Timing is Everything: Labor Market Winners and Losers during Boom-Bust Cycles*

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This version: November 10, 2024

Abstract

Sectoral expansions and contractions require labor reallocation out of declining industries and into booming industries. Which types of workers gain and lose during these transitions? Using linked employer-employee panel data from Brazil spanning boom-bust cycles in its oil and gas sector, our results suggest that timing of labor market entry is critical. Only highly educated workers hired into oil at the onset of a boom experience positive earnings and employment gains relative to matched workers who enter other sectors. Low-education workers and later entrants constitute firms' margin of adjustment during busts. These workers experience persistent earnings and employment penalties, reflecting a last-in, first-out pattern. We document potential mechanisms consistent with these between-cohort and within-cohort disparities: accumulated experience in professional occupations may insulate high-education early entrants from downturns, while a boom in sector-specific training could erode earnings of later entrants. We discuss implications for workers during the energy transition.

JEL Codes: Q33, J24, J31, I24.
Keywords: Boom and Bust, Labor Reallocation, Human Capital, Inequality, Energy Transition.

*This manuscript has received generous feedback from the editor and three anonymous referees, seminar participants at AERE, BEEER, EAERE, the Geneva Trade and Development Workshop, the Instituto de Pesquisa Econômica Aplicada, PacDev, the RIDGE Labor Workshop, SOLE, Tilburg GIFT, the University of Alaska-Anchorage, University of Amsterdam, University of Geneva, UW-Madison, and Vrije Universiteit Amsterdam. UW-Madison's Institutional Review Board approved the use of human subjects data in this project (Project ID 2019-1122). Katovich thanks the Instituto de Economia at the Universidade Federal do Rio de Janeiro for access to the RAIS dataset. Katovich (corresponding author): 1376 Storrs Road, Storrs, CT 06269, USA; erik.katovich@uconn.edu. Parker: 427 Lorch Street, Madison WI 53706, USA; dominic.parker@wisc.edu. Poelhekke: De Boelelaan 1105, 1081 HV Amsterdam, Netherlands; steven.poelhekke@vu.nl. This paper formerly circulated as 'Labor Reallocation, Human Capital Investment, and "Stranded Careers": Evidence from an Oil Boom and Bust.'

1 Introduction

Economic sectors expand and contract asymmetrically as a result of trade shocks, new technologies, commodity cycles, and policy changes – requiring a continuous process of intersectoral labor reallocation. A major global energy transition to clean energy, for example, will require widespread exit of workers from fossil fuel industries and entry into renewables (Hanson, 2023; Curtis et al., 2023; Rud et al., 2022; Weber, 2020). Labor transitions of this scale involve costly frictions and have distributional consequences.[1] Some workers may benefit by possessing in-demand skills during a boom, while others will be displaced during busts and left "stranded" if their skills do not match those needed in growing sectors (Braxton and Taska, 2023; van der Ploeg and Rezai, 2020).

Which types of workers benefit most from sectoral expansions, and which are most vulnerable to displacement during contractions?[2] What mechanisms drive heterogeneity in outcomes across entry timing and worker type? Answers to these questions will inform the design of labor market policies to capitalize on future booms (e.g., in clean energy sectors) while softening the impacts of sectoral declines (e.g., in fossil fuel sectors).

We study the heterogeneous effects of entry timing on workers' labor market outcomes through a longitudinal analysis of labor reallocation into and out of the volatile oil and gas sector in Brazil.[3] During the 2000s and 2010s, Brazil experienced oil booms and busts driven by changes in global prices and major offshore oil discoveries. These developments led to disproportionate and difficult-to-anticipate expansions and con-

[1]Labor reallocation frictions include search and matching costs (Pissarides, 2014), skill loss during unemployment (Jarosch, 2021; Ortego-Marti, 2017), and skill mismatch between declining and expanding sectors (Baley et al., 2022; Şahin et al., 2014; Wasmer, 2006).

[2]Previous studies on timing of labor market entry have largely focused on either entry during an economy-wide boom (e.g., Bütikofer et al., 2022) or recession (e.g., Von Wachter, 2020; Altonji et al., 2016; Oreopoulos et al., 2012). Much less is known about the individual and distributional consequences of entry timing across sector-specific boom-bust cycles, which is relevant for workers building careers in volatile industries.

[3]Throughout this paper, we use the terms "oil" and "oil-linked" to refer to the oil and natural gas sector, as well as closely linked upstream and downstream sectors.

tractions in oil-linked employment relative to the broader economy, providing an ideal context to study the effects of a sector-specific boom-bust cycle on workers. We focus on Brazil's oil industry for several additional reasons. First, oil-linked sectors are economically relevant – accounting for up to 10% of formal employment in Brazil and similar shares in the United States and other oil-producing countries (US Dept of Energy, 2023). Second, fossil fuel industries are projected to lose 5 million jobs globally by 2050 under net-zero scenarios, highlighting the importance of understanding distributional impacts of the energy transition on workers.

Using employer-employee linked panel data covering all formally employed workers in Brazil between 2003-2017, we estimate dynamic wage, employment, and earnings effects of exposure to sectoral volatility on workers who enter oil-linked sectors at different points along the boom-bust cycle.[4] Focusing on even years for reasons of brevity and data availability, we estimate effects using event study specifications that compare outcomes for workers hired into oil during (i) an early boom in 2006, (ii) a brief bust in 2008, (iii) the mid-boom in 2010, (iv) the peak of the boom in 2012, and (iv) a major bust in 2014, relative to closely matched control workers who are hired into non-oil sectors in those same years. This setup allows us to gauge the effects of entry timing by comparing dynamic outcomes across cohorts, and effects of worker characteristics by comparing within cohorts. Rich administrative data allow us to impose strict coarsened exact matching criteria (Iacus et al., 2012), restricting control workers to those with comparable previous labor market trajectories, observable characteristics, and locations.

We find that timing of entry into the oil industry is associated with stark disparities in individual labor market outcomes. Among experienced workers hired into oil, the boom-bust cycle appears to have only benefited early entrants while leaving most later entrants stranded. Workers hired into oil-linked sectors early in the boom (in

[4]We analyze two modes of entry into oil-linked or other sectors: (i) *experienced hires*, who voluntarily leave their previous firm and are promptly rehired, and (ii) *first-time hires*, who are hired into their first formal job prior to the age of 30, and who may make educational investments in response to sectoral dynamics.

2006) experience sustained earnings growth in subsequent years relative to matched controls hired into other sectors. Earning premiums for this group persist despite a sectoral downturn in 2008 provoked by the Global Financial Crisis and a broader oil bust in 2014. In contrast, workers hired into oil in later years are more likely to lose employment during the bust, and are employed an average 20-40% *fewer* months per year by 2017. This negative employment shock results in annual earnings *penalties* for later entrants relative to matched controls, revealing a "last-in, first-out" pattern.

Disparities in labor market outcomes are more pronounced when we split the sample by workers' educational attainment. Within the 2006 cohort of early entrants, workers with more than secondary schooling are the only ones to exhibit positive earnings growth relative to matched controls across the boom-bust cycle. For later cohorts, education attenuates the association between exposure to busts and negative earnings relative to matched controls, but does not insure against these effects completely. Workers with less than secondary schooling experience negative earnings relative to matched controls across all cohorts – including early entrants. Negative earnings outcomes for these low-education workers are driven by the extensive (employment) margin. For example, low-education workers entering oil in 2006 are employed for 86% fewer months per year in 2017, relative to matched controls who entered other sectors.

Why do highly educated early entrants capture almost all the earnings and employment benefits of the boom-bust cycle, while low-education workers and later entrants are displaced and stranded? After systematically excluding the possibility that results are driven by matching specification, estimator, definitions of treatment, or changes in cohort composition, we present suggestive evidence in support of potential worker- and sector-level mechanisms underlying this dynamic. First, we show that firms disproportionately hire experienced, high-education workers to fill knowledge-intensive professional roles at the beginning of boom periods – perhaps to set up production processes – allowing these workers to accumulate on-the-job knowledge that could protect them from busts. We document that these workers are significantly less likely to switch occupations or establishments after entering the oil industry. In contrast, low-education workers occupy easy-to-replace roles with little on-the-job knowledge accumulation, making them the margin of adjustment when firms face negative shocks.

At the sector-level, we use data from Brazil's Higher Education Census to show the oil boom was accompanied by rapid growth in oil-specific degree programs and graduations, and that this growth was strongest near oil industry hubs. Growth was driven by expansion of private-sector technical training programs focused on the oil industry, which increased from 82 graduates in 2003 to 12,177 in 2015, before falling to 8,500 in 2016. This lagged surge in the supply of sector-specific skills may have increased competition for later entrants, contributing to declining returns for high-education first-time hires over time. Stranded careers thus appear to be accompanied by degrees that are no longer in demand, revealing relatively irreversible human capital investment as a key channel underlying long-run adverse effects.

1.1 Related Literature and Contributions

Our findings contribute to the literature on sector-specific labor market shocks (García-Cabo et al., 2023; Kim and Vogel, 2021), with special relevance for commodity-dependent countries exposed to sectoral volatility (van der Ploeg and Poelhekke, 2009). Aligned with our findings, Autor et al. (2014) show sector-specific declines caused by trade exposure to China disproportionately harm low-wage workers in the US. In contrast to our findings, Hombert and Matray (2019) show skilled early entrants into the French IT sector earn *less* over time than similar workers in other sectors due to rapid skill obsolescence. This contrast suggests that sectoral characteristics (e.g., rate of technological change and scale of rents) and institutional features of the labor market (e.g., search and matching costs and seniority benefits) determine the magnitude of the first-movers' advantage.

Further, we show that a skill-biased boom provoked growth in sector-specific higher education. This provides a counterpoint to previous findings that a booming low-skill-intensive sector reduces higher education in aggregate (Balza et al., 2021; Charles et al., 2018; Emery et al., 2012; Black et al., 2005b). Our results align with Bütikofer et al. (2023) and Balza et al. (2022), who document overall reductions in higher education attainment but increased vocational training following resource booms. Chuan (2022) finds that oil booms reduce male college enrollment but increase later-life earnings through an on-the-job-skill-accumulation channel. This finding aligns with our result that entry during a boom and experience in skill-intensive roles is associated with larger labor market benefits than sector-specific academic training.

This paper contributes new evidence on the Resource Curse by documenting scarring effects of oil busts and revealing how oil volatility generates inter-cohort and intra-cohort inequality. Literature on this topic has increasingly shifted from country-level to subnational analyses (Pelzl and Poelhekke, 2021; Cavalcanti et al., 2019; Allcott and Keniston, 2018; Jacobsen and Parker, 2016; Cust and Poelhekke, 2015; Aragón and Rud, 2013; Black et al., 2005a), but continues to focus overwhelmingly on places rather than people. Exceptions include Bütikofer et al. (2022), Jacobsen et al. (2023), Kovalenko (2023), and Guettabi and James (2020), which use individual-level longitudinal data to measure impacts of exposure to oil booms and busts. We complement these studies by measuring heterogeneity in workers' labor market experiences by timing of entry and education across an oil boom-bust cycle.

Finally, we contribute worker-level evidence on the distributional consequences of energy transitions – which will involve declines in fossil fuel employment, growth in renewables, more demand for critical minerals, and frequent sectoral booms and busts driven by rapid technological change and sectoral policies (Curtis et al., 2023; Michieka et al., 2022; Rud et al., 2022; Sharma and Banerjee, 2021). We document mechanisms related to occupational skill content that drive heterogeneity in labor market outcomes for workers transitioning into and out of a fossil fuel industry.

2 Boom-Bust Effects on Workers: Empirical Questions

How does entry into a boom-bust industry affect workers' labor market outcomes, and why might effects depend on entry timing and educational attainment? To answer this, we formulate three empirical questions about which existing literature provides only mixed answers.

Empirical Question 1: *Are earnings premiums in a booming sector shared across workers of all education levels, or concentrated among high-skill workers?*

Studies show that firms affected by positive profitability shocks (e.g., increased oil prices) often share rents with employees (Macis and Shivardi, 2016; Card et al., 2014; Guertzgen, 2009). Bargaining models predict that workers with more bargaining power within firms capture most of these rents, while fairness and risk-sharing models predict more even rent-sharing across worker types (Martins, 2009).

Empirical Question 2: *Do firms respond to negative shocks by laying off low or high-skill workers, and what are the effects of job loss for these different groups?*

Firms can respond to negative shocks by laying off workers of different skill levels asymmetrically (Beuermann et al., 2021). Filling specialized high-skill positions is typically more costly for firms than filling low-skill positions, creating an option value of retaining skilled workers (Dolado et al., 2009; Albrecht and Vroman, 2002). High-skill roles may also involve greater accumulation of firm-specific knowledge, conveying hold-up power on high-skill workers that protects them during shocks (Bloesch, 2022). Binding minimum wages may protect low-skill workers' wages during downturns but push more of them into unemployment (Cockx and Ghirelli, 2016). On the other hand, job loss may result in larger earnings and employment penalties for experienced or higher-skill workers if separation resets their progression up the job ladder and destroys firm-specific human capital (Jarosch, 2023; Couch and Placzek, 2010).

Empirical Question 3: *How do experienced early entrants into a booming sector fare relative to new entrants with updated skills and training?*

The relative importance of different mechanisms determining labor market returns for early versus later entrants are not well established. Early entrants may earn higher premiums if firms hire workers into more skill-intensive roles at the onset of booms (Braxton and Taska, 2023; Modestino et al., 2016), if working in an immature sector entails higher risk (Black and de Meza, 1997), or if early entry avoids direct competition with specialized later entrants. Early entrants may benefit from sufficient time to climb the job ladder before encountering a bust (Jarosch, 2023). Finally, labor protections that increase with tenure may favor early entrants by creating seniority bias within firms. To the disadvantage of early entrants, later entrants may possess more up-to-date or specialized human capital (Hombert and Matray, 2019). In a sector where technology evolves quickly – as in software and energy industries – this may lead firms to favor newer entrants over incumbents.

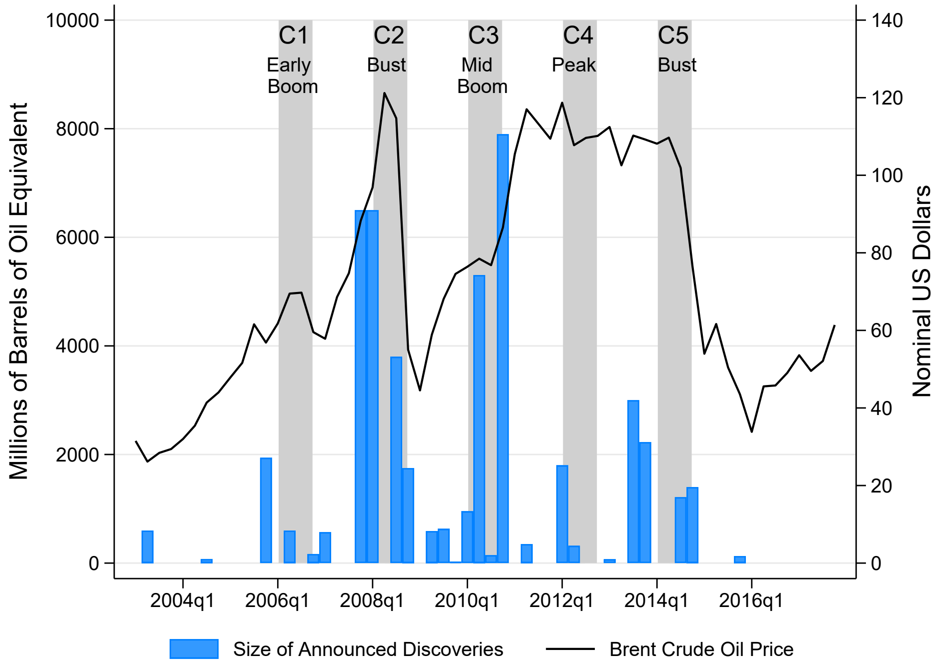
We next describe a context where exogenous and unpredictable sector-specific booms and busts allow us to assess these questions empirically.

3 Data and Context

3.1 Oil Boom and Bust in Brazil

Brazil's oil and gas sector presents an ideal context for this study, given the relative importance of oil-related employment in the country's economy and asymmetric shocks that amplified the oil industry's boom and bust dynamics beyond movements in other sectors. Beginning in 2006, Brazil made a series of giant offshore oil and gas discoveries, with the largest three discoveries each adding over six billion barrels of oil equivalent. Discoveries coincided with a rise of the global oil price, from an average nominal price of US$21 per barrel over the 1990-2003 period to a peak of US$134 in July 2008. Oil prices crashed in 2008 as a result of the Global Financial Crisis, but recovered quickly and remained above US$100 per barrel until 2014, after which they dropped sharply to US$30 per barrel by 2016 (Figure 1).[5]

Figure 1: World Oil Prices and Major Offshore Discoveries in Brazil



Note: Brent Crude oil prices are drawn from FRED. Announced discovery volumes are aggregated from a comprehensive list of discoveries compiled in Katovich (2024). As explained in more detail in Section 3.3 below, shaded intervals denote periods during which workers in selected study cohorts (2006 or C1, 2008 or C2, 2010 or C3, 2012 or C4, and 2014 or C5) began jobs in oil-linked sectors. These cohorts are chosen to allow 3 years of observable pre and post-periods inside the available 2003-2017 interval. Even years are selected for brevity. In Appendix Figure A1, we report Brent Crude world oil prices deflated to constant 2017 Brazilian Reals at contemporaneous USD/BRL exchange rates. Boom-bust patterns remain very similar.

Discoveries and high oil prices combined to provoke rapid growth in oil-sector investment during this period. Investments by Petrobras, Brazil's national oil company, increased from USD$4.3 billion in 2000 (constant 2010 values) to USD$59.1billion in 2013 (Petrobras, 2020). The subsequent collapse of world oil prices in 2014 reduced commercial viability of ultra-deep Pre-Salt fields and squeezed operating margins

[5]Brazil experienced a previous small oil boom between 1982-1985, when oil production increased from 85 to 220 million barrels/year. The oil & gas boom of the 2000s and 2010s examined here increased production to 1,157 million barrels/year by 2017. Between 2003 and 2017, Brazil's oil production averaged 2.4% of global supply (0.5% for natural gas), suggesting that changes within Brazil were unlikely to affect global prices (Energy Institute, 2023).

along the oil and gas supply chain. Also in 2014, a corruption scandal (*Lava Jato* in Portuguese) involving Petrobras caused the company to freeze much of its investment portfolio, which fell to USD$27.3 billion by 2017 (see Appendix A2).

3.2 Labor Market Dynamics

To study the labor market impacts of oil price shocks, discoveries, and investment levels, we use linked employer-employee administrative records covering the universe of formal establishments and employees in Brazil from the *Relação Anual de Informações Sociais* (RAIS).[6] Our outcomes of interest are hourly wages (in constant 2018 Brazilian Reals), months employed per year (ranging between 0 and 12, with zero ascribed to workers who do not appear in RAIS during a given year), and annual earnings (defined as total earnings across all formal jobs in constant 2018 Reals).[7]

We use 7-digit industry codes to identify workers employed 'directly' in the oil industry, defined as extraction and processing of petroleum & natural gas, oil & gas extraction supporting activities, and fabrication, maintenance, repair, and rental of machinery and equipment for oil prospecting. Brazil's oil industry is dominated by capital-intensive offshore production, but nevertheless employs over 100,000 workers directly (Figure 2). Oil also exerts strong upstream and downstream linkages, generating significant oil-*linked* employment (Negri et al., 2010). Upstream sectors

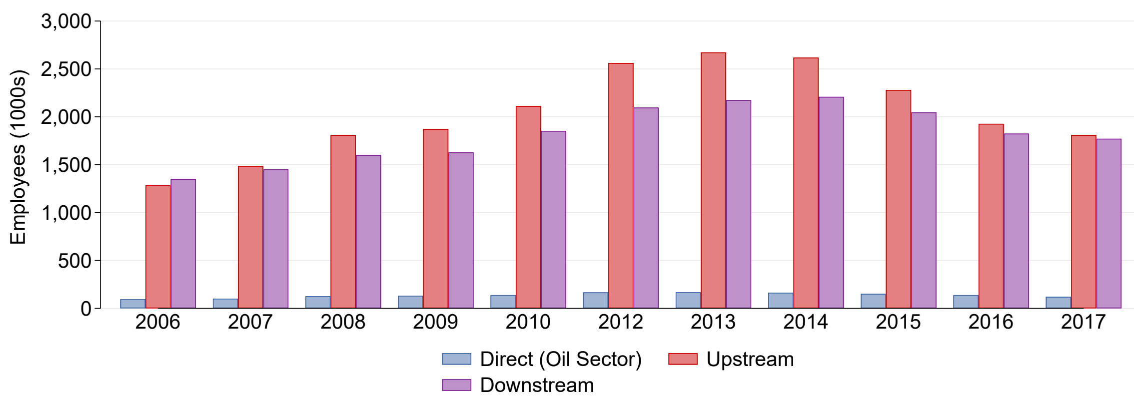
[6] We have access to the years 2003-2010 and 2012-2017, which contains between 40-73 million job-level observations per year. Data were cleaned using standardized procedures developed by Dahis (2020). While RAIS provides rich labor market data for the universe of formal establishments and employees, it does not report information for the informal sector. If workers do not appear in the RAIS dataset in a particular year, we cannot determine whether they are unemployed, self-employed, or informally employed in that period. In Appendix Figure A4, we draw on nationally representative household survey data from the *Pesquisa Nacional por Amostra de Domicílios* (PNAD) to document that oil-linked sectors exhibit significantly higher rates of formal employment than the Brazilian economy as a whole, suggesting we miss relatively fewer workers by focusing on these sectors. In Appendix Figure A5, we use PNAD to document that formal wages are significantly higher than informal wages, both in oil-linked sectors and the broader economy.

[7] Earnings are transformed using the inverse hyperbolic sine function to retain observations with zero-value outcomes (Bellemare and Wichman, 2019), to reduce the influence of upper-tail outliers, and to adjust effects into percentage terms. Wages are similarly transformed for consistency, but note that wages are never zero because we keep only employed workers when estimating wage effects. We do not transform the number of months employed per year.

include construction of ships and drilling rigs, fabrication of pumps and pipes, geological data collection and analysis, and maritime cargo and transport. Downstream sectors include refining of fuel products and lubricants and petrochemicals. Using Brazil's Input-Output Matrix (IBGE, 2010), we identify 109 upstream sectors, and 31 downstream sectors, reported in Appendix Table B3. This process is described in detail in Appendix B1. We include upstream and downstream linked sectors in our analysis to achieve a more comprehensive picture of oil sector impacts.

As illustrated in Figure 2, employment in Brazil's oil-linked sectors expanded and contracted along with the boom bust cycle described above. At the peak of the boom in 2013, Brazil's oil-linked sectors employed nearly 5 million formal workers, accounting for 10.3% of formal employment in the country. In Appendix Figure A3, we plot relative employment growth (net hiring rates) in oil-linked versus non-oil-linked sectors, showing that (i) expansions and contractions in oil-linked employment are larger in magnitude than movements in the broader economy, and (ii) sectoral hiring is sensitive to boom-bust dynamics.

Figure 2: Oil-Linked Employment (2006-2017)



Note: Bars denote thousands of employees in different segments of Brazil's oil sector (direct, upstream, and downstream). Direct, upstream, and downstream sectors are identified from input-output tables (IBGE, 2010), and are reported in Appendix B3. Formal employment is calculated from RAIS. Data from years prior to 2006 are omitted since detailed 7-digit industry codes are first reported in RAIS in this year.

3.3 Defining Study Cohorts

Exogenous world price shocks, offshore discoveries, and an unexpected corruption scandal generated strong asymmetric shocks to Brazil's oil-linked sectors, which translated into a boom and bust in the stock of sectoral employment and sharp peaks and crashes in the flow of net hires. Based on these dynamics, we define five cohorts of workers as the focus of our analysis: these workers entered oil-linked sectors in 2006, 2008, 2010, 2012, and 2014, and thus experienced different phases of the boom-bust cycle. We focus on these even-year cohorts for brevity, because of the availability of data (2003-2017, but 2011 is unavailable), and to allow observation of at least three pre- and post-periods for each cohort.

The entry timing of selected study cohorts relative to price and discovery trends is illustrated in Figure 1 above. Workers who entered oil in 2006 (**C1**) joined during what we loosely refer to as the "early boom." For this cohort, the oil and gas sector's full growth period was ahead of it, and subsequent years brought high prices, new discoveries, and large ramp-ups in investment. Workers who entered in 2008 (**C2**) promptly experienced a sharp crash in oil prices. Workers who entered in 2010 (**C3**) experienced a secondary "mid-boom," characterized by recuperation in prices and major new discoveries, but with less growth ahead of them before the coming 2014 bust. Workers who entered in 2012 (**C4**) joined during peak oil prices and proximate to the bust. Finally, workers who entered in 2014 (**C5**) immediately experienced a deep sectoral bust.

3.4 Education and Labor Market Policies

In the early 2000s, Brazil introduced sectoral policies to meet booming demand for workers with oil-relevant skills. Petrobras, together with public-private industry groups, implemented the Program for Mobilization of the National Oil and Gas Industry (Prominp), which facilitated technical training programs and graduated over 80,000 oil-sector professionals between 2007 and 2017(SINAVAL, 2020).

During this period, formal employment contracts were governed by relatively strong labor protections laid out in the *Consolidação das Leis do Trabalho* (CLT). Employers incur expenses to lay off a worker without just cause, such as an obligation to pay unemployment insurance equivalent to a fraction of the employee's highest pay-period and a fine of 40% of the accumulated value of deposits made monthly in the employee's Guarantee Fund for Time of Service (FGTS in Portuguese). These rules make it disproportionately expensive for firms to lay off high-earning or senior workers (CLT, 2017). Based on the limited duration and value of unemployment benefits (ranging from 1 to a maximum of 5 months and from 80% of highest salary for the lowest-paid workers to substantially less than 50% for high-paid workers), we believe workers faced strong incentives to find new employment opportunities after losing their job during the oil bust. Consequently, negative effects of the bust on employment likely reflect true difficulties for displaced workers, rather than a response to weak incentives to find new employment. Labor market regulations of this type are comparable to those found in many developing and OECD countries (Betcherman, 2014; Bassanini and Duval, 2006).

4 Empirical Strategy

What are the labor market impacts of being hired into the oil and gas industry at particular points along a boom-bust cycle? How do effects vary by workers' education level? We focus on two ways in which a booming energy sector may expand employment: (i) by poaching experienced workers, and (ii) by hiring new workers who graduate or age into the labor market. These first-time hires may have made educational investments in response to sectoral dynamics, such as obtaining oil-related degrees that could earn them higher wages – depending on how employers value updated training relative to on-the-job experience. We define experienced hires as those who left their previous job voluntarily and are rehired at a new firm within 4 months. We define first-time hires as workers aged 30 or less who obtain their first formal job. For each experienced worker hired in a particular year, we construct a complete 2003-2017 employment trajectory. For first-time hires, we construct their complete post-hire employment history.[8]

4.1 Sources of Bias

In an ideal experimental setting workers would be randomly hired into oil-linked and control establishments. In practice, however, workers may select or be selected into oil based on characteristics that correlate with labor market outcomes. Selection occurs both in workers' decision to quit their former job (for experienced hires) and in the decision to accept a particular new job (for both types of entrants).

While we are not able to fully control for these sources of bias in our non-experimental setting, we take steps to minimize selection bias by implementing a combination of matching on pre-switch characteristics and dynamic difference-in-differences estimation.[9] Matching restricts the sample to treated and control workers who are comparable across observable characteristics and pre-treatment labor market indicators that correlate with the endogenous decisions to switch jobs and to enter the oil sector. Inclusion of individual fixed effects absorbs time-invariant worker characteristic s (including risk preferences and ability insofar as these are fixed), and plotting of pre-trends allows us to assess whether workers who switched into oil versus other sectors were on similar pre-treatment career trajectories. Nevertheless, we cannot rule out that time-varying unobservables drive the decision to switch jobs. We thus interpret our results descriptively, rather than causally, throughout the manuscript.

[8] To explore predictors of being hired into oil as (i) an experienced or (ii) a first-time hire, we regress this outcome on worker characteristics, previous employment characteristics, and year and state fixed effects. Results, reported in Appendix B.2, indicate that higher-education, male, non-white, and older workers are significantly more likely to be hired into oil. Among experienced workers, those hired into oil-linked establishments tend to come from larger firms, and within their previous firms, they were *not*, on average, among top earners or levels of education or management.

[9] Others have proposed to overcome selection into/out of employment by exploiting displacements that are exogenous to the worker (Jacobson et al., 1993; Fallick et al., 2021). However, this approach would limit our analysis to a very particular type of entrant into oil (i.e., workers who were previously displaced), reducing the generalizability of our findings.

Further sources of potential selection bias come from the possibility that (i) workers anticipate oil sector booms and busts, or (ii) the composition of cohorts may change across the boom-bust cycle, confounding cross-cohort comparisons. Usefully for our purposes, workers hired into oil between 2006 and 2014 were exposed to asymmetric and difficult-to-anticipate labor market shocks driven by exogenous and unprecedented offshore discoveries and changes in global energy prices. Revelation of the *Lava Jato* corruption at Petrobras in 2014, which deepened Brazil's oil bust, was also unanticipated. The difficulty of foreseeing oil sector developments during this period reduces concerns that systematically different types of workers may have self-selected into oil. Our broad definition of "oil-linked" workers further reduces concerns over self-selection, as this definition includes not only sectors for which oil volatility is very salient (e.g., oil rig workers), but also many more workers in upstream and downstream sectors for whom oil volatility is much less salient (e.g., shipbuilders).

Could the composition of entrants hired into oil-linked sectors have changed over the course of the boom-bust cycle? Workers hired into oil in 2006 might be more forward-looking or risk-loving than laggards who enter the sector after observing its growth. The booming sector may also draw in workers with progressively lower productivity or sector-specific efficacy, as in Young (2014). To reduce these concerns, in Appendix C we compare a variety of characteristics across cohorts and estimate robustness checks that (i) restrict samples to workers who share common support on observables across cohorts, and (ii) restrict samples to workers with no previous oil experience. Results are similar to our preferred specification.

4.2 Matching

To reduce selection bias caused by different types of workers choosing to switch into oil versus other sectors, we first implement coarsened exact matching (CEM) on pre-switch characteristics and labor market indicators. This procedure identifies cohorts of comparable workers, some of whom (the "treated") are hired into an oil-linked sector, while others ("controls") are hired into other sectors in the same year.[10]

We match workers hired into oil-linked sectors with workers hired into other sectors within each year-cohort separately. For experienced workers, we match exactly on education, sex, a non-white race indicator, and labor market outcomes over a two-year retrospective matching window, including previous establishment, previous occupation category (low/high skill white collar and low/high skill blue collar), previous wage bin (0-1, 1-2, 2-3, 3-5, 5-10, 10-20, >20 minimum wages), previous age bin (e.g. $\leq$ 16, 16-20... 56-60, >60), and destination municipality. These variables capture most of the information a prospective employer would have access to when deciding whether or not to hire a new employee. By matching exactly on previous establishment, we restrict the sample to workers who came from the same pool of previous establishments, ensuring workers are comparable along the dimension of pre-switch productivity insofar as this is captured at the establishment level. By matching on destination municipality, we restrict the sample to workers who switch into the same locations, reducing concerns about treated or control workers being disproportionately affected by idiosyncratic spatial shocks. The retrospective matching window constrains the matched sample to experienced workers who were on similar labor market trends and who made similar past choices for particular employers and sectors – at least partially capturing workers' unobserved preferences and risk attitudes. Comparing experienced hires with each other (where both chose to leave the same employer, but each for a new job in different sectors) aims to reduce concerns over selection-into-treatment that would arise if we compared workers who chose to leave their job with workers who chose to stay.

For newly hired workers, we are unable to observe pre-hire characteristics, and

[10] We opt for CEM over other matching procedures due to CEM's: (i) transparent implementation that achieves exact matches on categorical variables (including establishment and municipality); (ii) *ex-ante* imposition of balance across observables, wherein choosing the balance criterion for one covariate does not affect balance across other covariates; (iii) customizable bins that respect context-sensitive cutoffs, such as education levels; (iv) retention of all matched observations in sample, rather than 1-to-1 pairs (Iacus et al., 2012).

can thus match on fewer variables that correlate with selection. We match exactly within each cohort on education, sex, a non-white race indicator, municipality of hire, and age (using finer two-year intervals), as well as workers' first wage (using the same bins described above), and first establishment size (defined by micro (<10 employees), small (10-49), medium (50-249), and large (>249) establishments).[11] We drop workers for whom no match can be found.

We present baseline descriptive statistics, sample sizes, and balance statistics for full and matched samples in Appendix B.3. One benefit of CEM matching is that balancing between treated and control groups is automatic for the (binned) variables that we match on, because individuals who do not share common support are dropped. The estimand obtained from Equation 1 is thus the average treatment effect in the matched sample (ATM), which is generalizable to the broader population in proportion to comparability between the matched and full sample. Tables B7 and B8 show that, among experienced workers, matched samples have slightly higher starting wages, more education, and lower age than unmatched samples. Among first-time workers, matched samples exhibit lower wages than unmatched samples. In Appendix C, we estimate a robustness check with significantly looser matching criteria that retains more of the full population and find similar results – strengthening external validity. To assess pre- and post-match balance, Tables B7 and B8 also report mean differences between full sample treated and control groups and matched treated and control groups for each cohort. The matching procedure substantially reduces differences in pre-treatment wages between treated and control groups. We include CEM matching weights in all regression estimations to further improve balance between treated and control units. As a final balance check, we plot the distribution

[11] The number of experienced workers hired into oil ranges from 15,347 in 2006 to 43,659 in 2014. In our preferred specification, between 10-26% of treated workers match with controls and are retained in sample for estimation. The number of workers obtaining their first formal job in oil ranges from 72,582 in 2006 to 84,554 in 2014, with between 25-51% matched. In general, matched sub-samples tend to exhibit slightly higher average wages and education levels relative to full unmatched samples, since individuals with higher education and wages are more likely to retain formal employment across the retrospective window and survive the matching procedure.

of pre-treatment annual formal earnings for unmatched and matched samples from each cohort in Appendix Figure A6. It is clear from this figure that the matching procedure improves balance in pre-treatment earnings along the entire distribution. Kolmogorov-Smirnov tests show that unmatched treated and control group distributions are statistically distinguishable, while matched treated and control group distributions are statistically *in*distinguishable.

4.3 Dynamic Difference-in-Differences

The next step in our approach to minimizing omitted variable and selection bias is estimation of event study specifications on the matched sub-sample. Specifically, we estimate dynamic effects of being hired into oil by comparing outcomes (e.g., hourly wages, months employed per year, annual formal earnings) for experienced or new workers hired into an oil-linked establishment in a particular year t with outcomes for closely matched workers hired into other sectors in year t. For worker i in cohort c in year t, let E_{ic} be the period when i is treated by entering an oil-linked sector. Then let $K_{ict} = t - E_{ic}$ be the number of years before or after this event. We regress individual-level outcome Y_{ict} on $\mathbb{1}(K_{ict} = k)$ relative year indicators. We include individual and year fixed effects, δi and λ_t, cluster standard errors at the individual level, and weigh observations by the CEM matching weight:

$$Y_{it} = \delta_i + \lambda_t + \sum_{k \neq -1} [\mathbb{1}(K_{it} = k)]\beta_k + \epsilon_{it} \tag{1}$$

We estimate this specification separately for the cohorts 2006, 2008, 2010, 2012, and 2014 (thus omitting the c subscript) to assess how timing of entry relative to the boom and bust cycle affects outcomes. This strategy avoids common pitfalls in event studies with staggered treatment timing, where recent studies have shown that two-way fixed effects specifications may produce biased estimates (Goodman-Bacon, 2021; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2020). Our specifications reduce these concerns by focusing on a series of single-event studies with not-yet-treated controls.[12]

To explore heterogeneity across education level, we re-estimate event studies separately for low, medium, and high education workers (defined as workers with less than high school, high school complete, and more than high school, respectively). For outcomes that apply only to employed workers (i.e., hourly wage or occupation), we drop unemployed worker-year observations from the dataset prior to estimation. For outcomes where post-hire unemployment is itself an outcome of interest (i.e., annual formal earnings and months employed per year), we preserve the balanced sample.[13]

5 Results

We first present results on wages, employment, and earnings of workers who enter oil-linked establishments as experienced hires. We then discuss results for first-time hires. For both groups, we show results for cohorts of workers hired in 2006, 2008, 2010, 2012, and 2014, relative to matched workers hired into other sectors in those same years.

5.1 Experienced Hires

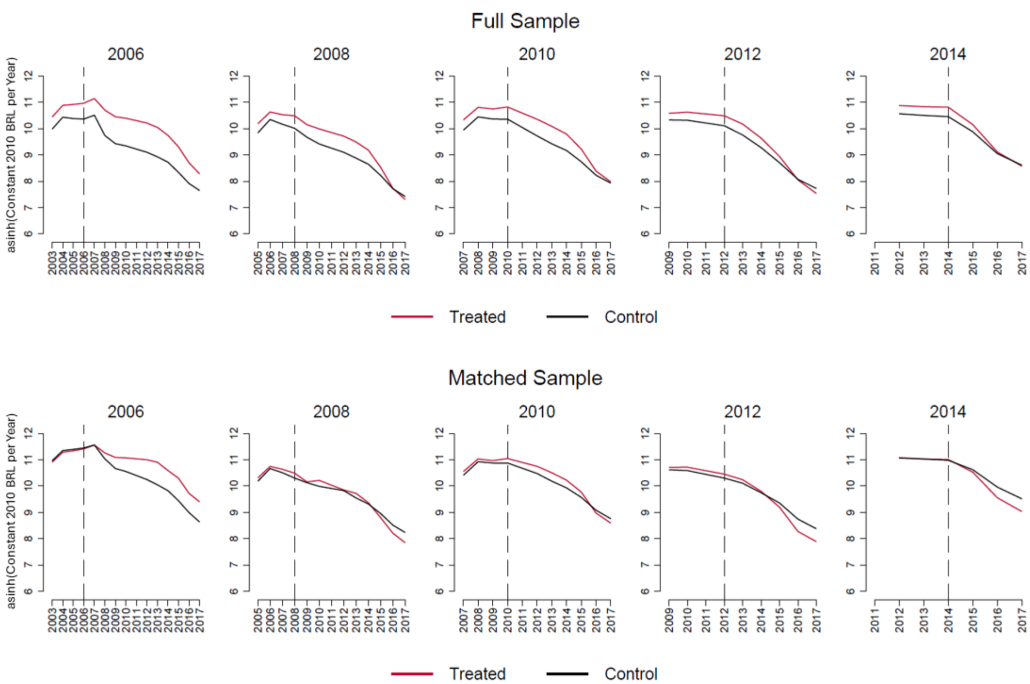
As motivating evidence, we plot mean annual formal earnings for "treated" experienced workers who switched into oil and "control" workers who switched into any other sector in Figure 3. The top row of sub-figures in Figure 3 shows mean earnings

[12]First-time hires are not treated prior to hire by definition. Among experienced hires, our retrospective matching procedure restricts the sample to individuals who have not previously changed jobs for two-years prior to period t. We place no restrictions on post-treatment employment outcomes, which means control workers could potentially be hired into oil after the event date. We thus refer to these matched counterfactual workers as not-yet-treated controls. To address potential bias from heterogeneous treatment effects across groups, we re-estimate event studies for experienced hires using Callaway and Sant'Anna (2021)'s *csdid* estimator as a robustness check, and find that results are nearly identical.

[13]Following Chen and Roth (2024), estimating separate effects at the extensive (employment) margin and intensive (wage) margin avoids problems associated with transformation of zero-value outcomes.

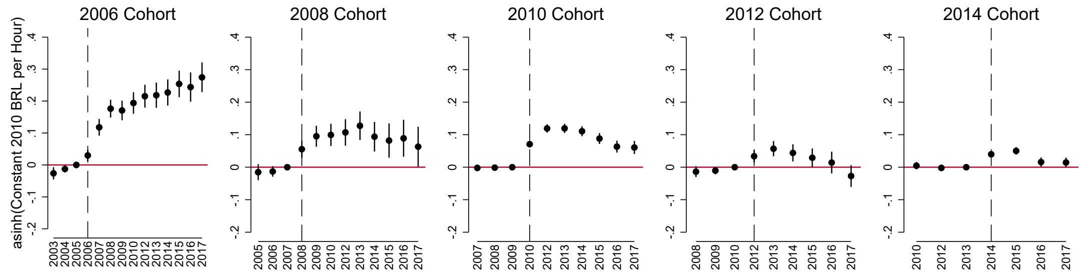
for workers in the full unmatched sample. The bottom row of sub-figures shows mean earnings after implementation of the coarsened exact matching procedure. We note that the matching procedure reduces pre-treatment differences between treated and control groups within each cohort. Further, we note that early-boom entrants into oil in 2006 and mid-boom entrants in 2010 earn more than workers in other sectors after their switch, and that the 2006 entrants retain this earnings advantage through the bust period while 2010 entrants see earnings fall below the control group during the bust. In contrast, workers who entered closer to bust periods in 2008, 2012, and 2014 experience reduced post-switch earnings relative to controls, despite similar pre-treatment earnings trajectories.

Figure 3: Mean Annual Formal Earnings for Treated and Control Groups



Note: Figure reports mean annual formal wages transformed using the inverse hyperbolic sine function for each study cohort from three years prior to workers sector switch until 2017. Treated workers are those who voluntarily leave their prior establishment and are hired into an oil-linked establishment within four months. Control workers voluntarily leave their prior establishment and are hired into a non-oil sector in the same year. The top row of sub-figures shows results for full, unmatched samples. The bottom row of sub-figures shows results for sub-samples of treated and control workers who survive the coarsened exact matching procedure described in section 4.

Figure 4: Hourly Wages After Hire into Oil-Linked Sector



Note: Event studies regress hourly wages on year indicators centered around year of hire into a oil-linked establishment ($t-1$ omitted), relative to being hired into an non-oil establishment. Wages are deflated to constant 2018 BRL and transformed using the inverse hyperbolic sine function. Standard errors are clustered at the individual level, individual and year fixed effects are included, and CEM matching weights are applied. This specification keeps only employed individuals. Sample: experienced hires. Workers match on wage and age bins, education, sex, race, occupation category, and establishment during a two-year window prior to being hired, as well as destination municipality. Raw data for 2011 is incomplete and thus omitted. Corresponding regressions are reported in Appendix Table B9.

Informed by this descriptive evidence, we next turn to event study estimation. To assess effects on *wages* (Figure 4), we limit the sample to employed workers. In the year of being hired into a new job, workers who enter an oil-linked establishment experience significant positive effects on wages across all cohorts, ranging from a 3% premium for the 2006 cohort to 7% for the 2010 cohort.[14] We find little evidence for diverging trends prior to being hired.[15] For the 2006 cohort, we observe a growing hourly wage premium for workers hired into oil-linked firms, which rises to +32% by 2017 and appears unaffected by the oil bust. In contrast, subsequent cohorts do not experience the same persistent wage growth after their entry into oil. Wage premiums for later cohorts grow to approximately +13% by 2013, then turn downwards (but remain non-negative) with the onset of the bust in 2014.[16]

[14] Semi-elasticities may be interpreted as the percentage change in wages upon switching from control to treated. For instance, $100 \times (e^{(0.03)} - 1) = 3.05\%$ for the 2006 cohort; $100 \times (e^{(0.071)} - 1) = 7.36\%$ for the 2010 cohort.

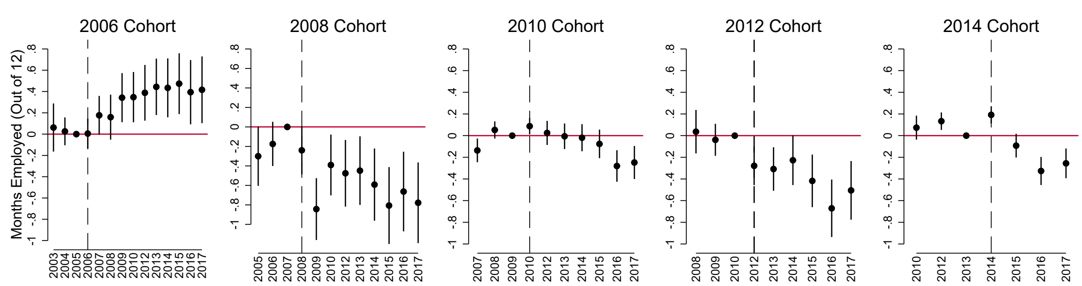
[15] Only the $t-3$ period coefficient of -0.026 for the full 2006 cohort is significant, but it is an order of magnitude smaller than the treatment effect. Adjusting for this slight upward pre-trend would imply that post-switch wage premiums for workers in the 2006 cohort are roughly in line with premiums observed for the 2008 and 2010 cohorts.

[16] Effects for oil-linked workers are not driven by confounding developments (e.g., booms, busts, or catch-up) in other sectors. As shown in Appendix Figure A3, employment in other sectors co-moves with oil – growing between 2006 and 2013 and declining thereafter – but to a much lesser magnitude. Consequently, effect estimates for oil-linked workers may be interpreted as a lower bound.

We next analyze the extensive margin of *employment*, retaining all matched workers in the sample and computing an outcome equal to the number of months in a year where a worker holds a formal job. Results, reported in Figure 5, contrast sharply with the positive wage premiums observed in Figure 4: being hired into oil has a significant negative effect on subsequent formal employment for all cohorts hired after 2006. Again the 2006 cohort stands out, being employed for 52% *more* months than matched workers by 2017. Subsequent cohorts experience significantly negative employment outcomes, with negative effects on months employed (in 2017) of -54% for the 2008 cohort, -22% for the 2010 cohort, -40% for the 2012 cohort, and -23% for the 2014 cohort.

The employment experience of the 2008 cohort in Figure 5 is noteworthy as it reveals persistent negative effects of bad entry timing. Workers hired into oil in 2008 entered just as the Global Financial Crisis provoked a brief but deep crash in oil prices. This crisis did not affect the already-established 2006 cohort, but led to significant job-loss among the new 2008 cohort – who are employed for 57% fewer months in 2009 relative to matched workers in other sectors.

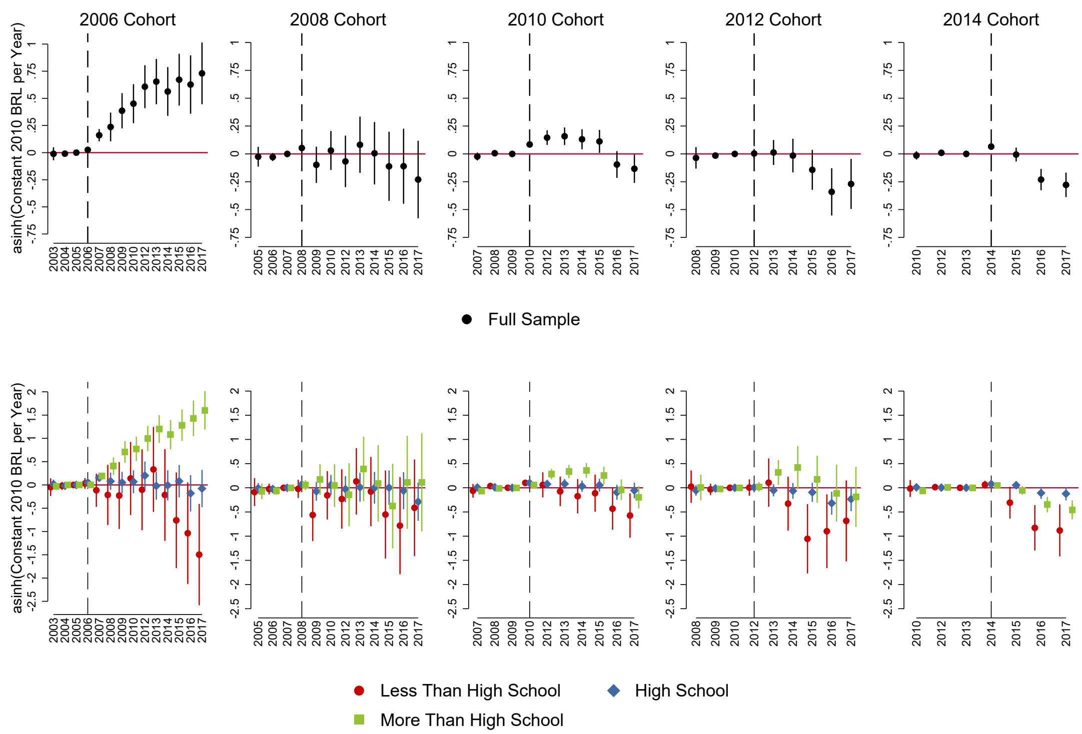
Figure 5: Months Employed Per Year After Hire into Oil-Linked Sector



Note: See also Figure 4. Months employed ranges from 0-12. This specification retains all treated individuals and matched controls in sample, with a value of zero months employed ascribed to workers who do not appear in RAIS during a given year. Raw data for 2011 is incomplete and thus omitted. Corresponding regressions are reported in Appendix Table B10.

We capture the combined effect of wages and employment in *annual formal earnings*, which is summed across formal jobs and imputed as zero when a worker does not appear in RAIS in a given year. As shown in the top panel of Figure 6, annual formal earnings for the 2006 cohort of experienced hires grow dynamically through 2017, despite the 2008-9 and 2014 oil busts. Later cohorts experience negative earnings effects after the onset of the 2014 bust.

Figure 6: Annual Earnings After Hire into Oil-Linked Sector



Note: See also note for Figure 4. Annual earnings refers to total earnings across all formal jobs. Earnings are transformed using the inverse hyperbolic sine function and deflated to constant 2018 BRL. This specification keeps all matched workers in a strongly balanced panel. In periods where individuals do not appear in RAIS, they are ascribed a value of zero formal earnings. Raw data for 2011 is incomplete and thus omitted. Corresponding tables are reported in Appendix Tables B11-B14.

Heterogeneity by education levels. The bottom panel of Figure 6 reports formal earnings effects disaggregated by workers' education levels, revealing substantial heterogeneity in the experience of workers with different educational attainment. The aggregate earnings gains of the 2006 cohort are entirely captured by high-education workers, who earn 117% more than matched controls in 2010 and 397% more in 2017.

Inequality in outcomes with later cohorts and with less-educated workers is stark: low-education workers hired into oil in 2006 never experience positive earnings effects during boom periods and experience significant negative effects on earnings after 2013 (-78% by 2017). Later cohorts experience at best small temporary gains, which turn negative as the 2014 bust sets in. Low-education workers hired in 2014 earn 59% less than matched controls by 2017.

In Appendix Figures A7 and A8 we also present results for wages and employment by education level. While wage effects in general do not vary by education level, dynamic growth in wage premiums for the 2006 cohort is concentrated among high-education workers, whose wages are 48% higher than those of matched controls in other sectors by 2017. Negative employment effects of being hired into oil are worst for low-education workers, who are especially likely to lose their jobs during busts.

We conclude that, among experienced hires, only highly educated early entrants persistently gain from and throughout the oil-linked boom and bust. All other workers at best earn more temporarily and are eventually left worse off than matched workers who joined other sectors. Many later entrants appear unable to find employment after their jobs disappear during the oil bust, reminiscent of the experience of US workers displaced by competition from China (e.g., Autor et al., 2014).

5.2 First-Time Hires

The experienced hires analyzed above already had jobs and predetermined skills before entering oil. However, the boom may also trigger an endogenous response from students, who could choose degree programs to acquire skills relevant to the booming sector. Thus, we next examine entry into oil for first-time hires, who are under 30 when they start their first formal job. We report results for first-time hires in Appendix A.3.

First-time hires into oil prior to 2014 appear to earn positive wage premiums relative to matched controls, but the magnitude of these premiums is less than half that for experienced hires. Turning to employment and annual earnings, we find that entry into oil leaves first-time hires no better off or significantly worse off than matched controls. Moreover, we no longer find that high-education workers earn more during boom years, suggesting that firms do not favor recently educated workers over older, experienced workers. Combined with the results for experienced hires, this suggests that on-the-job knowledge accumulation, rather than formal training, may account for gains enjoyed by experienced early entrants.

Thus, among workers who obtained their first formal job in an oil-linked sector, we find that exposure to oil volatility is associated with stranded careers. Among these workers, high-education early entrants do not exhibit the dynamic earnings growth enjoyed by experienced high-education early hires, suggesting a strong and persistent labor market premium for *experienced* skilled workers at the beginning of the boom.

5.3 Robustness Checks

In Appendix C we show that these results are robust to a number of checks. Workers employed in directly-linked sectors (e.g., petroleum extraction and support activities) show larger effects, as do workers near hubs of offshore oil activity (within 100 kilometers of a shipyard) (PortalNaval, 2020). Our results are not driven by workers selecting into public employment (including Petrobras, Brazil's national oil company) and are robust to using sub-samples of each cohort that share common support with the baseline 2006 cohort. We further reject the possibility that more productive workers within origin establishments may have gone into oil while less productive workers entered other sectors, or that a disproportionate numbers of workers with former experience in oil were "rehired" for the upcoming boom. Finally, we show robustness to implementing the Callaway and Sant'Anna (2021) estimator.

6 Potential Mechanisms

Why do highly-educated experienced hires in 2006 capture such large shares of overall earnings from the boom, and weather busts so well? In robustness checks (Appendix C), we showed this result is not sensitive to the matching specification, definition of treatment, or estimator. Likewise, it is not driven by a public employment effect, nor by changes in cohort composition over time. In this section, we highlight two potential mechanisms that could underlie the success of high-education early entrants.[17]

6.1 Entry and Stability in Professional Roles

We first assess whether high-education early entrants avoid negative shocks by (i) retaining jobs and occupations at the firms that originally hired them, or by (ii) possessing transferable skills that allow them to "jump ship" to other sectors during downturns. As reported in Appendix Figures A12 and A13, high-education hires in 2006 are significantly less likely to switch away from the occupation and establishment they were originally hired into.[18] Earnings premiums for high-education early entrants thus appear related to their ability to stay in the same job and occupation.

Why do firms retain these workers during downturns? One possibility is that seniority-biased labor regulations bind, creating a first-in, last-out dynamic. Alternatively, high-education early entrants may accumulate valuable knowledge and skills on the job, as in Gathmann and Schönberg (2010) and Burdett et al. (2020). If labor regulations drive outcomes, we would expect early *low-education* entrants to weather the post-2013 bust better than later low-education entrants due to their seniority.

[17] The analysis of potential mechanisms is descriptive, rather than causal, because we do not observe exogenous variation in the moderating variables. Regarding high-education early entrants' disproportionate employment in professional roles (Section 6.1), we cannot rule out that other factors correlated with education cause the different treatment effect. Regarding the boom in oil-linked higher education following the oil boom (Section 6.2), we are unable to directly tie oil-linked graduates to firms or labor market outcomes.

[18] In Appendix Figures A17 and A18, we report results from analogous specifications for first-time hires. Results suggest that high-education first-time hires into oil – in contrast to experienced workers – are more likely to switch occupations and establishments after their initial hire. This result is consistent with findings for Canada in Oreopoulos et al. (2012).

As shown in Figure 5, however, low-education early entrants lose jobs to an equal or greater extent than later entrants, suggesting seniority does not protect them.

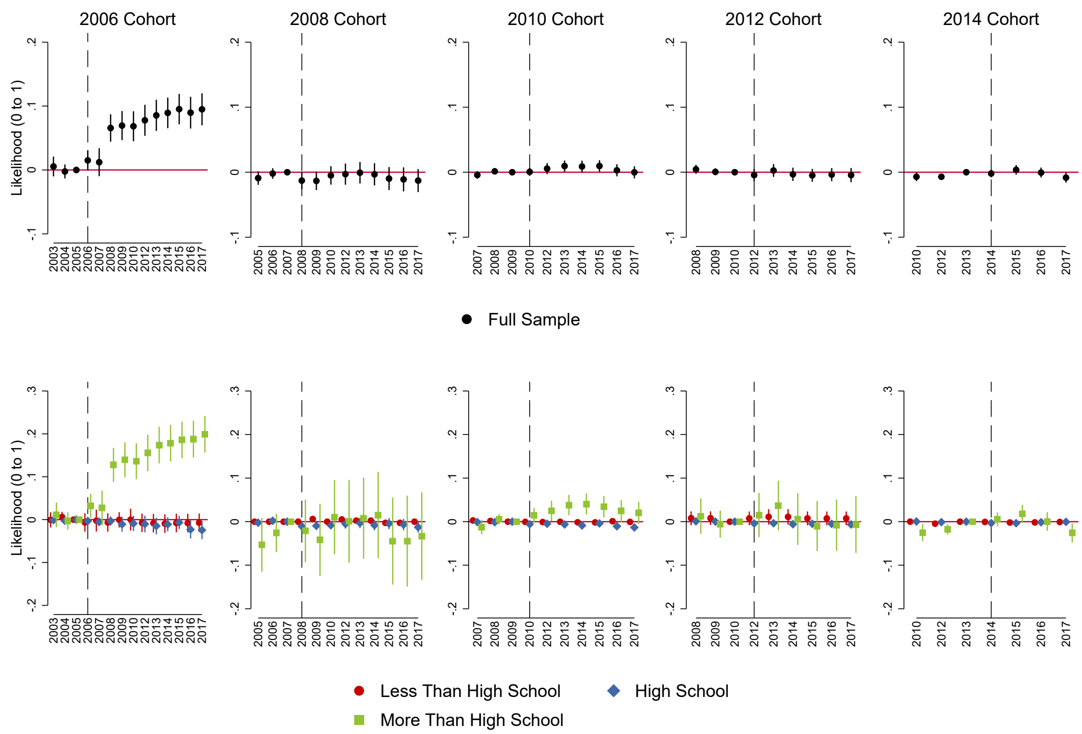
To assess on-the-job knowledge accumulation, we regress an indicator for holding a professional role (e.g., "researcher", "engineer", "analyst") on relative time indicators around being hired into oil (Figure 7). Results show that experienced workers hired during the "early boom" in 2006 (and to a lesser extent during the "mid-boom" in 2010) are significantly more likely to hold a knowledge-intensive professional role in subsequent years, with this effect driven by high-education workers.[19]

Taken together, these findings suggest that high-education early entrants with prior formal labor market experience disproportionately entered professional roles and retained these positions across the boom and bust cycle, enabling on-the-job accumulation of skills and experience that makes them sufficiently valuable to firms to retain them through downturns. In contrast, low-education hires do not hold knowledge-intensive roles that allow them to accumulate on-the-job skills, and are thus more likely to lose their jobs during busts.[20] This dynamic corroborates Modestino et al. (2016) and Modestino et al. (2020)'s documentation of "downskilling" during booms and "upskilling" during recessions.

[19]In Appendix Figure A14, we estimate analogous specifications for managerial occupations (e.g., "leader," "director," or "manager") and find experienced workers hired into oil in 2006 (and to a lesser extent 2010) are significantly *less* likely to hold managerial roles, with this effect again driven by high-education workers. This finding lends evidence to the interpretation that large returns for high-education early entrants are not the result of pure rent-capture (e.g., workers entering management roles that allow them to set their own compensation or protect themselves during downturns).

[20]Appendix Figures A15 and A16 report likelihoods of professional and managerial employment for inexperienced first-time hires and show null effects. This finding reveals that first-time hires into oil – even those with higher education – struggled to enter professional roles, which are precisely the occupations that confer rent sharing and job security on *experienced* entrants. This result helps explain the discrepancy in labor market outcomes between experienced and new entrants into oil.

Figure 7: Probability of Occupying Professional Role
after Hire into Oil-Linked Sector (Experienced Hires)



Notes: Professional roles are defined as CBO occupation codes beginning with 2, including "researcher", "scientist", "engineer", and "analyst". Outcome is a binary indicator for "professional role," which is regressed on individual and year fixed effects and relative time indicators around year of being hired into oil. The sample is limited to employed workers. Raw data for 2011 is incomplete and thus omitted.

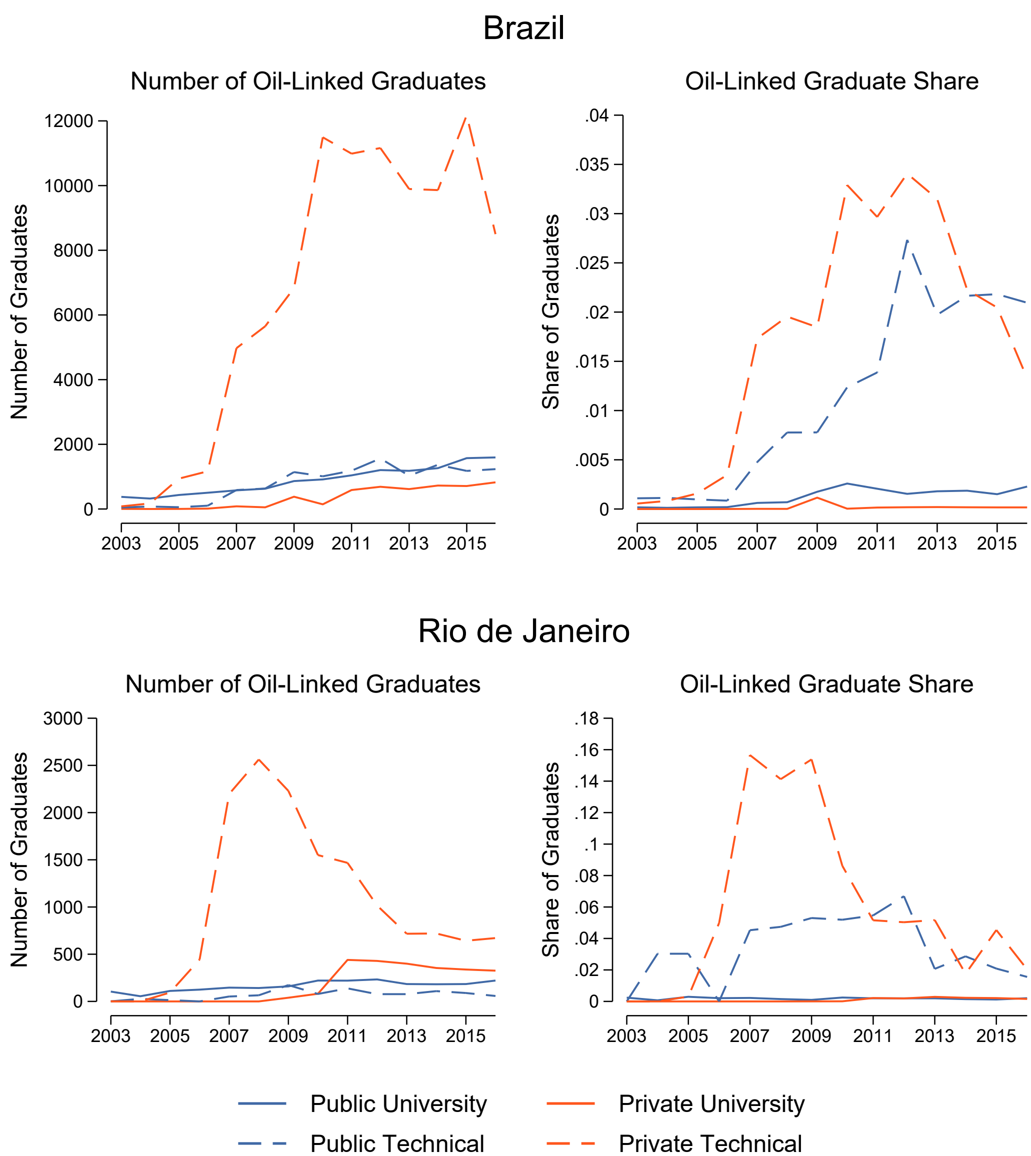
6.2 Sector-Specific Higher Education Response

In this section, we show that a plausible contributor to the decline in earnings premiums for later entrants into oil is an endogenous human capital investment response to the oil boom – driven by both the demand side (students) and supply side (degree-programs) – which creates a glut of skilled oil workers. This may affect first-time hires more than experienced hires, as the latter's prior experience places them in a segmented labor market relative to new entrants.

To assess these dynamics in the context of Brazil's oil boom and bust, we draw on data from the country's Higher Education Census, which reports the number of graduates at the institution-degree-year level for the universe of higher education institutions between 2003-2016. Using 6-digit degree-area (i.e., major) codes, we classify 24 out of 1,104 total degree programs as "oil-linked" based on contextual knowledge, and sum the number of graduates from oil-linked public/private and university/technical programs to the municipality-year level. We describe this process in more detail and list oil-linked degree programs in Appendix B21.

Figure 8 shows the number of graduates from oil-linked higher education programs each year between 2003-2016. The figure reports results for Brazil as a whole, and then for the state of Rio de Janeiro, where the country's oil sector is most prominent. The number and share of oil-linked graduates in Brazil increased sharply from 2006 onward, corresponding with the oil boom, and peaked around 2010-12. The increase was most dramatic in private technical-training, which increased from 82 graduates (0.1% of total graduates in this category) in 2003 to 11,493 (3.3%) in 2010 and 12,177 (2%) in 2015, before falling to 8,500 (1.3%) in 2016. Public technical graduates also grew dramatically, from 49 (0.1%) in 2003 to 1,564 (2.7%) in 2012, before declining to 1,234 (2.1%) by 2016. A clear contrast between technical and university degrees is that technical programs are sufficiently short-term for students to react to the oil bust. University programs take 4-6 years to complete, leading many university students who enrolled during boom years to graduate during unfavorable bust years. Rio de Janeiro's boom in oil-linked higher education preceded the national boom by approximately three years, likely due to stronger early-boom signals in this state.

Figure 8: Number and Share of Oil-Linked Graduates



Note: Number and share of graduates are calculated from Brazil's Higher Education Census (2003-2016). Oil-linked majors are defined in Appendix Table B21. University degrees typically take 4-6 years to complete; technical degrees typically take 1-2 years. Rio de Janeiro state is selected as an example since it is the center of Brazil's oil industry.

Growth in oil-linked graduations corresponded with expansion of oil-linked post-secondary degree programs. For Brazil as a whole, the number of oil-linked public university programs grew from 24 in 2003 to 75 in 2016. Private university programs grew from 1 in 2003 to 33 in 2016. Technical programs fluctuated even more dramatically. Private oil-linked technical programs grew from 12 in 2003 to 181 in 2012, then fell to 143 by 2016. Public technical programs grew from 7 in 2003 to 73 in 2014, then declined to 68 by 2016 (see Appendix Figure A19). Evidently, technical programs responded pro-cyclically to the oil boom and bust, while university programs continued to expand despite the 2014 downturn. In Rio de Janeiro, oil-linked private technical programs increased from 4 in 2003 to 28 in 2009, then declined to 11 by 2016. Similar trends hold in other states affected by the oil boom and bust (Appendix Figure A20).

We estimate a difference-in-differences specification to test whether oil-linked graduations were more likely to increase in municipalities near oil industry hubs (proxied by shipyards, which are supply-chain nexuses for oil inputs) during boom years. We regress outcome y_{mt} (number of graduates transformed using the inverse hyperbolic sine function, or share of STEM graduates in oil-linked majors in municipality m in year t) on a proxy for oil industry presence (municipality centroid within 50km of a shipyard), an indicator for the boom period (years 2006-2013), the interaction of these two terms, and state fixed effects, with standard errors clustered at the municipality-level:

$$y_{mt} = \beta Close_m + \gamma Boom_t + \delta(Close_m \times Boom_t) + \mu_s + \epsilon_{mt} \tag{2}$$

We report results in Table 1. The difference-in-differences interaction term of oil-proximity and oil boom period is significantly positive, indicating that oil-linked graduations increased most where the oil sector is most important (near shipyards) during the boom. Disaggregating effects across degree-program categories, we find that effects are driven by private technical training programs. The share of total STEM graduates earning oil-linked degrees is also higher during oil boom years and increases most near shipyards during the boom for technical training programs. These results provide evidence that students specialized in oil-relevant skills in response to Brazil's oil boom, increasing competition for later entrants into oil-linked sectors.

Table 1: Effects of Exposure to Oil Boom on Oil-Linked Graduates

	Number of Graduates from Oil-Linked Degree-Programs				
Variables	**Total**	**Pub. Uni.**	**Priv. Uni.**	**Pub. Tech.**	**Priv. Tech.**
<50km from Shipyard	0.382***	0.257***	0.095*	0.073	0.278***
	(0.099)	(0.063)	(0.052)	(0.048)	(0.081)
Boom Year (2006-2013)	0.197***	-0.001	0.001	0.032***	0.184***
	(0.018)	(0.008)	(0.004)	(0.009)	(0.016)
Near × Boom	0.415***	0.032	0.019	0.048	0.522***
	(0.158)	(0.095)	(0.075)	(0.072)	(0.144)
State FEs	YES	YES	YES	YES	YES
Observations	16,600	16,600	16,600	16,600	16,600
DV Mean (IHS) (Pre-Boom)	0.073	0.045	0.001	0.007	0.029
DV Mean (Pre-Boom)	0.937	0.283	0.003	0.037	0.614
R-squared	0.074	0.076	0.037	0.014	0.067
	Share of STEM Graduates in Oil-Linked Degree-Programs				
Variables	**Total**	**Pub. Uni.**	**Priv. Uni.**	**Pub. Tech.**	**Priv. Tech.**
<50km from Shipyard	-0.007	0.002**	0.000	-0.001	0.009
	(0.004)	(0.001)	(0.000)	(0.006)	(0.009)
Boom Year (2006-2013)	0.014***	0.001**	0.000	0.004***	0.027***
	(0.002)	(0.001)	(0.000)	(0.001)	(0.002)
Near × Boom	0.010	-0.001	0.000	0.008	0.065***
	(0.007)	(0.001)	(0.001)	(0.009)	(0.017)
State FEs	YES	YES	YES	YES	YES
Observations	16,600	16,600	16,600	16,600	16,600
DV Mean (Pre-Boom)	0.0076	0.0004	0.0001	0.0010	0.0009
R-squared	0.011	0.015	0.007	0.017	0.042

Note: Table reports coefficient estimates and standard errors from specifications that regress number or share of oil-linked graduates in a municipality-year pair on an indicator of that municipality's proximity to a shipyard (<50km), an indicator of whether that year falls during Brazil's oil boom period (2006-2013), a difference-in-differences type interaction of those indicators, and state fixed effects. Standard errors are clustered at the municipality level and number of graduates is transformed using inverse hyperbolic sine. Graduates are disaggregated into four categories: public university (*bacharelado* or *licenciatura* degrees from federal, state, and municipal higher education institutions); private university (*bacharelado* or *licenciatura* from private higher education institutions); public technical (*tecnólogo* degrees from federal, state, or municipal higher education institutions); and private technical (*tecnólogo* degrees from private higher education institutions). Share of graduates refers to the share of total STEM (*exatas*) graduates in that specific category who earn an oil-linked degree. Pre-boom dependent variable means refer values in 2005.

7 Conclusion

How does timing of entry relative to sector-specific expansions and contractions affect workers' careers? Using detailed employer-employee linked panel data from Brazil, we measure dynamic labor market outcomes of workers hired into oil-linked sectors at different points along a boom-bust cycle, relative to outcomes for closely matched workers hired into other sectors. Results suggest that timing of entry into a volatile sector has lasting impacts: only workers who enter at the beginning of a boom period earn substantial earnings premiums over the course of the cycle. For most later entrants, the decision to enter oil results in significant and persistent employment and earnings penalties. Sectoral volatility thus generates inequality *across* cohorts.

Further, we show that sectoral volatility also generates significant inequality *within* worker cohorts. Highly educated, experienced early entrants appear to capture almost the entirety of earnings benefits across the boom-bust cycle. These workers disproportionately transition into knowledge-intensive professional roles within oil-linked firms, enabling on-the-job skill formation that conveys job and occupation-stability even during busts. Low-education workers – occupying easy-to-replace roles with little on-the-job knowledge accumulation – constitute firms' margin of adjustment to downturns. These workers experience disproportionate job loss during busts and subsequently re-enter the formal labor market at lower rates.

Finally, we document rapid growth in graduations from oil-related higher education programs in response to the oil boom, which illustrates that, just as a boom can lead to over-investment in wells and pipelines, it can do the same for industry-specific human capital. The data patterns here suggest that surging labor market entry contributed to increased labor market competition and declining wage premiums for later entrants. Moreover, the average worker who obtains their first formal job in oil after undertaking higher education does not enter a professional occupation – i.e., the type of role that confers high earnings and job security on experienced workers. This suggests that degrees and professional experience are imperfect substitutes. Growth in sector-specific skills – which was promoted by public policies at the time – may thus have resulted in stranded human capital and a persistent mismatch of skills in the post-bust economy, as predicted in van der Ploeg and Rezai (2020).

These findings are particularly relevant for fossil fuel sectors, which directly employed 8 million workers worldwide in 2022 and supported many more upstream and downstream jobs (IEA, 2022). The International Energy Agency predicts fossil fuel employment will shrink by 5 million jobs by 2050 under a net zero scenario, and – in line with our results – Rud et al. (2022) find that job loss in the coal industry resulted in earnings and employment losses for workers. In the short run, localized fossil fuel employment booms will continue in response to major oil and gas discoveries, which have affected 39 countries since 2000 (Rystad Energy, 2022). Policymakers should thus consider both the role of oil volatility in exacerbating labor market frictions *and* the risk of stranded human capital before promoting training or labor market mobilization programs for fossil fuel industries. Policymakers may also prepare for (more foreseeable) long-term declines and (less predictable) short-term booms and busts in fossil fuel employment through training programs focused on transferable skills. While unemployment insurance may offer temporary respite for workers affected by declining sectors, more directly addressing a potential skills mismatch may help workers find new employment sooner.[21]

Our findings are also relevant for rapidly expanding clean energy sectors, which are projected to add 16 million jobs worldwide by 2050 (IEA, 2022). Early entrants may be positioned to capture most of the gains from this growth, unless rapid technological change gives later entrants the upper hand. On one hand, a surge in education programs to train renewable energy workers may deliver on the promise of high labor market returns, but on the other, endogenous entry of specialized workers bids down premiums. We hope future research will study these tradeoffs.

Furthermore, while the transition to clean energy may appear as a smooth upward trend in aggregate, it is likely to involve abrupt booms and busts in specific technology and mineral categories based on the rapid pace of technological change. For instance, the lithium industry has experienced repeated bubbles (Wang et al., 2023), and the once-booming offshore wind industry has recently faced a cost crunch and widespread project cancellations (Paulsson et al., 2023). Critical mineral sectors exhibit extreme price volatility (IEA, 2024). Energy investments respond to news shocks about policy

[21] Other policy approaches that have been implemented include early retirement options for end-of-career workers and place-based policies focused on regions with high levels of fossil fuel employment.

changes, propagating uncertainty (Noailly et al., 2024), and booms and busts are likely to be amplified by changing climate policy. For example, the US Inflation Reduction Act has precipitated a rush of investments into critical mining and green manufacturing (Temple, 2023), but this could change if a new administration changes policy. Trade restrictions and industrial policies – currently in vogue across developing and high-income countries alike – seek to benefit local workers but may lock them into sector-specific skills and job ladders within volatile industries. Based on our findings, policymakers should consider how sectoral volatility can create labor market disparities both across and within worker cohorts.

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Online Appendix

Timing is Everything: Labor Market Winners and Losers during Boom-Bust Cycles

Erik Katovich, Dominic Parker, and Steven Poelhekke

November 10, 2024

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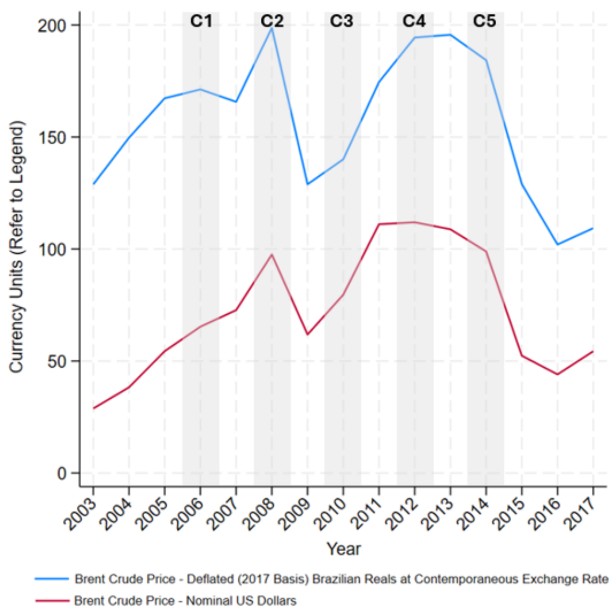
A Supplementary Figures

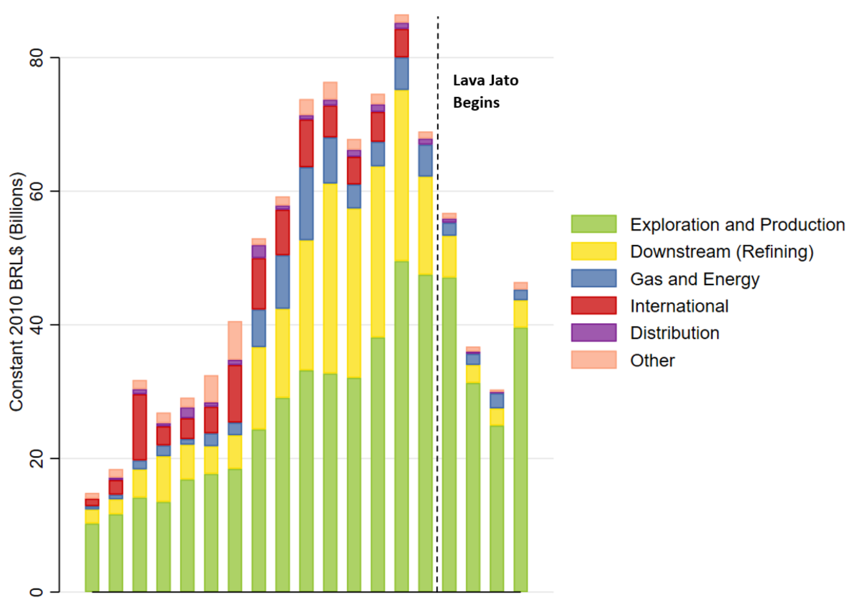
A.1 Descriptive Figures

Figure A1: Brent Crude World Oil Price in Local Real Values



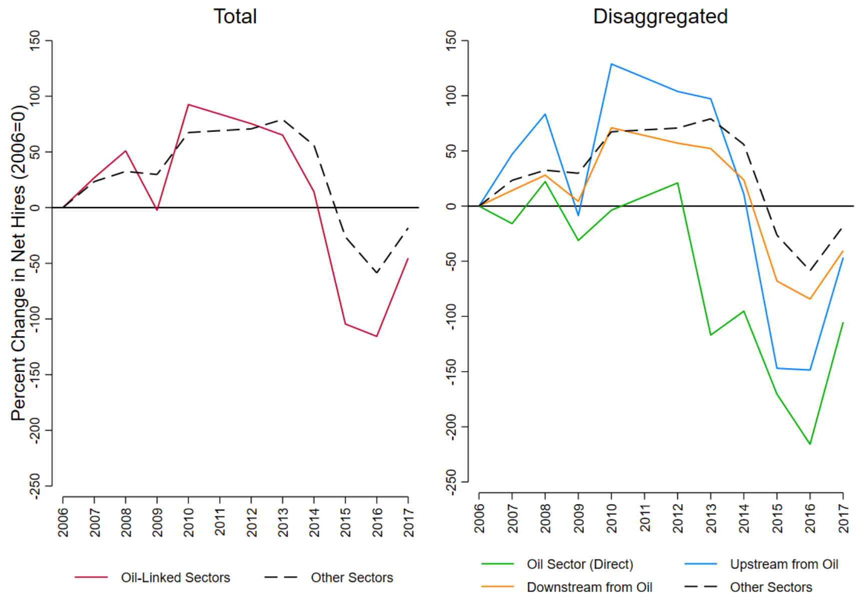
Note: Brent Crude oil prices are drawn from FRED. The USd/BRL exchange rate and INPC price deflator are from Ipeadata.

Figure A2: Petrobras: Annual Investment by Category (2000-2018)



Source: Petrobras (2020)

Figure A3: Percent Change in Net Hires (Oil-Linked and Other)



Source: RAIS (2006-2017)

Figure A4: Formal Employment in Oil-Linked Sectors Relative to Total

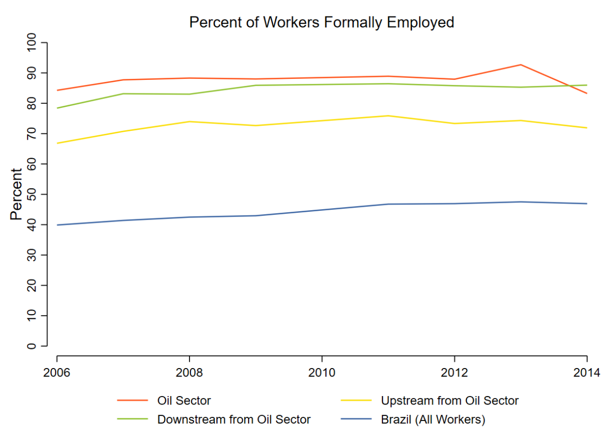
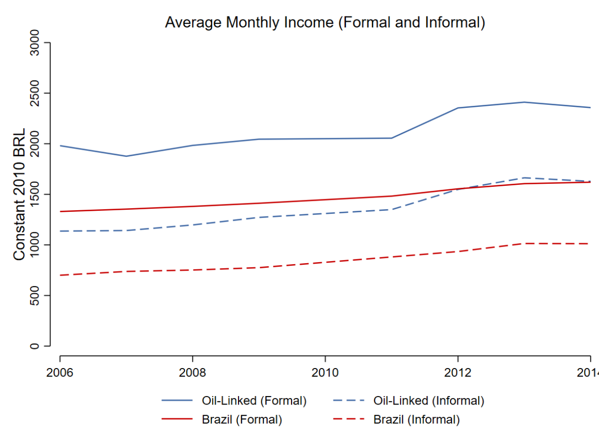
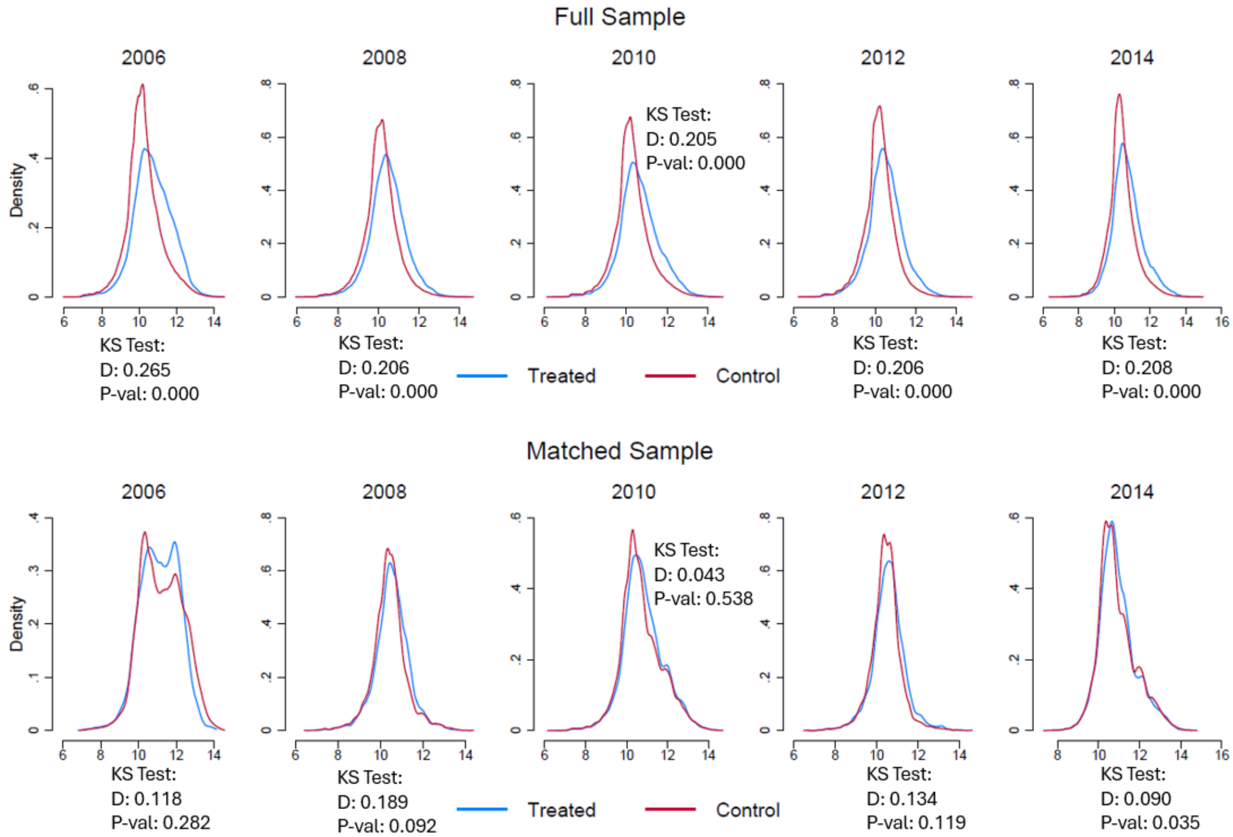


Figure A5: Average Monthly Earnings for Formal and Informal Workers



Note: Data are drawn from Brazil's *Pesquisa Nacional por Amostra de Domicílios* (PNAD), an annual nationally representative household survey. PNAD includes both formal and informally employed workers, allowing us to compute comparative statistics for formal sectors (corresponding to data available in the RAIS formal employment registry), and informal sectors (unobserved in RAIS). Figure A4 shows the percentage of workers in oil-linked sectors (direct, upstream, and downstream) with formal employment, relative to the average rate of formality for workers in Brazil as a whole. Figure A5 shows earnings for formal versus informal workers in oil-linked sectors, relative to formal and informal workers for Brazil as a whole.

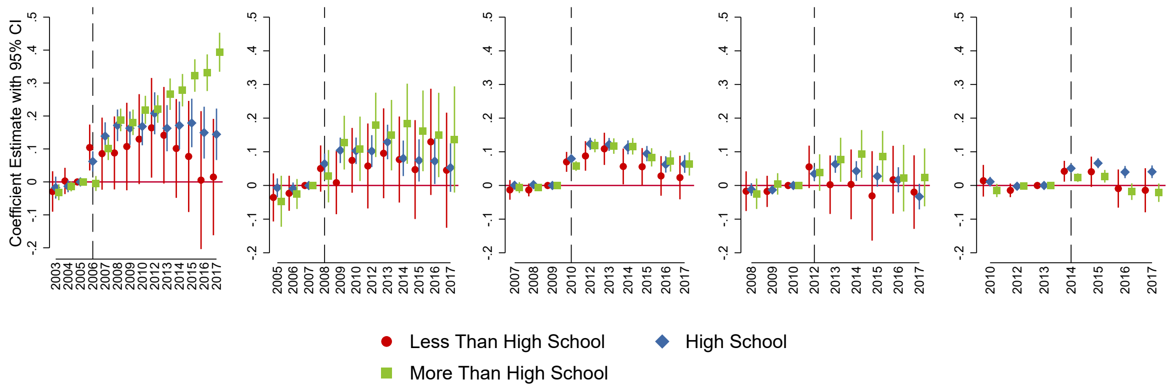
Figure A6: Distributions of asinh(Annual Formal Earnings) for Full and Matched Cohorts



Note: Sub-figures report kernel density plots of asinh(annual formal earnings) for experienced workers who switched into oil (treated) and into other sectors (control), across the three years prior to treatment. The top row of sub-figures reports distributions for the full, unmatched sample of switchers. The bottom row of sub-figures reports distributions for the CEM matched sample. D statistics and p-values from Kolmogorov-Smirnov tests of equality of distributions are reported to statistically compare distributions of the earnings outcome between treated and control workers within each cohort to assess pre and post-match balance. Kolmogorov-Smirnov tests are computed on 1% random samples from both the full and matched samples due to computational limitations.

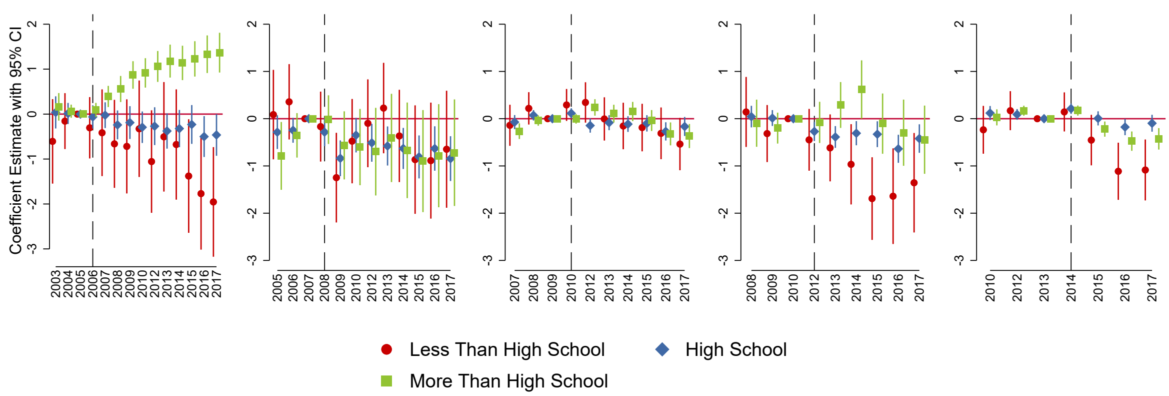
A.2 Additional Results: Experienced Hires

Figure A7: Hourly Wages After Hire into Oil-Linked Sector, by Education Level



Note: Event studies regress hourly wages on year indicators centered around year of hire into an oil-linked establishment ($t-1$ omitted), relative to being hired into an non-oil establishment. Wages are deflated to constant 2018 BRL and transformed using inverse hyperbolic sine. Standard errors are clustered at the individual level, individual and year fixed effects are included, and CEM matching weights are applied. This specification keeps only employed individuals. Sample: experienced hires. Workers match on wage and age bins, education, sex, race, occupation category, and establishment during a two-year window prior to being hired, as well as destination municipality. Corresponding regressions are reported in Appendix Table B9.

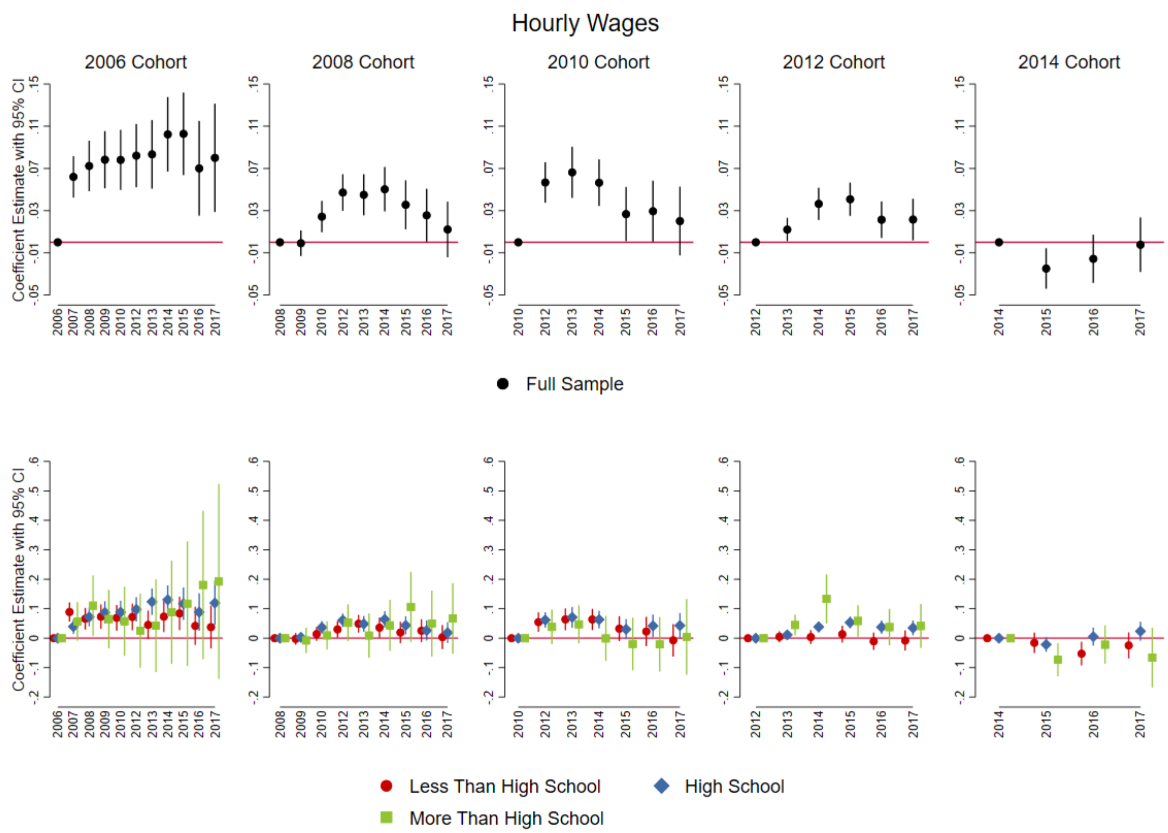
Figure A8: Months Employed Per Year After Hire into Oil-Linked Sector, by Education Level



Note: See also Figure 4. Months employed ranges from 0-12. This specification retains all treated individuals and matched counterfactuals in sample, with a value of zero months employed ascribed to workers who do not appear in RAIS during a given year. Corresponding regressions are reported in Appendix Table B10

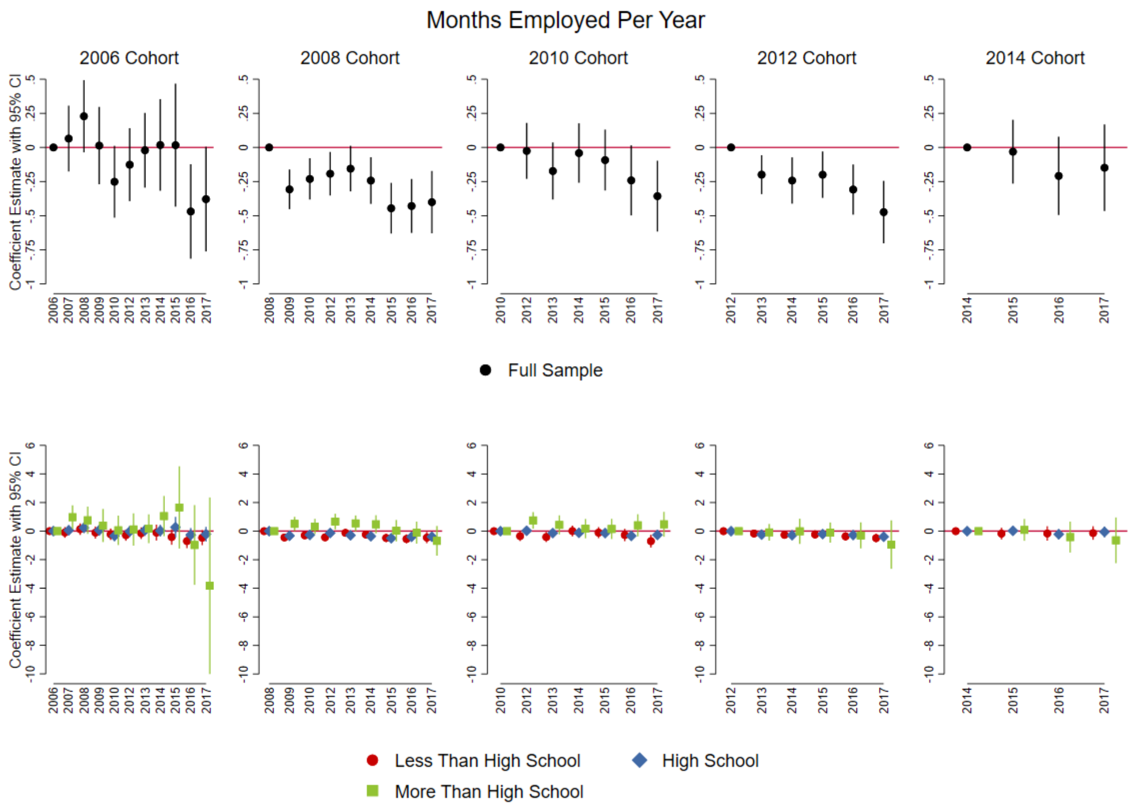
A.3 Results: First-Time Hires

Figure A9: Hourly Wages After New Hire into Oil-Linked Sector



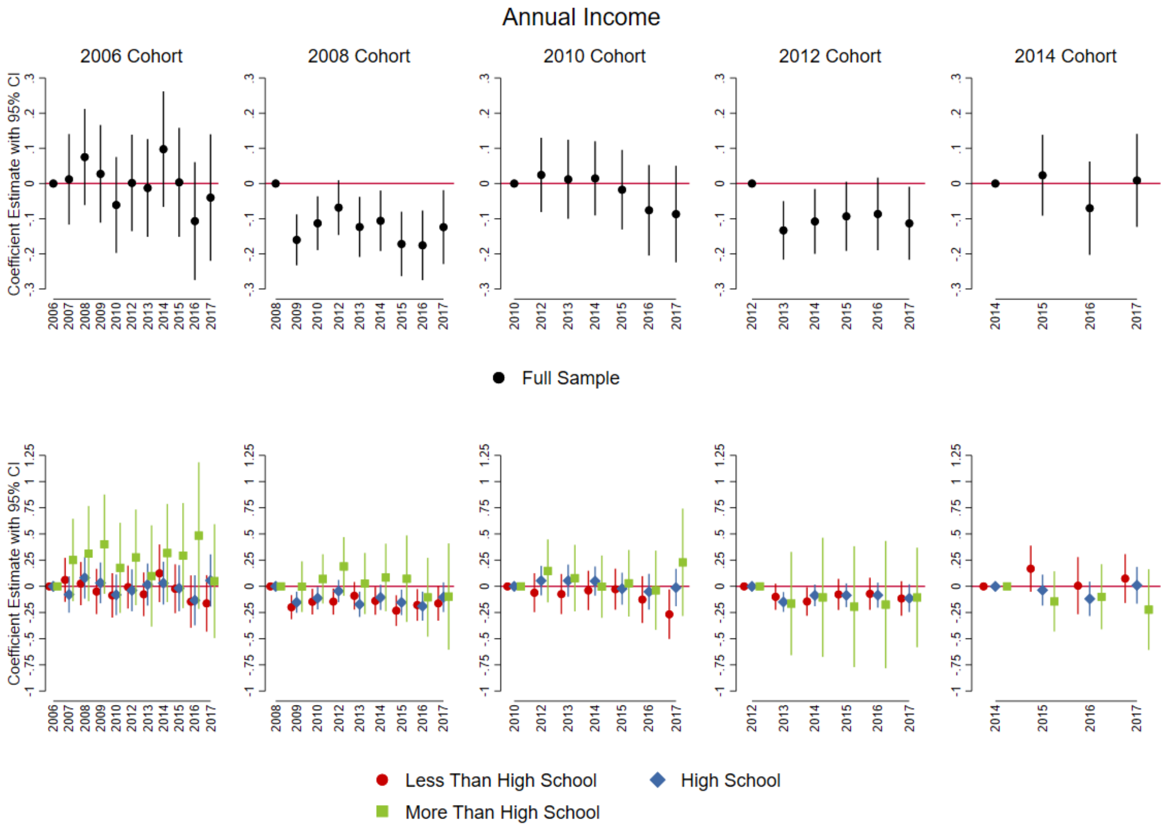
Note: Event studies regress hourly wages on relative time indicators centered around new hire into an oil-linked establishment (t omitted). Wages are deflated to constant 2018 BRL and transformed using inverse hyperbolic since. Standard errors are clustered at the individual level, and individual and year fixed effects are included. This specification keeps only employed individuals. Treated individuals (newly hired into oil-linked sector in year t) are compared to individuals newly hired into other sectors in year t who matched on age, education, sex, race, municipality, and wage and firm size bins in their first job. First-time hires are defined as workers who are hired into their firm formal job. Corresponding tables are reported in Appendix Table B15.

Figure A10: Months Employed Per Year After New Hire into Oil-Linked Sector



Note: See also note to Figure A9. Months employed ranges from a minimum of zero if the individual never appeared in formal employment registries during a year, to 12 if the individual was employed each month. This specification keeps all treated individuals and matched controls, whether formally employed or not, in a strongly balanced panel. Corresponding tables are reported in Appendix Table B16.

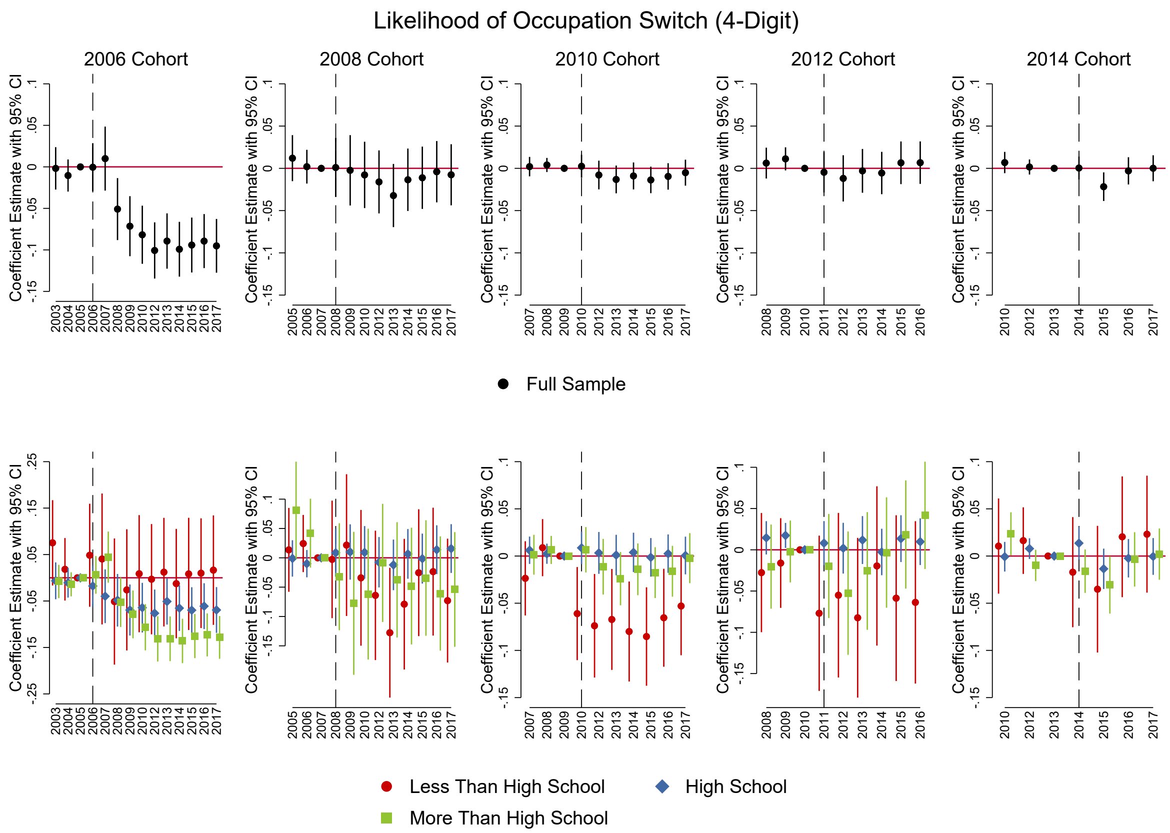
Figure A11: Annual Earnings After New Hire into Oil-Linked Sector



Note: See also note to Figure 4. Annual earnings refers to total formal earnings across all formal jobs. Earnings are transformed using the inverse hyperbolic sine transformation and deflated to constant 2018 BRL. This specification keeps all matched experienced hires, whether formally employed or not, in a strongly balanced panel. In periods where individuals do not appear in the panel, they are ascribed a value of zero formal earnings for this period. Corresponding tables are reported in Appendix Table B17-B20.

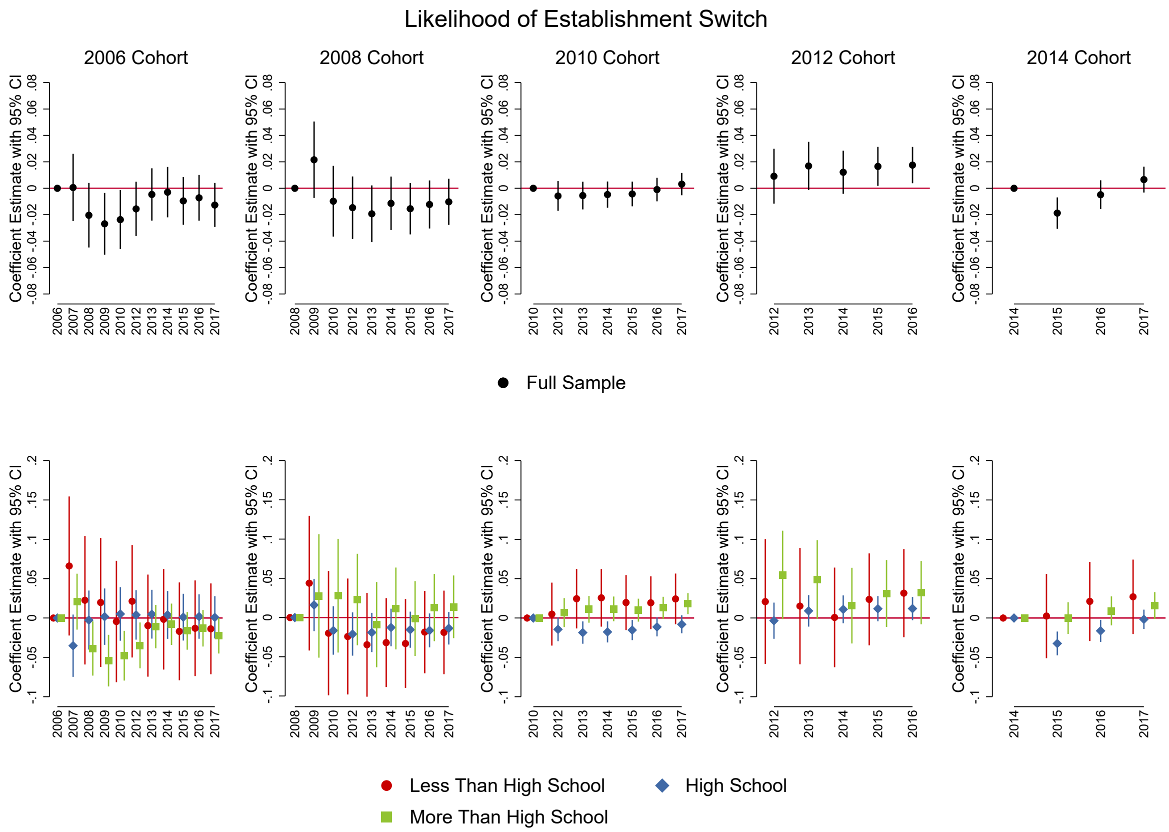
A.4 Results: Mechanisms

Figure A12: Probability of Occupation Switch after Hire into Oil-Linked Sector (Experienced Hires)



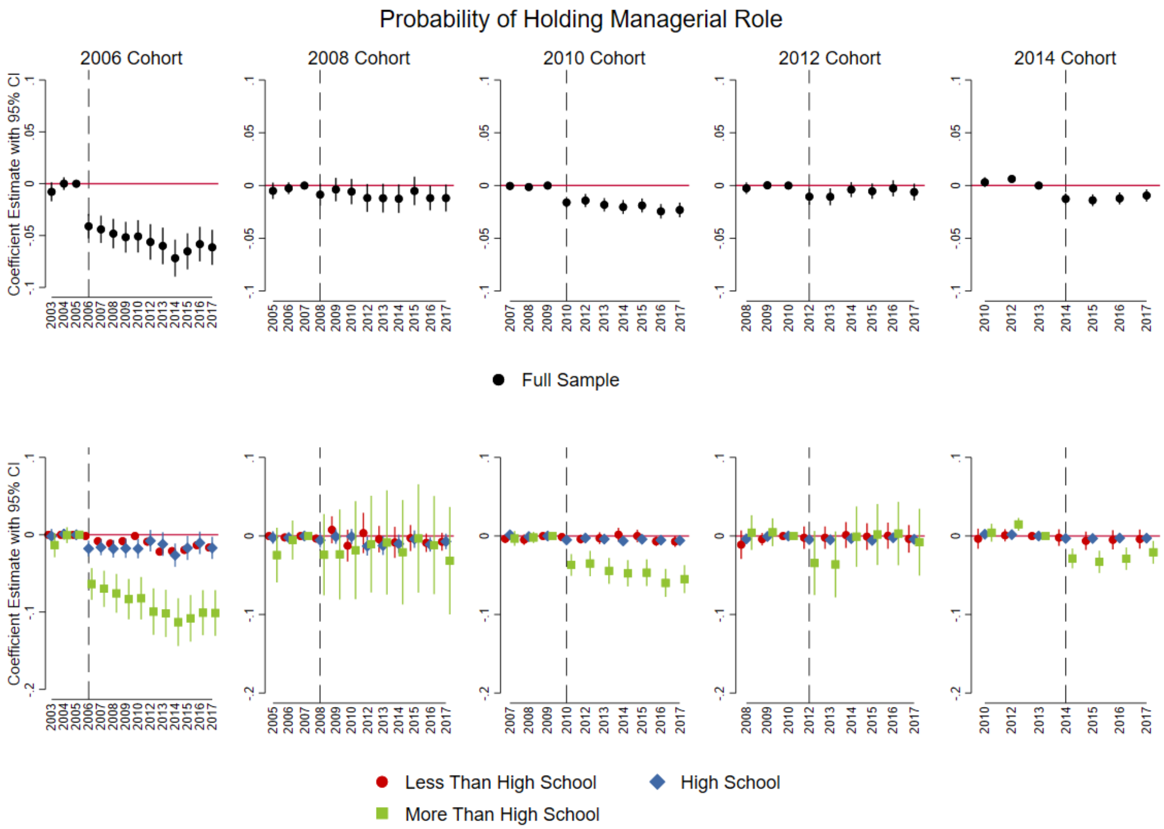
Notes: Outcome is an indicator assuming a value of zero in each period the worker holds the same 4-digit occupation code in their primary job as the one they were originally hired into, and a value of one when they hold a different occupation code. Sample is limited to employed workers.

Figure A13: Probability of Establishment Switch after Hire into Oil-Linked Sector (Experienced Hires)



Notes: Specifications are analogous to those described in Figure 7. Here, the outcome is an indicator assuming a value of zero in each period the worker holds a job in the same establishment they were originally hired into, and a value of one in each period they hold a job in a different establishment.

Figure A14: Managerial Roles after Hired into Oil-Linked Sector (Experienced Hires)



Notes: Managerial roles are defined as CBO occupations with codes beginning with 1. These roles are primarily described as "leader", "director", or "manager". Binary outcomes are regressed on individual and year fixed effects and relative time indicators around year of being hired into oil (baseline = $t-1$). Standard errors are clustered at the individual level.

Figure A15: Professional Roles after New Hire into Oil-Linked Sector

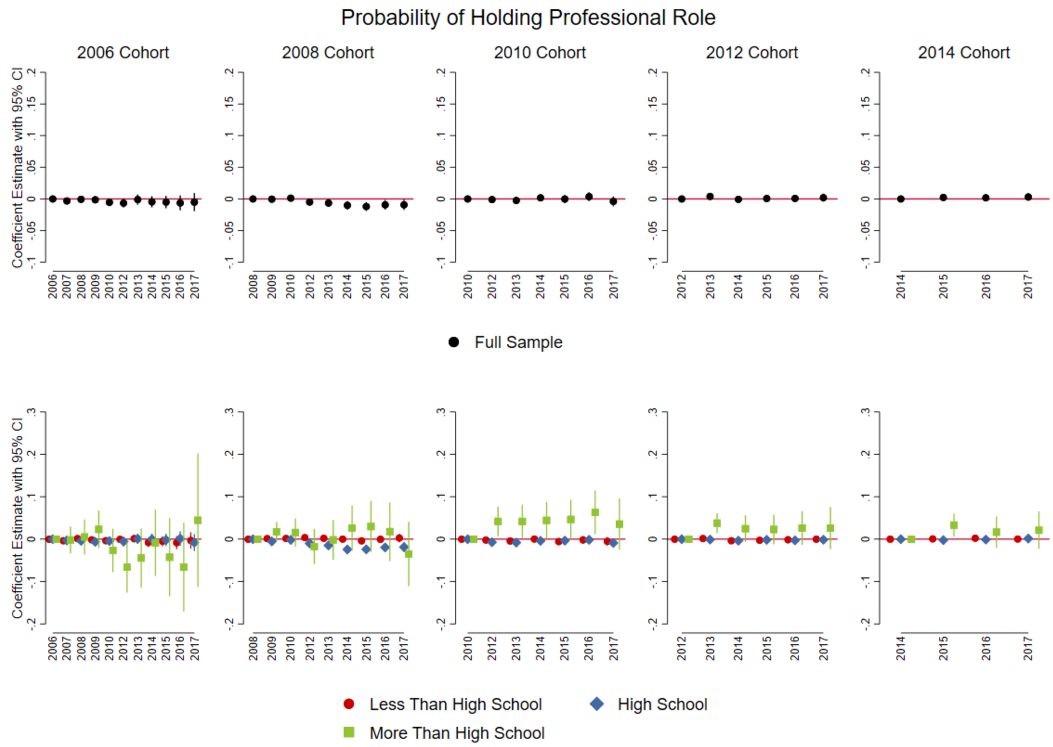


Figure A16: Managerial Roles after New Hire into Oil-Linked Sector

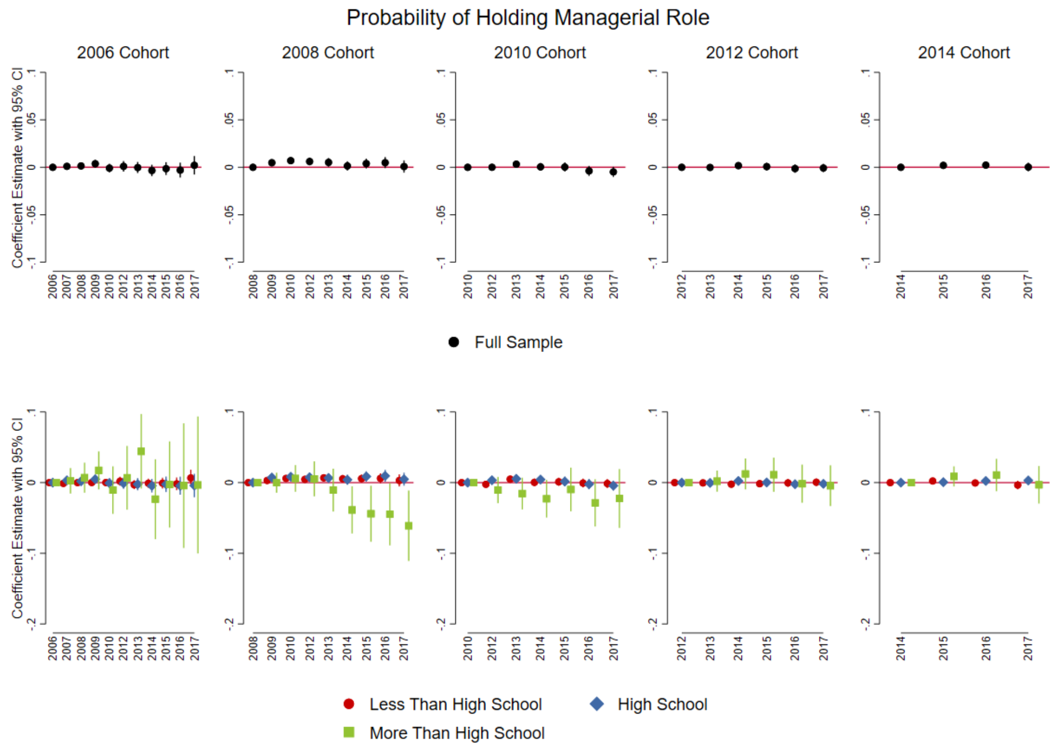


Figure A17: Occupation Switching after New Hire into Oil-Linked Sector

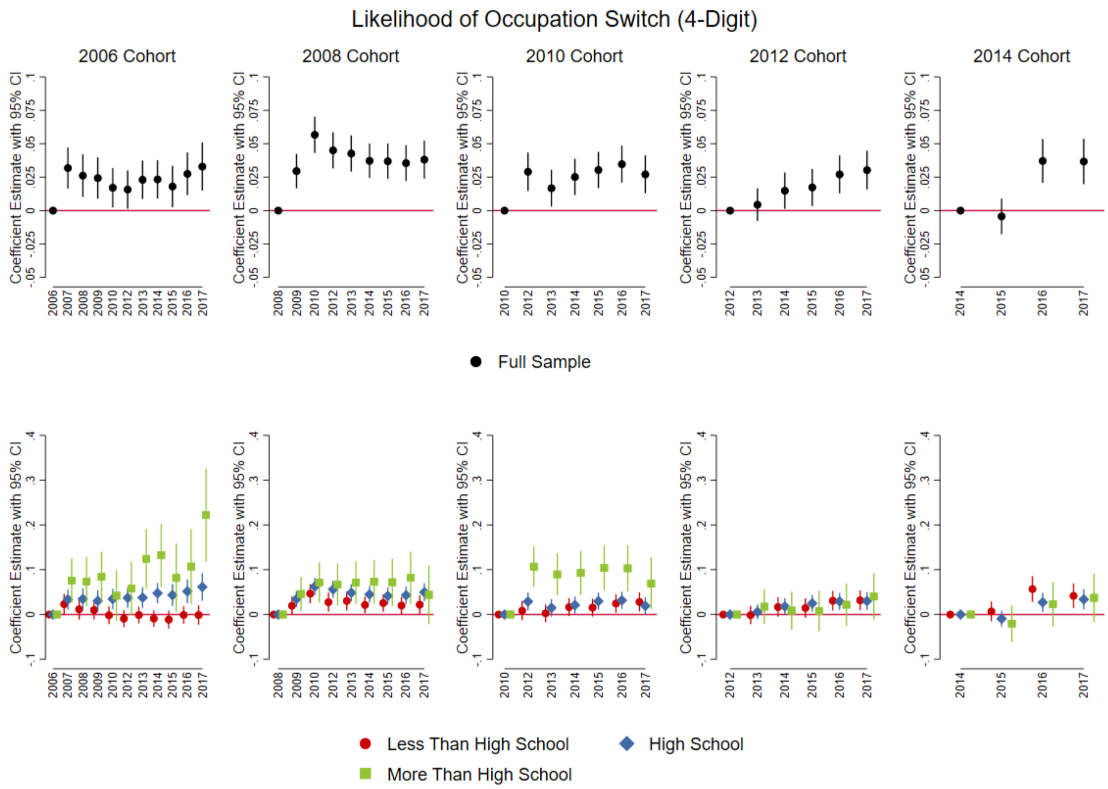
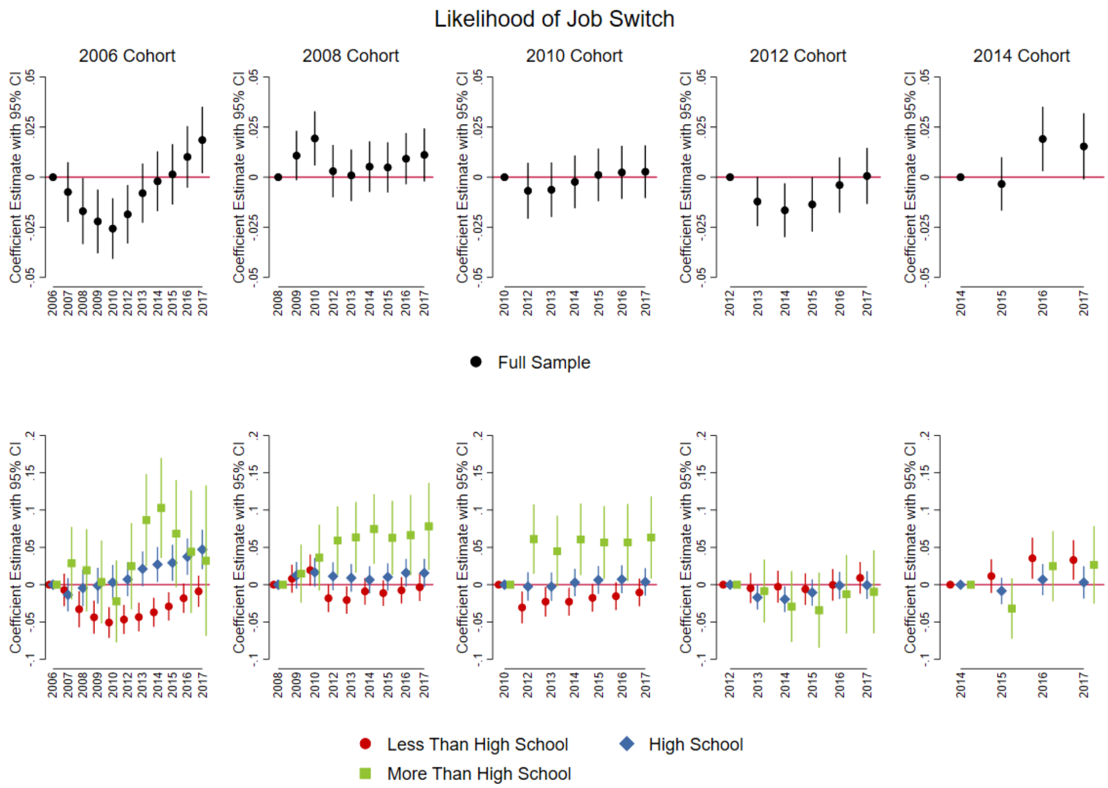


Figure A18: Establishment Switching after New Hire into Oil-Linked Sector



A.5 Oil-Linked Higher Education

Figure A19: Number of Oil-Linked Degree Programs

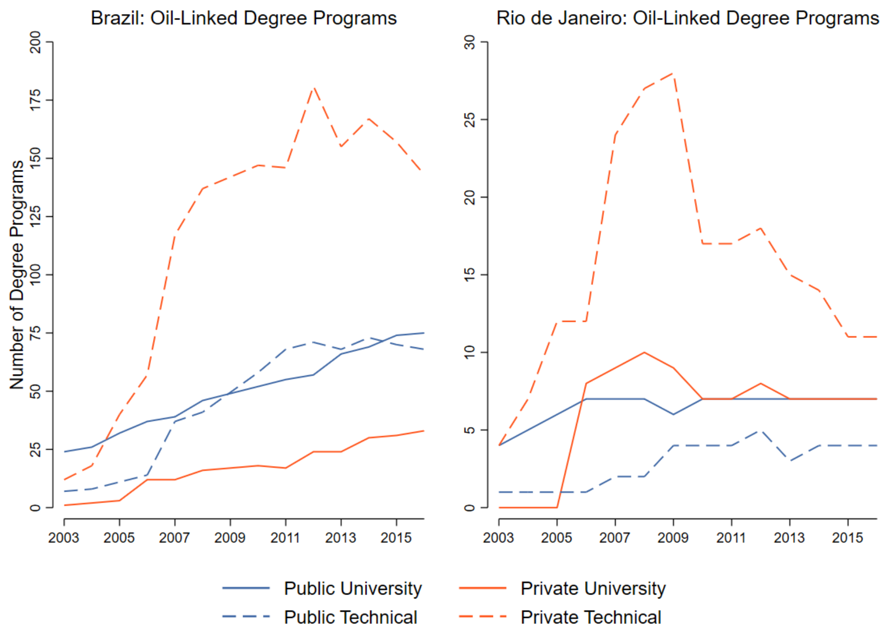
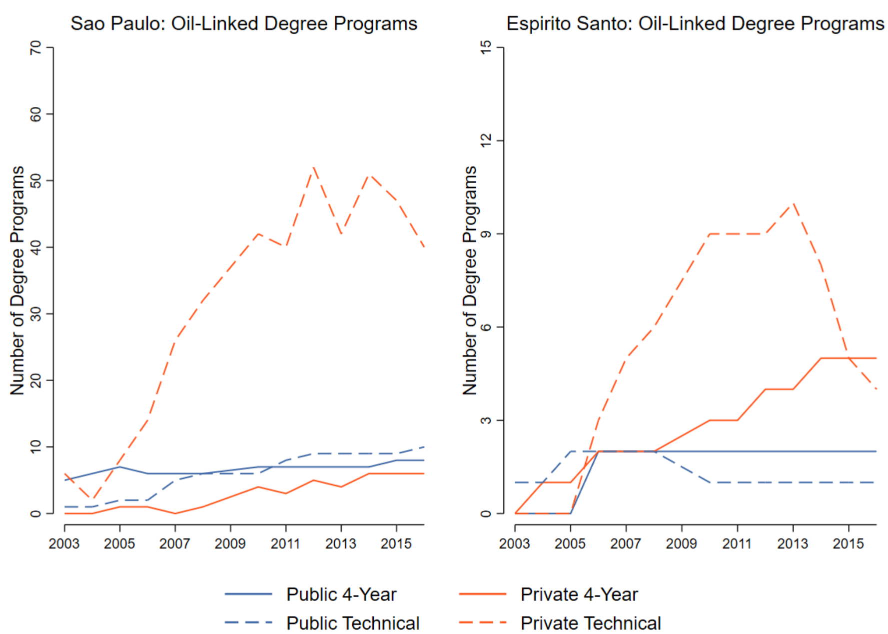


Figure A20: Oil-Linked Degree Programs (São Paulo and Espírito Santo)



B Supplementary Tables

B.1 Identifying Oil-Linked Sectors

We use Table 11 (Technical Coefficients of National Inputs) from Brazil's 2010 Input-Output Matrix (67 activities × 127 products), published by the *Instituto Brasileiro de Geografia e Estatística* (IBGE), to identify the top fifteen product categories located upstream and downstream from activity-code 0680 (Oil and Gas Extraction and Support Activities). We report these product categories in Appendix Table B1.

We translate product codes reported for each of these upstream and downstream activities into 2-digit CNAE 2.0 code roots, which is the activity classification system reported in RAIS. Each CNAE 2.0 code root is associated with numerous CNAE 2.0 subclasses, the finest available level of activity classification. For each CNAE 2.0 subclass, we manually inspect the activity description in order to assign the subclass to one or more of three oil-linked categories: direct oil-link (e.g., oil and gas extraction), upstream oil-link (e.g., fabrication of machinery for petroleum prospecting and extraction), or downstream oil-link (e.g., fabrication of petrochemical products). To check sensitivity to these definitions, we use stricter and looser assignment rules in robustness checks. In Appendix Table B2, we present examples of the translation of oil-linked I-O product codes into oil-linked CNAE 2.0 activity codes. In our preferred definition, we identify 14 directly oil-linked CNAE 2.0 subclasses, 109 upstream oil-linked subclasses, and 31 downstream oil-linked subclasses. We report the full set of oil-linked subclasses in Appendix Tables B3-B5.

Table B1: Input-Output Leontief Coefficients (Level 67 Product Codes): Direct Oil Ties and Top Upstream/Downstream Sectors

Oil Sector	**Leontief Coefficient**
Oil Extraction and Support Activities	1.068
Upstream Sectors	
Legal, Accounting, and Consulting Services	0.055
Land Transportation of Cargo	0.039
Petroleum Refining and Coke Plants	0.032
Fabrication of Machines and Mechanical Equipment	0.027
Production of Pig Iron, Alloys, Steel, and Steel Pipes	0.023
Storage and Logistics	0.021
Construction	0.021
Maintenance, Repair, and Installation of Machines and Equipment	0.020
Production of Organic and Inorganic Polymers and Resins	0.018
Architecture, Engineering, and R&D	0.018
Aquatic Transportation	0.017
Fabrication of Metal Products, Except Machines and Equipment	0.014
Non-Real Estate Rentals and Intellectual Property Management	0.011
Downstream Sectors	
Petroleum Refining and Coke Plants	0.411
Land Transportation of Cargo	0.088
Production of Organic and Inorganic Polymers and Resins	0.053
Electrical Energy and Utilities	0.047
Extraction of Non-Ferruginous Metals	0.045
Metallurgy of Non-Ferruginous Metals and Metal Casting	0.035
Extraction of Coal and Non-Metallic Minerals	0.029
Fabrication of Non-Metalic Mineral Products	0.029
Production and Refining of Sugar	0.029
Air Transportation	0.028
Production of Biofuels	0.027
Aquatic Transportation	0.027
Fabrication of Cellulose and Paper Products	0.026
Fabrication of Pesticides, Disinfectants, and Paints	0.026

Table B2: Translating 4-Digit IO Product Codes (Level 67) to 7-Digit CNAE 2.0 Activity Subclasses (Selected Examples)

IO Sector	**SCN Code**	**CNAE Roots**	**CNAE 2.0 7-Digit Subclasses (Upstream Oil-Linked)**	**CNAE 2.0 7-Digit Subclasses (Non Oil-Linked)**
Fabrication of Machines and Equipment	2800	28	Motors and Turbines, Except for Aircraft and Road Vehicles Hydraulic and Pneumatic Equipment, Except Valves Valves and Registers Industrial Compressors Industrial Ball Bearings Transmission Equipment, Except Ball Bearings Industrial Furnaces for Thermal Installations Industrial Stoves and Furnaces Lifting and Transport Machinery for People Lifting and Transport Machinery for Cargo Machinery for Industrial Refrigeration and Ventilation Machinery for Sewage and Environmental Cleanup Machine Tools Machinery for Petroleum Prospecting and Extraction Machinery for Metallurgical Industries	Compressors for Non-Industrial Uses Air Conditioning Machinery for Non-Industrial Uses Writing and Calculating Machinery for Offices Machines for General Uses Not Elsewhere Specified Tractors for Agriculture Irrigation Equipment for Agriculture Machines for Agriculture, Except Irrigation Machines for Mineral Extraction, Except Petroleum Tractors, Except for Agriculture Earth Moving, Planing, and Paving Machines Machines for Food, Drink, and Tobacco Production Machines for Textile Production Machines for Leather and Shoe Production Machines for Paper and Cardboard Production Machines for Plastic Production Machines for Industrial Uses Not Elsewhere Specified
Infrastructure Projects	4180	41, 42, 43	Construction of Pipelines, Except Water and Sewage Construction of Ports (Maritime and Riverine) Assembly of Metallic Structures Industrial Assembly Drilling and Test Boring Installation and Maintenance of Electrical Equipment Installation of Hydraulic, Sanitary, and Gas Equipment Installation and Maintenance of HVAC Systems Installation of Fire Prevention Systems Installation of Marine Navigation Systems Thermal, Acoustic, and Vibration Control Systems Project Management Services Operation and Supply of Transport and Lifting Equipment	Construction of Buildings Real Estate Development Construction of Highways and Railroads Painting and Signaling for Highways and Airports Construction of Special Art Projects Street, Plaza, and Sidewalk Projects Construction of Dams and Reservoirs for Energy Generation Construction and Maintenance of Energy Transmission Networks Construction and Maintenance of Telecommunication Networks Construction of Water and Sewage Sytems Irrigation Projects Construction of Sporting and Recreation Facilities Civil Engineering Not Elsewhere Specified Demolition of Buildings and Structures Preparation of Building Sites Earth Planing and Moving Other Site Preparation Services Installation of Billboards Installation and Maintenance of Elevators and Escalators Assembly and Installation of Public Lighting and Signaling Systems Other Installations Not Elsewhere Specified Water-Proofing in Civil Engineering Projects Installation of Doors, Windows and Roofs Plaster and Stucco General Painting Services Application of Resins (Interior and Exterior) Other Construction Finishing Services Foundation Laying Assembly and Disassembly of Scaffolding Masonry Drilling of Wells for Water

Note: Classification of CNAE 2.0 7-digit Subclasses as "oil-linked" or "non oil-linked" is based on text descriptions and contextual knowledge of each subclass. These classifications are informed by detailed descriptions of oil-linked upstream and downstream sectors provided by Oliveira (2010) and IPEA (2010).

Table B3: CNAE 2.0 Oil-Linked Subclasses

Subclass	Subclass Description	D	US	US Coef	DS	DS Coef
0600001	Extraction of Petroleum \& Natural Gas	1	0	1.068	0	1.068
0600003	Extraction \& Processing of Tar S\&s	1	0	1.068	0	1.068
0910600	Oil \& Nat. Gas Extract. Support Activ.	1	0	1.068	0	1.068
6911701	Legal Services	0	0	0.055	0	0.000
6911703	Industrial Property Management	0	0	0.055	0	0.000
6920601	Accounting Services	0	0	0.055	0	0.000
6920602	Account. \& Tax Consult. \& Audit.	0	0	0.055	0	0.000
7020400	Management Consulting	0	0	0.055	0	0.000
4911600	Rail Transport of Cargo	0	0	0.039	1	0.088
4930201	Road Transport of Cargo (Municipal)	0	0	0.039	1	0.088
4930202	Road Transport of Cargo (Inter-Munic.)	0	0	0.039	1	0.088
4940000	Pipeline Transport	0	1	0.039	1	0.088
1910100	Coke Plants	1	0	0.032	1	0.411
1921700	Fab. of Refined Oil Products	1	0	0.032	1	0.411
1922501	Formulation of Fuel Products	1	0	0.032	1	0.411
1922502	Refining of Oil Lubricants	1	0	0.032	1	0.411
1922599	Fab. of Other Petroleum Products	1	0	0.032	1	0.053
2811900	Fab. of Motors/Turbines (ex. Vehicles)	0	1	0.027	0	0.000
2812700	Fab. of Hydraulic \& Pneumatic Equip.	0	1	0.027	0	0.000
2813500	Fab. of Valves \& Registers	0	1	0.027	0	0.000
2814301	Fab. of Industrial Compressors	0	1	0.027	0	0.000
2815101	Fab. of Industrial Ball Bearings	0	1	0.027	0	0.000
2815102	Fab. of Transmission Equip.	0	1	0.027	0	0.000
2821601	Fab. Indust. Furnaces for Therm. Plants	0	1	0.027	0	0.000
2821602	Fab. of Industrial Stoves \& Furnaces	0	1	0.027	0	0.000
2822401	Fab. of Mach. for Transp./Elev. Ppl.	0	1	0.027	0	0.000
2822402	Fab. of Mach. for Transp./Elev. Cargo	0	1	0.027	0	0.000
2823200	Fab. of Machines for Industrial HVAC	0	1	0.027	0	0.000
2824101	Fab. of Indust. Air Conditioning Equip.	0	0	0.027	0	0.000
2825900	Fab. of Mach. for Sewage/Enviro. Treat	0	1	0.027	0	0.000
2840200	Fab. of Machine-Tools	0	1	0.027	0	0.000
2851800	Fab. of Mach./Equip. for Oil Prospect.	1	0	0.027	0	0.000
2861500	Fab. of Machines for Metallurg. Indust.	0	1	0.027	0	0.000
2411300	Prod. of Pig Iron	0	1	0.023	0	0.035
2412100	Prod. of Iron Alloys	0	1	0.023	0	0.035
2421100	Prod. of Semi-Finished Steel Products	0	1	0.023	0	0.035
2422901	Prod. of Steel Sheets	0	1	0.023	0	0.035
2422902	Prod. of Special Steel Sheets	0	1	0.023	0	0.035
2423701	Prod. of Steel Tubes (without Seams)	0	1	0.023	0	0.035
2423702	Prod. of Long Steel Sheets, ex. Tubes	0	1	0.023	0	0.035
2424501	Prod.s of Steel Wires	0	1	0.023	0	0.035
2424502	Prod. of Specialized Steel Products	0	1	0.023	0	0.035
2431800	Prod. of Steel Tubes (with Seams)	0	1	0.023	0	0.035
2439300	Prod. of Other Steel \& Iron Tubes	0	1	0.023	0	0.035
5212500	Loading \& Unloading of Cargo	0	1	0.021	0	0.000
5231101	Admin. of Port Infrastructure	0	1	0.021	0	0.000
5231102	Operation of Port Terminals	0	1	0.021	0	0.000
5232000	Maritime Activity Management	0	1	0.021	0	0.000
5239700	Aquatic Transport. Support Activities	0	1	0.021	0	0.000
5250804	Logistic Org. of Cargo Transport.	0	1	0.021	0	0.000

Note: Subclass refers to CNAE 2.0 Subclass. Subclass descriptions are abbreviated. D = Direct Oil; US = Upstream; US Coef. = Upstream Leontief Coefficient; DS = Downstream; DS Coef. = Downstream Leontief Coefficient. Direct, Upstream, and Downstream classifications are first made using Input-Output relationships (5-digit SCN codes) reported in Table B1. Each SCN code is translated into a 2-digit CNAE 2.0 code root using the official SCN/CNAE 2.0 Conversion Table from IBGE. Each 2-digit CNAE 2.0 code root is associated with multiple 7-digit subclasses. We manually assign selected CNAE 2.0 Subclasses as "oil-linked" using contextual knowledge and text descriptions of each subclass (Oliveira, 2010; IPEA, 2010). This process is illustrated in Table B2.

Table B4: CNAE 2.0 Oil-Linked Subclasses Cont'd.

Subclass	Subclass Description	D	U	U Coef.	D	D Coef.
4223500	Constr. of Pipe. (ex. Water/Sewage)	0	1	0.021	0	0.000
4291000	Port \& Maritime Projects	0	1	0.021	0	0.000
4292801	Construction of Metallic Structures	0	1	0.021	0	0.000
4292802	Industrial Construction Projects	0	1	0.021	0	0.000
4312600	Perforations \& Drilling	0	1	0.021	0	0.000
4321500	Electrical Install. \& Maint.	0	1	0.021	0	0.000
4322301	Hydraulic, Sanitary, \& Gas Install.	0	1	0.021	1	0.000
4322302	Install. \& Maint. of HVAC Systems	0	0	0.021	0	0.000
4322303	Install. of Fire Prevention Systems	0	1	0.021	0	0.000
4329102	Install. of Maritime Navigation Syst.	0	1	0.021	0	0.000
4329105	Treat. for Heat, Noise, Vibrat. Cont.	0	1	0.021	0	0.000
4399101	Project Management	0	1	0.021	0	0.000
4399104	Supply of Transport \& Elev. Equip.	0	1	0.021	0	0.000
3311200	Maint. \& Repair of Tanks (ex. Vehicles)	0	1	0.020	0	0.000
3312102	Maint. \& Repair of Measurement Instr.	0	1	0.020	0	0.000
3312104	Maint. \& Repair of Optical Instr.	0	0	0.020	0	0.000
3313901	Maint. \& Repair of Eletrical Generators	0	1	0.020	0	0.000
3313902	Maint. \& Repair Batteries (ex. Vehic.)	0	1	0.020	0	0.000
3313999	Maint. \& Repair of Other Electr. Mach.	0	1	0.020	0	0.000
3314701	Maint. \& Repair of Non-Elect. Motors	0	1	0.020	0	0.000
3314702	Maint. \& Repair Hydr./Pneum. Equip.	0	1	0.020	0	0.000
3314703	Maint. \& Repair of Industrial Valves	0	1	0.020	0	0.000
3314704	Maint. \& Repair of Compressors	0	1	0.020	0	0.000
3314705	Maint. \& Repair Indust. Transm. Equip.	0	1	0.020	0	0.000
3314706	Maint. \& Repair of Thermal Machines	0	1	0.020	1	0.000
3314707	Maint. \& Repair of HVAC Machines	0	0	0.020	0	0.000
3314708	Maint. \& Repair of Transp./Elev. Equip.	0	1	0.020	0	0.000
3314713	Maint. \& Repair of Machine Tools	0	1	0.020	0	0.000
3314714	Maint. \& Repair of Oil Prospect. Equip.	1	0	0.020	0	0.000
3314718	Maint. \& Repair Metal. Machines	0	1	0.020	0	0.000
3317101	Maint. \& Repair Ships/Floating Struct.	0	1	0.020	0	0.000
3321000	Install. of Industrial Machines	0	1	0.020	0	0.000
2014200	Fab. of Industrial Gases	0	1	0.018	1	0.053
2022300	Fab. of Interm. Plastics, Resins, Fibers	0	1	0.018	1	0.053
2021500	Fab. of Basic Petrochemical Products	1	0	0.018	1	0.053
2031200	Fab. of Thermoplastic Resins	0	1	0.018	1	0.053
2032100	Fab. of Thermosetting Resins	0	1	0.018	1	0.053
2033900	Fab. of Elastomeres	0	1	0.018	1	0.053
7111100	Architectural Services	0	0	0.018	0	0.000
7112000	Engineering Services	0	1	0.018	0	0.000
7119701	Cartog., Topog., \& Geo. Services	0	1	0.018	0	0.000
7119702	Geological Studies	0	1	0.018	0	0.000
7119703	Tech. Design Services Architect./Eng.	0	1	0.018	0	0.000
7119704	Workplace Safety Services	0	1	0.018	0	0.000
7119799	Other Eng. \& Architect. Service	0	1	0.018	0	0.000
7120100	Tests \& Technical Analyses	0	1	0.018	0	0.000
7210000	Exp. R&D in Phys. \& Nat. Sciences	0	1	0.018	0	0.000
5011401	Maritime Cargo Transport	0	1	0.017	0	0.027

Note: Subclass refers to CNAE 2.0 Subclass. Subclass descriptions are abbreviated. D = Direct Oil; US = Upstream; US Coef. = Upstream Leontief Coefficient; DS = Downstream; DS Coef. = Downstream Leontief Coefficient. Direct, Upstream, and Downstream classifications are first made using Input-Output relationships (5-digit SCN codes) reported in Table B1. Each SCN code is translated into a 2-digit CNAE 2.0 code root using the official SCN/CNAE 2.0 Conversion Table from IBGE. Each 2-digit CNAE 2.0 code root is associated with multiple 7-digit subclasses. We manually assign selected CNAE 2.0 Subclasses as "oil-linked" using contextual knowledge and text descriptions of each subclass (Oliveira, 2010; IPEA, 2010). This process is illustrated in Table B2.

Table B5: CNAE 2.0 Oil-Linked Subclasses Cont'd.

Subclass	**Subclass Description**	**D**	**U**	**U Coef.**	**D**	**D Coef.**
5012201	Maritime Cargo Transp. (Long-Dist.)	0	1	0.017	0	0.027
5030101	Maritime Navigation Support	0	1	0.017	0	0.027
5030102	Port Navigation Support	0	1	0.017	0	0.027
2511000	Fab. of Metallic Structures	0	1	0.014	0	0.000
2513600	Fab. of Heavy Boilers	0	1	0.014	0	0.000
2522500	Fabricatoin of Vapor Boilers	0	1	0.014	0	0.000
2531401	Prod. of Forged Steel Products	0	1	0.014	0	0.000
2531402	Prod. of Forged Iron Alloys	0	1	0.014	0	0.000
2532201	Prod. of Stamped Metal Products	0	1	0.014	0	0.000
2532202	Powder Metallurgy	0	1	0.014	0	0.000
2539000	Machining \& Welding Services	0	1	0.014	0	0.000
2539001	Machining \& Turning	0	1	0.014	0	0.000
2539002	Treatment \& Coating of Metals	0	1	0.014	0	0.000
2543800	Fab. of Tools	0	1	0.014	0	0.000
2592601	Fab. of Draw Metal Prod. (Stand.)	0	1	0.014	0	0.000
2592602	Fab. of Drawn Metal Prod. (Non-Stand.)	0	1	0.014	0	0.000
2599302	Metal Cutting \& Folding Services	0	1	0.014	0	0.000
7719501	Rental of Ships w.o. Crew (ex. Rec.)	0	1	0.011	0	0.000
7732201	Rental of Machines \& Equip. Constr.	0	1	0.011	0	0.000
7739001	Rental of Mach./Equip. for Petrol. Extr.	1	0	0.011	0	0.000
7739002	Rental of Scientific Equip.	0	0	0.011	0	0.000
7740300	Mgmt. Intangible Non-Financ. Assets	0	0	0.011	0	0.000
3011301	Construction of Large Ships	0	1	0.000	0	0.000
3511500	Electrical Energy Gen. (Deactivated)	0	0	0.000	1	0.047
3511501	Electrical Energy Generation	0	0	0.000	1	0.047
3511502	Coord. \& Control of Elect. Gen.	0	0	0.000	1	0.047
3512300	Electrical Energy Transmission	0	0	0.000	0	0.047
3513100	Wholesale Electr. Energy Comm.	0	0	0.000	0	0.047
3514000	Electricity Distribution	0	0	0.000	0	0.047
3520401	Prod. of Gas	0	0	0.000	1	0.047
3520402	Dist. of Fuel Gas to Urban Util.	0	0	0.000	0	0.047
2219600	Fab. of Rubber Products	0	0	0.000	1	0.024
2221800	Fab. of Plastic Tubes \& Sheets	0	0	0.000	1	0.024
2222600	Fab. of Plastic Packaging	0	0	0.000	1	0.024
2223400	Fab. of Plastic Tubes for Constr.	0	0	0.000	1	0.024
2229301	Fab. of Plastic Art. for Domest.	0	0	0.000	1	0.024
2229302	Fab. of Plastic Products for Industr.	0	0	0.000	1	0.024
2229303	Fab. Plast Prod. Constr. (ex. Tubes)	0	0	0.000	1	0.024
2229399	Fab. of Plast Prod. Other Use	0	0	0.000	1	0.024
1931400	Fab. of Ethanol	0	0	0.000	1	0.027
1932200	Fab. of Biofuels (ex. Ethanol)	0	0	0.000	1	0.027

Note: Subclass refers to CNAE 2.0 Subclass. Subclass descriptions are abbreviated. D = Direct Oil; US = Upstream; US Coef. = Upstream Leontief Coefficient; DS = Downstream; DS Coef. = Downstream Leontief Coefficient. Direct, Upstream, and Downstream classifications are first made using Input-Output relationships (5-digit SCN codes) reported in Table B1. Each SCN code is translated into a 2-digit CNAE 2.0 code root using the official SCN/CNAE 2.0 Conversion Table from IBGE. Each 2-digit CNAE 2.0 code root is associated with multiple 7-digit subclasses. We manually assign selected CNAE 2.0 Subclasses as "oil-linked" using contextual knowledge and text descriptions of each subclass (Oliveira, 2010; IPEA, 2010). This process is illustrated in Table B2.

B.2 Predicting Hire into Oil-Linked Sectors

Table B6: Predictors of Being Hired into Oil-Linked Sector (Logit)

Covariates	First-time Hires	Experienced Hires	Experienced Hires
Education	0.023	0.047	0.053
	(0.001)	(0.001)	(0.001)
Female	-1.54	-1.46	-1.47
	(0.003)	(0.003)	(0.003)
Nonwhite	0.187	0.175	0.175
	(0.002)	(0.002)	(0.002)
Age	0.048	0.029	0.030
	(0.001)	(0.001)	(0.001)
Age Squared	-0.001	-0.0003	-0.0003
	(0.000)	(0.000)	(0.000)
Wage in Previous Job		0.0001	
		(0.000)	
Previous Firm Size		0.033	
		(0.000)	
Wage Rank in Previous Firm			-0.267
			(0.004)
Education Rank in Previous Firm			-0.166
			(0.005)
Occupation Rank in Previous Firm			-0.475
			(0.008)
2007 (years relative to 2006)	0.153	0.093	0.097
	(0.006)	(0.006)	(0.006)
2008	0.273	0.241	0.248
	(0.006)	(0.006)	(0.006)
2009	0.206	0.0142	0.0204
	(0.006)	(0.006)	(0.006)
2010	0.330	0.151	0.162
	(0.005)	(0.006)	(0.006)
2011	0.451	0.095	0.134
	(0.006)	(0.006)	(0.006)
2012	0.470	0.128	0.173
	(0.005)	(0.005)	(0.005)
2013	0.419	0.080	0.127
	(0.005)	(0.005)	(0.005)
2014	0.351	-0.030	0.016
	(0.006)	(0.006)	(0.006)
2015	0.240	-0.222	-0.180
	(0.006)	(0.006)	(0.006)
2016	0.119	-0.311	-0.273
	(0.007)	(0.007)	(0.007)
2017	0.074	-0.236	-0.195
	(0.007)	(0.007)	(0.007)
State FEs	Y	Y	Y
Observations	40,712,468	23,042,525	23,042,525

Note: Marginal effects from logit models are reported with heteroskedasticity-consistent robust standard errors in parentheses. Estimates are obtained by regressing a binary indicator that takes a value of 1 if a worker was hired as a new or experienced worker into an oil-linked establishment on worker-level covariates and year and state fixed effects. Column 1 uses a pooled cross-sectional sample of all newly hired formal workers in Brazil between 2006-2017. Columns 2 and 3 use a pooled cross-sectional sample of all experienced hires between 2006-2017. For experienced workers, previous employment characteristics are observed and can therefore be included in regressions. Rank variables (wage, education, and occupation) are computed for each experienced worker's previous firm, such that the highest paid employee at the firm would have a wage rank of 1. Ranks are normalized to a 0-to-1 scale. Occupation rank is based on workers' occupation falling into categories ranging from manager or professional (highest), to technician (mid-rank), to worker (low-rank). Year fixed effects are reported relative to the omitted base year (2006).

B.3 Descriptive Statistics

Table B7: Descriptive Statistics: **Experienced Hires**

		Wage	Education	Age	Female	Nonwhite	n
2006	Population (Treated)	4,312	6.90	32.51	0.13	0.28	15,347
		(4457.5)	(1.63)	(8.38)	(0.34)	(0.45)	
	Population (Control)	2,580	6.58	31.39	0.33	0.30	294,342
		(3795.5)	(1.78)	(8.04)	(0.47)	(0.46)	
	Matched (Treated)	6,210	7.75	31.55	0.18	0.19	2,461
		(6037.2)	(1.33)	(6.45)	(0.38)	(0.39)	
	Matched (Control)	7,653	7.94	30.76	0.23	0.17	10,201
		(9220.4)	(1.24)	(5.71)	(0.42)	(0.37)	
	Unmatched Difference	1,732.0	0.32	1.13	-0.20	-0.02	
	Matched Difference	-1,442.8	-0.19	0.78	-0.06	0.02	
2008	Population (Treated)	3,171	6.43	32.64	0.10	0.39	14,760
		(3453.9)	(1.56)	(8.57)	(0.30)	(0.49)	
	Population (Control)	1,928	6.31	31.52	0.30	0.34	243,331
		(2305.0)	(1.68)	(8.12)	(0.46)	(0.47)	
	Matched (Treated)	3,041	6.81	31.13	0.08	0.34	1,437
		(3647.6)	(1.16)	(6.91)	(0.28)	(0.47)	
	Matched (Control)	2,530	6.97	29.87	0.10	0.31	4,961
		(3717.6)	(0.97)	(5.76)	(0.29)	(0.46)	
	Unmatched Difference	1,242.46	0.12	1.12	-0.20	0.05	
	Matched Difference	511.14	-0.16	1.26	-0.01	0.03	
2010	Population (Treated)	4,181	6.87	32.56	0.13	0.40	41,437
		(5053.3)	(1.52)	(8.51)	(0.34)	(0.49)	
	Population (Control)	2,522	6.73	31.78	0.35	0.36	662,855
		(3510.2)	(1.64)	(8.28)	(0.48)	(0.48)	
	Matched (Treated)	5,255	7.31	31.65	0.14	0.38	10,767
		(6619.4)	(1.26)	(7.12)	(0.35)	(0.48)	
	Matched (Control)	4,572	7.50	30.35	0.24	0.31	54,024
		(6638.2)	(1.18)	(6.09)	(0.43)	(0.46)	
	Unmatched Difference	1,658.56	0.14	0.79	-0.22	0.04	
	Matched Difference	683.32	-0.19	1.29	-0.10	0.06	
2012	Population (Treated)	3,217	6.56	33.35	0.11	0.48	22,371
		(3414.1)	(1.51)	(8.52)	(0.32)	(0.50)	
	Population (Control)	2,069	6.50	32.83	0.34	0.40	369,713
		(2240.0)	(1.59)	(8.39)	(0.48)	(0.49)	
	Matched (Treated)	3,075	6.86	32.42	0.09	0.48	2,899
		(3692.3)	(1.09)	(6.92)	(0.28)	(0.50)	
	Matched (Control)	2,447	6.98	31.55	0.14	0.44	11,327
		(3377.7)	(0.87)	(6.25)	(0.35)	(0.50)	
	Unmatched Difference	1,147.20	0.06	0.51	-0.23	0.07	
	Matched Difference	628.55	-0.12	0.87	-0.05	0.04	
2014	Population (Treated)	3,932	6.94	32.24	0.15	0.48	43,659
		(4728.5)	(1.46)	(8.51)	(0.36)	(0.50)	
	Population (Control)	2,542	6.86	32.25	0.41	0.42	869,401
		(3286.9)	(1.56)	(8.80)	(0.49)	(0.49)	
	Matched (Treated)	4,852	7.34	31.63	0.17	0.47	10,805
		(6038.9)	(1.20)	(7.13)	(0.37)	(0.50)	
	Matched (Control)	4,775	7.61	31.06	0.28	0.40	66,213
		(6690.3)	(1.12)	(6.44)	(0.45)	(0.49)	
	Unmatched Difference	1,389.70	0.08	-0.01	-0.25	0.06	
	Matched Difference	76.99	-0.27	0.56	-0.11	0.07	

Note: Table reports means and standard deviations (in parentheses) for the full population of formal workers who were hired as experienced workers from employment in other jobs (poached) in a given year, as well as for matched subsamples. "Treated" refers to workers who were hired into an oil-linked establishment; "control" refers to all other workers hired into other sectors from employment in other jobs. Monetary values are deflated to constant 2018 $BRL. Poached is defined as voluntary exit from previous firm and rehire at a new firm within 4 months. Coarsened exact matching criteria are: education, sex, non-white race indicator, occupation category, age bin, previous establishment, two lags of wage bins prior to poach (which implicitly matches on wage growth), and destination municipality. Unmatched differences (the difference between mean values for population (treated) and population (control)) and matched differences (the difference between mean values for matched (treated) and matched (control)) are also reported for each cohort to assess the effectiveness of the matching procedure in reducing mean differences between groups. While matching procedure does not always appear to balance raw sample means between treated and control groups, inclusion of matching weights in regression analyses ensures proper balancing.

Table B8: Descriptive Statistics: **First-time Hires**

		Wage	Education	Age	Female	Nonwhite	n
2006	Population (Treated)	1,491	5.44	26.15	0.13	0.47	72,582
		(2153)	(1.90)	(8.75)	(0.34)	(0.50)	
	Population (Control)	1,238	5.97	26.18	0.44	0.50	3,169,213
		(1661)	(1.80)	(8.95)	(0.50)	(0.50)	
	Matched (Treated)	1,298	6.01	23.22	0.13	0.39	3,592
		(1540)	(1.54)	(6.19)	(0.34)	(0.49)	
	Matched (Control)	1,173	6.41	21.56	0.25	0.33	15,953
		(1215)	(1.22)	(4.29)	(0.44)	(0.47)	
	Unmatched Difference	253.3	-0.53	-0.03	-0.31	-0.03	
	Matched Difference	125.2	-0.40	1.66	-0.12	0.06	
2008	Population (Treated)	1,642	5.76	26.01	0.15	0.49	99,771
		(2541)	(1.78)	(8.68)	(0.36)	(0.50)	
	Population (Control)	1,277	6.11	26.21	0.46	0.52	3,757,139
		(1679)	(1.74)	(8.94)	(0.50)	(0.50)	
	Matched (Treated)	1,423	6.15	23.93	0.15	0.46	9,184
		(2125)	(1.44)	(6.98)	(0.36)	(0.50)	
	Matched (Control)	1,175	6.45	22.33	0.28	0.46	80,985
		(1217)	(1.10)	(5.34)	(0.45)	(0.50)	
	Unmatched Difference	364.6	-0.35	-0.20	-0.31	-0.03	
	Matched Difference	248.1	-0.30	1.60	-0.13	0.00	
2010	Population (Treated)	1,799	5.95	26.42	0.15	0.53	106,114
		(2651)	(1.69)	(9.03)	(0.36)	(0.50)	
	Population (Control)	1,361	6.26	26.37	0.48	0.56	4,007,616
		(1754)	(1.67)	(9.14)	(0.50)	(0.50)	
	Matched (Treated)	1,468	6.38	24.06	0.15	0.50	6,228
		(1643)	(1.32)	(7.16)	(0.36)	(0.50)	
	Matched (Control)	1,301	6.58	22.49	0.26	0.47	26,556
		(1403)	(1.12)	(5.73)	(0.44)	(0.50)	
	Unmatched Difference	438.4	-0.31	0.05	-0.32	-0.03	
	Matched Difference	167.4	-0.20	1.58	-0.11	0.03	
2012	Population (Treated)	1,956	6.17	25.72	0.18	0.59	108,924
		(3032)	(1.63)	(9.03)	(0.38)	(0.49)	
	Population (Control)	1,410	6.36	25.90	0.49	0.47	3,906,395
		(1700)	(1.58)	(9.55)	(0.50)	(0.50)	
	Matched (Treated)	1,841	6.46	24.13	0.18	0.59	11,143
		(3265)	(1.32)	(8.04)	(0.39)	(0.49)	
	Matched (Control)	1,364	6.63	21.68	0.33	0.55	91,778
		(1909)	(0.97)	(6.31)	(0.47)	(0.50)	
	Unmatched Difference	546.1	-0.19	-0.18	-0.32	0.12	
	Matched Difference	477.0	-0.16	2.44	-0.15	0.03	
2014	Population (Treated)	1,959	6.27	25.50	0.19	0.58	84,554
		(3307)	(1.53)	(9.28)	(0.39)	(0.49)	
	Population (Control)	1,490	6.47	25.81	0.49	0.48	3,422,596
		(1821)	(1.58)	(9.77)	(0.50)	(0.50)	
	Matched (Treated)	1,613	6.60	23.14	0.21	0.59	4,745
		(2170)	(1.12)	(7.45)	(0.41)	(0.49)	
	Matched (Control)	1,415	6.71	20.77	0.37	0.58	26,758
		(2306)	(0.92)	(5.65)	(0.48)	(0.49)	
	Unmatched Difference	469.3	-0.21	-0.31	-0.30	0.10	
	Matched Difference	197.5	-0.10	2.37	-0.16	0.01	

Note: Table reports means and standard deviations (in parentheses) for the full population of formal workers who were newly hired in a given year, as well as for matched subsamples. "Treated" refers to workers who were hired into an oil-linked establishment; "control" refers to all other hired workers. Monetary values are deflated to constant 2018 $BRL. A new hire is defined as a worker who is hired to their first formal job and is 30 or younger. Coarsened exact matching criteria are: education, sex, non-white race indicator, municipality, age bin, and wage and firm size bins in first job. Unmatched differences (the difference between mean values for population (treated) and population (control)) and matched differences (the difference between mean values for matched (treated) and matched (control)) are also reported for each cohort to assess the effectiveness of the matching procedure in reducing mean differences between groups. While matching procedure does not always appear to balance raw sample means between treated and control groups, inclusion of matching weights in regression analyses ensures proper balancing.

*Matching is performed on a random subsample of 20% of the full population of first-time hires. Thus, when evaluating matched workers as a share of the population, note that the matching success rate is five times larger than suggested by reported sample sizes.

B.4 Regression Tables (Experienced Hires)

Table B9: Experienced Hires: **Hourly Wages**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2003	-0.026	0.01	-	-	-	-	-	-	-	-
2004	-0.013	0.006	-	-	-	-	-	-	-	-
2005	(base)	-	-0.015	0.013	-	-	-	-	-	-
2006	0.03	0.009	-0.013	0.008	-	-	-	-	-	-
2007	0.118	0.013	(base)	-	-0.002	0.005	-	-	-	-
2008	0.176	0.014	0.055	0.014	-0.001	0.003	-0.014	0.009	-	-
2009	0.171	0.016	0.095	0.016	(base)	-	-0.011	0.006	-	-
2010	0.194	0.017	0.099	0.017	0.071	0.005	(base)	-	0.005	0.006
2012	0.215	0.018	0.107	0.021	0.119	0.007	0.034	0.01	-0.002	0.003
2013	0.218	0.02	0.127	0.022	0.119	0.007	0.057	0.012	(base)	-
2014	0.227	0.021	0.094	0.023	0.111	0.008	0.044	0.014	0.04	0.004
2015	0.254	0.021	0.082	0.027	0.088	0.009	0.029	0.015	0.05	0.006
2016	0.244	0.023	0.089	0.029	0.063	0.009	0.014	0.017	0.016	0.007
2017	0.274	0.024	0.063	0.031	0.061	0.01	-0.027	0.017	0.014	0.008
n	12,563		6,357		64,302		14,095		76,333	
n×t	158,323		78,868		758,754		164,345		793,605	
N	309,689		258,091		704,292		392,084		913,060	
DV Mean	36.82		14.29		22.87		13.42		25.14	
Adj. R^2	0.842		0.678		0.808		0.681		0.788	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 4, Panel 1. Hourly wages are deflated to constant 2018 $BRL and transformed using inverse hyberbolic sine, then regressed on relative time indicators around year of being hired into an oil-linked establishment, with $t-1$ period omitted. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. For hourly wages, the sample is restricted to employed individuals. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B10: Experienced Hires: **Months Employed per Year**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2003	0.062	0.115	-	-	-	-	-	-	-	-
2004	0.026	0.067	-	-	-	-	-	-	-	-
2005	(base)	-	-0.301	0.155	-	-	-	-	-	-
2006	0.005	0.072	-0.175	0.116	-	-	-	-	-	-
2007	0.177	0.093	(base)	-	-0.136	0.056	-	-	-	-
2008	0.159	0.108	-0.240	0.126	0.052	0.041	0.036	0.102	-	-
2009	0.342	0.118	-0.843	0.162	(base)	-	-0.039	0.075	-	-
2010	0.347	0.120	-0.391	0.159	0.088	0.039	(base)	-	0.074	0.057
2012	0.388	0.133	-0.476	0.175	0.025	0.057	-0.278	0.091	0.133	0.041
2013	0.444	0.135	-0.449	0.180	-0.006	0.060	-0.308	0.103	(base)	-
2014	0.435	0.141	-0.592	0.189	-0.019	0.064	-0.227	0.117	0.192	0.041
2015	0.474	0.146	-0.807	0.201	-0.076	0.067	-0.418	0.124	-0.092	0.056
2016	0.394	0.154	-0.663	0.208	-0.28	0.074	-0.671	0.135	-0.326	0.066
2017	0.416	0.160	-0.778	0.211	-0.248	0.078	-0.505	0.138	-0.256	0.070
n	12,158		6,095		61,763		14,095		65,709	
n×t	169,779		85,330		864,682		197,330		919,926	
N	309,689		258,091		704,292		392,084		913,060	
DV Mean	11.57		10.13		10.84		11		11.02	
Adj. R^2	0.373		0.287		0.321		0.343		0.423	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 5, Panel 1. Months employed per year are regressed on relative time indicators around year of being hired into an oil-linked establishment, with $t-1$ period omitted. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. For brevity, each column reports coefficient estimates from every other year for a specific cohort. One pre-period is reported for each cohort to evaluate pre-trends. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B11: Experienced Hires: **Annual Formal Earnings**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2003	-0.011	0.030	-	-	-	-	-	-	-	-
2004	-0.009	0.009	-	-	-	-	-	-	-	-
2005	(base)	-	-0.026	0.045	-	-	-	-	-	-
2006	0.026	0.013	-0.028	0.018	-	-	-	-	-	-
2007	0.16	0.029	(base)	-	-0.023	0.018	-	-	-	-
2008	0.237	0.068	0.053	0.027	0.006	0.006	-0.036	0.049	-	-
2009	0.386	0.083	-0.098	0.083	(base)	-	-0.015	0.012	-	-
2010	0.451	0.092	0.030	0.089	0.085	0.008	(base)	-	-0.014	0.019
2012	0.607	0.101	-0.067	0.118	0.146	0.034	0.004	0.020	0.009	0.006
2013	0.654	0.106	0.082	0.129	0.158	0.040	0.013	0.057	(base)	-
2014	0.563	0.114	0.006	0.143	0.131	0.046	-0.015	0.077	0.065	0.009
2015	0.672	0.122	-0.112	0.158	0.112	0.053	-0.143	0.092	-0.006	0.032
2016	0.627	0.137	-0.111	0.172	-0.095	0.061	-0.341	0.108	-0.231	0.049
2017	0.73	0.145	-0.230	0.177	-0.133	0.065	-0.27	0.115	-0.278	0.056
n	12,158		6,095		61,763		14,095		65,709	
n×t	169,779		85,330		864,682		197,330		919,926	
N	309,689		258,091		704,292		392,084		913,060	
DV Mean	76064.82		27280.15		46162.49		27112.43		50684.74	
Adj. R^2	0.367		0.280		0.336		0.348		0.466	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 6, Panel 1. Annual formal earnings are deflated to constant 2018 $BRL, transformed with inverse hyperbolic sine, then regressed on relative time indicators around year of being hired into an oil-linked establishment, with $t-1$ period omitted. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B12: Experienced Hires: **Annual Formal Earnings (Less Than High School)**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2003	-0.048	0.097	-	-	-	-	-	-	-	-
2004	-0.020	0.043	-	-	-	-	-	-	-	-
2005	(base)	-	-0.089	0.147	-	-	-	-	-	-
2006	0.031	0.058	-0.025	0.057	-	-	-	-	-	-
2007	-0.114	0.181	(base)	-	-0.062	0.072	-	-	-	-
2008	-0.214	0.329	-0.022	0.096	0.035	0.025	0.025	0.171	-	-
2009	-0.227	0.366	-0.562	0.275	(base)	-	-0.033	0.060	-	-
2010	0.138	0.399	-0.156	0.255	0.102	0.034	(base)	-	-0.015	0.089
2012	-0.103	0.442	-0.232	0.312	0.060	0.132	0.005	0.086	0.013	0.030
2013	0.333	0.465	0.132	0.349	-0.074	0.156	0.107	0.255	(base)	-
2014	-0.215	0.501	-0.077	0.365	-0.174	0.181	-0.328	0.284	0.064	0.041
2015	-0.759	0.523	-0.552	0.463	-0.112	0.195	-1.055	0.365	-0.307	0.168
2016	-1.037	0.555	-0.783	0.511	-0.434	0.222	-0.898	0.388	-0.828	0.239
2017	-1.497	0.554	-0.413	0.508	-0.573	0.234	-0.684	0.425	-0.883	0.274
n	595		485		2,986		765		1,878	
n×t	8,297		6,790		41,804		10,710		26,292	
N	102,533		95,733		194,036		120,835		215,044	
DV Mean	16,782.5		17,846.7		15,777.2		18,337.6		19,138.2	
Adj. R^2	0.371		0.278		0.313		0.307		0.429	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 6, Panel 2. Annual formal earnings for subsample of workers with less than complete secondary education are deflated to constant 2018 $BRL, transformed with inverse hyperbolic sine, then regressed on relative time indicators around year of being hired into an oil-linked establishment, with $t-1$ period omitted. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B13: Experienced Hires: **Annual Formal Earnings (High School Complete)**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2003	0.018	0.049	-	-	-	-	-	-	-	-
2004	-0.018	0.016	-	-	-	-	-	-	-	-
2005	(base)	-	-0.009	0.055	-	-	-	-	-	-
2006	0.052	0.025	-0.025	0.021	-	-	-	-	-	-
2007	0.154	0.043	(base)	-	0.006	0.025	-	-	-	-
2008	0.080	0.095	0.069	0.032	0.011	0.009	-0.054	0.055	-	-
2009	0.051	0.118	-0.074	0.099	(base)	-	-0.013	0.014	-	-
2010	0.069	0.126	0.059	0.106	0.1	0.012	(base)	-	0.010	0.025
2012	0.200	0.154	-0.028	0.138	0.076	0.045	0.009	0.023	0.004	0.009
2013	-0.021	0.151	0.011	0.152	0.081	0.052	-0.051	0.064	(base)	-
2014	-0.008	0.165	-0.001	0.168	0.033	0.060	-0.064	0.084	0.078	0.012
2015	0.081	0.177	0.003	0.182	0.053	0.068	-0.094	0.101	0.054	0.043
2016	-0.180	0.199	-0.060	0.196	-0.094	0.079	-0.319	0.121	-0.107	0.063
2017	-0.077	0.205	-0.288	0.202	-0.055	0.083	-0.234	0.127	-0.123	0.070
n	4,641		4,670		35,366		11,184		36,700	
n×t	64,830		65,380		495,124		156,576		513,800	
N	132,673		124,471		349,400		212,235		483,765	
DV Mean	22,895.6		19,597.4		19,943.7		21,503.0		22,620.5	
Adj. R^2	0.329		0.273		0.325		0.348		0.453	

Note: Table reports coefficient estimates, standard errors, and sample stats corresponding with Figure 6, Panel 2. Annual formal earnings for workers with complete secondary education are deflated to constant 2018 $BRL, transformed with inverse hyperbolic sine, then regressed on relative time indicators around year of being hired into oil-linked establishment, with $t-1$ omitted. Worker and year fixed effects are included; standard errors clustered at matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. For brevity, each column reports every other coefficient estimate for a specific cohort. One pre-period is reported to evaluate pre-trends. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B14: Experienced Hires: **Annual Formal Earnings (More than High School)**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2003	-0.027	0.041	-	-	-	-	-	-	-	-
2004	0.000	0.010	-	-	-	-	-	-	-	-
2005	(base)	-	-0.071	0.082	-	-	-	-	-	-
2006	0.004	0.014	-0.062	0.041	-	-	-	-	-	-
2007	0.19	0.037	(base)	-	-0.07	0.028	-	-	-	-
2008	0.404	0.096	0.056	0.053	-0.012	0.008	0.006	0.135	-	-
2009	0.708	0.119	0.171	0.162	(base)	-	-0.023	0.023	-	-
2010	0.774	0.134	0.045	0.224	0.057	0.010	(base)	-	-0.06	0.031
2012	0.996	0.138	-0.143	0.331	0.282	0.055	0.019	0.049	0.013	0.008
2013	1.2	0.152	0.389	0.339	0.334	0.071	0.319	0.132	(base)	-
2014	1.08	0.162	0.091	0.402	0.358	0.077	0.418	0.227	0.049	0.010
2015	1.283	0.172	-0.377	0.446	0.253	0.093	0.177	0.248	-0.054	0.046
2016	1.432	0.195	0.113	0.490	-0.039	0.109	-0.112	0.301	-0.347	0.082
2017	1.604	0.210	0.113	0.518	-0.196	0.118	-0.188	0.317	-0.458	0.100
n	6,922		940		23,411		2,146		27,131	
n×t	96,652		13,160		327,754		30,044		379,834	
N	74,483		37,887		160,856		59,014		214,251	
DV Mean	116,809		70,315.9		89,645.7		59,474.3		90,830.8	
Adj. R^2	0.362		0.279		0.327		0.345		0.484	

Note: Table reports coefficient estimates, standard errors, and sample stats corresponding with Figure 6, Panel 2. Annual formal earnings for workers with > secondary education are deflated to constant 2018 $BRL, transformed with inverse hyperbolic sine, then regressed on relative time indicators around year of being hired into an oil-linked establishment, with $t-1$ omitted. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

B.5 Regression Tables (First-time Hires)

Table B15: First-Time Hires: **Hourly Wages**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2006	(base)	-	-	-	-	-	-	-	-	-
2007	0.036	0.006	-	-	-	-	-	-	-	-
2008	0.072	0.008	(base)	-	-	-	-	-	-	-
2009	0.068	0.008	0.012	0.005	-	-	-	-	-	-
2010	0.08	0.009	0.042	0.006	(base)	-	-	-	-	-
2012	0.1	0.010	0.059	0.007	0.048	0.006	(base)	-	-	-
2013	0.106	0.011	0.058	0.008	0.05	0.007	0.015	0.004	-	-
2014	0.103	0.012	0.056	0.008	0.055	0.008	0.031	0.005	(base)	-
2015	0.089	0.013	0.043	0.009	0.048	0.009	0.026	0.007	-0.006	0.006
2016	0.062	0.014	0.03	0.010	0.038	0.010	0.015	0.007	-0.010	0.007
2017	0.062	0.016	0.025	0.011	0.026	0.011	0.017	0.008	0.009	0.008
n	93,818		135,750		122,162		137,333		109,205	
nxt	666,401		798,751		624,650		611,985		345,068	
N	3,241,795		3,856,910		4,113,730		4,015,319		3,507,150	
DV Mean	7.17		7.12		8.13		8.79		9.53	
Adj. R^2	0.676		0.665		0.650		0.726		0.690	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 7, Panel 1. Hourly wages are deflated to constant 2018 $BRL and transformed using inverse hyberbolic sine, then regressed on relative time indicators around year of new hire into an oil-linked establishment, with period t as baseline. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. For hourly wages, the sample is restricted to employed individuals. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B16: First-Time Hires: **Months Employed per Year**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2006	(base)	-	-	-	-	-	-	-	-	-
2007	0.007	0.073	-	-	-	-	-	-	-	-
2008	0.089	0.079	(base)	-	-	-	-	-	-	-
2009	0.023	0.082	-0.277	0.061	-	-	-	-	-	-
2010	0.024	0.080	-0.162	0.062	(base)	-	-	-	-	-
2012	-0.054	0.083	-0.186	0.067	-0.096	0.062	(base)	-	-	-
2013	-0.047	0.087	-0.238	0.070	-0.058	0.066	-0.217	0.059	-	-
2014	-0.029	0.088	-0.23	0.071	-0.048	0.066	-0.283	0.062	(base)	-
2015	-0.125	0.114	-0.242	0.074	-0.122	0.070	-0.258	0.069	-0.376	0.066
2016	-0.224	0.104	-0.371	0.082	-0.151	0.076	-0.315	0.074	-0.575	0.078
2017	-0.029	0.118	-0.325	0.086	-0.171	0.080	-0.375	0.080	-0.429	0.081
n	94,511		137,222		123,639		139,349		112,145	
nxt	680,825		817,327		641,779		630,572		358,570	
N	3,241,795		3,856,910		4,113,730		4,015,319		3,507,150	
DV Mean	5.20		5.30		5.20		5.30		5.50	
Adj. R^2	0.346		0.370		0.366		0.427		0.364	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 8, Panel 1. Months employed per year are regressed on relative time indicators around year of new hire into an oil-linked establishment, with period t as baseline. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B17: First-Time Hires: **Annual Formal Earnings**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2006	(base)	-	-	-	-	-	-	-	-	-
2007	0.004	0.040	-	-	-	-	-	-	-	-
2008	0.069	0.041	(base)	-	-	-	-	-	-	-
2009	0.020	0.044	-0.144	0.029	-	-	-	-	-	-
2010	0.075	0.043	-0.082	0.030	(base)	-	-	-	-	-
2012	0.052	0.044	-0.083	0.032	-0.066	0.036	(base)	-	-	-
2013	0.073	0.045	-0.094	0.032	-0.061	0.034	-0.13	0.028	-	-
2014	0.073	0.046	-0.077	0.034	-0.054	0.033	-0.122	0.030	(base)	-
2015	0.036	0.051	-0.123	0.036	-0.09	0.038	-0.129	0.033	-0.2	0.033
2016	-0.003	0.053	-0.13	0.041	-0.082	0.041	-0.15	0.035	-0.264	0.039
2017	-0.015	0.059	-0.154	0.040	-0.11	0.041	-0.17	0.037	-0.169	0.041
n	94,511		137,222		123,639		139,349		112,145	
nxt	680,825		817,327		641,779		630,572		358,570	
N	3,241,795		3,856,910		4,113,730		4,015,319		3,507,150	
DV Mean	6,794		7,019		7,715		7,715		8,980	
Adj. R^2	0.299		0.294		0.264		0.270		0.240	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 9, Panel 1. Annual formal income is deflated to constant 2018 $BRL, transformed using inverse hyberbolic sine, then regressed on relative time indicators around year of new hire into an oil-linked establishment, with period t as baseline. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B18: First-Time Hires: **Annual Formal Earnings (Less Than High School)**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2006	(base)	-	-	-	-	-	-	-	-	-
2007	0.024	0.066	-	-	-	-	-	-	-	-
2008	0.14	0.065	(base)	-	-	-	-	-	-	-
2009	0.055	0.070	-0.2	0.048	-	-	-	-	-	-
2010	0.154	0.066	-0.136	0.051	(base)	-	-	-	-	-
2012	0.112	0.069	-0.122	0.054	-0.072	0.061	(base)	-	-	-
2013	0.108	0.069	-0.075	0.053	-0.007	0.065	-0.117	0.050	-	-
2014	0.153	0.072	-0.096	0.057	0.015	0.059	-0.124	0.055	(base)	-
2015	0.105	0.080	-0.175	0.059	-0.066	0.069	-0.146	0.061	-0.275	0.057
2016	0.030	0.080	-0.041	0.068	-0.016	0.076	-0.083	0.061	-0.469	0.073
2017	0.079	0.088	-0.181	0.066	-0.064	0.072	-0.174	0.064	-0.25	0.077
n	35,522		45,789		36,737		44,042		36,977	
nxt	257,118		271,637		190,403		198,311		115,009	
N	1,219,971		1,354,170		1,287,639		1,224,211		1,074,898	
DV Mean	4,468		4,535		4,649		4,630		4,422	
Adj. R^2	0.256		0.248		0.207		0.184		0.153	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 9, Panel 2. Annual formal earnings for subsample of workers with less than complete secondary education are deflated to constant 2018 $BRL, transformed using inverse hyberbolic sine, then regressed on relative time indicators around year of new hire into an oil-linked establishment, with period t as baseline. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B19: First-Time Hires: **Annual Formal Earnings (High School Complete)**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2006	(base)	-	-	-	-	-	-	-	-	-
2007	-0.015	0.052	-	-	-	-	-	-	-	-
2008	0.007	0.055	(base)	-	-	-	-	-	-	-
2009	-0.017	0.060	-0.149	0.039	-	-	-	-	-	-
2010	0.004	0.061	-0.067	0.041	(base)	-	-	-	-	-
2012	-0.004	0.062	-0.063	0.041	-0.108	0.048	(base)	-	-	-
2013	0.040	0.061	-0.13	0.044	-0.142	0.041	-0.181	0.036	-	-
2014	-0.001	0.062	-0.083	0.045	-0.132	0.042	-0.167	0.040	(base)	-
2015	-0.023	0.068	-0.091	0.049	-0.147	0.048	-0.147	0.043	-0.206	0.045
2016	-0.008	0.071	-0.211	0.052	-0.156	0.050	-0.211	0.047	-0.21	0.050
2017	-0.114	0.080	-0.153	0.053	-0.18	0.053	-0.183	0.050	-0.155	0.052
n	53,347		83,447		79,361		86,074		67,780	
nxt	390,602		502,728		414,042		389,987		218,539	
N	1,022,482		1,289,402		1,474,166		1,478,128		1,238,914	
DV Mean	5,691		5,899		5,883		6,200		6,623	
Adj. R^2	0.289		0.267		0.238		0.213		0.195	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 9, Panel 2. Annual formal earnings for subsample of workers with complete secondary education are deflated to constant 2018 $BRL, transformed using inverse hyberbolic sine, then regressed on relative time indicators around year of new hire into an oil-linked establishment, with period t as baseline. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

Table B20: First-Time Hires: **Annual Formal Earnings (More Than High School)**

	2006		2008		2010		2012		2014	
Year	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.	Coef.	St. Err.
2006	0.000	-	-	-	-	-	-	-	-	-
2007	-0.022	0.125	-	-	-	-	-	-	-	-
2008	-0.020	0.136	0.000	-	-	-	-	-	-	-
2009	-0.009	0.139	0.145	0.084	-	-	-	-	-	-
2010	-0.029	0.133	0.093	0.094	0.000	-	-	-	-	-
2012	-0.008	0.142	0.003	0.100	0.217	0.095	0.000	-	-	-
2013	0.030	0.150	0.031	0.098	0.218	0.111	0.095	0.080	-	-
2014	-0.050	0.157	0.055	0.097	0.148	0.100	0.120	0.082	0.000	-
2015	-0.130	0.197	-0.061	0.114	0.172	0.101	0.035	0.091	0.111	0.100
2016	-0.419	0.269	-0.146	0.149	0.106	0.120	-0.058	0.099	0.113	0.113
2017	-0.174	0.202	0.016	0.124	0.157	0.148	-0.083	0.103	0.066	0.124
n	5,642		7,986		7,541		9,233		7,388	
nxt	33,105		42,962		37,334		42,274		25,022	
N	260,282		317,018		340,212		322,458		313,782	
DV Mean	20,495		22,310		22,573		29,936		29,497	
Adj. R^2	0.380		0.379		0.360		0.443		0.386	

Note: Table reports coefficient estimates, standard errors in parentheses, and sample statistics corresponding with Figure 9, Panel 2. Annual formal earnings for subsample of workers with more than complete secondary education are deflated to constant 2018 $BRL, transformed using inverse hyberbolic sine, then regressed on relative time indicators around year of new hire into an oil-linked establishment, with period t as baseline. Worker and year fixed effects are included; standard errors are clustered at the matched worker level. Regressions are weighted to account for coarsened exact matching weights generated by CEM package in Stata. All matched workers (employed & unemployed) are retained in sample. n reports the number of matched individuals in that cohort sample; n×t reports number of observations in panel. N reports total number of hired workers in that cohort. DV Mean reports mean of dependent variable in $t-1$ period.

B.6 Oil-Linked Higher Education Degrees

Public higher education institutions are those classified as federal, state, or municipal; private institutions are those classified as private (for- or non-profit) and special. Universities are considered to be those institutions that award bachelors degrees (*bacharelado*) and full and short licensures (*licenciatura plena e curta*). Technical training institutions are those that award technician degrees (*tecnólogo*). To ensure consistency across the 2003-2016 panel, we exclude categories that are only defined in some years, including profession-specific degrees (*específico da profissão*) and short course specializations. In all cases, we include both in-person and distance learning options.

Table B21: Oil-Linked Majors

Oil-Linked Majors	
Petroleum Engineering	Environmental Management
Geological Engineering	Naval maintenance
Naval Engineering	Petrochemical Maintenance
Shipbuilding	Mining & Extraction
Shipbuilding (non-motorized)	Marine Navigation
Naval Construction	Operation of Ships
Environmental Control	Paleontology
Water Pollution Control	Petrology
Extraction of Petroleum & Gas	Processing of Petroleum & Petrochem.
Geoscience	Petroleum Refining
Geophysics	Environmental Cleanup
Geology	Environmental Protection Tech.

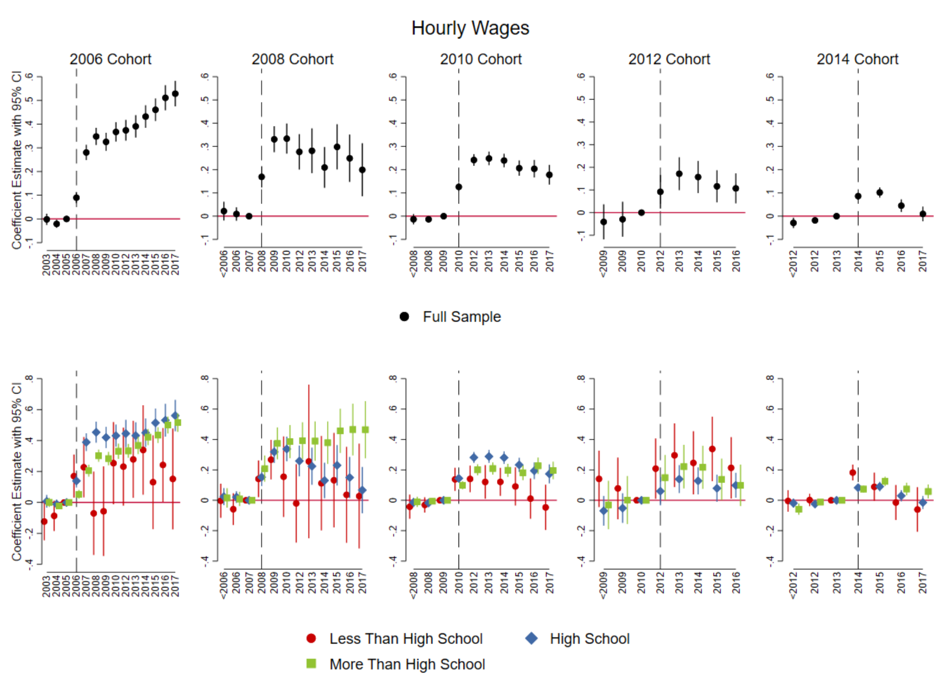
C Robustness Checks

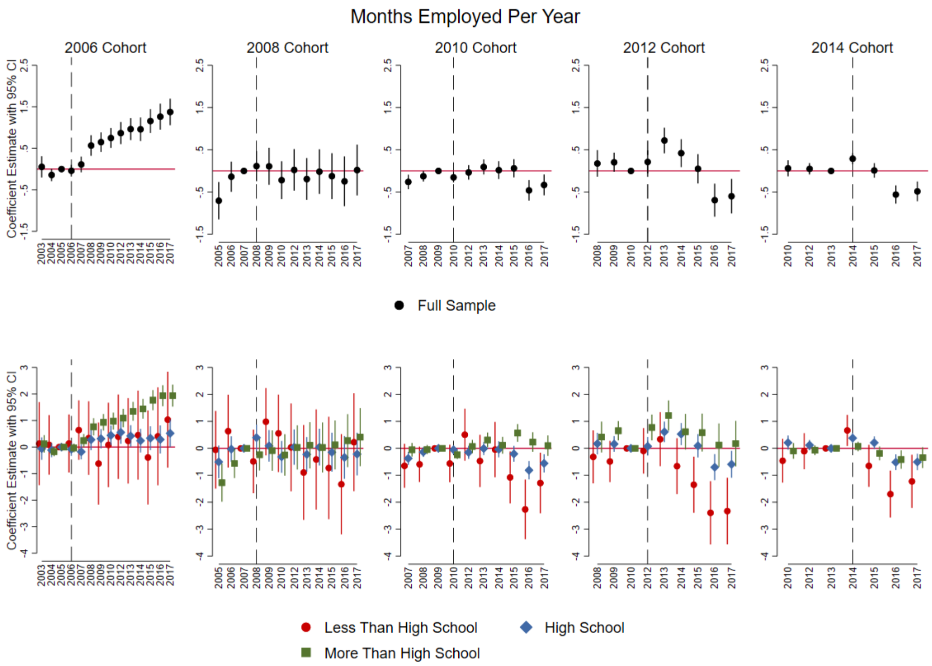
In this section, we test the sensitivity of results to alternative definitions of treatment, model specifications, and estimators. We also evaluate comparability across cohorts.

C.1 Keep only directly oil-linked workers with looser match

We re-estimate event studies using only directly-linked sectors (e.g., petroleum extraction and support activities) and looser matching criteria to retain more treated workers in the sample. Coefficient estimates are larger than under our preferred specification, but do not change our conclusions qualitatively, suggesting our preferred estimates are a lower bound for effects of joining the oil-linked sector. This is intuitive, as workers with closer ties to the booming and busting sector experienced the same trends as our broader sample, but to an exaggerated degree. One exception is that workers in the 2006 cohort with a high school degree experience better outcomes in this sample, suggesting that the premium of college relative to high school education is somewhat smaller in direct oil. With these looser matching criteria, we retain 61% of treated workers in sample – relative to 18% in our preferred specification – ensuring balance with the broader population of oil-linked workers and offering supporting evidence that our main findings are externally valid.

Figure C1: Robustness: Experienced Hires, Hired into Directly Oil-Linked Firms (Loose Match)




Months Employed Per Year
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

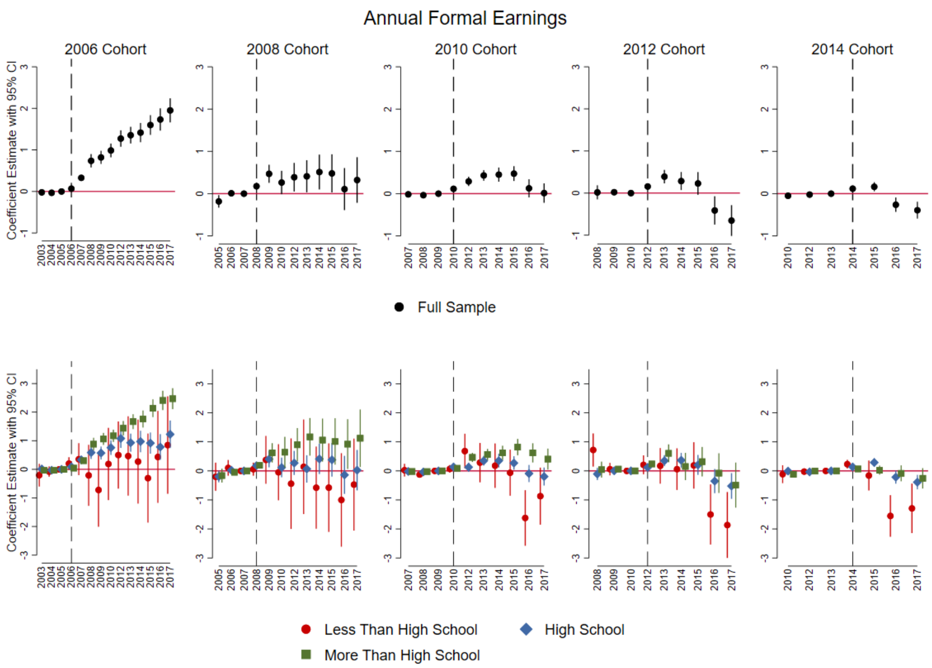
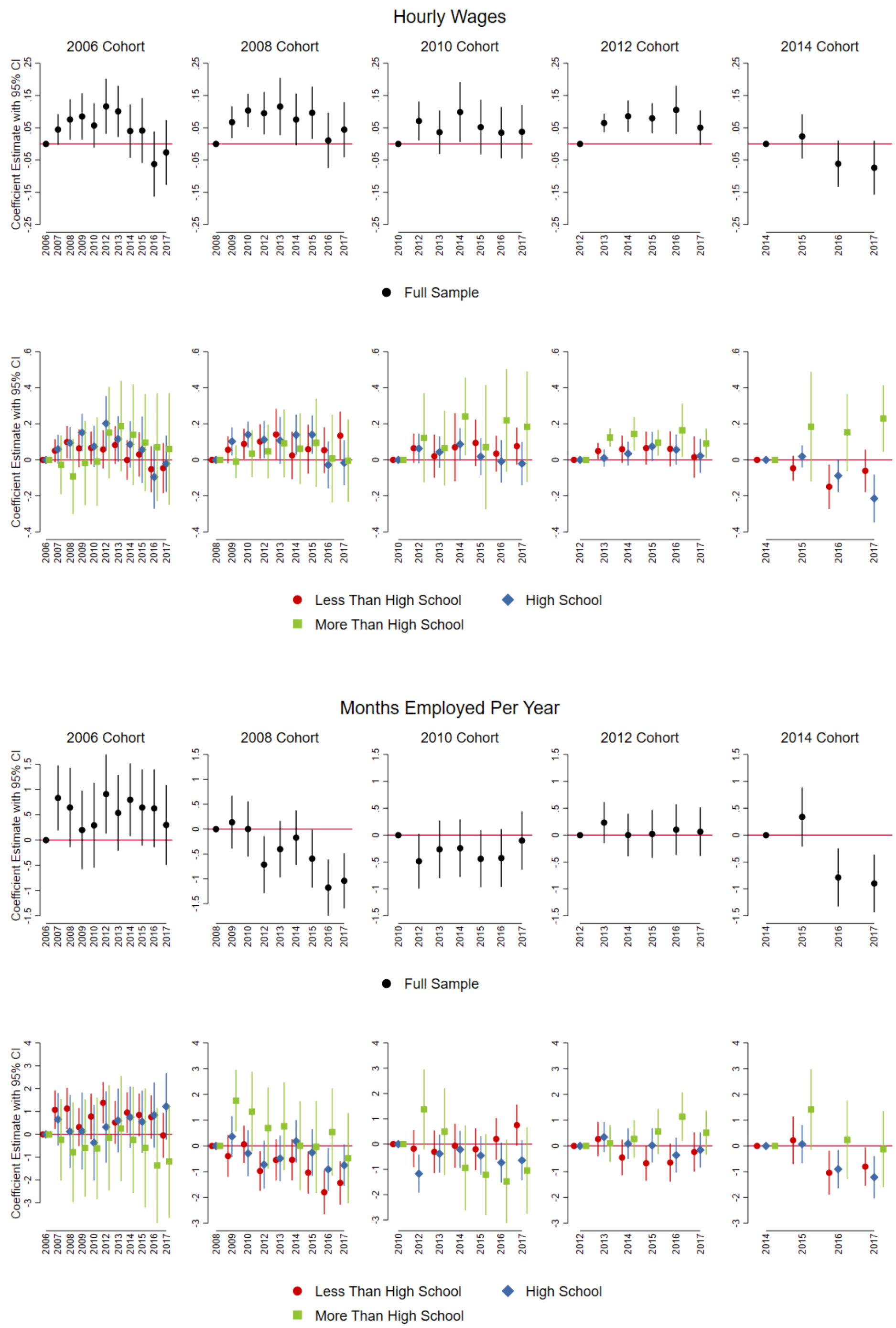
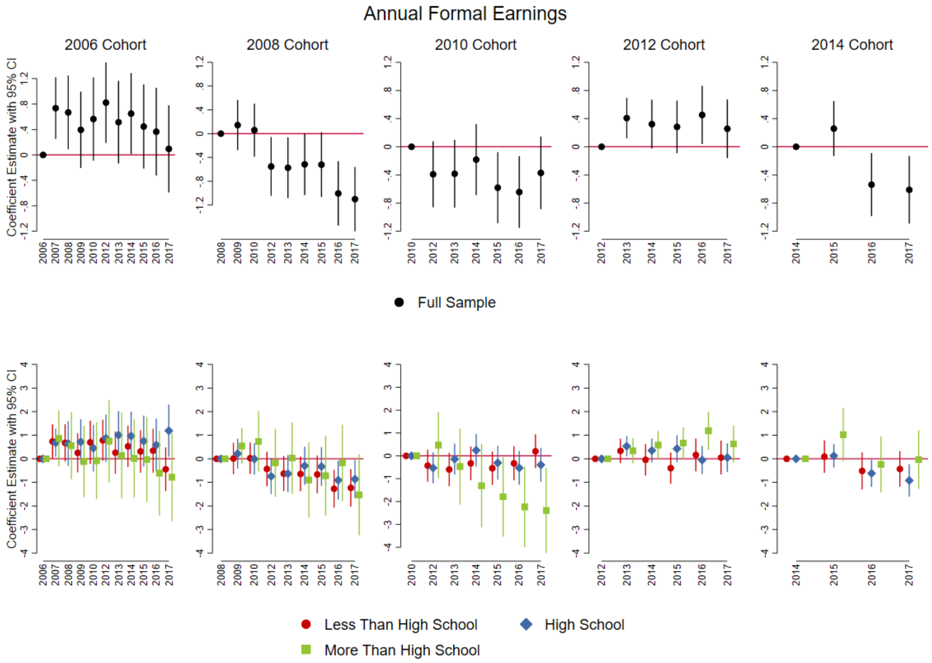

Annual Formal Earnings
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

Figure C2: Robustness: First-Time Hires, Hired into Directly Oil-Linked Firms (Loose Match)




Annual Formal Earnings
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

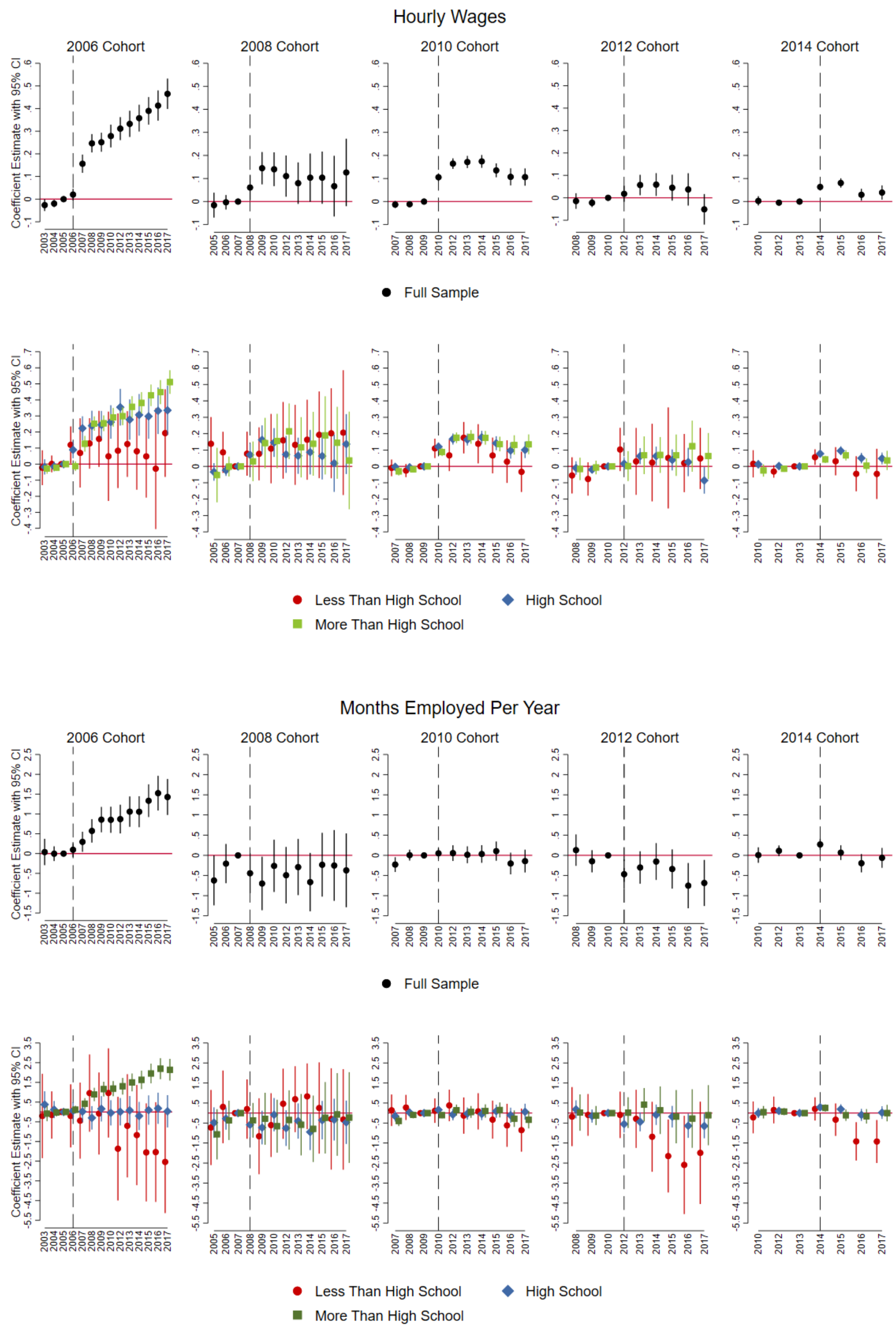
C.2 Keep only workers within 100 kilometers of a shipyard

Brazil is a large country with spatially concentrated hubs of offshore oil activity, which we proxy using the location of shipyards (which serve as assembly nodes in the oil supply chain; see list in Table C1). We re-estimate event studies with matched samples limited to experienced or first-time workers hired into destination municipalities that are within 100km of a shipyard to increase the likelihood that treated workers are truly oil-linked. Coefficient estimates in this robustness check are several times larger than those in our main specifications, but reflect the same trends. This finding is again intuitive: workers closer to oil hubs feel the effects of oil booms and busts more strongly.

Table C1: Oil-Linked Shipyards (PortalNaval, 2020)

Shipyard Name	**Region**	**State**	**Municipality**	**CEP**
Construção e Montagem Offshore - CMO	NE	PE	Ipojuca	55590-972
Estaleiro Atlantico Sul	NE	PE	Ipojuca	55590-970
Vard Promar	NE	PE	Ipojuca	55590-000
Enseada Indústria Naval - Unidade Paraguaçu	NE	BA	Maragojipe	44420-000
Estaleiro Jurong Aracruz	SE	ES	Aracruz	29198-046
Terminal de Serviços e Logística da Barra do Furado	SE	RJ	Quissama	28735-000
Estaleiro Cassinu	SE	RJ	São Gonçalo	24430-620
Navegação São Miguel	SE	RJ	São Gonçalo	24430-500
Estaleiro Alianca	SE	RJ	Niterói	24110-200
Equipemar	SE	RJ	Niterói	24110-205
Estaleiro Brasa	SE	RJ	Niterói	24040-005
Estaleiro Mauá – Ponta D'Areia	SE	RJ	Niterói	24040-290
Mac Laren Oil	SE	RJ	Niterói	24040-260
RENAVE e ENAVI	SE	RJ	Niterói	24110-200
UTC Engenharia	SE	RJ	Niterói	24110-814
Vard Niteroi	SE	RJ	Niterói	24050-350
EISA	SE	RJ	Rio de Janeiro	21920-630
Inhauma	SE	RJ	Rio de Janeiro	20936-900
Brasfels S.A.	SE	RJ	Angra dos Reis	23905-000
Estaleiro Detroit Brasil	S	SC	Itajaí	88311-550
Estaleiro Itajai	S	SC	Itajaí	88305-620
Estaleiro Oceana	S	SC	Itajaí	88311-045
Estaleiro Keppel Singmarine Brasil	S	SC	Navegantes	88375-000
Estaleiro Navship	S	SC	Navegantes	88375-000
RG Estaleiro ERG	S	RS	Rio Grande	96204-040
Estaleiro do Brasil	S	RS	São José do Norte	96225-000

Figure C3: Robustness: Experienced Hires, Hired into Oil-Linked Firms (<100km. from Shipyard)



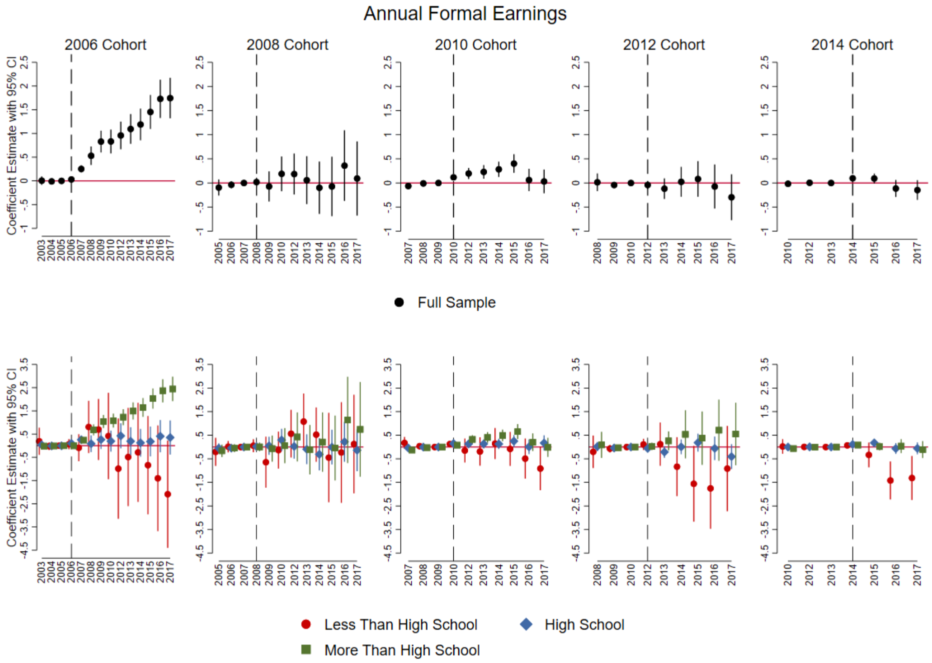
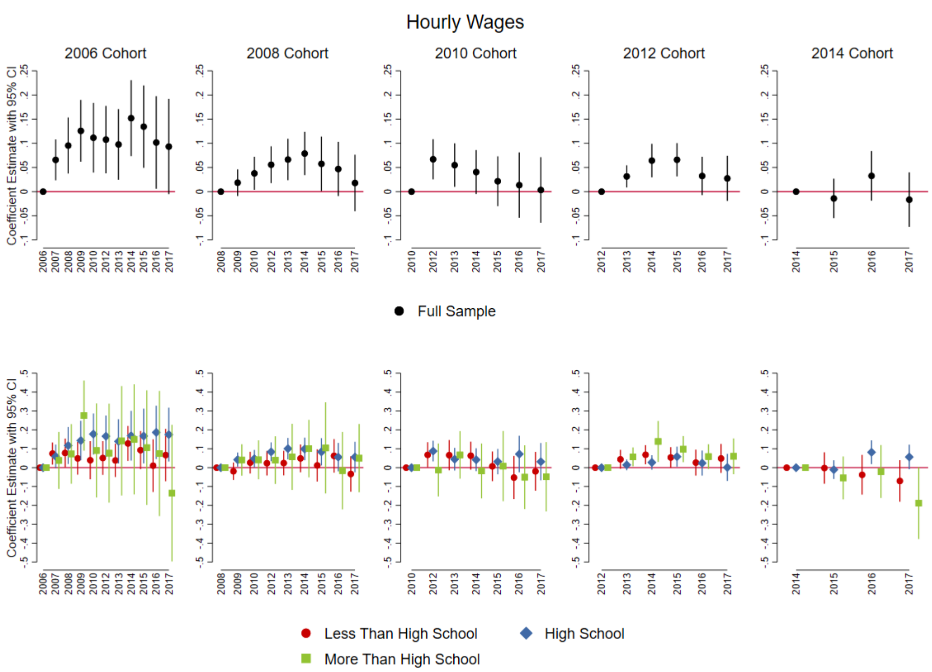
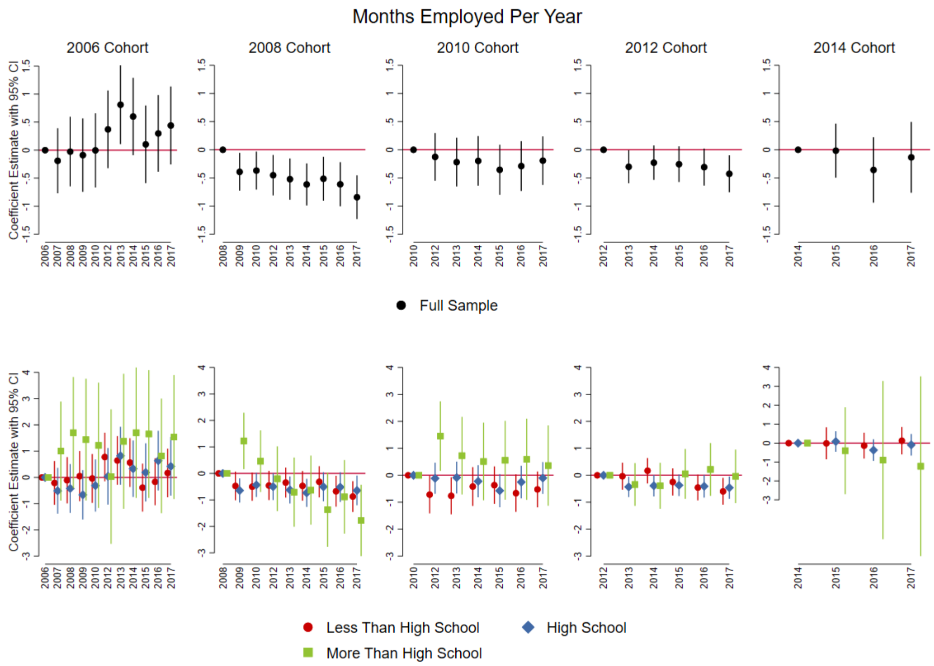


Figure C4: Robustness: First-Time Hires, Hired into Oil-Linked Firms (<100km. from Shipyard)



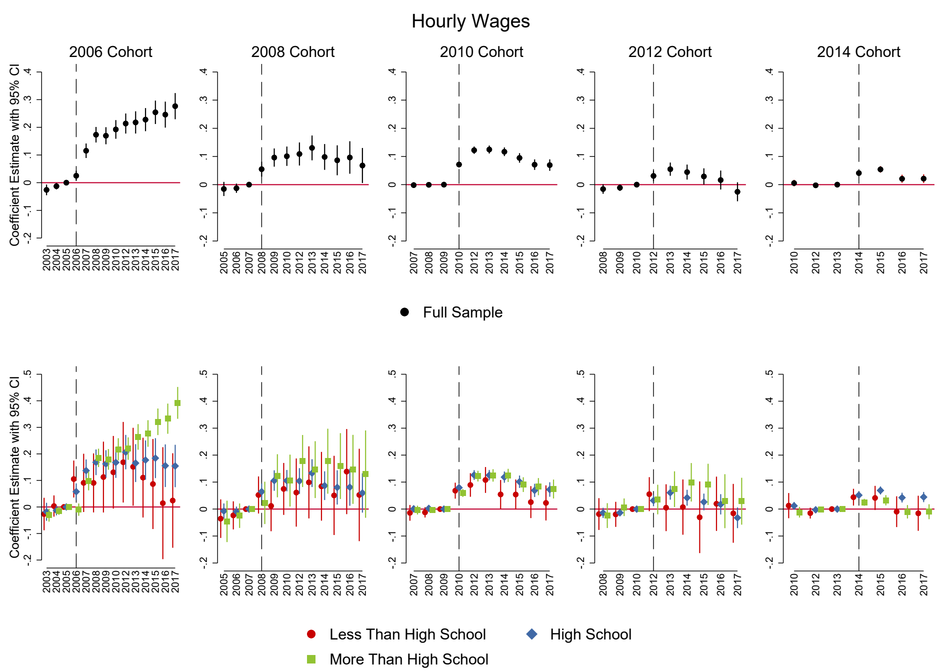

Months Employed Per Year
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

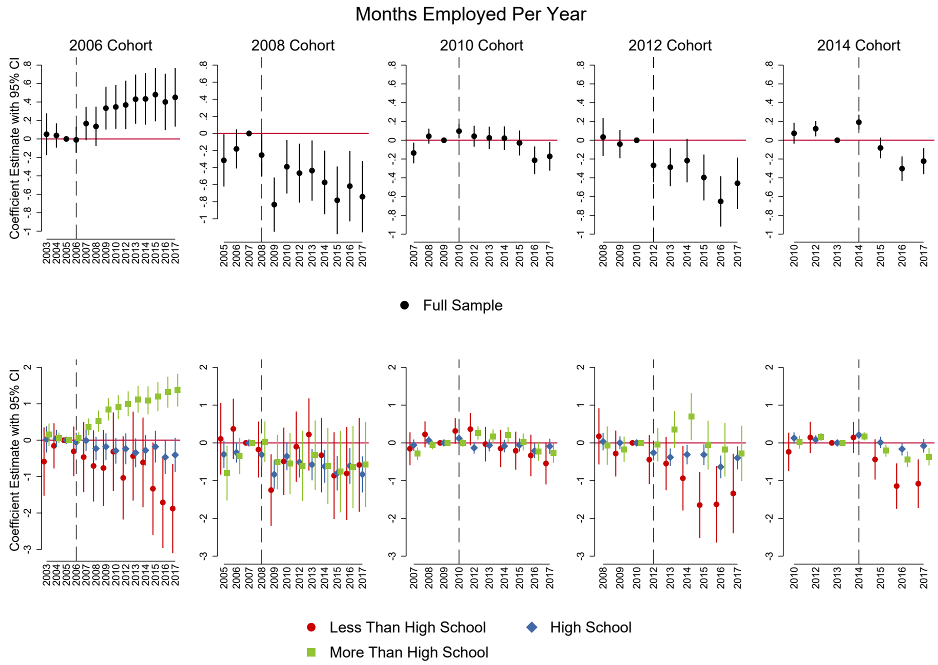

Annual Income
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

C.3 Omit publicly-employed workers (including Petrobras)

Could positive effects on specific subgroups of workers be explained by disproportionate entry into public employment (e.g., Petrobras, Brazil's national oil company), which conveys job stability and may not respond to market signals? Furthermore, workers who select into employment at Petrobras may be more cognizant and tolerant of oil sector volatility, making them less comparable to workers who enter non-oil sectors. We re-estimate event studies omitting publicly-employed workers (approximately 5% of treated workers and 75% of direct oil employees) from the sample. Results remain largely unchanged, confirming that estimated effects are not driven exclusively by (i) a public employment effect, or (ii) self-selection of workers into direct oil employment based on the specific features of the oil industry. In other words, our effects are not driven by systematic differences between workers in terms of choosing to work for a volatile sector: effects persist among upstream and downstream workers for whom the exposure of their chosen sector to oil volatility was much less salient at the time of entry.

Figure C5: Robustness: Experienced Hires, Hired into Oil-Linked Firms (Private Sector Only)




Months Employed Per Year
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

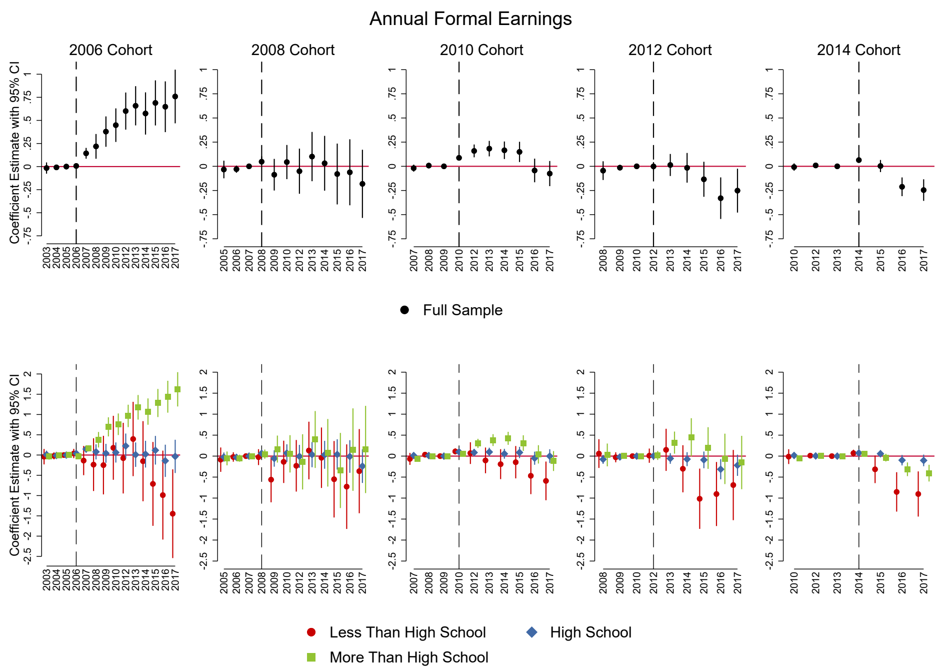
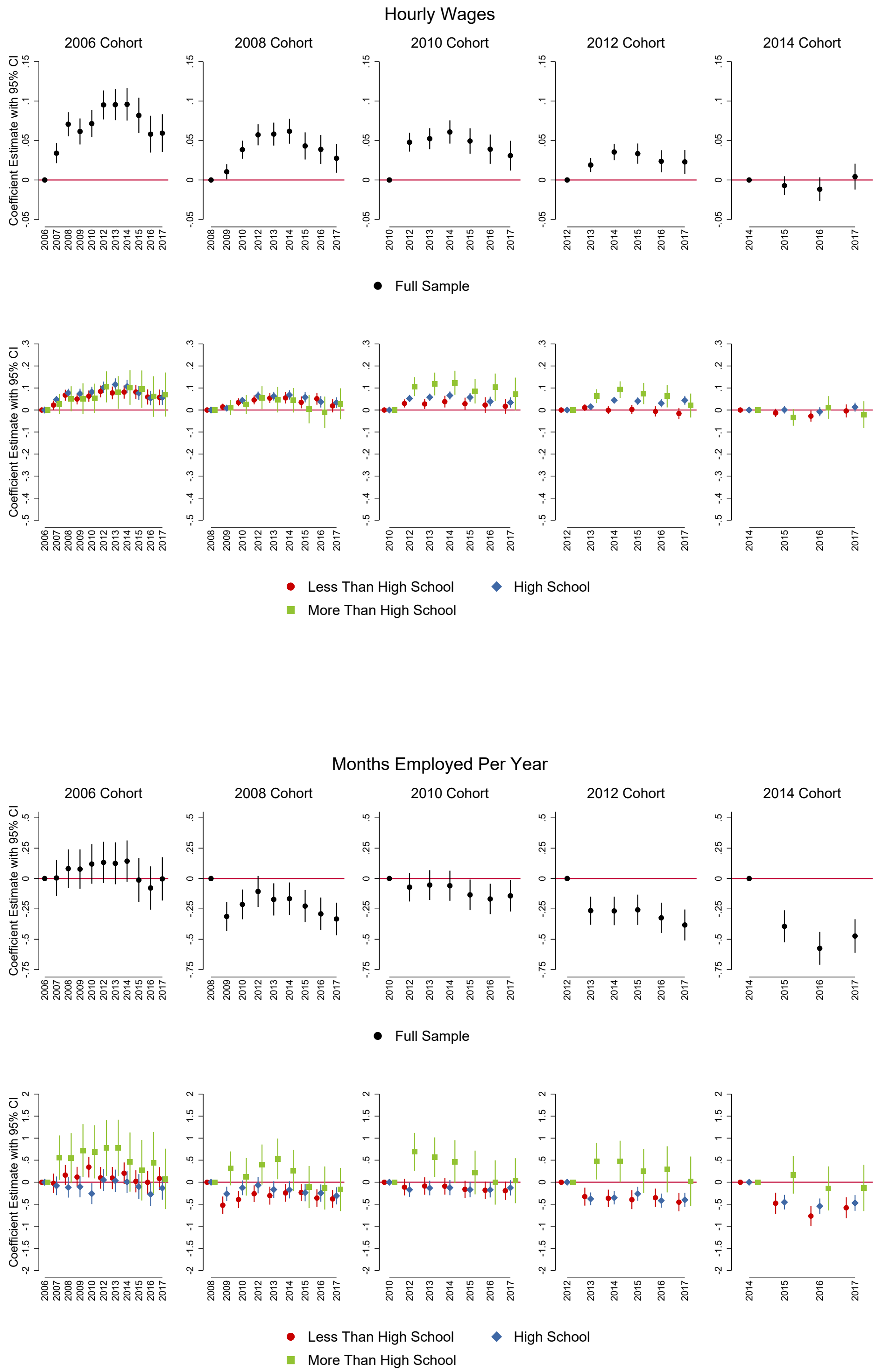
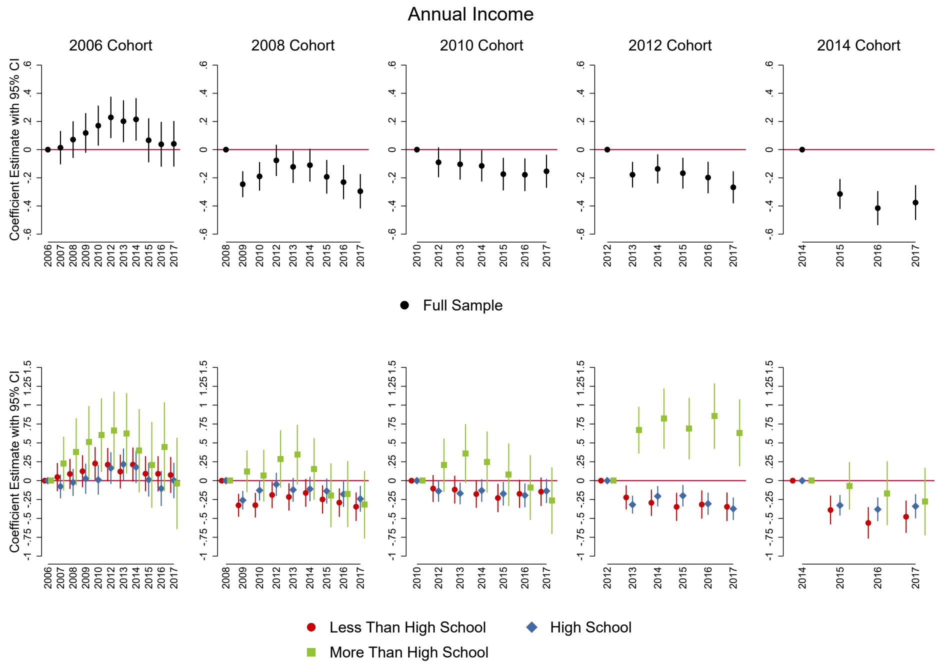

Annual Formal Earnings
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

Figure C6: Robustness: First-Time Hires, Hired into Oil-Linked Firms (Private Sector Only)





Annual Income
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

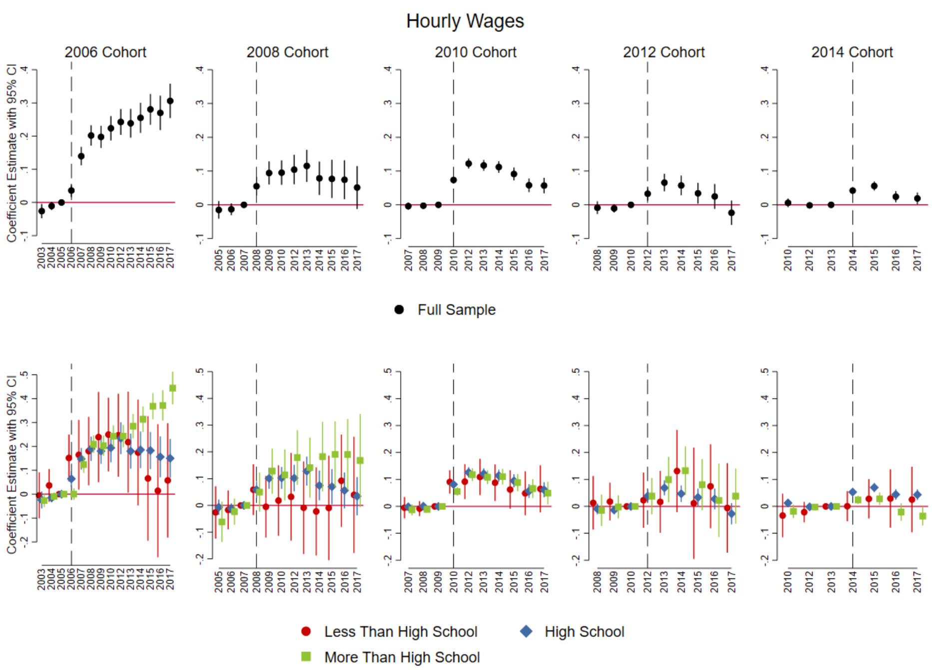
C.4 Evaluate comparability across cohorts

The progression of Brazil's oil boom could induce changes in the composition of cohorts entering the oil sector over time, compromising cross-cohort comparisons. To assess this possibility, we report average education, age, sex, and non-white share for each cohort of experienced and newly hired entrants into oil in Tables B7 and B8, respectively. On average, the 2006 cohort has comparable age and sex composition as later cohorts, but slightly higher education and share of white workers. To assess whether these variations in observable characteristics may underlie differences in labor market outcomes across cohorts, we re-estimate event studies using sub-samples of each cohort that share common support with the baseline 2006 cohort. Specifically, we preserve in sample only individuals from the 2006 cohort and subsequent cohorts who match exactly on education, sex, nonwhite indicator, and age bins. For experienced hires, we also match on previous job wage bins and firm size bins. For first-time hires, we also match on first-job wage bins and firm size bins. Results shown below are very similar to our preferred specifications. Potential differences between cohorts driven by *unobservable* characteristics (e.g., risk preferences or ability) are limited by the unpredictability of Brazil's oil booms and busts, which made it difficult for specific types of workers to foresee the timing of sectoral expansions and contractions.

To further evaluate comparability across cohorts, we assess pre-treatment "labor productivity signals" for workers in each cohort. First, we compute a measure of occupational dynamism for the occupation of origin of each experienced worker who enters oil. This measure assumes a value between 0 and 1 depending on how much employment in the worker's previous occupation grew in the three years prior to their entry into oil, relative to other occupations in the economy. In Table C2 below, we show that early (i.e., 2006) entrants came from slightly *less* dynamic occupations than did workers in later cohorts. Furthermore, workers entering oil in 2006 also came from slightly *lower* positions in the education distribution of their previous establishments – and comparable positions in the wage distribution – relative to later cohorts. Since we already matched workers on their previous establishment, these findings provide evidence that positive labor market outcomes for high-education early entrants were not driven by more productive workers within origin establishments going into oil while less productive workers entered other sectors.

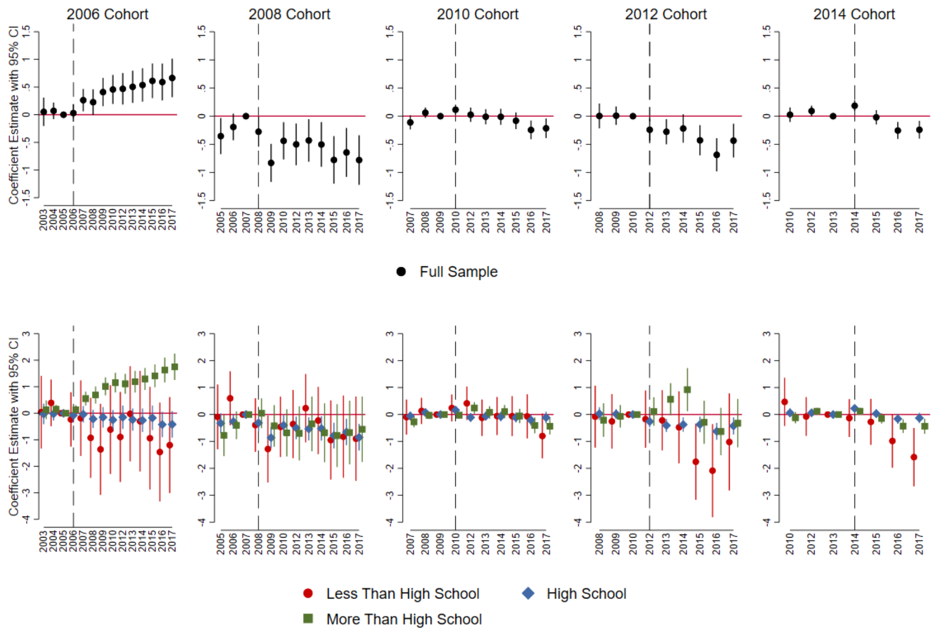
A final concern regarding cohort composition is that the early 2006 cohort may have contained disproportionate numbers of workers with former experience in oil who were "rehired" for the upcoming boom. In Table C2 below, we report the share of workers in each cohort who were previously employed by an oil-linked establishment in any of the three years prior to their switch into oil (this is the furthest we can look back in our data). Indeed, the share of workers with previous oil experience is slightly higher for the 2006 cohort (23% among high-education 2006 entrants versus 17.5% among later high-education entrants), suggesting that the onset of the boom may have first drawn in workers with prior experience in the sector. To ensure that this effect does not drive our main results, we re-estimate event studies excluding workers with any previous experience in an oil-linked establishment. Results, reported in Figure C9, remain very similar to our preferred specification.[22] Finally, in Table C3 below, we report the top five prior occupation groups for each cohort of entrants into oil-linked sectors. Prior occupations are stable across cohorts, dominated by metalworkers and mechanics, laborers from extractive (e.g., mineral and metal mining) and construction industries, industrial machine operators, administrative and office workers, and industrial technicians.

Figure C7: Robustness: Experienced Hires, Hired into Oil-Linked Firms (Common Support Across Cohorts (Baseline = 2006))



[22] As an alternative means of accounting for oil entrants' potential prior oil experience during the 1990s (during which Brazil's oil sector experienced moderate growth), we re-estimate event studies with samples restricted to workers who were 22 years old or less in 2000, thus omitting workers who could have both undertaken higher education and accumulated oil-relevant experience during that period. Results, reported in Figure C10, show that omitting older workers *does* attenuate positive earnings outcomes among high-education 2006 entrants – suggesting older workers captured some of the largest gains from the boom – but *does not* account entirely for high-education early entrants' disproportionately positive labor market outcomes – leaving the main takeaway from our preferred specification unchanged.

Months Employed Per Year



Annual Formal Earnings

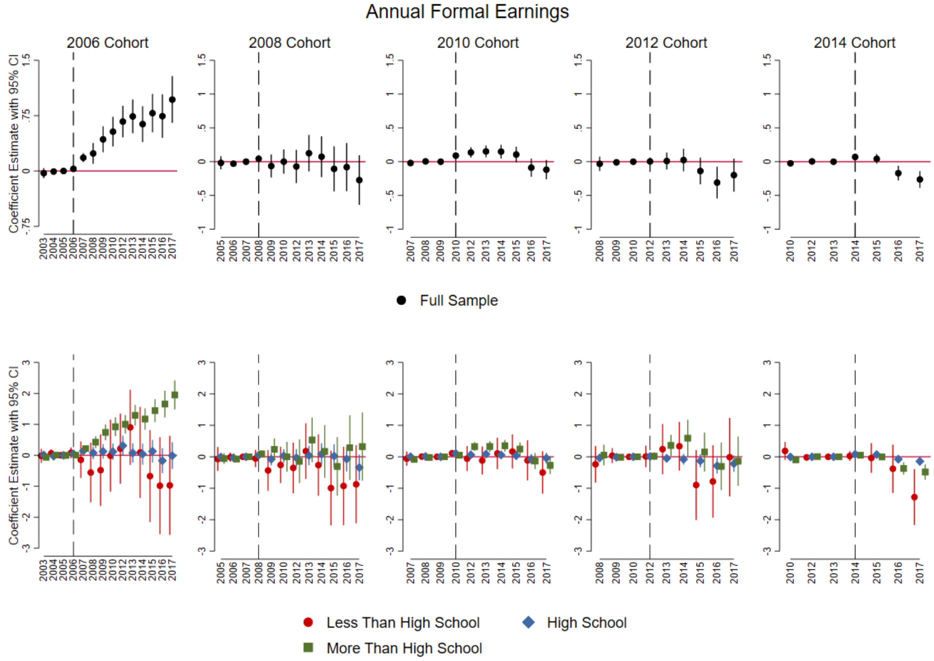
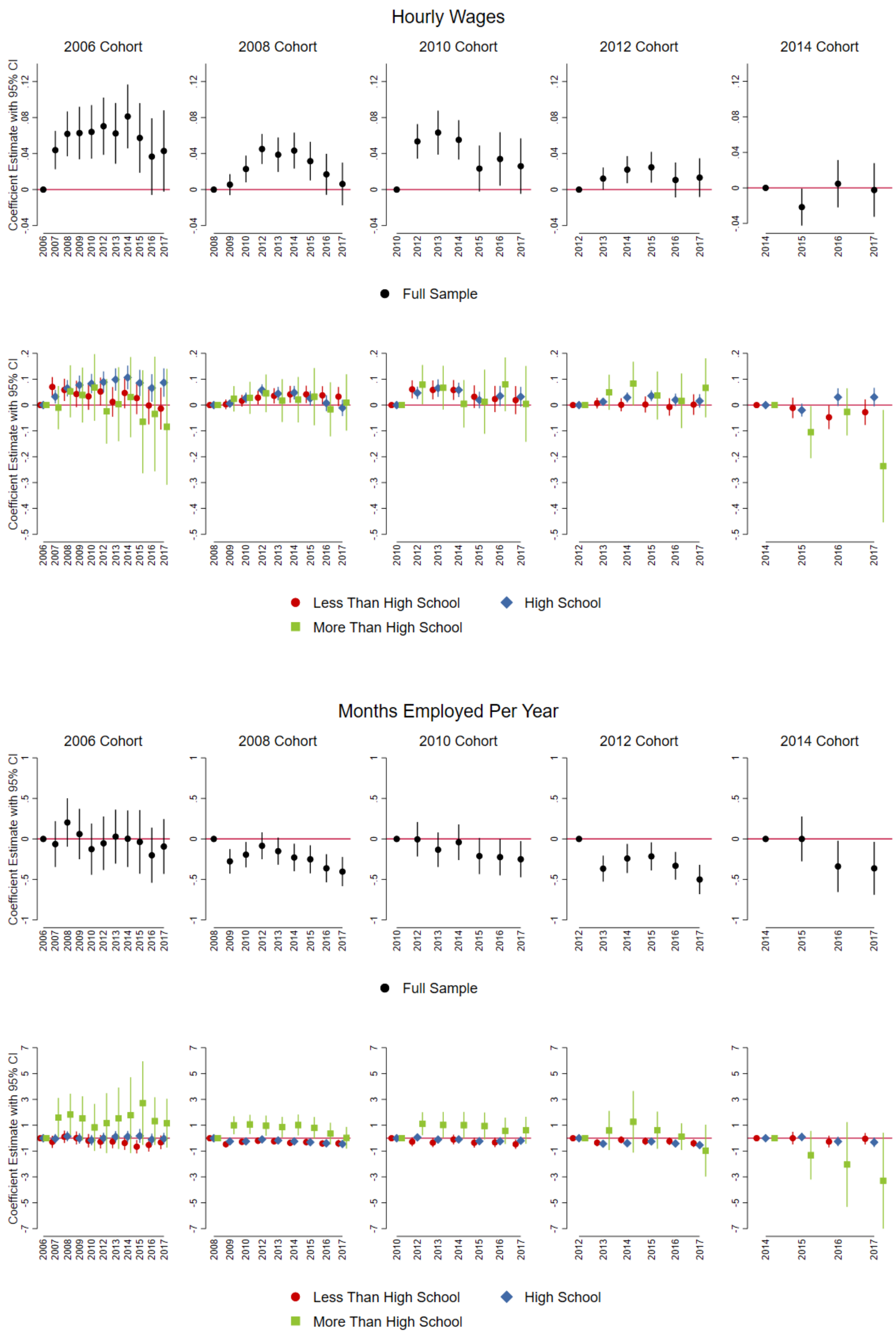


Figure C8: Robustness: First-Time Hires, Hired into Oil-Linked Firms (Common Support Across Cohorts (Baseline = 2006))



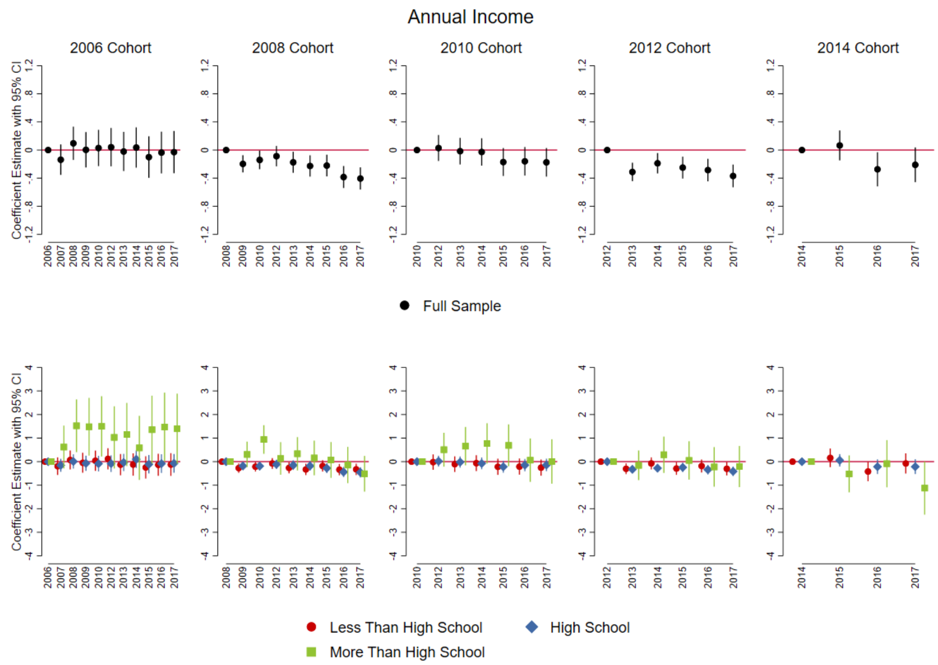

Annual Income
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

Table C2: Cohort Comparison: Prior Employment Characteristics

	Cohorts				
	2006	**2008**	**2010**	**2012**	**2014**
Prior Occupation Dynamism	0.57	0.63	0.60	0.62	0.63
	(0.20)	(0.17)	(0.19)	(0.17)	(0.19)
Education Rank at Prior Firm	0.41	0.45	0.44	0.46	0.44
	(0.20)	(0.19)	(0.20)	(0.18)	(0.19)
Wage Rank at Prior Firm	0.49	0.53	0.49	0.52	0.48
	(0.29)	(0.28)	(0.28)	(0.28)	(0.28)
Prior Oil Experience (Low Ed.)	0.23	0.21	0.18	0.26	0.20
	(0.42)	(0.41)	(0.39)	(0.44)	(0.40)
Prior Oil Experience (Med Ed.)	0.23	0.18	0.14	0.19	0.15
	(0.42)	(0.39)	(0.34)	(0.39)	(0.36)
Prior Oil Experience (High Ed.)	0.23	0.20	0.15	0.19	0.16
	(0.42)	(0.40)	(0.35)	(0.39)	(0.37)

Note: Table reports means with standard deviation in parentheses for matched treated sample in each cohort. Prior occupation dynamism is computed as a normalized 0-1 ranking of how much workers' occupation prior to entering oil grew in the three years prior to their switch, relative to all other occupations in the economy. Education and wage rank in prior firm are normalized 0-1 rankings of how a worker compared to other employees in their previous establishment prior to switch into oil, along dimensions of education level and wage. Prior oil experience is a 0/1 indicator of whether worker was employed in an oil-linked establishment in any of the three years prior to their entry into oil.

Figure C9: Robustness: Experienced Hires (excluding workers with previous oil experience)

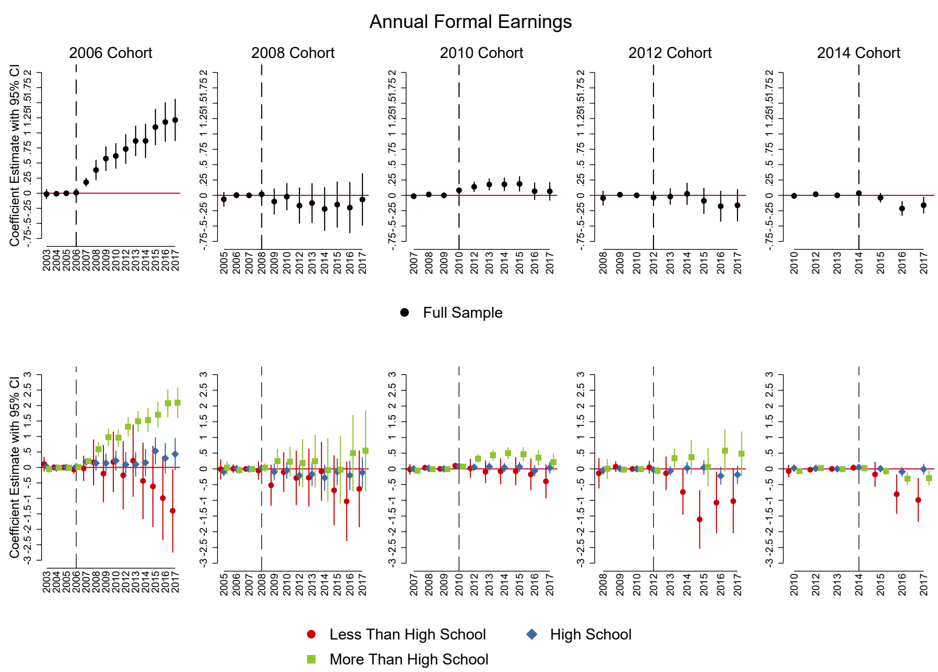


Figure C10: Robustness: Experienced Hires (excluding workers with 1990s labor market experience)

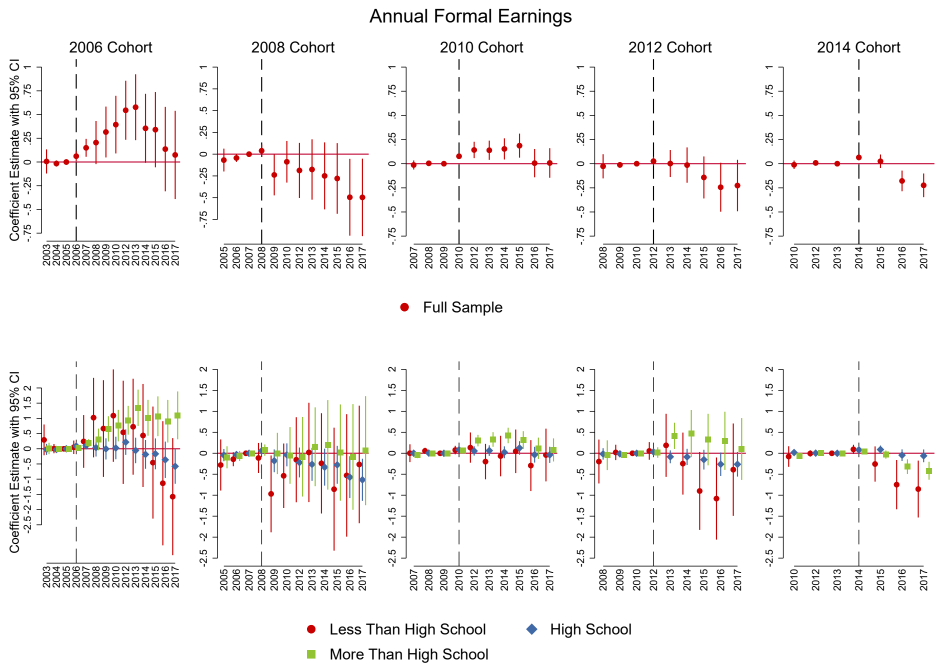


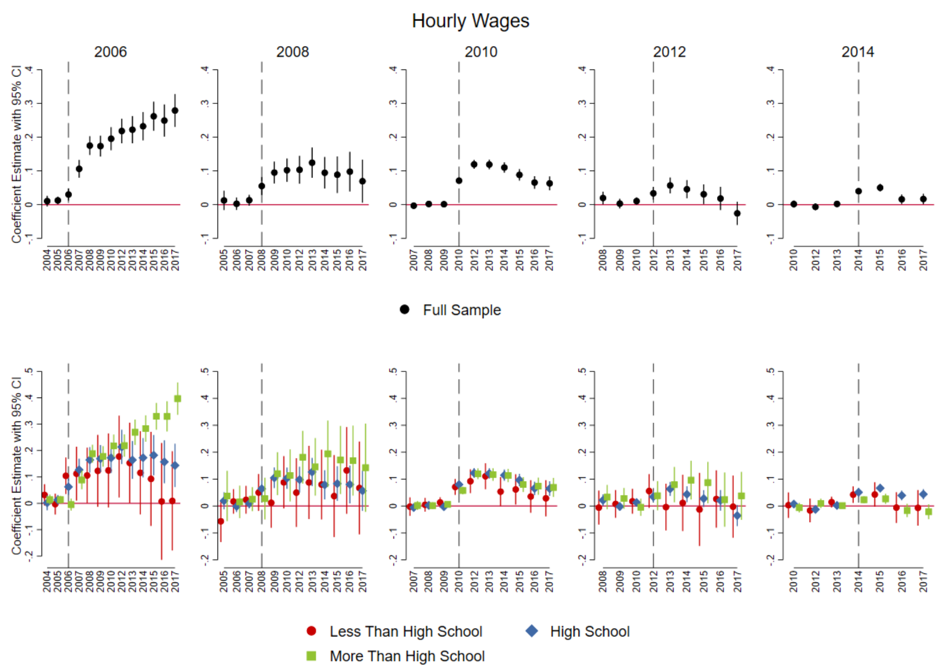
Table C3: Cohort Comparison: Top Prior Occupation Groups

		Top Occupation Groups
2006 Cohort	1	Metalworker/Mechanic
	2	Extractives/Construction Laborer
	3	Industrial Machine Operator
	4	Administrative/Office Worker
	5	Agricultural Worker
2008 Cohort	1	Metalworker/Mechanic
	2	Extractives/Construction Laborer
	3	Industrial Machine Operator
	4	Administrative/Office Worker
	5	Industrial Technician
2010 Cohort	1	Metalworker/Mechanic
	2	Extractives/Construction Laborer
	3	Industrial Machine Operator
	4	Administrative/Office Worker
	5	Industrial Technician
2012 Cohort	1	Extractives/Construction Laborer
	2	Metalworker/Mechanic
	3	Industrial Machine Operator
	4	Administrative/Office Worker
	5	Industrial Technician
2014 Cohort	1	Metalworker/Mechanic
	2	Extractives/Construction Laborer
	3	Industrial Machine Operator
	4	Administrative/Office Worker
	5	Industrial Technician

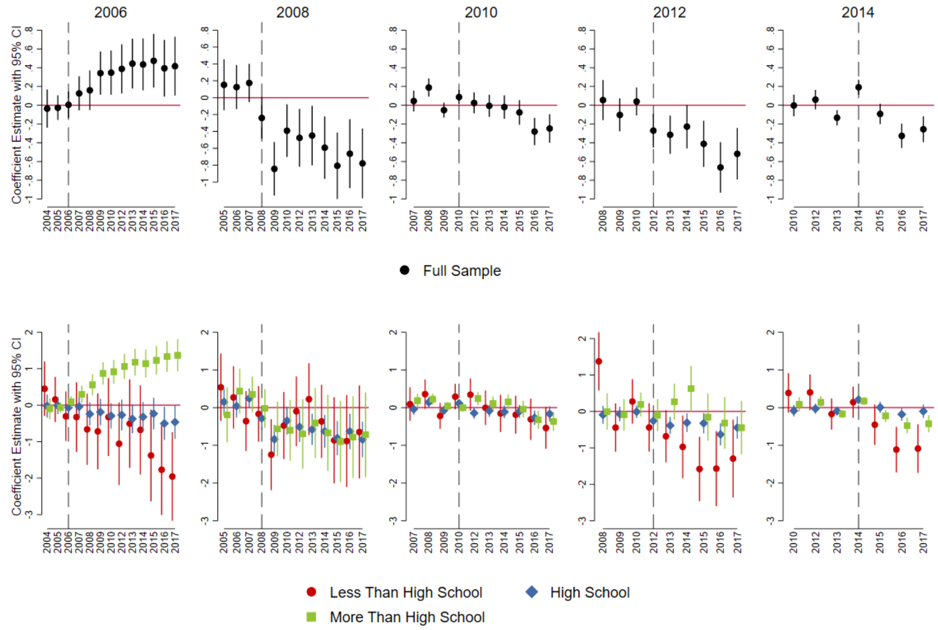
C.5 Implement Callaway and Sant'Anna (2021) estimator

By estimating event studies separately for each cohort using not-yet-treated controls, we avoid bias from inclusion of already-treated units that plagues two-way fixed effects estimation with staggered treatment timing (Goodman-Bacon, 2021). Nevertheless, dynamic and heterogeneous treatment effects may still introduce bias into our ATT estimates (de Chaisemartin and D'Haultfœuille, 2020). To address this threat, we re-estimate event study specifications using the estimator proposed in Callaway and Sant'Anna (2021). As reported in Appendix C.5, results closely resemble our preferred specification in sign, significance, and magnitude.

Figure C11: Robustness: Callaway and Sant'Anna (2021) *csdid* estimator



Months Employed Per Year



Annual Formal Earnings