Labor Reallocation, Human Capital Investment, and “Stranded Careers”: Evidence from an Oil Boom and Bust

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Abstract

Sectoral expansions and contractions have significant worker-level and distributional consequences. Using linked employer-employee panel data from Brazil–a country that experienced oil booms and busts during the 2000s and 2010s–we estimate dynamic effects of being hired into the volatile oil and gas sector on workers' subsequent wages, employment, and earnings. We find that oil generates inequality both between and within worker cohorts. Highly-educated early entrants capture nearly all the earnings benefits of the oil boom and are insulated from downturns by seniority and accumulation of institutional knowledge in professional roles within firms. Later high-education entrants must compete with a glut of new graduates from oil-specific degree programs, and suffer from “stranded careers” after oil busts. Low-education workers never enjoy earnings premiums during booms and lose their jobs during busts. Our findings contribute evidence on the job-creation potential of energy sectors and the distributional consequences of energy transitions.

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1 Introduction

The business cycle rarely affects all sectors equally, leading to a continuous process of labor reallocation in which workers leave declining sectors and join booming sectors. This process is far from frictionless, and may thus have long-run effects on workers. Search and matching costs (Pissarides, 2014), skill loss during unemployment (Edin and Gustavsson, 2008; Ortego-Marti, 2017), and skill mismatch between declining and expanding sectors (Wasmer, 2006; Şahin et al., 2014) can set displaced workers onto persistent negative trajectories or knock them off career ladders (Jarosch, 2021; Von Wachter, 2020).

Given these frictions, workers' choice of sector and timing of entry determine returns on human capital investments, with implications for lifetime earnings and employment (Kahn, 2010; Hombert and Matray, 2019). Workers who possess sector-specific skills and enter early as a sector expands can enjoy significant earnings premiums, which may get locked-in as these workers accumulate hold-up power (e.g., institutional knowledge) and seniority (Bloesch, 2021). Workers (especially those with low education) who enter a sector immediately prior to or during its decline often experience significantly reduced earnings and employment (Altonji et al., 2012; Davis and von Wachter, 2012).

To what extent do labor reallocation frictions harm workers and contribute to inequality? Can these frictions help explain the negative effects of volatile commodity sectors on growth and development, i.e., the resource curse?[1] Canonical models of booming sectors and Dutch Disease (Corden and Neary, 1982) assume frictionless labor markets and therefore fail to capture important worker-level phenomena, including stranded careers, low returns on human capital investment, persistent unemployment, and timing bias in job entry. We leverage matched employer-employee panel data on the universe of formal workers in Brazil to analyze these phenomena and their distributional consequences, revealing worker-level mechanisms underlying the resource curse.

Specifically, we study the long-term effects of labor reallocation into and out of an especially volatile natural resource sector: oil and gas. Evidence on the labor market impacts of expansion and contraction of the oil and gas sector is especially relevant for commodity dependent countries,[2] but also informs broader discussions of sector-specific labor market shocks (Autor et al., 2014), skill-biased structural change (Buera et al., 2015), and distributional consequences of energy transitions (Sharma and Banerjee, 2021).

[1] For comprehensive reviews of the resource curse literature, see van der Ploeg (2011) and Deacon (2012).

[2] Seventy-two low and middle-income countries, home to nearly 3.4 billion people, were export-dependent on natural resource sectors in 2016; 63 of these countries became more resource-dependent between 1996-2016 (Roe and Dodd, 2016).

Brazil recently experienced a dramatic boom (2006-2013) and bust (2014-2017) in its oil sector, driven by changes in global energy prices and domestic offshore discoveries. These relatively unpredictable developments led to a significant expansion and later contraction in oil-linked employment, including direct employment in the oil sector and indirect employment in closely-related upstream and downstream sectors. Using worker-level panels spanning from 2003-2017, we estimate dynamic wage, employment, and earnings effects of exposure to Brazil's oil boom and bust on two types of entrants into oil-linked sectors. First, we focus on poached workers (defined as workers who voluntarily leave their previous firm and are promptly rehired into oil), corresponding with the standard conception of workers who are drawn from other sectors into the resource sector in Corden and Neary (1982)'s model of Dutch Disease. Second, we focus on new hires (defined as workers hired to their first formal job), who, in contrast to poached workers, make education decisions in response to anticipated sectoral dynamics. We estimate earnings and employment effects separately for distinct cohorts of poached and newly hired workers and trace their distinct experiences over boom and bust cycles.

Methodologically, we identify the effects of entry into the oil and gas sector by constructing counterfactual control groups based on matching similar workers who entered non-oil sectors. Brazil's rich linked employer-employee administrative data allow us to impose strict coarsened exact matching criteria (Iacus et al., 2012), restricting control workers to those who are poached or newly hired into other sectors in the same year that their treated counterparts are hired into oil, and who are comparable along dimensions of education, sex, race, age, wage, municipality, and prior labor market experience. Non-parametric matching, in conjunction with panel data methods and a dynamic difference-in-differences strategy, reduces potential bias from endogenous selection-into-treatment without imposing model dependence (Ho et al., 2007). Together with the exogeneity and relative unpredictability of global energy price changes and offshore oil and gas discoveries, this strategy allows us to identify dynamic treatment effects of being poached or newly hired into oil on lifetime labor market outcomes.

We find that timing of entry into oil has major consequences for labor market outcomes. Workers poached into oil at the boom's onset in 2006 enjoy sustained wage and earnings growth relative to matched controls, even through a brief sectoral downturn in 2008 and a broader bust beginning in 2014. Poached workers in this cohort enjoy 31.6% higher hourly wages and average cumulative earnings premiums equivalent to 763% of baseline average annual earnings by 2017. In contrast, workers hired into oil-related sectors in 2008 are immediately hit by the 2008-2009 oil price crash provoked by the Global Financial Crisis, leading them to be employed an average of 57% fewer months in 2009 relative to matched controls who were poached into other sectors. The 2008 cohort never recovers from this early shock, earning 41.5% of baseline average annual earnings less than matched controls by 2017. The average worker poached in 2010 enters near the peak of the oil boom and earns 58.3% of baseline average annual income more by 2017. Finally, on average workers poached in 2012 and 2014 are employed 39.7% and 22.6% fewer months per year by 2017, relative to matched workers poached into non-oil sectors in these years. Summing up, early entrants into the oil sector (2006 cohort) captured nearly all earnings and employment benefits of Brazil's oil boom (totalling 107.3% of net gains by 2017, since subsequent cohorts experienced net losses on average). Later cohorts (with the modest exception of the 2010 cohort) experienced significantly worse cumulative employment and earnings outcomes than their matched controls. Thus, at the worker level, the commodity cycle benefited only a select few early entrants and left most later entrants stranded, unable to find work outside oil-related sectors during or after the bust.

Disaggregating oil-sector exposed workers by level of education reveals significant heterogeneity in labor market outcomes across workers with low (less than secondary), medium (secondary), and high (more than secondary) schooling: Low-education workers poached into oil-related sectors realize negative earnings effects across all cohorts, while high-education workers realize positive effects across all cohorts except 2014, which marks the onset of the bust. Negative earnings effects for low education workers are driven by the extensive margin: for example, low-education poaches into oil in 2006 are employed for 85.5% fewer months per year than matched controls in 2017.

The effects of job-entry timing into oil-related sectors are broadly similar for workers who are newly hired into the oil sector for their first formal job, rather than poached from previous employers. However, there are important differences. Among new hires, the average new entrant into oil at the beginning of the oil boom in 2006 cohort enjoys positive cumulative earnings effects totalling 93% of baseline average annual earnings by 2017, while all later cohorts earn less than matched workers who were newly hired into other sectors. High-education workers experience monotonically declining returns to being newly hired into oil as the commodity cycle progresses.

Although the average early-entrant new hire experiences positive earnings effects from joining an oil-related sector, they do not realize the dynamic wage and earnings growth realized by early-entrant poaches, suggesting that experienced poached workers enter firms on a different career ladder and through a segmented labor market. We explore this mechanism by re-estimating event studies with the outcome variable an indicator of whether the worker is employed in a (i) management or (ii) professional occupation. We find that poached workers in 2006 (and to a lesser extent 2010) are significantly more likely to be employed in professional roles (e.g., engineer, analyst, researcher) relative to matched controls, while poaches in 2008, 2012, and 2014 are no more likely to be employed in these professions when compared to their counterfactual matches. The 2006 poaches are significantly less likely to be in management positions, indicating that workers who were poached into oil at the beginning of the boom entered knowledge sectors and may have accumulated institutional knowledge of production that conveyed on them hold-up power and thus a share of oil boom rents. Since the average new hire is younger than the average poached worker, this worsens inequality.

Another contrast between poached and newly hired workers is that high-education poached workers in 2010 still enjoy significant earnings benefits from oil, while earnings premiums for high-education new hires decline monotonically across cohorts. We propose and provide evidence for a mechanism to explain this contrast: endogenous growth in oil-specific higher education in response to the oil boom, which led to a glut in the market for new oil hires.Using administrative data on the universe of higher education institutions in Brazil, we document that Brazil's 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, which increased from 82 graduates in 2003 to 12,177 in 2015 before falling to 8,500 in 2016 in delayed response to the bust. Many technical programs were organised by oil industry groups with the specific goal of ensuring an affordable supply of skilled workers. 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.

This paper adds to existing evidence on the effects of sectoral booms and busts on labor market outcomes. Hombert and Matray (2019) study the long-term earnings of skilled workers in the French IT sector and find they earn less than similar workers in other sectors due to rapid skill obsolescence. Autor et al. (2014) show sector-specific declines caused by trade exposure to China lead to negative earnings effects (especially on low-wage workers) in the United States. We extend this literature by exploiting an especially clear context: cohorts of workers who enter oil in well-defined ways (poach and new hire) at different times relative to an exogenous boom and bust cycle. Further, we document that a skill-biased oil boom provoked rapid growth in oil-linked higher education, adding nuance to previous findings that a booming sector reduces higher education in aggregate (Charles et al., 2018). Our finding that high-education workers are more likely to remain employed through the oil bust aligns with Beuermann et al. (2021), who show female workers in Barbados are less likely to lose their jobs during the COVID-19 pandemic if they are highly-educated.

Finally, our paper extends canonical booming sector models (e.g., Corden and Neary (1982)) to contribute novel and nuanced evidence about the resource curse. Literature on the resource curse has increasingly shifted from country-level to subnational analyses (Cust and Poelhekke, 2015; Jacobsen and Parker, 2016; Aragón and Rud, 2013), but continues to focus overwhelmingly on places rather than people. One exception is Jacobsen et al. (2021), who use household-level longitudinal data from the US to show that workers exposed to the oil boom and bust of the 1980s experienced reduced earnings and delayed retirement. However, the study is limited by survey size and does not explore heterogeneity in job-entry timing or worker education levels. Kovalenko et al. (2019) links school and employment records in Texas to measure the effects of local fracking booms on education and employment outcomes, finding that booms lead to less human capital accumulation but higher earnings over the medium term. Our study complements this work by exploring heterogeneity in labor market experiences by timing of entry and education-level across a full boom and bust cycle. Further, we document that both students and firms respond to the oil boom by increasing oil-linked higher education, making us one of the first studies to explore education responses to a resource boom at the degree-level, rather than in aggregate (Balza et al., 2021). In relation to the rich literature on Dutch Disease (Pelzl and Poelhekke, 2021; Smith, 2019), we show that, in the context of a middle-income country with significant informal employment, expansion of the oil sector mostly absorbs workers from unemployment or informality, rather than poaching them from other sectors. This reduces the scope for crowding out of tradeable sectors (a typical prediction of Dutch Disease models that assume full employment and inelastic labor supply). In contrast, we show the average worker is cursed by their choice to enter the oil sector and that heterogeneity in job-entry timing creates significant and persistent earnings inequalities.

The remainder of this paper proceeds as follows. In section 2, we outline an analytical framework that extends Corden and Neary (1982)'s booming sector model. In section 3, we explore the context of Brazil's oil boom and bust. In section 4, we describe data and sample construction. In section 5, we present our empirical strategy and discuss identification. In section 6, we present results. In section 7, we detail robustness checks. In sections 8 and 9, we propose and explore mechanisms underlying observed labor market outcomes. In section 10 we discuss our findings and conclude.

2 Analytical Framework

The canonical booming sector model developed by Corden and Neary (1982) provides a starting point for assessing labor market impacts of expansion and contraction in a resource sector. In this model, a small open economy consists of resource, tradeable, and nontradeable sectors. The pre-boom equilibrium is characterised by full employment, with homogeneous workers supplying labor inelastically. When triggered by a sector-specific shock, the resource sector expands, bidding up wages to draw workers away from other sectors (e.g., poaching, or what Corden and Neary refer to as the "resource movement effect"). Resource windfalls and the expansion of resource-sector employment increase demand for local nontradeable goods and services (e.g., construction, retail), raising wages and labor demand in the nontradeable sector (the "spending effect"). Tradeable sectors (e.g., manufacturing, commercial agriculture) cannot pass higher wages through to output prices as they face perfectly elastic demand. These sectors are thus crowded out unless countervailing forces act on them, such as agglomeration effects or other positive spillovers between the resource sector and tradeables. One example of positive spillovers could be backward linkages from the resource sector to tradeables, such that a resource boom stimulates local tradeable firms.

We propose extensions to this basic framework, which we explore empirically using our rich administrative dataset. First, in contrast to the booming sector model's assumption of full employment, many labor markets are characterised by high effective unemployment and informal employment (Meghir et al., 2015). Labor markets may therefore have substantial slack, allowing a booming resource sector to absorb unemployed and informal workers rather than pulling them from other sectors. Unemployment or informality thus reduce the resource movement effect, which may already be limited if the resource sector generates little direct employment. On the other hand, if the resource boom is skill-biased, firms may leapfrog unemployed or informal workers to poach skilled workers from other sectors. In section 3, we document that Brazil's expanding oil sector pulled substantially more workers from unemployment and informal sectors than it poached or newly hired.

Second, the standard booming sector model supposes homogeneous workers, whereas real labor markets are characterised by significant heterogeneity in worker education and skills. With variation in worker ability and job skill requirements, it may be more time-consuming and costly for firms to fill high-skill positions than low-skill positions (Albrecht and Vroman, 2002; Dolado et al., 2009). In Albrecht and Vroman (2002)'s model of a labor market in which workers vary in ability and jobs in skill requirements, skill-biased technical change increases wage-dispersion both within and between high and low-skill workers and increases unemployment among low-skill workers. Likewise, workers filling critical (e.g., skilled) roles in the production process may accumulate hold-up power, thus commanding higher wages and a larger pass-through share of firm rents (Bloesch, 2021). In this context, firms may be more likely to lay off easy-to-replace low-skill workers during a downturn and retain hard-to-replace, critical high-skill workers. Indeed, we document that high-education early-entrants into Brazil's oil sector disproportionately enter skilled professional roles and command wage increases throughout the boom and bust, while low-education workers never command wage premiums and lose their jobs during busts.

A third extension to the booming sector model is labor market regulations, such as minimum wages, employee benefits and protections, and layoff penalties. Regulations of this kind are widespread across both developed and developing countries (Betcherman, 2015). In the presence of a binding minimum wage, a resource bust may leave low-skill workers' wages unaffected but push more of them into unemployment (Cockx and Ghirelli, 2016). Labor protections that increase with tenure may create seniority bias, leading firms to shed new workers before more senior ones. We show that low-education workers who retain jobs through the Brazilian oil bust do not suffer wage cuts, while many low-education workers fall out of the formal labor market and experience significant negative effects on earnings. Furthermore, we show that early entrants are more likely to retain jobs after the bust, while later entrants are laid off, creating a last-in, first-out dynamic.

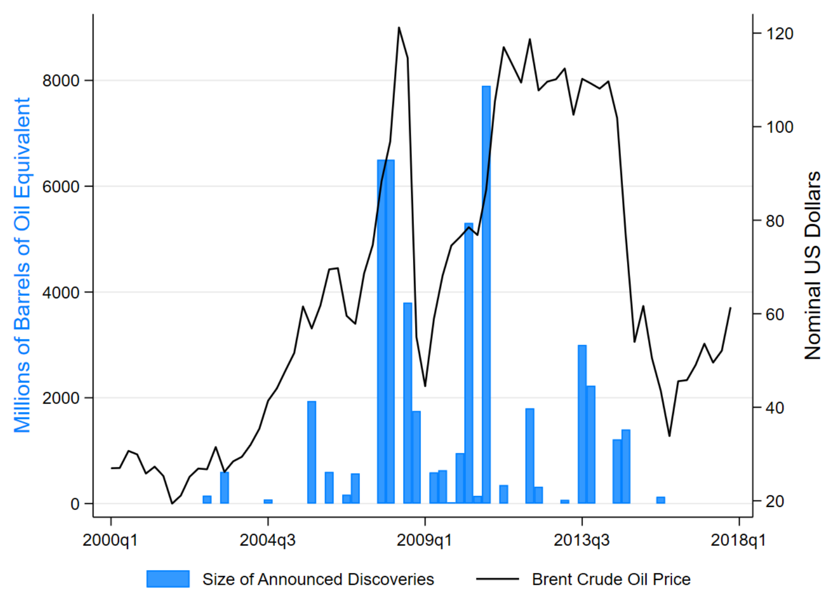
Finally, the booming sector model assumes all workers in the economy are already present in the pre-boom equilibrium. In reality, workers are constantly ageing or graduating into the labor market across the boom and bust cycle, and new entrants may make endogenous human capital investment decisions in response to the booming sector (Gylfason, 2001; Balza et al., 2021; Charles et al., 2018). In the context of a skill-biased resource boom, prospective entrants may choose to acquire sector-specific human capital (with a delay corresponding with the duration of technical or university degrees), which could lead them to graduate (favourably) as the sector continues to expand, or (unfavourably) after it begins to bust. Firms in the resource sector may open industry-led technical training programs to ensure access to skilled workers at affordable wages. Collectively, these responses may create a glut of new skilled entrants into the booming sector, reducing earnings premiums. We document that earnings premiums decline monotonically for high-education new hires into the Brazilian oil sector as the oil boom progresses.

3 Context: Boom and Bust in Brazil's Oil Sector

Brazil experienced a dramatic oil boom and bust during the 2000s and 2010s, driven by fluctuations in global oil prices and domestic discoveries. Beginning in 2004, Brent Crude oil prices began rising rapidly, from an average nominal price of US$21 per barrel over the 1990-2003 period to a peak of US$134 per barrel in July, 2008. Oil prices crashed sharply in late 2008 as a result of the global financial crisis, but recovered quickly and remained above US$100 per barrel until August, 2014, when prices entered a sustained downturn, bottoming out at US$30 per barrel in January, 2016.

The boom in world oil prices between 2004-2014 coincided with a wave of giant offshore oil and gas discoveries in Brazil, primarily located in the ultra-deepwater Pre-Salt layer of the Santos and Campos sedimentary basins off the coast of São Paulo, Rio de Janeiro, and Espírito Santo. Major Pre-Salt discoveries included the 5-8 billion barrel Tupi field in 2007 and the 4.5 billion barrel Franco field and 7.9 billion barrel Mero field in 2010. In total, 179 major discoveries averaging 429 million barrels each were announced between 2000 and 2017. Discoveries consisted of medium to light crude (API gravity averaging 28.4), further raising the expected value of these reserves (Katovich, 2021). Figure 1 plots annual announced discovery volumes and world oil prices over this period.

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



Note: Brent Crude oil prices are drawn from FRED and averaged at the quarterly level. Announced discovery volumes are aggregated from a comprehensive list of discovery announcements filed by multinational oil companies with Brazil's *Comissão de Valores Mobiliários*, made available in Katovich (2021).

Pre-Salt discoveries and high oil prices combined to provoke rapid growth in oil-sector investment during the boom period, which extended from approximately 2007-2013 (with the exception of the "mini-bust" in 2009). Investments by Petrobras, Brazil's semi-public national oil company, increased from BRL$7.6 billion in 2000 (deflated to constant 2010 values) to BRL$104 billion in 2013, with growth strongest in areas (exploration, production, and refining) that exert strong backward and forward linkages on other industrial and service sectors (IPEA, 2010) (Appendix 1, Figure 1). After 2013, Petrobras' investment declined sharply, falling to BRL$48 billion in 2017 (Petrobras, 2020). Foreign direct investment in oil exploration and development in Brazil followed a similar pattern, increasing from BRL$153 million in 2006 to nearly BRL$5.8 billion in 2013, then crashing to BRL$1.7 billion by 2017 (ANP, 2020a). Despite retractions in oil investment from 2014 onward, oil and gas production increased steadily over the 2000s and 2010s as major Pre-Salt discoveries came online. Production increased from approximately 1.1 million barrels of oil equivalent per day in 2000 (79% offshore) to 3.5 million barrels per day in 2020 (94% offshore) (ANP, 2020b).

Crisis in Brazil's oil sector began in 2014 when world oil prices collapsed, reducing the commercial viability of ultra-deep Pre-Salt fields and squeezing operating margins up and down the oil and gas supply chain. Simultaneously, a major corruption scandal (called *Lava Jato* in Portuguese) involving Petrobras caused the national oil company to freeze or cancel much of its investment portfolio, with the downstream refining sector most heavily hit. As Petrobras dominates the Brazilian oil sector (accounting for 94% of oil and gas production in 2010), these cuts sent ripple effects through ecosystems of upstream and downstream firms that depend on the oil giant.[3]

Negative price and investment shocks in 2014 added to existing strains on the Brazilian oil sector. First, following early announcements of major Pre-Salt discoveries in 2006, the Brazilian government shut down auctions of exploratory Pre-Salt blocks as it developed a new regulatory regime for these areas. The reform, approved in 2010, substituted a concession regime for production sharing and required a minimum 30% participation by Petrobras in Pre-Salt exploration and production. Auctions of new blocks only resumed in 2013. This regulatory overhaul strained Petrobras' capacity and reduced foreign investment in Pre-Salt exploration and development during peak high-price years (Florêncio, 2016). Second, the rise of US fracking from 2009 onward drew international energy capital away from enormous fixed investments in Brazilian offshore. The bust of the oil sector in 2014 had cascading effects across upstream and downstream sectors, leading to closures and layoffs

[3] In 2007, Petrobras maintained 18,365 firms in its registry of suppliers, from which it purchased BRL$42 billion (constant 2010 values) in goods and services. Registered Petrobras suppliers employed 1.8 million formal workers in 2007 (Negri et al., 2010).

at major shipyards, construction projects, and supplier firms (Pereira and Texeira, 2017; Folha de São Paulo, 2018), with significant implications for workers in Brazil's oil-linked labor market.

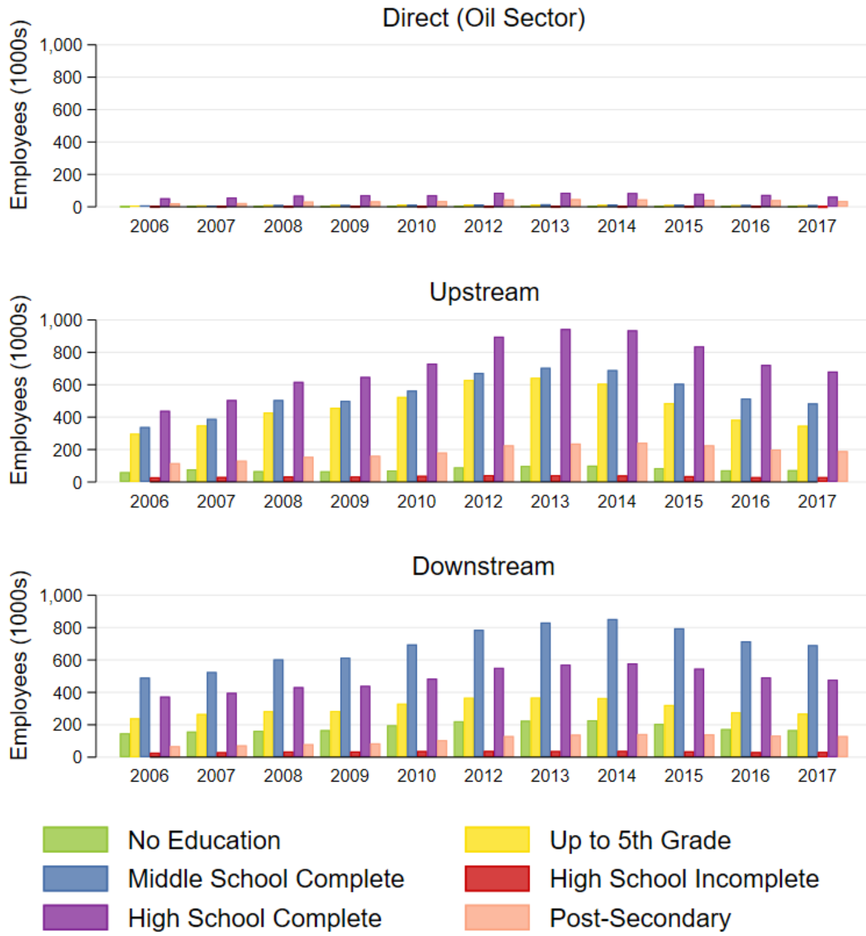
Oil-Linked Employment

Dominated by offshore production, Brazil's oil sector is highly capital intensive and employs relatively few workers directly (Figure 2). The oil and gas sector (e.g., oil and gas extraction and support activities) and directly-linked industrial sectors (e.g., production of equipment for oil and gas extraction, fabrication of refined oil products) employed 94.8 thousand formal workers in 2006 (all labor force numbers cited are from RAIS). This number increased to a peak of 169.1 thousand in 2013, before declining to 121.2 thousand in 2017. Workers in this sector hold occupations demanding high levels of education, with 53% of directly oil-linked jobs requiring a high school degree and 27% requiring post-secondary education over the 2006-2017 period.

Despite relatively small direct employment, the oil and gas sector exerts strong upstream (e.g., construction of ships, drilling rigs, and platforms; fabrication of extractive machinery and equipment; marine transportation; engineering services) and downstream (e.g., fabrication of plastics, fertilizers, and biofuels) linkages.[4] Sectors positioned closely upstream from the oil and gas sector employed 1.29 million formal workers in 2006, 2.67 million at the peak of the boom in 2013, and 1.81 million in 2017 after the bust. Among upstream oil-linked sectors, 35.4% of jobs required a high school degree, and 9.2% required post-secondary education over the 2006-2017 period. Downstream oil-linked sectors saw formal employment grow from 1.35 million in 2006 to 2.21 million in 2014, before declining to 1.77 million in 2017. Jobs in downstream sectors required lower levels of education than direct and upstream sectors, with 26.8% of jobs requiring a high school degree and 6.1% requiring post-secondary education.

[4] In Section 4 (Empirical Methods), we explain our classification of direct, upstream, and downstream oil and gas supply chain linkages in more detail. Classifications are based on a crosswalk between 7-digit CNAE 2.0 activity subclasses and Brazil's 2010 67×127 Input-Output Matrix, published by IBGE. We present our complete list of CNAE 2.0 subclass codes for direct, upstream, and downstream oil-linked sectors in an Appendix.

Figure 2: Oil-Linked Employment, by Skill Level (2006-2017)

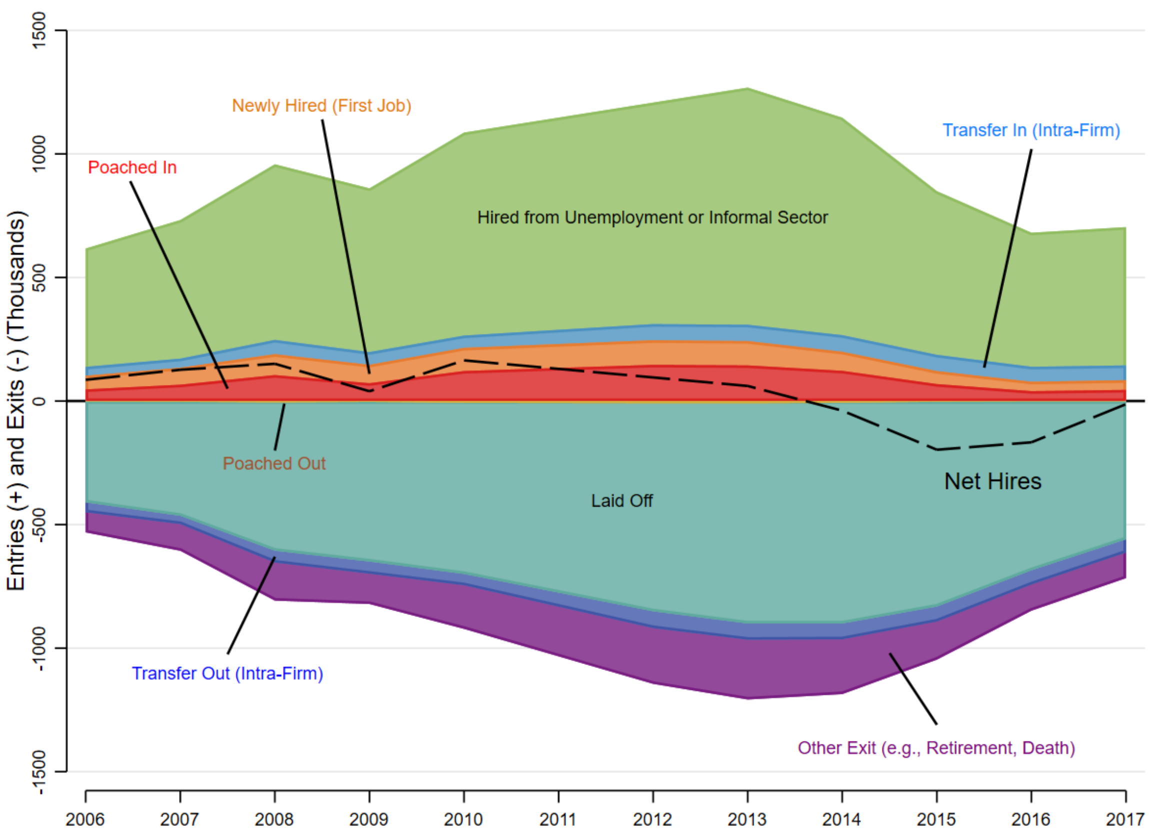


Note: Bars denote thousands of employees in different segments of Brazil's oil sector (direct, upstream, and downstream), disaggregated by minimum recommended education levels for each workers' occupation. Actual employees may or may not possess these recommended education levels. Minimum recommended education levels are drawn from textual descriptions of each occupation category in the 2002 *Classificação Brasileira de Occupações*. Direct, upstream, and downstream sectors are determined based on CNAE 2.0 Subclasses, and are reported in an Appendix. Number of formal employees is calculated from RAIS.

Expansion of Brazil's oil-linked sectors (direct, upstream, and downstream) between 2006-2013 generated new formal employment opportunities and pulled in workers from other sectors (e.g., poaching) or out of unemployment. Oil-linked sectors also expanded by hiring new first-time participants in the formal labor market (new hires) and through intra-firm transfers toward oil-linked activities. During the 2014-2017 bust period, oil-linked sectors expelled workers through poaches to other sectors, layoffs, intra-firm transfers out of oil-linked sectors, and retirements. There was also constant churn within oil-linked sectors and between these sectors and other parts of the economy, such that significant numbers of workers are poached, transferred out of, or laid-off from oil-linked sectors during boom years, and poached, transferred into, or newly hired into oil-linked sectors during bust years.

In Figure 3, we disaggregate employment flows into and out of oil-linked sectors using "type of hire" and "cause of separation" recorded at the job-year level in Brazil's *Relação Anual de Informações Sociais* (RAIS). Net formal employment growth in oil-linked sectors grew from 86,096 in 2006 to 164,817 in 2010, then declined steadily to 2014, when oil-linked sectors lost 38,708 formal jobs. The nadir occurred in 2016, when oil-linked sectors lost 166,747 net jobs, driven largely by a sharp drop in new entries relative to exits.

Figure 3: Disaggregated Job Flows Into and Out of Oil-Linked Sector



Job flow categories include poaches into oil, defined as workers who left a previous non-oil job voluntarily (*recisão sem justa causa por iniciativa do empregado*) and were rehired (*reemprego*) within four months into an oil-linked firm; new hire (*primeiro emprego*) into oil, defined as workers who are hired in their first formal job at an oil-linked firm; hire from unemployment into oil, defined as (i) workers who were laid off from their previous job (*recisão com/sem justa causa por iniciativa do empregador*) and rehired into an oil-linked firm, or (ii) any worker who is rehired into an oil-linked firm after 5 or more months without formal employment; and transfers into oil (*Transferência/movimentação do empregado/servidor, com/sem ônus para o cedente*), defined as workers who were transferred between establishments within a firm to an oil-linked establishment; poaches out of oil, defined similarly to poaches into oil; layoffs from oil (*recisão sem justa causa por iniciativa do empregador* or *termino de contrato*); retirements or deaths (*aposentadoria* and *falecimento*); and intra-firm transfers out of oil. Small numbers of other types of entry and exit (*cessão, redistribuição, mudança de regime*, etc.) are grouped into transfers-in and other exits, respectively.

Focusing on two sources of labor in-flow (new hires and poaches) we estimate logit models to explore predictors of being hired into an oil-linked establishment. Among pooled cross-sectional populations of all workers newly hired or poached into any formal establishment between 2006 and 2017, drawn from RAIS, we regress binary outcome $y_{it} = \mathbb{1}(\text{Hired into Oil-Linked Establishment} = 1)$ for worker i in year t on a vector of current period covariates, X_{it}, and, for poached workers, previous period covariates, $W_{i,t-1}$, as well as state and year fixed effects (γ_s and δ_t, respectively):

$$P(y_{it} = 1) = \alpha + X_{it}'\beta + W_{i,t-1}'\mu + \gamma_s + \delta_t + \epsilon_{ist} \tag{1}$$

Results reported in Table 1 indicate that higher education, male, non-white, and older workers are significantly more likely to be poached or newly hired into oil-linked establishments. Workers poached into oil-linked establishments come from larger firms. Within their previous firms, workers poached into oil were not amongst the top earners or top levels of education or management. Evidently, the expanding oil sector did not poach away top workers from other sectors, but rather drew more junior or lower education workers. Coefficient estimates for year fixed effects, relative to base year 2006, increase steadily until 2013, and then decline to 2017 (turning negative in 2014 among poached workers), reflecting the employment boom and bust in the oil sector during this period.

Institutional Features: Oil-Sector Policies and Labor Market Regulations

At the beginning of the oil boom period (early 2000s), Brazil adopted active industrial and labor market policies to stimulate local labor and input demand from the oil sector, and to meet that demand with increased supply of qualified workers and firms. Beginning in 2003, the National Oil Agency (ANP in Portuguese) began requiring companies bidding for exploratory rights to offshore fields to commit to minimum percentages of local content procurement. Requirements were further strengthened in 2005 by imposing item-specific local content requirements (Rocha, 2018). The local content requirement was significantly relaxed in 2015.

To meet increased demand resulting from the local content requirement, the Ministry of Mines and Energy, in conjunction with Petrobras and other partners, implemented the Quality Guarantee Program for Services and Materials (PGQMSA), through which technical inspectors verified and supported implementation of state-of-the-art production processes and technologies among Petrobras suppliers (Rocha, 2018). To meet booming demand for workers with oil-relevant skills (including engineers, petrochemicals, environmental cleanup and safety specialists, mechanics and machine

Table 1: Predictors of New Hire or Poach into Oil-Linked Sector (Logit)

Covariates	New Hire	Poached	Poached
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)
Constant	-5.304***	-4.497***	-3.903***
	(0.018)	(0.022)	(0.022)
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 newly hired or poached into an oil-linked establishment on 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 poached formal workers between 2006-2017. For poached workers, previous employment characteristics are observed and can therefore be included in regressions. Rank variables (wage, education, and occupation) are computed for each poached workers 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). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

operators, and electricians) Petrobras and public-private industry groups such as SENAI and FIRJAN implemented the Program for Mobilization of the National Oil and Gas Industry (Prominp), which facilitated technical education and training programs that graduated over 80,000 oil-sector professionals between 2007 and 2017, when the program was discontinued (SINAVAL, 2020).

During Brazil's oil boom (2006-2013) and bust (2014-2017) periods, formal employment contracts were governed by strong labor protections and guarantees laid out in the *Consolidação das Leis do Trabalho* (CLT). Formal labor market regulations may affect firms' hiring, firing, and salary-setting decisions (Ulyssea and Ponczek, 2021; Dix-Carneiro and Kovak, 2017). Requirements include limits on hours worked per day (8) and week (44) before overtime pay is required, mandatory vacation, sick leave, and maternity/paternity leave, additional pay for night-time, dangerous, or unhealthy work, and a 13th monthly salary in December. Employers must also make monthly contributions equivalent to 8% of an employee's salary to the Guarantee Fund for Time of Service (FGTS in Portuguese), which is made available to the employee if they are laid off without just cause, retire, or go three years without formal employment (CLT, 2017).

To lay off a worker without just cause, employers incur significant additional expenses, including a requirement that they give 30 days notice (increasing to 45 days for employees with five or more years of service) or pay the employee's salary for that number of days, including pro-rated vacation and 13th salary, if they wish to terminate the contract immediately. Employers must also pay unemployment insurance proportional to the employees' highest pay-period with the company for employees with more than one year of service, as well as a fine of 40% of the accumulated value of deposits made monthly in that employee's FGTS fund. Collectively, these rules make it disproportionately expensive for firms to lay off longer-serving workers without cause, as they must give earlier notice or buy out more days, pay higher values for unemployment insurance, and pay a larger FGTS fine (CLT, 2017; PontoTel, 2020).

4 Data and Sample Construction

This paper aims to measure the effects of being hired into Brazil's oil industry (including related upstream and downstream sectors) on workers' wages, employment, skill development, and total earnings. We focus on a period (2003-2017) during which the Brazilian oil sector experienced a major boom and bust cycle, driven by exogenous offshore discoveries and world price fluctuations. In particular, we explore whether timing of hire into oil relative to this boom and bust cycle matters for workers' long-term outcomes, and whether high and low education workers experience the effects differently. We use linked employer-employee data on the universe of formal establishments and employees in Brazil to reconstruct workers' complete formal employment trajectories between 2003 and 2017. For each year, we identify workers who are poached or newly hired into oil-linked firms, and construct matched control groups of comparable workers who are poached or newly hired into other sectors. Using event studies that compare workers within matched samples, we estimate dynamic effects of oil-sector exposure across the boom and bust cycle.

4.1 Data

In the following sections, we describe data sources and steps taken to define "oil-linked" sectors and construct worker-level panels. We also describe an important supplementary dataset on higher education outcomes in Brazil.

Using Input-Output Matrices to Identify "Oil-Linked" Activities

Our empirical strategy begins by defining "oil-linked" sectors. Brazil's oil industry is dominated by highly capital-intensive offshore production and generates relatively little direct employment, as illustrated in Figure 3 above. However, the oil industry exerts strong upstream and downstream linkages (e.g., shipyards and refineries, respectively), generating significant "oil-linked" employment. We draw on 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 2, Table 1.

Next, we translate 5-digit SCN 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 linked establishment-employee records. 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 associated with a 2-digit root derived from I-O products closely upstream or downstream from oil, 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 industrial gases). To check sensitivity to these definitions, we assign stricter and looser assignment rules relative to our preferred definition. In Appendix 2, Table 2, 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 2, Table 3.

RAIS: Establishment-Employee Linked Administrative Data

We use restricted-access, linked establishment-employee administrative records on the universe of formal establishments and employees in Brazil from the *Relação Anual de Informações Sociais* (RAIS).[5] RAIS is collected annually by the Brazilian government from all formally registered firms, and serves as the basis for the country's unemployment insurance system, among other programs. The dataset contains between 40-73 million job-level observations, and 2.5-3.9 million establishment-level observations per year over the 2003-2017 period. We link workers across jobs and years using unique stable worker IDs (PIS/PASEP). We link workers to establishments (sub-firm units that may be aggregated to the firm level) and link establishments across years using unique stable establishment IDs (CNPJ/CEI).

Worker observations are reported at the job-establishment-year level, allowing workers to appear multiple times in a year if they hold multiple jobs. Worker-level variables include demographic characteristics such as age, sex, race, nationality, and education, as well as complete formal employment histories: months of hire and separation, type of hire, cause of separation, wages and hours employed, type of employment contract, and 6-digit occupation codes defined by the 2002 *Classificação Brasileira de Occupações.* At the establishment level, RAIS reports CNAE 2.0 activity subclasses, which allow us to identify whether each firm (and thus, each linked worker) is oil-linked. Establishment-level observations also include municipality and ownership type. By aggregating employee observations to the establishment level, we can compute number of employees, wage and occupation distributions, R&D intensity, and hires and layoffs each year for each establishment.

We supplement RAIS variables with a municipality-level measure of distance to the nearest oil-linked shipyard. Since shipyards are the supply-chain nexus for upstream oil activities (where inputs are brought together into drilling rigs, tanker ships, etc.), they proxy for spatial variation in oil sector intensity. We compile a complete list of oil-linked shipyards in Brazil from PortalNaval, an

[5]The first author obtained access to the identified RAIS dataset through an institutional data use agreement with the Instituto de Economia at the Universidade Federal do Rio de Janeiro. RAIS data were cleaned using standardized procedures developed by Dahis (2020).

industry website. We restrict our sample to shipyards that existed prior to our analysis (year 2003) to avoid endogenous entry. We also create an original dataset of minimum recommended education levels for each of the 2,572 occupations defined in the 2002 *Classificação Brasileira de Occupações* to proxy for the skill level of each job in the RAIS dataset. Finally, we perform standard cleaning operations with the dataset, such as removing workers outside of the 16-75 age range and those with irregular IDs, working hours, wages, or other variables.[6]

Constructing Panels of Poached and Newly Hired Workers

In our empirical analysis, we focus on two standard ways in which a booming resource sector may expand in employment: (i) by poaching workers from other sectors; (ii) by hiring new workers who graduate or age into the labor market. We define poached workers as those who left their previous job voluntarily (*Recisão sem justa causa por iniciativa do empregado*) and are rehired at a new job within 4 months (in a robustness check, we use a 2-month cutoff). We define new hires as workers who are hired to their first formal job (*primeiro emprego*). When creating these subsamples, we restrict ourselves to each workers' primary job in a year, defined as the job in which they have the highest earnings. For each worker poached in a particular year, we construct a complete 2003-2017 employment trajectory by merging their unique ID with all former and subsequent years. For new hires, we construct their complete post-hire employment history by merging their ID with subsequent years. In years when workers do not appear in the data (e.g., they were unemployed or informally employed in that year), we impute zero-values for formal earnings. These procedures result in strongly balanced, year-specific panels (which we refer to throughout as cohorts) of workers poached and newly hired into oil-linked and other sectors.

[6] 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 Brazil, informal employment is widespread, ranging from 60% of total employment in 2006, to a low of 52.5% in 2013, to 58.4% in 2019. Intensity of informality also varies across demographics and sectors, with 62.4% of workers with incomplete elementary educations informally employed, and 21.9% of workers with complete higher education informally employed in 2019 (IBGE, 2019). In 2019, informality was highest in domestic services (72.5%), agriculture (67.2%), and construction (64.5%). Using complementary data from the *Pesquisa Nacional por Amostra de Domicílios* (an annual representative household survey), we compute the rate of formal employment in upstream, downstream, and directly oil-linked sectors between 2006-2014 (Appendix 1, Figure 2). Oil-linked sectors exhibit significantly higher rates of formal employment than the Brazilian economy as a whole, with directly oil-linked sectors employing between 84-93% of workers formally. Upstream and downstream oil-linked sectors employ between 66-75% and 78-87% of workers formally, respectively, compared to 39-48% economy-wide.Formal employment in Brazil brings extensive benefits (see Section 3). Thus, while we cannot observe informal employment in the RAIS data, we assume throughout that a workers' disappearance from formal employment records is likely associated with less-desirable labor market conditions and benefits. Likewise, when we refer to employment and earnings, we mean formal employment and earnings unless otherwise specified.

4.2 Matched Research Design

The aim of our empirical strategy is to estimate the causal effects of being hired into a volatile resource sector (oil) on subsequent wages, earnings, employment, skill development, and other outcomes. The primary threat to causal inference in this setup is that workers are not randomly hired into sectors. Rather, they may be selected based on both observable and unobservable characteristics that are correlated with their labor market outcomes. To overcome this challenge, we adopt a Coarsened Exact Matching strategy (Iacus et al., 2012) to construct samples of comparable workers, some of whom (the "treated") are poached or newly hired into an oil-linked establishment, while others ("controls") are poached or newly hired into other establishments in the same year.[7]

We take advantage of the exceptionally rich RAIS dataset to impose rigorous matching criteria, reducing concerns over selection on unobservables. For the universe of poached workers, we match workers poached into oil-linked establishments with workers poached into other sectors within each year-cohort separately. We first match exactly on education level, sex, a non-white race indicator, previous establishment ID (the establishment from which they were poached), previous occupation category (low/high skill white collar and low/high skill blue collar), and destination-municipality (where they are poached to). We also impose a two-year retrospective matching window, matching exactly on establishment ID in January of years t-1 and t-2. Finally, we bin ages by every four years (e.g. ≤ 16, 16-20... 56-60, >60), and bin wages by multiples of the minimum wage (0-1, 1-2, 2-3, 3-5, 5-10, 10-20, > 20). We match exactly on these age bins, and exactly on wage bins for each worker's previous job, as well as the jobs they held in January of t-2 and t-2.

We thus constrain comparisons to workers with nearly identical demographic characteristics, skill levels, and earnings profiles. By matching exactly on previous establishment, we are able to take into account important unobservable heterogeneity in productivity captured at the establishment level. By matching on destination municipality, we account for spatial shocks that affect all workers in specific places. We impose a two-year matching window to constrain the sample to workers who are on similar labor market trajectories, rather than those who are in only transitory alignment in the pre-poach period. In event studies, we present the binned t-3...t-n period to assess the parallel-

[7] We opt for Coarsened Exact Matching over Propensity Score Matching (PSM) or other methods due to CEM's: (i) transparent implementation that achieves exact matches on categorical variables (including exact matches on establishment and municipality) and binned continuous variables; (ii) *ex-ante* imposition of balance across observable covariates, wherein choosing the balance criterion for one covariate does not affect balance across other covariates; (iii) customizable bins that respect meaningful context-sensitive cutoffs, such as education levels; (iv) retention of all exactly-matched observations in sample, rather than 1-to-1 pairs (Iacus et al., 2012). Black et al. (2020) criticize CEM on the grounds that default settings in CEM software packages produce misleading bins. We manually define all matching bins to avoid this concern.

pretrends assumption. We present baseline descriptive statistics on full and matched samples of poached workers in Appendix Table B5.

For newly hired workers, we are unable to observe pre-hire characteristics. Thus, we match exactly within each year-cohort on education level, sex, a non-white race indicator, municipality of hire, and macro-sector (e.g., agriculture, manufacturing, retail, etc.). We bin the wage at which workers are first hired using the same minimum wage multiples described above; we bin establishment size of their first job into bins defined by micro (<10 employees), small (10-49 employees), medium (50-249 employees), and large (>249 employees) establishments, and bin age along a shifting scale to account for the fact that most new hires are young (i.e., ≤ 16, 16-18,18-20, 20-22, 22-24, 24-26, 26-28, 28-30, 30-35, 35-40, 40-50, 50-60, >60). We opt to match on first job characteristics (first wage and first firm size) as these variables likely reveal important, otherwise unobservable information about workers, i.e. ability as assessed by employers. These exercises result in matched panels for each year-cohort of comparable workers who were poached or newly hired into oil-linked versus other sectors. We present baseline descriptive statistics on full and matched samples of newly hired workers in Appendix Table B6. Full and matched sample sizes and match rates for both poached and newly hired workers are summarised in Appendix Table B21.

5 Empirical Strategy

We identify dynamic causal effects of being hired into a volatile resource sector by comparing outcomes (e.g., hourly wages, hours employed per year, annual formal earnings) for workers poached or newly hired into oil in a particular year t with outcomes for closely matched workers poached or newly hired into other sectors in year t (Schmidheiny et al., 2019).[8] Specifically, for worker i in cohort c in year t, let E_{ic} be the period when i is treated by a poach or new hire. Then let $K_{ict} = t - E_{ic}$ be the number of years before or after this event. We regress individual-level

[8] Ho et al. (2007) show that non-parametric matching prior to parametric regression estimation improves estimation accuracy and reduces model-dependence. A number of recent studies have implemented matching prior to dynamic difference-in-differences estimation in the context of rich administrative panel datasets, including Stepner (2019), Jager and Heining (2017), and Sarsons (2019). Abadie and Spiess (2021) consider robust inference after nearest-neighbor matching and conclude that ignoring the matching stage results in asymptotically valid standard errors in the regression stage if (i) matching is performed without replacement (i.e., unique 1-to-1 matches between treatment and control units), and (ii) the regression model is correctly specified, and (iii) regressions cluster standard errors at the matched pair (individual) level. This result does not clearly extend to the case of CEM, which involves no first stage estimation but rather trims the sample to observations lying on a common support across covariates. We argue that the advantages for robust inference in the nearest-neighbor approach do not justify limiting the sample to 1-to-1 pairwise comparisons, which would reduce sample size and could be liable to individual-level idiosyncratic shocks as we extend treated-control comparisons out to the end of sample.

outcome Y_{ict} on $\mathbb{1}(K_{ict} = k)$ relative year indicators for the fully-saturated set of indicators going to the beginning and end of sample. We include individual and year fixed effects, δi and λ_t, cluster standard errors at the unit (individual) level, and weight regressions by the CEM matching weight:

$$Y_{it} = \delta_i + \lambda_t + \sum_{k \neq -1} [\mathbb{1}(K_{it} = k)]\beta_k + \epsilon_{it} \tag{2}$$

We estimate this specification separately for selected cohorts (2006, 2008, 2010, 2012, and 2014) to assess how timing of entry relative to the boom and bust cycle affects outcomes. Event studies often pool observations from multiple treated cohorts and center them around generic relative time indicators, which is useful in identifying generalized effects of a treatment. In our context, however, we are interested in the effects of treatment at different points in real time, e.g., whether workers were hired into an oil-linked establishment in 2006, near the beginning of the oil boom, in 2010 near its peak, or in 2014 near the beginning of the bust.

To explore heterogeneity in treatment effects across workers of different characteristics, 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), as well as for male and female and young and old workers. As workers remain exact matched on these characteristics, results reveal whether being hired into oil has disproportionate labor market effects on workers of specific types.

We transform continuous outcomes using the inverse hyperbolic sine transformation to account for zero values (e.g., no formal earnings), and deflate monetary values to constant 2018 Brazilian Reals using the IPCA deflator from IPEA (2020). For outcomes that only apply to employed workers (i.e., wage, hours employed, and skill level), 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, months employed per year), we preserve the full sample.

5.1 Identification

In this event study specification, coefficient estimates of β_k identify the average treatment effect at length of exposure k from a poach or new hire into an oil-linked establishment, under the identifying assumption that, absent this hire, oil-treated workers would have followed the same trends as their matched counterparts who were poached or newly hired into other sectors. The parallel pre-trends assumption may be evaluated in pre-treatment periods ($\beta_k = 0$ for t<-1).

Selection into treatment (oil-linked employment) is a significant threat to causal inference in this context, given that both workers and employers may choose their opposite number based on unobservable characteristics that are correlated with outcomes (e.g., ability, motivation, or risk preferences). How much can our matching procedure reduce these concerns over endogenous selection into treatment? We argue that we match on a rich set of meaningful labor market variables that capture important information about workers. For instance, exact matching on sex, race, education, destination municipality, and previous firm for two years prior to a poach, and coarsened exact matching on age and wage over those two prior years, approximately captures all of the information a prospective employer would have access to when deciding whether or not to hire a new employee (besides an interview). In this sense, our matching procedure approximates the true data generating process. Further, the inclusion of individual fixed effects controls for time-invariant worker characteristics (including unobservables) and identifies treatment effects off of within-worker variation.

Compared to (extremely) closely matched counterfactual workers who were hired into other sectors in the same year, workers hired into oil-linked sectors are exposed to large and difficult-to-anticipate labor market shocks driven by exogenous changes in global oil prices and offshore discoveries. Brazil accounts for only 4% of world oil production (EIA, 2021) and did not drive the large global price variations that occurred during the study period. Offshore discoveries in Brazil were made by multinational oil companies (including the semi-private Petrobras) using international technologies and expertise. In short, price variations and discoveries that combined to produce Brazil's oil boom and bust were not driven by domestic labor market conditions, reducing concerns over reverse causality.

Since we seek to compare post-treatment (i.e., hire into oil) labor market trajectories across cohorts, a final concern is that the composition of entrants into oil-linked sectors may change in response to the boom. For instance, workers who are poached or newly hired into oil in 2006 may be more forward-looking or risk-loving than laggards who enter the sector only after observing it's growing success. Endogenous changes in cohort composition over time could therefore compromise our ability to causally interpret differences in outcomes across cohorts. To reduce these concerns, we estimate a robustness check (see Section 6) that restricts post-2006 cohorts to workers who share a common support with the 2006 cohort.

6 Results: Event Studies Around Poach or New Hire into Oil

This section reports dynamic event study estimates of the effects of being poached or newly hired into an oil-linked establishment on labor market outcomes (hourly formal wages, months formally employed per year, and annual formal earnings) for selected cohorts (2006, 2008, 2010, 2012, 2014) over the 2003-2017 sample window. We first present results for poached workers, and then new hires.

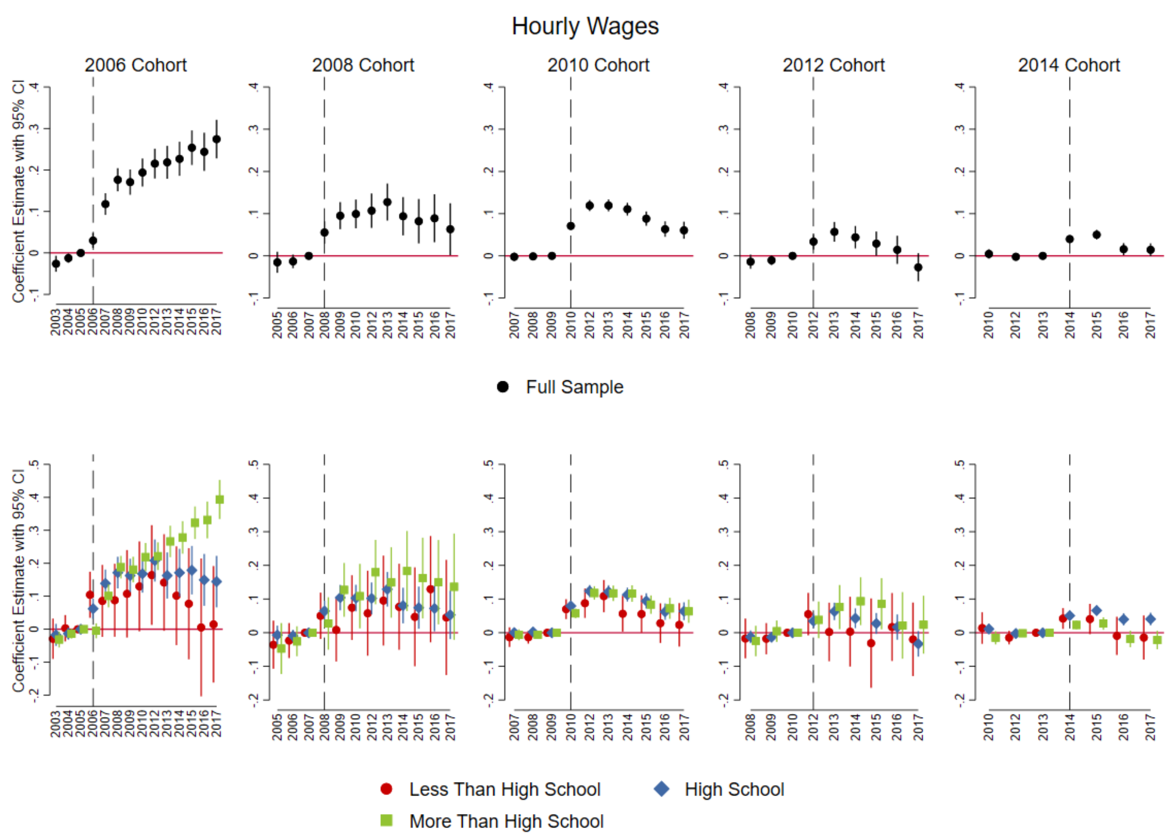
Poached Workers

Figure 4 reports dynamic coefficient estimates with 95% confidence intervals for three years prior to a poach into an oil-linked establishment, and all years after the poach (up to 2017). The top row of subfigures in Figure 4 shows treatment effect estimates for all workers treated by an oil-linked poach in each cohort relative to matched workers poached into other sectors in those years. The bottom row of subfigures disaggregates treatment effects along an important dimension of heterogeneity: education. This row reports coefficient estimates and 95% confidence intervals for individuals with low education (less than secondary complete), medium education (secondary complete), and high education (more than secondary complete) on the same graph, but estimated separately relative to matched workers with the same education levels who were poached into other sectors in that year. Coefficient estimates, standard errors, and sample statistics corresponding with figures in this section are reported in Appendix Tables B5-B16.

To assess affects on wages, we limit the sample in Figure 4 to employed workers. Coefficient estimates are statistically insignificant in the three years preceding the poach (excepting the $t-3$ period for the full 2006 cohort), lending credence to the parallel pre-trends assumption. In the poach year, treated workers in all cohorts experience significant positive effects on wages (as expected given our definition of poach as a voluntary exit from one job and hire at another), ranging from $100 \times (e^{(0.03)} - 1) = 3.05\%$ for the 2006 cohort to $100 \times (e^{(0.071)} - 1) = 7.36\%$ for the 2010 cohort, where semi-elasticities may be interpreted as the percentage change in wages upon switching from control to treated. For the 2006 cohort, the effect of being poached into an oil-linked establishment on hourly wage grows to +24% by 2012, and +31.6% by 2017. Subsequent cohorts do not experience the same dynamic wage growth after their poach. Wages for later cohorts growth until approximately 2013, then turn downwards (but remain positive) with the onset of the oil bust in 2014. The 2008 cohort's positive wage effects peak at +13.6% in 2013, then diminish to +6.5% by 2017. Wage premiums for the 2010 cohort peak at +12.7% in 2013 and decline to +6.25% by 2017.

Disaggregating wage effects by workers' education, it is apparent that dynamic post-poach wage gains in the 2006 cohort are driven by gains accruing to high-education workers, whose wages are 48.2% higher than matched controls by 2017. Workers with complete secondary education also enjoy significant wage premiums after their poach (peaking at +19.6% in 2015), but these turn downwards during the bust. Low-education workers experience weakly positive wage effects up to 2013, after which their wages fall back into line with matched counterfactual workers in other sectors. There is no significant heterogeneity in wage effects across education levels for subsequent cohorts.

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



Note: Event studies regress hourly wages on relative time indicators centered around poach into an oil-linked establishment ($t-1$ 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. To analyse effects at intensive margin, this specification keeps only employed individuals. Treated individuals (poached into oil-linked sector in year t) are compared to individuals poached into other sectors in year t who matched on wage and age bins, education, sex, race, occupation category, and firm during a two-year matching window prior to poach, and who were poached into the same destination municipality in t. Poaching is defined as voluntary exit from previous firm and rehire at a new firm within 4 months. The first row shows coefficient estimates and 95% confidence intervals for the full sample of matched poached workers; the second row reports coefficient estimates and 95% confidence intervals for each education category (less than high school, high school complete, more than high school) separately relative to its own matched controls.

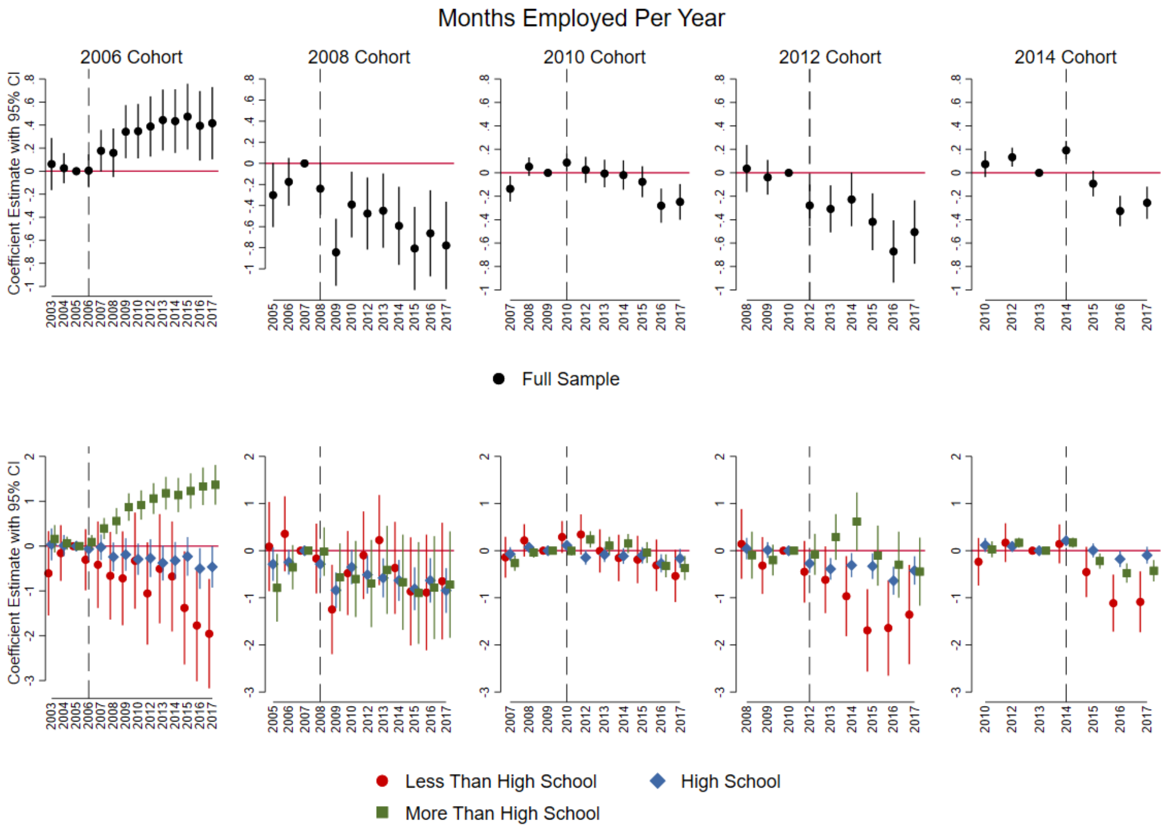
Turning from hourly wages to months employed, we retain all matched poached workers in sample. We assign a formal employment indicator equal to one in each month a worker has one or more formal jobs, and equal to zero in each month the worker is not formally employed, yielding a strongly balanced panel. We sum values for each year, resulting in an employment outcome ranging from 0-12. Results reported in Figure 5 show a sharp contrast to Figure 4. While wage effects of being poached into oil were significantly positive, or at worst non-negative for surviving workers who kept formal jobs, effects on formal employment are significantly negative for all cohorts after 2006. In the 2006 cohort, workers poached into oil are employed for 41.5% more months than matched workers in 2010, and for 51.7% more months in 2017. These positive results are driven completely by high education workers, who are employed for 292.5% more months than their former colleagues poached into other sectors by 2017. Medium-education workers in the 2006 cohort are weakly less often employed by 2017, and low education oil-linked workers are significantly less often employed after the oil bust in 2014 (-85.8% by 2017). Subsequent cohorts experience significantly negative and progressively worsening employment outcomes, with negative effects on months employed of -54.1% for the 2008 cohort, -22.0% for the 2010 cohort, -39.7% for the 2012 cohort, and -22.6% for the 2014 cohort by 2017. Negative employment effects of being poached into oil are worst for low-education workers, who are especially likely to lose their jobs during the bust. Low-education workers who are poached into oil in 2012 are employed for 74.2% fewer months by 2017; low-education workers poached in 2014 are employed for 66.2% fewer months by 2017.

The "Mini-Bust" of 2008-2009

The employment experience of the 2008 cohort in Figure 5 is noteworthy, as it reveals persistent negative effects of entering the oil sector at a disadvantageous moment. Workers poached in 2008 entered oil-linked establishments just as the Global Financial Crisis provoked a brief but deep crash in oil prices. This crisis did not affect employment or wages for the already-established 2006 cohort, but led to significant increases in job-loss among the new 2008 cohort, who are employed for 57.0% fewer months in 2009 relative to matched workers who were poached into other sectors in that year (and therefore not directly exposed to the 2008-09 oil price crash). Employment for the 2008 cohort never recovers from this transitory shock. In contrast, newly poached workers in 2010 are equally likely to keep their jobs as are matched controls, up to the oil bust in 2014. Disaggregating the anomalous 2008 findings, it is clear that low-education workers bore the brunt of firms' adjustment the 2008-9 price crash. Low-education workers poached into oil in 2008 are employed for 71.3%

fewer months than matched controls in 2009.

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

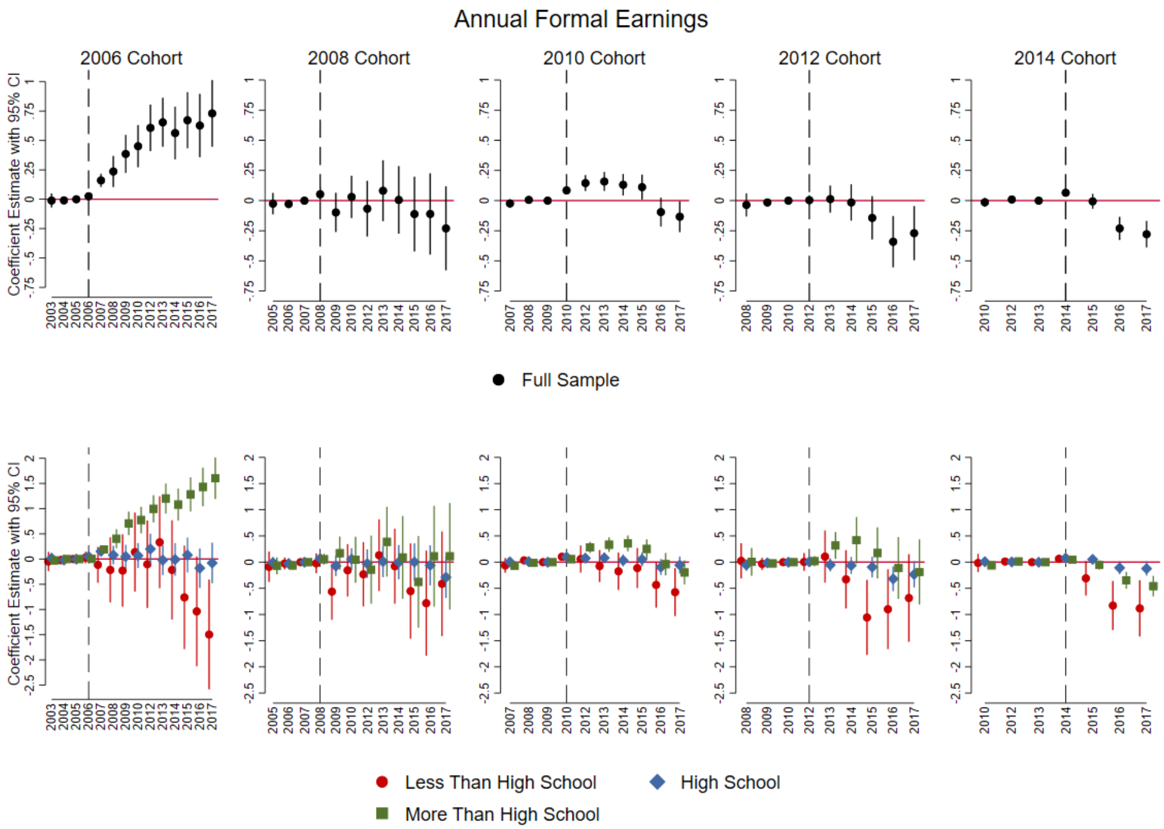


Note: Event studies regress months employed per year on relative time indicators centered around poach into an oil-linked establishment ($t-1$ omitted). Standard errors are clustered at the individual level, and individual and year fixed effects are included. Months employed ranges from a minimum of zero if the worker never appeared in formal employment registries during a year, to 12 if the individual was employed each month in at least one formal job. To analyse effects at the extensive margin, this specification keeps all treated individuals and their matched counterfactuals (whether formally employed or not) in a strongly balanced panel. Treated individuals (poached into oil-linked sector in year t) are compared to individuals poached into other sectors in year t who matched on wage and age bins, education, sex, race, occupation category, and firm during a two-year matching window prior to poach, and who were poached into the same destination municipality in t. Poaching is defined as voluntary exit from previous firm and rehire at a new firm within 4 months. The first row shows coefficient estimates and 95% confidence intervals for the full sample of matched poached workers; the second row reports coefficient estimates and 95% confidence intervals for each education category (less than high school, high school complete, more than high school) separately relative to its own matched controls.

Annual formal earnings are calculated as the sum of earnings from all formal jobs in a given year. They thus incorporate the effects of hourly wages and employment discussed above. To analyse earnings, we retain all matched poached workers in sample, ascribing formal earnings of zero in cases where workers do not appear in formal employment registries during a given period.[9]

[9]A limitation of the RAIS data is that we are unable to distinguish whether workers who do not appear in a given month are unemployed, self-employed, or informally employed during that time. In Appendices A4, we draw on data

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



Note: Event studies regress months employed per year on relative time indicators centered around poach into an oil-linked establishment ($t-1$ omitted). Standard errors are clustered at the individual level, and individual and year fixed effects are included. Annual earnings refers to total formal earnings for each worker across all formal jobs. Earnings are transformed using the inverse hyperbolic sine transformation and deflated to constant 2018 BRL. This specification keeps all treated individuals and their matched counterfactuals, 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. In reality these individuals may have been unemployed, informally employed, or self-employed during these gap periods. Treated individuals (poached into oil-linked sector in year t) are compared to individuals poached into other sectors in year t who matched on wage and age bins, education, sex, race, occupation category, and firm during a two-year matching window prior to poach, and who were poached into the same destination municipality in t. Poaching is defined as voluntary exit from previous firm and rehire at a new firm within 4 months. The first row shows coefficient estimates and 95% confidence intervals for the full sample of matched poached workers; the second row reports coefficient estimates and 95% confidence intervals for each education category (less than high school, high school complete, more than high school) separately relative to its own matched controls.

As shown in Figure 6, annual formal earnings for the 2006 cohort of poaches grow dynamically

from PNAD to show that oil-linked sectors are highly formalized (ranging from 70-90%) relative to the economy-wide average of 40-50%. Thus, our data likely captures the vast majority of workers in our sectors of interest, reducing concerns over the formality limitation. In Appendix A5, we again draw on data from PNAD to show that formal oil-linked workers earn approximately 50% more than informal workers in oil-linked sectors, and 150% more than informal workers generally. Further, formal employment conveys significant non-wage benefits and protections. We conclude (loosely) from these facts that workers who are not formally employed are very likely worse off than if they had retained their formal job.

through 2017, despite the 2014 oil bust. Earnings gains for this group are entirely captured by high-education workers, who earn 116.9% more than matched controls by 2010, and 397.2% more by 2017. In contrast, low-education workers poached into oil in 2006 never experience positive earnings during the boom period, and experience significantly negative effects on earnings after 2013 (-77.6% by 2017). The 2008 cohort, entering around the mini-bust of 2008-2009, never experiences positive earnings effects of subsequent boom years (2010-2013), highlighting the persistent effects of this cohort's unfavourable start. The 2010 cohort of poaches, entering at the peak of Brazil's oil boom, experience five years of positive earnings effects relative to their matched colleagues who were poached into other sectors in the same year (+15.7% in 2012). Earnings for the 2010 cohort fall after the 2014 bust (-12.5% in 2017). Workers poached into oil in 2012 never experience positive earnings effects of the boom, and suffer significant negative effects in 2015 (-1.5%) through 2017 (-23.7%). Similarly, workers in 2014 enter right as the sectors busts and earn 24.3% less than matched workers in 2017. Low-education workers bear the brunt of negative earnings effects across 2008-2014 cohorts, with these workers suffering earnings losses of -43.6% for the 2010 cohort, -49.5% for the 2012 cohort, and -58.6% for the 2014 cohort by 2017.

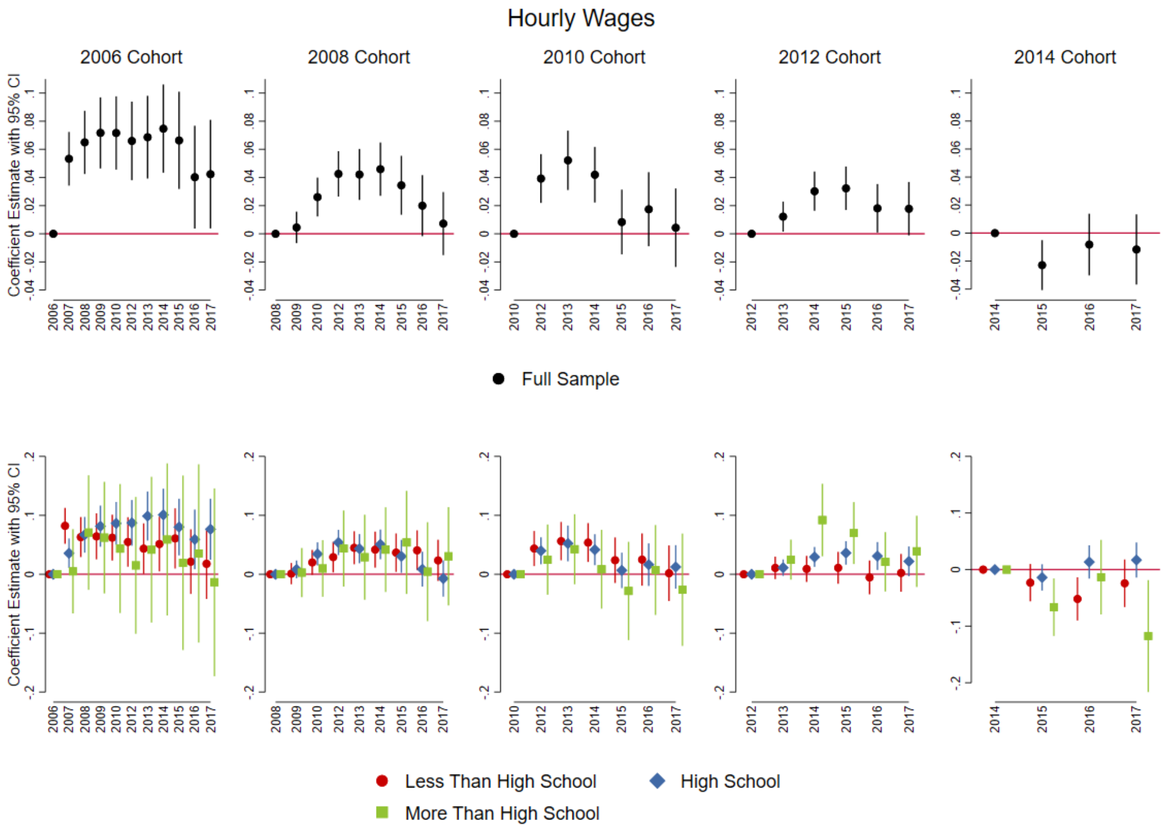
New Hires

Newly hired workers are those hired into their first formal job. They may be recent graduates, but could also include individuals with previous labor market experience in the informal sector. Figure 7 reports estimates of the effect of being newly hired into an oil-linked establishment on hourly wages, relative to matched workers newly hired into other sectors in the same year.

New hires into oil in 2006 earn significantly higher wages than matched workers in other sectors (+5.5% more in 2007, +6.8% in 2012, and +4.3% in 2017). Oil-linked new hires in 2008, 2010, and 2012 cohorts also earn higher wages, but with smaller magnitudes than in 2006. In contrast with wages for poached workers, wages for 2006 new hires do not diverge between high and low-education workers. Medium and low-education workers enjoy significantly positive wage effects after a new hire into oil. Compared to poached workers, the magnitudes of oil treatment effects on newly hired workers are substantially smaller (ranging between 0 and 10%, compared to 0-35% amongst poached cohorts). Notably, while poached workers of all education levels in 2010 enjoyed significant wage premiums in oil-linked sectors until 2013, this is not the case for new hires. Among 2010 new hires, low and medium-education workers enjoy wage premiums through 2014 (+5.8% and +5.4%, respectively in 2013), but high-education new hires in 2010 never enjoy wage premiums. In

Section 8, we explore how a boom in oil-linked higher education may have eroded wage premiums for high-education new hires in later cohorts.

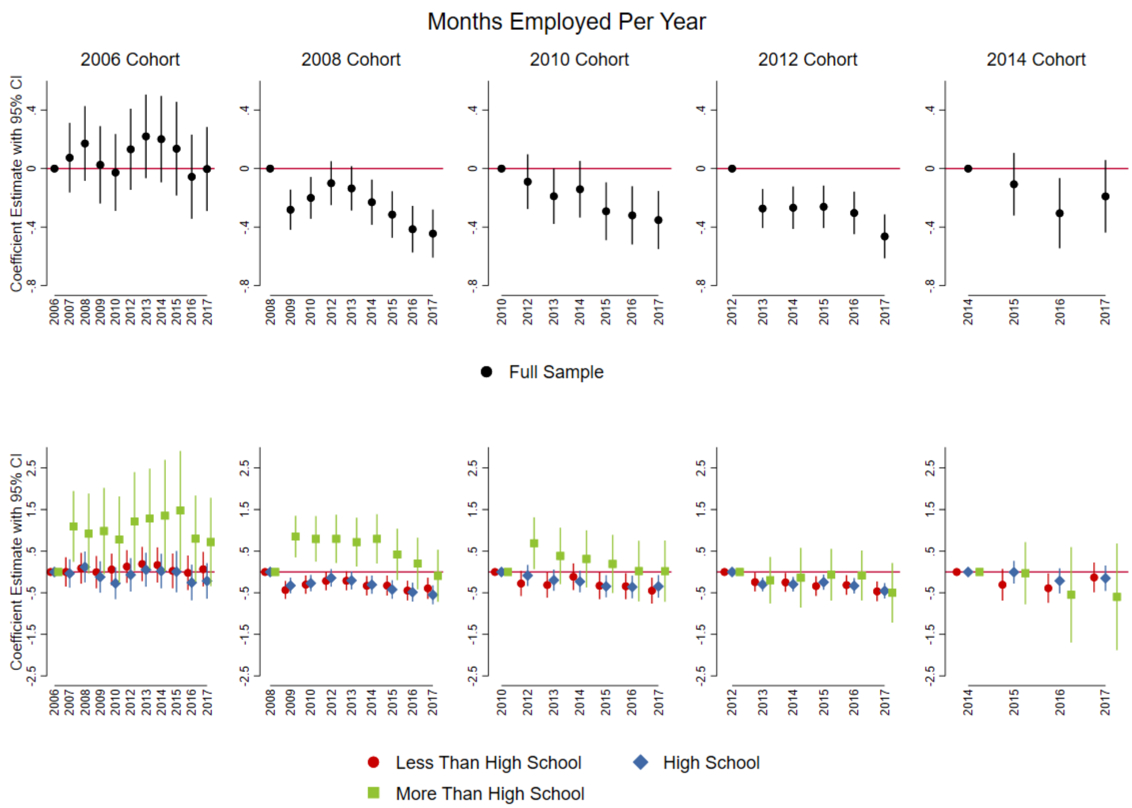
Figure 7: Hourly Wages After New Hire into Oil-Linked Sector



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. To analyse effects at the intensive margin, 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. New hires are defined as workers who are hired into their firm formal job. The first row presents results for the full sample of matched newly hired workers; the second row reports coefficient estimates and standard errors for each education category (less than high school, high school complete, and more than high school) separately relative to its own matched controls.

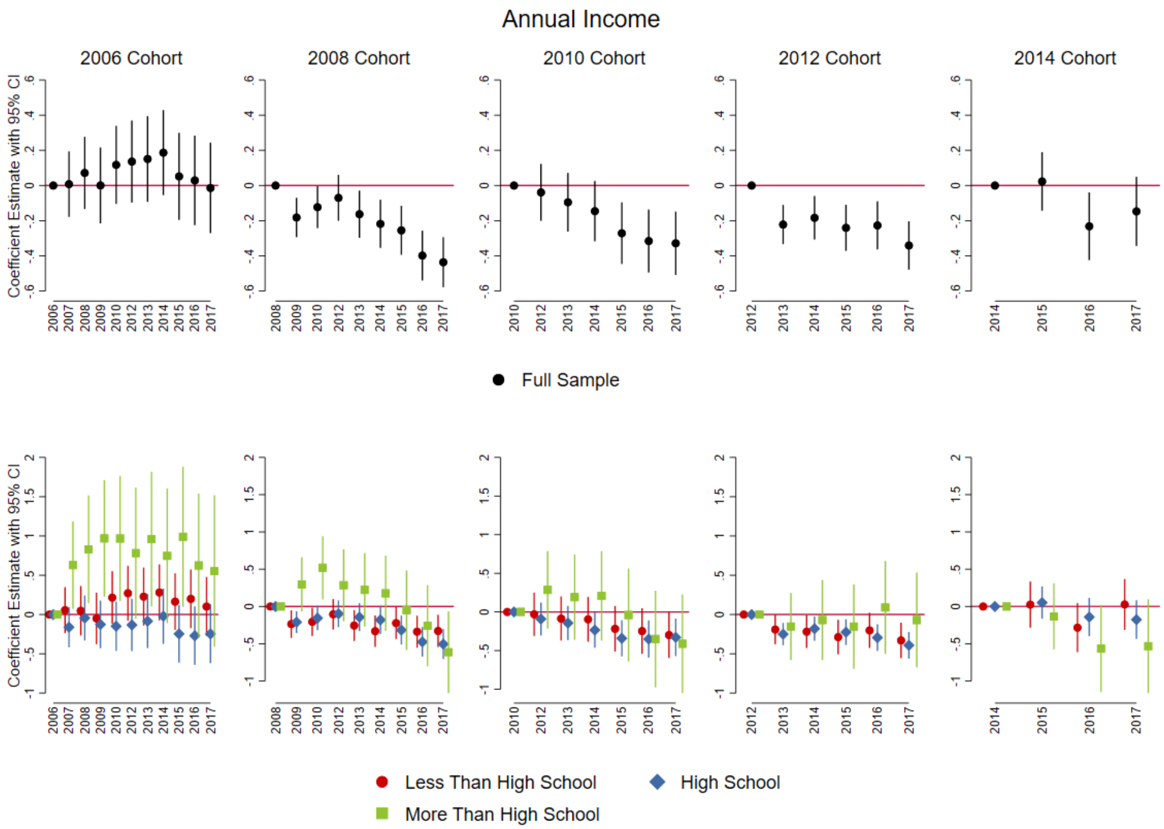
As with poached workers, it is apparent that positive wage effects of oil among survivors hide negative effects when the sample is expanded to include all workers (including those without formal jobs in later years). Figure 8 reports number of months employed per year for new hires. New hires into oil in 2006 are employed as much as matched controls in subsequent years. High-education new hires in 2006 enjoy weakly significant positive employment effects; medium and low-education workers are indistinguishable from matched controls. In later cohorts, oil-linked new hires are less often employed. New hires in 2008 experience significantly negative employment effects as a result of the brief 2008-2009 bust (24.5% fewer months employed in 2009), then recover slightly during the 2010-2013 boom years before again suffering significant negative outcomes after 2013 (35.9% fewer months employed in 2017). Negative effects for the 2008 cohort are driven by low and medium education workers, who are systematically less-often employed after being newly hired into oil-linked sectors. High-education new hires in 2008 enjoy significantly positive employment effects through 2014. New hires in 2010, 2012, and 2014 cohorts experience overwhelmingly negative employment effects (-29.7%, -37.1%, and -17.3% by 2017, respectively).

Figure 8: Months Employed Per Year After New Hire into Oil-Linked Sector



Note: Event studies regress months employed per year on relative time indicators after new hire into an oil-linked establishment (t omitted). Standard errors are clustered at the individual level, and individual and year fixed effects are included. 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. 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. New hires are defined as workers who are hired into their firm formal job. The first row presents results for the full sample of matched newly hired workers; the second row reports coefficient estimates and standard errors for each education category (less than high school, high school complete, and more than high school) separately relative to its own matched controls.

Figure 9: Annual Earnings After New Hire into Oil-Linked Sector



Note: Event studies regress annual formal earnings on relative time indicators after new hire into an oil-linked establishment (t omitted). Earnings 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. Annual earnings refers to total formal earnings for each worker across all formal jobs. This specification keeps all treated individuals and matched controls, 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. 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. New hires are defined as workers who are hired into their firm formal job. The first row presents results for the full sample of matched newly hired workers; the second row reports coefficient estimates and standard errors for each education category (less than high school, high school complete, and more than high school) separately relative to its own matched controls.

Figure 9 reports estimates of the effect of being newly hired into an oil-linked establishment on annual formal earnings. For these specifications, we again retain all newly hired matched workers in sample, whether or not they are formally employed in subsequent years. On average, the 2006 cohort earns approximately as much as matched workers in other sectors, with high-education workers enjoying weakly positive earnings effects through 2017. New hires in the 2008 cohort suffer momentarily from the 2008-2009 mini-bust, recover during peak boom years, then suffer earnings losses after the major oil bust of 2014. Low and medium-education new hires in 2008 earn significantly less in every subsequent year. New hires in the 2010 and 2012 cohorts suffer steadily worsening earnings effects over time (-28.0% and -28.9% by 2017, respectively), with nearly the entirety of these negative effects driven by earnings losses among low and medium-education workers (-28.1% and -32.4% by 2017, for low and medium-education workers in the 2012 cohort, respectively). New hires in 2014 experience negative earnings effects, though these are driven by negative effects among high-education workers (-41.4% in 2017).

Individual and Aggregate Welfare Effects

To better understand the lifetime earnings impacts of being hired into oil-linked sectors on individual workers, and the aggregate effects on cohorts and across the entire sample, we perform back-of-the-envelope calculations wherein we multiply baseline average incomes for the population of poached workers by semi-elasticities derived from year-by-year treatment effect estimates. We sum these multiples of baseline average incomes across years to calculate cumulative post-treatment net earnings effects.[10] Summaries of these back-of-the-envelope individual and aggregate net earnings calculations are reported in Appendix Tables B17 (poached) and B18 (new hires).

We find that, across all cohorts, workers poached into oil-linked establishments earn an individual average of R$28,814 more than what matched control workers earn (up to 2017, with all monetary values deflated to constant 2018 $BRL). However, all of the benefits are captured by the 2006 and (to a lesser degree) 2010 cohorts. Workers poached into oil in 2006 earn a net R$277,116 more than matched controls by 2017, which constitutes 763% of their baseline earnings, or R$23,093 net positive earnings per post-poach year. Workers poached in 2010 earn R$18,386 more over their careers, which is 58.3% of their baseline earnings or R$2,298 more per post-poach year. Workers poached in the 2008, 2012, and 2014 cohorts earn significantly less than matched controls in other sectors (R$-9,990, R$-16,513, and R$-12,228, respectively over the full post-poach period, or R$-999, R$-2,752, and R$-3,057 per post-poach year). 2008 poaches start off badly by coinciding with the 2008-2009 bust triggered by the Global Financial Crisis and never recover. Poaches in 2012 and 2014 start off too close to the major Brazilian oil bust of 2014. Excluding the 2006 cohort, workers poached into oil earn R$2,363 less than matched controls up to 2017, or R$338 less per post-poach year. In aggregate, workers poached into oil-linked establishments in 2006 make R$4,252,901,630

[10] Specifically, lifetime net oil earnings are calculated by (i) converting each relative year indicator's post-poach coefficient estimate into a semi-elasticity: $(100 * (e^{\hat{\beta}} - 1))$; (ii) multiplying these semi-elasticities by baseline average income; (iii) summing these "treated" incomes across all post-poach years; (iv) computing the difference between the sum of treated incomes after the poach and control incomes (an extrapolation of baseline average incomes across all years after the poach).

more as a result of being poached. Workers across all other cohorts make R$288,865,844 *less*.

Cohort-level averages disguise significant heterogeneity by level of education. For low-education workers (less than secondary school), all poached cohorts earn less than matched controls in other sectors (-R$29,143 total and -R$3,643 per post-poach year, equivalent to 155% of baseline income, by 2017). In aggregate, low-education workers earn R$1,012,233,789 less than matched control workers in other sectors. Medium-education workers (secondary school complete) poached in 2006 and 2010 earn more than matched controls (+R$15,420 and +R$6,923, respectively, or +R$1,285 and +R$865 per post-poach year), while other cohorts earn less. Overall, medium-education poached workers earn R$1,662 (R$-110,799,338 in aggregate) less than matched controls by 2017 when excluding the R$106,659,194 aggregate earnings benefits accumulated by the 2006 cohort. Among high-education workers (more than secondary education), all but the 2014 cohort of poached workers earn net positive values, ranging from an extraordinary R$1,617,690 net individual earnings for high-education poaches in 2006 (2000% of baseline earnings, or R$134,808 per post-poach year), to R$31,168 in 2008, R$99,032 in 2010, R$41,282 in 2012, and R$-41,568 in 2014. In aggregate, high-education poaches earn a net +R$7,823,920,897 by 2017, but only net +R$727,114,218 when the 2006 cohort is excluded. Thus, high-education poaches in 2006 retained 91% of all positive earnings effects enjoyed by high-education workers throughout the boom and bust cycle. In sum, high-education early entrants captured most of the benefits of the oil boom. Later entrants were left significantly worse off than their matched colleagues who were poached into other sectors. On average, low-education poached workers never benefited from the boom.

Among new hires, the 2006 cohort subsequently enjoys positive individual earnings effects of being hired into oil, totalling +R$14,972 in total by 2017 (93% of baseline earnings), or R$1,248 per post-hire year. All other cohorts of new hires earn less than matched workers newly hired into other sectors by 2017 (R$-2,834, R$-2,389, R$-3,308, and R$-1,561 per post-hire year, respectively for the 2008, 2010, 2012, and 2014 cohorts). In aggregate, new hires earned R$7,546,303,096 *less* than their matched counterparts by 2017 as a result of being hired into oil-linked sectors.

Among low-education new hires, only the 2006 cohort benefits, earning R$24,271 more than matched workers by 2017, or R$2,023 per post-hire year. All subsequent cohorts are left worse off, with an aggregate net earnings effect across all low-education cohorts equal to R$-1,785,922,491 by 2017. All medium-education cohorts experience earnings losses relative to matched controls (averaging R$-18,225 across all cohorts, or R$-2,278 per post-hire year), for an aggregate loss of R$-3,502,876,062 among this group. High-education workers experience monotonically declining returns to being newly hired into oil-linked sectors across cohorts. New hires in 2006 earn R$536,050 more than matched controls by 2017 (1410% of baseline), or R$44,671 per post-hire year. Subsequent cohorts earn R$58,261 more by 2017, or R$5,826 per post-hire year (2008 cohort); R$10,434 by 2017, or R$1,304 per post-hire year (2010 cohort); R$-12,969 by 2017, or R$-2,161 per post-hire year (2012 cohort); and R$-39,500 by 2017, or R$-9,875 per post-hire year (2014 cohort). In aggregate, high-education new hires earn +R$3,063,729,636 more than matched controls in other sectors, but only +R$113,310,818 when the 2006 cohort is excluded.

7 Robustness Checks

In our preferred specifications, we estimate dynamic effects of being poached or newly hired into an oil-linked establishment (i.e., an establishment in the oil industry or a closely-linked upstream or downstream sector) in year t, relative to being poached or newly hired into another sector in the same year. We use coarsened exact pre-matching to restrict our sample to treated and control workers who are exactly comparable (after binning continuous variables) across observable characteristics. The pre-matching step improves internal validity of our estimates, but may reduce external validity if the matched sample differs from the overall population. In this Section, we conduct robustness checks to test the sensitivity of our results to alternative definitions of "oil-linked" sectors and alternative matching specifications. We also test whether our results are sensitive to restricting samples to workers who are comparable across cohorts.

Restrict "Treatment" to Directly Oil-Linked Workers, with Looser Matching Criteria
First, we re-estimate our main event studies using a stricter definition of "oil-linked" that consists only of directly-linked sectors (e.g., petroleum extraction and support activities, petroleum refining, construction of ships and floating structures) and uses looser matching criteria to retain more treated workers in sample. Sectors included in this direct oil-linked classification are reported in Appendix B4. Looser matching criteria include exact matches on education level, sex, destination-municipality, coarsened salary bins [... more detail here]. Using these criteria, we match 65.9% of treated workers in the 2006 cohort, 47.8% in the 2008 cohort, 71.5% in the 2010 cohort, 54.2% in the 2012 cohort, and 69.6% in the 2014 cohort. This direct oil/loose match specification more reliably identifies oil-linked workers (since not all upstream and downstream oil-linked sectors are truly oil-linked) and improves external validity, as it retains a majority of treated workers in-sample.

We report results from the direct oil/loose match specification in Appendix Figures C1 and C2. Results align with our preferred specifications. Among workers poached into sectors with direct ties to oil, effects on labor market outcomes follow the same trend as shown for the broader sample. In this robustness check, the 2006 cohort enjoys significant and dynamic wage premiums, which are approximately three times larger than corresponding estimates in our preferred specification. Subsequent cohorts enjoy steadily declining, yet positive wage premiums that are also 2-3 times larger in magnitude than our preferred estimates. Directly oil-linked poaches in 2006 also enjoy dynamically growing employment and earnings benefits relative to matched controls, with positive effects driven entirely by high-education workers. Estimated magnitudes are again several times larger than our preferred specification. Medium-education oil workers do significantly better than matched controls in the 2006 and 2010 cohorts; low-education workers never benefit during boom years and suffer negative employment and earnings effects during busts. Appendix Figure C2 shows results for new hires. This figure also follows trends observed in our preferred specification. 2006 new hires earn significantly more than matched controls up to 2017, but do not exhibit the dynamic earnings growth enjoyed by poached workers. Subsequent cohorts are left worse off than matched controls. One stand-out finding is that high-education new hires into oil in 2010 earn significantly less than matched controls in other sectors, which aligns with the boom in oil-linked higher education graduations at this time. This endogenous oil-linked education boom may have created a glut among this category of workers. Overall, larger treatment effect estimates for the directly oil-linked sample are intuitive: workers with closer ties to the booming and busting sector experienced the same trends as our broader sample, but to an exaggerated degree.

Restrict Samples to 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 along the coast (shipyards serve as assembly nodes in the upstream oil supply chain). Establishments far from shipyards may be identified as oil-linked by their sector codes, but may not have ties to the oil sector in practice. For instance, an establishment that produces steel pipes would be classified as oil-linked in our preferred specification, but if this firm is located in the interior of the country–distant from most oil industry activities–it is likely that it produces pipes for non-oil end-users and consequently does not experience the oil boom and bust. In a second robustness exercise, we re-estimate our main specifications with matched samples limited to workers poached or newly-hired into destination municipalities that are within 50km. of a shipyard. Oil-linked establishments within these distance cutoffs are more likely to have true ties to the oil industry.

We report results from this exercise in Appendix Figures C3 and C4. Findings align with our preferred specifications, indicating that our results are not driven by workers who are distant from oil hubs and falsely-identified as oil-linked. Coefficient estimates in this robustness check are several times larger than those in our main specifications. As with the first robustness check, this finding is intuitive: workers closer to oil hubs are more exposed to the booming and busting oil sector, and thus feel both positive and negative treatment effects more strongly.

Restrict Samples to Workers Who Are Comparable Across Cohorts

The progression of Brazil's oil boom could induce changes in the composition of cohorts entering the oil sector over time. For instance, early entrants may be more forward-looking or risk-loving than later entrants. During boom periods, oil-linked firms may be desperate for workers and thus lower their hiring standards. On the other hand, workers may try to rush into oil during booms, giving oil-linked firms their pick of top workers. To account for potential differences in worker-type across cohorts (which could compromise our ability to make meaningful cross-cohort comparisons), we re-estimate event study specifications using sub-samples of each poached or newly hired cohort that share common support with the baseline 2006 cohort. Specifically, we preserve in sample only individuals from the 2006 poached and newly hired cohorts and subsequent cohorts (2008, 2010, 2012, and 2014) who match exactly on education, sex, nonwhite indicator, and age bins ($<$16, 17-22, 23-28, 29-32, 33-36, 37-40, 41-50, 51-60, $>$60). For new hires, we also match on first-job wage bins and firm size bins as described in section 4.2. For poaches, we also match on previous job wage bins and firm size bins. This procedure limits our matched treated and control samples to individuals who are comparable across observable characteristics with the baseline 2006 cohort.

We report results of this exercise in Appendix Figures C5 and C6. Results are very similar to results reported in our preferred specifications, confirming that changes in outcomes observed between cohorts are not driven by changes in cohort composition.

Apply looser matching criteria to preserve larger share of treated units in sample

Our preferred specifications restrict samples of poached and newly hired workers by imposing very strict matching criteria (e.g., poached from the same firm, very similar wages in years prior to poach). While this strengthens the internal validity of our estimates, it somewhat weakens external validity by making matched treated workers somewhat unbalanced when compared to the full population of poaches or new hires. To assess whether our decision to maximize internal validity through very strict matching criteria affects our ability to generalize results to the full treated population, we estimate a robustness check wherein we maintain our broad definition of oil-linked sectors (including upstream and downstream sectors) but apply much looser matching criteria to retain larger shares of treated workers in sample. In these specifications, we match exactly on education, race, and sex, age bins (<16, 17-22, 23-28, 29-32, 33-36, 37-40, 41-50, 51-60, >60), broad pre-poach wage bins (0-1, 2-5, 6-10, and >10 minimum wages), and destination microregion (equivalent to commuting zone).

Placebo Tests

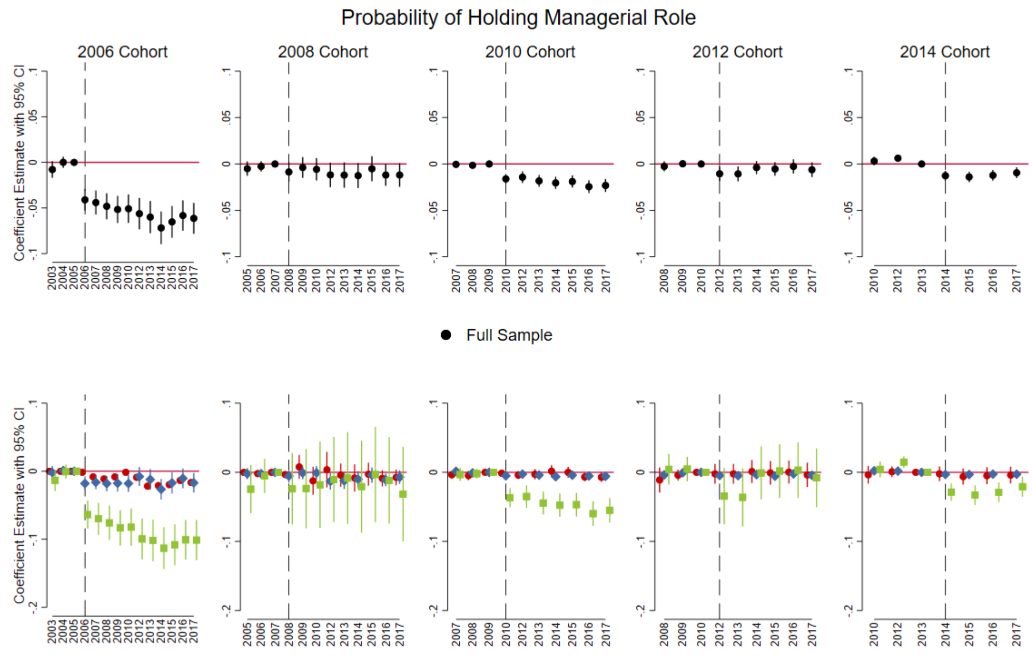
Finally, we conduct placebo tests to explore the possibility that our event study estimates could have arisen by chance. Within each cohort-level matched sample of poached and newly hired workers, we randomly assign placebo treatment to 20% of workers 100 times (since approximately 20% of each sample is truly treated by an oil-linked hire). We then re-estimate our main specifications for each placebo treatment and overlay real treated estimates for comparison.

We report results of this exercise for hourly wages and annual formal earnings in Appendix Figure C7. Results show that wage effect estimates for the 2006 and 2010 cohorts did not arise by chance: coefficients and standard errors for the true treated group are significantly different than the mass of treatment effect estimates for placebo treated groups. Wage effect estimates for other cohorts fall within the mass of placebo effect estimates, precisely because true treatment effect estimates for these cohorts are for the most part insignificantly different from null effects. Turning to earnings, coefficient estimates for the true treated group are larger than for any placebo treated group, and standard errors are only overlapped by a few extreme iterations of placebo treated groups, again confirming that these results are very unlikely to have arisen by chance. Likewise, negative earnings effects on the 2014 cohort fall below all but a few extreme iterations of placebo treated groups.

8 Mechanism I: Managerial and Professional Roles

Why do poached workers in 2006 capture such dramatic shares of overall earnings from the oil boom? In this section, we explore workers' occupations after being poached or newly hired into oil-linked sectors, relative to matched workers who are poached or newly hired into other sectors in the same year. Figure 10 reports coefficient estimates from linear probability models that regress an indicator for holding a managerial occupation (e.g., "leader," "director," or "manager") on relative time indicators around being poached into oil. Results show that workers poached into oil in 2006 (and to a lesser extent in 2010) are significantly less likely to hold managerial roles relative to matched workers in other sectors. These effects are driven by high-education workers.

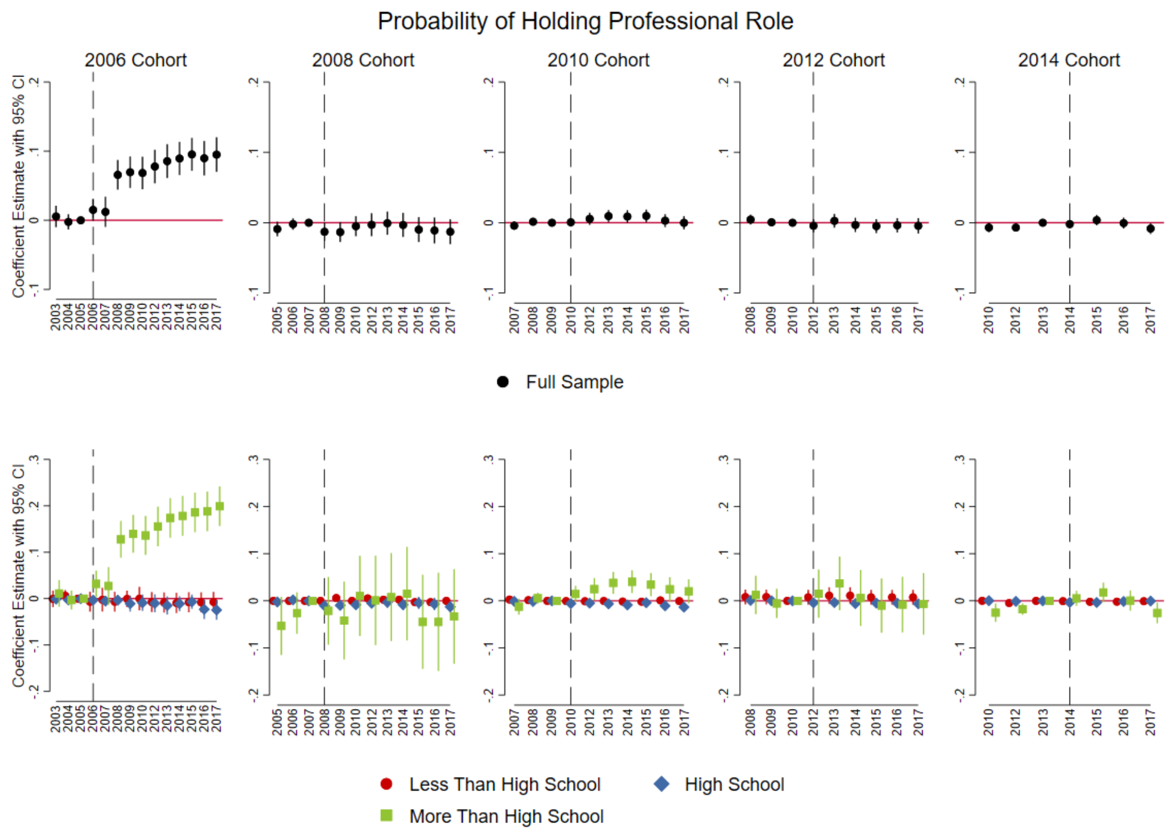
Figure 10: Probability of Holding a Managerial Role after Poach into Oil-Linked Sector



Note: Managerial roles are defined as CBO occupations with codes beginning with 1. These roles are primarily described as "leader," "director," or "manager." A binary indicator for "managerial role" turns from 0 to 1 within a worker-year observation if the worker held an occupation with code beginning in 1 in that year. Binary outcomes are regressed on individual and year fixed effects and relative time indicators around year of poach into oil (baseline $= t-1$) in a Linear Probability Model approach. Relative time indicators for never-treated matched controls are always set to -1. Standard errors are clustered at the individual level. Treated individuals (poached into oil-linked sector in year t) are compared to individuals poached into other sectors in year t who matched on wage and age bins, education, sex, race, occupation category, and firm during a two-year matching window prior to poach, and who were poached into the same destination municipality in t. Poaching is defined as voluntary exit from previous firm and rehire at a new firm within 4 months. The first row shows coefficient estimates and 95% confidence intervals for the full sample of matched poached workers; the second row reports coefficient estimates and 95% confidence intervals for each education category (less than high school, high school complete, more than high school) separately relative to its own matched controls.

Figure 11 shows results from the same specifications, but with an indicator for holding professional roles (e.g., "researcher," "scientist," "engineer," "pilot," "doctor," "professor," "lawyer," and "analyst"). Results show that workers poached into oil in 2006 (and to a lesser degree in 2010) are significantly *more* likely to hold a professional role in subsequent years, with this effect driven by high-education workers. These occupation effects correspond with the large earnings gains enjoyed by high-education oil-linked poaches in 2006 and 2010. Early entrants into professional occupations may have played key roles in setting up production processes, thus acquiring institutional knowledge and hold-up power that allowed them to retain their jobs and command significant earnings premiums even during bust periods. Appendix Figures A12 and A13 report corresponding results for new hires and show null effects, indicating that new hires into oil did not move systematically into professional roles or out of managerial roles as poached workers did.

Figure 11: Probability of Holding a Professional Role after Poach into Oil-Linked Sector



Note: Professional roles are defined as CBO occupations with codes beginning with 2. These roles are primarily described as "researcher," "scientist," "engineer," "pilot," "doctor," "nurse," "professor," "lawyer," and "analyst." A binary indicator for "professional role" turns from 0 to 1 within a worker-year observation if the worker held an occupation with code beginning in 2 in that year. Binary outcomes are regressed on individual and year fixed effects and relative time indicators around year of poach into oil (baseline = $t-1$) in a Linear Probability Model approach. Relative time indicators for never-treated matched controls are always set to -1. Standard errors are clustered at the individual level. Treated individuals (poached into oil-linked sector in year t) are compared to individuals poached into other sectors in year t who matched on wage and age bins, education, sex, race, occupation category, and firm during a two-year matching window prior to poach, and who were poached into the same destination municipality in t. Poaching is defined as voluntary exit from previous firm and rehire at a new firm within 4 months. The first row shows coefficient estimates and 95% confidence intervals for the full sample of matched poached workers; the second row reports coefficient estimates and 95% confidence intervals for each education category (less than high school, high school complete, more than high school) separately relative to its own matched controls.

9 Mechanism II: Oil-Linked Higher Education

High-education newly hired workers in oil-linked sectors experience monotonically declining earnings premiums across the boom-bust cycle. High-education new hires in 2006 earn R$44,671 per post-hire year more than matched counterfactual workers in other sectors. New hires in subsequent cohorts earn R$5,826, R$1,304, R$-2,161, and R$-9,875 per post-hire year for the 2008, 2010, 2012, and 2014 cohorts, respectively. Declining returns for new hires contrast with returns for high-education poached workers, who earn R$134,808, R$3,117, R$12,379, R$6,880, and R$-10,392 per post-poach year for the 2006, 2008, 2010, 2012, and 2014 cohorts, respectively. In this Section, we propose a mechanism that may explain declining returns to new, high-education entrants into the oil-linked labor market: endogenous responses to the oil boom by both the demand and supply side of oil-linked higher education (i.e., students and degree-programs), which combined to create a labor glut of skilled oil workers. Poached workers may have been somewhat immune to this glut since their prior experience places them in a segmented labor market relative to new entrants.

A booming resource sector may induce endogenous education choices among current or prospective workers (Balza et al., 2021). A boom in oil-skill-biased labor demand could: (i) induce individuals who are already in the workforce to acquire additional oil-linked training; (ii) draw individuals who would otherwise pursue other areas of higher education toward oil-linked courses, leaving total higher education attainment unchanged but shifting the composition toward oil-linked skills.[11]

Oil-linked firms may encourage oil-specific human capital formation by creating industry-led technical training programs or sponsoring oil-linked university degrees, in order to ensure access to a ready supply of skilled workers. Collectively, endogenous individual and firm responses to a booming oil sector may increase oil-specific higher education attainment with a delay corresponding to the time required to graduate from higher education courses. This could range from 1-2 years for some technical programs to 4-6 years for university degrees. We therefore expect to observe a surge in oil-linked higher education graduates, and share of total graduates, after the beginning of the oil boom in 2006. We expect this surge to be larger and earlier in places that feel the oil boom most strongly (e.g., Rio de Janeiro state, the hub of Brazil's oil industry).

By the time students begin to graduate from oil-linked degree programs, the oil sector may have peaked or entered into decline, leading to a glut of oil-linked graduates that bids down wage

[11] A booming oil sector could reduce aggregate higher education attainment by increasing low-skill service employment (Corden and Neary's "income effect"), which draws students away from higher education. Aggregate effects of Brazil's oil boom and bust go beyond the scope of this analysis.

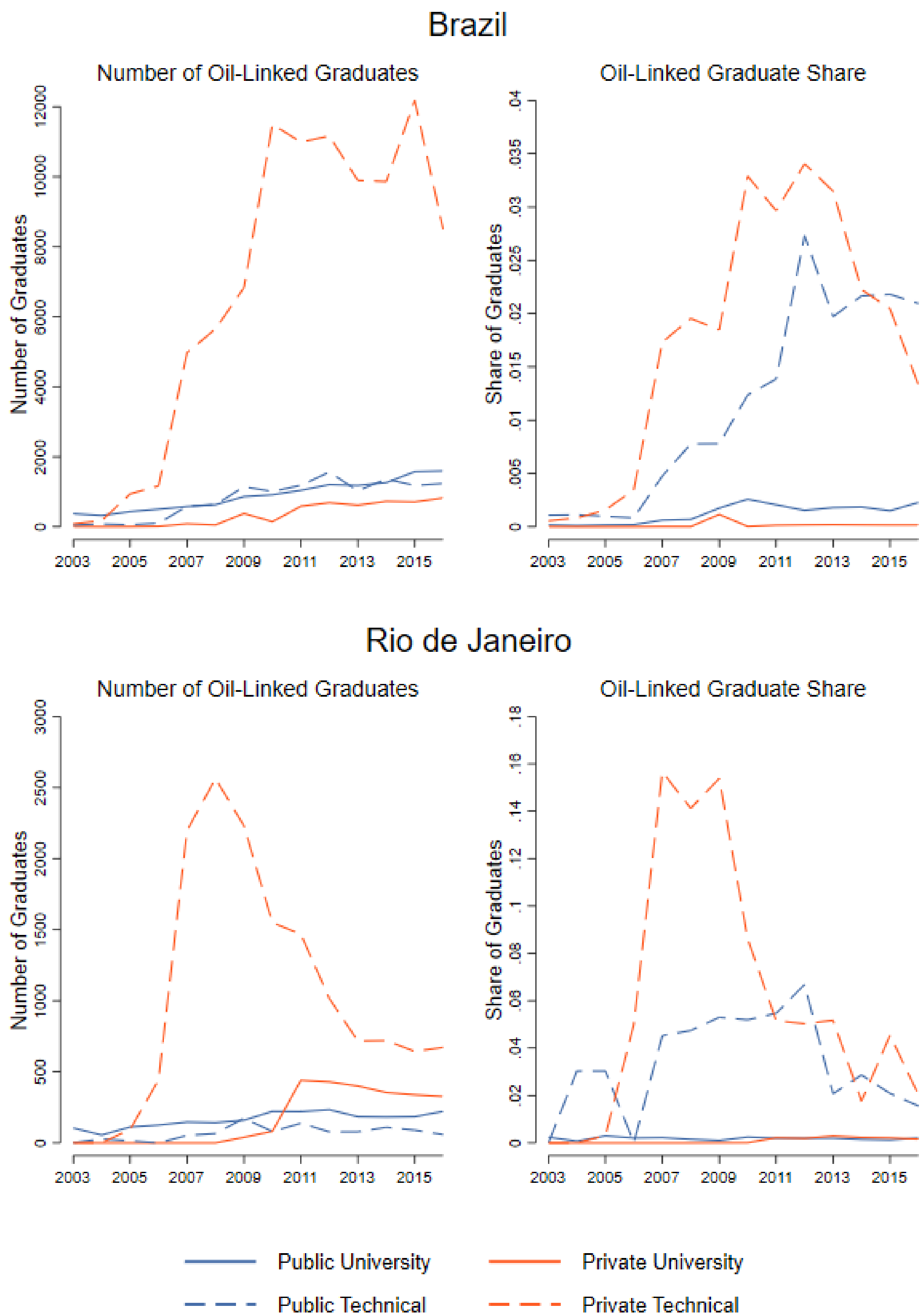
premiums of workers with higher education who are newly hired into oil-linked sectors. This is part and parcel of firms' motivation to open oil-linked training programs.

To assess these dynamics in the context of Brazil's oil boom and bust, we draw on data from Brazil's Higher Education Census, which reports number of graduates at the institution/degree-program/year level for the universe of Brazilian higher education institutions between 2003-2016.Using 6-digit degree-area (i.e., major) codes (*area do curso*), we classify 24 ouf of 1,104 total degree programs as "oil-linked" based on contextual knowledge. We categorise institutions as public/private and university/technical.[12] We sum number graduates to the municipality-year level for each combination of public/private, university/technical, and oil-linked/other. 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. For 233 institutions, we input missing municipality data manually. We list our definition of oil-linked majors in Appendix 2, Table 5.

Figure 12 shows number of graduates from oil-linked higher education degree programs each year between 2003 and 2016. Graduates are disaggregated into four types: public university, private university, public technical, and private technical. 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 higher education graduates in Brazil increased sharply from 2006 onward (corresponding with the oil boom), peaking around 2010-12. The increase was most dramatic in the private technical-training sector, which increased from 82 graduates (0.06% of total graduates in this category) in 2003 to 11,493 (3.29%) in 2010 and 12,177 (2.04%) in 2015, before falling to 8,500 (1.33%) in 2016. Public technical graduates also grew dramatically, from 49 (0.11%) in 2003 to 1,564 (2.73%) in 2012, before declining to 1,234 (2.10%) by 2016. Oil-linked public university graduates increased from 376 (0.02%) in 2003 to 1,205 (0.15%) by 2012, and further to 1,595 (0.23%) in 2016. Oil-linked private university graduates increased from 5 (0.001%) in 2003 to 688 (0.018%) in 2012, and further to 825 (0.017%) in 2016. A clear contrast between technical and university degrees is that technical programs are sufficiently short-term for enrolments (and later graduations) to react to the oil bust. University programs take 4-6 years to complete, leading many students who enrolled during boom years to graduate during unfavourable bust years.

[12]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*).

Figure 12: Number and Share of Oil-Linked Graduates



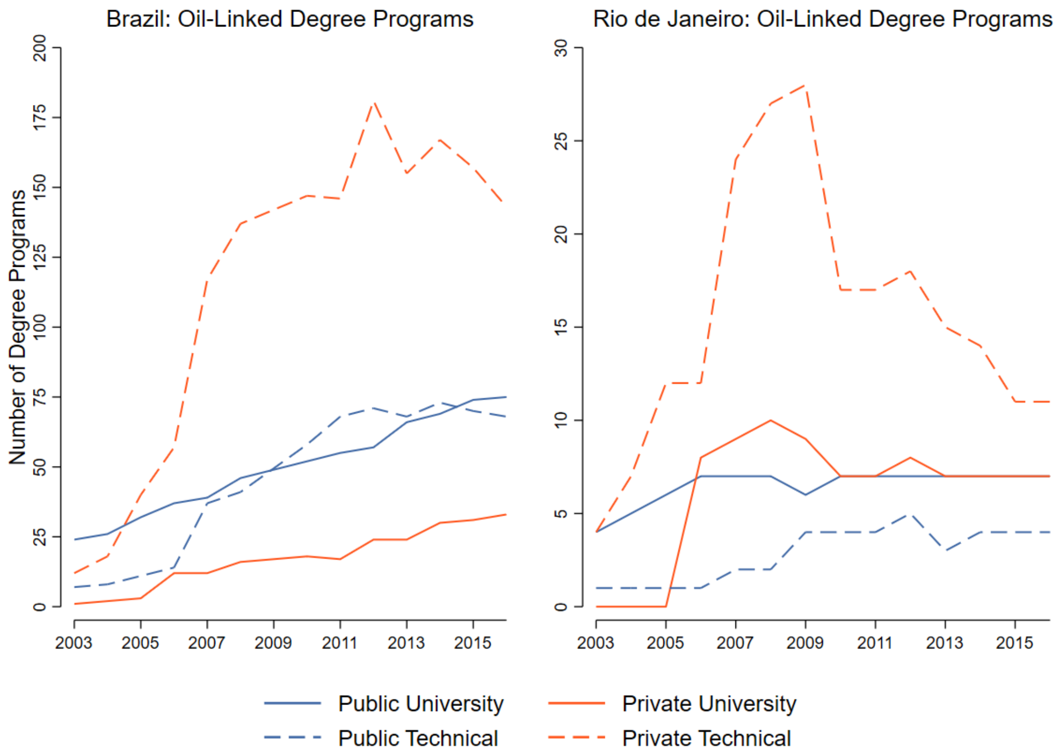
Note: Number and share of graduates are calculated from Brazil's Higher Education Census (2003-2016). Oil-linked majors are defined in Appendix 2, Table 4. University degrees typically take 4-6 years to complete; technical degrees typically take 1-2 years. Rio de Janeiro state is selected as an example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

Comparing country-level results to the state of Rio de Janeiro (the hub of Brazil's oil sector), it is evident that Rio's boom in oil-linked higher education preceded the national boom by approximately three years. This may be due to stronger early-boom signals felt by prospective students in this state. Oil-linked private technical graduates increased from 0 in 2003 to 2,563 (14.13% of total graduates in this category) in 2008, and then declined to 671 (2.03%) by 2016. Public technical graduates increased from 0 in 2003 to 173 (5.3%) in 2009, then declined to 58 (1.56%) in 2016. Oil-linked private university graduates increased from 0 in 2003 to 440 (0.003%) in 2009, and declined only slightly to 326 (0.16% of total) by 2016. Public university graduates were more stable, growing from 104 (0.24%) in 2003 to 233 (0.25%) in 2010, to 221 (0.21%) in 2016. As in the country-level results, technical degree graduations in Rio de Janeiro react more quickly to oil boom signals, and wind down more quickly with the oil bust.

Rapid growth in oil-linked graduations corresponded with public and private sector rollouts of oil-linked post-secondary degree programs. For Brazil as a whole, oil-linked public university programs grew from 24 in 2003 to 75 by 2016. Private university programs grew from 1 in 2003 to 33 by 2016. Technical programs fluctuated even more dramatically. Public oil-linked technical degree programs grew from 12 in 2003 to 181 in 2012, before declining to 143 by 2016. Public technical programs grew from 7 in 2003 to 73 in 2014, then declined slightly to 68 by 2016. Thus, it appears that technical programs responded pro-cyclically with the oil boom and bust, while 4-year programs continued to expand despite the 2014 downturn.

In Rio de Janeiro, oil-linked private technical degree programs increased from 4 in 2003 to 28 in 2009, then declined to 11 by 2016. Public technical programs followed a similar, though attenuated trend, growing from 1 in 2003, to 5 in 2012, to 4 in 2016. Private university programs expanded from 0 in 2003 to 10 in 2008, then declined to 7 by 2017. Finally, public oil-linked university programs grew from 4 in 2003 to 7 in 2016. Similar trends may be observed in other states affected by the oil boom and bust, including São Paulo and Espírito Santo (Appendix Figure A15).

Figure 13: Number of Oil-Linked Degree Programs



To further explore the effects of Brazil's spatially-concentrated oil boom on oil-linked graduates, we estimate a difference-in-differences specification to test whether oil-linked graduation increased more in municipalities close to shipyards (major supply-chain nexuses for oil inputs) during boom years. We aggregate number of oil-linked graduates to the municipality-year level in a panel dataset ranging from 2003-2016. We regress outcome y_{mt} (number of graduates or share of STEM graduates in oil-linked majors) on a proxy of oil boom intensity (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:

$$y_{mt} = \beta Close_m + \gamma Boom_t + \delta(Close_m \times Boom_t) + \mu_s + \epsilon_{mt} \tag{3}$$

Results, reported in Table 2, indicate that the number of graduates from oil-linked higher-education degree programs is significantly higher where the oil sector is most important to the economy (within 50km. of a shipyard) and during oil boom years (2006-2013). The difference-in-differences interaction term of oil-proximity and oil boom is significantly positive, indicating that oil-linked graduations increased most near shipyards during the boom. Disaggregating effects across degree-program categories, we observe that total effects are driven by significant effects of oil-proximity and oil boom on private technical training programs. The share of total STEM graduates earning oil-linked degrees is higher during oil boom years, and increases most near shipyards during the boom for technical training programs. These results provide suggestive evidence that an endogenous higher-education response occurred in which students specialised in oil-relevant skills in response to Brazil's oil boom.

Table 2: Effects of Exposure to Oil Boom on Number and Share of Oil-Linked Graduates (2003-2016)

	Number of Graduates from Oil-Linked Degree-Programs				
Variables	**Total**	**Public Univ.**	**Private Univ.**	**Public Tech.**	**Private 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
R-squared	0.074	0.076	0.037	0.014	0.067

	Share of STEM Graduates in Oil-Linked Degree-Programs				
Variables	**Total**	**Public Univ.**	**Private Univ.**	**Public Tech.**	**Private 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
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 and 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. 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 graduates in that specific category who earn an oil-linked degree.

10 Discussion

Oil Generates Intertemporal and Intra-cohort Inequality

Exposure to oil exerts drastically different effects across cohorts. Workers poached into oil-linked establishments at the beginning of the boom (2006) enjoy dynamic earnings growth relative to matched colleagues who are poached into other sectors in the same year. Workers poached in 2008 are immediately hit by job losses after the brief 2008-2009 oil price crash, and suffer persistently insignificant outcomes as a result, even through the following boom years. 2010 poaches enjoy significant positive earnings premiums until 2014, but these are an order of magnitude smaller than the premiums enjoyed by the 2006 cohort. Workers poached into oil in 2012 and 2014 enter at the cusp of the oil bust and consequently suffer negative earnings outcomes. The same trend holds for new hires, though it is less exaggerated. New hires in 2006 avoid earnings losses relative to matched workers in other sectors, while subsequent cohorts suffer significantly negative effects. This pattern introduces significant intertemporal inequality, wherein workers who enter the oil sector at the beginning of the boom capture almost the entirety of positive earnings effects. Workers who enter during the brief 2008-2009 downturn suffer persistent negative effects, and later cohorts enter too close to the 2014 bust, leaving them worse off than matched workers in other sectors.

Oil also generates significant inequality within cohorts. The entirety of earnings premiums enjoyed by the 2006 cohort is captured by high-education workers. Medium-education workers (with complete secondary education) are statistically indistinguishable from matched controls, and low-education workers poached into oil in 2006 are significantly worse off than their matched colleagues who were poached into other sectors. A similar trend persists across other poached cohorts and, to a lesser degree, across newly hired cohorts as well. Low-education workers never enjoy earnings premiums during oil boom years, and suffer job and earnings losses during busts.

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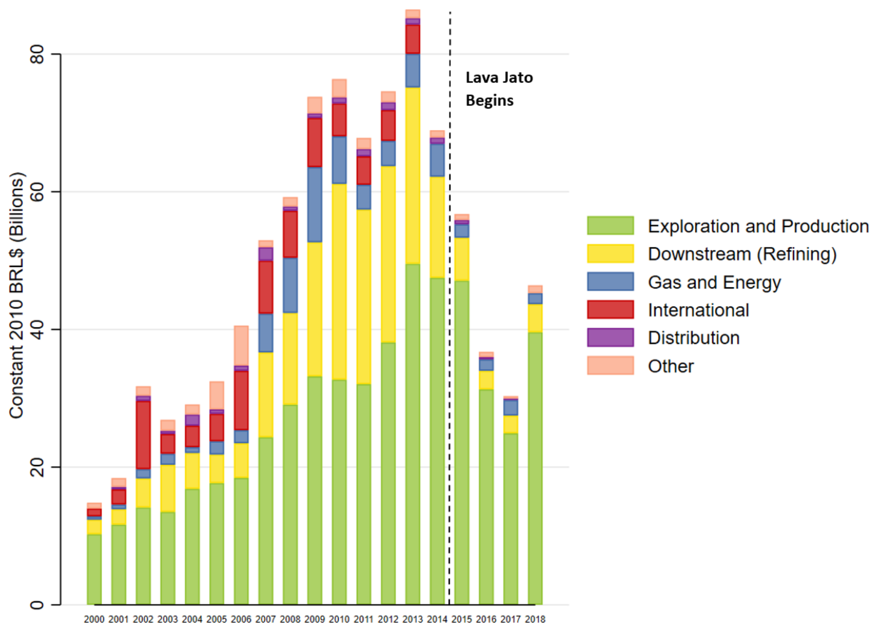
Appendices

Appendix A Supplementary Figures

Figure A1: Petrobras: Annual Investment by Category (2000-2018)



Source: Petrobras (2020)

Figure A2: Percent Change in Net Hires (Distance Bins)

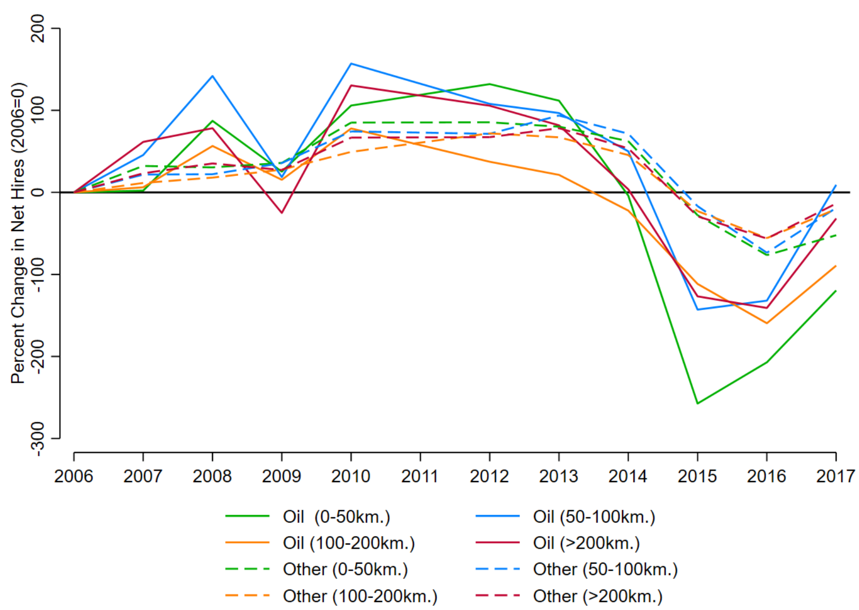


Figure A3: Percent Change in Net Hires (Supply Chain Linkages)

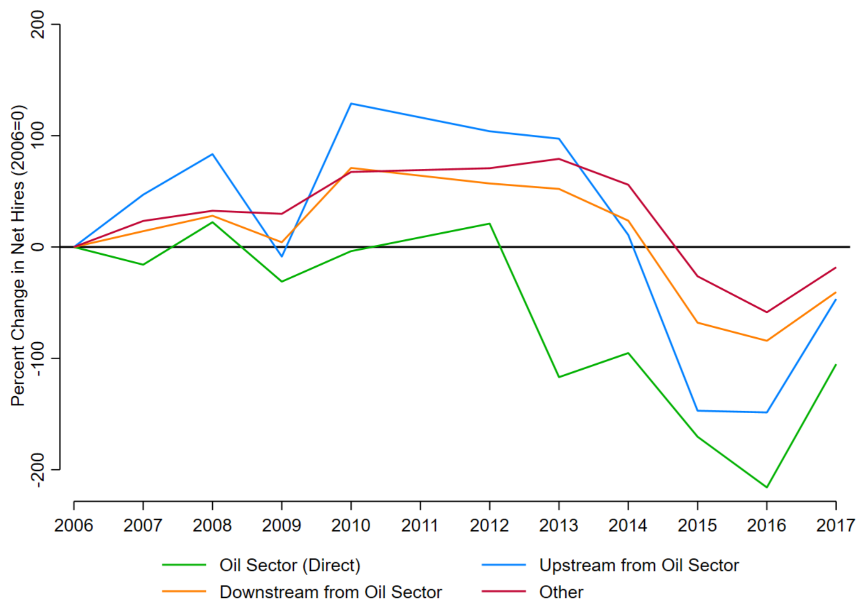


Figure A4: Formal Employment in Oil-Linked Sectors Relative to Total

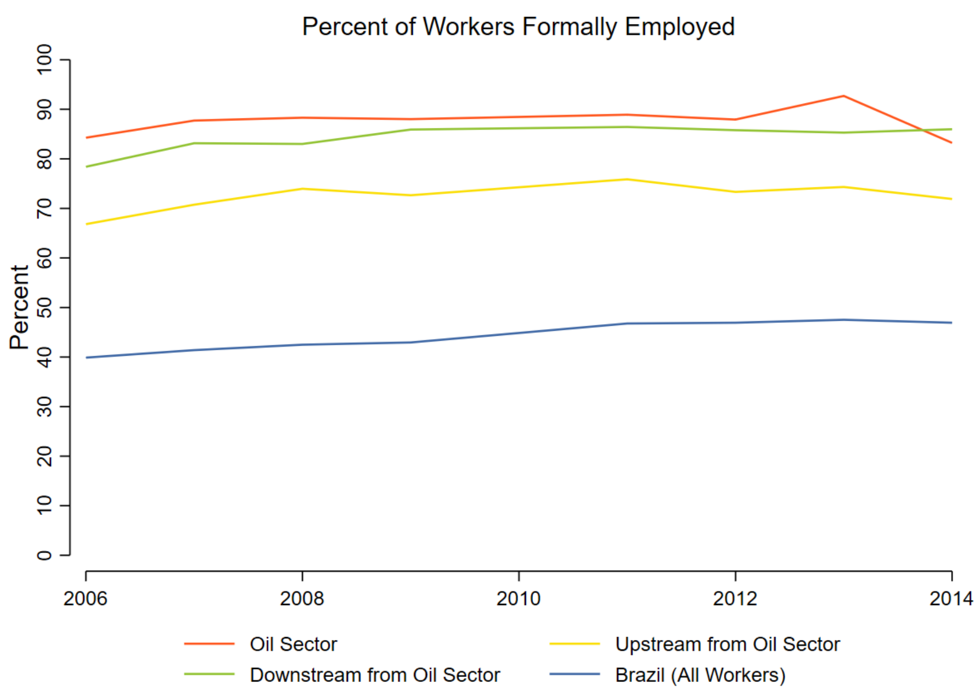


Figure A5: Average Monthly Earnings for Formal and Informal Workers

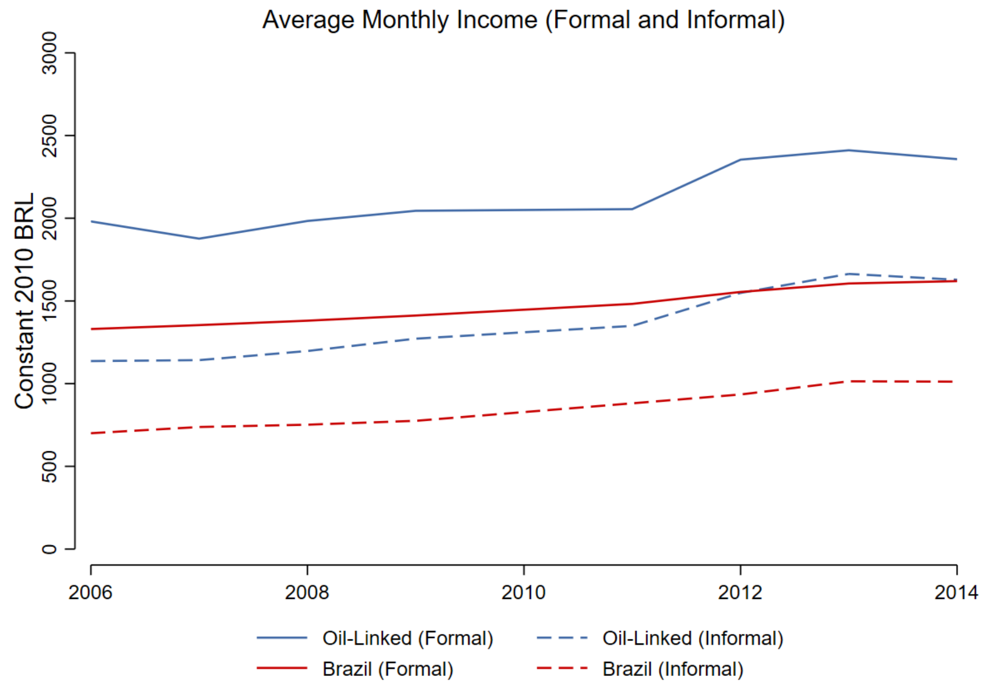


Figure A6: Hourly Wages After Poach into Oil-Linked Sector, by Age and Sex

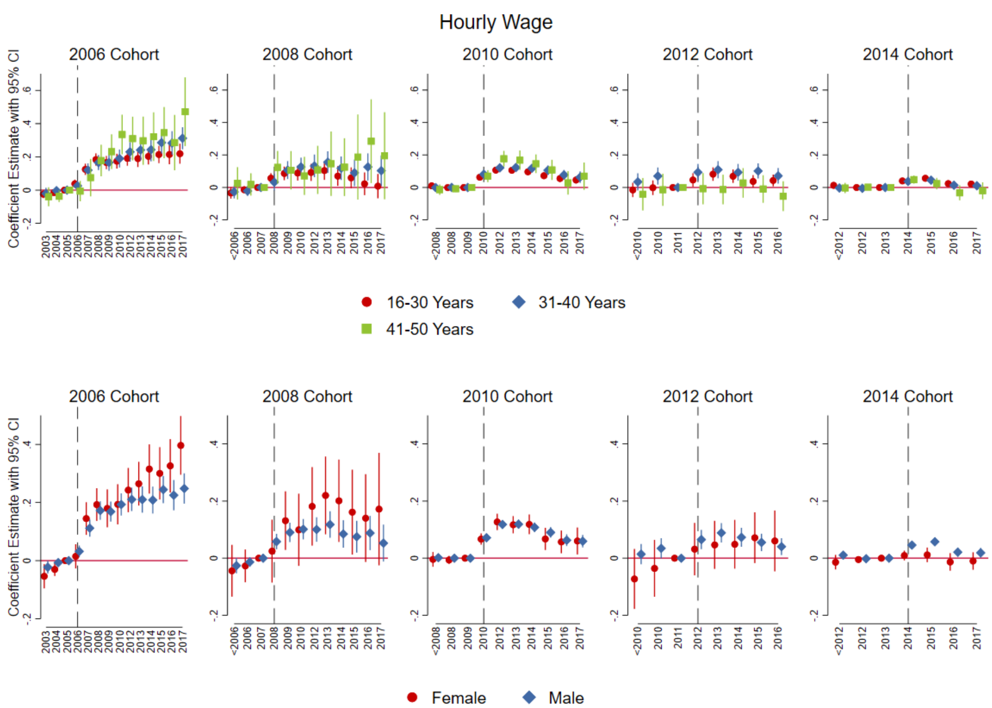


Figure A7: Months Employed Per Year After Poach into Oil-Linked Sector, by Age and Sex

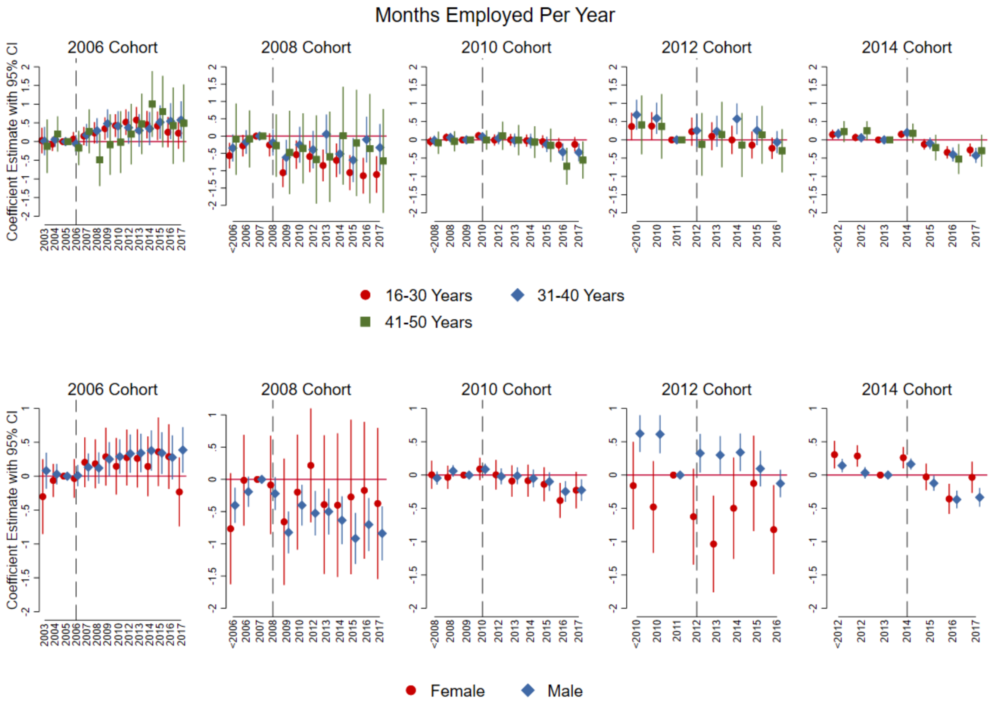


Figure A8: Annual Earnings After Poach into Oil-Linked Sector, by Age and Sex

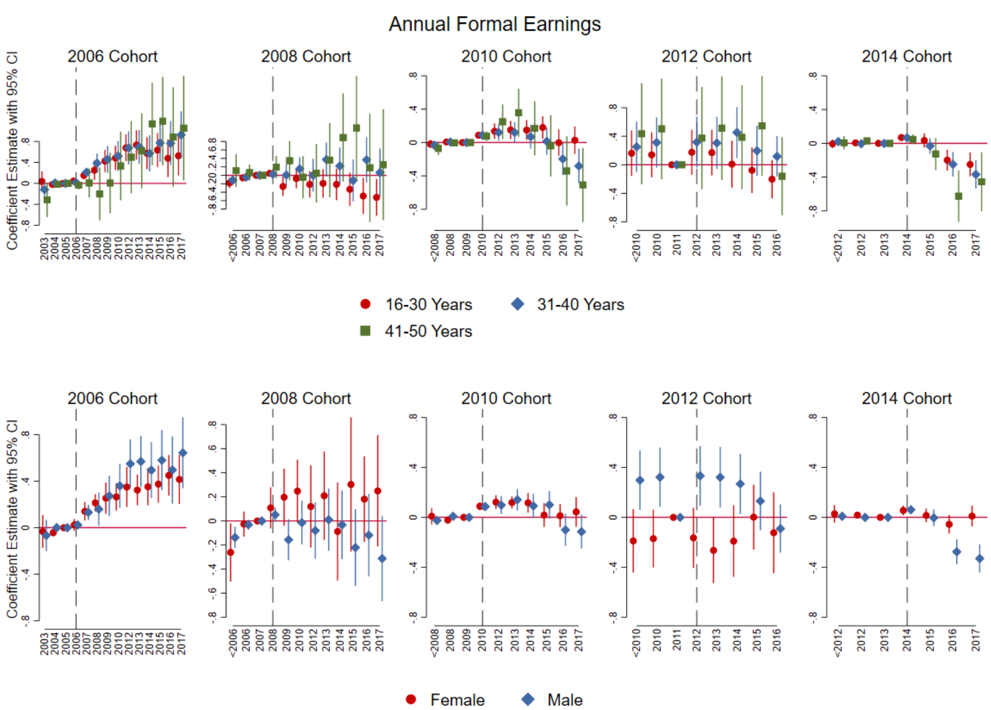


Figure A9: Hourly Wages After New Hire into Oil-Linked Sector, by Age and Sex

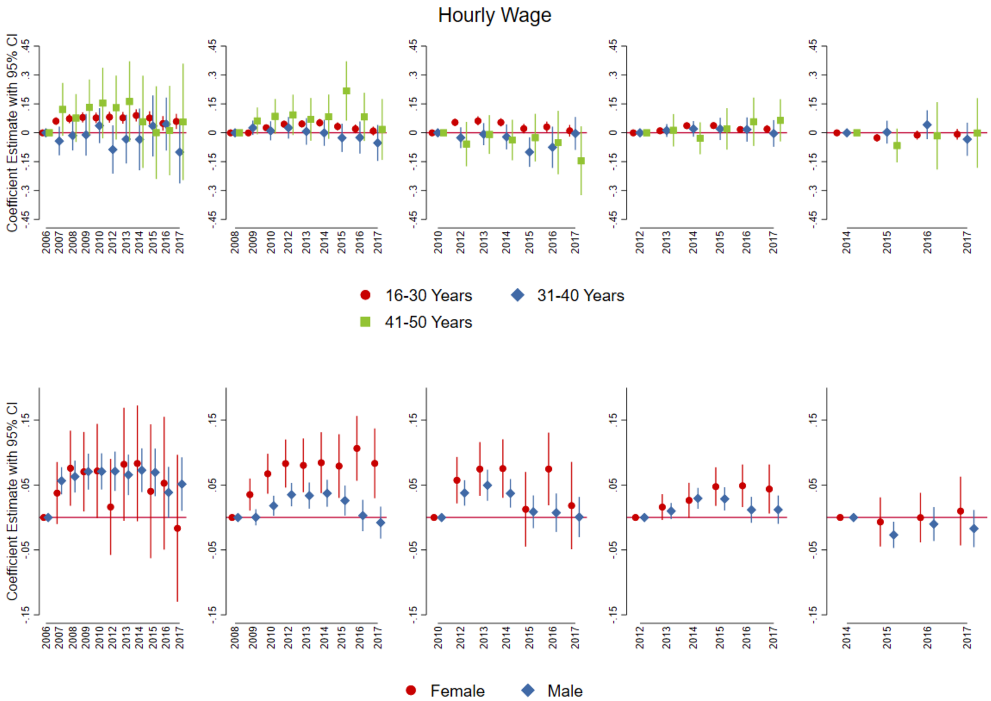


Figure A10: Months Employed Per Year After New Hire into Oil-Linked Sector, by Age and Sex

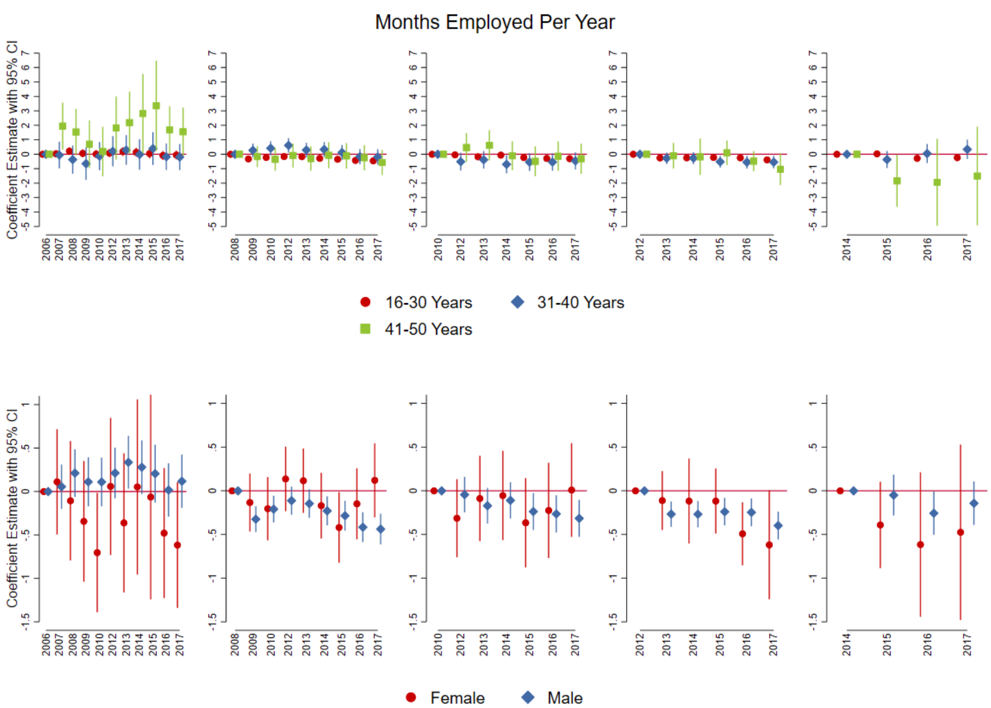


Figure A11: Annual Earnings After New Hire into Oil-Linked Sector, by Age and Sex

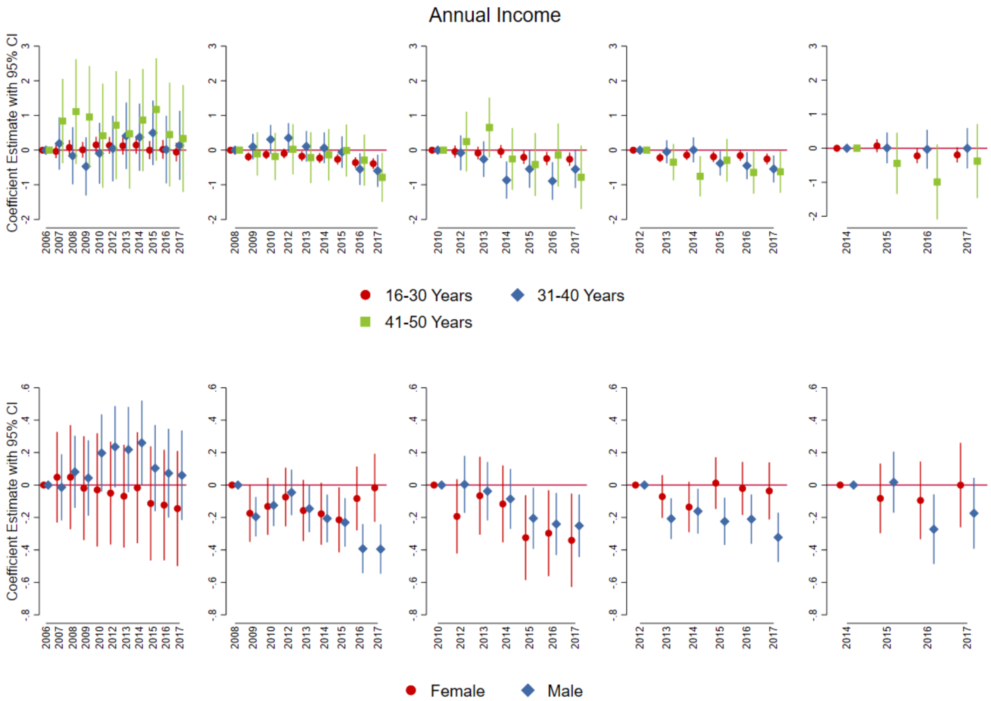


Figure A12: Probability of Holding a Managerial Role after New Hire into Oil-Linked Sector

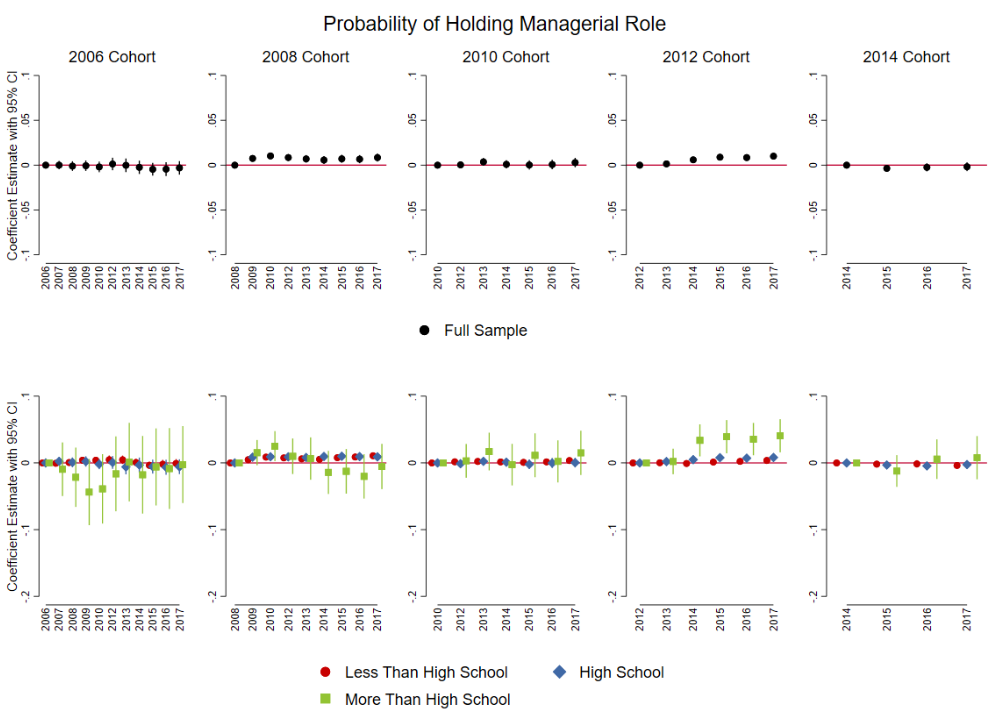


Figure A13: Probability of Holding a Professional Role after New Hire into Oil-Linked Sector

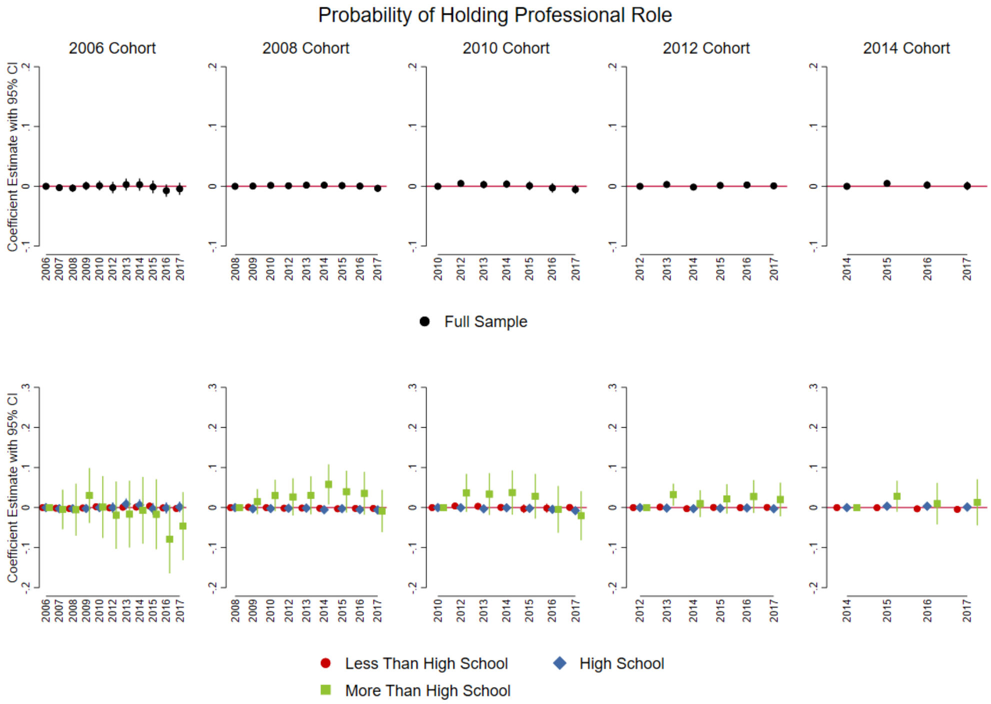


Figure A14: Number of Oil-Linked Degree Programs (São Paulo and Espírito Santo)

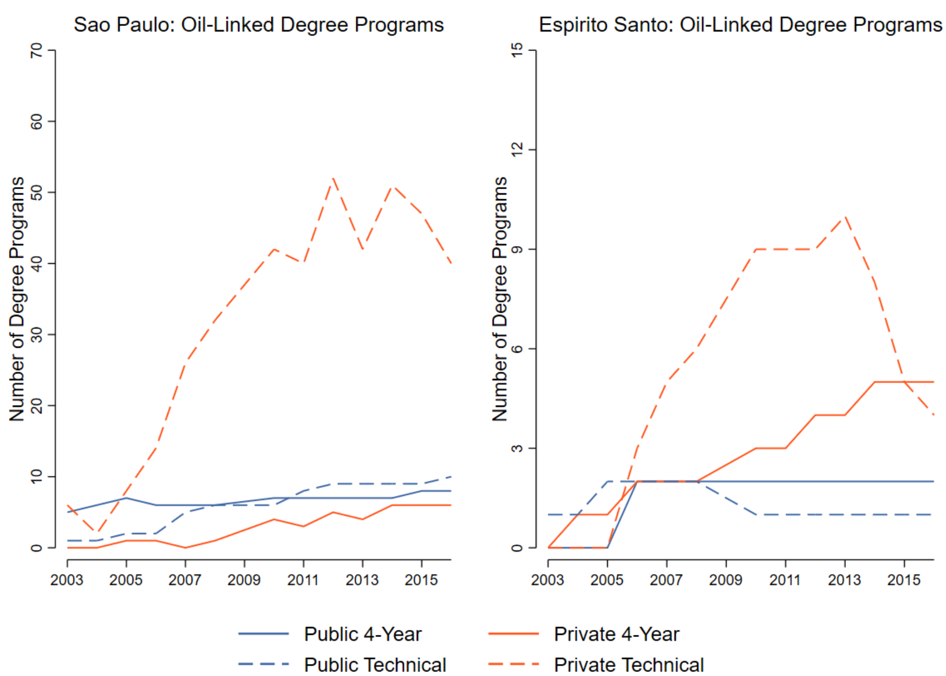
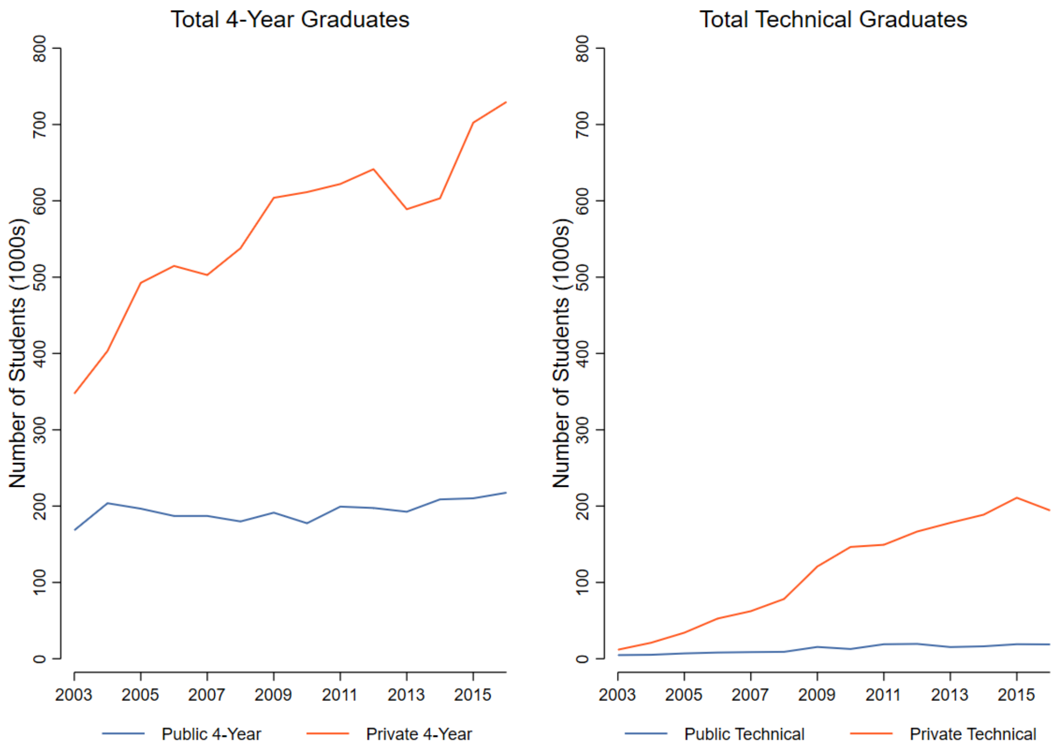


Figure A15: Total Number of Graduates (Brazil)



Appendix B Supplementary Tables

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 (Direct, Upstream, and Downstream)

Subclass	**Subclass Description**	**D**	**U (S)**	**U (L)**	**U Coef.**	**D (S)**	**D (L)**	**D Coef.**
0600001	Extraction of Petroleum and Natural Gas	1	0	0	1.068	0	0	1.068
0600003	Extraction and Processing of Tar Sands	1	0	0	1.068	0	0	1.068
0910600	Oil and Natural Gas Extraction Support Activities	1	0	0	1.068	0	0	1.068
6911701	Legal Services	0	0	1	0.055	0	0	0.000
6911703	Industrial Property Management	0	0	1	0.055	0	0	0.000
6920601	Accounting Services	0	0	1	0.055	0	0	0.000
6920602	Accounting and Tax Consulting and Auditing	0	0	1	0.055	0	0	0.000
7020400	Management Consulting	0	0	1	0.055	0	0	0.000
4911600	Rail Transport of Cargo	0	0	1	0.039	1	1	0.088
4930201	Road Transport of Cargo (Municipal)	0	0	1	0.039	1	1	0.088
4930202	Road Transport of Cargo (Inter-Municipal)	0	0	1	0.039	1	1	0.088
4940000	Pipeline Transport	0	1	1	0.039	1	1	0.088
1910100	Coke Plants	1	0	0	0.032	1	1	0.411
1921700	Fabrication of Refined Oil Products	1	0	0	0.032	1	1	0.411
1922501	Formulation of Fuel Products	1	0	0	0.032	1	1	0.411
1922502	Refining of Oil Lubricants	1	0	0	0.032	1	1	0.411
1922599	Fabrication of Other Petroleum Products	1	0	0	0.032	1	1	0.053
2811900	Fabrication of Motors/Turbines (ex. Air/ Road Vehicles)	0	1	1	0.027	0	0	0.000
2812700	Fabrication of Hydraulic and Pneumatic Equipment	0	1	1	0.027	0	0	0.000
2813500	Fabrication of Valves and Registers	0	1	1	0.027	0	0	0.000
2814301	Fabrication of Industrial Compressors	0	1	1	0.027	0	0	0.000
2815101	Fabrication of Industrial Ball Bearings	0	1	1	0.027	0	0	0.000
2815102	Fabrication of Transmission Equipment	0	1	1	0.027	0	0	0.000
2821601	Fabrication of Industrial Furnaces for Thermal Plants	0	1	1	0.027	0	0	0.000
2821602	Fabrication of Industrial Stoves and Furnaces	0	1	1	0.027	0	0	0.000
2822401	Fabrication of Machines for Transport/Elevation Ppl.	0	1	1	0.027	0	0	0.000
2822402	Fabrication of Machines for Transport/Elevation Cargo	0	1	1	0.027	0	0	0.000
2823200	Fabrication of Machines for Industrial HVAC	0	1	1	0.027	0	0	0.000
2824101	Fabrication of Industrial Air Conditioning Equipment	0	0	1	0.027	0	0	0.000
2825900	Fabrication of Machines for Sewage/Enviro. Treat	0	1	1	0.027	0	0	0.000
2840200	Fabrication of Machine-Tools	0	1	1	0.027	0	0	0.000
2851800	Fabrication of Machines/Equip. for Oil Prospecting	1	0	0	0.027	0	0	0.000
2861500	Fabrication of Machines for Metallurginal Industry	0	1	1	0.027	0	0	0.000
2411300	Production of Pig Iron	0	1	1	0.023	0	1	0.035
2412100	Production of Iron Alloys	0	1	1	0.023	0	1	0.035
2421100	Production of Semi-Finished Steel Products	0	1	1	0.023	0	1	0.035
2422901	Production of Steel Sheets	0	1	1	0.023	0	1	0.035
2422902	Production of Special Steel Sheets	0	1	1	0.023	0	1	0.035
2423701	Production of Steel Tubes (without Seams)	0	1	1	0.023	0	1	0.035
2423702	Production of Long Steel Sheets, except Tubes	0	1	1	0.023	0	1	0.035
2424501	Productions of Steel Wires	0	1	1	0.023	0		0.035
2424502	Production of Specialized Steel Products	0	1	1	0.023	0	0	0.035
2431800	Production of Steel Tubes (with Seams)	0	1	1	0.023	0	0	0.035
2439300	Production of Other Steel and Iron Tubes	0	1	1	0.023	0	0	0.035
5212500	Loading and Unloading of Cargo	0	1	1	0.021	0	1	0.000
5231101	Administration of Port Infrastructure	0	1	1	0.021	0	0	0.000
5231102	Operation of Port Terminals	0	1	1	0.021	0	0	0.000
5232000	Maritime Activity Management	0	1	1	0.021	0	0	0.000
5239700	Aquatic Transportation Support Activities	0	1	1	0.021	0	0	0.000
5250804	Logistic Organization of Cargo Transportation	0	1	1	0.021	0	0	0.000
4223500	Construction of Pipelines (except Water/Sewage)	0	1	1	0.021	0	0	0.000
4291000	Port and Maritime Projects	0	1	1	0.021	0	0	0.000
4292801	Construction of Metallic Structures	0	1	1	0.021	0	1	0.000
4292802	Industrial Construction Projects	0	1	1	0.021	0	1	0.000
4312600	Perforations and Drilling	0	1	0	0.021	0	0	0.000
4321500	Electrical Installation and Maintenance	0	1	1	0.021	0	1	0.000
4322301	Hydraulic, Sanitary, and Gas Installation	0	1	1	0.021	1	1	0.000
4322302	Installation and Maintenance of HVAC Systems	0	0	1	0.021	0	0	0.000
4322303	Installation of Fire Prevention Systems	0	1	1	0.021	0	0	0.000
4329102	Installation of Maritime Navigation Systems	0	1	1	0.021	0	0	0.000
4329105	Treatments for Heat, Noise, and Vibration Control	0	1	1	0.021	0	0	0.000
4399101	Project Management	0	1	1	0.021	0	0	0.000
4399104	Supply of Transport and Elevation Equipment	0	1	1	0.021	0	0	0.000
3311200	Maintenance and Repair of Tanks (except Vehicles)	0	1	1	0.020	0	1	0.000
3312102	Maintenance and Repair of Measurement Instruments	0	1	1	0.020	0	0	0.000
3312104	Maintenance and Repair of Optical Instruments	0	0	1	0.020	0	0	0.000
3313901	Maintenance and Repair of Eletrical Generators	0	1	1	0.020	0	0	0.000
3313902	Maintenance and Repair of Batteries (except Vehicles)	0	1	1	0.020	0	0	0.000
3313999	Maintenance and Repair of Other Electrical Machines	0	1	1	0.020	0	0	0.000

Note: Subclass refers to CNAE 2.0 Subclass. Subclass descriptions are abbreviated. D = Direct Oil-Link; U(S) = Upstream (Strict Definition); U(L) = Upstream (Loose Definition); U Coef. = Upstream Leontief Coefficient; D(S) = Downstream (Strict Definition); D(L) = Downstream (Loose Definition); D 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). We apply "strict" and "loose" classifications for robustness. This process is illustrated in Table B2.

Table B4: CNAE 2.0 Oil-Linked Subclasses (Direct, Upstream, and Downstream) Cont'd

Subclass	Subclass Description	D	U (S)	U (L)	U Coef.	D (S)	D (L)	D Coef.
3314701	Maintenance and Repair of Non-Eletrical Motors	0	1	1	0.020	0	0	0.000
3314702	Maintenance and Repair of Hydraulic/Pneumatic Equip.	0	1	1	0.020	0	0	0.000
3314703	Maintenance and Repair of Industrial Valves	0	1	1	0.020	0	1	0.000
3314704	Maintenance and Repair of Compressors	0	1	1	0.020	0	1	0.000
3314705	Maintenance and Repair of Indust. Transmission Equip.	0	1	1	0.020	0	1	0.000
3314706	Maintenance and Repair of Thermal Machines	0	1	1	0.020	1	1	0.000
3314707	Maintenance and Repair of HVAC Machines	0	0	1	0.020	0	0	0.000
3314708	Maintenance and Repair of Transport/Elevation Equip.	0	1	1	0.020	0	0	0.000
3314713	Maintenance and Repair of Machine Tools	0	1	1	0.020	0	0	0.000
3314714	Maintenance and Repair of Oil Prospecting Equip.	1	0	0	0.020	0	0	0.000
3314718	Maintenance and Repair of Metallurgical Machines	0	1	1	0.020	0	0	0.000
3317101	Maintenance and Repair of Ships/Floating Structures	0	1	1	0.020	0	0	0.000
3321000	Installation of Industrial Machines	0	1	1	0.020	0	1	0.000
2014200	Fabrication of Industrial Gases	0	1	1	0.018	1	1	0.053
2022300	Fabrication of Intermediate Plastics, Resins, Fibers	0	1	1	0.018	1	1	0.053
2021500	Fabrication of Basic Petrochemical Products	1	0	0	0.018	1	1	0.053
2031200	Fabrication of Thermoplastic Resins	0	1	1	0.018	1	1	0.053
2032100	Fabrication of Thermosetting Resins	0	1	1	0.018	1	1	0.053
2033900	Fabrication of Elastomeres	0	1	1	0.018	1	1	0.053
7111100	Architectural Services	0	0	1	0.018	0	0	0.000
7112000	Engineering Services	0	1	1	0.018	0	1	0.000
7119701	Cartography, Topography, and Geodesic Services	0	1	1	0.018	0	0	0.000
7119702	Geological Studies	0	1	1	0.018	0	0	0.000
7119703	Technical Design Services for Architecture/Engineering	0	1	1	0.018	0	0	0.000
7119704	Workplace Safety Services	0	1	1	0.018	0	0	0.000
7119799	Other Engineering and Architectural Services	0	1	1	0.018	0	0	0.000
7120100	Tests and Technical Analyses	0	1	1	0.018	0	0	0.000
7210000	Experimental R&D in Physical and Natural Sciences	0	1	1	0.018	0	0	0.000
5011401	Maritime Cargo Transport	0	1	1	0.017	0	0	0.027
5012201	Maritime Cargo Transport (Long-Distance)	0	1	1	0.017	0	0	0.027
5030101	Maritime Navigation Support	0	1	1	0.017	0	0	0.027
5030102	Port Navigation Support	0	1	1	0.017	0	0	0.027
2511000	Fabrication of Metallic Structures	0	1	1	0.014	0	1	0.000
2513600	Fabrication of Heavy Boilers	0	1	1	0.014	0	0	0.000
2522500	Fabricatoin of Vapor Boilers	0	1	1	0.014	0	0	0.000
2531401	Production of Forged Steel Products	0	1	1	0.014	0	0	0.000
2531402	Production of Forged Iron Alloys	0	1	1	0.014	0	0	0.000
2532201	Production of Stamped Metal Products	0	1	1	0.014	0	0	0.000
2532202	Powder Metallurgy	0	1	1	0.014	0	0	0.000
2539000	Machining and Welding Services	0	1	1	0.014	0	0	0.000
2539001	Machining and Turning	0	1	1	0.014	0	0	0.000
2539002	Treatment and Coating of Metals	0	1	1	0.014	0	0	0.000
2543800	Fabrication of Tools	0	1	1	0.014	0	0	0.000
2592601	Fabrication of Drawn Metal Products (Standardized)	0	1	1	0.014	0	0	0.000
2592602	Fabrication of Drawn Metal Products (Non-Standard)	0	1	1	0.014	0	0	0.000
2599302	Metal Cutting and Folding Services	0	1	1	0.014	0	0	0.000
7719501	Rental of Ships without Crew (except Recreation)	0	1	1	0.011	0	0	0.000
7732201	Rental of Machines and Equipment for Construction	0	1	1	0.011	0	0	0.000
7739001	Rental of Machines/Equip. for Petroleum Extraction	1	0	0	0.011	0	0	0.000
7739002	Rental of Scientific Equipment	0	0	1	0.011	0	0	0.000
7740300	Management of Intangible Non-Financial Assets	0	0	1	0.011	0	0	0.000
3011301	Construction of Large Ships	1	1	1	0.000	0	0	0.000
3511500	Electrical Energy Generation (Deactivated)	0	0	0	0.000	1	1	0.047
3511501	Electrical Energy Generation	0	0	0	0.000	1	1	0.047
3511502	Coordination and Control of Electricity Generation	0	0	0	0.000	1	1	0.047
3512300	Electrical Energy Transmission	0	0	0	0.000	0	0	0.047
3513100	Wholesale Electrical Energy Commerce	0	0	0	0.000	0	0	0.047
3514000	Electricity Distribution	0	0	0	0.000	0	0	0.047
3520401	Production of Gas	0	0	0	0.000	1	1	0.047
3520402	Distribution of Fuel Gas to Urban Utilities	0	0	0	0.000	0	0	0.047
2219600	Fabrication of Rubber Products	0	0	0	0.000	1	1	0.024
2221800	Fabrication of Plastic Tubes and Sheets	0	0	0	0.000	1	1	0.024
2222600	Fabrication of Plastic Packaging	0	0	0	0.000	1	1	0.024
2223400	Fabrication of Plastic Tubes for Construction	0	0	0	0.000	1	1	0.024
2229301	Fabricatoin of Plastic Artefacts for Domestic Use	0	0	0	0.000	1	1	0.024
2229302	Fabrication of Plastic Products for Industrial Use	0	0	0	0.000	1	1	0.024
2229303	Fabrication of Plastic Prod. for Construct. (ex. Tubes)	0	0	0	0.000	1	1	0.024
2229399	Fabrication of Plastic Products for Other Uses	0	0	0	0.000	1	1	0.024
1931400	Fabrication of Ethanol	0	0	0	0.000	1	1	0.027
1932200	Fabrication of Biofuels (except Ethanol)	0	0	0	0.000	1	1	0.027

Note: Subclass refers to CNAE 2.0 Subclass. Subclass descriptions are abbreviated. D = Direct Oil-Link; U(S) = Upstream (Strict Definition); U(L) = Upstream (Loose Definition); U Coef. = Upstream Leontief Coefficient; D(S) = Downstream (Strict Definition); D(L) = Downstream (Loose Definition); D 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). We apply "strict" and "loose" classifications for robustness. This process is illustrated in Table B2.

Table B5: Descriptive Statistics: Poached Workers

		Starting Wage	Education	Age	Female	Nonwhite	n
	Population (Treated)	4,312	6.90	32.51	0.13	0.28	15,347
		(4457.5)	(1.63)	(8.38)	(0.34)	(0.45)	
2006	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)	
	Population (Treated)	3,171	6.43	32.64	0.10	0.39	14,760
		(3453.9)	(1.56)	(8.57)	(0.30)	(0.49)	
2008	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)	
	Population (Treated)	4,181	6.87	32.56	0.13	0.40	41,437
		(5053.3)	(1.52)	(8.51)	(0.34)	(0.49)	
2010	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)	
	Population (Treated)	3,217	6.56	33.35	0.11	0.48	22,371
		(3414.1)	(1.51)	(8.52)	(0.32)	(0.50)	
2012	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)	
	Population (Treated)	3,932	6.94	32.24	0.15	0.48	43,659
		(4728.5)	(1.46)	(8.51)	(0.36)	(0.50)	
2014	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)	

Note: Table reports means and standard deviations (in parentheses) for the full population of formal workers who were poached in a given year, as well as for matched subsamples. "Treated" refers to workers who were poached into an oil-linked establishment; "control" refers to all other poached workers. Monetary values are deflated to constant 2018 $BRL. Poaching 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, previous wage bin during a two-year matching window prior to poach, and destination municipality.

Table B6: Descriptive Statistics: Newly Hired Workers

		Starting 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)	
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)	
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)	
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)	
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)	

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 newly hired into an oil-linked establishment; "control" refers to all other newly 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. Coarsened exact matching criteria are: education, sex, non-white race indicator, municipality, age bin, and wage and firm size bins in first job.

*Matching is performed on a random subsample of 20% of the full population of new 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.

Table B7: Poached Worker Outcomes: **Hourly Wages**

	Cohort				
Year	2006	2008	2010	2012	2014
2004	-0.013**				
	(0.006)				
2006	0.030***	-0.013			
	(0.009)	(0.008)			
2008	0.176***	0.055***	-0.001		
	(0.014)	(0.014)	(0.003)		
2010	0.194***	0.099***	0.071***	-0.011*	
	(0.017)	(0.017)	(0.005)	(0.006)	
2012	0.215***	0.107***	0.119***	0.034***	-0.002
	(0.018)	(0.021)	(0.007)	(0.010)	(0.003)
2014	0.227***	0.094***	0.111***	0.044***	0.040***
	(0.021)	(0.023)	(0.008)	(0.014)	(0.004)
2016	0.244***	0.089***	0.063***	0.014	0.016**
	(0.023)	(0.029)	(0.009)	(0.017)	(0.007)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	12,563	6,357	64,302	14,095	76,333
n×t (matched worker-years)	158,323	78,868	758,754	164,345	793,605
N (total poached workers)	309,689	258,091	704,292	392,084	913,060
Baseline DV Mean	36.82	14.29	22.87	13.42	25.14
Adj. R-Squared	0.8417	0.6775	0.8084	0.6808	0.7881

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 poach 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. For hourly wages, the sample is restricted to employed individuals. *** p<0.01, ** p<0.05, * p<0.1

Table B8: Poached Worker Outcomes: **Months Employed per Year**

			Cohort		
Year	2006	2008	2010	2012	2014
2004	0.026				
	(0.067)				
2006	0.005	-0.175			
	(0.072)	(0.116)			
2008	0.159	-0.240*	0.052		
	(0.108)	(0.126)	(0.041)		
2010	0.347***	-0.391	0.088**	-0.039	
	(0.120)	(0.159)	(0.039)	(0.075)	
2012	0.388***	-0.476***	0.025	-0.278***	0.133***
	(0.133)	(0.175)	(0.057)	(0.091)	(0.041)
2014	0.435***	-0.592***	-0.019	-0.227*	0.192***
	(0.141)	(0.189)	(0.064)	(0.117)	(0.041)
2016	0.394**	-0.663***	-0.280***	-0.671***	-0.326***
	(0.154)	(0.208)	(0.074)	(0.135)	(0.066)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	12,158	6,095	61,763	14,095	65,709
n×t (matched worker-years)	169,779	85,330	864,682	197,330	919,926
N (total poached workers)	309,689	258,091	704,292	392,084	913,060
Baseline DV Mean	11.57	10.13	10.84	11.00	11.02
Adj. R-Squared	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 poach 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 poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B9: Poached Worker Outcomes: **Annual Formal Earnings**

Year	2006	2008	Cohort 2010	2012	2014
2004	-0.009				
	(0.009)				
2006	0.026*	-0.028			
	(0.013)	(0.018)			
2008	0.237***	0.053*	0.006		
	(0.068)	(0.027)	(0.006)		
2010	0.451***	0.030	0.085***	-0.015	
	(0.092)	(0.089)	(0.008)	(0.012)	
2012	0.607***	-0.067	0.146***	0.004	0.009
	(0.101)	(0.118)	(0.034)	(0.020)	(0.006)
2014	0.563***	0.006	0.131***	-0.015	0.065***
	(0.114)	(0.143)	(0.046)	(0.077)	(0.009)
2016	0.627***	-0.111	-0.095	-0.341***	-0.231***
	(0.137)	(0.172)	(0.061)	(0.108)	(0.049)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	12,158	6,095	61,763	14,095	65,709
n×t (matched worker-years)	169,779	85,330	864,682	197,330	919,926
N (total poached workers)	309,689	258,091	704,292	392,084	913,060
Baseline DV Mean	76064.82	27280.15	46162.49	27112.43	50684.74
Adj. R-Squared	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 poach 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 every other coefficient estimate for a specific cohort. One pre-period is reported to evaluate pre-trends. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B10: Poached Workers' **Annual Formal Earnings (Less Than High School)**

Year	2006	2008	Cohort 2010	2012	2014
2004	-0.020				
	(0.043)				
2006	0.031	-0.025			
	(0.058)	(0.057)			
2008	-0.214	-0.022	0.035		
	(0.329)	(0.096)	(0.025)		
2010	0.138	-0.156	0.102***	-0.033	
	(0.399)	(0.255)	(0.034)	(0.060)	
2012	-0.103	-0.232	0.060	0.005	0.013
	(0.442)	(0.312)	(0.132)	(0.086)	(0.030)
2014	-0.215	-0.077	-0.174	-0.328	0.064
	(0.501)	(0.365)	(0.181)	(0.284)	(0.041)
2016	-1.037*	-0.783	-0.434*	-0.898**	-0.828***
	(0.555)	(0.511)	(0.222)	(0.388)	(0.239)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	595	485	2,986	765	1,878
n×t (matched worker-years)	8,297	6,790	41,804	10,710	26,292
N (total poached workers)	309,689	258,091	704,292	392,084	913,060
Baseline DV Mean	16782.48	17846.69	15777.24	18337.59	19138.20
Adj. R-Squared	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 poach 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 every other coefficient estimate for a specific cohort. One pre-period is reported to evaluate pre-trends. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B11: Poached Workers' **Annual Formal Earnings (High School Complete)**

Year	2006	2008	Cohort 2010	2012	2014
2004	-0.018				
	(0.016)				
2006	0.052**	-0.025			
	(0.025)	(0.021)			
2008	0.080	0.069**	0.011		
	(0.095)	(0.032)	(0.009)		
2010	0.069	0.059	0.100***	-0.013	
	(0.126)	(0.106)	(0.012)	(0.014)	
2012	0.200	-0.028	0.076	0.009	0.004
	(0.154)	(0.138)	(0.045)	(0.023)	(0.009)
2014	-0.008	-0.001	0.033	-0.064	0.078***
	(0.165)	(0.168)	(0.060)	(0.084)	(0.012)
2016	-0.180	-0.060	-0.094	-0.319***	-0.107*
	(0.199)	(0.196)	(0.079)	(0.121)	(0.063)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	4,641	4,670	35,366	11,184	36,700
n×t (matched worker-years)	64,830	65,380	495,124	156,576	513,800
N (total poached workers)	309,689	258,091	704,292	392,084	913,060
Baseline DV Mean	22895.63	19597.42	19943.68	21503.00	22620.51
Adj. R-Squared	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 poach 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 poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B12: Poached Workers' **Annual Formal Earnings (More than High School)**

Year	2006	2008	Cohort 2010	2012	2014
2004	0.000				
	(0.010)				
2006	0.004	-0.062			
	(0.014)	(0.041)			
2008	0.404***	0.056	-0.012		
	(0.096)	(0.053)	(0.008)		
2010	0.774***	0.045	0.057***	-0.023	
	(0.134)	(0.224)	(0.010)	(0.023)	
2012	0.996***	-0.143	0.282***	0.019	0.013*
	(0.138)	(0.331)	(0.055)	(0.049)	(0.008)
2014	1.080***	0.091	0.358***	0.418*	0.049***
	(0.162)	(0.402)	(0.077)	(0.227)	(0.010)
2016	1.432***	0.113	-0.039	-0.112	-0.347***
	(0.195)	(0.490)	(0.109)	(0.301)	(0.082)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	6,922	940	23,411	2,146	27,131
n×t (matched worker-years)	96,652	13,160	327,754	30,044	379,834
N (total poached workers)	309,689	258,091	704,292	392,084	913,060
Baseline DV Mean	116809.00	70315.85	89645.67	59474.32	90830.77
Adj. R-Squared	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 poach 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. For brevity, each column reports every other coefficient estimate for a specific cohort. One pre-period is reported to evaluate pre-trends. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B13: New Hire Outcomes: **Hourly Wages**

Year	2006	2008	Cohort 2010	2012	2014
2008	0.065***				
	(0.011)				
2010	0.072***	0.026***			
	(0.013)	(0.007)			
2012	0.066***	0.043***	0.039***		
	(0.014)	(0.008)	(0.009)		
2014	0.075***	0.046***	0.042***	0.030***	
	(0.016)	(0.010)	(0.010)	(0.007)	
2016	0.040**	0.020*	0.017**	0.018	-0.008
	(0.019)	(0.011)	(0.013)	(0.009)	(0.011)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	18,888	84,426	26,890	90,685	27,015
n×t (matched worker-years)	156,658	549,401	144,084	415,231	86,332
N (total new hires)	3,241,795	3,856,910	4,113,730	4,015,319	3,507,150
Baseline DV Mean	7.17	7.12	8.13	8.79	9.53
Adj. R-Squared	0.717	0.696	0.706	0.775	0.735

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 brevity, each column reports coefficient estimates from every other year for a specific cohort. For hourly wages, the sample is restricted to employed individuals. *** p<0.01, ** p<0.05, * p<0.1

Table B14: New Hire Outcomes: **Months Employed per Year**

Year	2006	2008	Cohort 2010	2012	2014
2008	0.171				
	(0.130)				
2010	-0.027	-0.201**			
	(0.134)	(0.073)			
2012	0.131	-0.100*	-0.090*		
	(0.141)	(0.076)	(0.095)		
2014	0.201	-0.230***	-0.141***	-0.267***	
	(0.151)	(0.079)	(0.099)	(0.074)	
2016	-0.056	-0.414***	-0.320***	-0.303***	-0.305**
	(0.147)	(0.081)	(0.102)	(0.074)	(0.122)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	19,533	90,051	32,768	102,798	31,474
n×t (matched worker-years)	214,863	810,459	229,376	616,788	125,896
N (total new hires)	3,241,795	3,856,910	4,113,730	4,015,319	3,507,150
Baseline DV Mean	5.21	5.30	5.21	5.33	5.48
Adj. R-Squared	0.424	0.451	0.472	0.521	0.507

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. For brevity, each column reports coefficient estimates from every other year for a specific cohort. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B15: New Hire Outcomes: **Annual Formal Earnings**

Year	2006	2008	Cohort 2010	2012	2014
2008	0.072				
	(0.105)				
2010	0.118	-0.123**			
	(0.113)	(0.061)			
2012	0.136	-0.070**	-0.039		
	(0.119)	(0.067)	(0.082)		
2014	0.187	-0.218***	-0.146*	-0.183***	
	(0.123)	(0.070)	(0.087)	(0.063)	
2016	0.029	-0.398***	-0.315***	-0.227***	-0.232**
	(0.130)	(0.072)	(0.091)	(0.070)	(0.098)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	19,533	90,051	32,768	102,798	31,474
n×t (matched worker-years)	214,863	810,459	229,376	616,788	125,896
N (total new hires)	3,241,795	3,856,910	4,113,730	4,015,319	3,507,150
Baseline DV Mean	6794.12	7018.53	7714.90	8809.95	8979.83
Adj. R-Squared	0.424	0.451	0.497	0.451	0.404

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. For brevity, each column reports coefficient estimates from every other year for a specific cohort. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B16: New Hires' **Annual Formal Earnings (Less Than High School)**

Year	2006	2008	Cohort 2010	2012	2014
2008	0.048				
	(0.161)				
2010	0.216	-0.206**			
	(0.171)	(0.097)			
2012	0.271	-0.105	-0.030		
	(0.178)	(0.104)	(0.143)		
2014	0.281	-0.332***	-0.095**	-0.218**	
	(0.182)	(0.108)	(0.147)	(0.106)	
2016	0.200	-0.339***	-0.248***	-0.202*	-0.284*
	(0.190)	(0.110)	(0.151)	(0.114)	(0.169)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	7,368	30,254	10,077	30,204	10,261
n×t (matched worker-years)	81,048	272,286	70,539	181,224	41,044
N (total new hires)	3,241,795	3,856,910	4,113,730	4,015,319	3,507,150
Baseline DV Mean	4587.46	4749.77	5046.52	4784.52	4470.57
Adj. R-Squared	0.420	0.435	0.482	0.414	0.355

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. For brevity, each column reports coefficient estimates from every other year for a specific cohort. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B17: New Hires **Annual Formal Earnings (High School Complete)**

Year	2006	2008	Cohort 2010	2012	2014
2008	-0.044				
	(0.146)				
2010	-0.148	-0.155*			
	(0.160)	(0.083)			
2012	-0.132	-0.097	-0.091		
	(0.169)	(0.092)	(0.109)		
2014	-0.021	-0.180*	-0.234	-0.180**	
	(0.179)	(0.097)	(0.117)	(0.079)	
2016	-0.269	-0.475***	-0.351	-0.293***	-0.141
	(0.190)	(0.101)	(0.123)	(0.086)	(0.131)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	10,760	54,632	19,813	65,518	18,393
n×t (matched worker-years)	118,360	491,688	138,691	393,108	73,572
N (total new hires)	3,241,795	3,856,910	4,113,730	4,015,319	3,507,150
Baseline DV Mean	5796.43	6339.85	6356.48	6554.59	6918.31
Adj. R-Squared	0.405	0.441	0.490	0.436	0.408

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. For brevity, each column reports coefficient estimates from every other year for a specific cohort. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B18: New Hires **Annual Formal Earnings (More Than High School)**

Year	2006	2008	Cohort 2010	2012	2014
2008	0.830**				
	(0.350)				
2010	0.968**	0.519**			
	(0.406)	(0.216)			
2012	0.782**	0.285	0.286		
	(0.426)	(0.246)	(0.256)		
2014	0.747**	0.177	0.207	-0.070	
	(0.437)	(0.258)	(0.295)	(0.260)	
2016	0.625	-0.258	-0.350	0.090	-0.565*
	(0.467)	(0.278)	(0.319)	(0.301)	(0.297)
Individual FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
n (matched workers)	1,405	5,165	2,878	7,076	2,820
n×t (matched worker-years)	15,455	46,485	20,146	42,456	11,280
N (total new hires)	3,241,795	3,856,910	4,113,730	4,015,319	3,507,150
Baseline DV Mean	25993.18	27482.51	26403.25	46869.96	38822.81
Adj. R-Squared	0.428	0.465	0.501	0.486	0.389

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. For brevity, each column reports coefficient estimates from every other year for a specific cohort. All matched poached workers (employed & unemployed) are retained in sample. *** p<0.01, ** p<0.05, * p<0.1

Table B19: **Poached Workers**: Net Lifetime and Aggregate Earnings Effects of Exposure to Oil-Linked Sectors

All Matched Poaches	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	36,316	24,057	31,530	25,128	31,563	29,719	28,070
N (oil-linked)	15,347	14,760	41,437	22,371	43,659	137,574	122,227
Lifetime Net Oil Earnings	277,116	-9,990	18,386	-16,513	-12,228	28,814	-2,363
Lifetime Net Oil Earnings (% of Baseline)	7.6	-0.4	0.6	-0.7	-0.4	97.0	-8.4
Lifetime Net Oil Earnings / Post-Treat Yrs	23,093	-999	2,298	-2,752	-3,057	3602	-338
Aggregate Lifetime Net Oil Earnings	4,252,901,630	-147,450,344	761,852,628	-369,406,219	-533,861,909	3,964,035,787	-288,865,844

Less Than Secondary Education	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	18862	17829	18335	19542	19731	18,860	18,859
N (oil-linked)	4043	5,070	9,714	6,610	9,296	34,733	30,690
Lifetime Net Oil Earnings	-40,511	-39,518	-15,973	-37,171	-26,595	-29,143	-27,646
Lifetime Net Oil Earnings (% of Baseline)	-2.1	-2.2	-0.9	-1.9	-1.3	-154.5	-146.6
Lifetime Net Oil Earnings / Post-Treat Yrs	-3,376	-3,952	-1,997	-6,195	-6,649	-3643	-3949
Aggregate Lifetime Net Oil Earnings	-163,787,238	-200,356,274	-155,164,573	-245,702,182	-247,223,522	-1,012,233,789	-848,446,551

Secondary Education Complete	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	25231	20600	22581	21495	23074	22,596	21,938
N (oil-linked)	6917	7,607	22,192	12,505	24,353	73,574	66,657
Lifetime Net Oil Earnings	15,420	-5,065	6,923	-14,474	-1,844	-56	-1,662
Lifetime Net Oil Earnings (% of Baseline)	0.6	-0.2	0.3	-0.7	-0.1	-0.2	-7.6
Lifetime Net Oil Earnings / Post-Treat Yrs	1,285	-507	865	-2,412	-461	-7	-237
Aggregate Lifetime Net Oil Earnings	106,659,194	-38,529,519	153,634,418	-180,997,431	-44,906,807	-4,140,144	-110,799,338

More Than Secondary Education	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	80545	51302	67037	49764	62669	62,263	57,693
N (oil-linked)	4387	2,083	9,531	3,256	10,010	29,267	24,880
Lifetime Net Oil Earnings	1,617,690	31,168	99,032	41,282	-41,568	267,329	29,225
Lifetime Net Oil Earnings (% of Baseline)	20.1	0.6	1.5	0.8	-0.7	429.4	50.7
Lifetime Net Oil Earnings / Post-Treat Yrs	134,808	3,117	12,379	6,880	-10,392	33416	4175
Aggregate Lifetime Net Oil Earnings	7,096,806,679	64,923,481	943,873,152	134,414,504	-416,096,919	7,823,920,897	727,114,218

Note: Monetary values are deflated to constant 2018 $BRL. Each of the first five columns refers to the cohort of workers poached in that year. Column 6 refers to the sum or average (where applicable) across the 2006, 2008, 2010, 2012, and 2014 cohorts. Column 7 is the same as 6, but excluding the 2006 cohort. Baseline annual income refers to total formal annual earnings in the year prior to workers' poach, averaged across the population of treated and control workers (not just matched). N (oil-linked) refers to the total number of workers poached into oil-linked establishments in that cohort (not just matched). Lifetime net oil earnings are calculated by (i) converting each relative year indicator's post-poach coefficient estimate into a semi-elasticity: $(100 * (e^{\hat{\beta}} - 1))$; (ii) multiplying these semi-elasticities by baseline average income; (iii) summing these "treated" incomes across all post-poach years; (iv) computing the difference between the sum of treated incomes after the poach and an extrapolation of baseline average incomes across all years after the poach. Net lifetime earnings for oil-treated poaches are then divided by baseline average income and multiplied by 100 to show the net treatment effect as a percentage of baseline income. Next, net lifetime earnings for oil treated workers are divided by the number of post-treatment years in sample to account for the fact that later cohorts have fewer post-treatment years. Finally, net lifetime oil-treated earnings are multiplied by the number of workers poached into oil in each cohort to arrive at an aggregate treatment effect for the cohort, or across all cohorts. Results are reported for all poached workers, and then separately for low, medium, and high education workers. Coefficient estimates are drawn from event study specifications that regress annual formal earnings (summed across all formal jobs) on relative time indicators around the year of poach, with individual and year fixed effects and standard errors clustered at the individual level. We calculate semi-elasticities based on coefficient point estimates, whether or not these are statistically significant at the 5% level. Furthermore, coefficients are estimated on the subsample of matched workers, but we extrapolate estimated treatment effects to all treated workers. Reported values should therefore be treated as approximate, back-of-the-envelope calculations.

Table B20: **Newly Hired Workers**: Net Lifetime and Aggregate Earnings Effects of Exposure to Oil-Linked Sectors

All Matched New Hires	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	16,166	16,724	17,836	18,519	19,514	17,752	18,148
N (oil-linked)	72,582	99,771	106,114	108,924	84,554	471,945	399,363
Lifetime Net Oil Earnings	14,972	-28,343	-19,114	-19,850	-6,245	-13,687	-18,896
Lifetime Net Oil Earnings (Share of Baseline)	0.9	-1.7	-1.1	-1.1	-0.3	-0.8	-1.0
Lifetime Net Oil Earnings / Post-Treat Yrs	1,248	-2,834	-2,389	-3,308	-1,561	-1,711	-2,699
Aggregate Lifetime Net Oil Earnings	1,086,726,925	-2,827,801,503	-2,028,255,078	-2,162,175,754	-528,070,760	-6,459,576,170	-7,546,303,096

Less Than Secondary Education	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	12,297	13,049	13,752	14,352	14,230	13,536	13,846
N (oil-linked)	44,406	54,476	52,799	48,645	36,429	236,755	192,349
Lifetime Net Oil Earnings	24,271	-24,965	-12,252	-15,534	-2,777	-7,543	-14,888
Lifetime Net Oil Earnings (Share of Baseline)	2.0	-1.9	-0.9	-1.1	-0.2	-0.6	-1.1
Lifetime Net Oil Earnings / Post-Treat Yrs	2,023	-2,496	-1,532	-2,589	-694	-943	-2127
Aggregate Lifetime Net Oil Earnings	1,077,783,168	-1,359,974,187	-646,912,395	-755,673,064	-101,146,012	-1,785,922,491	-2,863,705,658

Secondary Education Complete	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	14,340	14,733	15,474	16,241	16,895	15,537	15,836
N (oil-linked)	22672	36,663	44,209	49,148	39,511	192,203	169,531
Lifetime Net Oil Earnings	-21,172	-27,775	-20,709	-18,896	-4,059	-18,225	-17,831
Lifetime Net Oil Earnings (Share of Baseline)	-1.5	-1.9	-1.3	-1.2	-0.2	-1.2	-1.1
Lifetime Net Oil Earnings / Post-Treat Yrs	-1,764	-2,777	-2,589	-3,149	-1,015	-2278	-2547
Aggregate Lifetime Net Oil Earnings	-480,007,722	-1,018,306,038	-915,524,548	-928,682,105	-160,355,648	-3,502,876,062	-3,022,868,340

More Than Secondary Education	2006	2008	2010	2012	2014	Total (all cohorts)	Total (excluding 2006)
Baseline (t-1) Annual Income	38,146	37,161	39,189	39,875	40,702	39,015	39,232
N (oil-linked)	5,504	8,632	9,106	11,131	8,614	42,987	37,483
Lifetime Net Oil Earnings	536,050	58,261	10,434	-12,969	-39,500	71,271	3,023
Lifetime Net Oil Earnings (Share of Baseline)	14.1	1.6	0.3	-0.3	-1.0	1.8	0.1
Lifetime Net Oil Earnings / Post-Treat Yrs	44,671	5,826	1,304	-2,161	-9,875	8,909	432
Aggregate Lifetime Net Oil Earnings	2,950,418,819	502,906,385	95,014,602	-144,357,628	-340,252,542	3,063,729,636	113,310,818

Note: Monetary values are deflated to constant 2018 $BRL. Each of the first five columns refers to the cohort of workers newly hired in that year. Column 6 refers to the sum or average (where applicable) across the 2006, 2008, 2010, 2012, and 2014 cohorts. Column 7 is the same as 6, but excluding the 2006 cohort. Baseline annual income refers to total formal annual earnings in the first year of the new hire, averaged across the population of treated and control workers (not just matched). N (oil-linked) refers to the total number of workers newly hired into oil-linked establishments in that cohort (not just matched). Lifetime net oil earnings are calculated by (i) converting each relative year indicator's post-hire coefficient estimate into a semi-elasticity: $(100 * (e^{\hat{\beta}} - 1))$; (ii) multiplying these semi-elasticities by baseline average income; (iii) summing these "treated" incomes across all post-hire years; (iv) computing the difference between the sum of treated incomes after the hire and an extrapolation of baseline average incomes across all years after the hire. Net lifetime earnings for oil-treated new hires are then divided by baseline average income and multiplied by 100 to show the net treatment effect as a percentage of baseline income. Next, net lifetime earnings for oil-treated workers are divided by the number of post-treatment years in sample to account for the fact that later cohorts have fewer post-treatment years. Finally, net lifetime oil-treated earnings are multiplied by the number of workers newly hired into oil in each cohort to arrive at an aggregate treatment effect for the cohort, or across all cohorts. Results are reported for all newly hired workers, and then separately for low, medium, and high education workers. Coefficient estimates are drawn from event study specifications that regress annual formal earnings (summed across all formal jobs) on relative time indicators around the year of poach, with individual and year fixed effects and standard errors clustered at the individual level. We calculate semi-elasticities based on coefficient point estimates, whether or not these are statistically significant at the 5% level. Furthermore, coefficients are estimated on the subsample of matched workers, but we extrapolate estimated treatment effects to all treated workers. Reported values should therefore be treated as approximate, back-of-the-envelope calculations.

Table B21: Sample Sizes for **Main Specification** (Direct, Upstream, & Downstream Oil Link; Strict Match)

Poached Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	15,347	294,342	2,461	10,201	16.0
2008	14,760	243,331	1,437	4,961	9.7
2010	41,437	662,855	10,767	54,024	26.0
2012	22,371	369,713	2,899	11,327	13.0
2014	43,659	869,401	10,805	66,213	24.7

Newly Hired Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	72,582	3,169,213	3,592	15,953	24.7
2008	99,771	3,757,139	9,184	80,985	46.0
2010	106,114	4,007,616	6,228	26,556	29.3
2012	108,924	3,906,395	11,143	91,778	51.2
2014	84,554	3,422,596	4,745	26,758	28.1

Table B22: Sample Sizes for **Robustness II** (Direct Oil Link, Loose Match)

Poached Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	3,463	306,226	2,074	12,040	59.9
2008	1,429	256,662	683	5,673	47.8
2010	4,914	699,378	3,515	43,621	71.5
2012	2,178	389,906	1,180	14,986	54.2
2014	4,868	908,192	3,388	67,831	69.6

Newly Hired Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	4,851	3,236,944	638	9,441	65.8
2008	5,903	3,851,007	741	19,623	62.8
2010	5,333	4,108,397	731	15,558	68.5
2012	8,183	4,007,136	1,262	26,892	77.1
2014	6,256	3,500,894	806	22,252	64.4

Table B23: Sample Sizes for **Robustness III** (Direct, Upstream, & Downstream Oil Link; Strict Match; Near Oil Industry Hubs)

Poached Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	4,317	51,734	1,073	2,251	24.9
2008	3,376	39,443	333	742	9.9
2010	11,021	116,239	3,255	10,115	29.5
2012	5,765	65,909	804	2,338	13.9
2014	12,279	158,965	3,609	14,807	29.4

Newly Hired Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	9,958	475,655	546	2,171	27.4
2008	15,915	551,406	1,674	12,410	52.6
2010	19,425	634,144	1,245	4,667	32.0
2012	23,017	619,495	2,752	17,780	59.8
2014	17,538	547,854	1,116	5,666	31.8

Table B24: Sample Sizes for **Robustness IV** (Direct, Upstream, & Downstream Oil Link; Strict Match; Common Support with 2006 Cohort)

Poached Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	15,347	294,342	1,924	8,375	12.5
2008	14,760	243,331	1,254	4,596	8.5
2010	41,437	662,855	8,282	43,879	20.0
2012	22,371	369,713	2,355	9,902	10.5
2014	43,659	869,401	7,766	50,085	17.8

Newly Hired Workers					
	Before Matching		After Matching		
Cohort	Treated	Control	Treated	Control	% of Treated Matched
2006	72,582	3,169,213	2,413	12,887	16.6
2008	99,771	3,757,139	7,125	68,121	35.7
2010	106,114	4,007,616	4,728	21,409	22.3
2012	108,924	3,906,395	6,873	62,398	31.5
2014	84,554	3,422,596	2,944	16,624	17.4

Table B25: Oil-Linked Majors

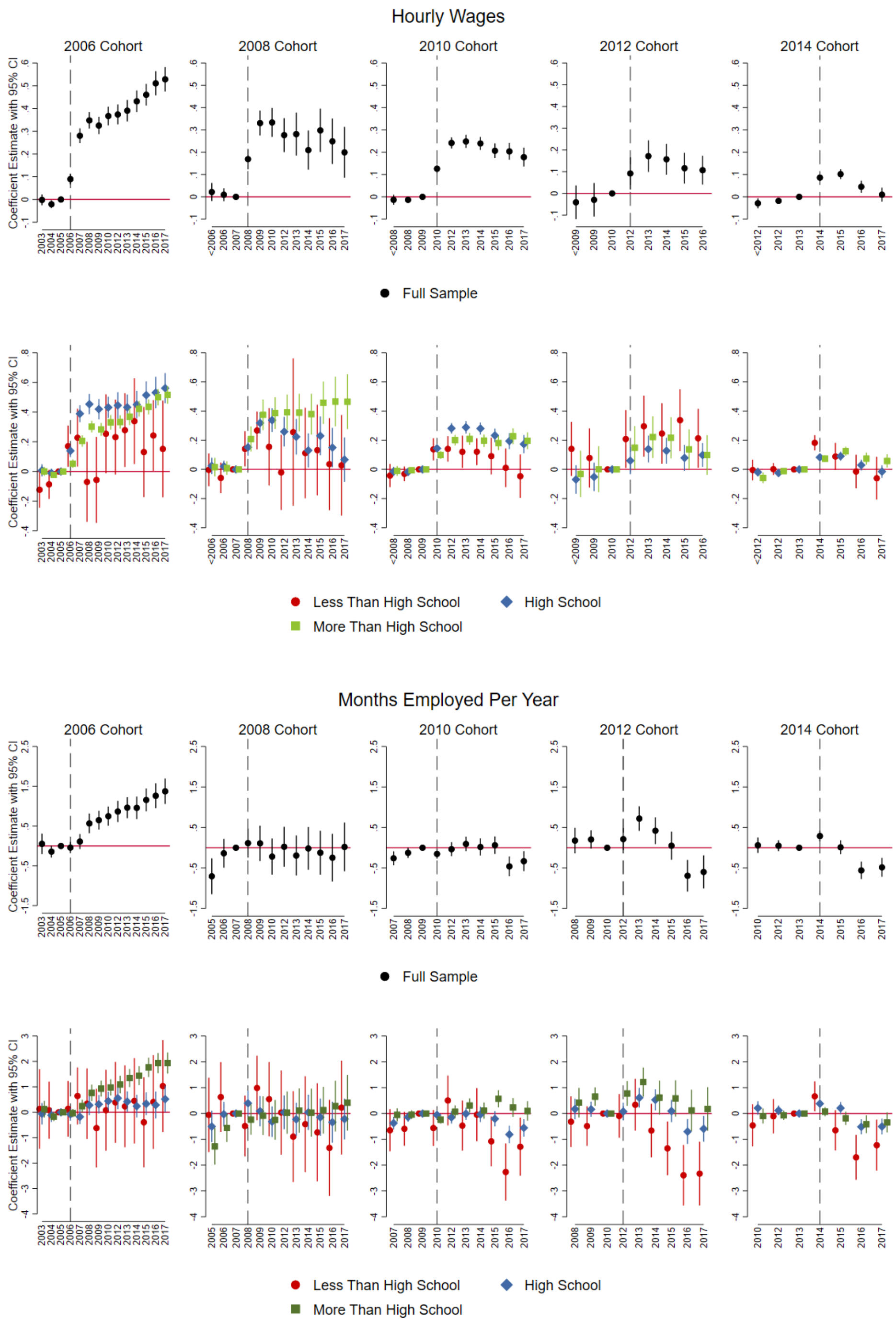
Oil-Linked Majors (Narrow Definition)	
Petroleum Engineering	Environmental Management
Geological Engineering	Naval maintenance
Naval Engineering	Petrochemical Maintenance
Shipbuilding	Mining & Extraction
Shipbuilding (non-motorized)	Marine Navigation
Naval Construction	Operation of Ships
Environmental Control	Paleontology
Water Pollution Control	Petrology
Extraction of Petroleum & Gas	Processing of Petroleum & Petrochemicals
Geoscience	Petroleum Refining
Geophysics	Environmental Cleanup
Geology	Environmental Protection Technology
Additional Oil-Linked Majors (Broad Definition)	
Civil Engineering	Conservation/Environmental Protection
Energy Studies	Engineering of Docks and Port Installations
Environmental Management	Environmental Preservation
Helicopter Construction	Industrial Electronics
Industrial Engineering	Marine Science
Mechanical Engineering	Mechanical Industrial Engineering
Mechanical Production Engineering	Mechatronical Engineering
Metallurgical Engineering	Metallurgical Industrial Engineering
Metallurgical Production Engineering	Metallurgical Technologies
Mining Technologies	Natural Resources
Natural Resources Conservation	Nautical Science
Steel Production	Teacher Training in Geology

Table B26: CBO Occupation Categories and Common Occupation Descriptors

Manager	**Professional**	**Technician**	**Worker**
Leader	Researcher	Technician	Support Staff
Director	Scientist	Designer	Receptionist
Manager	Engineer	Crafstman	Operator (Unskilled)
	Pilot	Supervisor	Food Service
	Doctor	Agent	Cleaning
	Nurse	Operator (Skilled)	Security
	Professor		Retail
	Lawyer		Agriculture
	Analyst		Manufacturing
			Extractive Industries
			Construction
			Mechanics/Maintenance

Appendix C Robustness Checks

Figure C1: Robustness: Poaches into Directly Oil-Linked Firms (Loose Match)



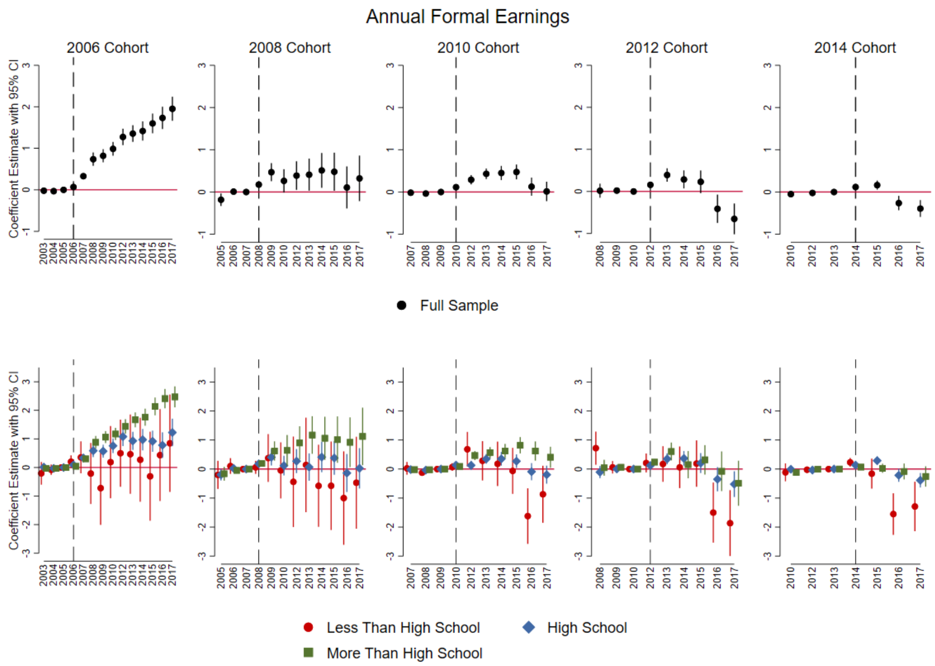
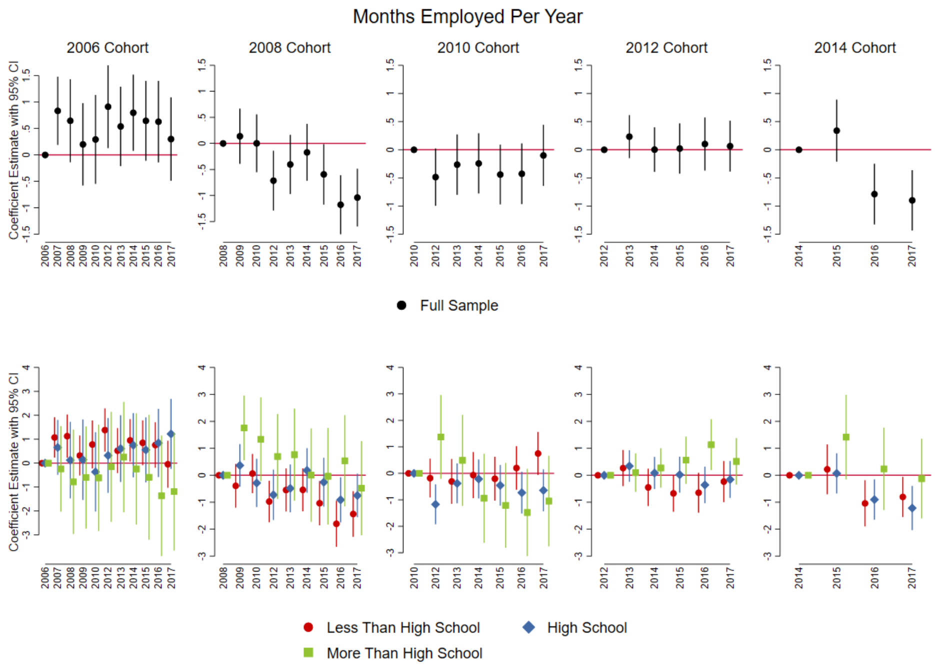


Figure C2: Robustness: New Hires into Directly Oil-Linked Firms (Loose Match)


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

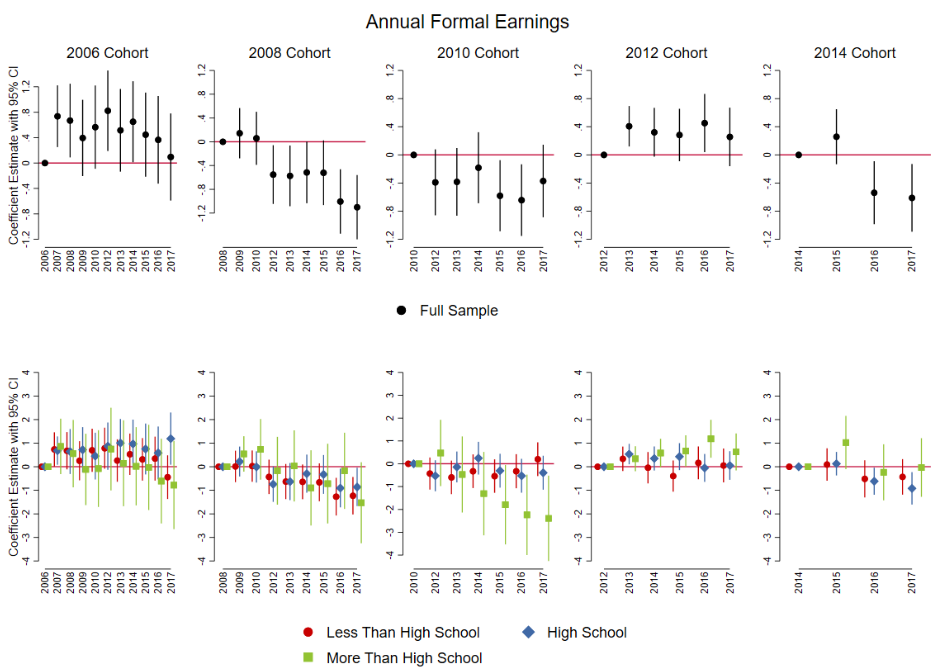
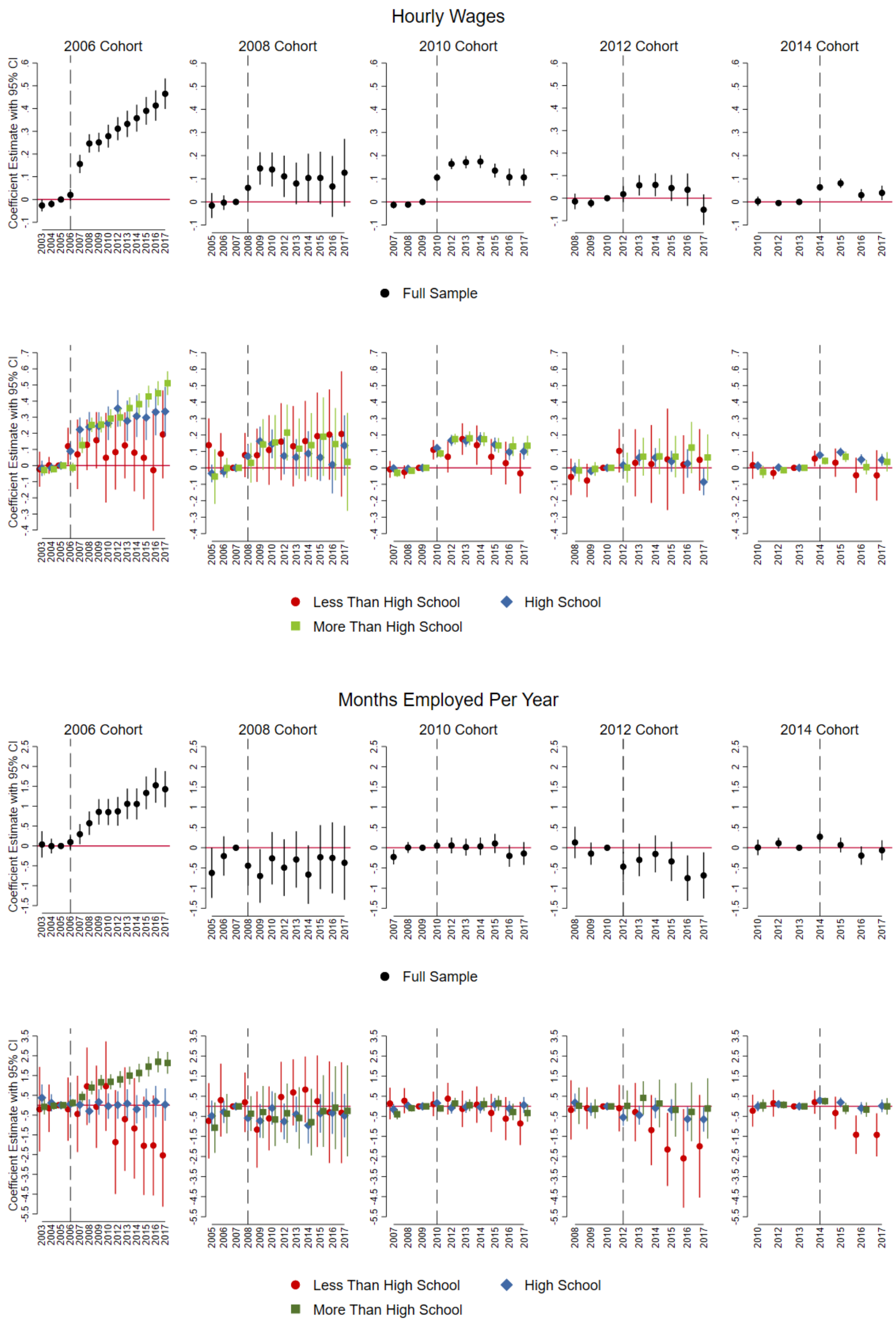

Annual Formal Earnings
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

Figure C3: Robustness: Poaches into Oil-Linked Firms (<100km. from Shipyard)



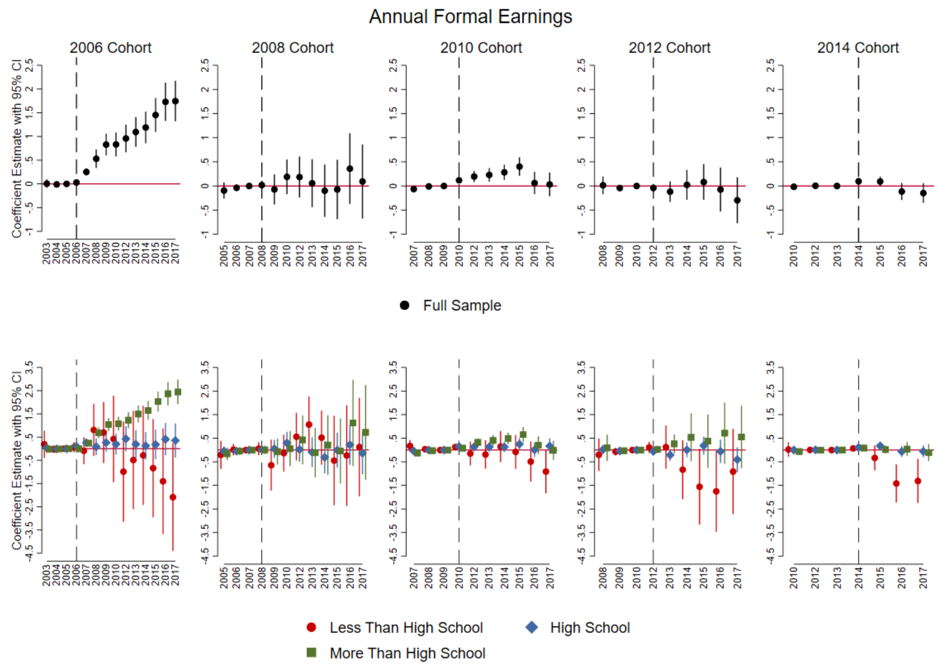
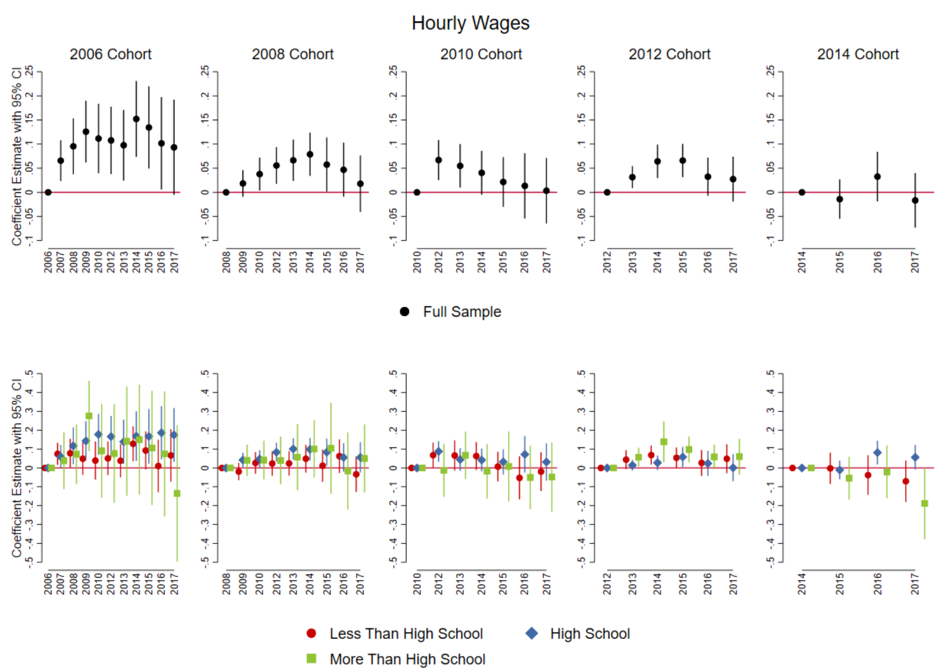
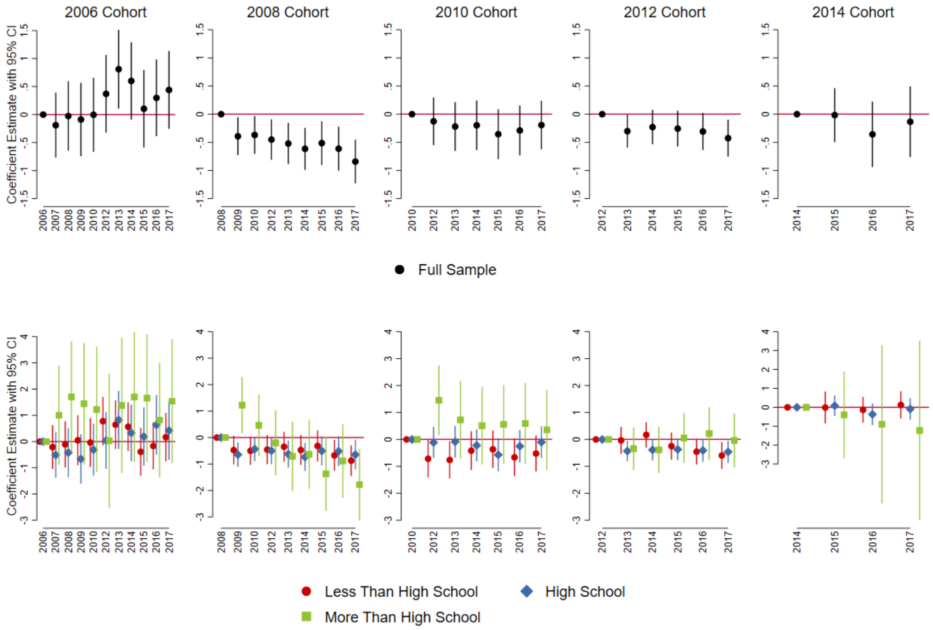


Figure C4: Robustness: New Hires into Oil-Linked Firms (<100km. from Shipyard)



Months Employed Per Year



Annual Income

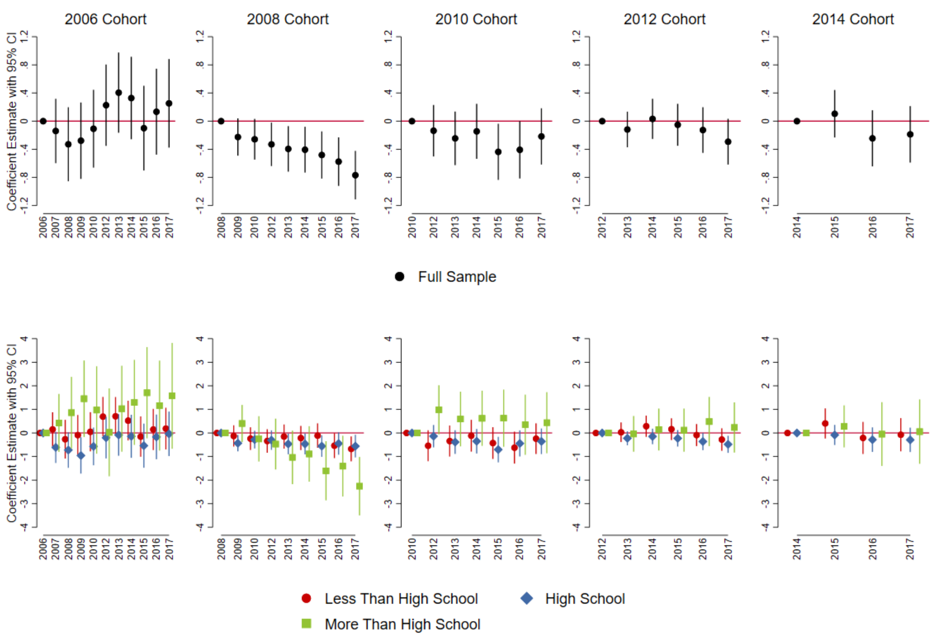
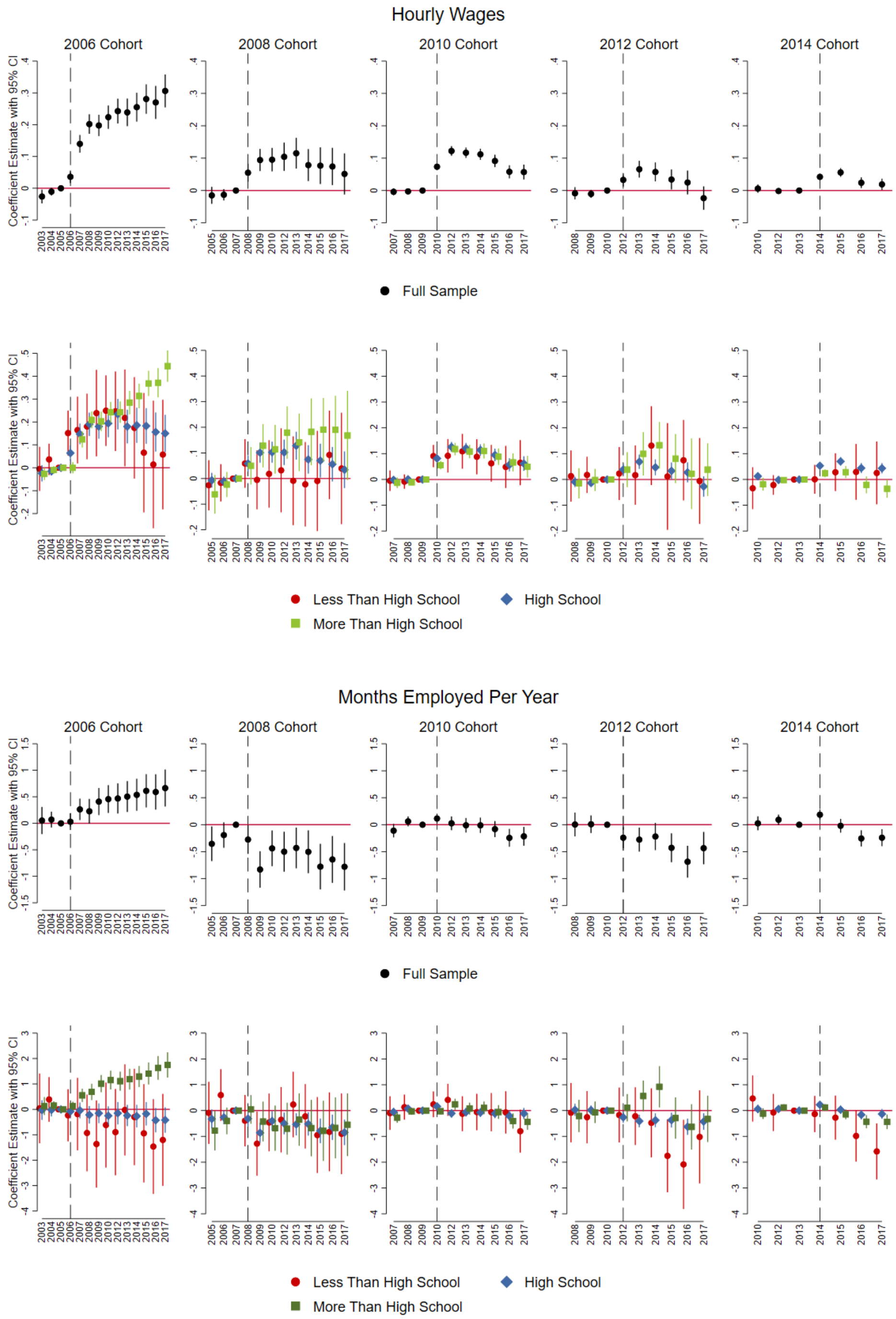


Figure C5: Robustness: Poaches into Oil-Linked Firms (Common Support Across Cohorts (Baseline = 2006))



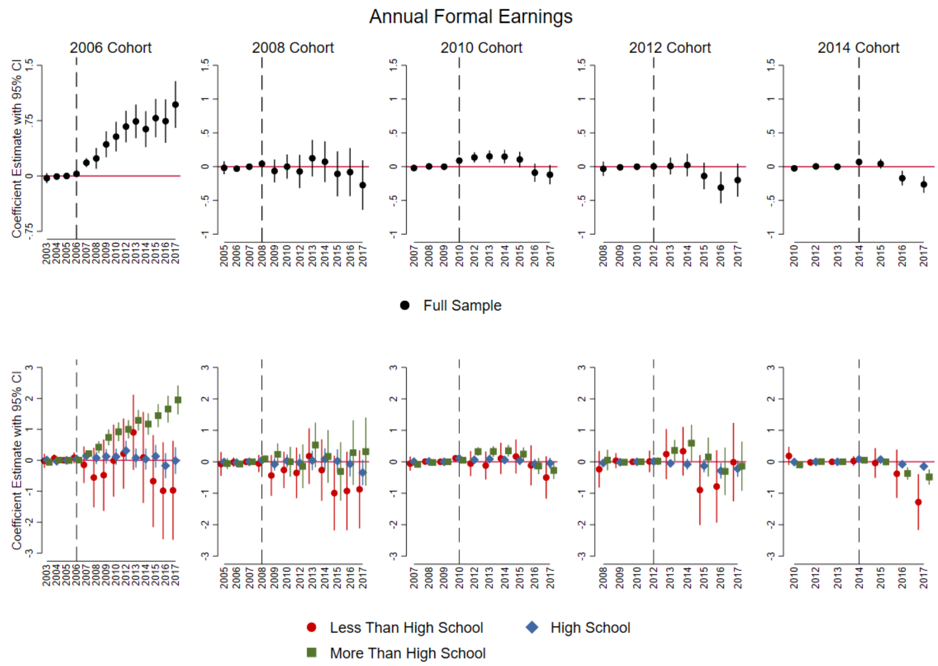
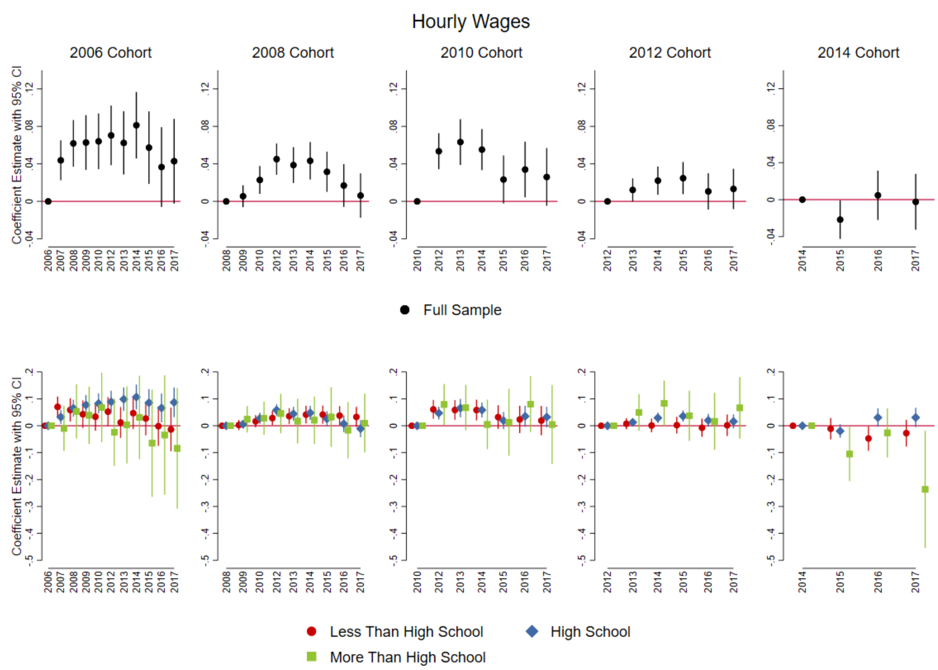
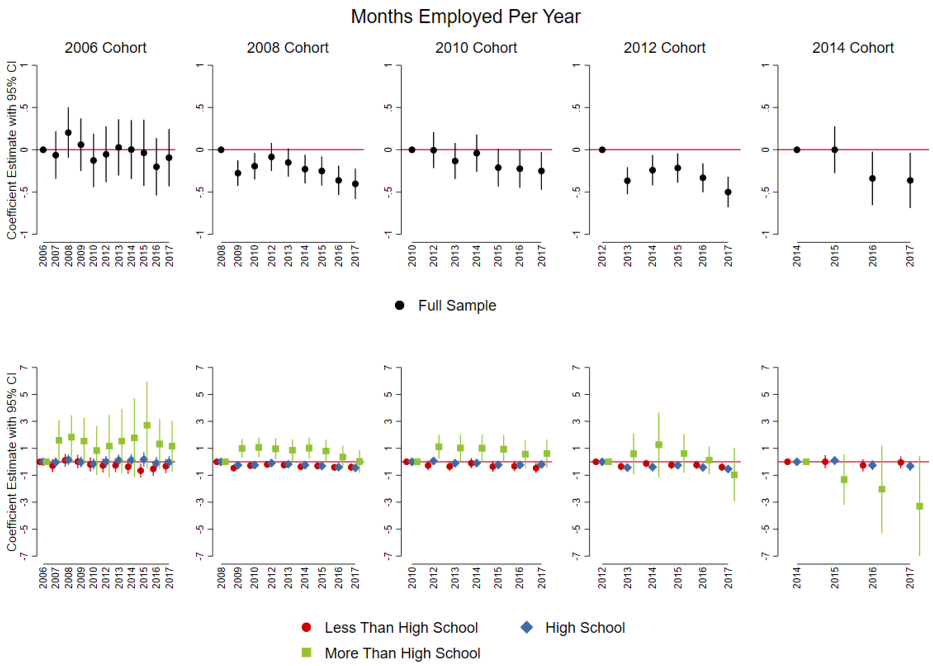


Figure C6: Robustness: New Hires into Oil-Linked Firms (Common Support Across Cohorts (Baseline = 2006))




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

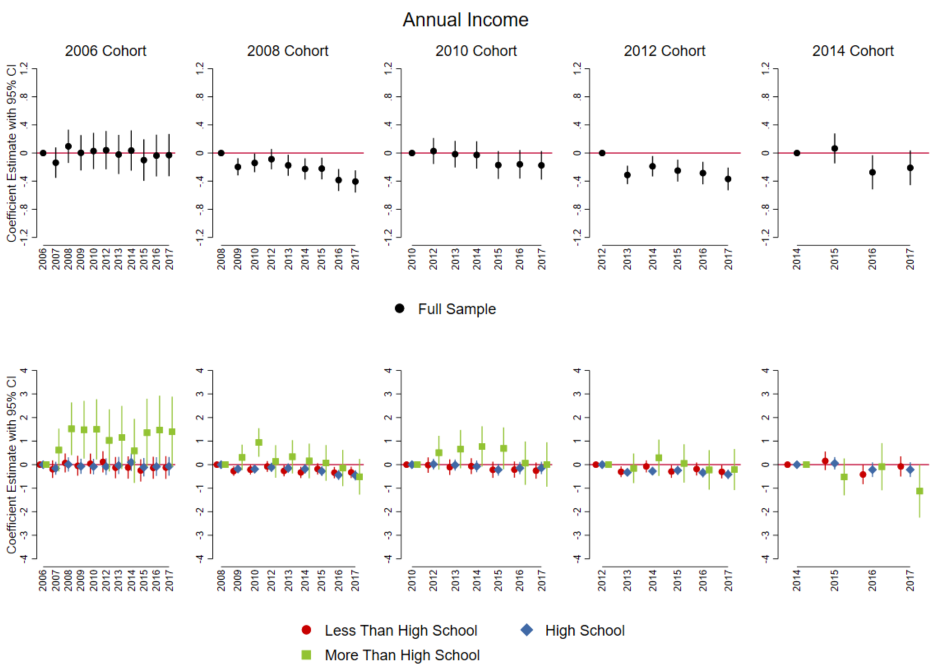

Annual Income
2006 Cohort
2008 Cohort
2010 Cohort
2012 Cohort
2014 Cohort
Coefficient Estimate with 95% CI
Full Sample
Less Than High School
High School
More Than High School

Figure C7: Placebo Test: Real Effect Estimates vs. 100 Random Treatment Assignments (Wages and Annual Formal Earnings)