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# Pennies from haven: Wages and profit shifting

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# Pennies from Haven: Wages and Profit Shifting

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# Pennies from Haven: Wages and Profit Shifting*

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**Abstract**

Increasing attention has been given to the fact that some multinational enterprises shift income to tax haven countries, an activity that generates inequality in corporate taxation. Here, we examine how profit shifting relates to wage inequality. Using rich matched employer-employee data from Norway, we find that profit-shifting firms pay higher wages, particularly among service firms where the wage premium is approximately 2%. Furthermore, this average effect masks significant within-firm heterogeneity with high-skill occupations – and managers in particular – earning higher shifting wage premiums. CEOs particularly gain, with their wages rising nearly 10%. These results thus suggest that profit shifting by multinationals meaningfully contributes to wage inequality, both between and within firms. Finally, our back-of-the-envelope calculations suggest these higher wages would generate additional income tax revenues which would offset around 3% of the fall in Norway's corporate tax revenues due to profit shifting.

**JEL classification:** F23, H26, J31, J32, M12

**Keywords:** Profit Shifting; Tax Haven; Tax Avoidance; Multinational Firms; Wage Distribution; Inequality

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# 1 Introduction

Concerns over multinational enterprises (MNEs) shifting profits to low-tax countries to reduce their tax liabilities have been growing for decades. Recent evidence provided by Tørsløv et al. (2018) underlines the size of this activity, with their estimates suggesting that in 2018 on the order of $948 billion in profits were routed to low-tax countries, primarily tax havens within the European Union. To address the issue, in 2013 the OECD and the G20 launched the ambitious Base-Erosion and Profit Shifting (BEPS) Action Plan, an agenda intended to curb aggressive tax planning. Although tax-motivated profit shifting obviously reduces the corporate tax base in high-tax countries and impacts government revenues, it is unlikely that this is the only significant impact corporate tax avoidance has. Still, very little is known about the ultimate real effects of profit shifting. In this paper, we use matched employer-employee data to provide compelling evidence on one potential effect: the relationship between firms' utilization of tax havens and employee wages.

If the use of havens increases after-tax firm profits, then these rents may be shared with employees via a profit shifting wage premium (akin to the tax windfall effects discussed by Saez et al. (2019) and Azémar & Hubbard (2015)). Using unique and highly-detailed administrative data from Statistics Norway that matches firms and employees, we provide the first evidence of this profit-shifting wage premium and, in particular, show that its size varies across industries and even within a given firm. Specifically, we find more evidence of a profit-shifting wage premium in services than manufacturing and among those workers in high-skill, high-income occupations. Across all industries, we find that high-skill occupations (managers, professionals, and technicians) earn approximately 2% more when working for a profit-shifting firm, with CEOs earning nearly 10% more. In contrast, when controlling for other factors such as industry, we generally find no significant wage premium for low-skill occupations. This suggests that tax-motivated profit shifting has the potential to increase wage inequality both across and within firms. Finally, our back-of-the-envelope calculations suggest that profit shifting firms would pay more in income tax revenues, but these would

only offset about 3% of the Norwegian corporate tax losses their profit shifting creates. Thus, the wage changes associated with profit shifting are likely to only compounding the societal frustrations created by the sense that MNEs do not pay their "fair" share of taxes.

In addition to broadening our understanding of the impacts of profit shifting, our results help to frame the fact that MNEs generally pay higher wages than domestic firms.[1] In our data, we identify firms as profit shifters (hereafter simply shifters) when they are part of a multinational that includes a tax haven affiliate. This ownership link is vital because otherwise payments to the haven are pure costs. Thus, paying inflated prices for imports, excessive fees for intellectual property, or taking on additional debt – all classic ways of shifting profits – is beneficial only when the shifter values the income held in the tax haven. We use our fine-grained data to show that the above wage differences hold when we compare profit shifters just to other MNEs or just to firms that do business with tax havens but do not have haven affiliates. This indicates that the higher wages are not just because profit shifters are MNEs or just because they interact with havens, but that there is something critical about the ownership linkages to havens. Thus while the added productivity and profitability of MNEs can help explain the higher wages their workers receive, an additional portion may be due to their ability to reduce tax burdens in ways unavailable to domestic firms.

In the next section, we review the literature to which we contribute, including that on profit shifting by multinationals and the work linking corporate taxation and wages. Section 3 introduces a simple model of rent sharing between firms and employees that both provides a link between shifting and wages as well as a rationale for why this relationship may differ across workers. In Section 4, we describe the data we use. In particular, this includes comparing the descriptive statistics between shifting and non-shifting firms. Section 5 builds on those comparisons by proceeding to regression analysis. In particular, this examines granular results across industries and workers. Section 5 also contains a battery of robustness

[1] See Setzler & Tintelnot (2021) for a recent overview.

checks to consider the potential endogeneity of a firm's profit-shifting status. Finally, Section 6 concludes.

## 2 Related Literature

Given our focus on the link between profit shifting by MNEs and wages, our analysis crosses three primary strands of literature.

First, we expand upon the existing work examining corporate tax avoidance. It is now well-established that MNEs make substantial tax savings by shifting profits earned in high-tax countries to tax havens. As described by Hines & Rice (1994), tax havens, beyond having a low tax rate, have a good communication infrastructure, legislation enabling business and banking secrecy, and promote themselves as an offshore financial center. Thus, a haven is not just about a low tax rate but also about facilitating the movement of profits. The scale of such profit shifting is sizable; Tørsløv et al. (2018) estimate that close to 40% of multinational profits are shifted to tax havens globally. A key focus of the literature on shifting to havens is in identifying the various shifting techniques. A variety of ways have been identified, including manipulating transfer prices between affiliates [Cristea & Nguyen 2016; Davies et al. 2018; Liu et al. 2020; Wier 2020], the strategic location of intellectual property [Dischinger & Riedel 2011; Karkinsky & Riedel 2012; Griffith et al. 2014], international debt shifting [Fuest et al. 2011; Buettner & Wamser 2013] and treaty shopping [Hong 2018; Riet & Lejour 2018]. We refer to Heckemeyer & Overesch (2017) and Beer et al. (2020) for comprehensive surveys.

While this strand of the literature has focused on where profits are sent and the ways in which this is achieved, it does not address the consequences of profit shifting. Desai & Dharmapala (2009) find a robust positive association between tax avoidance, measured by the book-tax gap, and firm value, meaning that investors expect a higher return when tax burdens are lower. This can arise not just from tax savings but also potential changes in

real activity by shifters. For example, Martin et al. (2020) argue theoretically and empirically that corporate tax avoidance has significantly contributed to the increase in industry concentration observed in the U.S. since the mid-1990s. This suggests that the use of tax havens may give firms a competitive advantage. In line with this result, Overesch (2009) and Goldbach et al. (2019) find that tax planning increases domestic investments by U.S. and German firms. Therefore, profit shifting firms may have a significant edge over other firms which has real effects on capital and, potentially, workers.

Second, our work resonates with recent empirical studies on the incidence of tax windfalls and rent sharing behavior. While there exists a sizeable theoretical literature pointing to pass-through effects of tax burdens onto workers in models with rent sharing or capital reallocation (see Azémar & Hubbard (2015) for an overview), credible identification poses a challenge. At least two conditions must be satisfied to compellingly gauge the implications of corporate taxation for firm outcomes. First, the analysis must appropriately account for the economic environment where tax changes occur. Second, there needs to be sufficient exogenous variation in corporate tax rates. The influential contribution by Fuest et al. (2018) is an important step in this direction. Building upon a wage bargaining model within firms, the authors examine 6,800 corporate tax changes across German municipalities over 20 years. Their event study results indicate that around 50% of tax incidence falls on workers' wages, with low-skilled, young, and female employees bearing the lion's share of the corporate tax burden.[2] Contrary to earlier papers that rely on cross-country variation in tax rates (see, e.g., Arulampalam et al. (2012) for evidence on a negative relationship between corporate tax liabilities and labor remuneration in the EU), Fuest et al. (2018) emphasize that looking within a single country is essential to capture general equilibrium effects and therefore the full incidence of national corporate tax changes.

In a similar fashion, Saez et al. (2019) examine Swedish data to gauge firm responses to a significant payroll tax cut explicitly targeted at young workers. Exploiting the age-based

[2]Compare this first finding to our result that high-skill occupations gain the most from profit shifting.

variation in payroll tax rates in a basic difference-in-differences design, they find strong employment effects for young workers but reject the standard prediction of frictionless labor market models that market wages fully absorb the tax cut. Using a French tax credit, Carbonnier et al. (2022) estimate the pass-through of corporate taxes to wages, finding that workers in high-skill occupations saw an increase in wages following the tax reduction. Low-skill workers, on the other hand, saw no change. Departing from tax-induced rent sharing, Kline et al. (2019) use U.S. patent allowances to examine how patent-induced shocks to firm performance translate into employee pay. Based on a unique linkage of patent applications to U.S. businesses and worker tax records, they infer that the earnings impact is heavily concentrated among board members and employees in the top quartile of the earnings distribution, a result reminiscent of the high-skill occupation results of Fuest et al. (2018) and Carbonnier et al. (2022). Given our theoretical discussion and preliminary data exploration in the next section, we expect similar results in our analysis and are careful to disentangle potential wage effects accordingly. However, a key distinction is that while we also look at workers and firms within a single country, our tax variation arises from outside Norway via the linkages MNEs have to tax havens.

To our knowledge, only two other papers consider the relationship between profit shifting and wages. Souillard (2020) uses US data to compare the compensation paid to executives (the CEO and the CFO) and between firms that have haven affiliates and those that do not. Comparable to our results, he finds that executives receive higher wages in shifting firms. He also compares the total compensation paid to all other workers between firms with and without haven affiliates. Here, he finds a slight decline in the total wage payment to all other workers. This does not, however, mean that individual workers necessarily experience a wage decline following the establishment of a haven affiliate since the firm may shed some workers leading to a decline in the total wage bill.[3] Lopez-Forero (2021), meanwhile, uses French firm-level data. She finds that, following the establishment of a haven affiliate, there is no

[3]This motivates our use of worker-firm fixed effects, which allows us to compare a worker's wages within the same firm both when it does and does not profit shift.

significant change in the average wage the firm pays, although there is a significant decline in the number of workers. Together, these two findings would be in line with Souillard's fall in total wages paid.[4] Excepting Souillard's results for executives, these results are at the firm level, not the level of an individual worker. Thus, our worker-level analysis compliments theirs by providing more detail. Further, since we can identify those workers who work for a given firm both before and after shifting begins, we can sidestep the issues that arise if workers join or leave a firm post-shifting. Finally, we provide granular results across industries and worker characteristics that point to relationships obscured when considering only the average impact.

The third literature we contribute to is the ongoing discussion about the distributional wage effects of globalization. Not only has globalization increased the possibilities for corporate tax avoidance (see Argilés-Bosch et al. (2020) or Evertsson (2016)), it has also been blamed for increasing income inequality.[5]. While early contributions by Feenstra & Hanson (1999) and Katz & Autor (1999) could only distinguish between skilled and unskilled labor and concluded that skill-biased technological change might have been more critical than globalization, more recent studies using granular microdata draw a different picture. Hummels et al. (2014) employ Danish linked employer-employee data to show that the net-wage effect of exporting varies considerably within the same skill type. Conditional on skill-level, the effect of offshoring highly depends on worker task characteristics, with workers who execute non-routine tasks reaping the highest earnings benefits. In the case of Germany, Klein et al. (2013) investigate the impact of exporting on within-firm wage inequality across skill groups and occupations. They report significant exporter wage premia for workers in the highest skill categories but a wage discount for low-skill workers. Notably, they find that export activity explains up to 30% of inequality within and between skill groups.[6] This is

[4]Note that neither Souillard (2020) nor Lopez-Forero (2021) distinguish between firms starting to profit shift and those ceasing to do so. If we follow suit, comparable to Lopez-Forero, we find no significant effect of shifting on the average wage a firm pays. See the Online Appendix for details.

[5]Alvaredo et al. (2017) documents changing income inequality over time

[6]On the import side, work by Autor et al. (2013) and Pierce & Schott (2016) has shown that import competition from China has adversely affected blue-collar workers in the U.S.

consistent with predictions from recent international trade theory models (see e.g., Helpman et al. (2010) and Sampson (2014), among others). In contrast, Egger et al. (2020) develop a structural empirical model with heterogeneous firms and workers and find no exporter wage premium for workers with low education. Keller & Olney (2021) argue that high-income earners and the top 1% may especially benefit from a firm's international activities due to non-market returns that have been largely overlooked in the literature. Using data from the U.S. publicly listed companies and instrumenting for export shocks, they provide evidence that globalization in the form of exports and foreign direct investment is a major driver of executive compensation. Unlike Keller & Olney (2021), we consider more than just executives and/or major firms but use information on all workers and firms. This allows us to take an important step forward in understanding the distributional effects of profit shifting. Further, our analysis sheds light on how not just FDI overall, but that specifically linked to tax avoidance, plays a role in globalization and inequality.

# 3 Theoretical Framework

In this section, we present a simple model of rent-sharing to illustrate a potential link between profit shifting and wages. As discussed by Fuest et al. (2018), several theoretical approaches link wages and a firm's tax burden. That said, their evidence, which looks at the impact of changes in German taxes on the wages paid in Germany, supports the collective bargaining approach we use. With this in mind, consider a firm based in Norway. To produce, the firm uses three inputs. The first is capital ($k$), which has a constant price of one, where a fraction $\alpha$ is deductible from the tax base. In addition, the firm employs two groups of workers, $n_h$ high-skill workers and $n_l$ low-skill workers.[7] Whereas capital can be used in a variable amount, the workers are fixed inputs. Revenues are $r(k)$ which is increasing and concave in

[7]For simplicity, we assume both of these groups are in Norway. This is because our data do not include information on overseas employees. Adding them to our model would mean that the rent generated by profit shifting would be spread across additional worker groups. This, however, would not alter the model's point: profit shifting increases wages, particularly for workers with the most bargaining power.

capital. The wages of the workers, which are bargained over, are $w_h$ and $w_l$ for the high- and low-skill workers, respectively.

If the firm does not profit shift, its after-tax profits are $(1-t)\left(r(k)-\alpha k-\frac{1-\alpha}{1-t}k-n_l w_l-n_h w_h\right)$. From this, the optimal capital level is where $r'(k^*)=\frac{1-t\alpha}{1-t}$. Note that the optimal capital level is decreasing in the tax rate. In this case, equilibrium after-tax profits are $(1-t)\left(r(k^*)-\frac{1-t\alpha}{1-t}k^*-n_l w_l-n_h w_h\right)$ or more compactly $(1-t)(\pi^{no}-n_l w_l-n_h w_h)$.

Alternatively, the firm can shift an amount of profit $\phi$ out of Norway to an affiliate where the tax rate is $t^*<t$.[8] The cost of this is $\beta(\phi)+\gamma$ which is non-tax deductible.[9] Assume that $\beta(\phi)$ is increasing and convex. The optimal amount of profit shifting is where the marginal benefit equals marginal cost, i.e. where $t-t^*=\beta'(\phi^*)$. Note that profit shifting is only desirable when the tax differential $t-t^*$ is large enough to justify both the variable and fixed costs. In particular, if enforcement of profit shifting regulations is laxer in tax havens, we expect such costs to be lower for tax haven affiliates meaning that profit shifting will be larger at the extensive and intensive margins. Note that profit shifting does not affect the optimal amount of capital. Therefore the after-tax profits are:

$$\begin{aligned}(1-t)\left(r(k^*)-\frac{1-t\alpha}{1-t}k^*+\frac{t-t^*}{1-t}\phi^*-\frac{1}{1-t}\left(\beta(\phi^*)+\gamma\right)-n_l w_l-n_h w_h\right)\\ =(1-t)\left(\pi^{shift}-n_l w_l-n_h w_h\right)\end{aligned} \tag{1}$$

if the firm profit shifts. From this, we see that $\pi^{shift}$ is falling in $t^*$, i.e., the lower the affiliate's tax, the greater the overall after-tax profits.

Wages within the firm are determined via bargaining.[10] If employed elsewhere, a high-

[8]Note that we abstract from the ownership pattern, that is whether the Norwegian firm is the parent, the subsidiary, or part of a corporate group that includes a tax haven affiliate.

[9]This cost of profit shifting harkens back to Allingham & Sandmo (1972) which is often interpreted as the expected cost of dealing with penalties and audits and/or the cost of hiring accountants and lawyers to handle the paperwork.

[10]Note that in our setup, the firm's global profits are used as the basis for bargaining. In the model of Krautheim & Schmidt-Eisenlohr (2016), workers bargain only over the surplus they observe. When they

skill worker earns $\overline{w}_h$. Similarly, a low-skilled worker's outside option is $\overline{w}_l$. Inside the firm, however, the wages $w_l$ and $w_h$ for the high- and low-skill workers are simultaneously bargained over. We assume that the workers cannot form a coalition across groups. Equilibrium wages are found by choosing $w_l$ and $w_h$ to maximize:

$$\left(n_l\left(w_l-\overline{w}_l\right)\right)^l\left(n_h\left(w_h-\overline{w}_h\right)\right)^h\left(\pi-n_l w_l-n_h w_h\right)^{(1-l-h)} \tag{2}$$

where $l$ and $h$ are the relative bargaining strengths of the low- and high-skilled workers (where $l+h<1$) and $\pi=\left\{\pi^{no},\pi^{shift}\right\}$ depending on whether or not the firm is profit shifting.[11] This results in equilibrium wages of:

$$w_h=\frac{h}{n_h}\left(\pi-n_l\overline{w}_l\right)+\left(1-h\right)\overline{w}_h \tag{3}$$

and

$$w_l=\frac{l}{n_l}\left(\pi-n_h\overline{w}_h\right)+\left(1-l\right)\overline{w}_l \tag{4}$$

with the remaining $\left(1-l-h\right)\left(\pi-n_l\overline{w}_l-n_h\overline{w}_h\right)$ going to the firm.

This reveals three things. First, the firm will profit shift so long as $\pi^{shift}\geq\pi^{no}$, i.e., if the tax savings overcome the costs of doing so. Second, the wages for both workers are increasing in the value of $\pi$. As such, if $t^*$ falls, both workers' wages increase (as do profits) so long as they have some bargaining power. The relative size of this increase depends on the "per-capita" bargaining power, $\frac{h}{n_h}$ for a high-skill worker and $\frac{l}{n_h}$ of a low-skill worker. If high-skill workers have hard-to-find skills, then all else equal, this could translate into greater bargaining power and thus a larger benefit from profit shifting. In particular, these specialized skills might include those used in profit shifting itself (e.g. accountants with particular training). Even when low-skill workers have more bargaining power as a group

assume that only the high-skill workers observe global profits, they find that high-skill wages rise after profit shifting, whereas low-skill wages fall because local profits decline as that surplus is shifted overseas.

[11] Note that the $(1-t)^{1-l-h}$ term from the firm's payoff does not affect the first-order conditions and is therefore omitted.

($l > h$), if there are sufficiently many such workers, they would gain less on an individual basis when compared to a high-skill counterpart.[12] Further, if $\frac{l}{n_l}$ is quite small (either because low-skill workers are easily replaced and lack bargaining power and/or there are relatively many such workers), then the low-skill gain from profit shifting will be negligible.

# 4 Data

In this section, we describe our data and sample. Our goal is to compare wages between those firms that shift profits to tax havens and those that do not. Our list of tax havens is based on Hines & Rice (1994) with the additions made by Tørsløv et al. (2018).[13] While we here provide the most salient details of the data, we relegate the complete discussion to the Online Appendix.

For our analysis, we draw on highly detailed administrative data that covers the universe of Norwegian firms and employees from 2008 to 2018.[14] The precise data sources for all our variables are reported in the Online Appendix. All data sets were de-identified by Statistics Norway prior to delivery but use a unique identifier to track workers and corporations across datasets and time. We focus on Norwegian corporations that appear in the shareholder register (meaning that we know the complete ownership composition of the firm at year-end) and are present in the data for at least three consecutive years.[15] Further, we only include firm-years with real economic activity, i.e., where there are positive turnover, positive total assets, positive wages, and at least four full-time employees.[16]

[12] The same would hold in a model where group bargaining power was a function of the number of workers (e.g., $l(n_l)$) so long as per-capita bargaining power ($\frac{l(n_l)}{n_l}$) was still decreasing in the number of workers.

[13] Our list of havens includes Andorra, Anguilla, Antigua & Barbuda, Aruba, Bahamas, Bahrain, Barbados, Belgium, Belize, Bermuda, Bonaire, British Virgin Islands, Cayman Islands, Cook Islands, Curacao, Cyprus, Dominica, Guernsey, Gibraltar, Grenada, Hong Kong, Ireland, Isle of Man, Jersey, Jordan, Lebanon, Liberia, Liechtenstein, Luxembourg, Macao, Maldives, Malta, Marshall Islands, Mauritius, Monaco, Montserrat, Nauru, Netherlands, Netherlands Antilles, Niue, Panama, Puerto Rico, Samoa, San Marino, Seychelles, St. Kitts and Nevis, St. Lucia, St. Martin, St. Vincent and the Grenadines, Singapore, Switzerland, Tonga, Turks and Caicos Islands, and Vanuata.

[14] The start date is driven by the availability of our transfer and FATS data.

[15] We also omit those that which appear in the data, disappear, and then reappear. For instance, firm A would be excluded if it existed in 2012 and then again in 2015 but is missing in 2013 and 2014.

[16] This final restriction reduces the number of firms in our sample by about 15% and the number of

## 4.1 Firm-level Data

Our comprehensive firm-level information includes accounting data for all non-financial corporations and their industry affiliation, where we use the two-digit SIC2007 industrial classification. We exclude firms in the agricultural and primary industries, including mining and oil (categories A and B).[17] We do so because of the special tax treatment these industries receive. Our data also include very detailed profit statements and balance sheets for tax purposes, with comprehensive income, costs, profits, tax, and asset information. In addition, the foreign trade register provides detailed firm-level information on imports and exports of goods.[18] We use these to construct importer and exporter dummies at the firm-year level.[19] Using the employee data described below, we construct the percentage of workers that are female, immigrant, unionized, and high-skill (defined as workers with an undergraduate degree or higher). Additional factors, such as macroeconomic or industry-specific shocks, are controlled for via industry-year fixed effects. Firm fixed effects meanwhile absorb time-invariant characteristics such as geographic location.

A unique feature of our data is the currency register, a dataset that contains comprehensive information about cross-border bank transfers by firms (including the value of a single transaction, its exact date, and the country where the account is registered). This allows us to see not only which firms send money abroad but which are sending money to tax havens in particular. This then permits us to compare firms engaged in arms-length business with tax havens (i.e., they send money to havens but do not have an affiliate there) with those potentially engaged in intra-firm transfers (that is, they both send money to a haven and have an affiliate there). While more detail can be found in the Online Appendix, as one might expect, transactions are generally highest with EU countries and the UK. Focusing on

firm-year observations by 14%.

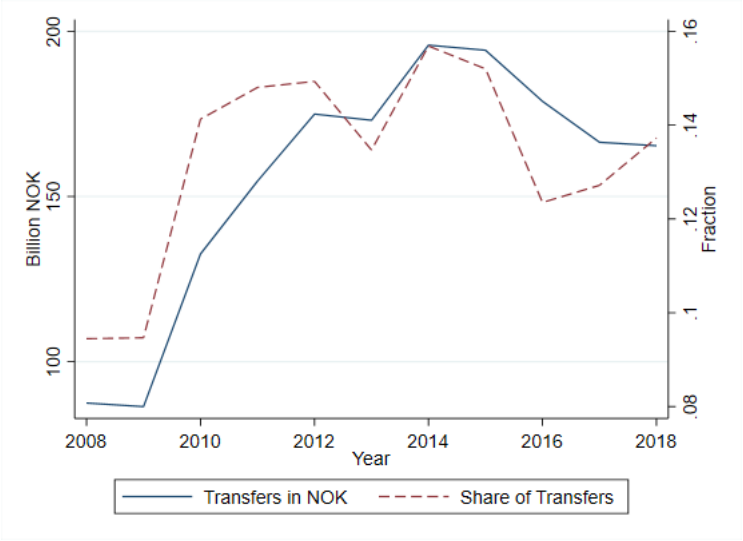
[17] Note that since Statistics Norway in their standard deliveries do not provide accounting data for financial corporations, they are also absent from the sample.

[18] Data on trade in services only span from 2014-2018 and are based on a survey rather than the universe of firms; hence we only consider goods trade.

[19] While additional information, such as year of incorporation, is available for a subset of firms, coverage is highly incomplete and we therefore do not use them.

the transfers to havens, Figure 1 shows these grew rapidly from 2008 to 2014 before leveling off. As the figure also shows, a similar pattern is found in the share of total transfers destined for havens (about 14% after 2010). Furthermore, and consistent with the evidence of Tørsløv et al. (2018), havens in the EU received the bulk of the haven-bound transfers in volume and share. Table 1 lists the top ten tax havens in 2018 ranked by the volume of transfers and the number of Norwegian affiliates they host; again, EU havens dominate. Since our goal is to compare shifters with otherwise comparable firms, we further omit purely domestic firms to focus on international ones. Thus, we require firms to have either outward transfers, a foreign affiliate, and/or a foreign shareholder in at least one year during our time period.

Figure 1: Cross-Border Transfers to Tax Havens (in Billion NOK), 2008-2018




Note: Based on 18,280 firms.

## 4.2 Multinationals and Profit Shifters

Our variable of interest is whether or not the firm has an ownership linkage to a tax haven, meaning that it is part of a multinational structure that includes a haven. Ownership information is drawn from three sources. First, the shareholder register provides exact end-of-year ownership shares for all owners of Norwegian firms. We aggregate these up to the firm-country-year level. When at least 50% of the ownership is outside Norway, we classify

Table 1: Top Tax Haven Destinations by Cross-Border Transactions and Majority-owned Affiliates, 2018

| Tax Haven | Outflows (in Billion NOK) | Tax Haven | Number of Affiliates |
|---|---|---|---|
| Ireland | 65.062 | Netherlands | 163 |
| Netherlands | 54.495 | Switzerland | 74 |
| Belgium | 27.673 | Singapore | 42 |
| Switzerland | 9.295 | Luxembourg | 39 |
| Luxembourg | 7.431 | Belgium | 25 |
| Singapore | 6.353 | Ireland | 19 |
| Hong Kong | 4.189 | Cyprus | 14 |
| Cayman Islands | 1.764 | Hong Kong | 12 |
| Bahamas | 1.565 | Bermuda | 9 |
| Cyprus | 0.273 | Guernsey | 3 |

Notes: Based on 13,806 firms in the sample in 2018. All monetary values are deflated and in Billions of Norwegian Krone (1 NOK $\approx$ 0.1 USD as of December 2021). The definition of tax haven affiliation is based on ownership shares of at least 50% (both inbound and outbound).

the firm as an MNE in that year. When owners hold at least 50% in tax havens, we further classify the firm as a profit shifter. Note that shifters are always MNEs, but the reverse is not true. Second, we use the Outward FDI (OFDI) database that, for every Norwegian firm, identifies all of its overseas affiliates and its share of ownership.[20] If this share is at least 50% for some affiliate in a given year, the firm is again classified as an MNE for that year.[21] Further, if this threshold is reached for an affiliate in a tax haven, it is tagged as a shifter. We complement the OFDI data with a third dataset, the Foreign Affiliate Statistics (FATS), a survey that covers the largest firms in Norway (approximately 1,700 per year). The benefit of the FATS data is that it provides information on indirect ownership so that if a Norwegian firm owns an affiliate in A that itself owns an affiliate in B, we would observe the connection to B in the FATS but not the OFDI.[22] As such, we can identify indirect linkages to tax havens for those firms included in the FATS. Since not all firms are included in the FATS (especially small ones), this may introduce imprecision to our profit shifter variable. If this is the case, it will bias our results towards insignificance.[23]

[20]Note that the ownership must be at least 20% to appear in the database.

[21]Due to the survey nature of the OFDI and FATS data, we classify a firm as an MNE in year $t$ and $t+2$, but there is no information in year $t+1$ it as an MNE in the missing year as well.

[22]We were also able to classify nine additional firms (twelve firm-years) as MNEs who were in the FATS but not the OFDI.

[23]In unreported results, we decomposed our shifting firms into those where the haven link results from inbound FDI and those where it was due to outbound investment. We found the largest effects for inbound

An important feature of the shifter designation is how it changes for a given firm over time. We can categorize this within-firm variation into four groups: *always-shifters*, *on-switchers*, *off-switchers* and *flip-floppers*. Always-shifters are defined as firms where the shifter dummy equals one throughout the sample period (257 firms). On-switchers, meanwhile, establish their first haven connection at some point during the sample with no further changes (177 firms). In contrast, off-switchers begin the sample as a shifter and then cease with no further changes (64 firms). Finally, flip-floppers are firms that change their treatment status more than once (85 firms). Beyond being an aspect of the data, these distinctions can be important for identifying the wage effects from shifting if wages are sticky downwards, i.e., it is more difficult to cut wages than increase them. If this is the case, then the impact from switching out of treatment (ceasing to shift) would have a smaller effect – or even none at all – when compared to switching into treatment. In additional results in the Online Appendix, we find no significant link between shifting and wages when using a single shifter dummy across all four categories.[24] However, if we allow for asymmetric treatment effects, we find coefficients for the always-shifters and on-switchers comparable to those reported here but no significant relationship between shifting and wages for off-switchers and flip-floppers.[25] This result is consistent with downward wage stickiness. With this in mind, we omit the off-switchers and flip-floppers from our sample in order to cleanly compare those shifters and non-shifters. As illustrated in Figure 2, the fraction of profit shifting firms rose steadily from 1.8% in 2008 to 2.8% in 2018 with this rise being driven by on-switchers and firms that enter the sample for the first time after 2008 but shift in all years.[26] The same figure shows the evolution of the share of employment in shifting firms which rose by about one-third from 2008 to 2014 before declining slightly. Note that although shifters only make up about 2% of firms, they

shifters, suggesting that the added ability to observe indirect linkages from the FATS data is not driving the result.

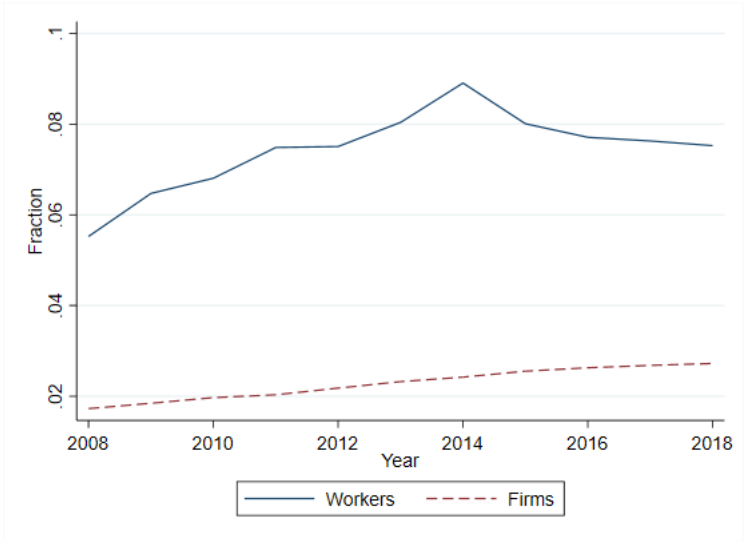
[24]This result is comparable to Lopez-Forero (2021) who does not distinguish between the two when estimating the firm's average wage.

[25]This is despite the greater within-firm variation for the flip-floppers.

[26]As discussed below, since shifters are on average larger than non-shifters they make up 10.5% of our total worker-year observations.

comprise about 8% of employment, i.e., shifters tend to be bigger.

Figure 2: Share of Profit Shifters in Firms and Employment, 2008-2018




Note: Based on 18,280 firms.

Table 2 shows the distribution of shifters and non-shifters across industries. Two things emerge. First, regardless of whether we consider the share of firms or the share of workers, shifters are more commonly found in services than manufacturing. This is notable because of the possibility that firms in services may have an easier time profit shifting because of the role intangible assets play in their activities.[27] Second, even within services, shifters tend to be found in certain industries, particularly wholesale trade. Note that this concentration can depend on whether we look at the percentage of firms or the percentage of workers. For example, the percentage of firms points to shifters in wholesale trade but this is not true for the percentage of workers (meaning that the average profit-shifting wholesaler is smaller than the average non-shifting wholesaler). On the other hand, computer programming shows the opposite pattern; while the share of firms is roughly the same across groups, profit shifters in this industry make up a noticeably larger share of employment.

[27]See, for example, the estimates of Lawless et al. (2018) who find that services are less sensitive to taxation than other sectors.

Table 2: Sectoral Distribution of Profit Shifting Firms

| | Profit Shifter % of: | | Non-shifter % of: | |
|---|---|---|---|---|
| Two-digit Industry | Firms | Employment | Firms | Employment |
| Wholesale trade, except for motor vehicles & motorcycles | 26.93 | 12.37 | 12.27 | 11.11 |
| Architectural & engineering activities | 7.38 | 9.44 | 4.57 | 4.74 |
| Water transport | 6.00 | 2.95 | 0.73 | 1.45 |
| Publishing activities | 4.97 | 3.07 | 2.20 | 2.68 |
| Warehousing & support activities for transportation | 4.80 | 3.87 | 1.53 | 2.78 |
| Computer programming; consultancy & related activities | 4.29 | 10.36 | 3.54 | 3.52 |
| Manufacture of machinery and equipment n.e.c. | 3.43 | 4.57 | 1.52 | 2.47 |
| Office administrative & support; Other business support | 2.57 | 1.20 | 1.11 | 1.00 |
| Manufacture of food products | 2.23 | 2.74 | 2.23 | 2.81 |
| Manufacture of computer, electronic & optical products | 2.23 | 3.96 | 0.46 | 0.89 |
| Other | 35.16 | 45.47 | 69.84 | 66.55 |

Notes: Based on 583 profit shifters (accounting for 11.32% of total employment) and 17,697 non-shifters (88.68% of total employment).

## 4.3 Haven Users or Profit Shifters?

In naming our variables, we referred to those with an ownership connection to a tax haven as a "profit shifter", a choice that presumes that those firms are actually shifting profits to their haven affiliates. Here, we consider the validity of this presumption by following Bilicka (2019) and comparing the return on assets (RoA, defined as taxable profits divided by total assets) for shifters and non-shifters. We do so in a regression that also controls for various lagged firm characteristics and industry-year fixed effects. If a firm is actively engaged in shifting, we would expect the RoA to be abnormally low, i.e. taxable profits are unexpectedly low relative to the firm's assets. These results, reported in Table 3, find that firms with a haven ownership link have an RoA that is on average 26% smaller than comparable firms without such a connection.[28] This figure recalls that of Godar (2021) who finds that, after a tax-haven affiliate is established, the net-of-tax profits of German firms fall by 36.5%. With this in mind, we continue to somewhat provocatively label firms with tax haven linkages as profit shifters.

[28]The mean RoA is 0.068.

Table 3: Profit Shifting Status and Return on Assets (2008-2017)

| Dep. Variable: Return on Assets | (1) | (2) |
|---|---|---|
| | No Controls | With Controls |
| Profit Shifter | –0.0327*** | –0.0177** |
| | (0.00789) | (0.00824) |
| log(Total Fixed Assets) | | –0.0135*** |
| | | (0.000565) |
| log(Turnover) | | 0.0220*** |
| | | (0.00146) |
| Bank Transfers/Turnover | | 0.00007 |
| | | (0.00009) |
| log(Employees) | | –0.0135*** |
| | | (0.00172) |
| Share High-Skill | | 0.0214*** |
| | | (0.00416) |
| Share Female | | –0.00384 |
| | | (0.00365) |
| Share Immigrant | | 0.000445 |
| | | (0.00765) |
| Share Unionized | | –0.0337*** |
| | | (0.00262) |
| Exporter | | –0.0147*** |
| | | (0.00187) |
| Importer | | –0.0122*** |
| | | (0.00173) |
| MNE | | –0.00137 |
| | | (0.00364) |
| Mean Dep. Variable | 0.068 | 0.068 |
| Industry-Year FE | YES | YES |
| $R^2$ | 0.0558 | 0.0839 |
| N | 134,526 | 134,526 |

Notes: Return on assets (ROA), defined as the ratio of taxable profits to total assets, is restricted between -1 and 1. Standard errors clustered at the firm-level in parentheses. The average ROA for never shifters is 0.070, while for profit shifters it is 0.022. All explanatory variables are lagged by one year. */**/*** indicates significance at the 10/5/1% levels.

## 4.4 Employee Data

For employees, we have rich data on socio-economic characteristics including education (nine categories from 'no education and pre-school education' to 'postgraduate education'), occupation (eight categories from 'elementary occupations' to 'managers', age, and marital status. We also separately identify the CEO via the firm's board composition. Worker fixed effects absorb time-invariant characteristics such as gender and country of birth. In addition, for each worker-firm-year, we have the total wage payments made to the employee. As discussed in the Online Appendix, we restrict ourselves to full-time workers aged 18-67 and allocate them to the firm which pays them the highest share of their wages in a given year. We also limit ourselves to worker-firm spells which last a minimum of three years to examine the relationship between shifting and wages within a fairly stable employment situation.[29]

In Figure 3, we plot the standard deviation of logged wages, a proxy for wage inequality, over time.[30] Two patterns emerge. First, wages are more unequal in services than manufacturing. Second, there has been a gradual upward trend in inequality. In particular, we see a noticeable increase in inequality within services during the latter part of the sample. Given that shifters are an increasingly large share of the workforce (Figure 2), this may point towards a link between profit shifting and wage inequality.

In the end, we are left with 18,280 firms (of which 257 shift throughout the sample and 177 begin to do so) and 742,839 workers with a total of 5,363,793 worker-year observations (of which 10.5% are for shifters). Within these, 19.8% of our shifters are in manufacturing, so again, the bulk of them are in services. Summary statistics can be found in the Table 4.

[29]In the Online Appendix, we report broadly similar results when restricting ourselves to worker-firm spells of at least seven years which, by virtue of the length of our data, means we do not include employment spells for multiple firms for a single worker.

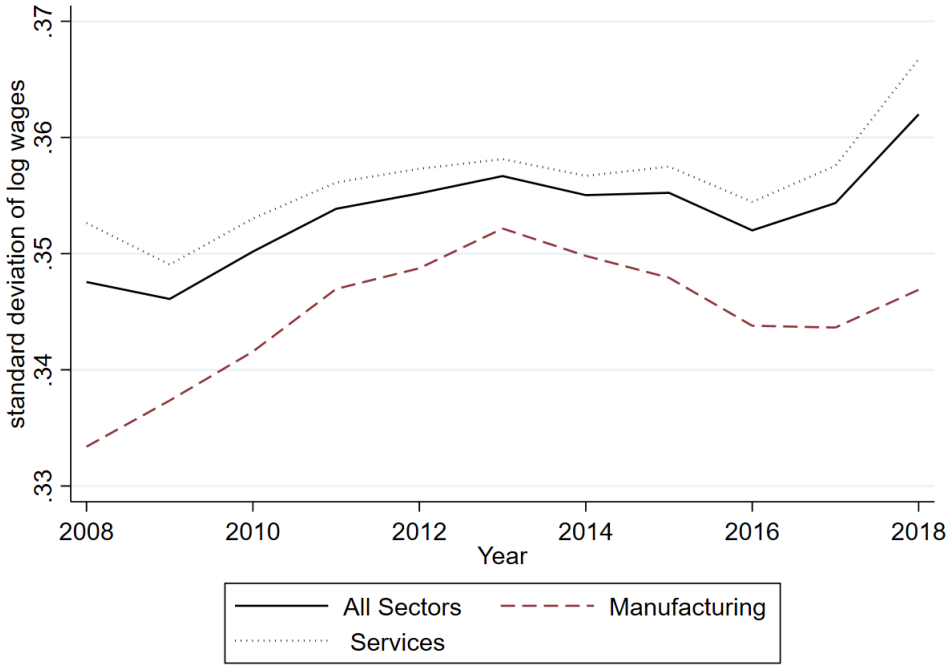
[30]For example, both Card et al. (2013) and Song et al. (2018) use the variance of wages as a measure of inequality.

Table 4: Summary Statistics by Profit Shifting Status, 2008-2018

| | *All Firms* | | *Non-Shifters* | | *Profit Shifters* | |
|---|---|---|---|---|---|---|
| VARIABLES | Mean | Std. Dev. | Mean | Std. Dev. | Mean | Std. Dev. |
| *Firm-year Panel* | | | | | | |
| Total Fixed Assets | 50,518.2 | 224,321.9 | 45,155.2 | 207,173.1 | 263,561.6 | 545,716.5 |
| Turnover | 124,327.7 | 304,719.5 | 118,049.9 | 291,977.3 | 373,710.0 | 575,507.3 |
| Bank Transfers/Turnover | 0.265 | 7.072 | 0.228 | 6.945 | 1.705 | 10.89 |
| Employees | 31.02 | 111.5 | 29.45 | 103.4 | 93.05 | 277.5 |
| Share High-skilled | 0.147 | 0.285 | 0.143 | 0.282 | 0.298 | 0.362 |
| Share Females | 0.118 | 0.250 | 0.118 | 0.250 | 0.132 | 0.228 |
| Share Immigrants | 0.028 | 0.113 | 0.028 | 0.113 | 0.024 | 0.092 |
| Share Unionized | 0.392 | 0.444 | 0.392 | 0.445 | 0.391 | 0.441 |
| Exporter | 0.337 | 0.473 | 0.327 | 0.469 | 0.745 | 0.436 |
| Importer | 0.658 | 0.474 | 0.651 | 0.477 | 0.942 | 0.234 |
| Firms | 18,131 | | 17,697 | | 434 | |
| Observations | 167,051 | | 162,949 | | 4,102 | |
| *Worker-year Panel* | | | | | | |
| Annual Wage | 589.0 | 310.3 | 579.7 | 289.9 | 706.2 | 485.7 |
| - Managers | 840.2 | 608.5 | 817.9 | 554.7 | 1,093.8 | 1,007.3 |
| - Professionals | 712.2 | 278.7 | 703.4 | 270.1 | 778.8 | 329.0 |
| Age | 43.31 | 11.34 | 43.27 | 11.37 | 43.80 | 10.94 |
| Married | 0.487 | 0.500 | 0.485 | 0.500 | 0.516 | 0.500 |
| Workers | 721,173 | | 674,701 | | 56,549 | |
| Observations | 5,181,200 | | 4,799,508 | | 381,692 | |

Notes: The sample consists of all firms with a positive foreign ownership share (inbound or outbound) and/or positive cross-border transactions in at least one year during the sample period. Profit shifters are defined as firms with inbound/outbound foreign ownership shares of at least 50% in a tax haven country. All monetary values are deflated and in 1,000 Norwegian Krone (1 NOK $\approx$ 0.1 USD as of December 2021). Total fixed assets, turnover and annual wages are winsorized at the 1st and 99th percentiles.

Figure 3: Wage Inequality, 2008-2018




Note: Based on 18,280 firms.

## 4.5 A Descriptive Comparison of Shifters and Non-Shifters

As an initial examination of the potential link between shifting and wages, we turn to Table 4 and compare the summary statistics for shifters and non-shifters.[31] Looking at the mean wage, we see that wages in profit-shifters are 21.8% higher than in non-shifters (706.2 vs. 579.7).[32] We refer to this difference as the shifting wage premium. For managers, this gap is 33.7%, suggesting that high-skill, high-income workers may get a higher shifting wage premium. Our model would predict this if managers have higher per-capita bargaining power. This comparison, however, must be understood as merely suggestive since there are other significant differences between shifters and non-shifters beyond their linkages to tax havens. For example, shifters are, on average, much larger than non-shifters. Compared

[31]The profit-shifter column includes all years for all shifters, i.e., it also includes information for on-switchers before they begin shifting.

[32]Note that the summary statistics reports wages in real levels while all other analysis uses their logged value.

to non-shifters, with higher levels of employment, turnover, and total fixed assets. Since larger firms tend to pay higher wages, the difference in means may be attributable to firm size differences. Alternatively, Table 4 shows that the share of high-skill workers in shifters is twice that of non-shifters. Thus, the difference may be due to profit-shifters hiring more high-skill (and high-wage) individuals.[33] This indicates a need to control for additional firm (and worker) characteristics via regression analysis.

Before doing so, in Figure 4 we compare the 2018 wage distributions for shifters and non-shifters. Again, although this does not control for any worker or firm characteristics, it is apparent that the overall wage distribution for a shifter is to the right.[34] Moreover, there appears to be a greater mass of higher-paid workers in these firms, again suggesting a particular need for high-skill workers. In Figure 5, rather than use all workers, we compare the distributions for the high-skill (Panel A) and low-skill (Panel B) occupations separately.[35] As before, we see a shift to the right in both distributions, indicating that the higher wages in shifters are not limited to managerial staff. To assess whether there is a statistically significant difference in the wage distributions in each of these figures, we used the Kolmogorov-Smirnov (KS) test and obtained a p-value of $< 0.001$ for all pairwise comparisons. Thus the difference in these distributions is statistically significant.[36] While these comparisons are again suggestive, it must again be remembered that they do not control for other firm or worker characteristics. Further, they do not speak to the size of the shifting wage premium within a given occupation. With these limitations in mind, we now turn to regression analysis.

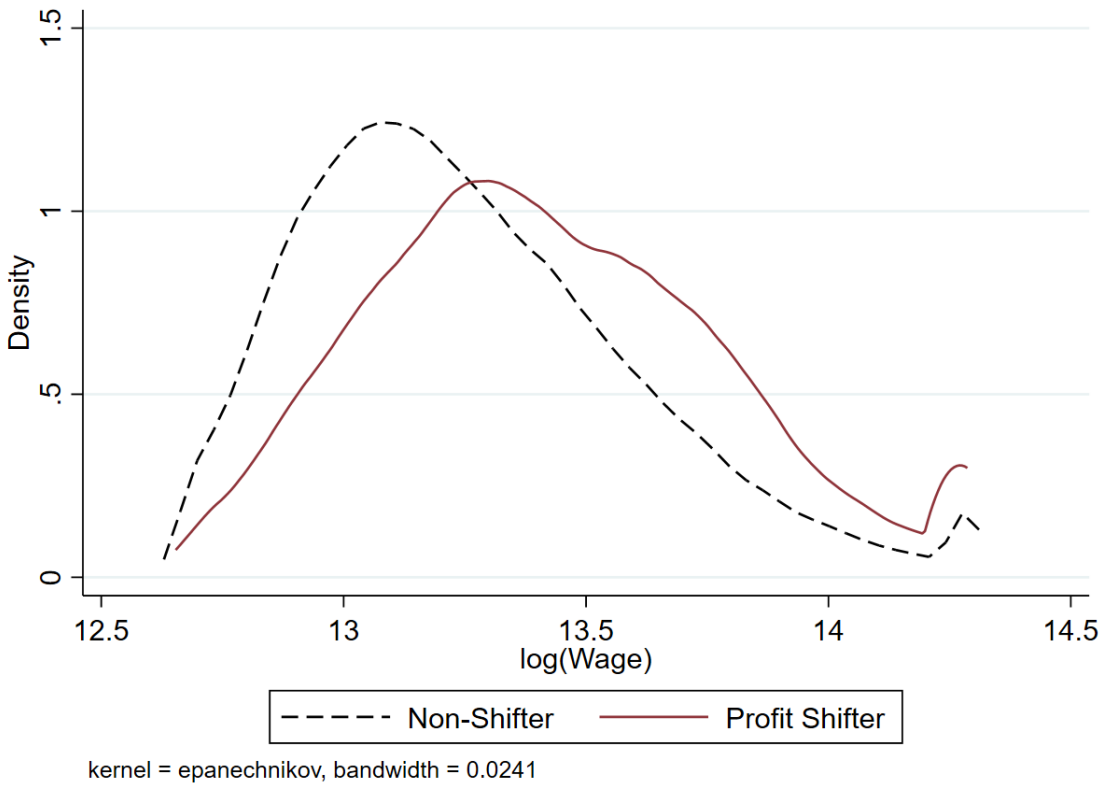
[33]This could be driven by industry differences. Alternatively, it could represent the need for high-skill workers (e.g., lawyers and accountants) to engage in shifting, something we explore in our robustness checks.

[34]We find similar results when looking just at manufacturing or services as found in the Online Appendix.

[35]Our high-skill occupations are managers, professionals, and technicians and associated professionals.

[36]Details available in the Online Appendix.

Figure 4: Wage Distribution, 2018




# 5 Results

In this section, we build on our descriptive comparison by using regression analysis to estimate an average shifter wage premium before decomposing this across occupations and income levels.

## 5.1 Baseline Estimation

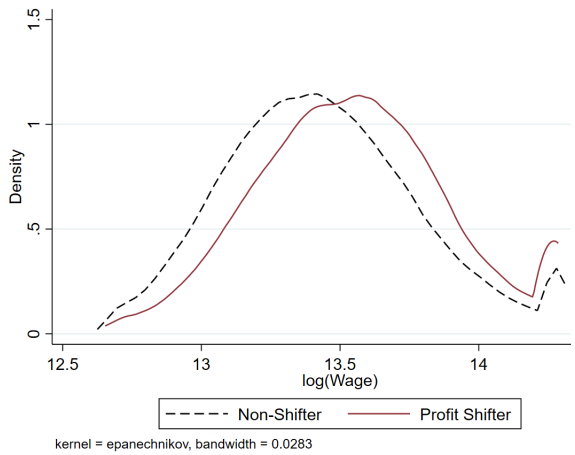
To quantify the impact of profit shifting on individual employee pay, we rely on the following empirical model:

$$log(Wage_{i,j,t}) = \alpha Shifter_{j,t-1} + \beta Z'_{j,t-1} + \gamma X'_{i,t} + \delta_i + v_j + \phi_{s,t} + \varepsilon_{i,j,t}, \tag{5}$$

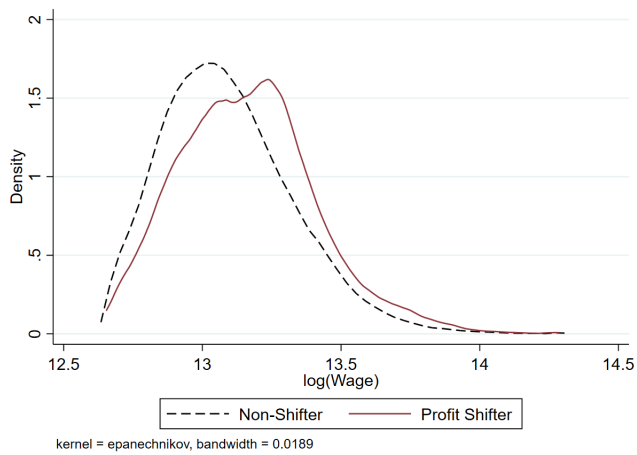
where the dependent variable $Wage_{i,j,t}$ denotes the log wages earned by worker $i$ employed by firm $j$ in year $t$. Our variable of interest, $Shifter_{j,t-1}$, indicates whether firm $j$ is classified as a profit shifter. Time-varying firm characteristics are captured by the vector $Z'_{j,t-1}$. These

Figure 5: Wage Distribution by Occupation Type, 2018

(a) High-Skill Occupations




(b) Low-Skill Occupations




include total fixed assets, turnover, and employment (all logged). We also control for four measures of the firm's international exposure: the ratio of international transfers relative to turnover and dummy variables indicating whether the firm exports, imports, and/or is a multinational. Finally, we control for the share of the firm's workforce that is high-skill, female, immigrant, and unionized. Note that all firm attributes are lagged by one period to mitigate endogeneity concerns (more on this below). The vector $X'_{i,t}$ consists of worker-level control variables including age, age squared, and marital status.[37] Additionally, we control for fixed effects at the worker ($\delta_i$), firm ($v_j$) and industry-year ($\psi_{s,t}$) level where $s$ is firm $j$'s industry affiliation). When including firm fixed effects, we rely on within-firm variation in profit shifting status to identify the coefficient, i.e., the on-switchers. In some specifications, we replace the worker and firm fixed effects with worker-firm fixed effects. In this alternative specification, the shifter coefficient is driven by within worker-firm variation, that is, by workers who were employed by an on-switcher both before and after it establishes an ownership linkage to a haven. This is in contrast to Souillard (2020) and Lopez-Forero (2021) whose analysis is at the firm rather than worker level.

Table 5 lays out our baseline results. In the table, odd-numbered columns include worker and firm fixed effects (as well as industry-year ones) whereas even-numbered columns use worker-firm fixed effects. In columns (1) and (2), we use the entire sample. Columns (3) and (4) restrict the sample to manufacturing, whereas columns (5) and (6) use just services firms. As noted above, shifters tend to be found in the services industries, and there is some

[37] In the Online Appendix, we report results also controlling for a worker's lagged wage, i.e. estimating the growth rate of wages. We find that, within services, shifting is associated with a higher wage growth, not just with a higher wage level.

belief that, due to intangible assets, service firms may find profit shifting relatively easy. Across samples and specifications, we see a strong correlation between firm size (turnover and the number of employees) and employee pay. In addition, married and older workers on average earn more, although the marginal effect of age declines as the worker gets older.

Turning to our profit shifting dummy, we find no significant coefficient when using all firms or just those in manufacturing. We do, however, find a strongly significant coefficient when looking just at services. This would be consistent with the argument that shifting may be easier, and thus more prevalent, in these industries. The point estimate from column (5) suggests that when a firm is profit shifting, workers earn 2.12% more. When we instead control for worker-firm fixed effects the estimate is somewhat smaller at 1.78%. This latter specification is particularly notable because the identification is driven by workers who are employed by a firm both before and after it begins shifting. This then argues against the concern that the results in column (5) are driven solely by firms shedding low-wage workers or high-wage workers joining firms that are already profit shifting.

## 5.2 Robustness Checks

Although these results indicate a difference in the average wage paid by shifters in the service sector, they do not themselves imply causality. We therefore now consider a set of robustness checks that speak to potential endogeneity. First, in Figure 6, we report coefficients from a modification of our baseline specification that looks for pre-trends in the wage gap between shifters and non-shifters.[38] Specifically, we now consider an event study specification where period 0 is the first year of treatment.[39] In this figure, we include all firms (both manufacturing and services). As can be seen, prior to treatment there is a minuscule difference between future shifters and other firms and this difference is not significant. Post-treatment, we see a positive coefficient, but in line with Table 5's column (2) it is insignificant. In Figure 7, Panel A repeats this for manufacturing while Panel B does so just for services. In both we see a similar pattern – no evidence of a pre-trend and positive, if insignificant, point estimates after entering into treatment. The exception to this is in services where we

[38]Here, we report results using worker-firm fixed effects.

[39]To be clear, the event is entering into treatment (starting to shift) in $t-1$ with wages in $t$ being the outcome variable. Note that we bin firm-year observations at least three years prior to treatment, including our never-shifters, in period -3 (see Schmidheiny & Siegloch (2019)). Likewise we bin those observations three or more years after shifting starts (including our always shifters). In recognition of the truncated observation of our always- and never-shifters (e.g., an always shifter shifts from 2008 onwards, but we do not know how long it had been shifting prior to 2008), we repeated this process using observations from 2011 and/or until 2015 so that we had at least three years before/after the sample by which to observe our always- and never-shifters. Similar patterns were found although with the shortened sample estimates were less precise. These results are available on request.

Table 5: Wages and Profit Shifting Status, 2008-2018

| Dep. Variable: log(Wage) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Firms | All Firms | Manuf. | Manuf. | Services | Services |
| Profit Shifter | 0.00974 | 0.00750 | –0.00075 | –0.00088 | 0.0212*** | 0.0178*** |
| | (0.00682) | (0.00627) | (0.00997) | (0.00923) | (0.00692) | (0.00651) |
| log(Total Fixed Assets) | 0.00118 | 0.00134* | 0.00266 | 0.00271 | 0.00089 | 0.00104 |
| | (0.00084) | (0.00079) | (0.00179) | (0.00166) | (0.00089) | (0.00084) |
| log(Turnover) | 0.0357*** | 0.0364*** | 0.0406*** | 0.0407*** | 0.0346*** | 0.0352*** |
| | (0.00443) | (0.00421) | (0.00503) | (0.00462) | (0.00539) | (0.00512) |
| Bank Transfers/Turnover | 0.00014 | 0.00014 | –0.00034 | –0.00038 | 0.00017 | 0.00017 |
| | (0.00012) | (0.00012) | (0.00057) | (0.00054) | (0.00012) | (0.00012) |
| log(Employees) | 0.0132*** | 0.0142*** | 0.0136** | 0.0142*** | 0.0137*** | 0.0144*** |
| | (0.00359) | (0.00341) | (0.00575) | (0.00532) | (0.00414) | (0.00391) |
| Share High-Skill | 0.00295 | 0.00317 | 0.00613 | 0.00606 | 0.00266 | 0.00265 |
| | (0.00465) | (0.00435) | (0.0178) | (0.0164) | (0.00454) | (0.00426) |
| Share Female | 0.00435 | 0.00433 | 0.0287*** | 0.0309*** | 0.00161 | 0.00121 |
| | (0.00407) | (0.00386) | (0.00935) | (0.00896) | (0.00442) | (0.00415) |
| Share Immigrant | –0.00499 | –0.00415 | 0.00752 | 0.00854 | –0.00717 | –0.00665 |
| | (0.00629) | (0.00585) | (0.0116) | (0.0107) | (0.00721) | (0.00670) |
| Share Unionized | –0.00078 | –0.00072 | –0.00734 | –0.00706* | 0.00183 | 0.00182 |
| | (0.00228) | (0.00211) | (0.00460) | (0.00426) | (0.00254) | (0.00234) |
| Exporter | –0.00068 | –0.00067 | –0.00272 | –0.00262 | –0.00041 | –0.00039 |
| | (0.00101) | (0.00094) | (0.00228) | (0.00210) | (0.00109) | (0.00101) |
| Importer | 0.00003 | 5.31e-07 | –0.0067*** | –0.0068*** | 0.0005 | 0.0005 |
| | (0.0009) | (0.0009) | (0.0025) | (0.0024) | (0.0010) | (0.0009) |
| MNE | –0.00123 | –0.00140 | –0.00228 | –0.00272 | –0.00100 | –0.00113 |
| | (0.00290) | (0.00269) | (0.00521) | (0.00480) | (0.00337) | (0.00315) |
| Age | 0.0194*** | 0.0186*** | 0.0196*** | 0.0193*** | 0.0191*** | 0.0183*** |
| | (0.00037) | (0.00036) | (0.00079) | (0.00076) | (0.00042) | (0.00041) |
| $Age^2$ | –0.0003*** | –0.0003*** | –0.0003*** | –0.0003*** | –0.0003*** | –0.0003*** |
| | (5.10e-06) | (4.92e-06) | (0.00001) | (9.92e-06) | (5.83e-06) | (5.58e-06) |
| Married | 0.00785*** | 0.00750*** | 0.00515*** | 0.00500*** | 0.00858*** | 0.00831*** |
| | (0.00076) | (0.00073) | (0.00138) | (0.00127) | (0.00090) | (0.00086) |
| N | 4,743,071 | 4,743,071 | 1,169,212 | 1,169,212 | 3,573,859 | 3,573,859 |
| $R^2$ | 0.865 | 0.874 | 0.873 | 0.878 | 0.865 | 0.873 |
| Worker FE | YES | | YES | | YES | |
| Firm FE | YES | | YES | | YES | |
| Worker-firm FE | | YES | | YES | | YES |
| Industry-year FE | YES | YES | YES | YES | YES | YES |

Notes: Standard errors clustered at the firm-level in parentheses. All explanatory variables are lagged by one year. Manufacturing firms are defined as those in industries 10-33 using the Standard Industrial Classification 2007 (SIC 2007). */**/*** indicates significance at the 10/5/1% levels.

do see a significantly positive shifter wage premium but only after three years. This delay following the establishment of a haven affiliate might be expected for two reasons. First, it may take time before the wage-increasing rents created by avoiding taxes manifest. Second, if there is downward wage stickiness, the firm may fend off demands to share the tax savings until it is more confident that the new affiliate will survive.[40] When combined with the results in Table 5, the estimates do not indicate significant differences between shifters and non-shifters prior to treatment, pointing towards a potential causal effect.

Figure 6: Event Study: All Firms

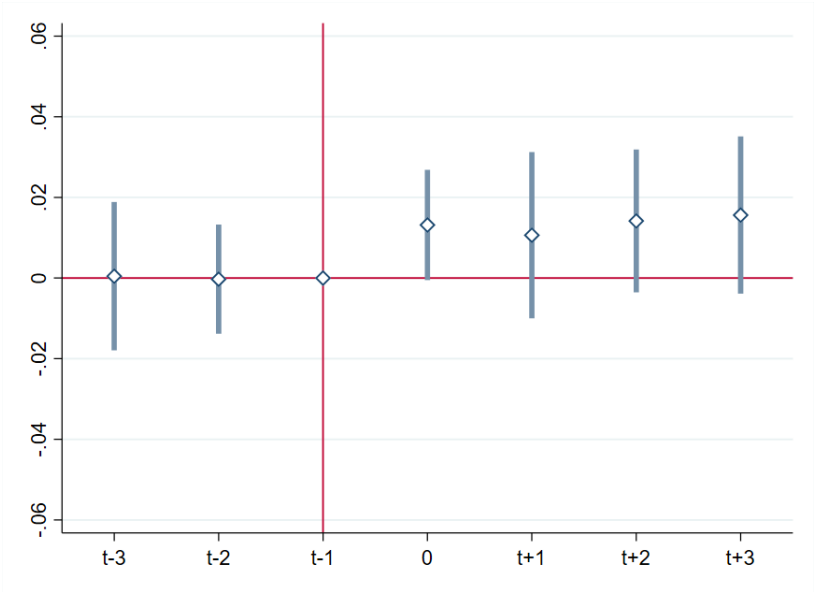
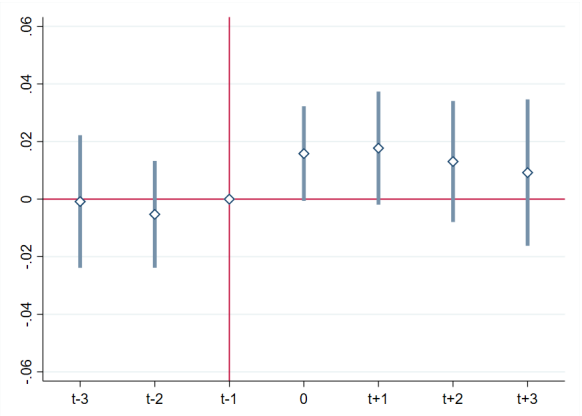
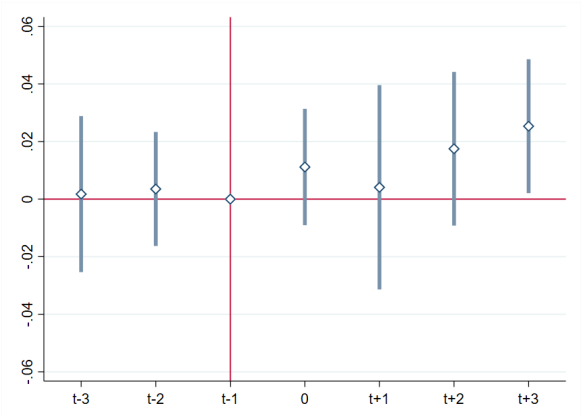



Figure 7: Event Study by Sector

(a) Manufacturing




(b) Services




[40] Using a sample of Swedish firms, Bandick (2010) finds that foreign-owned plants are actually less likely to survive than domestic plants. Ferragina et al. (2014) find similar results with Italian data.

Despite that reassurance, we next proceed with a number of further robustness checks designed to consider potential biases that could drive our results. In Table 6, we report the estimated shifter coefficients from these various alternatives where the six columns mirror those in Table 5.[41]

Our first two alternatives arise from recognizing that the estimated shifter coefficient compares firms currently shifting to all non-shifters. This raises the possibility that the treatment (shifter) and control (non-shifter) groups differ in some fashion not captured by the controls (including the constellation of fixed effects). This could introduce an endogeneity bias in our estimates. With this possibility in mind, we use two subsamples of the control group that may be arguably closer to the treatment group and thus mitigate such a bias. First, in Specification A of Table 6, rather than include all non-shifters in the sample, we use only those that, for at least one year, transfer at least 1 million NOK to havens.[42] The rationale behind this subsample is to compare shifters to non-shifters while restricting ourselves just to those firms that do significant business with tax havens.[43] As can be seen, we find a very similar pattern as when using all firms, namely significantly higher wages for services shifters (although the point estimates and significance levels are slightly lower than in Table 5). That this is true only when restricting attention to firms that engage with havens provides some reassurance that our results are not driven by an unobserved difference between firms that simply do business with havens – as opposed to specifically having an ownership link – and those that do not.

Second, Specification B only includes those firms that are MNEs at some point during the sample. As is well established, MNEs differ from non-MNEs in terms of size, productivity, and other factors (see Helpman et al. (2004), and the literature they inspired). While we already control for turnover, assets, and employment (and by virtue of their logged values, turnover per employee, a proxy for labor productivity), our specification would not capture non-linearities in these variables.[44] Thus, if MNEs, including shifters, differ in non-linearities or some other uncontrolled factor, again, the results may suffer from endogeneity bias. Nevertheless, as Table 6's Specification B reports, when using only MNEs for our control group, we again find higher wages for shifters in services industries. Thus, the data

[41] The full set of estimates for each specification are found in the Online Appendix.

[42] In results found in the Online Appendix, we instead include non-shifters with at least one year of positive transfers to tax havens. In comparison to these estimates, the coefficient on the shifter dummy for services was both more significant and slightly larger.

[43] Of the shifters, 82.3% sent over 1 million NOK to havens in a single year; 57.8% of them sent over 10 million NOK to havens in a single year.

[44] Although more refined measures of total factor productivity exist, they require detailed input data leading many studies to use turnover per employee as an alternative. See Pavcnik (2002) for a discussion. Bricongne et al. (2021) use French data and find that both value-added per worker and total factor productivity fall when a firm establishes a haven affiliate.

Table 6: Robustness Checks

| Dep. Variable: log(Wage) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Firms | All Firms | Manuf. | Manuf. | Services | Services |
| **Panel A: Haven Users (at least 1,000,000 NOK)** | | | | | | |
| Profit Shifter | 0.0050 | 0.0036 | –0.004 | –0.004 | 0.0174** | 0.0146** |
| | (0.0070) | (0.0064) | (0.009) | (0.009) | (0.0078) | (0.00730) |
| N | 2,620,676 | 2,620,676 | 890,975 | 890,975 | 1,729,701 | 1,729,701 |
| $R^2$ | 0.866 | 0.872 | 0.872 | 0.876 | 0.865 | 0.871 |
| **Panel B: Multinationals** | | | | | | |
| Profit Shifter | 0.0045 | 0.0032 | –0.005 | –0.005 | 0.0156** | 0.0133** |
| | (0.0070) | (0.0064) | (0.010) | (0.009) | (0.0072) | (0.0067) |
| N | 1,987,641 | 1,987,641 | 679,356 | 679,356 | 1,308,285 | 1,308,285 |
| $R^2$ | 0.869 | 0.874 | 0.871 | 0.874 | 0.870 | 0.874 |
| **Panel C: Profit Shifter in t-2** | | | | | | |
| Profit Shifter | 0.0087 | 0.0065 | –0.003 | –0.004 | 0.0224*** | 0.0188*** |
| | (0.0083) | (0.0074) | (0.011) | (0.010) | (0.0078) | (0.0072) |
| N | 4,208,121 | 4,184,526 | 1,031,931 | 1,028,802 | 3,172,910 | 3,155,724 |
| $R^2$ | 0.870 | 0.879 | 0.878 | 0.882 | 0.870 | 0.878 |
| **Panel D: Profit Shifter in t-3** | | | | | | |
| Profit Shifter | 0.0034 | 0.0015 | –0.008 | –0.008 | 0.0175** | 0.0145** |
| | (0.0112) | (0.0098) | (0.015) | (0.013) | (0.0079) | (0.0074) |
| N | 3,662,834 | 3,638,441 | 898,314 | 895,563 | 2,760,608 | 2,742,878 |
| $R^2$ | 0.877 | 0.884 | 0.884 | 0.888 | 0.877 | 0.883 |
| **Panel E: Profit Shifter in t-4** | | | | | | |
| Profit Shifter | 0.0045 | 0.0037 | –0.001 | –0.001 | 0.0134* | 0.0116 |
| | (0.0121) | (0.0105) | (0.016) | (0.015) | (0.0081) | (0.00733) |
| N | 3,128,682 | 3,106,908 | 767,458 | 763,754 | 2,357,917 | 2,343,154 |
| $R^2$ | 0.886 | 0.891 | 0.892 | 0.894 | 0.886 | 0.890 |
| **Panel F: No Board Members, Lawyers, Accountants** | | | | | | |
| Profit Shifter | 0.0071 | 0.0056 | –0.002 | –0.003 | 0.0180** | 0.0160** |
| | (0.0073) | (0.0067) | (0.011) | (0.010) | (0.0080) | (0.00745) |
| N | 3,851,998 | 3,849,586 | 982,360 | 982,034 | 2,869,281 | 2,867,552 |
| $R^2$ | 0.852 | 0.860 | 0.857 | 0.860 | 0.852 | 0.859 |
| Controls | YES | YES | YES | YES | YES | YES |
| Worker FE | YES | | YES | | YES | |
| Firm FE | YES | | YES | | YES | |
| Worker-firm FE | | YES | | YES | | YES |
| Industry-year FE | YES | YES | YES | YES | YES | YES |

Notes: Standard errors clustered at the firm-level in parentheses. All controls are lagged by one year. Manufacturing firms are defined as those in industries 10-33 using the Standard Industrial Classification 2007 (SIC 2007). */**/*** indicates significance at the 10/5/1% levels. Panel F excludes all board members, CEOs, managers, accountants and lawyers. Specifically, the work codes: Public authorized accountant (2511), Accountant (3432), Lawyers (2521), Economists and Ciliv Economists (2541), Senior Tax Lawyer (2412127).

suggest that our results do not arise from comparing shifters (a type of MNE) to non-MNEs but from an ownership linkage to tax havens as opposed to just any non-Norwegian ownership link.

In Figure 6, we saw that the significant coefficients for shifting came three years after shifting started. Although this delayed effect argues against endogeneity, it could still be present in the relatively contemporaneous shifter dummies. Additionally, one might worry about a confounding variable that is correlated with shifting that itself drives wages. With this in mind, rather than using the $t-1$ value of the shifter dummy as in the baseline results, we now replace it with its value in $t-2$ (Specification C), $t-3$ (Specification D), and $t-4$ (Specification E).[45] Similar to our baseline results, we find a significant coefficient on the shifter dummy, although with the longer lags it falls in terms of the point estimate and significance level.[46] This argues against a confounding variable since if this were still an issue, it would need to be reasonably stable over a four-year window and we would therefore expect the confounder's influence to be largely captured by the firm (or worker-firm) fixed effects.

Finally, one might worry over reverse causality driven by the notion that firms hire certain costly workers because they intend to profit shift. Indeed, a common interpretation of the transfer pricing costs in Allingham & Sandmo (1972), which inspired the profit shifting cost in our model, is that this is the added legal and accounting costs needed to profit shift (or deal with the legal ramifications should a firm be challenged by the tax authorities). If this were the case, one might expect that wages would rise in shifters before they establish their haven affiliate. Although Figure 6 argues against this, as a final robustness check in Specification F, we rerun the baseline estimation but omit all board members (including CEOs and other senior managers), accountants, lawyers, and economists, that is, those workers that one might believe are engaged in setting up and running a profit-shifting affiliate. Despite leaving out these workers, we find very similar results, further supporting the argument against reverse causality as the driver of our results.

## 5.3 Occupation Heterogeneity

To this point, we have compared the wages between shifters and non-shifters but assumed that the shifting wage premium is the same across all workers (at least within a sector). As laid out in the model of Section 3, this need not be true. In particular, those workers with greater per-capita bargaining power will see their wages rise more when the firm begins

[45] All other controls continue to be measured at their $t-1$ value.

[46] For each year of a longer lag, we lose just over 10% of our sample. This, combined with a slow-down in the growth of the profit shifting in the latter years of the sample, may contribute to this declining significance.

profit shifting. To the extent that those in high-skill occupations (and thus on the higher end of the income distribution) have greater influence spread across fewer individuals, we anticipate that they will have a higher shifting wage premium. With this in mind, we now look at two dimensions of worker heterogeneity: occupation and income.

In Table 7, we expand on the baseline specification by including dummy variables for seven occupation classes (with elementary occupations being the excluded eighth category), both on their own and interacted with the shifter dummy. For legibility, here we report only the interactions' coefficients, nevertheless all specifications include the same set of controls as before as well as occupation dummies.[47] In addition, we include a CEO dummy and its interaction.[48] Also, as before, the columns indicate which firms and fixed effects we use.

From this, two patterns emerge. First, we now find significant coefficients for the full sample as well as for manufacturing and services alone (although significance remains higher in services than in manufacturing). Second, we see that wage gains vary significantly across occupations with the higher-skill occupations having a higher shifting wage premium.[49] Specifically, elementary workers see no wage differences between shifters and non-shifters (as evidenced by the shifter dummy itself, which is only significant in column (6) and then barely so). Likewise, we observe no significant coefficient on the shifter interaction with the dummies for machine workers, those in sales, or clerical support, i.e., there is no difference between the shifting wage premium for these occupations and the insignificant wage premium for elementary workers.[50] This is not the case for the higher-skill occupations where we find significant interaction terms meaning that, unlike elementary workers, they have a significantly positive shifting wage premium. Focusing on column (2), we see that a manager employed by a given firm earns 2.49% more when that firm is profit shifting.[51] Professionals and technicians meanwhile earn 2.71% and 1.82% more. CEOs, in particular, have higher wages when working for a shifting firm with an estimated wage premium of 9.69% (the sum of the coefficients for the shifter dummy as well as its interactions with the CEO and manager variables). This difference, which mirrors Souillard (2020), is also strongly significant for both manufacturing and services with the point estimate higher in manufacturing than services (17.7% versus 7.68%).[52]

[47] The full tables are found in the Online Appendix.

[48] Note that for CEOs, both this and their other occupation dummy are equal to one. Roughly 80% of our CEOs are managers. Another 11.5% are professionals with a further 6.7% classified as technicians.

[49] This recalls Fuest et al. (2018) who find that high-skill workers benefit the most from domestic tax cuts.

[50] Trade workers, on the other hand, exhibit no robust pattern across columns.

[51] Again, this column includes worker-firm fixed effects so that this is 2.49% more than the *same* worker earned while working for the *same* firm before it established an ownership link to a tax haven. Note that the 2.49% estimate is the sum of the shifter dummy on its own and its interaction with the manager dummy.

[52] Again, these are the sum of the shifter coefficient and its interactions with the CEO and manager dummies, using the estimates in columns (4) and (6).

Although we do not report the results here, we find a comparable pattern when using just non-shifters with significant haven transactions (analogous to Table 6, Specification A), just MNEs (Specification B), and longer time lags on the shifter dummy (Specifications C-E).[53] Furthermore, in the Online Appendix, we present estimates where we fix a worker's occupation as their first observed one so that the time-series variation in the shifter-occupation interactions is driven solely by changes in shifting status. Once again, we find that the wage gains are found among the high-skill occupations. As a final robustness check found in the Online Appendix, we reestimated Table 7 for male and female workers separately. We do so in light of Fuest et al. (2018) and Carbonnier et al. (2022) who find that domestic tax reductions benefit men more than women. Suppose men are more likely to be found in the high-skill occupations (for example only about 10% of our CEOs are female). In that case, the occupation differences could result from confounding gender differences. Nevertheless, we found comparable results in both subsamples.

Taken as a whole, these occupation results suggest two aspects of the data. First, important differences across workers can be masked when looking just at averages as in Souillard (2020). Second, higher wages in shifting firms are primarily enjoyed by those in high-skill occupations. These granular results would then be consistent with our model if high-skill occupations have greater per-capita bargaining power.

Again, one might worry that these occupation-specific estimates are uncontrolled for differences between shifters and non-shifters. With this in mind, Figure 8 repeats our event study approach using subsamples differentiated by sector (manufacturing and services) and occupation skill level (high-skill occupations in Panel A, low-skill occupations in Panel B).[54] For the high-skill occupations, we find results comparable to Figure 6, i.e., no significant pretrend, higher point estimates post-treatment, and significant differences for services after a (now shorter) delay. While the low-skill occupations also exhibit no clear pre-trend, we do not find differences post-treatment, which is consistent with Table 7. Thus, the estimates suggest a causal effect of profit shifting on wages for high-skill occupations that is most apparent within services.

Although our estimates point to significant shifting wage premia for individual workers in high-skill occupations, here, we estimate a back-of-the-envelope calculation of what those may mean in the aggregate. To do so, we added up the wages paid in 2018 by shifters to their CEOs, managers, professionals, and technicians. Then for each group, we used our estimated

[53] These are available in the Online Appendix.

[54] Note that within a subsample, we revert to using a single shifter dummy across occupations as in Figure 6, so that Panel A has a shifter dummy for $t-3$ common across managers, professionals, and technicians, another for $t-2$, and so on. Figures not distinguishing between manufacturing and services in a single regression are in the Online Appendix.

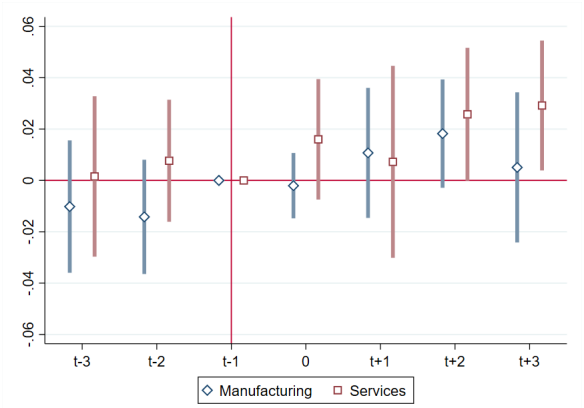
Table 7: Wages and Profit Shifting by Occupation, 2008-2018

| Dep. Variable: log(Wage) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | All Firms | All Firms | Manuf. | Manuf. | Services | Services |
| Profit Shifter | –0.00632 | –0.0164 | –0.00592 | –0.00958 | –0.0171 | –0.0252* |
| | (0.0135) | (0.0135) | (0.0185) | (0.0186) | (0.0160) | (0.0150) |
| x CEO | 0.0925*** | 0.0720*** | 0.156*** | 0.159*** | 0.0688*** | 0.0449*** |
| | (0.0157) | (0.0154) | (0.0376) | (0.0360) | (0.0173) | (0.0164) |
| x Managers | 0.0330** | 0.0413*** | 0.0272 | 0.0282 | 0.0525*** | 0.0571*** |
| | (0.0134) | (0.0137) | (0.0189) | (0.0190) | (0.0169) | (0.0160) |
| x Professionals | 0.0324** | 0.0435*** | 0.0292* | 0.0352* | 0.0477*** | 0.0543*** |
| | (0.0127) | (0.0131) | (0.0177) | (0.0181) | (0.0164) | (0.0155) |
| x Technicians/Assoc. Professionals | 0.0253** | 0.0346*** | 0.0208 | 0.0263* | 0.0397** | 0.0448*** |
| | (0.0117) | (0.0120) | (0.0153) | (0.0156) | (0.0160) | (0.0151) |
| x Clerical Support Workers | 0.000878 | 0.00966 | –0.00507 | –0.00041 | 0.0176 | 0.0223 |
| | (0.0110) | (0.0113) | (0.0121) | (0.0122) | (0.0164) | (0.0155) |
| x Service & Sales Workers | –0.00246 | 0.00374 | –0.00462 | 0.00472 | 0.0103 | 0.0119 |
| | (0.0125) | (0.0130) | (0.0118) | (0.0123) | (0.0170) | (0.0157) |
| x Craft & Related Trades Workers | –0.0182 | –0.0146 | –0.0356** | –0.0359** | 0.0297 | 0.0381** |
| | (0.0135) | (0.0140) | (0.0155) | (0.0166) | (0.0186) | (0.0189) |
| x Plant/Machine Operators & Assemblers | –0.0101 | –0.00431 | –0.0129 | –0.00958 | –0.0124 | –0.0161 |
| | (0.0114) | (0.0123) | (0.0125) | (0.0133) | (0.0288) | (0.0281) |
| Controls | YES | YES | YES | YES | YES | YES |
| N | 4,743,071 | 4,743,071 | 1,169,212 | 1,169,212 | 3,573,859 | 3,573,859 |
| $R^2$ | 0.866 | 0.875 | 0.874 | 0.878 | 0.866 | 0.874 |
| Worker FE | YES | | YES | | YES | |
| Firm FE | YES | | YES | | YES | |
| Worker-firm FE | | YES | | YES | | YES |
| Industry-year FE | YES | YES | YES | YES | YES | YES |

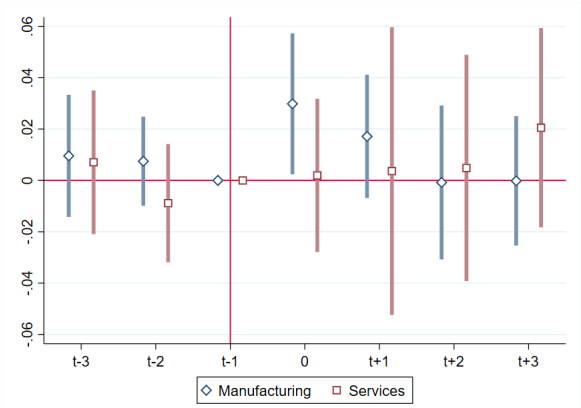
Notes: Standard errors clustered at the firm-level in parentheses. All explanatory variables are lagged by one year. Manufacturing firms are defined as those in industries 10-33 using the Standard Industrial Classification 2007 (SIC 2007). */**/*** indicates significance at the 10/5/1% levels.

Figure 8: Event Study by Occupation

(a) High-Skill




(b) Low-Skill



coefficients to calculate how much of these wages are associated with their employers' profit shifting.[55] This exercise suggests that an additional 449.2 million NOK were paid to those 18,508 workers, with 46.6 million NOK going to the 219 CEOs alone. Relative to the median wage in our sample for that year (517,794 NOK), this means that the average shifter wage premium for the high-skill occupations was around 4.4% of the median wage. On the other hand, CEOs had a shifter wage premium equal to 41.1% of the median wage. As a final point of interest, this 449.2 million NOK in additional wages would have generated an extra 273.1 million NOK in tax revenues for the Norwegian government.[56] This additional labor tax revenue would be equivalent to the corporate tax income on 1.24 billion NOK of profits (the Norwegian corporate tax rate is 22%). Given that the calculations of Tørsløv et al. (2018) suggest that 43.7 billion NOK were shifted from Norway in 2015, this would suggest that on the order of 2.8% of the lost corporate tax revenues would be offset by higher income taxes.

## 5.4 Income Heterogeneity

We conclude our analysis by examining how the shifting coefficient varies according to workers' income levels. Although correlated, ranking workers according to their income is not necessarily equivalent to grouping them according to their occupational status. As such, looking at how the impact varies with income level provides a more direct understanding of the potential role of profit shifting on income inequality both across firms as well as within them. If, as the occupation results suggest, shifting is most associated with higher wages among high-skill occupations, then shifting can exacerbate income inequalities even within a given firm. We therefore turn to unconditional quantile regressions using the methodology of Firpo et al. (2009). This estimation approach uses all the observations but allows the coefficients to vary according to where a given worker falls in the income distribution. These coefficients can be reported in two ways, either as a table for a set of specific quantiles (to easily compare the estimated coefficients for all the variables across quantiles) or as a figure that plots the estimated coefficient of a single variable across all quantiles. We use both here.

First, Table 8 presents the coefficients for select centiles of the income distribution using all firms (so that column (1) shows the estimates for workers in the tenth centile, those in column (2) for the 25th centile, and those in column (6) for the 95th centile).[57] Although

[55] Specifically, where $p$ is the estimated premium and $w$ is the actual wage paid, the difference is $\frac{p}{1+p}w$.

[56] This base would have incurred additional social security contributions, where the rate is 14.2%, and income taxes, where the top marginal rate is 46.6%.

[57] Thus, there are 100 quantiles. Here, we only present results with worker and firm fixed effects. This is

some controls have comparable coefficients for all workers (e.g., worker age or firm turnover), others are felt only by some workers (such as the positive wage effects associated with higher employment). Turning to the shifter variable, we find a pattern across income levels reminiscent of the occupation results, namely that only high-income workers earn higher wages when working for a shifter. Looking at, for example, the median worker (column (3)), we see that they earn the same regardless of whether they work for a shifter or not. In comparison, those in the 75th centile earn 1.77% more when working for a shifter, although this is only marginally significant. Those in the 90th centile or higher employed by a shifter, however, have significantly higher wages, on the order of 5-6% (roughly twice the wage premium found for managers in Table 7). Figure 9 provides an alternative take to these estimates by plotting the estimated shifter coefficient and its 95% confidence interval across the entire income distribution. Overall, there is a clear positive correlation, i.e., the shifter wage premium increases with income. Starting with the lowest-paid workers, we see a negative and marginally significant point estimate. This wage-reducing effect from profit shifting could arise if low-income workers' pay is only based on domestic, rather than global, profits as in the more complex rent-sharing model by Krautheim & Schmidt-Eisenlohr (2016). For workers from the tenth to 60th centiles, the relationship is fairly flat and insignificant, i.e. there is no marked difference from their peers in non-shifting firms, nor is there much difference across workers within a shifting firm. From that point on, however, there is a steady increase in the wage difference as a function of income level. In particular, workers above the 75th centile earn significantly more when they are employed by a shifter (although this significance fades among the highest paid workers, perhaps because of greater noise in individual pay for those top workers).

Figure 10 repeats this exercise but uses only firms in manufacturing (Panel A) or services (Panel B). In both, we again see the broad pattern that wage differences are fairly flat across income levels until one reaches the highest-paid workers. However, significant shifter wage premia are only found among services firms. There, the data show a clear premium for shifter employees, especially at the highest income levels. Thus, the unconditional quantile regressions confirm our above results, namely, that wage differences between shifters and non-shifters are most prominent in services and that the bulk of those higher wages are received by high-income workers employed in high-skill occupations. This then suggests that profit shifting has the potential to exacerbate income inequality both across firms for the average worker and within a firm.

because, due to insufficient within worker-firm variation in the profit shifter dummy for some centiles, we were not able to estimate coefficients for every centile. As a result, while the resulting plot of coefficients had a very similar shape, the unestimated coefficients made it needlessly confusing.

Table 8: Unconditional Quantile Regression: Wages and Profit Shifting Status, 2008-2018

| Dep. Variable: log(Wage) | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | $\tau = 0.10$ | $\tau = 0.25$ | $\tau = 0.50$ | $\tau = 0.75$ | $\tau = 0.90$ | $\tau = 0.95$ |
| Profit Shifter | –0.0126** | –0.00523 | –0.00184 | 0.0177* | 0.0523*** | 0.0602** |
| | (0.00582) | (0.00806) | (0.00604) | (0.00938) | (0.0161) | (0.0252) |
| log(Total Fixed Assets) | –0.00087 | 0.00080 | 0.00148* | 0.00123 | 0.00290 | 0.00361 |
| | (0.00086) | (0.00080) | (0.00085) | (0.00127) | (0.00205) | (0.00319) |
| log(Turnover) | 0.0129*** | 0.0218*** | 0.0335*** | 0.0455*** | 0.0585*** | 0.0695*** |
| | (0.00342) | (0.00277) | (0.00346) | (0.00580) | (0.00902) | (0.0115) |
| Bank Transfers/Turnover | 0.00010 | 0.00009 | 0.00014 | 0.00021 | 0.00027 | 0.00044 |
| | (0.00010) | (0.00012) | (0.00015) | (0.00019) | (0.00018) | (0.00042) |
| log(Employees) | 0.0188*** | 0.0186*** | 0.0152*** | 0.0123*** | 0.00683 | 0.00265 |
| | (0.00467) | (0.00422) | (0.00363) | (0.00444) | (0.00619) | (0.00762) |
| Share High-Skill | 0.00042 | 0.00253 | 0.00841** | 0.00101 | –0.00705 | –0.00489 |
| | (0.00400) | (0.00382) | (0.00409) | (0.00769) | (0.0153) | (0.0203) |
| Share Female | 0.00400 | –0.00434 | –0.00118 | 0.00826 | 0.0133 | 0.0189* |
| | (0.00822) | (0.00437) | (0.00452) | (0.00661) | (0.00862) | (0.0105) |
| Share Immigrant | 0.0224 | –0.00534 | –0.0114 | –0.0240** | –0.00718 | –0.00472 |
| | (0.0157) | (0.0102) | (0.00911) | (0.0110) | (0.0133) | (0.0174) |
| Share Unionized | 0.00173 | 0.00050 | –0.00081 | –0.00571 | 0.00037 | –0.00114 |
| | (0.00259) | (0.00233) | (0.00286) | (0.00380) | (0.00492) | (0.00641) |
| Exporter | –0.00035 | –0.00157 | –0.00211* | 0.00019 | 0.00152 | 0.00303 |
| | (0.00117) | (0.00109) | (0.00114) | (0.00161) | (0.00218) | (0.00274) |
| Importer | 0.00058 | 0.00103 | –0.00023 | –0.00077 | –0.00214 | –0.00131 |
| | (0.00151) | (0.00115) | (0.00112) | (0.00133) | (0.00191) | (0.00258) |
| MNE | –0.00442 | –0.00060 | 0.00032 | 0.00160 | –0.00385 | –0.00428 |
| | (0.00436) | (0.00369) | (0.00378) | (0.00389) | (0.00582) | (0.00710) |
| Age | 0.0242*** | 0.0247*** | 0.0245*** | 0.0180*** | 0.00569*** | –0.00034 |
| | (0.00056) | (0.00044) | (0.00054) | (0.00089) | (0.00083) | (0.00106) |
| $Age^2$ | –0.0003*** | –0.0003*** | –0.0003*** | –0.0002*** | –0.0001*** | –0.0000 |
| | (7.40e-06) | (5.94e-06) | (7.34e-06) | (0.00001) | (0.00001) | (0.00001) |
| Married | –0.00323*** | 0.00004 | 0.0103*** | 0.0198*** | 0.0122*** | 0.00787*** |
| | (0.00107) | (0.00087) | (0.00099) | (0.00161) | (0.00209) | (0.00277) |
| N | 4,743,071 | 4,743,071 | 4,743,071 | 4,743,071 | 4,743,071 | 4,743,071 |
| $R^2$ | 0.533 | 0.665 | 0.740 | 0.762 | 0.740 | 0.709 |
| Worker FE | YES | YES | YES | YES | YES | YES |
| Firm FE | YES | YES | YES | YES | YES | YES |
| Industry-year FE | YES | YES | YES | YES | YES | YES |

Notes: Standard errors clustered at the firm-level in parentheses. All explanatory variables are lagged by one year. */**/*** indicates significance at the 10/5/1% levels.

Figure 9: Unconditional Quantile Estimation: All Firms

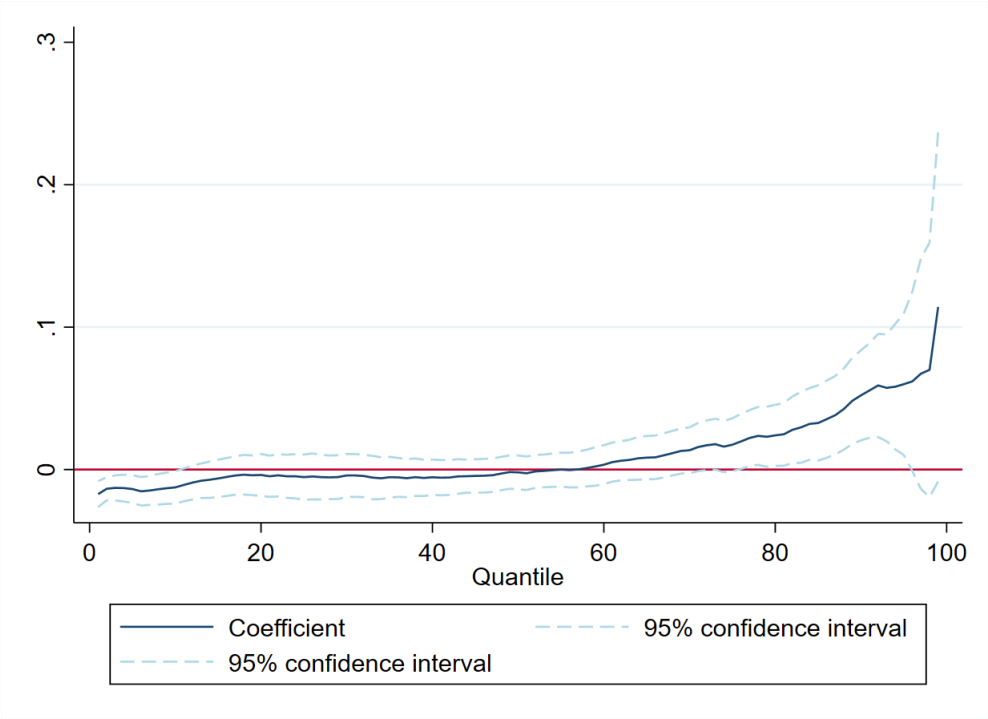
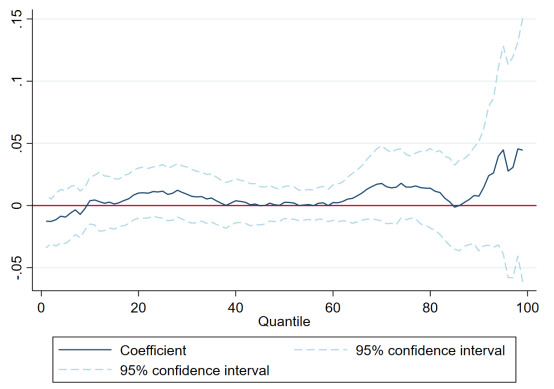
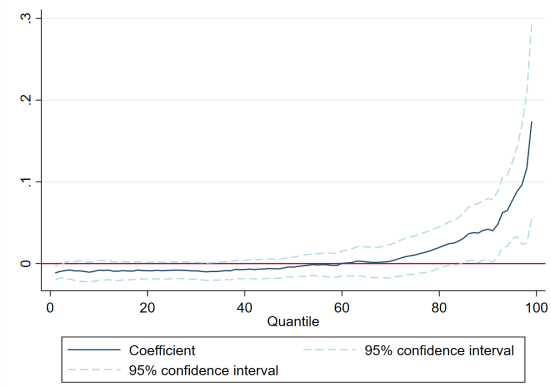



Figure 10: Unconditional Quantile Estimation by Industries

(a) Manufacturing




(b) Services



# 6 Conclusion

The debate on whether multinationals shift profit to tax havens has been settled with the key methods for doing so identified. One consequence of these efforts is a fall in government corporate tax revenues in high tax countries, a decline on the order of 15-20% according to Tørsløv et al. (2018). We contribute to the debate by identifying a second consequence, namely changes in workers' wages employed by profit-shifting firms. Using detailed data from Norway, we provide the first granular analysis of the relationship between profit shifting and wages. Mirroring other tax-related windfalls, we find that establishing a link to a tax haven via foreign direct investment increases the wages of some workers. In particular, we find wage gains among those in high-skill, high-income occupations, especially when they work in services. CEOs particularly gain from profit shifting with their wages rising nearly 10%. Although these wage increases do raise additional income taxes, our back-of-the-envelope calculations suggest that they only offset around 3% of Norway's lost corporate tax revenues. Thus, our results suggest that in addition to lowering government tax revenues, shifting profits to tax havens can affect wage inequality, both across and within firms.

As a final point, in this paper, we consider only the wage implications of profit shifting. It is reasonable to anticipate that if profit shifting leads to higher after-tax profits, this may increase non-wage remuneration (e.g., via stock options). As these benefits are largely found among the high-skill occupations, we are likely underestimating the impact of profit shifting on total income inequality. In addition, increases in stock valuations would benefit non-employee (and arguably high-income) shareholders. These additional effects of tax avoidance may well exacerbate the general public's frustration about the ability of some multinationals to avoid taxation, not just at home but anywhere. Therefore, we believe it is important to look beyond changes in tax receipts to gauge the full impact of profit shifting on societies.

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