		
Ridge	Treated	-966849	434518	0.16	0.08	97.8	14.5
	Control	-712374	174033	0.32	0.04		

Notes: 5 fold CV performed on 80% train sample. All statistics presented in this table are based on the 20% holdout sample. Ten outer folds are used. See Figure 1 for more details.

Table 4: Average Treatment Effects: Level Earnings. Comparison across models.

Model	N	ATE	CI (ATE)
OLS (S-learner)	5441	64.41	[8.16, 120.66]
T-learner (GBR)	5441	88.38	[30.72, 137.15]
T-learner (LASSO)	5441	110.08	[4.01, 182.49]
T-learner (Ridge)	5441	108.95	[46.84, 183.05]
Doubly Robust (GBR)	5441	68.85	[50.91, 82.07]
Doubly Robust (LASSO)	5441	54.64	[27.97, 72.74]
Doubly Robust (Ridge)	5441	61.74	[45.7, 78.86]
Hierarchical Linear Model	5441	63.22	[0.63, 121.70]
Gaussian Process	5441	61.01	[12.63, 109.51]
Bayesian Causal Forests	5441	84.51	[26.28, 141.17]

Notes: Sample of 25 or older respondents who had completed a degree at any point between 2002 and 2017. Total completions: 1,383.

Figure 1: Selecting and Evaluating Model Class

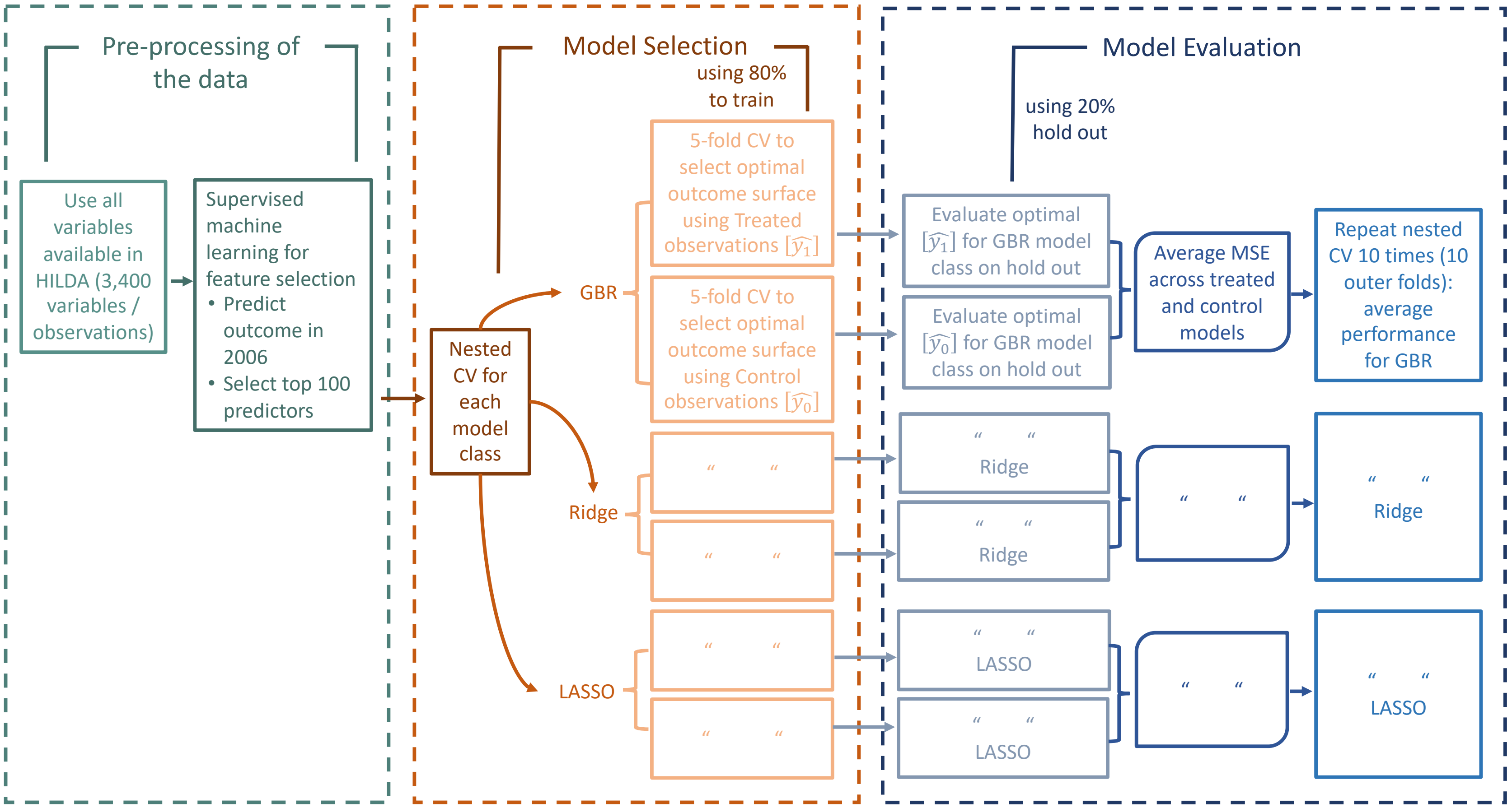

Pre-processing of the data
Use all variables available in HILDA (3,400 variables / observations)
Supervised machine learning for feature selection
• Predict outcome in 2006
• Select top 100 predictors
Model Selection
using 80% to train
Nested CV for each model class
GBR
Ridge
LASSO
5-fold CV to select optimal outcome surface using Treated observations $[\widehat{y_1}]$
5-fold CV to select optimal outcome surface using Control observations $[\widehat{y_0}]$
“ “
“ “
“ “
“ “
Model Evaluation
using 20% hold out
Evaluate optimal $[\widehat{y_1}]$ for GBR model class on hold out
Evaluate optimal $[\widehat{y_0}]$ for GBR model class on hold out
“ “ Ridge
“ “ Ridge
“ “ LASSO
“ “ LASSO
Average MSE across treated and control models
“ “
“ “
Repeat nested CV 10 times (10 outer folds): average performance for GBR
“ “ Ridge
“ “ LASSO

Figure 2: Generating Uncertainty Parameters

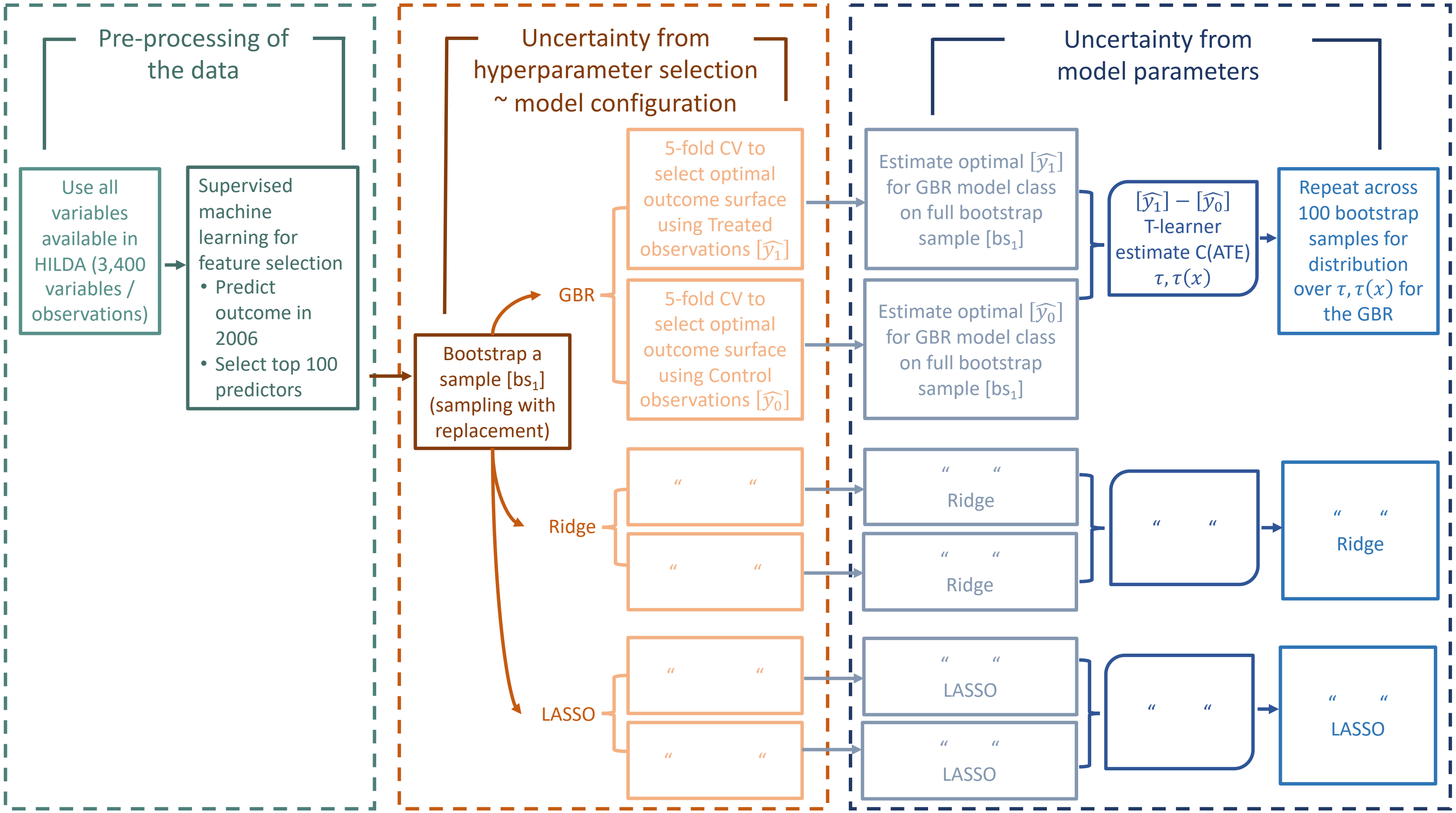
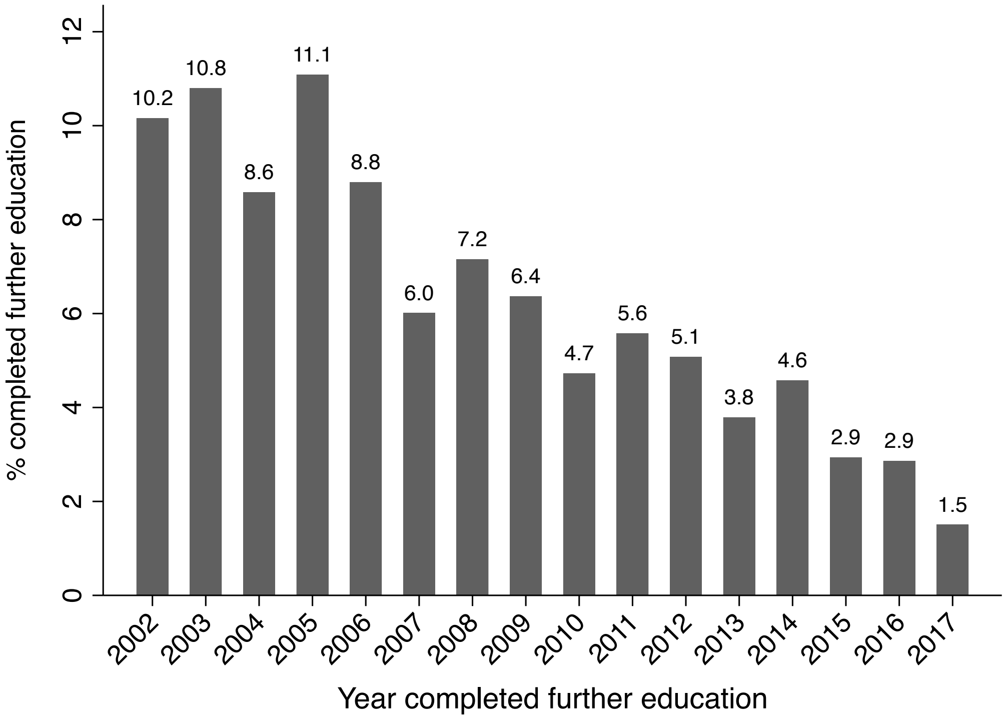

Pre-processing of the data
Use all variables available in HILDA (3,400 variables / observations)
Supervised machine learning for feature selection
• Predict outcome in 2006
• Select top 100 predictors
Uncertainty from hyperparameter selection ~ model configuration
Bootstrap a sample [bs_1] (sampling with replacement)
GBR
5-fold CV to select optimal outcome surface using Treated observations [$\widehat{y_1}$]
5-fold CV to select optimal outcome surface using Control observations [$\widehat{y_0}$]
Ridge
" "
" "
LASSO
" "
" "
Uncertainty from model parameters
Estimate optimal [$\widehat{y_1}$] for GBR model class on full bootstrap sample [bs_1]
Estimate optimal [$\widehat{y_0}$] for GBR model class on full bootstrap sample [bs_1]
[$\widehat{y_1}$] – [$\widehat{y_0}$] T-learner estimate C(ATE) $\tau, \tau(x)$
Repeat across 100 bootstrap samples for distribution over $\tau, \tau(x)$ for the GBR
" " Ridge
" " Ridge
" "
" " Ridge
" " LASSO
" " LASSO
" "
" " LASSO

Figure 3: Timing of Completion



Notes: Sample of 25 or older respondents who had completed a degree at any point between 2002 and 2017. Total completions: 1,383.

Figure 4: Degree completions by age

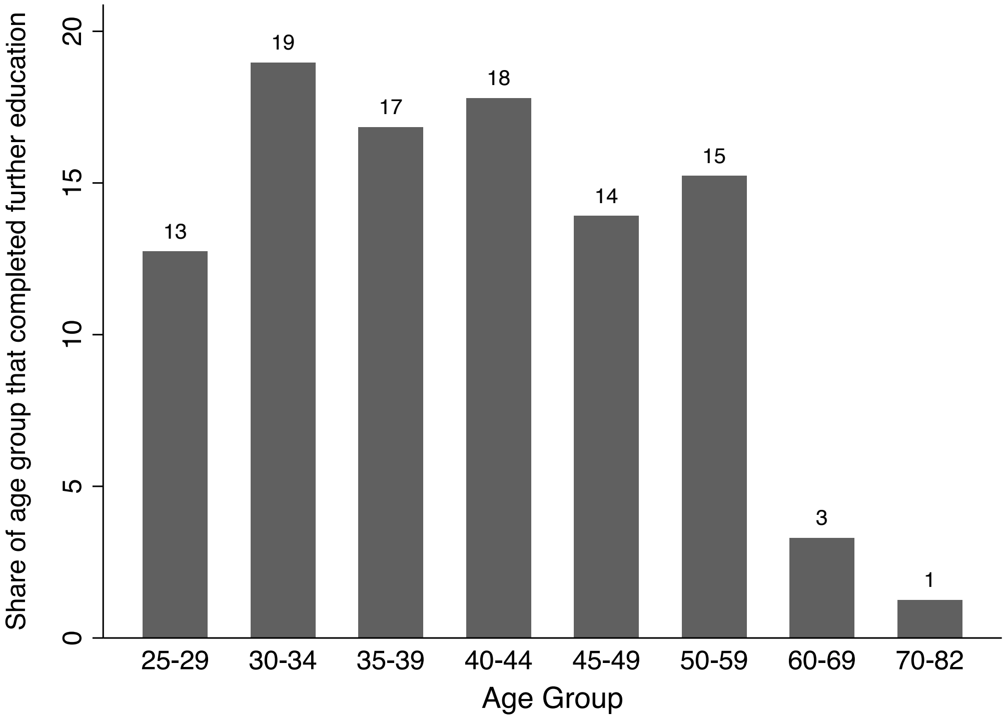
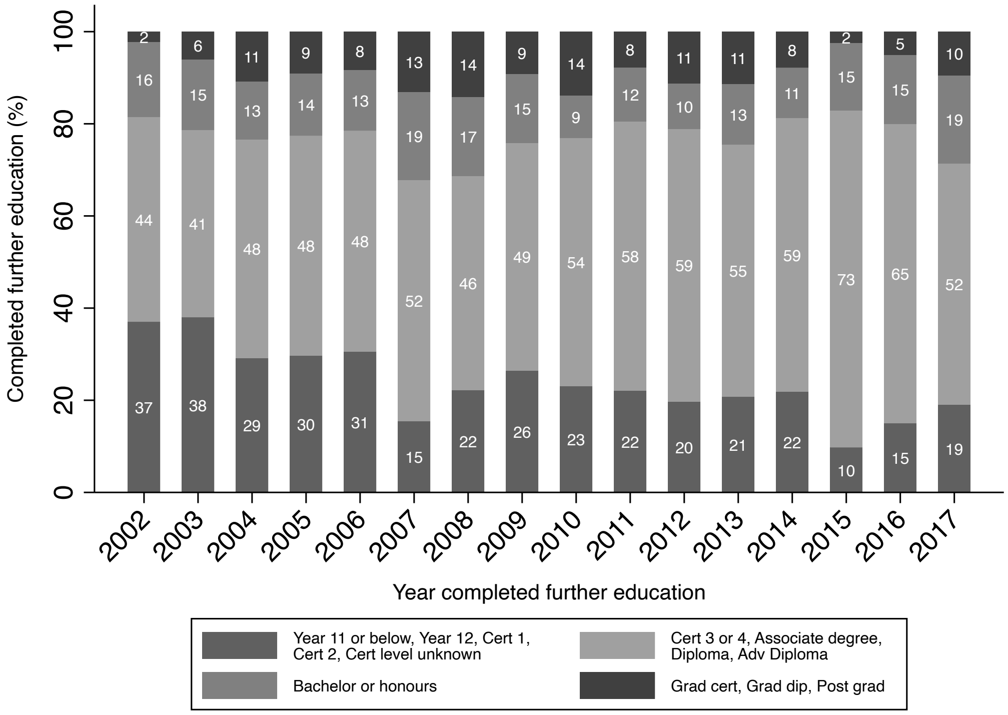


Figure 5: Timing of Completion by Type of Degree



Notes: Sample of 25 or older respondents who had completed a degree at any point between 2002 and 2017. Total completions: 1,383.

Figure 6: Degree completions by sex

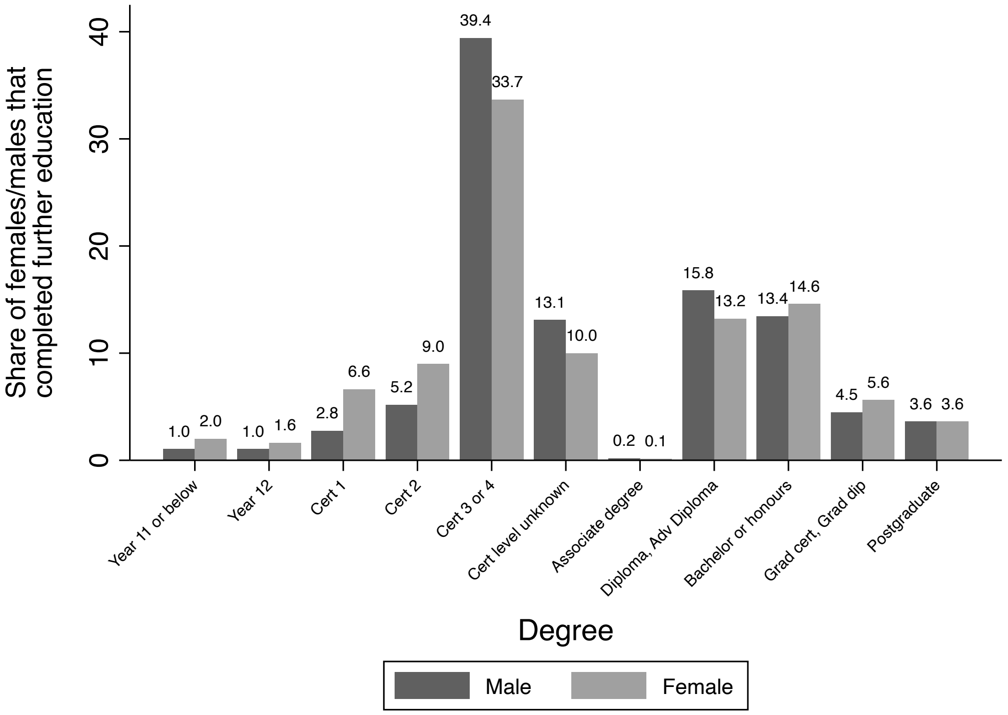


Figure 7: Earnings and Employment by year

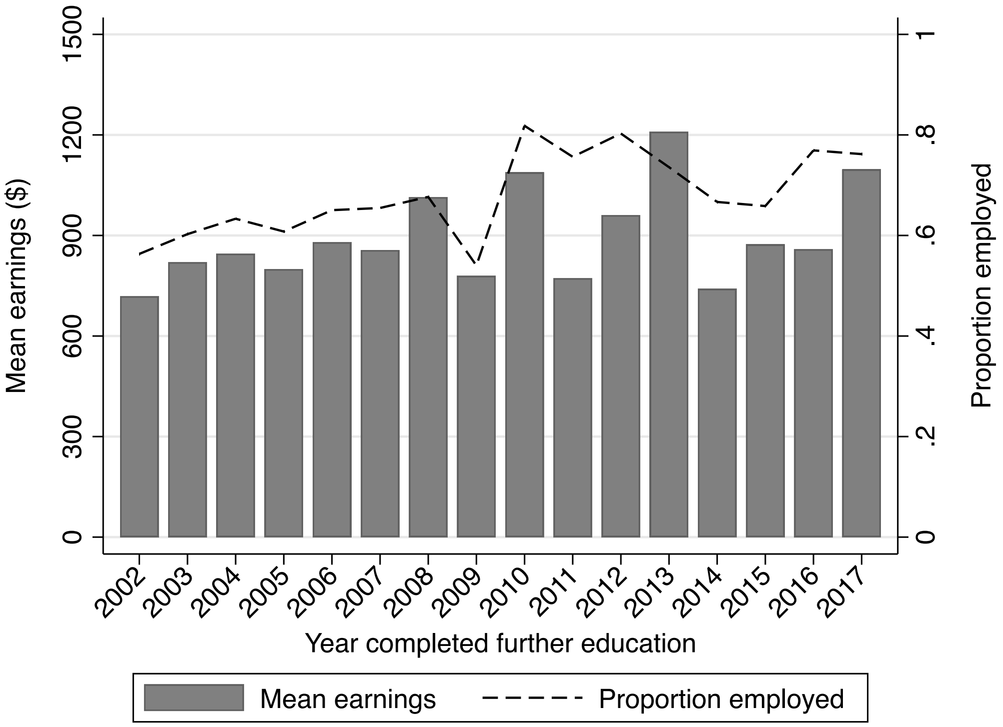
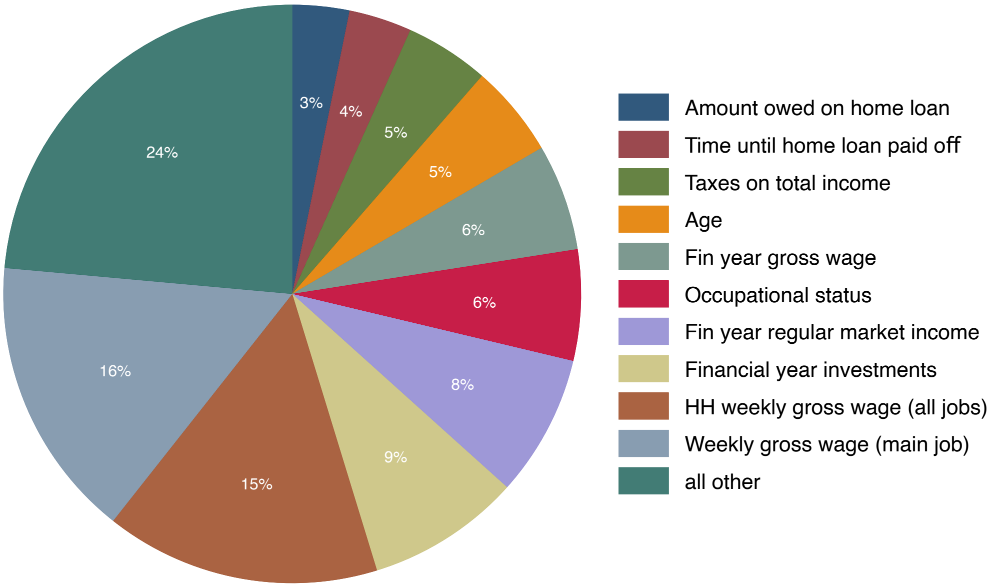
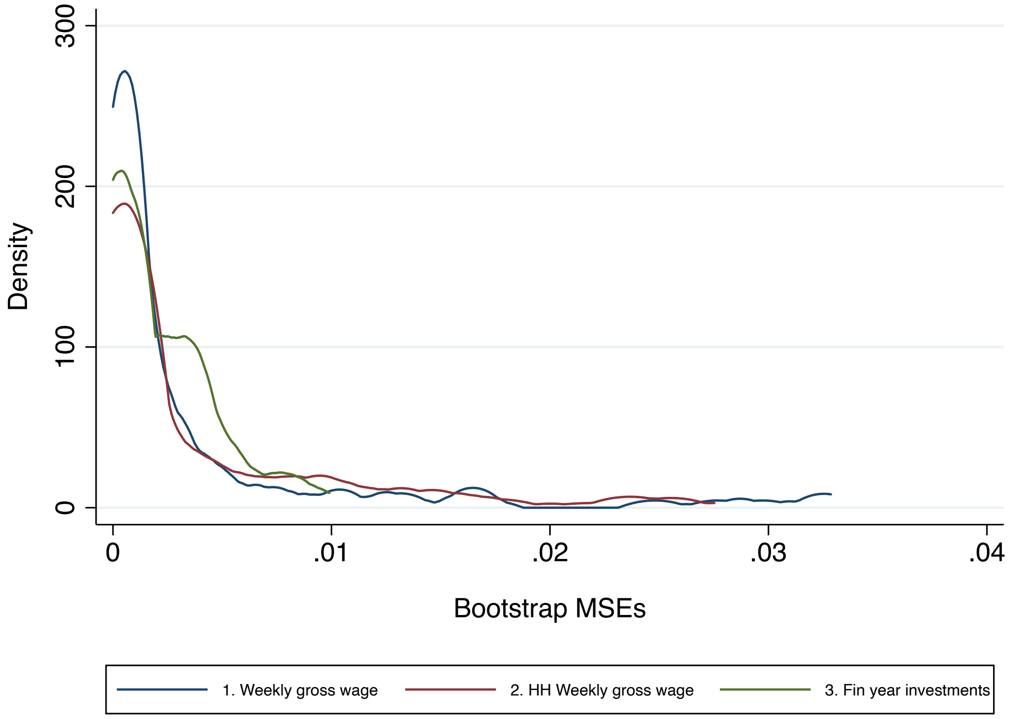


Figure 8: Important Features in Heterogeneous Treatment Effects Estimation using DR: Level Earnings



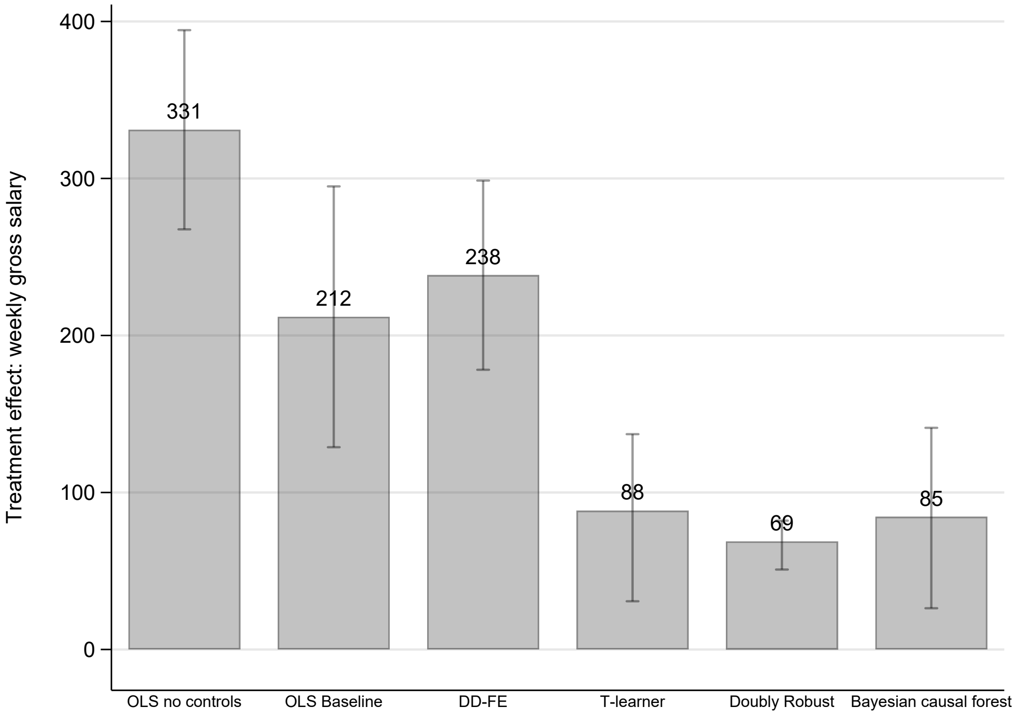
Notes: Sample of 25 or older who had completed a degree at any point between 2002 and 2017. Total number of observations 5,441.

Figure 9: Top 3 Features Distribution of Importance using DR: Level Earnings



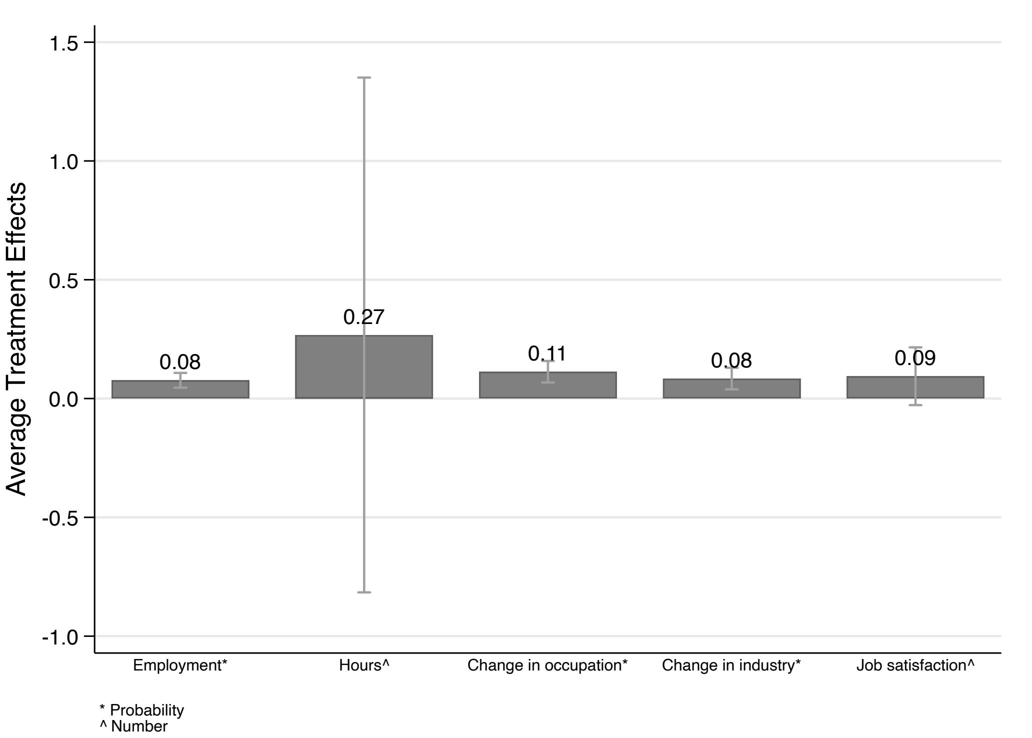
Notes: Sample of 25 or older who had completed a degree at any point between 2002 and 2017. Total number of observations 5,441.

Figure 10: Comparison of Treatment Effects across Different Methods



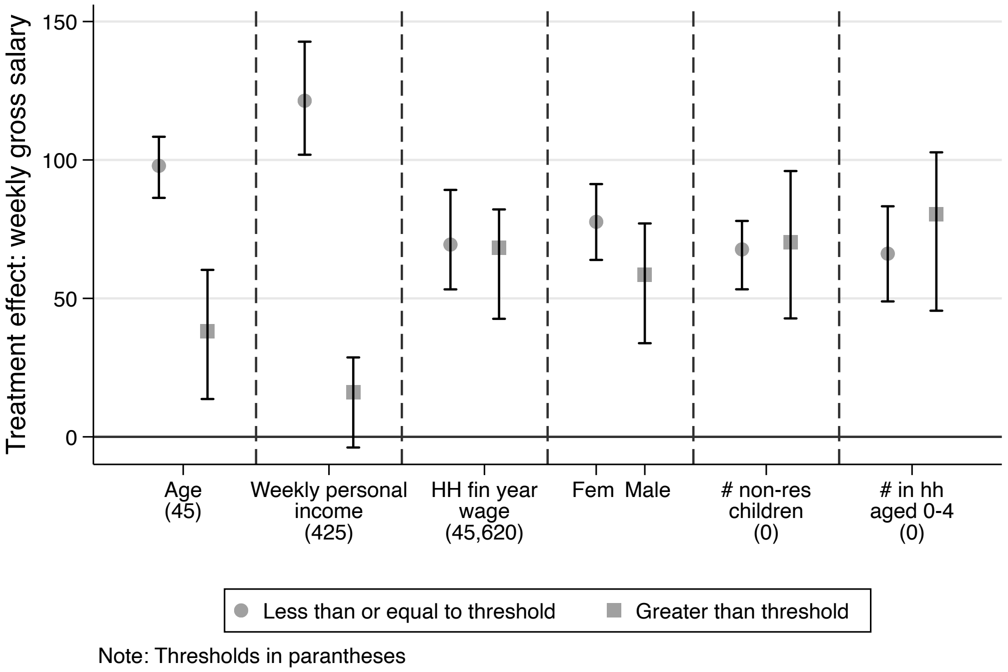
Notes: Unless stated otherwise, the method uses a sample of 25 or older respondents who had completed a degree at any point between 2002 and 2017. Total number of observations 5,441. The OLS Baseline model uses the features manually selected in models by Chesters (2015). The Difference-in-Difference Fixed Effects (DD-FE) model uses the same individuals as the other methods but follows them over two waves: 2001 and 2019 (i.e. there are 10,882 person-wave observations); person and wave fixed effects included. The T-learner and Doubly Robust results are based on the Gradient Boosted Regression. The last bar is based on the Bayesian Causal Forest.

Figure 11: Other Employment Outcomes



Notes: The impact of a new qualification. Sample of people who are 25 or older in 2001. Observation sizes vary depending on the outcome variable. All results are estimated using the LASSO algorithm.

Figure 12: Earnings HTEs: DR



Notes: Sample of 25 or older who had completed a degree at any point between 2002 and 2017. Total number of observations 5,441.

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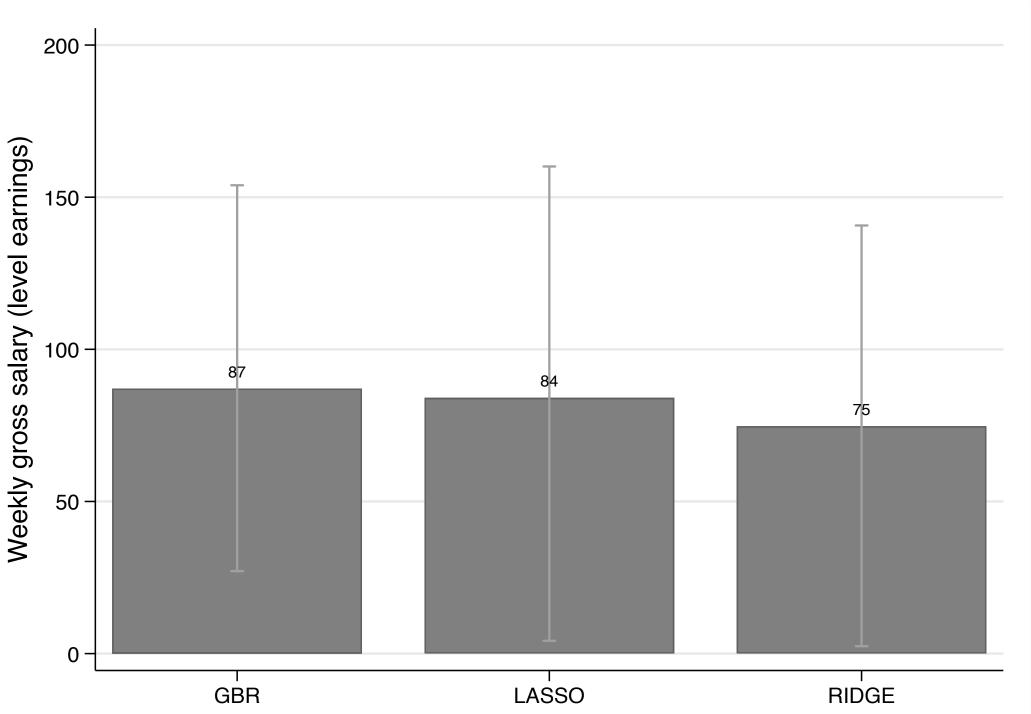
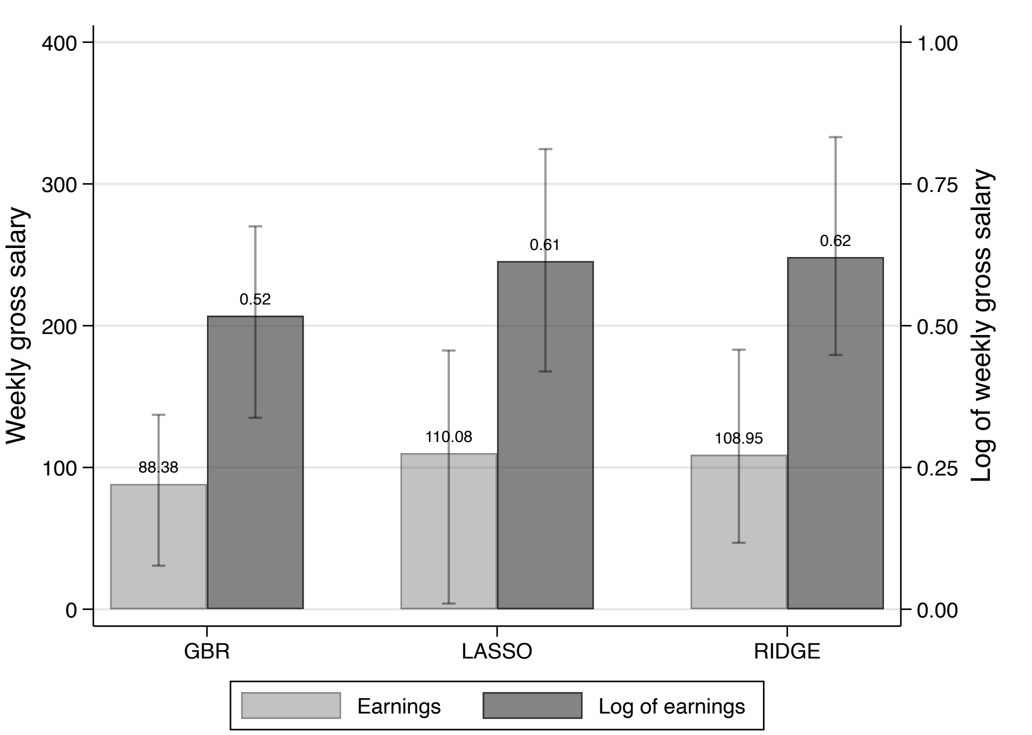
10 Appendix

Figure 13: Value-add in earnings: 25-45 year-old sample



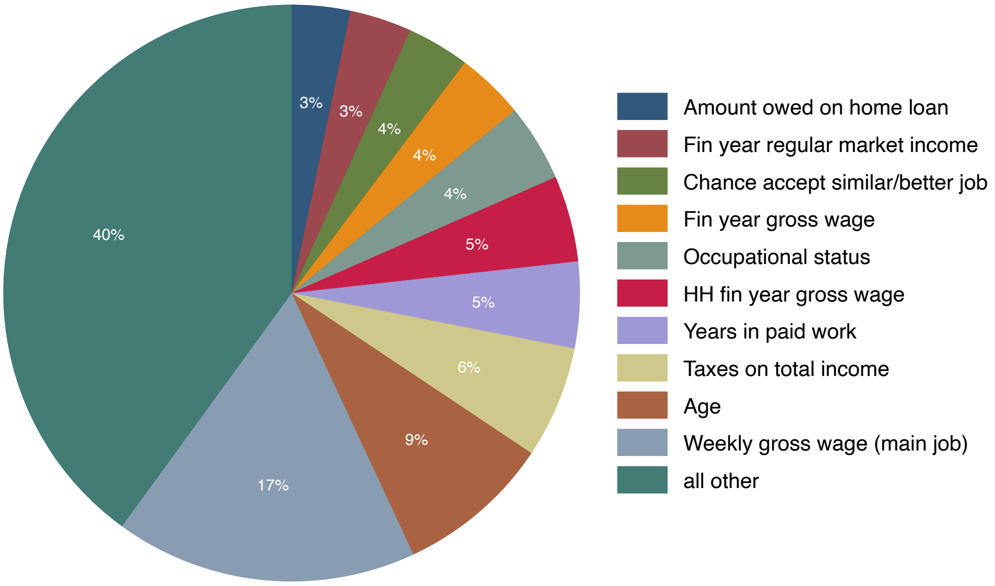
Notes: Sample of 25-45 who had completed a degree at any point between 2002 and 2017. Total number of observations 3,684.

Figure 14: Value-add in log earnings



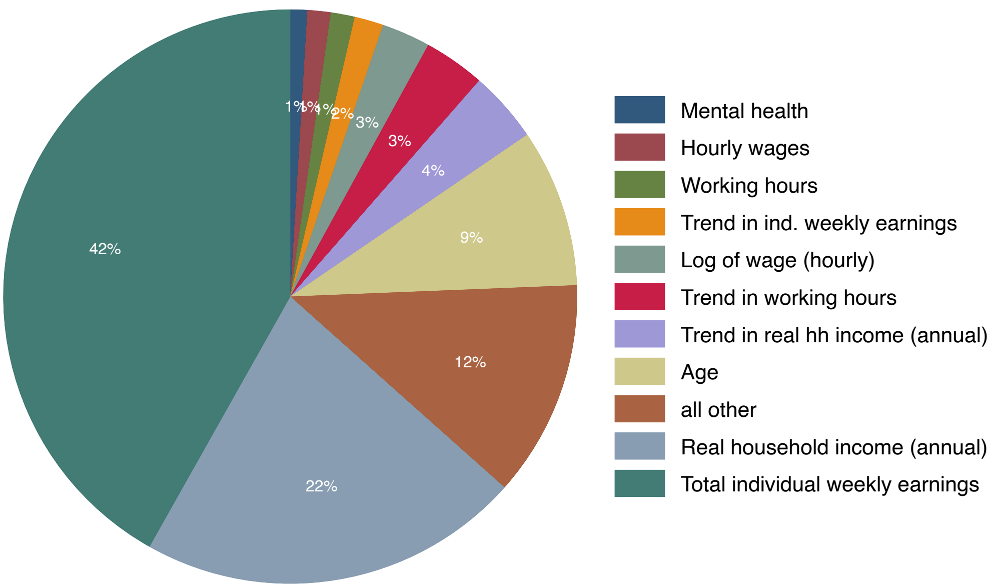
Notes: Sample of 25 or older who had completed a degree at any point between 2002 and 2017. Total number of observations 5,441.

Figure 15: Important Features in Heterogeneous Treatment Effects Estimation using T-Learner (GBR): Level Earnings



Notes: Sample of 25 or older who had completed a degree at any point between 2002 and 2017. Total number of observations 5,441.

Figure 16: Important Features in Heterogeneous Treatment Effects Estimation using panel sample (GBR): Level Earnings



Notes: Sample of 21 or older individuals who had completed a degree at any point between 2003 and 2015, inclusive. Outcomes are defined 4 years after a study spell began and features are defined in both the two years preceding the start of a study spell. There were 1,814 individuals who started and completed a further educational degree, and 60,945 non-unique control observations who never completed a further degree.