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DEDICATION

To my dearest Stefania for being always on my side, in the sunny days and especially in the stormy ones. To my grandmother, Lucia, for her inspiring juvenile energy. To my mother, Bruna, for teaching us the virtues of perseverance, and my father, Giovanni, for teaching us to trust our heart more than anything else. To my sisters, Caterina, Lucia and Maria Nives, for their constant support.

EPIGRAPH

On the mountains of truth you can never climb in vain: either you will reach a point higher up today, or you will be training your powers so that you will be able to climb higher tomorrow.

—Friedrich Nietzsche

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ABSTRACT OF THE DISSERTATION

Essays in labor and public economics

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This dissertation explores three topics in labor and public economics. Chapter 1 studies how coordination of hours among coworkers affects labor supply decisions and wage rates. Using rich data from Denmark we find that greater coordination of hours within firms is associated with higher wages, attenuated response to tax rate changes and spillover effects on hours worked by workers who are not directly affected by a tax change. Chapter 2 estimates the short-term effects of migration on employment of native workers in Italy using the exogenous, unanticipated and temporary migration resulting from the Arab Spring. I find significant and offsetting short-term effects across industries. The positive employment effects are consistent with a rise in sectoral employment operating through increased demand from immigrants. Both positive and negative effects on employment tend to dissipate over time. Chapter 3 uses rich data from Brazil to show evidence that exporters prepare to export by hiring workers from other exporters. We also show that poaching workers from other exporters is a strong predictor of various aspects of export-market success at the poaching firm.

Chapter 1

Coordination of Hours within the Firm

Abstract

Teamwork has become increasingly important in many firms, yet little is known about how coordination of hours among heterogeneous coworkers affects pay, productivity and labor supply. In this paper we propose a framework where differently productive firms choose whether or not to coordinate hours in exchange for productivity gains. In this framework, we show that more productive firms select into coordinating hours and pay compensating wage differentials, leading to attenuated labor supply responses and spillovers from tax changes. Next, we bring the model predictions to the data using linked employer-employee registers in Denmark. We first document evidence of positive correlations between wages,

productivity and the degree of hours coordination - measured as the dispersion of hours - within firms. We estimate that hours coordination can explain around 4% of the variance of firm-level wages. We then estimate labor supply elasticities using changes to the personal income tax schedule in 2010 which affected high-wage earners differently. We find evidence of higher labor supply elasticity in firms with lower hours coordination. Furthermore, we find evidence of spillover effects on hours worked by coworkers not directly affected by the reform that are consistent with our model of firm level coordination of hours.

1.1 Introduction

Over the past few decades firms have become more collaborative, with coworkers spending a greater share of their working time interacting with each other (Delarue, Van-Hootegem, Procter, and Burridg 2008; Cross and Gray 2013). One key aspect of cooperation within firms is that it necessitates some degree of coordination of hours. Specifically, a greater need for interaction may require that coworkers work a more similar number of hours, despite possibly different labor supply preferences. While existing studies suggest that greater cooperation is associated with improved worker productivity (e.g. Hamilton, Nickerson, and Owan 2003; Chan 2016), to date little is known about how hours coordination affects worker behavior or firm performance.

A better understanding of hours coordination however, is important for at least two reasons. First, hours coordination is an unexplored dimension along which firms differ that may help explain the observed link between productivity and wages in a firm.[1] Second, coordination can serve as a mechanism that amplifies or attenuates the effects of policies that affect labor supply. In the specific case of tax reforms, this could provide an explanation for the low elasticity of labor supply to tax changes found in several other studies (e.g. Chetty 2012).

In this paper we first document the features of coordinated firms. We propose a novel measure of hours coordination and show how this correlates with other characteristics of the firm. Importantly, we find that coordination positively correlates with firm productivity and predicts wage differentials across firms. Next, we explore how coordination can distort the effects of a policy change. We examine the effects on labor supply of a Danish tax reform that predominantly affected high income workers, who, arguably, have a different desired number of work hours than low income workers. In low-coordination firms, we find sizable labor supply responses, while in high-coordination firms we estimate insignificant labor supply elasticities for high income workers. Furthermore, we find labor supply spillovers on low income workers who were not directly targeted by the tax reform.

We conceptualize the link between firm profitability, coordination of hours, wages and labor supply elasticities in a framework where differently productive firms employ workers with heterogeneous desired work hours. In this framework, firms

[1] It is well documented that productivity and wage differentials across firms strongly correlate (see Card, Cardoso, Heining, and Kline 2016 for a summary of these studies).

can choose whether to coordinate hours or not. Coordination enhances productivity but requires hours worked to be the same across heterogeneous coworkers. We derive three main predictions. (1) More productive firms coordinate hours and pay compensating wage differentials for imposing sub-optimal hours. (2) Coordination attenuates the labor supply responses of workers targeted by a tax change. (3) In coordinated firms a tax change that affects one type of workers has spillovers on hours worked by other coworkers.

We test these predictions using linked employer-employee registers of the Danish population. Denmark is a particularly fitting setting for our study. In fact, in 2010 the government mandated a personal income tax reform that substantially lowered the marginal tax rates on high incomes while leaving almost unchanged the marginal tax rates of low income workers. Additionally, the Danish data allow us to link number of hours worked to individual and firm characteristics. Furthermore, compared to other European countries, Denmark has a relatively flexible labor market where employers have considerable discretion in setting wages and hours (Botero, Simeon, Porta, Silanes, and Shleifer 2004; Hummels, Rasmus, Jakob, and Chong 2014).

We measure coordination using the standard deviation of average hours worked across skill groups in a firm. In doing so we assume that workers in different skill groups have different labor supply preferences, and that a lower dispersion of hours implies a greater overlap of workers at the workplace. Therefore low dispersion is interpreted as high-coordination.[2] Validation exercises performed using alternative measures of coordination from O*NET, the Survey of Adult Skills, and the Danish Time Use Survey support this interpretation. A descriptive analysis based on our coordination measure reveals that more coordinated firms are more productive, employ better able workers, are less likely to employ part-time or hourly workers, require a more intense use of social skills (Deming 2015), and are more likely to be in the service sector.

With our measure of coordination in hand, we first explore how the degree of coordination at a firm relates to the wage premium paid to workers. We estimate the premium as the firm fixed effect from a regression of hourly wages on individual, firm fixed effects and time varying characteristics (Abowd, Kramarz, and Margolis 1999). Then we regress this premium on our measure of coordination. In line with the model, we find a strong and positive association between the firm component of wages and coordination of hours across and within sectors. This correlation is robust to a number of firm characteristics that are known to affect wage inequality across firms.[3] Conditional on other characteristics, we estimate that a one standard deviation increase in coordination is associated with a 0.5% increase in wages. In

[2]Ideally we would measure coordination based on the degree to which coworkers with different labor supply preferences work at the same time of the day or interact with each other. Unfortunately data of this type do not exist on a such large scale. We focus on full-time workers because Danish Time Use Survey data reveal that part-timers are more likely to start working later during the day or to work over weekends.

[3]For instance, we control for firm size (Mueller, Ouimet, and Simintzi 2015), exporter status (e.g. Helpman, Itskhoki, Muendler, and Redding 2016), the skills and gender composition of the workforce (Card, Cardoso, and Kline 2016, Song, Price, Guvenen, Bloom, and Watcher 2016), average number of hours, unionization rate (e.g. Dickens 1986), overtime premiums (Cardoso, Hamermesh, and Varejo 2012).

the same specification, exporter status has a similar predictive power while firm size is not as predictive as coordination.

After controlling for measures of firm productivity the correlation between wages and coordination is insignificant. This suggests that only highly productive firms can afford to pay higher wages to achieve greater coordination. Specifically, we estimate that coordination can explain around 4% of the wage inequality due to productivity across firms within 3-digit industries. While descriptive, these findings suggest that a relevant part of the documented correlation between the firm-component of wages and productivity may reflect wage differentials for greater coordination in more productive firms.

In the second part of the paper we analyze the effects of a tax reform which abolished the middle bracket of a 3-bracket progressive tax schedule and lowered the top tax rates. This resulted in a sizeable reduction of the marginal tax rates of workers who used to be in the top and middle tax bracket prior to the reform (high-skilled).

To identify the attenuating effects of coordination we estimate the elasticity of hours worked by high-skilled workers in high versus low-coordination firms. In doing so, we use the tax reform as an instrument for the observed changes in after-tax wages (Gruber and Saez 2002). In line with the model predictions, we find an elasticity close to zero and insignificant in high-coordination firms, and a negative and significant elasticity of -0.1 in low-coordination firms.

Next, we test the existence of labor supply spillovers estimating the elasticity of hours worked by low-skilled workers to the tax-driven change of average hours worked by high-skilled coworkers. We find an elasticity of 0.88 that implies an increase of 0.85 hours worked by low-skilled for each additional hour provided by high-skilled coworkers. Consistent with our framework we find a lower elasticity among workers in low-coordination firms. Importantly, the effects of coordination that we document do not reflect other time invariant firm characteristics, and are based off workers who stayed at the same employer throughout the reform.

Our findings of attenuating and spillover effects have a variety of implications. First, the elasticity of labor supply captures only a part of the efficiency costs associated with a tax change (Feldstein 1999) since it neglects the indirect effects on untargeted coworkers. Including spillovers we estimate an increase of 15% in the marginal excess burden from the 2010 Danish tax reform. Second, due to hours coordination, using workers who are not directly targeted by a tax change as a control group can produce downward biased estimates of the labor supply elasticity (e.g. Eissa 1995; Blundell, Duncan, and Meghir 1998). Finally, the effects of coordination are not only relevant to the evaluation of tax policies. They also apply to any policy that affects the preference over hours of one group of workers in a firm, such as parents or old workers.[4]

This study relates to multiple strands of the literature. First, we speak

[4] In this sense our research supports the findings of a growing body of literature that emphasizes the importance of employer-employees interactions in shaping workers' responses to policy changes. For instance, Choi, Laibson, Madrian, and Metrick 2004; Gelber 2011; Chetty, Friedman, Leth-Petersen, Nielsen, and Olsen 2014; Fadlon, Laird, and Nielsen 2016 document the importance of employers in determining employees' contribution to retirement plans.

to the set of studies that analyze the effects of taxation when employers impose constraints on hours (Chetty, Friedman, Olsen, and Pistaferri 2011; Best 2014; Battisti, Michaels, and Park 2015). Some of these studies show evidence of bunching of workers who do not directly face tax schedule kinks that is consistent with our finding of labor supply spillovers. Using newly available data on hours and the quasi-experimental variation deriving from a tax reform, we provide firm-level evidence of a mechanism - coordination of hours - through which preferences over hours spill over to other coworkers.[5]

Second, we contribute to the extensive literature on wage and productivity differentials across firms (e.g. Syverson 2011; Card, Cardoso, Heining, and Kline 2016). Specifically, we offer a look inside firms by modeling, and empirically quantifying, the importance of coordination of hours as a rationale that leads more productive firms to pay higher wages. In this respect, our results document a specific mechanism that can explain recent findings suggesting that compensating differentials are an important source of wage inequality across firms (Sorkin 2015; Lavetti and Schmutte 2016).[6] Relative to the literature on compensating differentials

[5]Battisti, Michaels, and Park 2015 present evidence of reduced intertemporal elasticities from structural simulations of a policy that only affects a fraction of the firm workforce. This evidence is consistent with the attenuating effects of coordination on steady-state elasticities that we document. However, we are able to measure coordination using firm-level data on hours and base our evidence on a real preference shock deriving from a tax reform. Our results also help to shed light on existing evidence at more aggregate levels. Kahn and Lang 1991 finds the elasticity of actual hours to be lower than the elasticity of desired hours. Our findings suggest that such difference may be linked to firm-level coordination. Hamermesh, Myers, and Pocock 2008 documents synchronization of working schedules across US states. Our results indicate that coordination among coworkers is associated to co-movement of hours.

[6]While we can not exclude the possibility that wage premiums partially reflect rent sharing, drawing on a correction exercise in the spirit of Lavetti and Schmutte 2016 would suggest that, in that case, our estimates of compensating differentials due to coordination would be a lower bound

from less desirable hours, our results emphasize the importance of looking at the dispersion of hours in a firm as a way to measure dis-amenities from lower flexibility at the workplace (e.g. Rosen 1986; Abowd and Ashenfelter 1981; Hamermesh 1999; Goldin and Katz 2017; Card, Cardoso, and Kline 2016; Mas and Pallais 2016).

Finally, our study complements a recent literature that highlights the positive correlation between social skills and wages (Heckman and Kautz 2012; Deming 2015). We document, in fact, that workers in highly coordinated firms make more intense use of social skills. Compensating differentials from coordination can therefore be viewed as a channel through which higher wages are associated with social skills.[7]

The remainder of the paper is organized as follows. Section 1.2 presents the conceptual framework, Section 1.3 describes the data and the institutional setting. Section 1.4 presents the empirical relation between coordination, wages and firm productivity. Section 1.5 quantifies the effects of coordination on the elasticity of labor supply. Finally, Section 1.6 concludes.

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for the actual compensating differentials. Siow 1987 found higher wages in industry-occupations with less volatile hours. Our research moves to the linked employer-employee level. This allows us to measure the dispersion of hours between coworkers and examine how this relates to wage inequality across firms.

[7]In this respect our empirical findings support the theoretical work that links synchronization of working schedules to the potential for better communication and cooperation (Lewis 1969; Weiss 1996).

1.2 Conceptual framework

Underlying the standard labor supply model is the assumption that employers are indifferent to the hours supplied by their employees. Hours worked however vary across sectors and, most notably, across firms within a sector. Figure 1.1 shows the distribution of weekly hours worked across six major sectors in Denmark. The distribution is considerably more concentrated in the service sector than in agriculture, manufacturing or construction, even though the latter sectors are more unionized than services.

The variation in the hours worked between sectors, however, accounts only for a small part of the overall variation in hours. A decomposition of the variance of total annual hours worked in Denmark into between and within sector variability first, and then into cross and within firm variability shows that cross-firm variation explains more than 35% of the overall variance, whereas merely 4% of the overall variation occurs between 1-digit sectors (Figure 1.2).[8] This descriptive evidence suggests that employers may indeed affect their workers supply of hours. Motivated

[8] The variance of hours is decomposed into between and within group components as follows:

$$\frac{1}{N_t}\sum_i \left(h_{it}-\overline{h_t}\right) = \frac{1}{N_t}\sum_g \sum_{i\in g}\left(h_{it}-\overline{h_{gt}}\right) + \frac{1}{N_t}\sum_g N_{gt}\left(\overline{h_{gt}}-\overline{h_t}\right)$$

Where workers are indexed by i and years by t, g denotes groups (i.e. firms or sectors) while N_{gt} and N_t denote respectively the number of groups and the number of workers. h_{it}, $\overline{h_{gt}}$ and $\overline{h_t}$ are respectively the worker hours, the average hours within each group and the average hours across all workers. The variance is decomposed in each year between 2003 and 2008. Figure 1.2 shows average shares across all years. To the extent that hours are measured with errors the within firms component of the variance may be overestimated which means that hours between firms may vary even more than our measure shows.

by this evidence, in this section we propose a model where firms endogenously choose whether to restrict the range of hours available to their employees. Then we examine how this affects wages and labor supply elasticities.

1.2.1 Workers

There are two types i of workers, N_H workers with high skill ($i = H$) and N_L workers with low skill ($i = L$). Workers have preferences over a continuum of consumption goods $\omega \in \Omega$ and leisure ℓ_i of the following type (Dixit and Stiglitz 1977; Prescott 2004):

$$U\left(Q_i, \ell_i\right) = \log\left[\int_{\omega\in\Omega} q(\omega)^{\frac{\sigma-1}{\sigma}}\, d\omega\right]^{\frac{\sigma}{\sigma-1}} + \eta\, v\left(\ell_i\right), \tag{1.1}$$

where $(Q_i)^{(\sigma-1)/\sigma} \equiv \int_{\omega\in\boldsymbol{\Omega}} q_i(\omega)^{(\sigma-1)/\sigma} \mathrm{d}\omega$ is the (exponentiated) consumption index for a worker of skill i and $\sigma > 1$ is the elasticity of substitution between any two goods. We assume that the utility of leisure function $v(\ell_i)$ is increasing and concave with $v'(\ell_i) > 0$ and $v''(\ell_i) < 0$.

Workers can take employment either in the non-coordinated or in the coordinated labor market. In the non-coordinated labor market, workers face equilibrium wages w_i^* and pick their optimal hours $h_i^* = 1 - \ell_i^*$, allowing for an optimal consumption level Q_i^* with individual product demand $q_i^*(\omega)$, and resulting in a utility level $U_i^* \equiv U(Q_i^*, h_i^*)$ (see Appendix 1.9.1).

In contrast, workers employed in the coordinated labor market must work for a prescribed number of hours $\hat{h}$ regardless of their skill level. In the coordinated market, firms offer skill-specific hourly wages $\hat{\mathrm{w}}_H$ and $\hat{\mathrm{w}}_L$ that are discussed in the next subsection. Workers in this segment consume $\hat{Q}_i$ and $\hat{q}_i(\omega)$, resulting in utility $\hat{U}_i \equiv U(\hat{Q}_i, \hat{h}_i)$.

Workers face a skill-specific tax rate t_i that generates tax revenues distributed through a lump-sum transfer T that balances the government's budget. The overall labor market for each skill group clears so that $N_i^* + \hat{N}_i = N_i$ for equilibrium wages w_i^* and $\hat{\mathrm{w}}_i$.

1.2.2 The wage-hour function

We assume perfect worker mobility between firms in the non-coordinated and coordinated segments of the labor market. An implication is that, in equilibrium, a coordinated labor market can only co-exist with the non-coordinated labor market if workers are indifferent between employment in either market segment. The indifference condition for each type i worker between coordinated and non-coordinated labor market segments is:

$$U\left(\frac{\hat{\mathrm{w}}_{\mathrm{i}}}{P}\,\hat{h}\,(1-t_i) + \frac{T+\bar{\pi}}{P},\,\hat{h}\right) = U\left(\frac{\mathrm{w}_{\mathrm{i}}^*}{P}\,h_i^*\,(1-t_i) + \frac{T+\bar{\pi}}{P}, h_i^*\right), \tag{1.2}$$

where $P^{\sigma-1} \equiv \int_{\omega\in\boldsymbol{\Omega}} p(\omega)^{-(\sigma-1)}\,\mathrm{d}\omega$ is the (exponentiated) price index, and $\bar{\pi} \equiv \int_{\omega\in\boldsymbol{\Omega}} \pi(\omega)\mathrm{d}\omega/(N_H+N_L)$ represents the equal distribution of firm profits as dividends. This condition implicitly defines the wage rates $\hat{\mathrm{w}}_i$ for each type i worker as a function of the hours worked $\hat{h}$. To illustrate this, in Figure 1.3 we assume that $\hat{h} > h_i^*$. For the sake of clarity in the figure we ignore T and $\bar{\pi}$ and assume $t_i = 0$, $P = 1$. Figure 1.3 shows that the wage rate $\hat{\mathrm{w}}_{\mathrm{i}}$ that makes the worker indifferent between working h_i^* at the rate $\mathrm{w_i}^*$ and working $\hat{h}$ is greater than the equilibrium wage $\mathrm{w_i}^*$. Since this applies to any hours choice $\hat{h} \neq h_i^*$, condition (1.2) defines a function $\hat{\mathrm{w}}_i(\hat{h})$, that has $\mathrm{w_i}^*$ as parameter, and that we refer to as the *wage-hour function*.

Regarding the properties of this function, under standard regularity conditions on the shape of the utility function, it can be shown that $\hat{\mathrm{w}}_i'(\hat{h}) < 0$ if $\hat{h} < h_i^*$. In this case a marginal increase in $\hat{h}$ shortens the distance between $\hat{h}$ and h_i^* thus requiring a lower extra compensation to make the worker indifferent between working $\hat{h}$ and working h_i^*. Similarly, $\hat{\mathrm{w}}_i'(\hat{h}) > 0$ if $\hat{h} > h_i^*$, whereas if $\hat{h} = h_i^*$ no extra compensation is needed and thus $\hat{\mathrm{w}}_i'(\hat{h}) = 0$. Additionally, it can be shown that $\hat{\mathrm{w}}''(\hat{h}) > 0$ (Appendix 1.9.2).[9] Therefore, the resulting wage-hour function is U-shaped with minimum at the equilibrium wage w_i^* where hours $\hat{h} = h_i^*$.

The economic insight behind this function is that firms in the coordinated market need to offer higher wages to both skill groups when the coordinated hours differ from optimal hours.[10]

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[9] As we show in Appendix 1.9.2 there are condition on the curvature of the leisure preferences or economy-wide productivity that ensure $\hat{\mathrm{w}}''(\hat{h})$ to be positive.

[10] In presence of search frictions, coordinated firms would still pay higher wages compared to their non-coordinated peers as long as search costs do not exceed the utility losses from accepting standardized hours $\hat{h}$

1.2.3 Firms

There is a continuum of firms, each producing a different variety ω of consumption goods under monopolistic competition. Every firm produces with a constant-returns-to-scale technology $q(\omega) = \gamma\,\phi\,G(n_H h_H, n_L h_L)$, where ϕ is a productivity parameter that differs from firm to firm under some probability distribution (similar to Melitz 2003), γ is a Hicks neutral productivity shifter that varies with hours coordination and $G(\cdot,\cdot)$ is the production function. The firm employs n_H high-skilled and n_L low-skilled workers. In what follows we denote with $G_H(\cdot,\cdot)$ the first derivative of $G(\cdot,\cdot)$ with respect to its argument $(n_H h_H)$, and with $G_L(\cdot,\cdot)$ the first derivative with respect to $(n_L h_L)$. For simplicity, we do not allow for market entry (Chaney 2008). However, firms can choose whether to operate in the non-coordinated or in the coordinated labor market. In the non-coordinated labor market $\gamma = 1$ so that firms produce with productivity ϕ. In the coordinated labor market $\gamma = \hat{\gamma} > 1$ so that firms can raise their productivity to $\hat{\gamma}\phi$ but must pay a fixed cost $\hat{F}$ to achieve hours coordination.[11]

Non-coordinated labor market

In the non-coordinated labor market, firms take equilibrium wages $\mathrm{w}_{\mathrm{i}}^*$ and workers' preferred hours h_i^* as given. Thus they choose the number of high and

[11]The fixed costs of coordination can be thought of as the infrastructure needed to sustain coordinated production such as office space, conference rooms, scheduling software, and the like.

low-skilled workers that minimizes costs:

$$C^*(\omega) \equiv \min_{n_H,n_L} \mathrm{w}_\mathrm{H}^* n_H h_H^* + \mathrm{w}_\mathrm{L}^* n_L h_L^* \qquad \text{s.t.} \quad G\left(n_H h_H^*, n_L h_L^*\right) \geq q^*(\omega)/\phi. \quad (1.3)$$

The first-order conditions imply that

$$\frac{G_H(n_H^* h_H^*, n_L^* h_L^*)}{G_L(n_H^* h_H^*, n_L^* h_L^*)} = \frac{\mathrm{w}_\mathrm{H}^*}{\mathrm{w}_\mathrm{L}^*}.$$

As by convention, we assume $G_H(\cdot,\cdot) > G_L(\cdot,\cdot)$ so that $\mathrm{w}_\mathrm{H}^* > \mathrm{w}_\mathrm{L}^*$ and $h_L^* \neq h_H^*$, with $h_L^* < h_H^*$ if the substitution effect prevails and the opposite if the income effect prevails.

Coordinated labor market

Firms in the coordinated labor market offer contracts for a single number of hours $\hat{h}$ that workers of all skill levels must accept, but offer skill-specific wages along the wage-hours function $\hat{\mathrm{w}}_i(\hat{h})$ so that each type i worker is indifferent between employment in the coordinated or non-coordinated labor market. This results in the following cost minimization problem:

$$\begin{aligned} \hat{C}(\omega) \equiv \min_{n_H,n_L,h} \hat{\mathrm{w}}_\mathrm{H}\, n_H\, h + \hat{\mathrm{w}}_\mathrm{L}\, n_L\, h \qquad & \text{s.t.} \quad h\, G(n_H, n_L) \geq q^*(\omega)/(\hat{\gamma}\phi), \\ & U\left(h \frac{\hat{\mathrm{w}}_i}{P}(1-t_i) + \frac{T+\bar{\pi}}{P}, h\right) = U(Q_i^*, h_i^*) \\ & \text{for } i = H, L. \end{aligned}$$

From which the first-order condition that implicitly defines $\hat{h}$ is (see Appendix 1.9.3):

$$\hat{n}_H \, \hat{\mathrm{w}}'_{\mathrm{H}}(\hat{h}) = -\hat{n}_L \, \hat{\mathrm{w}}'_{\mathrm{L}}(\hat{h}). \tag{1.4}$$

Condition (1.4) has several implications. First, it implies that optimal hours $\hat{h}$ are in between h^*_L and h^*_H. In fact, since $h^*_H \neq h^*_L$, $\hat{h}$ cannot be equal to either h^*_L or h^*_H. Furthermore, if $\hat{h}$ is greater than h^*_L and h^*_H then $\hat{\mathrm{w}}'_H > 0$ and $\hat{\mathrm{w}}'_L > 0$ and thus (1.4) cannot be satisfied. For a similar reason, $\hat{h}$ cannot be smaller than h^*_L and h^*_H to satisfy (1.4). Second, (1.4) establishes that optimal hours are such that marginal costs of increasing hours in coordinated firms equal marginal benefits. To understand this let us consider the case in which high-skilled desire to work more than low-skilled workers ($h^*_H > h^*_L$). For any choice of coordinated hours $h^*_L < \hat{h} < h^*_H$ a marginal increase in $\hat{h}$ moves them closer to h^*_H. Therefore, it results in lower wage premiums paid to high-skilled and thus in wage bill savings in the amount of $\hat{n}_H \, \hat{\mathrm{w}}'_{\mathrm{H}}$. However, the same increase in hours moves $\hat{h}$ further away from h^*_L. Thus it results in higher wages paid to low-skilled workers and therefore in a higher wage bill in the amount of $\hat{n}_L \, \hat{\mathrm{w}}'_{\mathrm{L}}$. At the optimum savings from marginally higher hours equal costs. Finally, (1.4) implies that $\hat{h}$ is set to be closer to the desired hours of the larger group of workers in the firm.[12]

Based on (1.4), both high and low-skilled workers in coordinated firms work suboptimal hours and therefore are compensated with wage premiums. It follows

[12] A greater $\hat{n}_i$ in (1.4), raises the marginal costs of increasing $\hat{h}$ if $\hat{h} > h^*_i$ or decreases the marginal benefits of increasing $\hat{h}$ if $\hat{h} < h^*_i$. This implies that $\hat{h}$ moves closer to h^*_i as $\hat{n}_i$ goes up.

that:

Prediction 1 *Firms that coordinate work-time to a common number of hours for both skill groups pay higher hourly wages than non-coordinated firms, which take the supply of work hours as given.*

Endogenous market segmentation

We now establish the conditions for the existence of the coordinated labor-market segment in equilibrium. A firm producing variety ω maximizes its profits by setting the variety-specific price $p(\omega)$ given total demand. Maximized profits in the two segments are (Appendix 1.9.4):

$$\begin{aligned}\pi^*(\phi) &= \left(\frac{\sigma-1}{\sigma}\right)^{\sigma-1}\left(\frac{P}{\mu^*}\right)^{\sigma-1}\frac{E}{\sigma}\,\phi^{\sigma-1},\\ \hat{\pi}(\phi) &= \left(\frac{\sigma-1}{\sigma}\right)^{\sigma-1}\left(\frac{\hat{\gamma}P}{\hat{\mu}}\right)^{\sigma-1}\frac{E}{\sigma}\,\phi^{\sigma-1}-\hat{F},\end{aligned}$$

where $E = PQ$ are economy-wide expenditures, and μ^*, $\hat{\mu}$ are respectively minimized marginal production costs in the uncoordinated and coordinated segment. Based on this, a firm with productivity ϕ will choose to enter the coordinated labor market if and only if

$$\hat{\pi}(\phi) > \pi^*(\phi).$$

If $\hat{\gamma} > \hat{\mu}/\mu^*$, this inequality can be rewritten in terms of a firm's productivity ϕ

$$\phi > \frac{\sigma}{\sigma - 1} \frac{\hat{F}^{1/(\sigma-1)}}{E^{1/(\sigma-1)} P} \frac{\hat{\mu}}{\hat{\gamma} - \hat{\mu}/\mu^*} \equiv \hat{\phi}, \tag{1.5}$$

where $\hat{\phi}$ is the productivity threshold above which firms select into the coordinated segment. Intuitively, the higher the fixed cost $\hat{F}$ of coordinating or the higher the marginal cost $\hat{\mu}$ of producing in the coordinated market, the more elevated the entry threshold would be. Conversely, a less competitive market with a high overall price level P and a larger aggregate economy with higher E facilitates entry and therefore reduces the entry threshold. The inequality would be reversed if $\hat{\gamma} < \hat{\mu}/\mu^*$ and a coordinated labor market would not exist. Therefore we can state:

Prediction 2 *If a firm's productivity premium resulting from coordinating work hours is sufficiently large, $\hat{\gamma} > \hat{\mu}/\mu^*$, a coordinated labor market co-exists with a non-coordinated labor market. Firms with productivity above a unique threshold $\hat{\phi}$ coordinate work time, whereas firms with productivity weakly below that threshold remain non-coordinated.*

Assuming $\hat{\gamma} > \hat{\mu}/\mu^*$, we indicate with $\hat{M}$ and M^* respectively the total mass of non-coordinated and coordinated firms in equilibrium. It follows that the total number of each type i worker in the two labor market segments is $\hat{N}_i = \hat{M} \cdot \hat{n}_i$ and $N_i^* = M^* \cdot n_i^*$.

1.2.4 The effect of a tax rate change on hours worked

In this section we explore the consequences of a change of the tax rate faced by high-skilled workers t_H on optimal hours in the coordinated sector of the economy. Based on (1.4), one can derive the following expression (see Appendix 1.9.3):

$$\frac{d\hat{h}}{dt_H} = -\left[\hat{\mathrm{w}}_{\mathrm{H}} \frac{U_{cc,H} U_{\ell,H}}{U_{c,H}^2 (1 - t_H)} + \frac{P\, U_{\ell,H}}{U_{c,H} \hat{h} (1 - t_H)^2}\right] \times \left[\hat{\mathrm{w}}_H''(\hat{h}) + \alpha\, \hat{\mathrm{w}}_L''(\hat{h})\right]^{-1}, \quad (1.6)$$

where $U_{cc,H}(< 0)$, $U_{c,H}(> 0)$ and $U_{\ell,H}(> 0)$ are respectively the second derivative of the utility function relative to consumption, the marginal utility of consumption and the marginal utility of leisure for high-skilled workers, whereas $\alpha = \hat{n}_L/\hat{n}_H$.[13]

Since $\hat{\mathrm{w}}_i''(\hat{h}) > 0$ (Section 1.2.2), the sign in (1.6) depends on the first term in brackets that is made of two terms. Starting from the left the first term captures the income effect, while the second term is the substitution effect. If the income effect prevails over the substitution effect, the derivative is positive. In that case, desired hours of high-skilled workers go up when t_H increases and so do the hours worked in the coordinated sector. Conversely the derivative is negative if the substitution effect prevails over the income effects. Based on this we can state:

Prediction 3 *(Spillovers) At firms that coordinate work-hours, changes in tax rates that only affect high-skilled have spillover effects on hours worked by low-skilled*

[13] Here we consider the case of a generic additive separable utility function of which (1.1) is an example. Since firms simultaneously optimize hours worked and the number of workers of each type, the envelope theorem implies that $\alpha = \hat{n}_L/\hat{n}_H$ is not affected by changes of t_H.

workers. Hours worked by high and low-skilled workers move together.

Hours worked by high-skilled in coordinated firms however, are less elastic to the tax change than high-skilled hours in uncoordinated firms. To visualize this in Figure 1.4 we plot the case, consistent with our empirical findings, in which high-skilled workers desire to work more hours than low-skilled, the tax rate on high-skilled workers goes down, and the income effect from the tax change prevails. In this case, as t_H goes down desired hours decrease from h^*_{0H} to h^*_{1H}, and thus optimal hours in coordinated firms shift down from $\hat{h}_0$ to $\hat{h}_1$. If hours in the coordinated sector were to go down as much as desired hours do ($\mid \hat{h}_1 - \hat{h}_0 \mid = \mid h^*_{1H} - h^*_{0H} \mid$), the benefits for coordinated firms to marginally increase hours would remain unchanged relative to the pre-tax change period. At the same time however, the marginal costs from increasing hours would be lower because coordinated hours after the tax change are closer to the desired hours of low-skilled workers. Therefore, due to convexity of the wage-hours function, a marginal increase in hours would imply a lower increment in wage premiums to low-skilled workers than prior to the tax change. As a result, marginal benefits would exceed marginal costs and hours would optimally move up. This implies that $\mid \hat{h}_1 - \hat{h}_0 \mid < \mid h^*_{1H} - h^*_{0H} \mid$ and therefore:

Prediction 4 *(Attenuation): High-skilled workers in coordinated firms are less responsive to tax rate changes compared to workers in uncoordinated firms.*

The model also implies that the magnitude of the spillovers on low-skilled workers is increasing in the relative share of high skilled-workers. This is shown graphically in Figure 1.4 where the dashed line corresponds to a lower α. In this case, as an effect of the tax change, the equilibrium moves from C to D implying a greater reduction in hours than in the case of a higher α.[14]

Finally, in this setting a tax change that moves coordinated hours has effects on wage rates in the coordinated segment. While our main analysis focuses on the hours worked, in Appendix 1.9.5 we discuss the consequences of a tax change on wage rates.

1.3 Institutional Framework and Data Sources

We base the empirical part of the study on a panel of Danish workers. In this section we describe the main features of the Danish labor market and the main sources of our data.

1.3.1 The Danish labor market

Denmark is a particularly fitting setting for our study. In fact, a soft employment protection legislation combined with a generous social safety net makes the Danish labor market one of the most flexible in the world (Botero, Simeon, Porta, Silanes, and Shleifer 2004). In the past, wages and working time used to be

[14]The algebra behind prediction 4 remains difficult to treat even assuming specific functional forms for the utility function. Therefore, we only propose a graphical examination of this prediction. In the model of this section we do not explicitly consider unions. As long as unions' preferences reflect workers' preferences, including unions would not change the main predictions. However, the magnitude and timing of the effects might be affected due to union's rents or the timing of the renegotiation of the collective labor agreements. In the empirical analysis however, we do not find sizable differences between highly versus lowly unionized firms.

set at the industry level through collective bargaining, but over time the system has gone through a decentralization process that has made the negotiation much more firm-level based.

As an effect of this process and despite the fact that around 70% of the workers in the private sector are unionized, the wages of about 85% of them are negotiated directly at the worker-firm level (Hummels, Rasmus, Jakob, and Chong 2014). The wage premium for workers who work overtime is usually equivalent to 50% of the normal wage for the first 3 hours and 100% of the normal wage for each hour of overtime that exceeds the first 3 hours (Appendix 1.10.1).

Regarding the working time regulation, sectoral agreements usually define the normal week to be composed of 37 hours on average and by not more than 8 hours of overtime work. Firms however, have made increasing use of "opening clauses", which allow the union representatives at the company to develop local regulations that can deviate from sector-level agreements. In 2008 about 60% of full-time workers in the private sector were estimated to be covered by this type of local regulation (Dansk-Arbejdsgiverforening 2012). Similarly, the length of the reference period to determine the average number of weekly hours has been substantially increased. In 2008 it was 12 or more months for about 77% of the workers in the private sector (Dansk-Arbejdsgiverforening 2012).[15] In addition, an increasing number of employers have made use of local framework agreements that

[15]In 1988 the length of the reference period did not exceed 6 moths and it was of 0 to 4 months for 68% of the workers.

allow working time conditions to be negotiated between employers and employees at the individual level. In 2005 around one third of the private firms had signed an agreement of this type (Jørgensen 2006). Finally, workers have the option to convert hours of vacation in earnings at their relative wage rates. This provides extra variation in yearly hours of both salaried and hourly workers. The relative flexibility that Danish firms have in setting hours is consistent with the substantial variation in hours worked across firms within sectors that we observe in the data (Figure 1.2).

1.3.2 The data

In this section we outline our data sources and construction (for more details see Appendix 1.10). The empirical analysis is based on data from multiple sources (Appendix Table 1.10). We use data on individual socio-economic characteristics such as tax returns, earnings and education from the Integrated Database for Labor Market Research (IDA) that collects annual data on the entire Danish population. Data on annual hours of regular and overtime work are extracted from *Lønstatistikken* (LON).[16] Unfortunately, not all workers in IDA can be matched to LON. For our study however, it is particularly important to observe hours of as many workers as possible within a firm. For this reason we only consider firms in which the number of hours worked in a year are available for at least 95% of their

[16]Normal hours include vacation, weekends, legal holidays or lunch breaks, whereas unpaid leave and overtime hours are excluded. Data on hours are reported by employers.

workforce. Hourly wages are obtained as annual earnings over the sum of regular and overtime hours worked.

We use firm-level data from the Firm Statistics Register (Firmstat) and the Danish Foreign Trade Statistics Register that provide information on firm characteristics such as number of employees, industry affiliation, accounting and trade data. These registers cover the totality of private firms with more than 50 full-time equivalent employees and a representative sample of smaller private firms. We link each employee to the highest paying employer in week 48 of each year using the Firm-Integrated Database for Labor Market Research (FIDA). For workers whose spell in week 48 lasted less than 1 entire year, we use annualized hours and earnings.

We focus on full-time employees who were 15 to 65 years old in the period 2003-2011 when data are available from all sources. Following the official definition in place during that period, we define full-timers as those working more than an average of 26 weekly hours over a year period, which are about 90% of the workers in the sample.[17] We leave out part-timers for two main reasons: first, because they are more likely to work at unusual hours or fewer days in a week and this can be problematic for measuring coordination (Section 1.4.3). Second, because focusing on full-timers makes our results more easily comparable to other studies, especially those on wage inequality across firms.

[17]The weekly hours used to identify part-time workers are calculated by dividing regular annual hours by 52.

The final sample that we use includes more than 400,000 employees and around 8,300 firms. Table 1.1 shows descriptive statistics on individual and firm characteristics. In column 1 we consider the entire population (IDA), the second column is based on the sample of workers in IDA that can be linked to data on firms (FirmStat) and hours (LON). The last column refers to the final sample composed by firms where data on hours are reported for 95% or more of the workforce. Moving from the first to the second column, we notice that workers are older, more educated and earn more. This reflects the characteristics of the firms covered in FirmStat that are private and predominately large (average firm size of 51). Workers in columns 2 and 3, instead, show similar socio-economic characteristics. This suggests that our final sample, while providing better information on hours worked among coworkers, does not substantially distort the composition of the population for which records on individual, firm characteristics and hours are available.

1.4 Coordination and wage differentials across firms

1.4.1 The empirical model

In this section we study the relationship between employer-specific wage premiums and the coordination of hours. To do so we use an empirical model that relates the average wage premium paid by each firm j to all its workers over the time period of the study ($\widehat{\psi_{j(i,t)}}$) with a measure of the average coordination of hours over the same period (σ_j) and a vector of average firm controls ($\bar{Z}_j$). The estimating equation is as follows:

$$\widehat{\psi_{j(i,t)}} = \delta_0 + \delta_1 \sigma_j + \delta_2 \bar{Z}_j + v_j \tag{1.7}$$

where $\widehat{\psi_{j(i,t)}}$ is the firm fixed effect from a firm-worker fixed effect model of the type described in Abowd, Kramatz and Margolis (1999) (henceforth AKM) that we discuss in Sections 1.4.2. The term σ_j measures the average dispersion of hours worked across skill groups in a firm. Higher dispersion is interpreted as lower coordination. In Section 1.4.3 we discuss the details behind this variable. Based on prediction 1 from the stylized model, we expect $\hat{\delta_1}$ to be negative.

Existing studies have shown that wage differentials across firms correlate with a number of other firm characteristics some of which may confound the estimated correlation between coordination of hours and wages. For this reason in our empirical specifications we include in $\bar{Z}_j$ an extensive set of controls aimed at reducing these concerns. Among the controls we include detailed geographic and industry fixed effects, controls for the composition of the workforce of a firm both in terms of gender and ability, as well as other firm characteristics such as firm size or exporter status all of which have been found to correlate with wage differentials across firms.

Furthermore, one may worry that a negative correlation might be driven by institutional factors. In particular, workers in high paying firms may work longer hours, and in doing so they may "bunch" at 37 hours that is the upper limit imposed on the average number of hours by most of the collective labor agreements. For a similar reason, if workers in high paying firms are more likely to work overtime, higher wages may reflect statutory overtime premiums rather than compensating wage differentials. To take these factors into account, first, in all the specifications we control for the average number of hours worked. Then, in a set of robustness checks, we explicitly explore these potential concerns by excluding firms that bunch at 37 hours and by considering only the earnings from regular hours.

While we control for a large number of confounding factors, in the absence of an exogenous change in coordination, the results of this analysis remain of a correlational nature. However, due to the little evidence that exists on coordination of hours among coworkers we see this analysis as an important first step towards the understanding of a relevant economic phenomenon.

A growing number of studies have found evidence of a positive correlation between wage and productivity differentials across firms (e.g. Card, Cardoso, Heining, and Kline 2016). In the setting of our study the coordination of hours can be seen as a factor by which higher productivity in a firm translates into higher wages through compensating wage differentials. To measure the share of the correlation between wages and productivity in a firm that can be predicted through coordination, we first estimate equation (1.7) omitting σ_j and including measures of firm productivity such as value added and total factor productivity (TFP). From this alternative specification of equation (1.7) we obtain the partial R-squared associated with value added and TFP. This measures the share of the variance of $\widehat{\psi_{j(i,t)}}$ that is explained by productivity once we control for the variables in $\bar{Z}_j$. Then we measure the predictive power of hours coordination as the ratio of the partial R-squared associated to σ_j from equation (1.7) and the partial R-squared associated to valued added and TFP. From now on we refer to this ratio as the *Coordination share*.

1.4.2 The firm component of wages

We estimate the average wage premium paid by a firm to all workers as the firm fixed effect in the following regression model:

$$ln\, \mathrm{w_{ijt}} = \alpha_i + \psi_{j(i,t)} + \beta_1 X_{ijt} + r_{ijt} \tag{1.8}$$

where $\mathrm{w_{ijt}}$ is the gross hourly wage earned by individual i in firm j in year t. X_{ijt} is a vector of time varying controls while α_i controls for individual fixed effects.[18] The variable of primary interest to us is the firm fixed effect $\psi_{j(i,t)}$ that measures the fixed component of the wage that is specific to firm j once we control for individual

[18]Following Card, Heining, and Kline 2013, we include in X_{ijt} a set of interactions between year dummies and educational attainments as well as interaction terms between quadratic and cubic terms in age and educational attainments. In addition, we also control for firm characteristics that change over time such as value added, sales, capital per employee, exporter status and the share of hourly workers. These extra firm controls isolate the average wage premium paid by a firm from temporary fluctuations due to firm-level shocks. The results obtained when we only include individual characteristics are noisier but still in line with the baseline regression and are shown in the robustness section. We estimate this regression on all workers and firms for which data on hourly wages, individual and firm characteristics are available (column 2 in Table 1.1).

fixed and time varying characteristics.

Equation (1.8) is similar to the model used in AKM and several other studies. But, unlike in most other studies, we use hourly wages rather than annual or monthly earnings as a dependent variable to better fit the first model prediction that refers to wage rates. Furthermore we consider both male and female workers since coordination of hours involves all coworkers in a firm regardless of their gender. As in other studies, we focus on full-time workers only.

We estimate equation (1.8) using the methodology developed by Abowd, Creecy, and Kramarz 2002 to identify sets of connected firms. These consist of firms that have movers in common. In the analysis that follows we focus on the largest set of connected firms. Due to the high mobility that characterizes the Danish labor market and the relatively long time period considered, the largest connected set contains more than 99% of the workers and firms in the sample so that restricting the analysis to this group results in negligible changes in the parameters estimated from equation (1.8) (see Table 1.11 in the Appendix). The simultaneous identification of the firm and the individual wage component requires setting to zero either one firm fixed effect or one individual fixed effect. Thus the firm effect $\psi_{j(i,t)}$ has to be interpreted as the proportional wage premium or discount paid by firm j to all employees.

The AKM wage decomposition rests on the assumption of exogenous worker mobility conditional on observables. Following Card, Heining, and Kline 2013, in Appendix 1.11 we present a number of tests performed with the aim to investigate the plausibility of this assumption by analyzing the wage trends of movers. The results of these tests suggest that endogenous mobility is unlikely to be a major issue in our setting and, therefore, that the matching between firms and workers in our sample is predominately based on a combination of permanent firm and individual characteristics. Other recent studies reach similar conclusions (e.g. Card, Heining, and Kline 2013; Card, Cardoso, and Kline 2016; Song, Price, Guvenen, Bloom, and Watcher 2016).

1.4.3 Coordination of hours: measures and facts

Ideally, we would measure coordination based on the degree to which coworkers with different labor supply preferences work at the same time of the day or interact with each other. Unfortunately, data of this type do not exist on a large scale. In what follows we introduce an alternative measure of coordination based on the number of hours worked. Then we use survey data to validate it, finally we discuss how this measure correlates with other firm characteristics.

Our measure of coordination is the standard deviation of hours worked across skill groups:

$$\sigma_{jt} = \left[\frac{1}{S_{jt}} \sum_{s=1}^{S_{jt}} \left(\tilde{h}_{sjt} - \mu_{jt} \right)^2 \right]^{1/2}, \tilde{h}_{sjt} = \frac{1}{N_{sjt}} \sum_{i=1}^{N_{sjt}} h_{isjt} \tag{1.9}$$

where S_{jt} is the number of skill groups in firm j in year t, N_{sjt} is the number of workers in skill group s in a firm-year while $\tilde{h}_{sjt}$ is the average number of annual hours (regular and overtime) in skill group s in firm j at time t. Finally, μ_{jt} is the average of $\tilde{h}_{sjt}$ across skill groups. We interpret a low value of this standard deviation as implying greater overlap of workers at the workplace and thus greater coordination. σ_j in equation (1.7) is the average of σ_{jt} over the years 2003-2011.

In measuring coordination, we use skill groups to proxy for differences in desired hours. Labor force survey data on desired hours support this assumption showing that desired hours increase with skills (Table 1.16). We use two alternative definitions of skill groups. First, starting from the estimated coefficients from equation (1.8), we measure skills as the sum of the fixed and the time varying individual components of the hourly wages: $\widehat{s_{ijt}} = X_{ijt}\hat{\beta}_1 + \hat{\alpha}_i$ (Iranzo, Schivardi, and Tosetti 2008 and Irarrazabal, Moxnes, and Karen-Helene 2014). We thus assign workers in each year to one of 10 skill groups defined as deciles of the distribution of $\widehat{s_{ijt}}$. Given that this measure of skills is based on fixed and time varying individual characteristics, it might reflect more closely a worker's hours preference, thus also capturing the possible sorting of similar workers across firms. In a setting where wages depend on hours however, $\widehat{s_{ijt}}$ might still reflect equilibrium outcomes to the extent that those are not fully captured by the firm component of wages in equation (1.8). For this reason in Appendix 1.12.2 we present the results of a parallel analysis in which we define skills at the intersection of 3 educational groups (i.e. primary, secondary and tertiary education) and 3 broad occupational categories (i.e. manager, middle manager and blue collar). The results obtained from these two alternative definitions of skills do not differ in a sensitive way.

Since we do not observe the days and times when workers provided hours, our measure of coordination may be misleading if coworkers work a similar number of hours at different times of the day, in different days of the week or in different periods of the same year. For the latter case, since the great majority of the workers in our sample work for the entire year this is unlikely to play a major role.[19] Furthermore, by focusing on full-time workers in private firms we reduce concerns regarding whether they work different days of the week or at different times of the working day. In fact, descriptive evidence from time use survey data (TUS) indicates that around 70% of full-time workers in Denmark start working between 7am and 9am.[20] Of the remaining 30% the great majority are employed in either manufacturing or the health-care sector. However, the former sector emerges as one of the least coordinated from our analysis (Section 1.4.3) while most the health-care sector is public and thus excluded from the sample. Similarly, around 60% of full-time workers in TUS do not work on weekends and those that do work are mostly concentrated in the health care sector.

While focusing on full-timers reduces the concerns mentioned above, this may come at the cost of ignoring some of the variation that is of interest to us. In particular firms at low degree of coordination may hire relatively more part-timers. This concern, however, is mitigated by the fact that our measure of coordination strongly correlates with the share of part-timers, so that, based on σ_{jt}, more

[19] More than 75% of the workers in our sample have yearly employment spells that last more than 360 days.

[20] We use 2001 Time use survey data in Denmark . Details on this survey can be found in Appendix 1.11.2.

coordinated firms also hire fewer part-timers (Section 1.4.3).

Validation exercises

In this section we use O*NET data to validate our measures of firm level coordination. O*Net is a survey that provides information on 277 occupation-specific descriptors such as work style, work content, interests and experience on 965 occupations. It is based on an ongoing survey of workers in the United States. We use the US survey because a similar survey is not available in Denmark. For each descriptor O*Net provides a measure of its importance in each of the occupations surveyed. We match this information to Danish registers based on occupation.[21] We select the 3 descriptors in O*NET that capture aspects of a job that involve coordination of hours across skills. Similar descriptors are used in other studies to capture skill complementary (Bombardini, Gallipoli, and Pupato 2012). The descriptors are: *Contact*: "How much does this job require the worker to be in contact with others (face-to-face, by telephone, or otherwise) in order to perform it?"; *Teamwork*: "How important is it to work with others in a group or team in this job?"; *Communication*: "How important is communicating with supervisors, peers, or subordinates to the performance of your current job?".

The measure of importance of these 3 descriptors ranges between 1 and 100. We take the median score across coworkers each year as a measure of the

[21]We map the ISCO-88 classification of the Danish registers to the SOC classification in O*Net using the cross-walk provided by the National Crosswalk center.

importance of each factor in a specific firm in that year.[22] In Figure 1.5 we plot the standard deviation of hours versus the importance of the 3 descriptors across firm-year observations. A negative and statistically strong correlation emerges between each of the above descriptors and the standard deviation of hours across skill groups. That is, in firms where coordination of hours is low the importance of aspects that involve coordination is also low.

In Appendix 1.11 we discuss an additional set of validation exercises based on the Survey of Adult Skills and the Danish Time Use Survey. The evidence emerging from these surveys is consistent with the evidence we found with data from O*NET.

Coordination and firm characteristics

In this section we document a few facts that emerge when we look at the correlations between our measures of coordination and a number of firm characteristics.

Table 1.2 shows the standardized coefficients obtained from a regression of coordination on a number of firm characteristics. A few interesting facts emerge from the table. First, firms that coordinate are more profitable: they have higher value added, sales per employee and total factor productivity. This evidence supports our theoretical framework in which more productive firms select into coordination. Along the same lines, firms that coordinate are more likely to be exporters and

[22] We break ties in median scores using the average.

to employ a greater share of tertiary educated workers. Second, less coordinated firms employ relatively more hourly and part-time workers suggesting that greater flexibility in these firms is achieved through the hiring of these workers. Third, lower coordination is associated with higher unionization rates. This suggests that low dispersion of hours is not systematically linked to institutional constraints imposed by unions.

Existing studies document that that managerial ability in a firm strongly correlates with the use of more advanced management practices and higher productivity (Ichniowski, Kathryn, and Prennushi 1997, Bloom, Sadun, and Van-Reenen 2015). In a recent study by Bender, Bloom, Van Reenen, and Wolter 2016 managerial ability is measured as the average individual fixed effect (α_i) from an AKM model among the workers in the top quartile of the distribution of α_i in each firm. In Table 1.2 we look at the correlation between this measure of managerial ability and hours coordination and we find a strong positive association between the two. This suggests that hours are more coordinated in better managed firms.

Deming 2015 highlights the importance of social skills in reducing the costs of coordination among workers. To examine how coordination of hours correlates with social skills at the firm level we construct 4 measures of social skill intensity within firms. These are based on the same O*NET descriptors used in Deming 2015 to measure the intensity of social skills at the occupational level (i.e. *Coordination*, *Negotiation*, *Persuasion* and *Social Perceptiveness*).[23] Consistent with Deming

[23]Coordination in O*NET is defined as measuring the importance of "Adjusting actions in

2015 we find a strong and negative correlation between the dispersion of hours in a firm and social skill intensity.

Table 1.3 compares coordination in different sectors. Based on this, firms in the service industry coordinate more on average than those operating in the agriculture, manufacturing or construction sectors. Most of the correlations discussed in this section however, hold within narrowly defined sectors suggesting that they are driven by differences across firms within sectors (Table 1.2).

1.4.4 Results

In this section we discuss the correlation between the firm component of wages and hours coordination. We start by estimating this correlation across all firms and checking for the importance of other confounding factors. Then we study how wages and coordination of hours correlate across firms within sectors and finally we assess the importance of coordination in linking productivity to wages in a firm.

Column 1 in Table 1.4 shows the standardized correlation between coordination and the firm component of wages excluding controls for other firm characteristics. In line with the model prediction, higher coordination in a firm is associated with higher relative wage premiums.

relation to others' actions", Negotiation as "Bringing others together and trying to reconcile differences", Persuasion as "Persuading others to change their minds or behavior" and Social Perceptiveness as "Actively looking for ways to help people".We match O*NET to the Danish registers based on occupation and we take the average importance of each one of these descriptors in a firm as a measure of social skill intensity in that firm.

However, from the discussion of the previous section one may worry that this correlation may be driven by other firm characteristics. Thus in columns 2 we control for firm size and exporter status to account for the fact that large firms and exporters pay higher wages (e.g. Mueller, Ouimet, and Simintzi 2015, Helpman, Itskhoki, Muendler, and Redding 2016, Macis and Schivardi 2016). We also include region fixed effects to control for geographic differences in pay. In this last specification we also control for the share of female workers in the firm because females are more likely to sort in low paying firms or to bargain lower wages (Card, Cardoso, and Kline 2016). Finally, we control for the share of unionized workers as a way to capture rents from unions (Dickens 1986), and the average number of hours worked to control for compensating differentials due to long hours.

In line with the literature, we find that firm size and export status are positively associated with wages, and that better paying firms employ fewer female workers. Importantly, as in other recent studies we find no evidence of compensating differentials due to long hours (Card, Cardoso, and Kline 2016). In contrast, we find that the magnitude, the sign and significance of the correlation between wages and coordination is unaffected by these controls. This result highlights the importance of measuring relative hours in a firm to capture dis-amenities from working time.

In column 3 we add to the previous specification extra controls for the skill composition of the workforce in a firm. Recent studies in fact, show that the sorting of better able workers in better paying firms is important in determining wage inequality between firms (Card, Heining, and Kline 2013, Song, Price, Guvenen, Bloom, and Watcher 2016). We control for the skill composition of the workforce in two ways. First we include controls for the share of workers in each skill group. Then, to account for the fact that workers in the same skill group might differ across unobserved dimensions, we also control for the average values of the individual fixed effect (α_i) in each quartile of the firm distribution of α_i. The average α_i in the top quartile of the firm distribution has been found to correlate strongly with better managerial practices (Bender, Bloom, Van Reenen, and Wolter 2016). Therefore this extra set of controls provides also a way to proxy for differences in managerial practices across firms. The findings from this specification are reassuring because the coefficient attached to coordination retains its sign and significance while the magnitude increases.

The correlation remains negative and of similar magnitude when we exclude from the analysis firms that bunch at 37 hours (average hours between 36.5 and 37.5) or when we consider earnings and coordination from normal hours only, thus excluding overtime (columns 4 and 5). This suggests that the results are not affected much by these other institutional factors.

From the results of the previous section, we know that coordination positively correlates with the intensity of social skills in a firm. These skills have been associated to higher wages (Deming 2015). In light of this, one possible reason for the higher returns associated to social skills may be that they allow for a greater degree of hours coordination that requires compensating wage differentials. However, to the extent that the returns to social skills are associated to other factors such as, for instance, the low substitutability with new production technologies, it is important to check how much of the correlation between coordination of hours and wages can be linked to social skills. Thus in column 6 we add to the baseline specification the 4 measures of social skill intensity described in the previous section. We find that around 1/3 of the correlation estimated in column 3 can be associated to these skills, suggesting that most of the returns from coordination are not driven by social skills.

The strong correlation between the firm component of wages and coordination of hours persists within 1, 2 or 3-digit sectors (columns 1 to 3 in Table 1.5) suggesting that coordination plays a non-negligible role in predicting wage inequality across firms within sectors.[24]

In most of the specifications the magnitude of the correlation between wages and coordination is greater than the association between wages and firm size or capital per employee, and of comparable magnitude as export status. These findings establish hours coordination as an important predictor of between-firm wage inequality. From column 3 in Table 1.4, we infer that an increase of hours coordination by one standard deviation (95 yearly hours) is associated with an increase in firm-level wages equivalent to 0.5%.[25]

If we maintain the baseline assumption that there are no mobility frictions

[24]The correlation within 2 or 3-digit industries is less precisely estimated. This is likely due to outliers. If coordination is measured through the median absolute deviation from the median hours in fact, the coefficients are negative and strongly significant (columns 4 to 6 in Table 1.5).

[25]The effect is obtained by multiplying the coefficient (0.07) by the standard deviation of the firm-component of wages (0.26) that gives a 0.0156 log wage change equivalent to around 1 DKK or 0.5% of the average wages.

between coordinated and non-coordinated firms, this correlation reflects the compensating differential to keep workers indifferent between the two labor-market segments. In contrast, if we allow for mobility frictions, we cannot exclude the possibility that the cross-firm wage differentials also reflect rent sharing at better paying firms (Burdett and Mortensen 1998). In the case of mobility under frictions, Lavetti and Schmutte 2016 have recently proposed an estimation procedure to account for this, by which the estimated correlation between wages and firm dis-amenities is a lower bound of the actual compensating differentials. We may therefore interpret our findings as arguably indicative lower bounds for relevant compensating differentials from hours coordination. This interpretation is in line with other recent studies that identify compensating differentials as an important determinant of wage inequality across firms using alternative methodologies (e.g. Sorkin 2015).

In Appendix 1.11.3 we discuss a set of additional robustness checks to the results presented in this section including, for instance, a discussion of measurement errors in hours.

Coordination of hours, wages and firm productivity

A growing literature finds that the firm components of wages strongly correlates to productivity in a firm (e.g. Card, Cardoso, Heining, and Kline 2016). In our stylized model more productive firms select into coordination and pay wage premiums. Consistent with this, conditional on measures of firm productivity, such as value added per employee, the coefficient on the standard deviation of hours goes down and becomes insignificant while value added per employee strongly and positively correlates with wage premiums (column 8 in Table 1.5).

To get a sense of the importance of hours coordination in explaining the wage inequality across firms that is due to productivity we use the *coordination share* described in Section 1.4.1. In line with the evidence provided in the previous paragraph, this measure rests on the assumption that coordination only affects wages through productivity. We estimate a coordination share of 20% across all firms (column 3 in Table 1.4) and of 4% within 3-digit industries (columns 3 in Table 1.5). This suggests that coordination predicts a non-negligible share of the variation of firm wages that is linked to productivity differentials between and within sectors, and that cannot be explained by other factors that are known to affect wages and productivity.

1.5 Coordination, labor supply and tax rate changes

1.5.1 The 2010 Danish Tax Reform

We base the analysis presented in this section on the changes to the Danish personal tax schedule mandated by the 2010 tax reform. This reform led to a substantial decrease of the marginal tax rate on labor income faced by high income earners while it left the tax rate of low income workers almost unchanged. To the extent that low and high income workers differ in desired work hours, the reform provides an ideal setting to test for spillovers and attenuating effects from coordination.

The Danish income tax system is based on different types of income that are aggregated in multiple ways to form different tax bases taxed at different rates. A detailed description of the tax system can be found in Appendix 1.10.5. For what concerns our analysis, prior to the 2010 reform income was taxed using a three-bracket progressive tax schedule (Figure 1.6). As an effect of the 2010 reform the middle tax bracket was abolished while tax rates at the bottom and top bracket went down by respectively 2 and 7 percentage points between 2008 and 2011. The reform also increased the income amount at which the top bracket starts that went up by around 9% in real terms between 2008 and 2011. This led to a substantial decrease of the marginal tax rate on labor income faced by workers in the middle and top tax bracket. For them in fact, marginal tax rates went down respectively by around 16% and 10% (Figure 1.7). The decrease was less pronounced in the bottom bracket where the marginal tax rate went down by around 4% (more details in Appendix 1.10.5).[26]

Based on this, from now on, we refer to low-skilled workers as the workers who were either tax exempt or in the bottom tax bracket in 2008 (left of the dashed

[26]The net-of-tax rate in the top, middle and bottom bracket went up respectively by 3%, 15% and 19%.

line in Figure 1.8). Conversely, we define high-skilled workers as the workers who were in the middle or top tax bracket in 2008. From this group however, we exclude workers who were in the top bracket in 2008 and who, based on the 2008 real income and the tax schedule in place after the reform, are predicted to be in the bottom tax bracket in 2011. We refer to these workers as the residual group. Workers in this group had incomes just above the lower limit of the top bracket in 2008 (dotted line in Figure 1.8). When the reform increased this limit (solid line in Figure 1.8) and abolished the middle bracket, these workers ended up (mechanically) in the bottom bracket after the reform.

Relative to the high-skilled, workers in the residual group experienced a net-of-tax rate change about 3 times as large (Figure 1.9). As an effect of this, while for high-skilled workers the income effect prevails and hours go down as a consequence of the reform (Section 1.5.6), for workers in the residual group the substitution effect prevails and the estimated labor supply elasticity is positive but insignificant (Appendix 1.11.4). In Appendix 1.11.4 we argue that the insignificant effects may be due to the fact that these workers are close, in terms of income, to the top bracket and thus unwilling to work more hours to avoid substantially higher taxes.

Since the supply of hours in the residual group is unchanged by the reform and in order to keep the empirical framework as close as possible to the stylized model, in the baseline specification we only study the spillovers from high to low-skilled workers. However, we then show that including the residual group does not affect the conclusions of the baseline analysis. Based on this classification, around 34% of the workers in our sample are low-skilled, 54% are high-skilled, the remaining 12% are in the residual category (Figure 1.9).[27]

1.5.2 The Tax Data

We base the tax analysis on records from the Danish Tax Register that collects detailed information on all the items that determine individual tax liabilities in Denmark. Marginal tax rates however, are not directly observable. For this reason we use the available tax records to simulate marginal tax rates for each worker using a simulator model of the Danish tax system. We do so by extending the tax simulator used in Kleven and Schultz 2014 to the years 2006-2011. In this simulator marginal tax rates on labor income are obtained as the increase in tax liabilities due to a rise of labor income by 100 DKK. In particular, since the tax liability T() is a function of labor income (z_{LAB}) and other income components ($z_1, ...z_N$), the marginal tax rate on labor income is derived as follows $\tau = [T(z_{LAB} + 100, z_1, ...z_N) - T(z_L, z_1, ...z_N)]/100$.

In the empirical models that we use we relate changes of labor supply to changes in marginal tax rates over 3-years intervals. In the baseline specification we focus on the interval 2008-2011. We do this to reduce the possibility that the effects measured could capture lagged effects of a prior tax reform that occurred

[27]The share of workers in the low, high and residual group in the entire population is (respectively) 50%, 40% and 10%. The greater share of high-skilled workers reflects the characteristics of our sample where large firms that employ more educated workers are over-represented (Table 1.1).

in 2004. However, as a robustness check, we also consider the years 2006 to 2008, but we exclude the years prior to 2006 as they would be too close to the 2004 reform. Intervals of 3 years are commonly used in the taxation literature (e.g. Feldstein 1995, Gruber and Saez 2002). In particular, a recent study by Kleven and Schultz 2014 that analyzes the effects of a large number of tax reforms in Denmark, argues in favor of intervals of 3 years as the right compromise to account for the sluggishness of the response to tax reforms while preserving the variation and power from the tax change.[28]

1.5.3 The attenuating effects of coordination

We analyze the effect of the tax reform on the labor supply of high-skilled workers using the following empirical model:

$$log\left(\frac{h^H_{it+3}}{h^H_{it}}\right) = \beta_0 + \beta_1\, log\left(\frac{1-\tau^H_{it+3}}{1-\tau^H_{it}}\right) + \beta_3\, X_{ijt} + \upsilon_{ijt} \tag{1.10}$$

In this model the dependent variable is the log change in hours worked by high-skilled workers between 2008 and 2011. We relate this to the individual variation of the marginal net-of-tax rate on labor income (1-τ) that occurred over the same period. We control for a number of individual (i) and firm (j) characteristics X_{ij} measured in 2008 (time t). The effect of the reform is captured by β_1 that measures the elasticity of hours worked to changes of the marginal net-of-tax rate.

[28]Studying changes over 3 years intervals also minimizes the concerns related to the intertemporal shift of earnings for tax avoidance purposes that likely happened between 2009 and 2010 (Kreiner, Leth-Pedersen, and Skov 2016).

To test whether the response of high-skilled workers in more coordinated firms is lower than that of similar workers in less coordinated firms, we estimate this model separately on workers employed in high versus low-coordination firms. In presence of attenuating effects, the elasticity β_1 is expected to be smaller, in absolute terms, for workers in high-coordination firms.

In this specification the labor supply elasticity is inclusive of the income effect. In Appendix 1.11.5 we make an attempt to separate the uncompensated elasticity of labor supply from the income elasticity. However, our study is based on a single tax change that mostly affected workers in the upper part of the income distribution. Therefore, unlike in other existing studies, we have limited variation in tax rates across the income distribution that is needed to separately estimate the two effects in a precise way. Despite the noisy estimates, the results in Appendix 1.11.5 support our baseline findings.

1.5.4 The spillover effects of a tax change

In firms that coordinate hours worked, a tax rate change that targets one type of workers can affect hours worked by other workers in the same firm (prediction 3). We test this prediction by relating the effects of a tax-driven change in hours worked by high-skilled workers to changes in the supply of hours of low-skilled coworkers. The estimating equation takes the following form:

$$log\left(\frac{h^L_{ijt+3}}{h^L_{ijt}}\right) = \alpha_0 + \alpha_1\, log\left(\frac{\overline{h^H_{jt+3}}}{\overline{h^H_{jt}}}\right) + \alpha_2\, log\left(\frac{1-\tau^L_{it+3}}{1-\tau^L_{it}}\right) + \alpha_3\, X_{ijt} + \epsilon_{ijt} \quad (1.11)$$

The dependent variable in this model is the log change in the number of hours worked by low-skilled worker i in firm j between 2008 and 2011. The regressor of key interest is

$$log\left(\frac{\overline{h_{jt+3}^{H}}}{\overline{h_{jt}^{H}}}\right)=log\left(\frac{H_{jt+3}^{-1}\sum_{h=1}^{H_{jt+3}}h_{hjt+3}}{H_{jt}^{-1}\sum_{h=1}^{H_{jt}}h_{hjt}}\right) \tag{1.12}$$

This term captures the log change in the average number of hours worked by high-skilled workers in firm j. We isolate the tax related component of this change using the average variation of the marginal net-of-tax rate on labor income among high-skilled in firm j as an instrument for the change in hours. Section 1.5.5 describes this instrument in details. Based on the model prediction, we expect α_1 to be positive and greater in magnitude in more coordinated firms.

The term $log\left(1-\tau_{it+3}^{L}/1-\tau_{it}^{L}\right)$ in equation (1.11) captures the changes of the marginal net-of-tax rate on labor income faced by low-skilled between 2008 and 2011. Since the reform lowered the marginal tax rate paid by low-skilled, this term controls for the direct effect of the reform on the supply of hours of low-skilled workers. Finally, X_{ijt} is a vector of firm and individual controls measured in 2008.

The empirical specifications that we have so far discussed differ from the standard model in the taxable income literature (e.g. Gruber and Saez 2002) along two important dimensions. First, we estimate the effect of tax changes on hours worked rather than on labor income. In our setting in fact, a tax rate change can move hours and wage rates in opposite directions making it difficult to interpret the overall effect on labor income. Second, in equation (1.11) we augment the standard model with one extra term that captures the spillover effects of the tax change among coworkers. This is done to reflect a key feature of our framework where hours worked by one type of workers depend on the hours worked by the other workers in the same firm. Section 1.9.6 in the appendix describes how to adapt the standard economic model underlying the empirical specification used in the literature to the specific features of our setting.

1.5.5 Identification

The identification of the effects of the reform from equation (1.10) and (1.11) needs to address multiple issues. First, due to the non-linearity of the tax schedule, the marginal tax rate in the post-reform period depends on post-reform income that is endogenous to the supply of hours. This creates a correlation between $\Delta\, log\ (1 - \tau_{it})$ and the error terms in our specifications. Second, changes of the supply of hours by high-skilled workers in equation (1.11) might be correlated to changes of the supply of hours worked of low-skilled coworkers in endogenous ways. This might be the case, for instance, if both types of workers experience the same unobserved local labor market shocks, local policy reforms or changes specific to a firm (e.g. firm organizational changes, changes to the technologies used in production).

To address the first set of concerns, following the literature (e.g. Gruber and Saez 2002) we construct a set of instruments based on mechanical tax rate changes that are driven only by variations of the tax laws. In practice, for each individual in the sample we use a simulator of the Danish tax system to obtain marginal tax rates on labor income (τ_{Mit+3}) in the post-reform period (time t+3) based on income in the pre-reform period (time t) adjusted for inflation. We then construct the mechanical change of the marginal net-of-tax-rate on labor income of high-skilled workers as $log\left(1-\tau^{H}_{Mit+3}\right)-log\left(1-\tau^{H}_{it}\right)$ and we use this as an instrument for the observed change $\Delta\, log\ \left(1-\tau^{H}_{it}\right)$ in equation (1.10). Similarly, we use the mechanical change of the marginal net-of-tax rate of low-skilled workers $log\left(1-\tau^{L}_{Mit+3}\right)-log\left(1-\tau^{L}_{it}\right)$ as an instrument for the observed change $\Delta\, log\ \left(1-\tau^{L}_{it}\right)$ in equation (1.11).

By holding real income constant between t and $t+3$ these instruments exploit the variation of the marginal tax rates due to changes of the tax schedule only. To give a sense of the identifying variation, Figure 1.9 plots the average mechanical change of the marginal net-of-tax rates among high and low-skilled workers between 2008 and 2011. Due to the nature of the reform, the change is more pronounced for high-skilled (18%) than for low-skilled (2%).

While these instruments are exogenous to post-reform income they still depend on pre-reform income which is problematic if the latter correlates with the error term. The literature has focused on two main channels through which this may occur (e.g. Slemrod 1998, Saez, Slemrod, and Giertz 2012). First, the labor supply of workers at different levels of pre-reform income might follow different long term trends unrelated to the tax reform. Second, high incomes in one year tend to have lower income in the following periods and vice versa (i.e. mean reversion). This might generate a negative correlation between the error term and the instruments.

To deal with this, we follow the existing literature and we perform a set of additional regressions in which we control for pre-reform income in a flexible way. Overall however, we find that our baseline results are not affected in a noticeable way by these controls. This may be due to the fact that, unlike in most other studies, we estimate separate regressions on rather homogeneous groups of workers (i.e. low-skilled and high-skilled). Furthermore, we study a relatively short time period thus limiting the concerns related to long term trends.

Turning to the identification of the spillover effects (α_1) from equation (1.11), we use simulated marginal tax rates to construct the mechanical change of the average marginal net-of-tax rate on labor income faced by high-skilled workers in each firm j:

$$log\left(\frac{\overline{1-\tau^H_{Mjt+3}}}{\overline{1-\tau^H_{Mjt}}}\right) = log\left[\frac{H^{-1}_{jt+3}\sum_{h=1}^{H_{jt+3}}\left(1-\tau_{Mhjt+3}\right)}{H^{-1}_{jt}\sum_{h=1}^{H_{jt}}\left(1-\tau_{Mhjt}\right)}\right] \tag{1.13}$$

We then use this term as an instrument for $log\left(\overline{h^H_{jt+3}}/\overline{h^H_{jt}}\right)$ in equation (1.11). This instrument isolates the component of the change in hours of the high-skilled due to the tax reform from other confounding factors. Its validity hinges on the assumption that the instrument affects hours worked by low-skilled workers only through changes in the average hours of high-skilled coworkers (i.e. the exclusion restriction). This assumption may be violated if, for instance, the tax reform, while changing the supply of hours of high-skilled workers, led also to the adoption of new technologies that required a different supply of hours by low-skilled workers. In that case in fact, hours worked by low-skilled workers would vary through channels different from coordination for reasons correlated to the instrument. However, we fail to find significant effects of the reform on firm size, physical capital and the share of high relative to low-skilled workers in a firm, suggesting that firm technologies were not affected by the reform (Appendix 1.11.6).

Finally, one general concern of the instruments that we use is that they might capture other unobserved changes that occurred between t and $t+3$ thus confounding the estimated effect of the tax reform (e.g. other policy reforms or macroeconomic shocks). For this reason we present additional specifications in which we follow the workers from the baseline regressions back to 2006, then we estimate our baseline models on all 3-year intervals between 2006 and 2011 adding base-year fixed effects. These specifications also allow to control for unobserved characteristics specific to all firm workers by using firm fixed-effects. While these models have some advantage over the baseline, they result in weaker first stages (Section 1.5.6) and are more likely to capture lagged effects of the 2004 tax reform.

1.5.6 Results

Coordination and attenuating effects

Table 1.6 shows the elasticity of hours worked by high-skilled workers to the net-of-tax rate estimated from equation (1.10). In columns 1 to 3 we estimate the regression on all high-skilled workers in the sample while in columns 4 to 7 we differentiate between workers in high versus low-coordination firms. The base year in all the specifications is 2008. We measure the degree of coordination of each firm in the base year using the standard deviation of hours worked across skill groups described in Section 1.4.3. Highly coordinated firms are in the bottom half of the distribution of the standard deviation across firms, while low-coordination firms are in the top half. To attach each workers to the right measure of coordination we restrict the analysis to high-skilled who are at the same firm in 2008 and 2011.[29]

The first column in Table 1.6 shows the OLS estimates while all other columns are based on the IV model described in the previous section. In absence of controls for pre-reform income, the elasticity from the IV model in column 2 is around -0.07. Probably due to mean reversion, the elasticity goes up to -0.05 when we control for income in 2008 (column 3). Based on this estimate, total hours of high-skilled went down by around 0.8% or about 15 hours on a yearly basis as an effect of the reform.[30]

When we break the sample between workers at firms with high (column 4) versus low (column 5) degree of coordination however, we find substantial differences between the two groups. In line with the model predictions, we estimate a statistically significant elasticity of around -0.1 in low-coordination while in

[29]Relative to the other workers in our sample, the workers we use for estimation are on average one year older, the high-skilled have lower average annual earnings (by about 2,000$). However, the workers in the two groups look similar across many other dimensions such as gender, hours worked, geographic location and education.

[30]-0.5% is obtained as the product of the the elasticity (-0.047) and the average log change of the net-of-tax rate between 2008 and 2011 (17%). -0.8% is then multiplied by the average number of hours worked in 2008 by the high-skilled workers in the estimating sample (i.e. 1924) to obtain the change in hours due to the reform.

high-coordination firms the elasticity is insignificant and of about -0.02. The two elasticities are statistically different at the 5% level. The difference across workers in the two types of firms is even more pronounced when we consider regular hours only (columns 6 and 7). Therefore based on these estimates, hours worked by high-skilled workers in firms with high degree of coordination were not significantly affected by the reform, while high-skilled hours in low-coordination firms went down by around 1.6%, that is 30 hours per year, 20 of which are estimated to be regular hours.[31]

The difference between the two elasticities widens as we move towards the extremes of the distribution of coordination. In fact, workers in the top 25% most coordinated firms show even lower elasticities than in the baseline. Conversely, workers in the bottom 25% least coordinated firms are more responsive than the baseline (columns 1 and 2 in Table 1.7). This suggests that the attenuating effects increase with the degree of hours coordination in a firm.

The differential effects in the two types of firms are not driven by unobserved characteristics of a firm or by other unobserved factors that occurred between 2008 and 2011. In fact, the results hold conditional on firm and base-year fixed effects (columns 3 to 5 of Table 1.7).

In agreement with the existing literature, we find an average elasticity of hours across all firms close to zero (Pencavel 1986, Triest 1990, Chetty 2012).

[31]The average change in hours worked is derived as the product of the elasticities in low-coordination firms (i.e. -0.097 for total hours and -0.061 for regular hours), the average net-of-tax rate change (17%) and the average number of hours worked by high-skilled workers in low-coordination firms (i.e. 1914 total hours and 1858 regular hours).

However, we document pronounced attenuating effects associated to coordination that provide a mechanism to explains the low elasticity of previous studies. Other studies that use a similar methodology and focus on labor income (rather than hours) find small and positive elasticities in Denmark (Kleven and Schultz 2014). However, these studies consider the entire population while we focus on full-time workers in private firms for whom data on hours are available. Using a comparable sample to analyze the effects on labor income we find results that are in line with other studies (Appendix 1.11.4).

While coordination attenuates behavioral responses, it also lowers the deadweight burden of taxation on high-skilled. Based on our results, we can conclude that if workers in high-coordination firms were to change their supply of hours as workers in low-coordination firms do, then the marginal excess burden would be twice as large as the one estimated from the tax reform.[32]

Coordination and spillovers

Table 1.8 shows the estimated elasticity of low-skilled hours to the average hours of high-skilled coworkers obtained from equation (1.11). In these specifications the base year is 2008 and we focus only on low-skilled workers who are at the same firm in 2008 and 2011. Column 1 shows the OLS estimates, while columns 2 to 7 show the IV estimates. In the first 5 columns we estimate the effects on regular

[32]The marginal excess burden (MEB) is defined as the ratio between the change in tax revenues due to behavioral responses to the tax reform and the total change in tax revenues (see also Appendix 1.9.6).

hours while in the last two we examine the effects on total hours.

In line with our theory, we estimate positive and significant spillovers that are robust to controls of pre-reform income (columns 3 and 4).[33] Specifically, we estimated an elasticity of regular hours of low-skilled workers to average hours of high-skilled coworkers of 0.9. This implies an increase of 0.85 hours worked by low-skilled workers for each additional hour that high-skilled coworkers provide on average. Based on this, we estimate that normal hours of low-skilled coworkers went down by around 8 hours (or 0.5%) on a yearly basis as an effect of the reform.[34] Thus, while the elasticity of low-skilled to high-skilled hours is high, the estimated spillovers from the tax reform are relatively low, due to the fact that high-skilled hours do not change much.

The elasticity of total hours (regular and overtime) is estimated to be higher suggesting even stronger spillovers from overtime (column 6). However, the point estimate from this specification might be inflated by the low power of the instrument (F-stat of about 4).

Our framework implies stronger spillovers in firms at high degree of coordination. Ideally we would compare high and low-coordination firms. Based on the results from the previous section, however, hours worked by high-skilled in

[33] In column 3 to 7 we control for pre-reform income using piece-wise splines of income at $t-1$ and the log change of income between time $t-1$ and t (similar to Kopczuk 2005). We select this specification based on the strength of the first stage. Alternative controls of pre-reform income provide similar results (Appendix 1.11.4).

[34] The elasticity of normal hours worked by high-skilled workers across all firms is estimated to be around -0.03 (Table 1.23 in the Appendix) which, at the average annual hours of 1888, implies a reduction of around 10 yearly hours worked as an effect of the reform.

high-coordination firms were not affected by the reform. As a result, we lack the identifying variation to estimate the spillovers in these firms. Thus in column 4 and 7 of Table 1.8 we restrict the analysis to low-skilled workers in firms at low degree of coordination where hours of high-skilled coworkers are more elastic. Among these workers we find lower elasticities of regular and total hours than across all workers which suggests weaker spillovers in low-coordination firms.

The theory predicts stronger spillovers in firms that employ relatively more high-skilled workers. In column 1 of Table 1.9 we interact the average change in hours of high-skilled workers with a dummy for being in a firm in which more than half of the workers were high-skilled in 2008. Despite the imprecise estimate, the sign of the interaction is suggestive of stronger spillovers in firms with a greater share of high-skilled workers.

The significance and magnitude of the spillovers that we find is robust to the inclusion of firm and base year fixed effects capturing unobserved characteristics of a firm, or of the time period over which the reform occurred (columns 2 and 3 in Table 1.9). In addition, the spillovers from high-skilled workers remain of similar magnitude and significance when we control for the average change in hours among coworkers in the residual group (column 4). Consistent with the fact that hours in the residual group are unaffected by the reform, we do not find significant spillovers from this group on low-skilled coworkers (column 4).

The existence of spillovers has two main implications. First, it implies extra tax efficiency costs. Specifically, taking spillovers into account we estimate an increase in the marginal excess burden from the tax reform of around 15% (Appendix 1.9.6). Second, with spillovers the use of untargeted workers as a control group to estimate the labor supply elasticity provides downwards biased estimates. This is yet another reason that may explain the low elasticity estimated by some of the existing studies (e.g. Eissa 1995; Eissa and Hoynes 2004; Blundell, Duncan, and Meghir 1998; Kreiner, Leth-Pedersen, and Skov 2016).

Recent studies find evidence of excess mass in the distribution of taxable income at kinks of the tax schedule (bunching) among a minority of workers who do not face these kinks (Chetty, Friedman, Olsen, and Pistaferri 2011, Best 2014). This is interpreted as evidence that firms and unions offer bundles of hours and wages that reflect the preferences of the majority of workers. Differently from existing studies, we use new firm-level data on hours and the variation deriving from an actual tax rate change. Leveraging on these aspects we are able to bring new evidence on coordination as a firm-level mechanism through which changes in preferences over hours spillover to other coworkers. Furthermore, the effects that we find go beyond bunching and can be shown to affect a larger share of workers. In fact, excluding taxpayers close to the major kinks of the Danish tax schedule, the spillovers remain significant and of similar magnitude suggesting that most of the action that we find is among workers who do not bunch (column 5 of Table 1.9).

In Appendix 1.11.4 we check the robustness of the baseline specifications to flexible controls of pre-reform income. Overall we find that the effects are not extremely sensitive to these controls. In addition, in the appendix we present a set of additional results and robustness checks that include the estimation of attenuating and spillover effects based on an alternative database on hours worked, the use of other measures of coordination and the estimation of specifications that separate the uncompensated elasticity from the income elasticity (Appendix 1.11.5).

1.6 Conclusions

This paper explores how the coordination of hours affects the firm-component of wages. Our findings indicate that coordination strongly correlates with wage differentials across firms. Moving forward, future work might investigate how coordination is associated to other dimensions that are linked to firm wage inequality such as, for instance, the gender gap (Card, Cardoso, and Kline 2016).

We also find attenuated responses to tax changes in high-coordination firms and spillovers on the supply of hours of coworkers not targeted by a tax reform. These suggest that the labor supply elasticity of the workers directly targeted by a tax reform captures only a part of the efficiency costs of a tax change. Therefore, future research and policy evaluations should take these effects into account when assessing the excess burden associated to a tax reform.

Finally, the implications of our results go beyond tax reforms and apply to any policy intervention that affects the preferences over hours of one group of workers in a firm. For instance, policies that target the supply of hours of older workers might indirectly affect the supply of hours of younger coworkers. Similarly, policies that directly affect workers with children may have spillovers on other coworkers. It would be interesting to evaluate, in these other settings, the effects of coordination of hours among workers with similar skills and incomes.

1.7 Acknowledgments

Chapter 1 is currently being prepared for submission for publication of the material. Labanca, Claudio; Pozzoli, Dario."Coordination of Hours within the Firm". The dissertation author was the principal researcher and author of this paper.

1.8 Figures and Tables

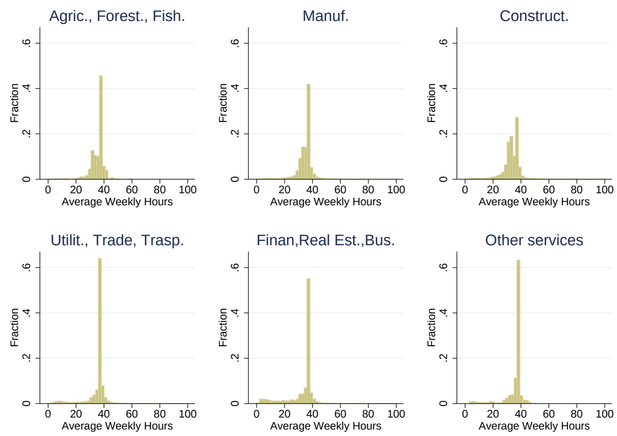


Figure 1.1: The distribution of hours across sectors in Denmark

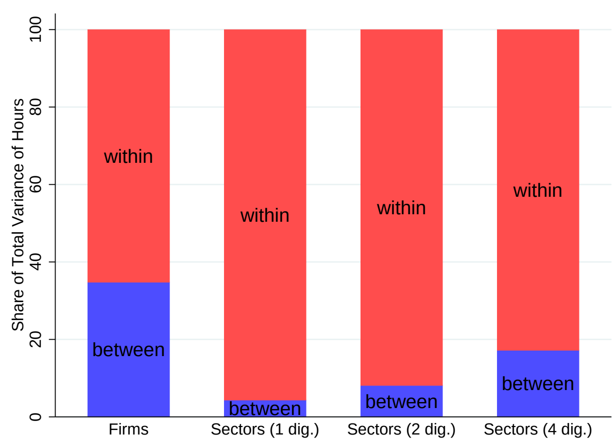


Figure 1.2: Variance of hours decomposition: between and within component

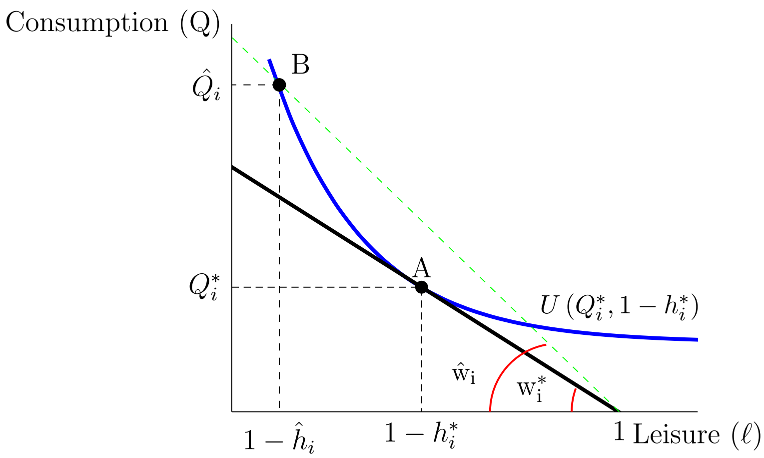


Figure 1.3: Wage rates and hours worked

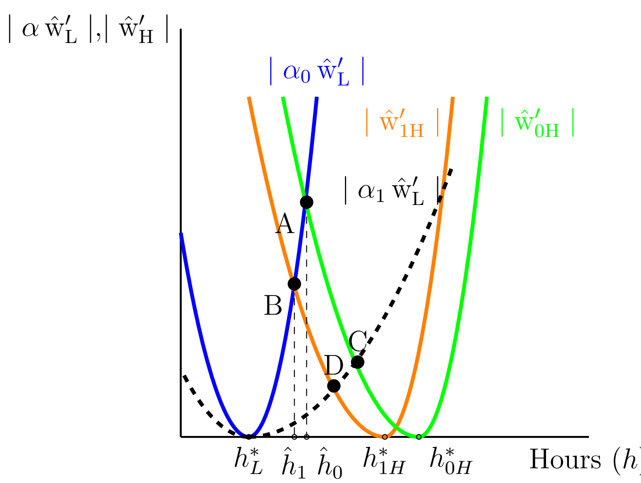


Figure 1.4: The effects of a tax rate change on wages

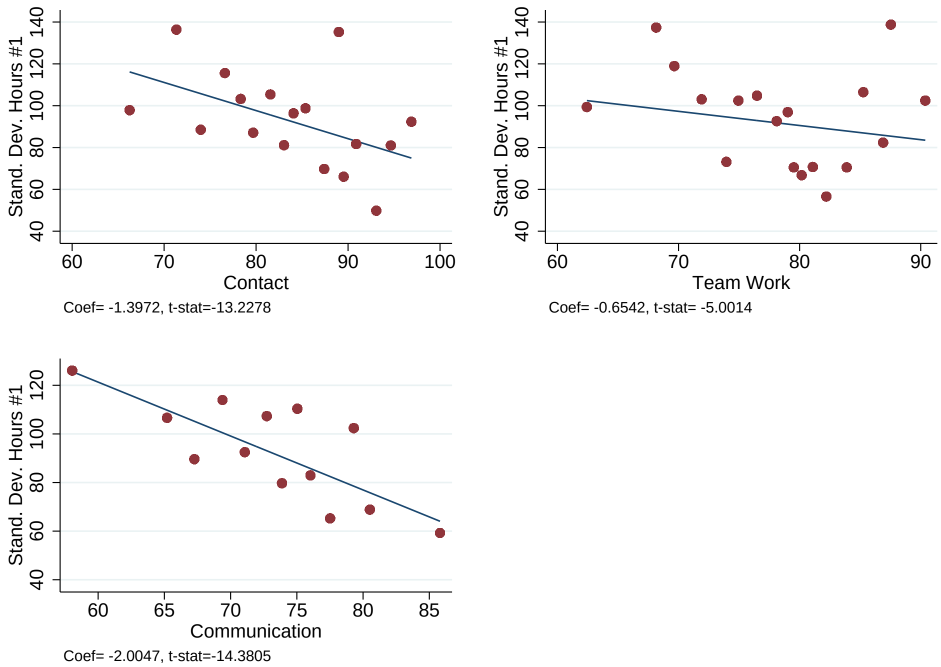


Figure 1.5: Validation: Standard Deviation of Hours vs Coordination in O*NET

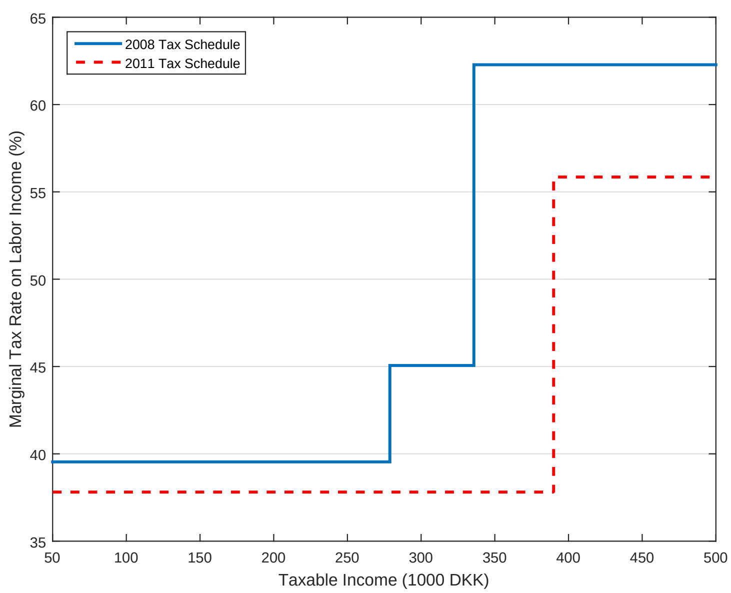


Figure 1.6: The Danish Tax Schedule

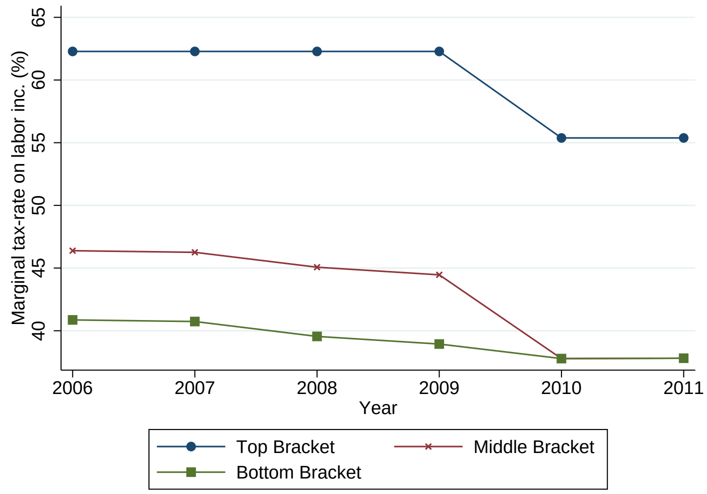


Figure 1.7: The evolution of the marginal tax rate on labor income

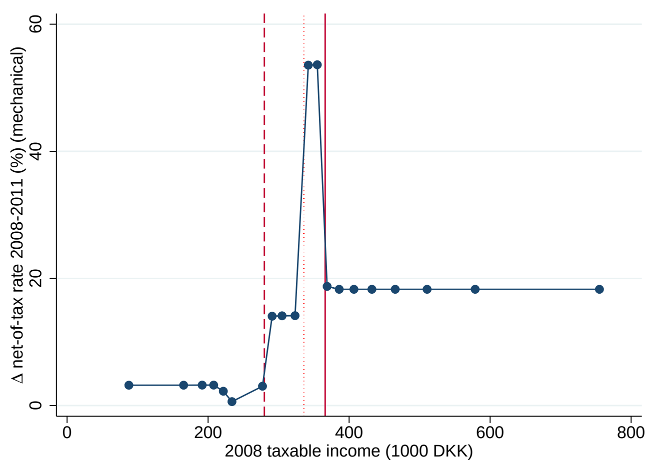


Figure 1.8: Mechanical marginal net-of-tax rate change across taxable income

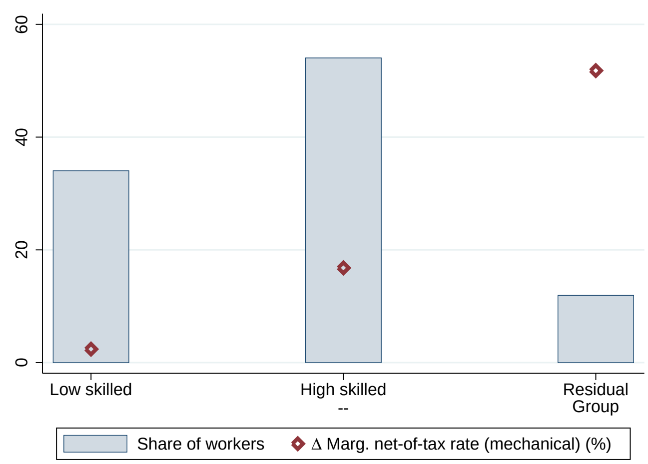


Figure 1.9: Average (mechanical) marginal net-of-tax rate change across groups

Table 1.1: Descriptive Statistics

	IDA Sample		**IDA -Firmstat-LON sample**		**Final sample**	
	(1)	(1)	(2)	(2)	(3)	(3)
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Workers Characteristics						
Mean Age	39.82	12.87	41.11	11.09	42.05	10.91
Fraction < 30 years old	0.27	0.44	0.19	0.39	0.16	0.37
Fraction > 50 years old	0.27	0.44	0.25	0.43	0.27	0.45
Fraction Males	0.50	0.50	0.66	0.47	0.70	0.46
Fraction Unionized	0.70	0.46	0.73	0.44	0.77	0.15
Fraction Hourly	0.17	0.37	0.24	0.42	0.28	0.45
Fraction Primary Educ.	0.33	0.47	0.28	0.45	0.29	0.45
Fraction Secondary Educ.	0.40	0.49	0.52	0.50	0.51	0.50
Fraction Tertiary Educ.	0.27	0.43	0.20	0.39	0.20	0.39
Hourly wage (in DKK)			187.07	141.14	183.65	124.37
Annual Labor Income (in 1000 DKK)	267.00	448.30	357.93	288.35	349.36	248.68
Total Annual Hours			1907.99	213.01	1896.19	197.24
Overtime Annual Hours			27.82	95.55	27.62	87.60
Workers by sector (% of total)						
Agriculture, forestry and fishing, mining and quarrying	2.52		0.37	6.05	0.16	4.00
Manufacturing	26.60		32.48	46.83	35.73	47.92
Construction	10.35		8.67	28.15	9.43	29.23
Electricity, gas, steam and air conditioning supply,						
Trade and transport	30.14		43.46	49.57	40.82	49.15
Financial and insurance, Real estate, Other business	22.95		14.82	35.53	13.71	34.39
Other services	7.44		0.2	4.46	0.15	3.92
Firms Characteristics						
Mean Firm Size			51.42	328.24	43.37	302.3649
Mean Capital per employee (1000 DKK)			423.49	7339.72	963.66	43505.13
Mean Value Added per employee (1000 DKK)			436.30	3040.25	504.30	1773.43
Mean Revenues per employee (1000 DKK)			1687.35	6511.18	2132.89	8693.84
Exporters (%)			39.40	48.86	39.96	48.98
Number of observations	22,379,298		4,466,676		787,683	
Number of individuals	3,518,236		1,205,301		400,653	
Number of firms	266,196		25,249		8,369	

Notes: The table shows the mean and the standard deviations for a set of variables on 3 groups of employees. In all 3 groups we only consider workers who are between 15 and 65 years old in the years 2003-2011. The "IDA Sample" refers to the entire Danish population. The "IDA-Firmstat-LON" sample refers to the sample of workers in IDA that can be matched to Firmstat and LON. The "Final sample" is composed of all the workers from IDA-Firmstat-LON who are employed in firms in which information on hours is available for at least 95% of the workforce. Data on employment by industry for the entire population are from Statistikbanken (Statistics Denmark) that does not provide standard errors around mean values. Annual and hourly earnings, value added, capital and sales are expressed in Danish Kroner (DKK) and deflated using the CPI index with base year 2000 (1 DKK $\simeq$ 8 USD in 2000).

Table 1.2: Coordination and Firm Characteristics

	Stand. Dev. Of Total Hours		**Obs.**
	(1)	(2)	
V.A. /employee	-0.038***	-0.013**	17807
	(0.008)	(0.006)	
Capital/employee	-0.006	-0.005***	17807
	(0.007)	(0.001)	
Sales/employee	-0.040***	-0.014	17807
	(0.007)	(0.009)	
TFP	-0.133***	-0.080***	16212
	(0.008)	(0.012)	
Firm size	-0.032***	-0.095***	17807
	(0.007)	(0.021)	
Share of tertiary educ.	-0.178***	-0.080***	17807
	(0.007)	(0.013)	
Exporter status	-0.141***	-0.005	17807
	(0.007)	(0.009)	
Fraction of hourly work.	0.337***	0.257***	17807
	(0.007)	(0.016)	
Fraction of Unionized work.	0.084***	0.017	17807
	(0.008)	(0.012)	
Fraction of Females	-0.035***	0.035**	17807
	(0.008)	(0.015)	
Fraction of Part-Time work	0.225***	0.120***	17807
	(0.008)	(0.014)	
Mean Managerial Ability	-0.069***	-0.019*	16420
	(0.008)	(0.012)	
Negotiation	-0.310***	-0.146***	13441
	(0.009)	(0.016)	
Persuasion	-0.313***	-0.153***	13441
	(0.009)	(0.016)	
Social Perceptiveness	-0.289***	-0.116***	13441
	(0.009)	(0.015)	
Adjust Actions to others	-0.160***	-0.077***	13441
	(0.009)	(0.013)	
			13441
5 digits industry f.e.	NO	YES	

*Notes: The table shows standardized coefficients from a regression of the standard deviation of hours across skill groups (Section 1.4.3) on firm characteristics. Each cell in the table corresponds to a different regression. In column 2 we add 5-digit industry fixed effects to the baseline classification. We use the Danish industry classification DB07 that for the first 4-digit corresponds to NACE rev.2. Regressions are based on firm-year observations from the firms in our final sample (Table 1.1) over the years 2003-2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. TFP (Total Factor Productivity) is obtained following Ackerberg, Caves, and Frazer. 2015 (Appendix 1.10.4). To avoid confusion we label the O*NET descriptor "Coordination" as "Adjust Actions to Others". Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.3: Coordination by sector

	Stand. Dev. Of Total Hours		Unionization rate
Coordination by Industry (2003-2011)			
	Mean	Std. Dev.	
Agriculture, forestry and fishing, mining and quarrying	118.69	90.47	0.71
Manufacturing	104.08	86.92	0.77
Constructions	140.70	104.12	0.72
Utilities,Trade and Transport	76.04	88.49	0.64
Financial and insurance, Real estate, Other business services	84.72	84.09	0.63
Other services	65.20	57.37	0.71
Overall sectors	95.59	94.00	0.68
Observations	8182		

Notes: The first 2 columns of the table show the mean and standard deviation of the standard deviation of hours across skill groups (Section 1.4.3) in each of the 6 major sectors of the Danish economy. The last column shows the average share of workers unionized in each sector. For each firm in the sample (8182 total) and in each year (2003-2011) we compute the share of workers unionized and the standard deviation of hours across skill groups within that firm-year. Then we take the average (and standard deviations) within each sector.

Table 1.4: Coordination and wage premiums

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.	(6) Firm f.e.
Stand. Dev.	-0.075***	-0.053***	-0.066***	-0.090***		-0.041**
	(0.016)	(0.016)	(0.018)	(0.018)		(0.015)
Stand. Dev. Normal Hours					-0.070***	
					(0.016)	
Firm size		0.014*	0.010	0.033***	0.010	0.011
		(0.007)	(0.007)	(0.010)	(0.007)	(0.007)
Exporter status		0.061***	0.059***	0.054**	0.059***	0.049***
		(0.015)	(0.016)	(0.021)	(0.016)	(0.013)
Union. Rate		-0.002	0.031	0.035	0.030	0.062**
		(0.027)	(0.024)	(0.031)	(0.024)	(0.027)
Female Share		-0.055	-0.109**	-0.126***	-0.106**	-0.086***
		(0.045)	(0.043)	(0.041)	(0.043)	(0.022)
Average Hours		0.004	0.004	0.015	0.004	-0.041
		(0.025)	(0.026)	(0.024)	(0.025)	(0.028)
log(Cap/empl)		0.039***	0.024*	0.049***	0.024*	0.032***
		(0.012)	(0.013)	(0.014)	(0.013)	(0.012)
Negotiation						0.348***
						(0.105)
Persuasion						-0.259***
						(0.093)
Social Perceptiveness						0.008
						(0.036)
Adjust Actions to others						0.017
						(0.017)
Region F.E.	NO	YES	YES	YES	YES	YES
Compos. cntr	NO	NO	YES	YES	YES	YES
Ability Measures	NO	NO	YES	YES	YES	YES
Av. Hours b/w 36.5 and 37.5	YES	YES	YES	NO	YES	YES
Part. R-sq SD Hours	0.008	0.003	0.006	0.008	0.007	0.002
Part. R-sq VA and TFP	0.022	0.010	0.032	0.038	0.032	0.020
Coordination Share	0.349	0.321	0.200	0.196	0.233	0.097
R-sq	0.008	0.033	0.106	0.126	0.108	0.135
N	7312	7312	7312	4415	7299	6089

*Notes: In this table we show the results of estimating equation (1.7). The dependent variable is the firm fixed effect from the AKM model (1.8). Hours coordination is measured using the standard deviation of the average total (regular and overtime) hours worked across skill groups within a firm (labelled as "Stand. Dev.", see also Section 1.4.3). The "Stand. Dev. Normal hours" is the standard deviation of the average regular hours worked across skill groups within a firm. Skill groups are defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM model (1.8). All regressions show standardized coefficients. The exporter dummy is defined as the modal exporter status between 2003 and 2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. "Compos. cntr" refers to a vector of controls for the share of workers in each skill group. "Ability Measures" indicate a vector containing the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. The dependent variable (firm f.e.) in column (5) is based wage rates from regular hours only. To avoid confusion we label the O*NET descriptor "Coordination" as "Adjust Actions to Others". Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and TFP" (Section 1.4.1). "Part. R-sq VA and TFP" is from Table 1.19. Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.5: Coordination and wage differentials within sectors

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.	(6) Firm f.e.	(7) Firm f.e.	(8) Firm f.e.
Stand. Dev.	-0.060***	-0.031*	-0.028*				-0.064***	-0.018
	(0.018)	(0.016)	(0.016)				(0.019)	(0.017)
Median Abs. Dev.				-0.075***	-0.045***	-0.040**		
				(0.014)	(0.014)	(0.015)		
Firm size	0.009	0.006	0.017*	0.010	0.006	0.018*	0.011	0.010*
	(0.006)	(0.005)	(0.009)	(0.007)	(0.005)	(0.009)	(0.008)	(0.005)
Exporter status	0.065***	0.030**	0.021	0.062***	0.029**	0.020	0.063***	0.032**
	(0.018)	(0.013)	(0.013)	(0.018)	(0.013)	(0.013)	(0.015)	(0.014)
Union. Rate	0.040	0.039	0.039	0.042	0.040	0.040	0.032	0.051**
	(0.025)	(0.029)	(0.030)	(0.025)	(0.029)	(0.030)	(0.024)	(0.022)
Female Share	-0.140***	-0.069**	-0.057*	-0.140***	-0.069**	-0.057*	-0.113***	-0.120***
	(0.040)	(0.027)	(0.029)	(0.038)	(0.026)	(0.028)	(0.042)	(0.034)
Average Hours	-0.006	-0.033	-0.039*	-0.018	-0.038*	-0.043**	0.001	-0.034
	(0.022)	(0.023)	(0.023)	(0.021)	(0.021)	(0.021)	(0.026)	(0.022)
log(Cap/empl)	0.028**	0.031***	0.035***	0.028**	0.030***	0.035***	0.022*	-0.089***
	(0.013)	(0.010)	(0.010)	(0.013)	(0.010)	(0.010)	(0.013)	(0.023)
log(VA/empl)								0.381***
								(0.070)
1 digit Sector f.e.	YES	NO	NO	YES	NO	NO	NO	NO
2 digits Sector f.e.	NO	YES	NO	NO	YES	NO	YES	YES
3 digits Sector f.e.	NO	NO	YES	NO	NO	YES	YES	YES
Part. R-sq SD Hours	0.004	0.001	0.001	0.006	0.002	0.001	0.009	
Part. R-sq VA and TFP	0.033	0.016	0.014	0.033	0.016	0.014		
Coordination Share	0.113	0.049	0.042	0.181	0.113	0.095		
R-sq	0.113	0.155	0.162	0.115	0.156	0.162	0.112	0.104
N	7306	7306	7306	7306	7306	7306	7060	7060

*Notes: In this table we show the results of estimating equation (1.7). The dependent variable is the firm fixed effect from the AKM model (1.8). "Stand. Dev." in the table is the standard deviation of the average total (regular and overtime) hours worked across skill groups within a firm (Section 1.4.3). The "Median Abs. Dev." is the the median absolute deviation of median hours across skill groups within a firm. Skill groups are defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM model (1.8). All regressions show standardized coefficients. The exporter dummy is defined as the modal exporter status between 2003 and 2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. "Compos. cntr" refers to a vector of controls for the share of workers in each skill group. "Ability Measures" indicate a vector containing the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. In column (8) TFP is used as an instrument for valued added per employee (log(V.A./empl)). TFP is obtained as in Ackerberg, Caves, and Frazer. 2015 (Appendix 1.10.4). Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and TFP" (Section 1.4.1). "Part. R-sq VA and TFP" is from Table 1.20. Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.6: The elasticity of hours of high-skilled workers

	(1)	(2)	(3)	(4) High Coord.	(5) Low Coord.	(6) High Coord.	(7) Low Coord.
Dependent Variable	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$
$\Delta log\,(1-\tau^H)$	-0.067***	-0.069***	-0.047***	-0.017	-0.097***	-0.008	-0.062**
	(0.008)	(0.018)	(0.014)	(0.016)	(0.025)	(0.013)	(0.025)
Log base-year income			-0.008***	-0.002	-0.023***	-0.002*	-0.022***
			(0.003)	(0.003)	(0.006)	(0.001)	(0.006)
IV	NO	YES	YES	YES	YES	YES	YES
Overtime Hours	YES	YES	YES	YES	YES	NO	NO
Mean Hours	1924.47	1924.47	1924.47	1928.33	1914.91	1900.34	1858.41
Pvalue High=Low				0.01		0.06	
F-stat Excl. Inst.		1355.19	754.51	1293.74	192.94	1293.74	192.94
P-value Excl. Inst.		0.00	0.00	0.00	0.00	0.00	0.00
N Firms	1167	1167	1167	584	583	584	583
N	26488	26488	26488	18875	7613	18875	7613

Notes: This table reports the results from estimating equation (1.10). It shows the elasticity of high-skilled hours to the net-of-tax rate (1-τ^H). In columns 4 to 7 we distinguish between high and low-coordination firms. High-coordination firms are in the bottom half of the distribution of the standard deviation of hours across skill groups in 2008, and conversely low-coordination firms are in the top half. Specifications in columns 2 to 7 use mechanical changes of the net-of-tax rate on labor income as an instrument for observed changes of 1-τ^H (Section 1.5.5). First Stage Regressions in Table 1.31. Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). "P-value High=Low" refers to the p-value of the null hypothesis that the coefficient attached to $\Delta log\,(1-\tau^H)$ in low and high-coordination firms is equal. We only consider high-skilled workers who are at the same firm between 2008 and 2011, and in firms that employ at least 1 low-skilled worker. We estimate this regression on 3 years changes between 2008 and 2011. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. p<0.10, ** p<0.05, *** p<0.01.*

Table 1.7: Elasticity of high-skilled hours: additional specifications

	(1) High Coord. Top 25%	(2) Low Coord. Bottom 25%	(3) High Coord.	(4) Low Coord.	(5) High Coord.	(6) Low Coord.
Dependent Variable	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$
$\Delta log\,(1-\tau^H)$	0.003	-0.147***	-0.027	-0.075***	-0.011	-0.050*
	(0.018)	(0.055)	(0.017)	(0.026)	(0.014)	(0.027)
Log base-year income	-0.001	-0.038*	-0.006*	-0.019***	-0.003	-0.016***
	(0.003)	(0.022)	(0.004)	(0.004)	(0.002)	(0.004)
Overtime hours	NO	NO	YES	YES	NO	NO
Firm F.E.	NO	NO	YES	YES	YES	YES
Base-year F.E.	NO	NO	YES	YES	YES	YES
Mean Hours	1917.40	1870.33	1935.47	1922.85	1901.60	1864.17
Pvalue High=Low	0.01		0.02		0.06	
F-stat Excl. Inst.	566.19	133.53	1542.40	353.25	1542.40	353.25
P-value Excl. Inst.	0.00	0.00	0.00	0.00	0.00	0.00
N Firms	293	291	785	675	785	675
N	8307	2371	26497	10267	26497	10267

*Notes: This table reports the results from estimating equation (1.10). It shows the elasticity of high-skilled hours to the net-of-tax rate (1-τ^H). In columns 1 and 2 we only consider respectively firms in the bottom 25% and top 25% of the distribution of the standard deviation of hours across skill groups in 2008. In the other columns we distinguish between high and low-coordination firms based on whether the firm in respectively in the bottom or top half of the distribution of the standard deviation of hours across skill groups in 2008. All specifications use mechanical changes of the net-of-tax rate on labor income as an instrument for observed changes of 1-τ^H (Section 1.5.5). First Stage Regressions are in Table 1.32. Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). "P-value High=Low" refers to the p-value of the null hypothesis that the coefficient attached to $\Delta log\,(1-\tau^H)$ in low and high-coordination firms is equal. We only consider high-skilled workers who are at the same firm between 2008 and 2011, and in firms that employ at least 1 low-skilled worker. In column 1 and 2 we consider 3 years changes between 2008 and 2011. In columns 3 to 6 we consider 3 years changes over the period 2006-2011. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.8: The spillover effects on hours worked by low-skilled

	(1)	(2)	(3)	(4)	(5) Low Coord.	(6)	(7) Low Coord.
Dependent Variable	$\Delta log\, h^L$	$\Delta log\, h^L$	$\Delta log\, h^L$	$\Delta log\, h^L$	$\Delta log\, h^L$	$\Delta log\, h^L$	$\Delta log\, h^L$
$\Delta log\, \overline{h^H_{normal}}$	0.540***	0.899***	0.878***	0.894**	0.624**		
	(0.112)	(0.304)	(0.301)	(0.373)	(0.297)		
$\Delta log\, \overline{h^H_{total}}$						1.375**	0.706**
						(0.612)	(0.345)
$\Delta log\,(1 - \tau^L)$	-0.005	0.023	0.051	0.053	-0.060	0.056	-0.053
	(0.009)	(0.088)	(0.114)	(0.126)	(0.115)	(0.138)	(0.115)
IV	NO	YES	YES	YES	YES	YES	YES
Region F.E.	YES	YES	YES	YES	YES	YES	YES
Splines of log t-1 Inc. and Δlog inc. t-1-t	NO	NO	YES	YES	YES	YES	YES
Log Mean Inc. High Sk.	NO	NO	NO	YES	NO	NO	NO
Overtime Hours	NO	NO	NO	NO	NO	YES	YES
F-stat Excl. Inst.		13.09, 160.40	15.45,76.76	4.66, 55.84	11.90, 48.55	4.43, 76.72	8.39, 50.92
P-value Excl. Inst.		0.00, 0.00	0.00, 0.00	0.03,0.00	0.00, 0.00	0.04, 0.00	0.00, 0.00
Mean Hours Low Sk.	1812.51	1812.51	1812.51	1812.51	1742.05	1828.87	1760.74
Mean Hours High Sk.	1875.00	1875.00	1875.00	1875.00	1846.56	1905.60	1879.90
N Firms	968	968	968	968	484	968	484
N	10091	10091	10091	10091	4100	10091	4100

Notes: This table reports the results from estimating equation (1.11). It shows the elasticity of low-skilled hours to the average hours worked by high-skilled coworkers. We consider both regular (normal) hours (columns 1 to 5) and total (regular and overtime) hours (columns 6 and 7). Specifications in columns 2 to 7 use mechanical changes of the average net-of-tax rate among high-skilled in a firm as an instrument for the average change in hours, and the mechanical change of the net-of-tax rate of low-skilled as an instrument for observed changes of 1-τ^L (Section 1.5.5). First Stage results are in Table 1.33. Low-coordination firms (columns 5 and 7) are defined as being in the top half of the distribution of the standard deviation of hours across skill groups in 2008. Each regression contains the following controls measured in the base year: work experience,work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). "Splines" refer to a flexible piecewise linear functional form with 5 components. We only consider low-skilled workers who are at the same firm between 2008 and 2011. We estimate this regression on 3 years changes between 2008 and 2011. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. $p<0.10$, ** $p<0.05$, *** $p<0.01$.*

Table 1.9: The spillover effects on low-skilled hours: additional specifications

	(1) $\Delta log\, h^L$	(2) $\Delta log\, h^L$	(3) $\Delta log\, h^L$	(4) $\Delta log\, h^L$	(5) $\Delta log\, h^L$
$\Delta log\, \overline{h}^H_{normal}$	0.958	0.888***		0.983**	0.893***
	(0.997)	(0.333)		(0.445)	(0.303)
$\Delta log\, \overline{h^H}$ × (Share High Sk.>50)	0.083				
	(1.126)				
$\Delta log\, \overline{h}^H_{total}$			1.217**		
			(0.576)		
$\Delta log\, \overline{h}^{Residual}_{normal}$				-0.179	
				(0.567)	
Share High Sk.>50	-0.005				
	(0.009)				
$\Delta log\, (1 - \tau^L)$	0.020	0.163*	0.151	0.026	0.064
	(0.115)	(0.088)	(0.094)	(0.069)	(0.116)
Overtime hours	NO	NO	YES	NO	NO
Firm f.e.	NO	YES	YES	NO	NO
Base-year f.e.	NO	YES	YES	NO	NO
Workers at kinks	YES	YES	YES	YES	NO
Mean Hours Low Sk.	1813.05	1815.25	1833.23	1811.60	1811.95
Mean Hours High Sk.	1875.14	1873.63	1906.57	1877.83	1874.93
F-stat Excl. Inst.	1.20,71.31,37.34	6.23, 24.55	2.45, 25.57	122.94, 12.16, 4.41	13.97, 77.48
P-value Excl. Inst.	0.27,0.00,0.00	0.01, 0.00	0.12, 0.00	0.00, 0.00, 0.04	0.00,0.00
N Firms	977	835	835	799	958
N	10196	15985	15985	9606	9979

Notes: This table reports the results from estimating variants of equation (1.11). We consider both regular (normal) hours (columns 1, 2, 4 and 5) and total hours (column 3). In column 1 we interact the average change of high-skilled regular hours with a dummy if a firm has a share of high-skilled greater than 50% in 2008. All specifications use mechanical changes of the average net-of-tax rate among high-skilled in a firm as an instrument for the average change in hours, and the mechanical change of the net-of-tax rate of low-skilled as an instrument for observed changes of 1-τ^L (Section 1.5.5). First Stage results are in Table 1.34 and Table 1.35. In column 4 we also consider change in average hours among workers in the residual group within the same firm. We instrument for the average change in hours in this group using the average mechanical change of the net-of-tax rate among workers in the residual group. Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm and 5 components splines of income at t-1 and income change between t-1 and t. Workers close to the kink points (column 5) are defined as having taxable income within 5,000 DKK of the top kink or 2,000 DKK of the bottom kink (Kleven and Schultz 2014). In evaluating the closeness of workers to kinks, base year income is measured in 2005 DKK (1DKK ≃ 6 USD in 2005). Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. p<0.10, ** p<0.05, *** p<0.01.*

1.9 Supplementary derivations

1.9.1 The optimal demand of consumption and leisure

Workers with skill i maximize utility (1.1) given an hourly wage rate w_i and an income tax rate t_i, facing the budget constraint

$$E_i \equiv \int_{\omega\in\mathbf{\Omega}} p(\omega)q_i(\omega)\,\mathrm{d}\omega \;\leq\; h_i\mathrm{w}_i(1-t_i)+T+\bar{\pi} \;\equiv Y_i, \tag{1.9.1}$$

where E_i is expenditure, Y_i is after-tax income under a lump-sum transfer T that balances the government's budget (there are no other government expenditures), and $\bar{\pi} \equiv \int_{\omega\in\mathbf{\Omega}} \pi(\omega)\mathrm{d}\omega/(n_H+n_L)$ represents the equal distribution of firm profits as dividends. A worker i's optimal product demand then is

$$q_i^*(\omega) = \left[\frac{p(\omega)}{P}\right]^{-\sigma} Q_i, \tag{1.9.2}$$

and labor supply is implicitly given by

$$\eta\, v'\,(\ell^*\,) = \frac{\mathrm{w_i^*}\,(1-t_i)}{P\,Q}, \tag{1.9.3}$$

for the (exponentiated) price index $P^{\sigma-1} \equiv \int_{\omega\in\mathbf{\Omega}} p(\omega)^{-(\sigma-1)}\,\mathrm{d}\omega$. Finally note that, in optimum, $E_i = PQ_i$.

1.9.2 Wage-hours function and optimal hours: the case of an additive separable utility function

Since the indifference condition (1.2) implicitly defines the wage rate as a function of the hours worked, it can be used to express $\hat{\mathrm{w}}'(\hat{h})$ in term of marginal utilities. Thus starting from:

$$\Phi(\hat{\mathrm{w}}_{\mathrm{i}}, \hat{h}) = U\left(\mathrm{P}^{-1}\hat{\mathrm{w}}_{\mathrm{i}}\left(1-t_i\right)\hat{h} + P^{-1}(T\bar{\pi}), 1-\hat{h}\right) - U\left(\mathrm{w}_{\mathrm{i}}^{*}\left(1-t_i\right)h_i^{*} + +P^{-1}(T\bar{\pi}), 1-h_i^{*}\right) = 0, \tag{1.9.4}$$

we have:

$$\hat{\mathrm{w}}_i'(\hat{h}) = -\left(\frac{\partial\Phi(\hat{\mathrm{w}}_{\mathrm{i}},\, \hat{h})}{\partial\hat{h}}\right)\left(\frac{\partial\Phi(\hat{\mathrm{w}}_{\mathrm{i}},\, \hat{h})}{\partial\hat{\mathrm{w}}_{\mathrm{i}}}\right)^{-1} = -\frac{[P^{-1}U_C\hat{\mathrm{w}}_{\mathrm{i}}(1-t_i) - U_\ell]}{P^{-1}U_C\hat{h}(1-t_i)}. \tag{1.9.5}$$

Under decreasing marginal rates of substitution

$$\hat{\mathrm{w}}_i'(\hat{h}) = -\frac{[P^{-1}U_C\hat{\mathrm{w}}_{\mathrm{i}}(1-t_i) - U_\ell]}{P^{-1}U_C\hat{h}(1-t_i)} \begin{cases} < 0 & \text{if } \hat{h} < h_i^{*} \\ = 0 & \text{if } \hat{h} = h_i^{*} \\ > 0 & \text{if } \hat{h} > h_i^{*} \end{cases} . \tag{1.9.6}$$

Assuming that the utility function is additive separable as in (1.1), the second derivative of the wage rate with respect to hours is:

$$\hat{\mathrm{w}}_i''(\hat{h}) = -\left[\frac{\hat{\mathrm{w}}_i'\,\hat{h} - \hat{\mathrm{w}}_i}{\hat{h}^2}\right] - \left[\frac{P}{\hat{h}^2(1-t_i)}\right]\frac{U_\ell}{U_C} - \frac{U_C U_{ll} + U_{CC}U_\ell\left[P^{-1}\hat{\mathrm{w}}_i'\,\hat{h}\,(1-t_i) + P^{-1}\hat{\mathrm{w}}_{\mathrm{i}}(1-t_i)\right]}{P^{-1}U_C^2(1-t_i)\,\hat{h}}. \tag{1.9.7}$$

Thus rearranging the terms in (1.9.7) we have[35]:

$$\hat{\mathrm{w}}_i''(\hat{h}) = -\frac{2}{\hat{h}}\,\hat{\mathrm{w}}_i' - \frac{U_C U_{ll} + U_{CC}U_\ell\left[P^{-1}\hat{\mathrm{w}}_i'\,\hat{h}\,(1-t_i) + P^{-1}\hat{\mathrm{w}}_i\,(1-t_i)\right]}{P^{-1}U_C^2\,(1-t_i)\,\hat{h}}. \tag{1.9.8}$$

In (1.9.8) we notice that:

$$\begin{aligned}\left[P^{-1}\hat{\mathrm{w}}_i'\,\hat{h}\,(1-t_i) + P^{-1}\hat{\mathrm{w}}_i\,(1-t_i)\right] &= \\ \frac{-P^{-1}U_C\hat{\mathrm{w}}_{\mathrm{i}}(1-t_i) + U_\ell + P^{-1}U_C\hat{\mathrm{w}}_{\mathrm{i}}(1-t_i)}{U_C} = \frac{U_\ell}{U_C} &> 0.\end{aligned} \tag{1.9.9}$$

Assuming $U_C > 0$, $U_\ell > 0$, $U_{CC} < 0$ and $U_{ll} < 0$, it follows that the second term in (1.9.8):

$$-\frac{U_C U_{ll} + \frac{U_{CC}U_\ell^2}{U_C}}{P^{-1}U_C^2(1-t_i)\,\hat{h}} > 0. \tag{1.9.10}$$

(1.9.10) captures the loss in terms of marginal utilities from working one extra hour. This loss requires wage rates to increase at an increasing rate when hours go up.

[35] The rearrangement here consists in substituting (1.9.5) into the first term on the right hand side of (1.9.7). Then we take the sum of the first two terms. To gain a more transparent intuition of the results, I then express the sum of the first two terms in (1.9.7) in terms of w'(h).

Combining (1.9.10) and (1.9.8) we have:

$$\hat{\mathrm{w}}_i''(\hat{h}) = -\frac{2}{\hat{h}}\,\hat{\mathrm{w}}_i' - \frac{U_C U_{ll} + \frac{U_{CC}U_\ell^2}{U_C}}{P^{-1}U_C^2(1-t_i)\,\hat{h}}. \tag{1.9.11}$$

If $\hat{h} = h^*$ since $\hat{\mathrm{w}}_i'(\hat{h}) = 0$ then $\hat{\mathrm{w}}_i''(\hat{h}) > 0$. If $\hat{h} < h_i^*$ then $\hat{\mathrm{w}}_i'(\hat{h}) < 0$ and $\hat{\mathrm{w}}_i''(\hat{h}) > 0$. Finally, if $\hat{h} > h_i^*$ then $\hat{\mathrm{w}}_i'(\hat{h}) > 0$ and the sign of $\hat{\mathrm{w}}_i''(\hat{h})$ is ambiguous. Using (1.9.5) to rearrange (1.9.11) $\hat{\mathrm{w}}_i'' > 0$ implies:

$$2\frac{\hat{\mathrm{w}}_i(1-t_i)}{P} > \frac{U_\ell}{U_C} + \frac{U_{\ell\ell}}{U_C} - \frac{U_{CC}U^2}{U_C^2}. \tag{1.9.12}$$

This is the case when P is particularly small and/or $U_{\ell\ell}$ particularly high.

1.9.3 Optimal hours worked in coordinated firms: derivations

The first order condition relative to the minimization problem of section 1.2.3 are:

$$\hat{\mathrm{w}}_{\mathrm{L}}'\hat{h}\hat{n}_L + \mathrm{w}_{\mathrm{L}}\hat{n}_L + \hat{\mathrm{w}}_{\mathrm{H}}'\hat{h}\hat{n}_H + \hat{\mathrm{w}}_{\mathrm{H}}\hat{n}_H = G_H\hat{n}_H + G_L\hat{n}_L, \tag{1.9.13}$$

$$G_H = \hat{\mathrm{w}}_{\mathrm{H}}(\hat{h}), \tag{1.9.14}$$

$$G_L = \hat{\mathrm{w}}_{\mathrm{L}}(\hat{h}), \tag{1.9.15}$$

$$\hat{\gamma}\phi\, G(\hat{n}_L\, \hat{h},\, \hat{n}_H\, \hat{h}) = \hat{q}(\omega). \tag{1.9.16}$$

Replacing G_H from (1.9.14) and G_L from (1.9.15) into (1.9.13) we obtain

$$\hat{\mathrm{w}}'_{\mathrm{H}}(\hat{h})\hat{n}_H\hat{h} + \hat{\mathrm{w}}'_{\mathrm{L}}\hat{n}_L\hat{h} = 0, \tag{1.9.17}$$

dividing by $\hat{h}$ we obtain condition (1.4).

The optimality condition (1.4) implicitly defines optimal hours in coordinated firms as a function of the marginal tax rate faced by high-skilled workers. Thus it can be used to obtain the derivative of $\hat{h}$ with respect the tax rate t_H. Defining the implicit function:

$$\Phi_{t_H}(h, t_H) = \hat{\mathrm{w}}'_{\mathrm{H}}(\hat{h}) + \alpha\hat{\mathrm{w}}'_{\mathrm{L}} = 0. \tag{1.9.18}$$

We have:

$$\frac{d\hat{h}}{dt_H} = -\left(\frac{\partial\Phi_{t_H}}{\partial t_H}\right)\left(\frac{\partial\Phi_{t_H}}{\partial\hat{h}}\right)^{-1}, \tag{1.9.19}$$

using (1.9.5) in solving for the numerator in (1.9.19) gives equation (1.6).

1.9.4 The product market: prices, revenues and profits

A firm producing variety ω maximizes its profits by setting the variety-specific price $p(\omega)$ given total demand. Summing the demand indexes Q_i^* and $\hat{Q}_i$ over all consumers, of different skills and with employment in different labor markets, we arrive at aggregate consumption Q, which firms take as given under monopolistic competition. However, in the product market for their individual variety ω, firms are monopoly price setters, taking demand for their variety into account:

$$q(\omega) = [p(\omega)/P]^{-\sigma} Q,$$

after summing (1.9.2) over all consumer groups.[36] The generic profit maximization problem is

$$\pi(\omega) \equiv \max_{p(\omega)} p(\omega)\, q(\omega) - \frac{\mu}{\gamma\phi}\, q(\omega) - F \qquad \text{s.t.} \quad q(\omega) = \left[\frac{p(\omega)}{P}\right]^{-\sigma} Q, \qquad (1.9.20)$$

where the constant μ is the marginal production cost (given constant returns to scale). Note that $F = 0$, $\gamma = 1$ and $\mu = \mu^*$ in the non-coordinated market, whereas $F = \hat{F}$, $\gamma = \hat{\gamma} > 1$ and $\mu = \hat{\mu}$ for firms that enter the coordinated market. Applying Euler's rule to constant-returns-to-scale production (with homogeneity of degree one in production factors), the minimized cost function in uncoordinated firms takes the form

$$C^*(\omega) = \frac{\mu^*}{\phi}\, q^*(\omega) \qquad \text{with} \quad \mu^* \equiv \mu(\mathrm{w}^*_H, \mathrm{w}^*_L, h^*_H, h^*_L),$$

where μ^* is the Lagrange multiplier of the constrained minimization problem (1.3), and $q^*(\omega) = \phi\, G(n^*_H h^*_H, n^*_L h^*_L)$, whereas the function $\mu(\cdot)$ also depends on the parameters of the production function. In coordinated firms the minimized costs

[36]Concretely, aggregate demand is $Q \equiv \sum_{i=H,L} N^*_i Q^*_i + \hat{N}_i \hat{Q}_i$, where $Q^*_i = E^*_i/P$ and $\hat{Q}_i = \hat{E}_i/P$ with $E^*_i = h^*_i w^*_i(1-t_i) + T$ and $\hat{E}_i = \hat{h}\hat{w}_i(1-t_i) + T$.

function takes the form:

$$\hat{C}(\omega) = \frac{\hat{\mu}}{\hat{\gamma}\phi}\,\hat{q}(\omega) \qquad \text{with} \quad \hat{\mu} \equiv \mu(\hat{\mathrm{w}}_H, \hat{\mathrm{w}}_L; \hat{h}(\eta, P, t_H, t_L; \phi)),$$

where $\hat{\mu}$ is the Lagrange multiplier of the constrained minimization problem in Section 1.2.3 and $\hat{q}(\omega) = \hat{\gamma}\,\phi\,\hat{h}\,G(\hat{n}_H, \hat{n}_L)$. The optimal prices resulting from (1.9.20) are

$$p^*(\omega) = \frac{\sigma}{\sigma - 1}\frac{\mu^*}{\phi} \qquad \text{and} \qquad \hat{p}(\omega) = \frac{\sigma}{\sigma - 1}\frac{\hat{\mu}}{\hat{\gamma}\phi}. \tag{1.9.21}$$

By profit maximization (1.9.20), firms with the same ϕ choose the same optimal price–over-cost markups, production and revenue, regardless of their specific product variety ω. We therefore adopt the simplifying notation that optimal prices are $p(\phi)$, optimal production is $q(\phi)$, and optimal revenues are $p(\phi)q(\phi)$. Summing (1.9.2) over all consumer groups, total demand for a firm's output can be written $q(\phi) = [p(\phi)/P]^{-\sigma}Q$ and the firm's equilibrium revenues are

$$p(\phi)q(\phi) = [p(\phi)/P]^{-(\sigma-1)}PQ = [p(\phi)/P]^{-(\sigma-1)}E,$$

where $E = PQ$ is economy-wide expenditure, aggregated over all consumer groups. By (1.9.20), profits of a firm with productivity ϕ are

$$\pi(\phi) = \frac{p(\phi)q(\phi)}{\sigma} - F = \left[\frac{p(\phi)}{P}\right]^{-(\sigma-1)}\frac{E}{\sigma} - F.$$

Using optimal prices (1.9.21) for non-coordinated and coordinated firms in this profit relationship, we can state a firm ϕ's prospective profits in the two labor market segments as in Section 1.2.3.

1.9.5 Tax changes and wage rates with coordination

In the setting described in Section 1.2, a tax change that affects coordinated hours also affects wage rates through the wage-hours function. The sign of the effect on wages depends on whether the income or the substitution effect prevails and on whether high-skilled desire to work more or less than low-skilled workers. Figure 1.4 shows the case in which the tax rate goes down, the income effect prevails and high-skilled desire to work more (i.e. $h_H^* > h_L^*$). In this case a drop of the tax rate moves the equilibrium from A to B. At the new equilibrium both $\mid \mathrm{w}'_{\mathrm{H}} \mid$ and $\mid \mathrm{w}'_{\mathrm{L}} \mid$ are lower implying lower wage rates for both high and low-skilled workers. Intuitively, the lower supply of hours induced by the tax drop moves low-skilled workers (who work more than desired at the original equilibrium) closer to the optimum. This results in lower wage premiums for low-skilled workers. Turning to high-skilled workers, the reform drives down both their actual and desired hours worked. Actual hours however, decrease less than the desired one, thus shrinking the gap between the optimum and the actual hours. This results in lower wage rates. The other possible cases can be derived following a similar reasoning and they lead to the conclusion that wage rates and hours move together if, in equilibrium, low-skilled prefer to work less than high-skilled, while hours and wages move in opposite directions if low-skilled prefer to work more.

From an empirical point of view however, we do not find significant effects on wages. This may be due to the fact that hours changed too little to trigger a change in wages. It may also be however that wages are stickier than hours and since data after 2011 are not available, we might be unable to capture the variation in wages.

1.9.6 A framework for the empirical model of taxation with spillovers

Similar to Gruber and Saez 2002, we assume that type i workers maximize an utility function that depends on consumption (c) and labor income (z). For simplicity we assume that labor income is given as the product of wage rates and hours worked so that the utility function takes the following form: $U_i\left(c_i,\, h_i \mathrm{w_i}\right)$. Following Kleven and Schultz 2014, we define $c_i = z_i - T_i\left(z\right) = z_i\left(1-\tau_i\right) + y_i$, where $T_i\left(z\right)$ is tax liability, $\tau_i = T_i'()$ and virtual income is defined as $y_i = z_i\tau_i - T_i\left(z\right)$. In uncoordinated firms the wage rate is exogenously set by the market at $\mathrm{w_i} = \mathrm{w_i^*}$. The optimal choice of hours is then a function of the marginal net-of-tax rate, virtual income and the exogenous wage rate: $h_i = h\ \left(1-\tau_i\,, y_i\,, \mathrm{w_i^*}\right)$. In this framework, changes in τ_i and y_i affect the supply of hours as follows:

$$dh_i = -\frac{\partial h}{\partial\left(1-\tau_i\right)}\, d\tau_i + \frac{\partial h}{\partial y_i}\, dy_i \tag{1.9.22}$$

Defining the uncompensated elasticity of hours with respect to the net-of-tax rate as $\alpha_2 = [(1-\tau_i)\,/h_i]\,[\partial h\,/\partial\,(1-\tau_i)]$ and the income elasticity as $\alpha_3 = (1-\tau_i)\;[\partial h\,/\partial y_i]$, then the terms in equation (1.9.22) can be rearranged as:

$$\frac{dh_i}{h_i} = -\alpha_2\,\frac{d\tau_i}{(1-\tau_i)} + \alpha_3\,\frac{dy_i}{h_i\,(1-\tau_i)} \tag{1.9.23}$$

Using a log-log specification, equation (1.9.23) can be estimated as:

$$\Delta log(h_i) = \alpha_0 + \alpha_2\,\Delta log\,(1-\tau_i) + \alpha_3\,\Delta log(y_i) + \varepsilon_i \tag{1.9.24}$$

The compensated elasticity of hours to a net-of-tax rate change can be obtained from α_2 and α_3 using the Slutsky equation: $\zeta^c = \alpha_2 - \alpha_3$.

In case firms coordinate hours among workers then the supply of hours by type i workers in a firm will also depend on the hours worked by other types of workers in the same firm. Hours worked by other types will in turn depend on the net-of-tax rate, the virtual income and the market wage rate that the other types face. We assume there is one type of other workers only that this is indexed as $-i$. Hours worked by type i workers can then be expressed as: $h_i = h\;(1-\tau_i\,, y_i\,, h_{-i}\,, \mathrm{w}_{\mathrm{i}}^*)$, where $h_{-i} = h\;\left(1-\tau_{-i}\,, y_{-i}\,, \mathrm{w}_{-\mathrm{i}}^*\right)$. In defining h_{-i}, we assume that hours worked by type $-i$ workers are independent of the tax rate and virtual income faced by type i workers. This assumption, while restrictive, fits well our empirical setting where tax changes experienced by low-skilled workers (type i)

are of small magnitude and do not affect hours worked by high-skilled (type $-i$) in a significant way. We assume that the assignment of workers to a type does not change when the tax rate changes. This is consistent with our framework where workers are defined as high or low-skilled based on the marginal tax rate that they face prior to the reform and the mechanical marginal tax rates that they face after the reform.

In this framework, changes in τ_i, y_i, τ_{-i} and y_{-i} affect the supply of hours of type i workers as follows:

$$\frac{dh_i}{h_i} = -\alpha_2 \frac{d\tau_i}{(1-\tau_i)} + \alpha_3 \frac{dy_i}{h_i\,(1-\tau_i)} + \frac{\partial h}{\partial h_{-i}} \frac{1}{h_i} \left[-\beta_2 \frac{h_{-i}\, d\tau_{-i}}{(1-\tau_{-i})} + \beta_3 \frac{dy_{-i}}{(1-\tau_{-i})} + \right] \tag{1.9.25}$$

In a log-log specification, (1.9.25) can be estimated using the following empirical model:

$$\Delta log(h_i) = \alpha_0 + \alpha_1 \,\Delta \widehat{log\,(h_{-i}})\, + \alpha_2 \,\Delta log\,(1-\tau_i) + \alpha_3 \,\Delta log(y_i) + \varepsilon_i \tag{1.9.26}$$

Where $\Delta \widehat{log\,(h_{-i}})$ is predicted using using $\Delta log\,(1-\tau_{-i})$ and $\Delta log(y_{-i})$ as instruments.

Marginal excess burden with hours coordination

We measure the marginal excess burden (MEB) as the ratio of the change in tax revenues due to behavioural responses (dB) to total changes in tax revenues (dR). Abstracting from spillovers we have:

$$MEB = \frac{dB}{dR} = \frac{dB_H + dB_L}{dM_H + dM_L + dB_H + dB_L}$$

where the change in tax revenues due to behavioural responses for a worker type i is defined as $dB_i = (\, e_i \cdot h_i \cdot w_i \cdot \frac{\tau_i}{1-\tau_i} d\tau_i) \times N_i$, and e_i, h_i, w_i, τ_i, N_i are respectively the elasticity of type i hours, average hours, average wage rates, average marginal tax rates and number of type i workers in our sample. $d\tau_i$ measures the average change in marginal tax rates on labor income due to the reform among type i workers. The mechanical change in tax revenues is defined as $dM_i = d\tau_i \cdot h_i \cdot w_i$ and captures losses (gains) in revenues due to changes of the tax schedule absent behavioural changes.

In our setting, e_L is insignificant so that dB_L can be ignored. In comparing MEB with coordination relative to the one that would be implied by low-coordination, we first estimate MEB assuming $e_H = -0.05$. This is the elasticity across all firms. Then we compute MEB under $e_H = -0.1$ that is the elasticity in low-coordination firms (Table 1.6).

Including spillovers we have:

$$MEB^{Spillover} = \frac{dB^{Spillover}}{dR} = \frac{dB_L^{Spillover} + dB_H + dB_L}{dB_L^{Spillover} + dM_H + dM_L + dB_H + dB_L}$$

where $dB_L^{Spillover} = e_L^{Spillover} \cdot (dh_H/h_H) \cdot w_L \cdot h_L \cdot \tau_L$. Here $e_L^{Spillover}$ is the elasticity of low-skilled hours to the hours of high-skilled coworkers, and dh_H is the change in hours of high-skilled due to the reform. In practice, we consider spillovers from normal hours only because these have better power in first stage regressions (Table 1.8).

1.10 Extra details on institutions and data

1.10.1 The overtime regulation in Denmark

Overtime work is defined in the large majority of collective agreements as the number of weekly hours worked beyond the normal hours set in the employment contract.[37] In order to remunerate overtime work there are two options: i) an hour of paid leave for each hour of overtime work or ii) an increase in the hourly wage according to the rates set in the collective agreements.[38] Many agreements for example set the overtime premium to 50% for the first three hours of overtime and to 100% for overtime over three hours. Work on Sundays and during public holidays is also considered overtime work and it is usually rewarded with a 100% increase in the hourly rate. Collective agreements generally establish a cap on overtime hours per week, unless explicitly agreed upon differently by the employer and the union representatives at company level.[39]

[37]For a large number of hourly wage employees the number of hours set in the contract is around 37.

[38]This is not the case for salaried workers who are not entitled to overtime pay.

[39]In the manufacturing sector the cap on overtime work is currently of 8 hours and it can be increased to 12 hours in relation to reparation of machines (*Industriens Overenskomst 2014-2017*). In the transport sector the same cap is set to 3 hours per week (*Industriens Overenskomst 2014-*

Moreover overtime work is also indirectly affected by two laws regarding working time. The first one states that every worker is entitled to rest at least 11 hours per day on average and at least one day per week (Health and Safety Act, passed in 1996).[40] The daily rest period of 11 hours can be reduced by a local agreement, even though not below 8 hours per day on average.

The second one is the rule that sets the maximum weekly working hours, including overtime work, to an average of 48 hours per week over a reference period (Directive on working time, passed in 2002).[41] The reference period, however, can vary substantially from sector to sector. For instance, both in the manufacturing and in the public sector the 48-hours maximum is always determined over a reference period of 4 months, unless a shorter or longer period of maximum 12 months is negotiated at the company level. In the service sector the picture is more blurred. The reference period is 4 months for employees working in shops, but those employees working in offices and warehouses have a reference period of 6 months.[42] However, deviations from the 4 or 6 months period can be specified at sector level. Finally, employees in transportation have stricter limitations on maximum weekly hours that should not exceed the 42 hours.

2017). In the financial sector there is not an explicit limit on overtime work (*Standardoverenskomst 2014- Finansforbundet*) but there is a reference to the rule on maximum weekly working hours.

[40] *Arbejdsmiljloven (2010)*

[41] *Bekendtgrelse af lov om gennemfrelse af dele af arbejdstidsdirektivet (2004)*

[42] In the financial sector the reference period is set to 13 weeks (*Standardoverenskomst 2014-Finansforbundet*).

1.10.2 Construction of the data on hours and earnings

In equation (1.8) we use hourly wages derived as the ratio of labor earnings gross of taxes and total working hours. We use hours and earnings relative to the highest paying job in the spell of November. This is the only spell that can be matched to employers data through *FIDA*. For workers whose November spell lasts less than 1 entire year, we annualize hours and earnings multiplying by the inverse of the share of the year that they stayed in the spell. We exclude from the analysis as outliers the workers with annualize earnings lower than 2000$ (13000 DKK) or those having annual hours greater than 5,616 ($18 \times 6 \times 52$). This results in the exclusion of around 10,000 observations over the years 2003-2011 (Table 1.10).

We use the gross labor earnings variable called *joblon* contained in *IDA* that is based on yearly labor earning records and that includes all forms of labor compensation excluding pension contributions.[43] *IDA* also contains two alternative measures of earnings. The first is *lonind* and it measures the gross annual labor earnings for the whole year and not just for the spell of November. The second one is *timelon* and it measures hourly wages. This variable however is missing for around 20,000 observations in our final sample so we prefer not to use it in the main

[43] In Denmark, workers save for their old age in a number of ways. One is through the Additional Pension from the Labour Market, called ATP. Employers make contributions for each employee to a pension fund and they increase with hours worked. Additionally there are additional pension contributions administered by the employer, which are measured by the variables *arbpen10-arbpen15* and private pension contributions measured by the variables *pripen10-pripen15*. Additional details about how the gross annual earnings are measured can be found at: `http://www.dst.dk/da/TilSalg/Forskningsservice/Dokumentation/hoejkvalitetsvariable/loenforhold-der-vedroerer-ida-ansaettelser-/joblon`

analysis. As in Kleven and Schultz 2014, in the tax simulator we use information on labor and total earnings stemming from the income register (*INDK*).[44] As a deflator for the income variables we use the Consumer Price Index with base year 2000 from Statistics Denmark.[45]

Normal working hours are from *Lønstatistikken* and they are inclusive of vacation, weekends, legal holidays or lunch breaks, whereas unpaid leave and overtime hours are excluded. *Lønstatistikken* also repots information on overtime hours (i.e. *overtid*) that takes value zero for around 70% of our final sample. Among the salaried workers this share goes up to 81%, while among hourly workers this share is around 42%. All the information contained in *Lønstatistikken* originates from employers, specifically data in *Lønstatistikken* are collected for the public companies from the administrative salary system (*Arbejdstidsregnskabet*). For most private companies (with the equivalent of at least 10 full time employees) the data are collected by the Danish employers confederation (*Dansk Arbejdsgiverforening and Finanssektorens Arbejdsgiverforening*). Over the years 2003-2011 only about 55% of the observations in IDA can be matched in LON. Attrition can be partially explained by the fact that data on about 15% of the firms surveyed are judged of low quality by Statistics Denmark and they are not released in LON. Data on hours are also available in 2002 when, however, only 30% of the observations in IDA can be matched in LON. For this reason, we exclude 2002 from the analysis.

[44] In this register the variable capturing labor earnings is *qlontmp2*.
[45] This can be accessed at `http://www.statistikbanken.dk/PRIS6`

With the introduction of the e-income registry (*E-indkomst*) the Danish tax authorities obtained information on hour worked by all employees over the age of 14, including employees in smaller enterprises, on a monthly basis.[46] This database is only available in the years 2008-2011. For this reason we use *E-indkomst* as a secondary source of data to check the robustness of our baseline results. We make hours in *E-indkomst* comparable to those in LON by aggregating monthly hours into annual hours and we exclude observations to which hours are imputed.

1.10.3 Accounting Data

As far as firms variables are concerned, capital stock (MAAT) is measured as the value of land, buildings, machines, equipment and inventory is from the Accounting Statistics register (Regnskabsstatistik).[47] We obtain total sales (OMS) from the same register. The definition of value added is the one suggested by Statistics Denmark. This changes over the sample period to account for changes in accounting standards. Specifically from 2002 to 2003, the value added is calculated as:

$$(OMS + AUER + ADR + DLG) -$$
$$(KRH + KENE + KLOE + UDHL + UASI + UDVB) -$$
$$(ULOL + EKUD + SEUD)$$

where AUER is the value of work performed for own purposes and capitalized as

[46] The hours variable that we use is called *ajoloentimer*.

[47] http://www.dst.dk/da/Statistik/dokumentation/Times/regnskabsstatistik-for-firmaer/

part of fixed assets, ADR represents other non-operating income (such as interest rates payments), DLG measures inventories, KRH consists of purchases of raw materials , finished goods and packaging (excluding electricity) , KENE are energy purchases, KLOE are labor costs, UDHL measures rents, UASI losses on small inventories, UDVB are the costs of hiring workers from other companies (such as temporary agency employment), ULOL are the leasing costs, EKUD represents other external costs (a part from secondary costs) and SEUD measures secondary costs.

From 2004 to 2012, the valued added is calculated as:

$$(OMS + AUER + ADR + DLG) -$$
$$(KVV + KRHE + KENE + KLOE + UASI + UDHL + UDVB) -$$
$$(ULOL + EKUD + SEUD)$$

where KVV is the purchase of goods for resale while KRHE measures consists of purchases of raw materials , finished goods and packaging (excluding electricity). Finally the number of full-time equivalent workers (FANSH) is from Firmstatistik.

1.10.4 Total Factor Productivity

Total Factor Productivity (TFP) is obtained from a Cobb-Douglas production function:

$$y_{it} = \beta_0 + \beta_l \, \ell_{it} + \beta_k \, k_{it} + v_{it} + \varepsilon_{it} \tag{1.10.1}$$

where y is log value added, ℓ is the log number of full time employees and k is the log of physical capital in firm i at time t. We assume that the error component ε_{it} cannot be observed or predicted by firms, while the productivity shock v_{it} is assumed to follow a Markov process so that $p\left(v_{it+1} \mid I_{it}\right) = p\left(v_{it+1} \mid v_{it}\right)$, where I_{it} - the information held by a firm at time t- includes realization of v_i up to t (Olley and Pakes 1996). This assumption implies that:

$$v_{it} = g(v_{it-1}) + \xi_{it} \tag{1.10.2}$$

where $E\left[\xi_{it} \mid I_{it}\right] = 0$ by construction. We assume that capital at t is a function of capital and investments at $t-1$: $k_{it} = \kappa\left(k_{it-1}, i_{it-1}\right)$, while labor is chosen after $t-1$. Furthermore, following Ackerberg, Caves, and Frazer. 2015 (henceforth ACF) we assume that labor is part of the demand of intermediate inputs (m_{it}):

$$m_{it} = f(k_{it}, v_{it}, \ell_{it}) \tag{1.10.3}$$

As in other studies we assume that $f()$ is strictly increasing in v_{it} so that:

$$v_{it} = f^{-1}(k_{it}, m_{it}, \ell_{it}) \tag{1.10.4}$$

and replacing this in (1.10.1) we have:

$$y_{ijt} = \beta_0 + \beta_l\, \ell_{it} + \beta_k\, k_{it} + f^{-1}(k_{it}, m_{it}, \ell_{it}) + \varepsilon_{it} = \Phi_{it}(k_{it},\, \ell_{it}, m_{it}) + \varepsilon_{it} \tag{1.10.5}$$

As in ACF we use the following moment condition to obtain an estimate of Φ_{it} ($\hat{\Phi}_{it}$) through GMM:

$$E\left[\varepsilon_{it} \mid I_{it}\right] = E\left[y_{it} - \Phi_{it}(k_{it},\, \ell_{it}, m_{it}) \mid I_{it}\right] = 0 \tag{1.10.6}$$

Then we estimate of β_0, β_l and β_k through GMM from the following moment condition:

$$\begin{aligned} E\left[\varepsilon_{it} + \xi_{it} \mid I_{it-1}\right] = & \\ & E\left[y_{it} - \beta_0 - \beta_l\, \ell_{it} - \beta_k\, k_{it} \mid I_{it-1}\right] - \\ & E\left[g\left(\Phi_{it}(k_{it-1},\, \ell_{it-1}, m_{it-1}) - \beta_0 - \beta_l\, \ell_{it-1} - \beta_k\, k_{it-1}\right) \mid I_{it-1}\right] = 0 \end{aligned} \tag{1.10.7}$$

Finally TFP is derived as:

$$TFP_{it} = \hat{\Phi}_{it} - \hat{\beta}_l\, \ell_{it} - \hat{\beta}_k k_{it} \tag{1.10.8}$$

In practice we proxy for $f^{-1}()$ using a 4th order polynomial function of k, ℓ, m and a full set of interactions among these terms, while $g()$ is assumed to be a quadratic function of v_{it-1}.

1.10.5 The Danish Tax System

Table 1.21 reports all types of income relevant to the Danish tax system.[48] The taxable income (TI) is defined as the sum of personal income (PI) and capital income (CI) minus deductions (D). Personal income is given by the sum of labor income (LI) and other sources of income such as transfers or grants. Table 1.22 shows tax rates and tax bases in the years 2008-2011. The tax system consists of a flat regional tax[49], progressive national taxes, labor market and EITC contributions. Income deriving from stocks (SI) is taxed following a separate progressive schedule. The tax rates that are shown in the table are cumulative. This means that the tax rate for a taxpayer in the top tax bracket for instance, is the sum of the tax rates in the bottom, middle and top tax bracket along with the regional tax rate, the labor market contribution and the EITC contribution rates. The sum of the tax rates however, can not exceed a marginal tax rate ceiling. If it does then the ceiling is binding.

As shown in Table 1.22, several changes to the tax system occurred over the years that we consider. In 2009 the income cut-off of the middle and top tax brackets were equalized, while the bottom tax rate went slightly down. The changes were particularly beneficial to taxpayer in the middle bracket for which the marginal tax rate ceiling was not binding and who had a tax base wide enough

[48]We base Table 1.21 on Table 1 in Kleven and Schultz 2014. We update the table to reflect the tax code relevant in the period that we analyze.

[49]The regional tax consists of a church, a municipality and a county tax. In the exposition that follows we show regional tax rates on the average municipality.

to fully exploit the change in bottom tax rates. In the following year, the 2010 Tax Reform abolished the middle tax bracket, it lowered the bottom tax rate from 5.04% to 3.67%. As an effect of those changes the marginal tax rate ceiling was also lowered from 59% to 51.5%. As a result, between 2008 and 2011, the marginal tax rate on labor income in the top tax bracket went down from 62.28% to 55.83%, while in the middle tax bracket it went from 45.06% to 37.78% (Figure 1.7). Finally in the bottom tax bracket the marginal tax rate on labor income went from 39.54% to 37.78%. The same reform also introduced a 40000 DKK deduction on capital income in the top bracket while increasing the income cut-off of the top tax bracket. The lowest income amount to be considered in the top tax bracket in fact, went up in nominal term from 335,800 DKK to 389,900 DKK that corresponds to an increase of 9% in real terms that further reduced the actual marginal tax rate faced by high incomes.

1.11 Additional results

1.11.1 The conditional exogenous mobility assumption

The estimation of unbiased coefficients from equation (1.8) requires that the unobserved component of the hourly wage rate r_{ijt} is mean independent of individual, firm fixed effects and time varying characteristics:

$$\mathbb{E}\left(r_{ijt}|X_{ijt},\, \alpha_i,\, \psi_{j(i,t)}\right) = 0 \tag{1.11.1}$$

To gain a better understanding of the problematic cases, following Card, Heining, and Kline 2013 (henceforth CHK), we assume that the error component r_{ijt} is made of 3 parts:

$$r_{ijt} = \eta_{ij(i,t)} + \zeta_{it} + \varepsilon_{it} \tag{1.11.2}$$

$\eta_{ij(i,t)}$ is a match specific component that captures an idiosyncratic wage premium (or discount) earned by individual i at firm j. This is assumed to have mean zero for all i and j. ζ_{it} is a unit root component meant to capture drifts in the portable component of the individuals earnings power (e.g. health shocks, unobserved human capital accumulation etc.). This is assumed to have zero mean. Finally ε_{it} is a residual mean reverting component.

Under these assumptions, $\mathbb{E}\left(r_{ijt}\alpha_i\right) = 0$ for all i and t. Furthermore, assuming that the components of X_{ijt} are exogenous (i.e. $\mathbb{E}\left(r_{ijt}X_{ijt}\right) = 0 \ \forall \ i, t$) then condition (1.11.1) holds if the vector of firm fixed effects is exogenous to the error component (i.e. $\mathbb{E}\left(r_{ijt}\psi_{j(i,t)}\right) = 0 \ \forall \ i, t$). As it is showed in CHK, a sufficient condition for this to hold is that the assignment of workers to firms obeys a strict exogeneity condition (i.e. the "conditional exogenous mobility").

Following CHK, we investigate the plausibility of the "conditional exogenous mobility" assumption considering 3 cases in which the assumption is violated. First, we consider the case of sorting based on the idiosyncratic employer-employee match component of wages $\eta_{ij(i,t)}$. This type of sorting is problematic because workers are paid differently at each firm depending on the match component. Absent any match effect, the average wage gains and losses from moving from high to low wage firms are expected to be symmetric. This is the case for both males and females. The existence of match effects however, will tend to offset the losses associated with moving to a low wage firm. In the limit if all transitions are voluntary and if selection is based only on the match component movers would experience no wage losses.

To check this we follow CHK and we construct mean log coworkers wages for each person in each year obtaining a distribution of coworkers wages in each year. Thus we assign each worker to a quartile of the coworkers wage distribution in a year based on the average log wage of his/her coworkers in that year. We then identify movers as workers who move from one firm to the other and who can be observed for two consecutive years in both the sending and the receiving firm. Thus we derive average wage rates of movers in the two years before and after the move in each quartile of the coworkers wage distribution.[50] Figure 1.10 shows the wage trends of movers from the 1st (i.e. low paying) or 4th (i.e. high paying) quartile of the coworkers wage distribution. Similar to other studies, we find rather symmetric wage losses and wage gains for workers moving from high to low paying firms and the opposite. This evidence is confirmed in Table 1.13 and 1.14 that show the average log wage changes associated to transitions from and to each quartile of the coworker wage distribution. We also fail to find big changes in

[50]Since our sample period ranges between 2003 and 2011 this implies that we focus on movers who moved in the years 2005-2009.

wages of workers moving across firms in the same quartile of the coworkers wage distribution. Taken together, this evidence suggests that the sorting based on a match component is likely to play a minor role in our setting.

A second case in which the exogenous conditional mobility is violated is when mobility is related to the drifts to the expected wage a person can earn at all jobs (i.e. the shocks at the unit root component of ζ_{it}). For instance, if a worker ability is revealed slowly over time and if it is valued differently at different firms, workers who turn out to be more productive than expected will experience rising wages at their initial employer and may be more likely to move to higher paying firms. The absence of any systematic trend in wages prior to a move for workers who move to high versus low paying firms (Figure 1.10) suggests that this type of mobility likely plays a minor role in our setting.

Finally a third problematic case might arise if mobility is related to the transitory fluctuations in the unobserved component ε_{it} of wages. This is the case for example, if workers tend to leave firms that experience negative shocks and join firms that experience positive shocks. This type of correlation would imply systematic dips in the wage of leavers and unusual growth in the wage of joiners that we fails to find in our data (Figure 1.10).

Related to the particular framework discussed by this paper, mobility might be due to unobserved shocks to preferences over hours worked. An unexpected disease for instance, might induce a worker to move to a lower paying firm in exchange for a working schedule that better fits the new desired hours. If this is the case however, we would observe substantial changes in hours worked by movers. This should be especially true for workers moving from bottom to top paying firms and the opposite. Table 1.15 shows the average percentage change in annual hours worked by movers in the two years prior versus the two years after the job change. Hours worked by movers are relatively stable across employers paying different wages. This is the case for males and females, independently on whether they move between the top and bottom paying firms or not.[51] This suggests that unobserved shocks to preferences over hours play a minor role in determining mobility in our sample. The sample that we consider however, is composed of full-time workers who move between firms in the private sector only. As a result we do not consider movers from full-time to part-time work and from the private to the public sector for which we might expect more variation in hours (Arizo, Hotz, and Per 2016).

1.11.2 Validation of coordination measures using survey data

Survey of Adult Skills (PIAAC)

The Survey of Adult Skills (PIAAC) collects, among other variables, information on a range of generic skills required of individuals in their work. The survey covers around 166,000 adults aged 16-65 who were surveyed in the following countries: Australia, Austria, Belgium (Flanders), Canada, the Czech Republic,

[51]The average wage changes by quartiles of the coworkers wage distribution in the sending firm never go above 0.5%, that is equivalent to around 9 hours on an yearly basis.

Denmark, Estonia, Finland, France, Germany, Ireland, Italy, Japan, Korea, the Netherlands, Norway, Poland, the Slovak Republic, Spain, Sweden, the United Kingdom (England and Northern Ireland), the United States, Cyprus and the Russian Federation. The data collection took place from 1 August 2011 to 31 March 2012 in most participating countries.

In the analysis that follows we exclude from PIAAC workers in the public sector, self-employed and students. We focus on the following two characteristics of a job: *Sharing work related information* and *Time cooperating with coworkers* both of which can be though to imply coordination of hours. These characteristics are measured on a discrete scale ranging from 1 to 5 where 1 means that the characteristic is not important and 5 that it is extremely important. In order to merge this information to Danish Registers we first take the modal value of each characteristic within each 4-digit occupation. Then we merge with registers data based on (4-digit) occupation (ISCO-08) and we take the average value of each characteristic in a firm as a measure of the importance of that characteristic. Figure 1.11 plots the standard deviation of hours across skill groups against each one of these measures in each firm-year in our sample. As expected we find a strong and negative correlation of the standard deviation with each one of the two variables. That is, in firms where these characteristics are more important hours turn out to be are more coordinated.

Measures of coordination in time use survey data

The Time Use Survey was conducted in 2001 and 2008 by the Danish National Institute of Social Research. Industry information however, is only available in the 2001 survey and for this reason in the following analysis we only use 2001 data. The data collection consists of a questionnaire interview that collects information on demographic and labor market characteristics and two diaries, one diary is for a weekday while the other one for a weekend day. Each diary is divided in 10 minutes intervals and stretches from 4am to 4am the day after. In each interval the respondent has to inform: i) what he/she did (the primary activity) and ii) where he/she was. The survey includes a representative sample of approximately 3,000 individuals. We restrict our analysis to full-time employees (>26 weekly hours) in the private sector or approximately 750 observations.[52]

Based on this, we construct a coordination index as follows: we group workers in two educational groups, the tertiary educated and all others. For each educational group and in each sector and hour of the day we compute the share of workers who are at work relative to the total number of workers in that educational group:

$$Share_{ehs} = \frac{N_{ehs}}{N_{es}} \tag{1.11.3}$$

where e indicates either tertiary educated (t) or other workers (o), h is hour of

[52]The variable that identifies workers in the private sector is missing for 1,073 observations out of 3,000. We also exclude from the analysis self-employed, students and those whose industry of employment is missing.

the day and it ranges between 4am to 4am of the day after, while s indicates sector. Due to the limited number of observations we use a 1-digit sector definition analogous to the one used in Table 1.3. The coordination index in a sector is computed as the correlation between the share of tertiary and other workers across the 24 hours of the day:

$$Coordination\, index_s = correlation\,(Share_{ths}\,,\, Share_{ohs}) \tag{1.11.4}$$

High correlation between the share of differently educated workers over the day can be interpreted as signaling high-coordination and viceversa.

Table 1.12 show the coordination index in each sector. In line with Table 1.3, the index is extremely high in some of the service industries such as utilities, trade and the financial sector while it takes relatively low values in agriculture and construction. In line with Table 1.12 the index is higher in manufacturing than in construction and agriculture but lower than in most of the service sectors. Differently from Table 1.3 the residual sector (i.e. "Public administration, education, health and arts") shows a relatively lower index relative to the other services. In our final sample however only 29 firms out of more than 8,000 are part of this sector.

1.11.3 Coordination and wages differentials: additional robustness checks

Hours worked might be measured with errors and this might bias the estimated correlation between coordination and wage premiums. To get a sense of the size and the direction of this bias, in column 1 of Table 1.17 we use the average importance of the *Contact*, *Teamwork* and *Communication* in a firm (see Section 1.4.3) as an instrument for the standard deviation of hours in equation (1.7). To the extent that the importance of these factors is correlated with the coordination of hours, this IV approach allows to better separate the coordination component from the measurement error in σ_j. The coefficient from this specification is negative and greater in magnitude than in the baseline model. This suggests that measurement errors generate attenuation bias and that the division bias (Borjas 1980) is unlikely to play a major role in our setting.[53]

In line with this, columns 2 in Table 1.17 show the results obtained while using the median absolute deviation from the median hours (MAD) as an alternative measure of coordination. This measure is less sensitive to outliers. The magnitude of the standardized coefficients in this specification goes up suggesting that, if anything, outliers might drive down the correlation between wages and coordination.

Van Reenen 1996 finds that innovation in a firm causes higher wages. While we can not directly measure innovation, if we control for the stock of immaterial

[53] If the first and second moments of the distributions of the errors and the actual hours are uncorrelated, then measurement error can be shown to generate downward biased estimates.

assets in a firm we find that the coefficient on coordination is barely affected (column 3). Moreover, coordination may be expected to be more important among workers of the same plant. In fact, when we restrict the analysis to single plant firms (80% of the sample) we find the coefficient to be greater in magnitude than in the baseline (column 4). In the last column (5) of Table 1.17 we control for the number of skill groups in a firm as a way to take out any spurious correlation between high dispersion in hours and the skill diversity of the workforce in a firm. The results are robust to this control.

In the baseline specification we only focus on the firms where attrition in hours worked is low (i.e. less than 5% of the workforce in a year). Columns 1 and 2 in Table 1.18 reports the coefficients estimated when we consider all firms in the largest set of connected firms. The coefficient is negative and significant and the Coordination share within 3-digit industry (column 2) is similar to the one estimated in the baseline model.

In the baseline version of equation (1.8) we control for firm time varying characteristics to isolate the the firm fixed effect from capturing temporary fluctuations in wages due to firm specific shocks.[54] As a robustness check in columns 3 and 4 of Table 1.18 we shows the results obtained while estimating equation (1.8) including in X_{ijt} only individual time varying controls.[55] The coefficients from these regressions are still negative and significant even if less precisely estimated

[54]The time varying characteristics that we use are value added, sales per employee, exporter status and the share of salaried workers

[55]These are a set of interactions between year dummies and educational attainments and interaction terms between quadratic and cubic terms in age and educational attainments.

possibly because the temporary variations in wages add some noise to the firm fixed effect in this specification.

Finally, in order to check whether the correlation that we find is driven by other factors specific of some years in our data we divide the overall sample period in 3 subperiods (2003-2005, 2006-2018 and 2009-2011). Then we estimate equation (1.8) separately on each one of these shorter panels to obtain the firm-component of the wages specific of a subperiod ($\psi^s_{j(i,t)}$). In the second step we then relate $\psi^s_{j(i,t)}$ to coordination in that subperiod σ^s_j, a set of controls and subperiod fixed effects γ^s.

$$\widehat{\psi^s_{j(i,t)}} = \delta_0 + \delta_1 \, \sigma^s_j + \delta_2 \, \bar{Z}^s_j + \gamma^s + v^s_j \tag{1.11.5}$$

While the fixed effects allow to control for factors specific of a subperiod, this panel regression is based on firm fixed effects ($\psi^s_{j(i,t)}$) estimated on shorter panels and thus on a lower number of movers. This might reflect in less accurate estimates. With this caveats in mind, column 5 in Table 1.18 shows δ_1 estimated from this regression. The coefficient remains negative and significant even if less precisely estimated. The lower precision however, is likely due to outliers because when we use the median absolute deviation of hours as a measure coordination the coefficient is much more precisely estimated (column 6).

1.11.4 Additional robustness checks on coordination labor supply and tax changes

Table 1.24 shows the labor supply elasticity of normal hours in the residual group. This is obtained through the same empirical model used to for high-skilled (equation (1.10)). Independently on the specific controls for base-year income, the elasticity remains positive, close to zero and insignificant (columns 2 to 5). At the point estimate however, the elasticity is twice as large among workers who are in the bottom half of the income distribution in the residual group. These are also more distant from the top tax bracket that is suggestive of weaker responses among workers who are more likely to end up in the top bracket by increasing hours.

In columns 1 and 2 of Table 1.25 we examine the labor supply response of high-skilled women with children and of high-skilled in the top 10% of the income distribution in 2008 respectively. In line with other recent studies, we find stronger responses among women and top incomes. Differently from high-skilled males, we estimate a positive elasticity among women. The Gruber-Saez type of specification that we use assumes away bunching at the kink points. With significant bunching however, this may create bias. Thus in column 3 we exclude workers at the major kink points of the tax schedule. The estimated elasticity is extremely robust to this specification. Finally in column 4 we estimate the effect of the reform on labor income rather than on hours. In order to compare our results with those of other studies, we estimate this specification on all wage earners. In line with Kleven and

Schultz 2014, we estimate a positive and small (0.03) elasticity of labor income. This suggests that the negative elasticity of hours that we find might be linked to the specific sample for which data on hours are available.

For the reasons discussed in Section 1.5.5, the instrumental variables that we use depends on income at time t. This can be problematic due to mean reversion or to the existence of other trends that unevenly affect the labor supply of workers across the distribution of income at the same time as the tax reform. To check whether the baseline results from Table 1.6 are sensitive to controls of base-year income, in Table 1.26 we estimate equation (1.10) controlling for pre-reform income in a number of flexible ways. In columns 1 and 2 we control for 5-piece splines of income at time t (similar to Gruber and Saez 2002), in columns 3 and 4 we control for a 5th order polynomial function of income at time t and an indicator function for positive base-year income (as in Dahl and Lochner 2012), finally in column 5 and 6 we include 5-piece splines of income at $t-1$ and the change of income between $t-1$ and t (similar to Kopczuk 2005) .[56] The results from these alternative specifications are very much in line with the baseline ones. In particular, the labor supply in low-coordination firms is significantly more elastic than in firms at high degree of coordination in all the specifications. The magnitude of the elasticity in low-coordination firms is close in the one estimated in the baseline regressions and it ranges from -0.07 to -0.1 depending on the specification.

[56] Gruber and Saez 2002 use 10-piece splines while we use 5-piece splines of the base year income. Since we focus on a limited sample of the Danish population and since we only exploit one tax reform, we do not have in fact enough power to estimate more than 5-piece splines of income.

In Table 1.27 we perform a similar set of robustness checks on the spillover effects estimated through equation (1.11). In these specifications we control for base-year income (column 1), 5-piece splines of income at t (column 2) and a 5th order polynomial function of income at time t (column 3). The coefficient on $\Delta log\,\overline{h^H}$ remains significant, positive and of comparable magnitude as in the baseline results.

In columns 1 to 4 in Table 1.28 we present the results obtained from using the alternative measure of coordination described in Section 1.4.3 where skill groups are defined from the intersection of education (primary, secondary and tertiary) and occupation (blue collar, middle and top manager) groups. In column 1 and 2 we estimate equation (1.10) on workers in high and low-coordination firms. As in the baseline model the labor supply in low-coordination firms remains significantly more elastic and the magnitude of the coefficients is close to the baseline. Columns 3 and 4 show the results obtained from estimating equation (1.11) on workers in low-coordination firms. In column 3 we focus on normal hours of work while in column 4 we consider total hours inclusive of overtime. The spillovers remain significant and of similar magnitude as in the baseline regression model.

In columns 5 and 6 in Table 1.28 we estimate equation (1.10) using data on hours worked from *E-indkomst* (called "BFL hours" in the tables). This is an alternative source of administrative data on hours worked available in the years 2008-2011 only (see Appendix 1.10.2). We restrict the analysis to the workers included in the baseline specification that can be matched in *E-indkomst*. As in the baseline regressions we do not find significant effects on the elasticity of hours of high-skilled workers in high-coordination firms. The elasticity in low-coordination firms remains significant and of similar magnitude as in the baseline regressions. In column 7 we estimate (1.11). The spillovers remain significant and of comparable but greater magnitude. However, the magnitude has to be interpreted with caution because of the low power in some of the first stage regressions (F-stat lower than 2).

1.11.5 Income and uncompensated elasticity to tax changes

In the specifications that we discuss in the paper the labor supply elasticity is inclusive of the income effect. In the robustness section we also present separate estimates of the income effects for both high and low-skilled workers. To estimate the income effects we follow the standard model used in the taxable income literature and we modify equation (1.10) and equation (1.11) as follows:

$$log\left(\frac{h^H_{ijt+3}}{h^H_{ijt}}\right) = \theta_0 + \theta_1\, log\left(\frac{1-\tau^H_{it+3}}{1-\tau^H_{it}}\right) + \theta_2\, log\left(\frac{vy^H_{it+3}}{vy^H_{it}}\right) + \theta_3\, X_{ijt} + \upsilon_{ijt} \quad (1.11.6)$$

$$log\left(\frac{h^L_{ijt+3}}{h^L_{ijt}}\right) = \mu_0 + \mu_1\, log\left(\frac{\overline{h^H_{jt+3}}}{\overline{h^H_{jt}}}\right) + \mu_2\, log\left(\frac{1-\tau^L_{it+3}}{1-\tau^L_{it}}\right) + \mu_3\, log\left(\frac{vy^L_{it+3}}{vy^L_{it}}\right) + \mu_4\, X_{ijt} + \epsilon_{ijt} \quad (1.11.7)$$

In these models the terms $log(vy^L_{it+3}\,/\,vy^L_{it})$ and $log(vy^H_{it+3}\,/\,vy^H_{it})$ indicate the changes in virtual income of respectively low and high-skilled workers between time t and $t+3$. Due to the same endogeneity problems that we discuss in Section 1.5.5, we estimate these specifications using mechanical changes of the virtual incomes and net-of-tax rates as instruments for the observed changes of these variables. Mechanical changes of the virtual income are obtained from simulating the post-reform virtual income while assuming that the real income stayed constant between t and $t+3$ as described (Section 1.5.5).

Following Kleven and Schultz 2014, we define virtual income as $\tau z_{LAB} + \sum_{n=1}^{N} t^n z_n - T(z_{LAB}, z_1, ..z_N)$ where T() indicates total tax liabilities, τ is the marginal tax rate on labor income (z_{LAB}) and t^n is the marginal tax rate on the n^{th} component of income z_n. This characterization is a generalization of the standard virtual income definition to a situation with multiple income components. It differs from the definition used in some of the existing studies (e.g. Gruber and Saez 2002) where virtual income is defined as after-tax income. Based on this, the coefficients θ_1 and μ_2 measure the uncompensated elasticity of hours worked to the marginal net-of-tax rates. θ_2 and μ_3 measure the elasticity of hours with respect to virtual income (see Section 1.9.6).[57]

In columns 1 and 2 of Table 1.29 we estimate equation (1.11.6) respectively

[57] Other studies in this literature use the after tax income rather than virtual income in estimating similar type of regressions (e.g. Gruber and Saez 2002). In these studies, the analogue of θ_1 or μ_2 in our specification measure the compensated elasticity of hours. In our specification, θ_1 and μ_2 can be combined to respectively θ_2 and μ_3 using the Slutsky equation to obtain the compensated elasticity (Section 1.9.6).

in high and low-coordination firms. Unfortunately, due to the fact that the our identifying variation is based on one tax reform only we miss the power to estimate separately the income effect and the uncompensated elasticity . Even if imprecisely estimated, the point estimates show a substantial difference in both the income and the uncompensated elasticity between firms at high versus low degree of coordination. In fact, in line with the baseline results the uncompensated elasticity and the income effects are greater in magnitude in firms at low-coordination. In the last column of Table 1.27 we show the spillover effects obtained from estimating equation (1.11.7). In this specification we use the mechanical change of the virtual income of low-skilled workers as an instrument for the observed change of virtual income. In the first stage regressions we also use the average virtual income of high-skilled coworkers as an additional instrument. Adding these additional controls does not have sizeable effects on the estimated spillovers that remain significant and of a similar magnitude as in the baseline model.

1.11.6 The effect of the 2010 Tax reform on firm characteristics

We investigate the effects of the tax reform on firm characteristics using the following regression model:

$$log\left(\frac{y_{jt+3}}{y_{jt}}\right) = \gamma_0 + \gamma_1\, log\left(\frac{\overline{1-\tau^H_{jt+3}}}{\overline{1-\tau^H_{jt}}}\right) + \gamma_2\, Z_{jt} + \varepsilon_{jt} \qquad (1.11.8)$$

We estimate this model considering 4 different y variables : firm size, the share of high-skilled, the share of low-skilled workers in a firm and the amount of physical capital. The regressor of interest in this model is:

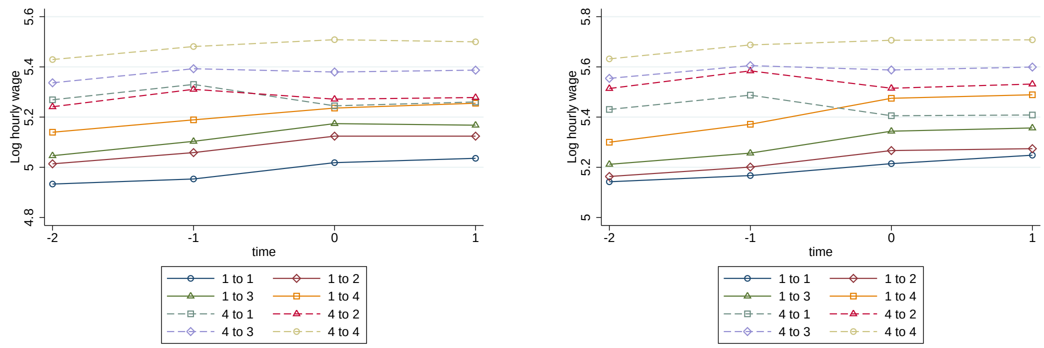
$$log\left(\frac{\overline{1-\tau^{H}_{jt+3}}}{\overline{1-\tau^{H}_{jt+3}}}\right) = log\left[\frac{H^{-1}_{jt+3}\sum_{i\in\ H_{jt+3}}(1-\tau_{ijt+3})}{H^{-1}_{jt}\sum_{i\in\ H_{jt}}(1-\tau_{ijt+3})}\right] \tag{1.11.9}$$

This measures the log change of the average net-of-tax-rate on labor income faced by high-skilled workers in a firm. We see this as a proxy of the intensity of the effect of the tax reform on firm j. For reasons similar to those discussed in Section 1.5.5, we use the mechanical change $log\left(\overline{1-\tau^{H}_{Mjt+3}}\right) - log\left(\overline{1-\tau^{H}_{jt}}\right)$ defined in equation (1.13) as an instrument for the actual change defined in equation (1.11.9). Z_{jt} is a vector of firm characteristics measured in the base year.

Table 1.30 shows the results from this model. The coefficient of interest in these specifications is the one attached to the variable $\Delta log\,(\overline{1-\tau^{H}})$ that corresponds to γ_1 in equation 1.11.8. Each column of the table reports the effects on a different outcome variable y. In column 1 the outcome variable is the log change in firm size, in columns 2 and 3 respectively we analyze the effects on the log change of the share of high-skilled and the share of low-skilled workers in a firms. Finally in column 4 we look at the effects on the amount of physical capital in a firm. The coefficient γ_1 estimated in these specifications remains small and insignificant across all columns. This is reassuring and it corroborates the assumptions that firms did not change their production technologies as an effect of the reform.

1.12 Additional Tables and Figures

1.12.1 Additional graphs and tables



(a) Females (b) Males

Figure 1.10: Wage Dynamics of Movers

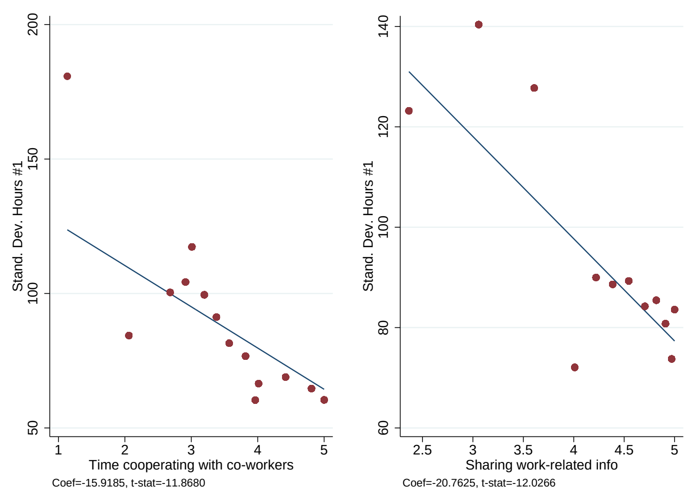


Figure 1.11: PIAAC validation exercise coordination

Table 1.10: Steps of the data preparation

	Obs.	Workers	Firms	Obs. share tot.	Workers share tot.	Firms share tot.
1. Entire Population	22,379,298	3,518,236	266,196	100	100	100
2. Lønstatistikken sample	12,130,358	2,649,618	39,778	54.20	75.31	14.94
3. Firms administrative data sample	5,211,149	1,485,789	29,957	23.29	42.23	11.25
4. Keep firms with more than 2 workers	5,209,536	1,485,478	29,576	23.28	42.22	11.11
5. Keep full time workers only	4,476,222	1,207,580	29,116	20.00	34.32	10.94
6. Drop Outliers in hours and income	4,466,676	1,205,301	29,111	19.96	34.26	10.94
7. Keep firms with less than 5% of obs. missing	787,683	400,653	8,293	3.52	11.39	3.12

Notes: Workers younger than 15 and older than 65 are excluded from the entire population.

Table 1.11: Summary Statistics of the AKM regression

	All Sample	Largest group of connected firms
Person and estabilishment parameters		
Number of person effects	1205295	1195884
Number of firm effects	26227	26121
Summary of parameters estimates		
Std. dev. of person effects	0.962	0.960
Std. dev. of firm effects	0.141	0.137
Std. dev. Of Xb	0.829	0.828
Adjusted R-squared	0.913	
Std. dev. of log wages	0.451	0.450
Number of person-year observations	4466655	4445484

Notes:Controls in first step (AKM) regressions: year dummies interacted with education dummies, quadratic and cubic terms in age interacted with education dummies, VA per employee, capital per employee, sales per employee, exporter status, fraction of salaried workers

Table 1.12: Coordination index by sector using TUS data

	Coordination index
Agriculture, forestry and fishing, mining and quarrying	0.833
Manufacturing	0.978
Construction	0.956
Electricity, gas, steam and air conditioning supply, trade and transport	0.982
Financial and insurance, Real estate, Other business	0.986
Public administration, education, health, arts	0.929
Observations	748

Table 1.13: Mobility and wage changes: Males

		Log wages of movers (mean)		Log wage change	
Origin to destination quartile	Number of moves	2 years before	2 years after	Raw	Adjusted
1 to 1	2895	5.14	5.25	0.11	0.00
1 to 2	1515	5.16	5.28	0.12	0.03
1 to 3	965	5.21	5.36	0.15	0.05
1 to 4	500	5.29	5.48	0.19	0.09
2 to 1	960	5.22	5.25	0.03	-0.06
2 to 2	2443	5.29	5.35	0.06	-0.02
2 to 3	1824	5.33	5.43	0.10	0.02
2 to 4	925	5.39	5.51	0.13	0.04
3 to 1	612	5.37	5.37	0.00	-0.07
3 to 2	2110	5.39	5.43	0.05	-0.03
3 to 3	6217	5.40	5.46	0.06	0.00
3 to 4	2120	5.49	5.59	0.10	0.02
4 to 1	304	5.43	5.41	-0.02	-0.10
4 to 2	760	5.51	5.55	0.03	-0.05
4 to 3	2354	5.55	5.60	0.05	-0.02
4 to 4	6395	5.62	5.70	0.08	0.00

Notes: Entries are observed mean log real hourly wages in the period 2003-2011 for job changers with at least 2 years of wages at the old and new job. Job refers to the firm of main occupation in the year. Origin/destination quartiles are based on mean wages of coworkers in year before (origin) or year after (destination) job move. Four year wage changes in regressions-adjusted include controls for age, age squares and cubs, education dummies, and quadratic in age fully interacted with education.

Table 1.14: Mobility and wage changes: Females

Origin to destination quartile	Number of moves	Log wages of movers (mean) 2 years before	2 years after	Log wage change Raw	Adjusted
1 to 1	2869	4.94	5.04	0.10	0.00
1 to 2	759	5.01	5.12	0.11	0.02
1 to 3	496	5.04	5.17	0.13	0.03
1 to 4	240	5.12	5.24	0.12	0.03
2 to 1	511	5.08	5.12	0.04	-0.05
2 to 2	1128	5.11	5.18	0.07	-0.01
2 to 3	869	5.13	5.23	0.10	0.01
2 to 4	465	5.19	5.29	0.10	0.01
3 to 1	324	5.15	5.17	0.03	-0.06
3 to 2	873	5.18	5.24	0.06	-0.02
3 to 3	2934	5.24	5.30	0.06	0.00
3 to 4	1064	5.29	5.40	0.11	0.02
4 to 1	195	5.27	5.27	0.00	-0.08
4 to 2	419	5.24	5.28	0.04	-0.05
4 to 3	1371	5.34	5.39	0.05	-0.01
4 to 4	3177	5.41	5.49	0.07	-0.01

Notes: Entries are observed mean log real hourly wages in the period 2003-2011 for job changers with at least 2 years of wages at the old and new job. Job refers to the firm of main occupation in the year. Origin/destination quartiles are based on mean wages of coworkers in year before (origin) or year after (destination) job move. Four year wage changes in regressions-adjusted include controls for age, age squares and cubs, education dummies, and quadratic in age fully interacted with education.

Table 1.15: Dynamics in Hours of Movers

Average change in annual hours worked by movers (%)
Breakdown by quartiles of the coworkers wage distribution

Type of origin firm	Males		Females	
	Obs.	Mean change (%)	Obs.	Mean change (%)
1st Quartile	6709	0.05	4920	-0.25
2nd Quartile	7182	0.01	3444	-0.31
3rd Quartile	12924	0.27	5952	0.06
4th Quartile	11549	0.04	5913	-0.39

Mean change (%) in annual hours worked by movers
Detailed Breakdown for movers in the 1st and 4th quartile

Sending to Receiving firm	Males		Females	
	Obs.	Mean change (%)	Obs.	Mean change (%)
1st to 1st	3284	0.02	3202	0.43
1st to 2nd	1775	0.04	853	-1.06
1st to 3rd	1084	0.08	575	-0.40
1st to 4th	566	0.24	290	0.04
4th to 1st	351	0.01	220	-0.52
4th to 2nd	995	0.00	502	-0.70
4th to 3rd	2709	0.23	1541	0.10
4th to 4th	7494	0.07	3650	-0.45
Mean Hours		1935		1930

Notes: Panel A in the table shows the average percentage change in hours worked by movers broken down the quartile of the coworkers wage distribution of the sending firm. In Panel b we then further break down the hours change within the 1st and 4th of the sending firm depending on the quartile of the coworkers wage distribution of the receiving firm. We do this in each interval 2003-2007, 2004-2008, 2005-2009, 2006-2010 and 2007-2011. In the table we show the average change across these periods.

Table 1.16: Desired Hours by Skill Groups

Skills Definion 1	Average desired weekly hours	Obs.
skill $\leq$ 10th percentile	37.34	465
10th percentile$<$ skill $<$ 20th percentile	36.78	462
20th percentile$<$ skill $<$ 30th percentile	37.69	463
30th percentile$<$ skill $\leq$40th percentile	37.72	461
40th percentile$<$ skill $\leq$ 50th percentile	38.55	461
50th percentile$<$ skill $\leq$ 60th percentile	38.33	463
60th percentile$<$ skill $\leq$ 70th percentile	38.48	463
70th percentile$<$ skill $\leq$ 80th percentile	39.33	461
80th percentile$<$ skill $\leq$ 90th percentile	38.79	462
skill $>$ 90th percentile	40.42	461
Skills Definition 2	**Average desired weekly hours**	
Primary education, blue collar	37.67	963
Secondary education, blue collar	37.73	1,512
Tertiary education, blue collar	38.31	106
Primary education, middle manager	38.39	245
Secondary education, middle manager	38.25	852
Tertiary education, middle manager	39.17	693
Primary education, manager	41.55	43
Secondary education, manager	41.72	113
Tertiary education, manager	43.97	96

Notes: Information on desired hours is obtained from the 2008-2010 Danish labor force survey data. We focus on workers whose reference week is in November to better match information in the Labor Force Survey to registers data. Skills Definition 1 refers to skill groups defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM regression model. AKM regressions are estimated on the years 2008-2010. Skills definition 2 refers to skill groups defined at the intersection of occupational and educational category.

Table 1.17: Coordination and wage differentials: Measurement error and regular hours

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.
Stand. Dev. Tot. Hours	-0.342**		-0.069***	-0.072***	-0.061***
	(0.172)		(0.018)	(0.018)	(0.017)
Median Abs. Dev. Tot. Hours		-0.085***			
		(0.015)			
Firm size	0.003	0.010	0.009	0.148*	0.004
	(0.006)	(0.007)	(0.007)	(0.075)	(0.004)
Exporter status	0.023	0.072***	0.065***	0.059***	0.051***
	(0.029)	(0.018)	(0.016)	(0.019)	(0.015)
Union. Rate	0.068**	0.035	0.030	0.030	0.020
	(0.029)	(0.023)	(0.023)	(0.022)	(0.023)
Female Share	-0.113***	-0.108**	-0.104**	-0.087**	-0.111**
	(0.038)	(0.042)	(0.044)	(0.040)	(0.044)
Average Hours	0.024	-0.001	0.008	0.006	0.002
	(0.043)	(0.025)	(0.026)	(0.027)	(0.025)
log(Cap/empl)	0.019	0.029**	0.025*	0.038***	0.028**
	(0.015)	(0.013)	(0.013)	(0.014)	(0.013)
Numb. of skill groups					0.072***
					(0.012)
(Intang. Assets)/empl			0.019**		
			(0.009)		
O*NET IV	YES	NO	NO	NO	NO
Multi-plant firms	YES	YES	YES	NO	YES
Coordination Share		0.279	0.256	0.273	0.200
F-stat excl. instr.	8.942				
R-sq	0.020	0.118	0.101	0.101	0.105
N	6089	7374	7312	5695	7312

*Notes: The Stand. Dev. of Total hours is the standard deviation of the average hours worked across skill groups within a firm. The Median Abs. Dev. is the the median absolute deviation of median hours across each skill groups within a firm. Skill groups are defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM model. O*NET IV refers to a vector composed by the average importance of the Contact, Teamwork and Communication in the firm (Section 1.4.3). All regressions show standardized coefficients. Exporter and industry dummies are based on the median value between 2003 and 2011. (Cap/emp) stands for physical capital per employee. Intang. Assets/empl indicates Intangible assets per employee. All regression include a vector of controls for the share of workers in each skill group and for the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and Sales". Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.18: Wage differentials and coordination: additional robustness

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.	(6) Firm f.e.
Stand. Dev. Def. 1	-0.041***	-0.021**	-0.051***		-0.030*	
	(0.011)	(0.010)	(0.018)		(0.016)	
Median Abs. Dev. Def. 1				-0.069***		-0.034***
				(0.016)		(0.012)
Firm size	0.007***	0.011***	0.010	0.011	0.009	0.010
	(0.002)	(0.002)	(0.007)	(0.008)	(0.007)	(0.008)
Exporter status	0.048***	0.022**	0.044***	0.042***	0.013	0.012
	(0.011)	(0.009)	(0.015)	(0.016)	(0.009)	(0.009)
Union. Rate	0.041***	0.040***	0.038	0.042	0.027	0.029
	(0.015)	(0.013)	(0.026)	(0.026)	(0.018)	(0.018)
Female Share	-0.150***	-0.089***	-0.131***	-0.134***	-0.055**	-0.057**
	(0.039)	(0.020)	(0.044)	(0.042)	(0.027)	(0.026)
Average Hours	-0.021**	-0.045***	-0.015	-0.028	-0.045**	-0.055**
	(0.010)	(0.010)	(0.024)	(0.022)	(0.022)	(0.021)
log(Cap/empl)	0.022*	0.036***	0.026**	0.026**	0.017	0.017
	(0.013)	(0.010)	(0.012)	(0.012)	(0.012)	(0.012)
Connected set sample	YES	YES	NO	NO	NO	NO
3 digits Sector f.e.	NO	YES	NO	NO	NO	NO
3-year sub-period f.e.	NO	NO	NO	NO	YES	YES
AKM individual controls	NO	NO	YES	YES	NO	NO
Part. R-sq SD Hours	0.002	0.001	0.003	0.003	0.001	0.001
Part. R-sq VA and Sales	0.022	0.008	0.014	0.014	0.004	0.004
Coordination Share	0.084	0.074	0.182	0.209	0.198	0.190
R-sq	0.153	0.200	0.092	0.094	0.380	0.380
N	20766	20766	7305	7305	8487	8487

*Notes: The Stand. Dev. of Total hours is the standard deviation of the average hours worked across skill groups within a firm. The Median Abs. Dev. is the the median absolute deviation of median hours across each skill groups within a firm. Skill groups are defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM model. All regressions show standardized coefficients. Exporter and industry dummies are based on the median value between 2003 and 2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. Specifications (7) also include quadratic and cubic terms of value added per employee. All regression include a vector of controls for the share of workers in each skill group and for the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and Sales". Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.19: Value Added, Sales and and wage premiums relative to Table 1.4

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.	(6) Firm f.e.
log(VA/empl)	0.122***	0.095***	0.168***	0.168***	0.166***	0.157***
	(0.020)	(0.019)	(0.020)	(0.025)	(0.021)	(0.022)
TFP	0.049	0.031	0.097***	0.113***	0.096***	0.059**
	(0.034)	(0.024)	(0.029)	(0.029)	(0.029)	(0.023)
Firm size		0.016**	0.013*	0.041***	0.013*	0.013**
		(0.007)	(0.007)	(0.012)	(0.007)	(0.006)
Exporter status		0.062***	0.046***	0.047**	0.047***	0.037***
		(0.017)	(0.015)	(0.020)	(0.015)	(0.013)
Union. Rate		-0.001	0.038	0.045	0.039	0.067***
		(0.026)	(0.024)	(0.031)	(0.024)	(0.025)
Female Share		-0.058	-0.107***	-0.111***	-0.105***	-0.098***
		(0.040)	(0.035)	(0.035)	(0.035)	(0.020)
Average Hours		-0.020	-0.031	-0.030*	-0.030	-0.063***
		(0.022)	(0.021)	(0.018)	(0.021)	(0.023)
log(Cap/empl)		0.019	-0.008	0.023	-0.007	-0.007
		(0.012)	(0.013)	(0.016)	(0.013)	(0.015)
Persuasion						-0.188**
						(0.074)
Social Perceptiveness						0.025
						(0.044)
Adjust Actions to others						0.005
						(0.017)
Negotiation						0.254**
						(0.097)
Region F.E.	NO	YES	YES	YES	YES	YES
Compos. cntr	NO	NO	YES	YES	YES	YES
Ability Measures	NO	NO	YES	YES	YES	YES
Av. Hours b/w 36.5 and 37.5	YES	YES	YES	NO	YES	YES
Part. R-sq VA and Sales	0.022	0.010	0.032	0.038	0.032	0.020
R-sq	0.022	0.041	0.148	0.153	0.147	0.165
N	7117	7117	7060	4279	7047	5904

*Notes: All regressions show standardized coefficients. Exporter and industry dummies are based on the median value between 2003 and 2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. All specifications control for quadratic and cubic functions of value added per employee and TFP. TFP is obtained from as described in Appendix 1.10.4. "Compos. cntr" refers to a vector of controls for the share of workers in each skill group. "Ability Measures" indicate a vector containing the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and Sales". Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.20: Value Added, Sales and and wage premiums relative to Table 1.5

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.
log(VA/empl)	0.159***	0.148***	0.142***
	(0.019)	(0.020)	(0.019)
TFP	0.122***	0.083***	0.084***
	(0.029)	(0.021)	(0.021)
Firm size	0.012**	0.007*	0.018*
	(0.005)	(0.004)	(0.010)
Exporter status	0.034**	0.018	0.010
	(0.013)	(0.012)	(0.012)
Union. Rate	0.044*	0.042	0.043
	(0.026)	(0.028)	(0.028)
Female Share	-0.136***	-0.083***	-0.066***
	(0.030)	(0.023)	(0.025)
Average Hours	-0.041**	-0.052***	-0.057***
	(0.017)	(0.018)	(0.017)
log(Cap/empl)	-0.005	-0.001	0.006
	(0.013)	(0.013)	(0.011)
Region f.e.	YES	YES	YES
Compos. and Ability cntr.	YES	YES	YES
1 digit Sector f.e.	YES	NO	NO
2 digits Sector f.e.	NO	YES	NO
3 digits Sector f.e.	NO	NO	YES
Part. R-sq VA and Sales	0.033	0.016	0.014
R-sq	0.156	0.183	0.188
N	7055	7055	7055

*Notes: Notes: All regressions show standardized coefficients. Exporter and industry dummies are based on the median value between 2003 and 2011. All specifications control for quadratic and cubic functions of value added per employee and TFP. TFP is obtained as described in Appendix 1.10.4. "Compos. cntr" refers to a vector of controls for the share of workers in each skill group. "Ability Measures" indicate a vector containing the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and Sales". Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.21: Income Types in the Danish Tax System

Acronym	**Income Type**	**Main Intems Included**
LI	Labor income	Salary, wages, honoraria, fees, bonuses, fringe benefits, business earnings
PI	Personal income	LI+ transfers, grants, awards, gifts, received alimony -Labor market contribution, certain pension contributions
CI	Capital income	Interest income, rental income, business capital income -interest on debt (mortgage, bank loan, credit cards, student loans)
D	Deductions	Commuting costs, union fees, UI contribution, other work expenditures, charity, paid alimony
PCP		Private capital pension contribution
ECP		Employer paid capital pension contribution
TI	Taxable income	PI+CI-D
SI	Stock Income	Dividends and realized capital gains from shares

Table 1.22: Personal Income Tax System in Denmark

	2008			**2009**		
Tax type	**Base**	**Rate**	**Tax Bracket (DKK)**	**Base**	**Rate**	**Tax Bracket (DKK)**
Regional tax*	TI	33.16		TI	33.21	
National taxes						
Bottom tax	PI+CI(>0)	5.48	0 - 279799	PI+CI(>0)	5.04	0 - 347199
Middle tax	PI +CI(>0)	6.0	279800 - 335799	PI +CI(>0)	6.0	>347200
Top tax	PI+CI(>0)+PCP+ECP	15.0	335800	PI +CI(>0)+PCP+ECP	15.0	>347200
Labor market contribution	LI	8.0		LI	8.0	
EITC	LI	4.0		LI	4.25	
Tax on stock income	SI	28.0, 43.0, 45.0		SI	28.0, 43.0. 45.0	
Marginal tax ceiling	PI/CI/TI	59.0		PI/CI/TI	59.0	
	2010			**2011**		
Tax type	**Base**	**Rate**	**Tax Bracket (DKK)**	**Base**	**Rate**	**Tax Bracket (DKK)**
Regional tax*	TI	33.32		TI	33.38	
National taxes						
Bottom tax	PI+CI(>0)	3.67	0 - 389899	PI+CI(>0)	3.64	0 - 389899
Middle tax	-	-		-	-	
Top tax	PI +CI(>40000)+PCP+ECP	15.0	>389900	PI +CI(>40000)+PCP+ECP	15.0	>389900
Labor market contribution	LI	8.0		LI	8.0	
EITC	LI	4.25		LI	4.25	
Tax on stock income	SI	28.0, 42.0		SI	28.0, 42.0	
Marginal tax ceiling	PI/CI/TI	51.5		PI/CI/TI	51.5	

Notes: Acronyms are explained in Table 1.21. The regional tax includes municipal, county and church taxes. The Regional Tax Rate in the table is the average across municipalities. Tax rates are cumulative. For example, the marginal tax rate in the top bracket (in the average municipality) in 2008 is equal to 33.16 + 5.48 + 6 + 15 = 59.64 percent. Since this exceeds the marginal tax ceiling (59 percent) however, the ceiling is binding. For labor income, there is a labor market contribution of 8 percent on top of the tax ceiling, but at the same time labor income enters all the other tax bases net of the labor market contribution. The effective tax ceiling on labor income in 2008 is therefore equal to 8.0 + (1 0.08) 59.0 = 62.3 percent. The sum of regional and National taxes (with the exclusion of the stock income tax) can not exceed the Marginal Tax ceiling.

Table 1.23: Elasticity of high-skilled hours: normal hours worked

	(1) $\Delta log\, h^H$	(2) $\Delta log\, h^H$	(3) $\Delta log\, h^H$
$\Delta log\,(1-\tau^H)$	-0.022***	-0.050***	-0.028**
	(0.007)	(0.016)	(0.013)
Log base-year income			-0.008***
			(0.002)
IV	NO	YES	YES
Region F.E.	YES	YES	YES
Overtime Hours	NO	NO	NO
Mean Hours	1888.27	1888.27	1888.27
F-stat Excl. Inst.		1355.00	754.53
P-value Excl. Inst.		0.00	0.00
N Firms	1166	1166	1166
N	26489	26489	26489

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects,firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). We only consider regular hours worked. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. First Stage Regressions are available from the authors upon request. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.24: Elasticity of hours of workers in the residual group

	(1) $\Delta log\, h^H$	(2) $\Delta log\, h^H$	(3) $\Delta log\, h^H$	(4) $\Delta log\, h^H$	(5) $\Delta log\, h^H$	(6) $\Delta log\, h^H$
$\Delta log\,(1-\tau^{Residual})$	-0.014**	0.007	0.007	0.011		0.017
	(0.006)	(0.020)	(0.019)	(0.020)		(0.026)
$\Delta log\,(1-\tau^{Residual}_{5th})$					0.011	
					(0.024)	
IV	NO	YES	YES	YES	YES	YES
Splines of inc. at t	NO	NO	YES	NO	NO	YES
Splines of log t-1 inc. and Δ log inc. t-1-t	NO	NO	NO	YES	NO	NO
5th ord. polynomial inc. t	NO	NO	NO	NO	YES	NO
Base-year inc. above median only	NO	NO	NO	NO	NO	YES
Mean Hours	1876.15	1876.15	1876.15	1879.48	1870.05	1878.65
F-stat Excl. Inst.		407.80	476.59	348.64	377.72	291.47
P-value Excl. Inst.		0.00	0.00	0.00	0.00	0.00
N Firms	932	932	932	792	965	742
N	6246	6246	6246	4962	4958	3123

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects,firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). We only consider regular hours worked. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. First Stage Regressions are available from the authors on request. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.25: Elasticity of hours and labor income: extra specifications

	(1) $\Delta logh^H$	(2) $\Delta logh^H$	(3) $\Delta logh^H$	(4) $\Delta log(\,Labor\,income^H)$
$\Delta log(1-\tau^H)$	0.071**	-0.063*	-0.045***	0.0336***
	(0.035)	(0.037)	(0.015)	(0.0087)
Log base-year income	-0.012	-0.003	-0.008***	-0.1988***
	(0.012)	(0.005)	(0.003)	(0.0063)
Women with kids only	YES	NO	NO	NO
Workers at kinks	YES	YES	NO	YES
Top 10\% income only	NO	YES	NO	NO
Mean Hours	1888.72	1951.85	1927.68	
F-stat Excl. Inst.	189.17	14.46	678.35	5.66e+04
P-value Excl. Inst.	0.00	0.00	0.00	0.00
N	2998	2648	24736	1865067

*Notes: Regression in columns 1 to 3 contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects,firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). We consider both regular and overtime hours worked. In column 4 to be consistent with Kleven and Schultz 2014 we include the following controls: labor market experience, experience, squared, age, gender, marital status, number of kids aged 018 years, educational degree, industry, municipality, local unemployment rate, and base-year fixed effects. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. First Stage Regressions are available from the authors on request. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.26: Elasticity of high-skilled hours: income controls

	(1) High Coord. Top 50%	(2) Low Coord. Bottom 50%	(3) High Coord. Top 50%	(4) Low Coord. Bottom 50%	(5) High Coord. Top 50%	(6) Low Coord. Bottom 50%
Dependent Variable	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^H$
$\Delta log\,(1-\tau^H)$	-0.020	-0.082***			-0.024**	-0.072**
	(0.014)	(0.027)			(0.012)	(0.029)
$\Delta log\,(1-\tau\_5th^H)$			-0.023	-0.115***		
			(0.022)	(0.031)		
IV	YES	YES	YES	YES	YES	YES
Region F.E.	YES	YES	YES	YES	YES	YES
Splines of inc. at t	YES	YES	NO	NO	NO	NO
5th ord. polynomial inc. t	NO	NO	YES	YES	NO	NO
Splines of log t-1 inc. and Δ log inc. t-1-t	NO	NO	NO	NO	YES	YES
Pvalue High=Low	0.05		0.02		0.02	
Mean Hours	1904.10	1847.66	1904.29	1850.89	1907.00	1853.11
F-stat Excl. Inst.	1298.25	461.91	307.72	79.46	857.62	250.09
P-value Excl. Inst.	0.00	0.00	0.00	0.00	0.00	0.00
N Firms	584	583	584	581	537	519
N	19067	7421	17852	6814	15619	5649

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects,firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). "Splines" refer to a flexible piecewise linear functional form with 5 components. τ_{5th} refers to marginal tax rates obtained as in Dahl and Lochner 2012. "P-value High=Low" refers to the p-value of the null hypothesis that the coefficient attached to $\Delta log\,(1-\tau^H)$ in low and high-coordination firms is equal. Observations are weighted by labor income. Coordination is measured using Std. Dev. Definion 1. Standard errors in parentheses are clustered at the firm level. First Stage Regressions are available from the authors on request. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.27: Spillover effects: income controls

	(1) $\Delta log\, h^L$	(2) $\Delta log\, h^L$	(3) $\Delta log\, h^L$
$\Delta log\, \overline{h^H}$	1.152***	1.160***	1.115**
	(0.373)	(0.365)	(0.464)
$\Delta log\,(1-\tau^L)$	0.050	0.044	
	(0.105)	(0.123)	
$\Delta log\,(1-\tau^L_{5th})$			0.030**
			(0.015)
Log base-year income	YES	NO	NO
Splines of inc. at t	NO	YES	NO
5th ord. polynomial inc. t	NO	NO	YES
F-stat Excl. Inst. 13.65, 105.11	17.17, 62.25	3.91, 459.04	
P-value Excl. Inst.	0.00, 0.00	0.00, 0.00	0.05, 0.00
Mean Hours Low Sk.	1809.02	1809.02	1809.49
Mean Hours High Sk.	1877.51	1877.51	1877.50
N Firms	1157	1157	1151
N	14402	14402	13654

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm. "Splines" refer to a flexible piecewise linear functional form with 5 components. τ_{5th} refers to marginal tax rates obtained as in Dahl and Lochner 2012. Observations are weighted by labor income. First Stage Regressions are available from the authors on request. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.28: Elasticity of high-skilled hours: alternative definitions of coordination and data on hours

	(1) High Coord. Top 50% Def. 2	(2) Low Coord. Bottom 50% Def. 2	(3) Low Coord. Bottom 50% Def. 2	(4) Low Coord. Bottom 50% Def. 2	(5) High Coord. Top 50% BFL Hours	(6) Low Coord. Bottom 50% BFL Hours	(7) BFL Hours
Dependent variable	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^L$	$\Delta log\, h^L$	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^L$
$\Delta log\,(1-\tau^H)$	-0.001	-0.092***			-0.008	-0.091**	
	(0.012)	(0.022)			(0.041)	(0.042)	
$\Delta log\,\overline{h^H_{normal}}$			0.684**				
			(0.307)				
$\Delta log\,\overline{h^H_{total}}$				0.760**			
				(0.319)			
$\Delta log\,\overline{h^H_{blf}}$							1.015**
							(0.400)
$\Delta log\,(1-\tau^L)$			-0.016	-0.077			0.187
			(0.107)	(0.113)			(0.291)
Log base-year income	-0.001	-0.022***			-0.022**	-0.010	
	(0.002)	(0.007)			(0.009)	(0.010)	
Overtime hours	YES	YES	NO	YES	NO	NO	NO
BFL hours	NO	NO	NO	NO	YES	YES	YES
Mean Hours	1905.27	1863.52	1760.44	1783.84	1901.01	1854.16	1851.93
Pvalue High=Low	0.00				0.15		
F-stat Excl. Inst.	1034.04	282.28	5.43,35.78	9.88,35.78	962.85	179.52	1.37,33.69
P-value Excl. Inst.	0.00	0.00	0.00,0.00	0.00,0.00	0.00	0.00	0.26,33.69
N Firms	583	583	489	489	477	521	802
N	15701	10788	4749	4749	15521	6330	8562

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). Column 3, 4 and 7 contain controls for flexible piecewise linear functions with 5 components of income at t-1 and the change in income between t-1 and t. BFL hours refer to hours from E-indkomst. Total hours refer to the sum of normal and overtime hours. Coordination is measured using the St. Dev definition 2 in columns 1 to 4 and the St. Dev. definition 1 in columns 5 to 7. Observations are weighted by labor income. Standard errors in parentheses are clustered at the firm level. First Stage Regressions are available from the authors on request. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.29: Uncompensated elasticity and virtual income

	(1) High Coord. Top 50%	(2) Low Coord. Bottom 50%	(3)
Dependent variable	$\Delta log\, h^H$	$\Delta log\, h^H$	$\Delta log\, h^L$
$\Delta log\,(1-\,\tau^H)$	-0.028**	-0.552	
	(0.014)	(6.212)	
$\Delta log\, vy^H$	-0.013	-1.154	
	(0.017)	(15.801)	
$\Delta log\, \overline{h^H}$			0.957***
			(0.283)
$\Delta log\,(1-\,\tau^L)$			-0.008
			(0.065)
$\Delta log\, vy^L$			-0.008
			(0.020)
Log base-year income	0.002	0.429	0.010
	(0.007)	(6.200)	(0.013)
Overtime hours	YES	YES	NO
Mean Hours	1924.91	1907.33	1812.58
Pvalue $\Delta log\,(1-\,\tau^H)$ High=Low	0.98		
Pvalue $\Delta log\, vy^H$ High=Low	0.98		
F-stat Excl. Inst.	2049,43.8	0.65,0.01	23.84,5,78,29.7
N Firms	583	584	968
N	18824	7618	10066

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects,firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). In column 3 we only consider regular hours worked. Observations are weighted by labor income. "P-value $\Delta log\,(1-\,\tau^H)$ High=Low" refers to the p-value of the null hypothesis that the coefficient attached to $\Delta log\,(1-\tau^H)$ in low and high-coordination firms is equal. "P-value $\Delta log\,(1-\,vy^H)$ High=Low" refers to the p-value of the null hypothesis that the coefficient attached to $\Delta log\,(1-\,vy^H)$ in low and high-coordination firms is equal. First Stage Regressions are available from the authors on request. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.30: The effects of the tax reform on firm characteristics

	(1)	(2)	(3)	(4)
	$\Delta log\,(FirmSize)$	$\Delta log\,(ShareHighSk.)$	$\Delta log\,(ShareLowSk.)$	$\Delta log\,(PhysicalCapital)$
$\Delta log\,(\overline{1-\tau^H})$	-0.204	0.161	-0.466	0.063
	(0.398)	(0.349)	(0.357)	(1.481)
Firm Size	-0.000	0.000	-0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Ind. Exp.	-0.055***	0.034**	-0.071***	0.251**
	(0.020)	(0.016)	(0.022)	(0.101)
Ind. Mupltiplant	-0.036*	-0.011	0.025	0.003
	(0.021)	(0.014)	(0.020)	(0.106)
Share of Low Sk.	0.053	-0.527***	-0.214	-0.599
	(0.100)	(0.089)	(0.141)	(0.567)
Share of High Sk.	0.042	-0.128	-0.800***	-0.315
	(0.095)	(0.081)	(0.125)	(0.542)
Mean Log base year (t) income	-0.047	-0.011	0.243**	0.299
	(0.116)	(0.068)	(0.111)	(0.455)
IV	YES	YES	YES	YES
Region F.E.	YES	YES	YES	YES
F-stat Excl. Inst.	116.04	116.04	116.04	117.07
P-value Excl. Inst.	0.00	0.00	0.00	0.00
N Firms	968	968	968	963

*Notes: Each regression contains the following additional controls measured in the base year: average work experience, average work experience squared, share of males, share of married workers, average workers age, average number of children per worker, local unemployment (firm municipality), share of primary, secondary and tertiary educated workers region fixed effects. "Mech." stands for mechanical change. First Stage Regressions are available from the authors on request. F-stat Excl. Inst. refers to the Angrist-Pischke F-statistic. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.31: First Stage regression relative to Table 1.6

	(2)	(3)	(4) High Coord. Top 50%	(5) Low Coord. Bottom 50%	(6) High Coord. Top 50%	(7) Low Coord. Bottom 50%
Dependent variable	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$
$\Delta log\,(1-\tau^H)$ Mech.	1.935***	2.086***	1.942***	2.429***	1.942***	2.429***
	(0.053)	(0.076)	(0.054)	(0.175)	(0.054)	(0.175)
Log base-year income		-0.030***	-0.016***	-0.056***	-0.016***	-0.056***
		(0.007)	(0.004)	(0.016)	(0.004)	(0.016)
Overtime Hours	YES	YES	YES	YES	NO	NO
F-stat	1.36e+03	7.55e+02	1.29e+03	1.93e+02	1.29e+03	1.93e+02
p-value	0.00	0.00	0.00	0.00	0.00	0.00
N Firms	1167	1167	584	583	584	583
N	26488	26488	18858	7630	18858	7630

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). The abbreviation "Mech." stands for mechanical changes. Observations are weighted by labor income. Coordination is measured using Std. Dev. Definion 1. F-stat Excl. Inst. refers to the Angrist-Pischke F-statistic. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.32: First Stage regression relative to Table 1.7

	(1) High Coord. Top 25%	(2) Low Coord. Bottom 25%	(3) High Coord. Top 50%	(4) Low Coord. Bottom 50%	(5) High Coord. Top 50%	(6) Low Coord. Bottom 50%
Dependent variable	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$	$\Delta log\,(1-\tau^H)$
$\Delta log\,(1-\tau^H)$ Mech.	1.952***	2.499***	1.835***	2.182***	1.835***	2.182***
	(0.082)	(0.216)	(0.047)	(0.116)	(0.047)	(0.116)
Log base-year income	-0.010	-0.057***	-0.012***	-0.038***	-0.012***	-0.038***
	(0.006)	(0.014)	(0.004)	(0.008)	(0.004)	(0.008)
Overtime hours	NO	NO	YES	YES	NO	NO
Region f.e.	YES	YES	YES	YES	YES	YES
Firm F.E.	NO	NO	YES	YES	YES	YES
Base-year F.E.	NO	NO	YES	YES	YES	YES
F-stat	566.19	133.53	1542.40	353.25	1542.40	353.25
p-value	0.00	0.00	0.00	0.00	0.00	0.00
N Firms	293	291	785	675	785	675
N	8307	2371	26497	10267	26497	10267

*Notes:Each regression contains the following controls measured in the base year: work experience, work experience squared, sex, age, number of children, marital status, education, local unemployment (municipality),region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). "Mech." stands for mechanical changes. Observations are weighted by labor income. F-stat Excl. Inst. refers to the Angrist-Pischke F-statistic. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01*

Table 1.33: First Stage regression relative to Table 1.8

	(1) $\Delta log\,\overline{h^H}$	(2) $\Delta log\,(1-\tau^L)$	(3) $\Delta log\,\overline{h^H}$	(4) $\Delta log\,(1-\tau^L)$	(5) $\Delta log\,\overline{h^H}$	(6) $\Delta log\,(1-\tau^L)$	(7) $\Delta log\,\overline{h^H}$	(8) $\Delta log\,(1-\tau^L)$	(9) $\Delta log\,\overline{h^H_{total}}$	(10) $\Delta log\,(1-\tau^L)$	(11) $\Delta log\,\overline{h^H_{total}}$	(12) $\Delta log\,(1-\tau^L)$
$\Delta log\,(\overline{1-\tau^H})$ Mech.	-0.432***	-0.185*	-0.432***	-0.178*	-0.438**	0.139	-0.545***	-0.187	-0.277	-0.178*	-0.495**	-0.187
	(0.163)	(0.111)	(0.163)	(0.097)	(0.193)	(0.118)	(0.192)	(0.152)	(0.178)	(0.097)	(0.194)	(0.152)
$\Delta log\,(1-\tau^L)$	-0.063*	0.649***	-0.061	0.492***	-0.061	0.478***	-0.143**	0.858***	-0.038	0.492***	-0.107*	0.858***
	(0.036)	(0.051)	(0.037)	(0.060)	(0.037)	(0.059)	(0.056)	(0.113)	(0.037)	(0.060)	(0.061)	(0.113)
Region F.E.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Splines of log t-1 Inc. and Δlog inc. t-1-t	NO	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Log Mean Inc. High Sk.	NO	NO	NO	NO	YES	YES	NO	NO	NO	NO	NO	NO
Overtime Hours	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES	YES	YES
F-stat Excl. Inst.	13.09	160.40	15.45	76.76	4.66	55.84	11.90	48.55	4.43	76.72	8.39	50.92
P-value Excl. Inst.	0.00	0.00	0.00	0.00	0.03	0.00	0.00	0.00	0.04	0.00	0.00	0.00
N Firms	968	968	968	968	968	968	484	484	968	968	484	484
N	10091	10091	10091	10091	10091	10091	4100	4100	10091	10091	4100	4100

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared,sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted). Observations are weighted by labor income. "Mech." stands for mechanical change. F-stat Excl. Inst. refers to the Angrist-Pischke F-statistic. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01 .*

Table 1.34: First Stage regression relative to Table 1.9 columns 1-2

	(1) $\Delta log\,\overline{h}^H_{normal}$	(2) $\Delta log\,(1-\tau^L)$	(3) $\Delta log\,\overline{h}^H \times$ (Share High Sk.>50)	(4) $\Delta log\,\overline{h}^H_{normal}$	(5) $\Delta log\,(1-\tau^L)$
$\Delta log\,(\overline{1-\tau^H})$ Mech.	-0.165	-0.273**	-0.015	-0.441**	-0.017
	(0.241)	(0.135)	(0.025)	(0.194)	(0.092)
$\Delta log\,(1-\tau^L)$	-0.076*	0.892***	0.015	0.007	0.444***
	(0.043)	(0.078)	(0.020)	(0.012)	(0.090)
$\Delta log\,(\overline{1-\tau^H})\,Me.\times$(Share HS>50)	-0.622	-0.757**	-0.794***		
	(0.387)	(0.310)	(0.291)		
Overtime hours	NO	NO	NO	NO	NO
Firm F.E.	NO	NO	NO	YES	YES
Base-year F.E.	NO	NO	NO	YES	YES
F-stat Excl. Inst.	1.20	71.31	37.34	6.23	24.55
P-value Excl. Inst.	0.27	0.00	0.00	0.01	0.00
N Firms	977	977	977	835	835
N	10196	10196	10196	15985	15985

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared,sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted) and 5 components splines of income at t-1 and income change between t-1 and t. "Share HS" indicates share of high-skilled. Observations are weighted by labor income. F-stat Excl. Inst. refers to the Angrist-Pischke F-statistic. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01*

Table 1.35: First Stage regression relative to Table 1.9 columns 3-5

	(1) $\Delta log\,\overline{h}^H_{total}$	(2) $\Delta log\,(1-\tau^L)$	(3) $\Delta log\,(1-\tau^L)$	(4) $\Delta log\,\overline{h}^H_{normal}$	(5) $\Delta log\,\overline{h}^{Residual}_{normal}$	(6) $\Delta log\,\overline{h}^H_{normal}$	(7) $\Delta log\,(1-\tau^L)$
$\Delta log\,(\overline{1-\tau^H})$ Mech.	-0.357	-0.017	-0.541***	-0.346*	0.007	-0.437***	-0.149
	(0.266)	(0.092)	(0.150)	(0.192)	(0.203)	(0.165)	(0.092)
$\Delta log\,(\overline{1-\tau}^{Residual})$ Mech.			0.006	0.105	0.149**		
			(0.069)	(0.071)	(0.065)		
$\Delta log\,(1-\tau^L)$	0.025*	0.444***	0.883***	-0.066	-0.064	-0.063*	0.487***
	(0.015)	(0.090)	(0.079)	(0.041)	(0.044)	(0.038)	(0.059)
Overtime hours	YES	YES	NO	NO	NO	NO	NO
Firm F.E.	YES	YES	NO	NO	NO	NO	NO
Base-year F.E.	YES	YES	NO	NO	NO	NO	NO
Workers at kinks	YES	YES	YES	YES	YES	NO	NO
F-stat Excl. Inst.	2.45	25.57	122.94	12.16	4.41	13.97	77.48
P-value Excl. Inst.	0.12	0.00	0.00	0.00	0.04	0.00	0.00
N Firms	835	835	799	799	799	958	958
N	15985	15985	9606	9606	9606	9979	9979

*Notes: Each regression contains the following controls measured in the base year: work experience, work experience squared,sex, age, number of children, marital status, education, local unemployment (municipality), region fixed effects, firm size, exporter status, share of high and low-skilled workers in the firm (the residual group is omitted) and 5 components splines of income at t-1 and income change between t-1 and t. First stage regression on column 4 and 5 of Table 1.9 are not shown and available upon request from the authors. "Mech." stands for mechanical change. Observations are weighted by labor income. F-stat Excl. Inst. refers to the Angrist-Pischke F-statistic. Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01*

1.12.2 Standard Deviation of hours Definition 2: tables and graphs

In this section we present the results of a parallel analysis performed using the standard deviation of hours across skills groups, where skill groups are defined at the the intersection of 3 educational groups (i.e. primary, secondary and tertiary education) and 3 broad occupational categories (i.e. manager, middle manager and blue collar) (Section 1.4.3).

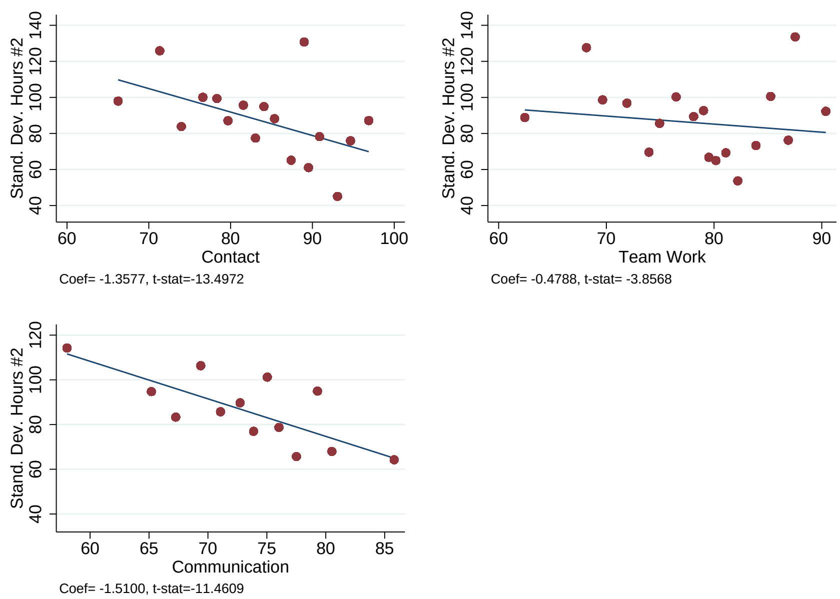


Figure 1.12: Tasks and Coordination of hours (Def. 2 Education-Occupation)

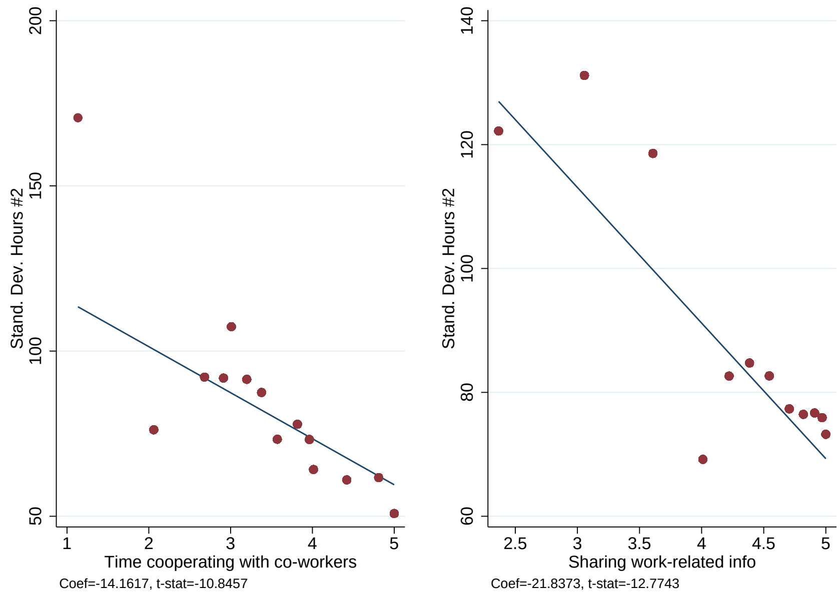


Figure 1.13: PIAAC validation exercise coordination (Def. 2)

Table 1.36: Coordination by sector (def. 2)

	Std. Dev. hours Def. 2 (education occupation)	
Coordination by Industry (2003-2011)		
	Mean	Std. Dev.
Agriculture, forestry and fishing, mining and quarrying	112.25	101.70
Manufacturing	98.55	80.31
Constructions	129.04	96.06
Electricity, gas, steam and air conditioning supply, Trade and transport	68.15	86.97
Financial and insurance, Real estate, Other business	79.00	80.38
Public administration, education, health, arts, entertainment and other services	67.41	65.92
Overall sectors	87.79	89.60
Observations	8182	

Notes: The table shows average values over the period 2003-2011.

Table 1.37: Coordination and Firm Characteristics (Def 2)

	Stand. Dev. Def. 2 (education-occupation)		**Obs.**
	(1)	(2)	
V.A. /employee	-0.037***	-0.014*	17714
	(0.008)	(0.007)	
Capital/employee	-0.006	-0.005***	17714
	(0.007)	(0.001)	
Sales/employee	-0.042***	-0.004	17714
	(0.009)	(0.020)	
TFP	-0.112***	-0.061***	16148
	(0.008)	(0.013)	
Firm size	-0.018**	-0.050***	17714
	(0.007)	(0.015)	
Share of tertiary educ.	-0.139***	-0.061***	17714
	(0.008)	(0.014)	
Number of plants	-0.022***	-0.027	17714
	(0.007)	(0.017)	
Exporter status	-0.133***	-0.009	17714
	(0.007)	(0.010)	
Fraction of hourly work.	0.317***	0.235***	17714
	(0.007)	(0.017)	
Fraction of Unionized work.	0.095***	0.025**	17714
	(0.008)	(0.012)	
Fraction of Females	-0.019**	0.061***	17714
	(0.008)	(0.016)	
Fraction of Part-Time work	0.207***	0.121***	17714
	(0.008)	(0.014)	
Mean Managerial Ability	-0.055***	-0.022**	17714
	(0.008)	(0.011)	
Negotiation	-0.291***	-0.128***	16401
	(0.009)	(0.015)	
Persuasion	-0.298***	-0.134***	13353
	(0.009)	(0.015)	
Social Perceptiveness	-0.277***	-0.099***	13353
	(0.009)	(0.015)	
Adjust Actions to others	-0.146***	-0.063***	13353
	(0.009)	(0.013)	
5 digits industry f.e.	NO	YES	

*Notes: The table shows standardized coefficients from a regression of the standard deviation of hours across skill groups on firm characteristics. Each cell is a different regression. TFP is obtained from the procedure described in Appendix 1.10.4. To avoid confusion we label the O*NET descriptor "Coordination" as "Adjust Actions to Others". Standard errors in parentheses are clustered at the firm level. * p<0.10, ** p<0.05, *** p<0.01.*

Table 1.38: Coordination and wage premiums

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.	(6) Firm f.e.
Stand. Dev. Def. 2	-0.070***	-0.047**	-0.042**	-0.077***		-0.038**
	(0.019)	(0.018)	(0.018)	(0.016)		(0.016)
Stand. Dev. Normal Hours					-0.044**	
					(0.019)	
Firm size		0.015*	0.014**	0.038***	0.014**	0.012**
		(0.007)	(0.006)	(0.014)	(0.007)	(0.005)
Exporter status		0.069***	0.083***	0.085***	0.084***	0.081***
		(0.015)	(0.017)	(0.025)	(0.018)	(0.016)
Union. Rate		-0.003	0.047*	0.038	0.046*	0.053**
		(0.025)	(0.026)	(0.029)	(0.026)	(0.025)
Female Share		-0.055	-0.070**	-0.077***	-0.067*	-0.049**
		(0.045)	(0.034)	(0.028)	(0.035)	(0.019)
Average Hours		0.003	-0.011	0.002	-0.012	-0.039
		(0.025)	(0.025)	(0.023)	(0.025)	(0.026)
log(Cap/empl)		0.038***	0.067***	0.083***	0.067***	0.064***
		(0.012)	(0.013)	(0.017)	(0.013)	(0.014)
Negotiation						0.201
						(0.123)
Persuasion						-0.151***
						(0.056)
Social Perceptiveness						0.017
						(0.068)
Adjust Actions to others						-0.034*
						(0.017)
Region F.E.	NO	YES	YES	YES	NO	YES
Compos. cntr	NO	NO	YES	YES	NO	YES
Ability Measures	NO	NO	YES	YES		YES
Av. Hours b/w 36.5 and 37.5	YES	YES	YES	NO	NO	YES
Part. R-sq SD Hours	0.006	0.002	0.002	0.002	0.002	0.001
Part. R-sq VA and Sales	0.022	0.010	0.006	0.006	0.008	0.005
Coordination Share	0.276	0.251	0.280	0.260	0.255	0.227
R-sq	0.006	0.031	0.072	0.073	0.072	0.079
N	7285	7285	7285	4392	7271	6067

*Notes: The "Stand. Dev." is the standard deviation of the average total hours worked across skill groups within a firm. The Stand. Dev. of Normal hours is the standard deviation of the average normal hours worked across skill groups within a firm. Skill groups are defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM model. All regressions show standardized coefficients. The exporter dummy is derived as the modal exporter status between 2003 and 2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. "Compos. cntr" refers to a vector of controls for the share of workers in each skill group. "Ability Measures" indicate a vector containing the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. The dependent variable (firm f.e.) in column (5) is based on the wage rate from normal hours. To avoid confusion we label the O*NET descriptor "Coordination" as "Adjust Actions to Others" Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and TFP". "Part. R-sq VA and Sales" is from Table 1.19. Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Table 1.39: Coordination and wage differentials within sectors

	(1) Firm f.e.	(2) Firm f.e.	(3) Firm f.e.	(4) Firm f.e.	(5) Firm f.e.	(6) Firm f.e.	(7) Firm f.e.	(8) Firm f.e.
Stand. Dev. Def. 2	-0.038**	-0.031*	-0.028				-0.038**	-0.032*
	(0.016)	(0.017)	(0.017)				(0.019)	(0.019)
Median Abs. Dev. Def. 2				-0.049***	-0.037**	-0.034**		
				(0.015)	(0.015)	(0.015)		
Firm size	0.013**	0.009*	0.021*	0.013**	0.009*	0.020*	0.015**	0.014**
	(0.006)	(0.005)	(0.011)	(0.005)	(0.005)	(0.010)	(0.007)	(0.006)
Exporter status	0.058***	0.039***	0.031**	0.054***	0.037***	0.029**	0.086***	0.077***
	(0.017)	(0.013)	(0.013)	(0.017)	(0.013)	(0.013)	(0.017)	(0.018)
Union. Rate	0.038	0.035	0.033	0.038	0.035	0.033	0.050**	0.058***
	(0.028)	(0.031)	(0.031)	(0.029)	(0.032)	(0.032)	(0.025)	(0.022)
Female Share	-0.085**	-0.037	-0.016	-0.085**	-0.037	-0.017	-0.078**	-0.063**
	(0.036)	(0.024)	(0.021)	(0.036)	(0.025)	(0.023)	(0.033)	(0.025)
Average Hours	-0.019	-0.030	-0.036	-0.022	-0.033	-0.038*	-0.013	-0.019
	(0.023)	(0.024)	(0.023)	(0.021)	(0.022)	(0.021)	(0.025)	(0.025)
log(Cap/empl)	0.057***	0.043***	0.044***	0.058***	0.045***	0.047***	0.067***	0.021
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.013)	(0.029)
log(VA/empl)								0.145**
								(0.071)
Region f.e.	YES	YES	YES	YES	YES	YES	YES	YES
Compos. and Ability cntr.	YES	YES	YES	YES	YES	YES	YES	YES
1 digit Sector f.e.	YES	NO	NO	YES	NO	NO	NO	NO
2 digits Sector f.e.	NO	YES	NO	NO	YES	NO	NO	NO
3 digits Sector f.e.	NO	NO	YES	NO	NO	YES	YES	YES
Part. R-sq SD Hours	0.001	0.001	0.001	0.002	0.001	0.001	0.002	
Part. R-sq VA and Sales	0.009	0.005	0.004	0.009	0.005	0.004		
Coordination Share	0.163	0.171	0.150	0.113	0.276	0.237		
R-sq	0.065	0.087	0.091	0.066	0.088	0.092	0.076	0.083
N	7240	7240	7240	7306	7306	7306	7035	7035

*Notes: The "Stand. Dev." is the standard deviation of the average total hours worked across skill groups within a firm. The Median Abs. Dev. is the the median absolute deviation of median hours across each skill groups within a firm. Skill groups are defined as deciles of the distribution of $\alpha_i + \beta X_{ijt}$ from the AKM model. All regressions show standardized coefficients. Exporter and industry dummies are based on the median value between 2003 and 2011. (Cap/empl) stands for physical capital over number of full-time equivalent employees. In column (8) TFP is used as an instrument for valued added per employee (log(V.A./empl)). TFP is obtained as described in Appendix 1.10.4. "Compos. cntr" refers to a vector of controls for the share of workers in each skill group. "Ability Measures" indicate a vector containing the average value of the individual fixed effects α_i in each quartile of the distribution of α_i within a firm. Coordination Share is derived as the ratio of "Part. R-sq SD Hours" and "Part. R-sq VA and TFP". "Part. R-sq VA and Sales" is from Table 1.20. Standard errors are clustered at the 2-digit industry level. *, ** and *** are 10, 5 and 1 percent significance levels.*

Chapter 2

The Effects of a Temporary Migration Shock: Evidence from the Arab Spring Migration through Italy

Abstract

This study estimates the short-term effects of migration on employment of native workers in Italy using the exogenous, unanticipated and temporary migration resulting from the Arab Spring. While migration does not have overall effects on native employment, I find significant and offsetting short-term effects across industries. In negatively affected sectors, I estimate quarterly displacement effects

that range between 0.68 and 0.8 displaced natives for every immigrant hired. The positive employment effects are consistent with a rise in sectoral employment operating through increased demand from immigrants. Both positive and negative effects on employment tend to dissipate over time.

2.1 Introduction

The debate over the effects of migration on wages and employment of native workers is a long standing one in the labor economics literature. Several studies exploit the heterogeneous distribution of immigrants across local labor markets to derive causal estimates. Most of these studies find little or no impact of migration on wages and employment.[1] This evidence is inconsistent with the standard labor market model[2] and it fails to explain why migration remains at the core of the political debate in many countries. To account for the lack of effects, the literature has proposed different channels through which either native workers or firms respond to migration over time, equalizing wages and employment opportunities across local labor markets.[3] These findings suggest that the short-run effects of migration on

[1]See for example Altonji and Card 1991, Card 1990, Card 2005, Card and Lewis 2007 in the Unites States, Hunt 1992 in France, Pischke and Velling 1997 in Germany, Dustmann, Fabbri, and Preston 2005 in the UK and González and Ortega 2011 in Spain.

[2]This is true assuming that the labor demand is neither perfectly elastic nor perfectly inelastic. In a case where the labor demand is perfectly inelastic, migration has no effect on employment. On the contrary, if the labor demand is assumed to be perfectly elastic, migration has no effect on wages.

[3] Natives might move out of the regions more affected by migration toward those less affected, thus offsetting the differential effects of migration across regions (Borjas 2003; Borjas 2006; Monras 2014). Native and immigrants might specialize in performing complementary tasks (Peri and Sparber 2009). Firms might adjust their use of labor inputs to the local labor supply, thus reducing the effects of migration on wages and employment (Lewis 2003; Card and Lewis 2007;

labor market outcomes may be different from the medium or long-run effects.

While the long-term effects of migration have been widely studied, to date little has been done to study how labor markets react to migration in the very short run. This study provides new estimates of the impact of migration on the employment of natives that isolate the short-term component from other confounding factors or longer-term responses. This is accomplished by using quarterly data while exploiting the unique characteristics of the migration to Italy resulting from the Arab Spring.

There are multiple reasons why migration following the Arab Spring is particularly suitable for this analysis. First, the political instability in Northern Africa was likely exogenous to the dynamics prevailing in Italian local labor markets at the time of the uprisings. Second, the Arab Spring caused a large spike in immigrants to Italy in a relatively short time period. In the first 6 months of 2011 the share of immigrants residing in Italy and originating from the Arab Spring countries of Egypt, Libya, Tunisia and Yemen increased by 23%. In the first three quarters of 2011 more than 57,000 individuals entered Italy illegally, averaging 19,000 individuals per quarter. This compares to an average of 1,300 individuals per quarter in the two years prior to the uprisings. Finally and most importantly, the Arab Spring induced a temporary migration wave. It is estimated that 95% of the legal immigrants who arrived in Italy during the uprisings left the country within one year.

Lewis 2011).

The temporary, unanticipated and exogenous nature of the Arab Spring migration combined with the use of quarterly data, makes it possible to isolate the immediate effects of migration from the longer-run adjustments that it induces. Several existing studies analyze migration events that last for years or decades. In doing so they estimate the long-run effects of migration. Fewer studies focus on short-lived migration waves triggered by exogenous factors and those that do, use data at annual or lower frequency (e.g. Card 1990; Hunt 1992; Carrington and Lima 1996). While the short-lived migration events previously studied are usually associated with immigrants who take up permanent residence in the destination countries, the Arab Spring migration was temporary. Most of the immigrants in fact, left Italy within a short time period. The temporary nature of the migration wave analyzed here and my use of data at higher frequency, both result in a lower chance of capturing attenuating responses to migration.

My estimation strategy exploits the heterogeneous distribution of immigrants across Italian regions. As in many studies that use the geographical variation of migration flows, I account for endogenous settlement by using the share of immigrants from the same origin country previously living in each Italian region as an instrument for the distribution of immigrants across regions during the Arab Spring. I combine this static measure of migration intensity with the change in the flow of illegal entries into Italy over the same period to form a dynamic instrument.

Two general findings emerge from this analysis. First, while the short-run effect of migration on overall native employment is small and insignificant, I find significant and offsetting effects across industries. In particular, I find that the Arab Spring migration has a positive effect on the employment of natives in construction and educational services and a negative effect in mining, wholesale trade, hotels and restaurants. To explain the coexistence of positive and negative employment effects, I show evidence of a shift of native workers across industries. With some assumptions, I estimate that an inflow of 1,000 new immigrants into a region caused around 50 native workers to move from either mining, wholesale trade or hotels and restaurants into construction. These findings corroborate those of other recent studies that find that workers who experience negative local shocks are more likely to move across firms and industries (Foged and Peri 2013; Dix-Carneiro and Kovak 2015). I also discuss the possibility that employment in educational services and in construction increased as an effect of the contemporaneous increase in the demand of educational services and housing induced by the Arab Spring migration.[4] The role of immigrants as consumers whose actions impact native employment better explain the positive effects of short-term migration on employment than the standard complementarity argument.[5]

Second, in the industries in which employment is affected, temporary migration has significant and sizable immediate effects that tend to dissipate as the migration wave ends. I estimate quarterly displacement effects that range

[4]The rise in demand for housing can be seen as caused either directly by immigrants seeking housing, or indirectly through state-financed housing programs for refugees.

[5]Differently from what implied by the standard complementarity argument in fact, I find no evidence of immigrants entering the sectors in which native employment goes up. I also find no positive effects of migration on the earnings of native workers.

in between 0.68 and 0.8 displaced natives for every immigrant that finds a job. These are up to 2.6 times as large as the annual displacement effects estimated by other studies in Europe.[6] As immigrants flee Italy, I find that native employment gradually converges back to the pre Arab Spring level in most of the sectors. Across all sectors that migration has impacted, I estimate that around 50% of the quarterly effects disappear within 1 year. While the temporary nature of the Arab Spring makes comparisons with other studies difficult, my findings are generally in line with the literature on the dynamic adjustments of wages to migration. This literature, based on more permanent migration events, also finds effects that shrink over time (Cohen-Goldner and Paserman 2011; Monras 2014).

Estimating the short-term effects of migration is relevant for multiple reasons. First, it provides a way of understanding what happens soon after immigrants enter a labor market. A finer assessment of those mechanisms may better inform the political debate on migration and help to reconcile the empirical evidence of little or no effect of migration with the predictions from the standard labor market model. Second, while the literature has extensively focused on the effects of migration in final destination regions, to date there is little evidence on the effects of migration in states that are located between origin and final destination countries (i.e. intermediary regions). The results of this study could thus be relevant to other intermediary regions.[7] In Europe for example, the Balkan States and the

[6]Glitz 2012 and Campos Vazquez 2008 estimate annual displacement effects of 0.3 unemployed natives for each immigrant hired.

[7]The motivation and the conclusions of my paper are aligned with the recent literature that highlight how temporary migration can have very different effects than permanent migration

Southern European countries serve as a bridge between the Middle East or Africa and Northern European countries. In Central America, Mexico is known to channel immigrants from Central and South America to the United States and Canada (Hamilton and Chinchilla 1991; García 2006). In the United States, border states such as Arizona or New Mexico are subject to the temporary migration of illegals from the Mexican border.[8]

The paper is organized as follows. Section 2.2 describes the migration through Italy resulting from the Arab Spring. Section 2.3 discusses the data. Section 2.4 introduces the empirical methodology. Section 2.5 and 2.6 present the main results and the robustness checks respectively. Lastly, Section 2.7 concludes.

2.2 The Arab Spring induced migration through Italy

The series of revolts that has become known as the Arab Spring was sparked by the first protest that occurred in Sidi Bouzid (Tunisia) on December 18, 2010. In less than one month protesters succeeded in overthrowing the existing Tunisian government. The success of this first revolt triggered a wave of violent uprisings in the neighboring countries. By August 2011, governments had been overthrown in four countries: Egypt, Libya, Tunisia and Yemen. The chaos that followed the

(Dustmann and Gorlach 2015).

[8] Despite their status as border states, in 2013 Arizona and New Mexico together hosted fewer unauthorized Mexican immigrants (299,000 and 373,000, respectively) than Illinois (Source: MPI elaboration on ACS data) .

ousting of Ben Ali[9] in January 2011 led to the temporary disruption of the coastal control activities performed by the Tunisian police. The lack of controls and the political instability of the region triggered a substantial migration from the unstable countries to Italy, the nearest European country.

Figure 2.1 provides graphical evidence of this migration. The top panel plots legal immigrants from Egypt, Libya, Tunisia and Yemen as a percentage of the Italian population. Consistent with the timing of the revolts, in the first half of 2011 the number of immigrants from the Arab Spring countries increased by 23%.[10] This is equivalent to an inflow of about 56,000 individuals, around 42,000 of whom were of working age. In the same 6 months the growth rate of the Italian population remained close to zero at about 0.005% (Source: Italian Labor Force Survey Data).

The bottom panel of Figure 2.1 plots illegal entries into Italy through the Central Mediterranean route as a percentage of the Italian population.[11] Due to the proximity of Sicily and the Italian Pelagic islands to Tunisia, the Central Mediterranean route is believed to have channeled most of the illegal immigrant flows induced by the uprisings (Fontex 2012; Fargues and Fandrich, September 2012). Consistent with the timing of the revolts, the bottom panel in Figure

[9]The Tunisian president at the time of the revolts.

[10]The pattern followed by legal immigrants in 2009 seems to reflect the reduction in employment opportunities caused by the Euro crisis. This is, in fact, often mentioned as one of the reasons behind the decline in migration flows toward Europe in the second half of 2009 (see the quarterly report *FRAN 2010 Q1* from Frontex). In this study I focus on the migration flows induced by the uprisings. These are seen as plausibly exogenous to the local labor market conditions at the time of the revolts in Italy (see also Section 2.4.3).

[11]The Central Mediterranean route refers to irregular migration flows from northern Africa towards Italy and Malta through the Mediterranean sea (Frontex).

2.1 shows a spike in illegal entries in the first 3 quarters of 2011. Such a spike amounts to a total inflow of more than 57,000 illegal immigrants, averaging more than 19,000 immigrants a quarter. In contrast, only 10,000 illegal immigrants entered Italy through the Central Mediterranean route in the 2 years prior to the Arab Spring, averaging 1,300 individuals a quarter. The sizable inflow of illegal immigrants within such a short period received extensive coverage by the Italian and the international media.[12] The event prompted a declaration of a state of emergency in February 2011. The joint effort of Tunisian, Italian and European institutions, succeeded in stopping the exceptionally high flow of illegals, which returned to pre-crisis levels by the end of 2011.

The top panel in Figure 2.1 highlights one other important feature of the migration resulting from the Arab Spring. Of the immigrants who arrived in Italy, only a small fraction remained in the country for a period longer than one year. In particular, 95% of the 56,000 legal immigrants who entered Italy in the first half of 2011 left the country by the end of that same year.[13] In other words, the Arab Spring brought about a temporary rather than permanent migration to Italy. The temporary migration flow of Africans through Italy has been increasingly documented in the Italian press over the last few years.[14] Once in Europe, African

[12]E.g. "Italy declares state of emergency after 4,000 illegal immigrants fleeing Tunisia unrest land at its ports in four days" *The Daily Mail*, February 13, 2011. "Italy declares state of emergency over influx of 5,000 Tunisian immigrants" *The Telegraph*, February 13, 2011. "Crescono le richieste di asilo. Il 102% in piu' da Tunisia e Libia" *La Repubblica*, December 5, 2011.

[13]In the last quarter of 2011 there were around 197,000 legal immigrants from Egypt, Libya, Tunisia and Yemen in Italy, only around 3,000 more than in the last quarter of 2010 (Italian Labor Force Survey data).

[14]E.g. "Immigrati, boom di sbarchi ma pochi restano in Italia" *La Stampa*, June 23, 2014. "Immigrati, ne arrivano sempre meno, se ne vanno sempre di piu' e 300 mila sono senza documenti"

immigrants try to move to those countries that can offer better job opportunities (e.g. Germany and Scandinavian countries) and/or lower linguistic barriers (i.e. France).

Despite their short stay, these immigrants were active in the labor market according to data from the Italian Labor Force Survey. In the first two quarters of 2011 in fact, the number of legal immigrants from Arab Spring countries who were employed in Italy went up by 36%. There are several reasons why these temporary immigrants were active in the (formal or informal) labor market. First, the Arab Spring immigrants are likely to have made unplanned decisions to migrate. Once in Italy, they likely worked for a short time period to finance further migration. Second, immigrants with a pending or refugee status are in principle eligible to receive state-financed assistance. As the Italian and the international media have documented however[15], these assistance programs could accommodate only a very limited number of people. The other immigrants were left to fend for themselves. To quote Laura Boldrini, the spokeswoman for the United Nations High Commissioner for Refugees in Italy at the time of the uprisings: "If you're not lucky enough to get one of those (spots in the assistance programs), you're on your own. You have to find a way to support yourself, learn the language, get a house and a job".[16]

La Repubblica, December 10, 2013.

[15]E.g. "Gli hotel della disperazione. L'emergenza infinita dei rifugiati" *La Repubblica*, January 2, 2013.

[16]Quote extracted from "In Italy, Shantytowns of Refugees Reflect Paradox on Asylum" *New York Times*, December 26 2012.

2.3 The Data

I collect labor market and demographic information from the restricted version of the Italian Quarterly Labor Force Survey (LFS). This survey collects data on about 70,000 households in 1,246 Italian municipalities for a total of 175,000 individuals representing 1.2% of the overall Italian population. The reference population consists of all household members residing in Italy. The survey is representative of the population at the regional level.

Consistent with the timing of the uprisings, I restrict the analysis to the years 2009-2012. The analysis is conducted at the regional level. I restrict the sample to the Italian citizens between 15 and 64 years old, who are defined as natives.[17] Starting from micro level data I derive the employment rate of natives in each one of the 20 Italian regions, for each quarter and industry. I use the two-digit SIC code information contained in the LFS, to classify employed workers in 12 industries.[18] Tables 2.11 and 2.12 in the Appendix show descriptive statistics on regional employment and earnings of natives by sector. Following the same aggregation procedure, I construct an extensive set of regional controls. Table 2.1 shows descriptive statistics on the complete list of controls used throughout the analysis. The LFS records both formal and informal employment relations[19];

[17]In Section 2.6 I also consider a specification in which natives are defined as individuals born in Italy.

[18]More details on the industry classification can be found in the online Appendix 2.10. In aggregating individual level data to the regional and sectoral level, I use the individual weights provided by ISTAT.

[19]The studies that compare administrative and LFS data confirm that survey data capture

however, it is impossible to distinguish between the two. In light of this limitation, we will only be able to analyze the combined effects of migration on formal and informal native employment.

Since the analysis is centered around the migration resulting from the Arab Spring, I restrict the attention to the immigrants born in Egypt, Libya, Tunisia or Yemen. In all of these countries the government was overthrown by popular rebellion resulting in considerable chaos and political instability, conditions that in turn led people to migrate. For each Italian region and quarter I derive the change in the total number of immigrants from the Arab Spring countries relative to the native population.[20] Migration flows from those countries, measured through LFS data, can be considered a reliable proxy of the actual flows. The correlation between (annual) administrative data and LFS data on the number of immigrants in Italian regions is around 0.95.[21] In between the last quarter of 2010 and the first six months of 2011 (i.e. the peak of the Arab Spring), the average share of legal immigrants from these countries increased by 0.05%, or the equivalent to an inflow of around 900 individuals in the average region. Some of these immigrants

informal employment. In particular, Cascioli 2006 compares administrative data (*INPS*) and LFS data on employment in four Italian provinces and finds a 23% higher employment figure in the LFS than in the administrative data. A greater difference is found in segments of the labor market characterized by a greater concentration of informal jobs such as those employing workers younger than 23, or jobs in agriculture, construction, trade, hotels and restaurants.

[20]Unfortunately, the available information on the dates of entry of immigrants does not appear to be reliable enough to be used. According to that measure in fact, most of the immigrants arrived prior to 2011. If that were true, then the pattern we observe in the top panel of Figure 2.1 would have to be explained either as incorrectly recorded data on the date of entry or as the result of temporary returns by immigrants who had previously lived in Italy. Other variables in the same survey however, indicate that there was no increase in return migration in 2011.

[21]Administrative data from "Resident foreigners on January 1st - Citizenship" (ISTAT).

actively participated in local labor markets. During this period in fact, the number of employed immigrants from the Arab Spring countries went up by 24% in the average region (Table 2.1).

I use ISTAT data on the number of Egyptian, Libyan, Tunisian and Yemenite citizens residing in Italy in 2003, to reconstruct the past shares of Arab Spring immigrants living in each region.[22] The 2003 shares of resident immigrants vary widely across regions with some of the regions showing shares that are more than 9 times smaller than the average (Table 2.1). This variation is important to the estimation strategy described in Section 2.4. As part of the instrumental variable approach that I develop, I use Frontex data on the *number of illegal border crossing detections* through the Central Mediterranean route net of arrivals on Malta.[23] The Central Mediterranean route refers to irregular migration flows from northern Africa toward Italy and Malta through the Mediterranean sea. Due to a mix of political and geographical reasons[24], this is the route that channeled most of the illegal migration associated with the uprisings.

Table 2.2 compares demographic and socioeconomic characteristics of natives and legal immigrants from the Arab Spring countries. For the immigrants only, I also differentiate between the pre- and the post-Arab Spring period. More than 60%

[22]The data can be accessed through ISTAT - "Foreign resident population on the January 1st - focus on citizenship". The earliest year for which data on residents by nationality are made freely available online by ISTAT is 2003.

[23]Frontex is the agency that coordinates and develops the European border management. The data on illegal entries are collected by Frontex and provided by the 30 FRAN (Frontex Risk Analysis Network) border control authorities of the member states.

[24]Among the political reasons, the temporary disruption of border patrol activities in Tunisia played a key role (see also Section 2.4). Geography is also important as Italy and Malta are closer to Tunisia than is any other European country (Section 2.2).

of the legal immigrants in the database are males. Most of them are from Tunisia, the closest country, and Egypt, the most populated country. Even if immigrants and natives have similar educational attainments, the former are much more likely to work in agriculture, construction, hotels and restaurants. This descriptive evidence is generally consistent with the part of the literature showing that similarly educated immigrants and natives often work in very different industries (Steinhardt 2011) or perform different tasks (Peri and Sparber 2009). More generally, Table 2.2 highlights the importance of analyzing the effects of migration across different industries. Because the Arab Spring immigrants are unevenly distributed across sectors, we would expect immigration to impact some industries more than others. A comparison between the pre- and post-Arab Spring period suggests that the immigrants moved by the uprisings are more likely to be Tunisian males, and are by comparison younger and less educated than the immigrants from the Arab Spring countries who lived in Italy prior to the revolts. This is consistent with the information available from Frontex on the characteristics of the immigrants who arrived in Italy during the uprisings.[25]

One limitation of the LFS data is that it does not measure illegal migration. To be part of the reference population for the LFS in fact, individuals must have a resident permit valid for at least one year. Because of the reasons underlying the Arab Spring migration, some of the illegals likely received resident permits

[25]In particular, the quarterly reports from Frontex *FRAN Q1 2011* and *Q2 2011* document the abnormal migration of Tunisians during the uprisings. *Fran* Q2 2012 describes the Tunisians arriving in Italy as "young (18-35 years) unmarried males with primary-school level of education".

within 3 months of their arrival. Based on Eurostat data, around 30% of the asylum applications submitted in 2011 were accepted in first-instance decisions.[26] When granted, asylum allows immigrants to receive a resident permit valid for at least one year and permission to work legally in Italy. Italian laws prescribe a limit of 8 days from arrival to apply for asylum. Authorities then have 30 days to interview the applicant and to collect and receive the required documentation. The decision on an asylum application must take place within 3 working days from the interview. Immigrants on a pending or refugee status are allowed to move within the Italian territory. In the case of exceptions to this framework the procedure might be lengthened or shortened. In particular (and most relevant to our case), the illegal immigrants who are held in an Identification and Expulsion Center (CEI) are subject to a prioritized procedure that reduces the length of the entire process to 9 days.[27] Since most of the illegal immigrants from the Arab Spring arrived on the Italian Pelagic Islands and were then transferred to the mainland, most of them were likely held in a CEI. For immigrants held in a CEI the average length of detention was 38 days[28], suggesting that many of them received a decision within our reference period of 3 months. Even if some of the illegals became legals and were therefore likely captured in the LFS, the data at hand do not provide a

[26]The Italian authorities received 24100 asylum applications, 16960 of which were rejected. Source: Eurostat-*First instance decisions on applications by type of decision.*

[27]If the authorities require further documentation or if there are exceptional circumstances the time limit can be extended to a maximum of 18 months. More detailed information is available from The Asylum Information Database under the section "Short overview of the asylum procedure" made available by the Italian Council for Refugees.

[28]Source: Asylum Information Database - Annual Report 2012/2013.

measure of the flows of illegal immigrants who never became legal. In Section 2.4 I discuss how to better estimate the employment effects of migration taking this limitation into account.

2.4 The Empirical Strategy

2.4.1 The short-term effects of migration

To estimate the short-term effects of the Arab Spring migration on changes in employment I exploit the variation of migration flows across Italian regions. The estimating equation takes the following form:

$$\frac{E_{rt+1} - E_{rt}}{pop_{rt}} = \alpha_1 + \alpha_2 \, \frac{(Im^{AS}_{rt+1} - Im^{AS}_{rt})}{pop_{rt}} \times 100 + \alpha_3 \, \Delta X_{rt+1} + \gamma_y + \gamma_t + \gamma_r + \Delta\epsilon_{rt+1} \tag{2.4.1}$$

where $\Delta Im^{AS}_{rt+1} = \left[(Im^{AS}_{rt+1} - Im^{AS}_{rt})/pop_{rt}\right] \times 100$, is the quarterly percentage change in the number of immigrants from the Arab Spring countries in region r relative to the lagged value of the working age native population in the same region (i.e. pop_{rt}). E_{rt} is the number of native workers employed in region r at time t, X_{rt} is a set of region specific controls[29]; and γ_y, γ_t, γ_r are (respectively) year, quarter and region fixed effects. The coefficient of interest in equation (2.4.1) is α_2. Under

[29]These controls include: average age, the share men in the population, the regional population, full-time, white collar and tenured workers as a fraction of the workers employed, the share of high school and college graduates.

the assumptions discussed in Section 2.4.3, this captures the short-term effects of migration on the change of native employment as a fraction of the population in a region. To account for heterogeneous effects of migration across industries, I also estimate equation (2.4.1) separately for employment of natives in 12 different sectors. I do so by using the change in regional native employment relative to the population in each sector as the dependent variable in equation (2.4.1), while keeping the same set of independent variables. A separate regression is estimated for each sector.

2.4.2 The dynamic effects of temporary migration

To analyze the dynamic evolution of the effects of the Arab Spring migration, I relate quarterly migration flows to employment changes measured at a progressively lower frequency. To account for the effects of migration flows that occurred after the end of a quarter (i.e. contemporaneous flows), I also control for the change in the number of immigrants between the end of the quarter and the end of the reference period for the employment change. The estimating equation follows:

$$\begin{aligned}\frac{E_{rt+i} - E_{rt}}{pop_{rt}} = \alpha_1^i + \alpha_2^i \frac{(Im_{rt+1}^{AS} - Im_{rt}^{AS})}{pop_{rt}} \times 100 + \alpha_3^i \frac{(Im_{rt+i}^{AS} - Im_{rt+1}^{AS})}{pop_{rt+1}} \times 100 + \\ \alpha_4^i \Delta X_{rt+i} + \Gamma_{ytr}^i + \Delta\epsilon_{rt+i}\end{aligned} \tag{2.4.2}$$

In estimating this model I focus on employment changes over 3, 6, 9 and 12 months so that the subscript i ranges between 1 and 4.[30] I control for Γ^i_{ytr} that is a vector of region, year and quarter fixed effects, while the other terms follow the same notation as in equation (2.4.1). The coefficient of interest in this model is α^i_2. When estimated over progressively greater i, this describes the evolution of the short term effects of migration over time. Equation (2.4.2) is estimated over a rolling panel in which I keep the observation window fixed (i.e. 2009-2012). I restrict the analysis of the dynamic effects to the sectors that are significantly affected on a quarterly basis by migration.

2.4.3 Identification

In this empirical model, the identification of the effects of migration on employment hinges on two conditions. First, there must be variation in migration flows across Italian regions. Second, migration flows to each region need to be uncorrelated with unobserved factors driving changes in native employment.

As regards the first condition, Figure 2.2 shows the distribution of legal migration flows across regions at the peak of the Arab Spring migration. The figure highlights a substantial heterogeneity, with the northern regions experiencing larger relative inflows. Presumably because of their proximity to the countries affected by the uprisings, some regions in the south, such as Sicily and Apulia, also experienced moderately large flows. Turning to the second condition, Figure 2.2 suggests that

[30] At frequencies lower than 12 months, the instrument is weak due to the limited number of observations. This results in noisier second stage estimates which are shown in the Appendix (see Section 2.5).

it is unlikely to be satisfied in this setting. The figure shows that the Arab Spring immigrants moved to the richer regions in the north. These regions were also more likely to experience higher employment growth. This suggests that there might be a positive spurious correlation between migration flows to a given region and employment changes. This would create an upward bias in the estimated effects of migration on employment.

To deal with the endogenous settlement of immigrants across regions, I use the share of immigrants from the Arab Spring countries living in each region in 2003 as an instrument for the distribution of immigrants across regions during the Arab Spring. I combine this static measure of migration intensity with the quarterly change in illegal entries from the Central Mediterranean route to form a dynamic instrument which takes the following form:

$$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right) \tag{2.4.3}$$

where $\Delta Illegal_{t+1}$ is the quarterly change in the illegal entries through the Central Mediterranean route. Im^{AS}_{r2003} is the number of immigrants from Egypt, Libya, Tunisia and Yemen (i.e. the Arab Spring countries) living in region r in 2003. Im^{AS}_{2003} is the total number of immigrants from the Arab Spring countries living in Italy in 2003.[31] When used to predict migration flows at a frequency lower than 3

[31]One alternative way of instrumenting migration flows would be to use the breakdown on single nationalities. Unfortunately when migration flows to each region in each quarter are broken down by nationality, the number of observations I am left with for some regions is very small. The resulting quarter to quarter changes in migration flows are thus much more volatile and the instrument lacks the precision to perform reliable analyses. I use the same instrument for all

months, the instrument (2.4.3) is adjusted to match the timing of the endogenous variable. In particular, in the case of the endogenous variable $Im^{AS}_{rt+i} - Im^{AS}_{rt+1}$ in equation (2.4.2), $\Delta Illegal$ in (2.4.3) is set equal to $Illegal^{AS}_{t+i} - Illegal^{AS}_{t+1}$.

The relevance of the instrument relies on the fact that immigrants tend to move to those regions that host large communities of immigrants from the same countries of origin. This is likely due to older immigrants providing new arrivals with primary help while integrating them into existing networks that offer better chances of finding a job (Munshi 2003).[32]

The validity of the instrument requires that three conditions are satisfied. First, the unobserved factors that determine the variation of native employment in each region or in a specific sector in each region, need to be uncorrelated to the determinants of settlement choices in 2003. Second, the flow of illegal entries through the Central Mediterranean route needs to be uncorrelated with the unobserved part of the variation of the native employment in each Italian region or in a given industry in each region. Third, the instrument affects the changes in employment of natives only through migration changes (i.e. the exclusion restriction).

As concerns the first condition, the main threat to its validity is the existence of unobserved factors specific to a region or a region's industry that drive employment growth and that persistently affect the settlement of immigrants over

sectors because administrative data on 2003 shares of immigrants by sector of employment are not available.

[32] This regularity was at first noticed in Bartel 1989 and then used in many other studies to estimate the effects of migration on employment and wages (e.g. Altonji and Card 1991; Hunt 1992; Pischke and Velling 1997; Cortes 2008)

time. In order to reduce these concerns, I have region fixed effects in the baseline specification. When the model is estimated separately for each industry, these fixed effects capture constant differential in sectoral employment growth rates across regions. In Section 2.6 I then show that the results are robust to an exhaustive set of robustness checks that add, for instance, regional and sectoral trends to the baseline model or that use the 1995 shares in place of the ones from 2003. The fact that the baseline results are not affected in a significant way by the inclusion of these controls suggests that this issue is not likely to play a determinant role in my setting.

Turning to the second validity condition, the spike in illegal entries over the first six months of 2011 is due, to a large extent, to the disruption of the border patrol activities on the Tunisian shores. This was one of the unintended consequences of the revolts. Following the fall of Ben Ali's regime, part of the domestic security forces (e.g. police, national guards) deserted the police stations as they had become targets of violent attacks during the revolution. For weeks, the Tunisian army remained the main law enforcement body operating within the entire Tunisian territory. This resulted in loosened border patrol activities and thus in massive departures of immigrants from Tunisia towards Italy (Boubakri 2013). The flow of illegals due to this shock is likely to be uncorrelated with the unobserved part of native employment changes in local labor markets in Italy.[33]

[33]Migration flows from the Arab Spring could be measured using national legal flows (as in Cortes 2008). We believe that illegal flows better capture the exogenous part migration resulting from the Arab Spring.

Regarding the third condition for validity, we might worry that the revolts had disruptive effects on the trade flows between Italy and the Arab Spring countries. If the regions to which immigrants move are also those that maintain closer trade relations with the Arab Spring countries, then the existence of trade effects might violate the exclusion restriction. In fact, the Arab Spring might affect native employment through trade and not only through migration. As I discuss in Section 2.5, most of the employment effects that I find are in sectors that produce non-tradable goods. This finding suggests that the issue of trade plays no major role in this setting.

2.4.4 Legal and Illegal migration

One data limitation that we face is the unavailability of measures of illegal migration at the local level (Section 2.3). Such a limitation comes into play in our empirical strategy because if illegal and legal flows to a region are correlated, the exclusion restriction is not satisfied. The estimated employment effects would, in fact, need to be interpreted as a combination of the effects of legal and illegal migration rather than the effects of legal migration only. To take this problem into account, I evaluate the employment effects of migration under three different specifications of equation (2.4.1). One specification combines legal and estimated illegal flows, another considers legals only and the third one considers a regression in reduced form. In what follows, I describe these specifications in details.

In the first specification, I consider both legal and illegal flows. Since I do not observe actual illegal flows to a region, I estimate them assuming that illegal entries, net of those who become legal, distribute across regions in the same way as legal immigrants do. In particular, my measure of illegal flows to a region is:

$$\Delta Illegal_{rt+1} = \frac{Illegal_{t+1}}{pop_{rt}} \times \left(\frac{Legal^{AS}_{rt+1}}{Legal^{AS}_{t+1}}\right) \times (1 - Asylum_{t+1}) \times 100 \qquad (2.4.4)$$

where $Illegal_{t+1}$ is the number of illegal entries in Italy through the Central Mediterranean route at time $t+1$ and $Asylum_{t+1}$ is the ratio between the number of asylum applications that are accepted in first instance decision and the total number of asylum applications at time $t+1$.[34] $Legal^{AS}_{rt+1}$ is the total number of legal Arab Spring immigrants in region r at time $t+1$ while $Legal^{AS}_{t+1}$ is the total number of legal immigrants in Italy at $t+1$. Legal migration is measured using LFS data. In estimating the employment effects, I then combine the illegal flows from (2.4.4) and the legal flows ($\Delta Legal^{AS}_{rt+1}$), to construct a measure of the total migration to a region. The estimating equation takes the following form:

$$\frac{E_{rt+1} - E_{rt}}{pop_{rt}} = \alpha_1 + \alpha_2 \left(\Delta Legal^{AS}_{rt+1} + \Delta Illegal_{rt+1}\right) + \alpha_3 \Delta X_{rt+1} + \gamma_y + \gamma_t + \gamma_r + \Delta\epsilon_{rt+1}. \qquad (2.4.5)$$

where $\Delta Legal^{AS}_{rt+1} = \left[(Legal^{AS}_{rt+1} - Legal^{AS}_{rt})/pop_{rt}\right] \times 100$ is the quarterly percentage change in legal immigrants from the Arab Spring countries relative to the population.

In the second specification, I consider legal migration only. This is done by

[34] Source: Eurostat-*First instance decisions on applications by type of decision.*

estimating the following model:

$$\frac{E_{rt+1} - E_{rt}}{pop_{rt}} = \alpha_1 + \alpha_2 \, \Delta Legal^{AS}_{rt+1} + \alpha_3 \, \Delta X_{rt+1} + \gamma_y + \gamma_t + \gamma_r + \Delta\epsilon_{rt+1} \quad (2.4.6)$$

On the one hand, because it is based on data available in the LFS, this specification does not require specific assumptions to estimate migration flows to a region. On the other hand however, if the correlation between legal and illegal flows is positive, α_2 from equation (2.4.6) would overestimate the effects of migration.

The third and last specification is a reduced form model in which I substitute the instrument (2.4.3) to ΔIm^{AS}_{rt+1} in estimating equation (2.4.1). This approach however, requires that the number of immigrants who entered Italy legally at the time of the uprisings was relatively small.[35] Otherwise, it overestimates the employment effects of migration.

In estimating both equation (2.4.5) and equation (2.4.6), I use the interaction (2.4.3) as an instrument for the quarterly migration flows to a region. Specifications analogous to equation (2.4.5) and equation (2.4.6) are used in the dynamic case (equation (2.4.2)) to estimate the evolution of the quarterly effects over time.[36]

[35] Unfortunately we are not able to distinguish in the data between legal immigrants who received asylum and those who had legal status already at the time of their entry into Italy.

[36] In this case, illegal flows in each quarter are obtained from (2.4.4) and they are summed over quarters to obtain flows at frequencies lower than 3 months.

2.5 Results

Table 2.3 shows the results obtained from estimating the short-term employment effects of the Arab Spring migration. Column (1) in Panel A shows the first stage regression of the specification that only accounts for legal flows (i.e. equation (2.4.6)). The instrument is relevant and its predictive power is robust to the inclusion of year, region and quarter fixed effects. Based on these estimates, a rise in illegal border crossing detections equivalent to a 1% of the native population increases the number of legal immigrants from the Arab Spring countries by 0.27% in the average (in terms of 2003 shares) region.[37] This is generally in line with the 30% asylum acceptance rate recorded by Eurostat in 2011 in Italy (Section 2.3).

Column (1) in Panel B shows the second stage estimates (i.e. α_2 in equation (2.4.6)). I find no evidence of significant effects of the Arab Spring migration on overall native employment (see row labelled "All sectors"). The aggregate results, however, hide significant differences across industries. When I estimate the effects separately for each sector in fact, I find evidence of negative and significant effects on employment in mining, wholesale trade and hotels and restaurants. In these sectors, an increase of 1% in the ratio of Arab Spring immigrants over natives reduces employment by respectively 0.15%, 1.7% and 1.2%. These negative effects are countered by the positive effects in construction and educational services. I

[37] The average 2003 share across regions is 0.1669. The figure 0.27 is obtained as the product of 0.1669 and the estimate coefficient (i.e. 1.590).

find that an increase in migration flows equivalent to 1% of the native population of a region increases native employment by 1.17% and 1.76% (respectively) in construction and educational services. The OLS estimates reported in column (2) are generally larger than the corresponding IV estimates. This is consistent with the descriptive evidence from Figure 2.2 showing that immigrants moved to faster growing regions in northern Italy.

Column (3) shows the results obtained from estimating equation (2.4.5). Unlike in column (1), in this specification I consider both legal and illegal migration flows. The evidence from this specification is very much in line with column (1). The magnitude of the second stage coefficients is slightly lower in column (3). This reflects the smaller relative effect from a larger overall flow. The difference between the coefficients in column (1) and (3), however, is rather small and statistically insignificant. Column (4) shows the results from the reduced form specification of the baseline equation (2.4.1). While the signs of the coefficients and the standard errors are in line with those of the baseline model, the magnitude of the coefficients is generally greater than in column (1). In fact, this model only exploits the variation in illegal entries through the Central Mediterranean route. It thus ignores legal flows and/or illegal flows from different sources. In doing so, it attributes relatively large effects to a small flow. In comparison with the other two specifications however, the difference is statistically small or insignificant.

Table 2.4 translates the relative coefficients from Table 2.3 into the number of native workers employed in the industries significantly impacted by migration.

The Arab Spring migration led to around 900 new legal immigrants entering the average region.[38] Using the proxy for illegal flows from equation (2.4.4), I estimate an average total inflow of legal and illegal immigrants at around 1500 individuals. With the exception of mining, all the other sectors that were affected experienced non negligible average short-term effects on native employment from this inflow. The impact on the number of people employed was greater when both legal and illegal flows were considered, while it was smaller when only legal or only illegal flows (i.e. reduced form model) were accounted for. The difference between these models however, is statistically small.

Figure 2.3 and Table 2.5 show the evolution of the short-term effects over time in the sectors that are significantly affected on a quarterly basis. The figure plots the coefficient α_2^i from equation (2.4.2) estimated on employment changes over 3, 6, 9 and 12 months.[39] The figure plots both coefficients obtained from legal migration flows and those estimated from legal and illegal flows. It shows that within one year, the short-term effects converge toward zero in most of the sectors involved. At annual frequency the estimated coefficients are statistically indistinguishable from zero in mining, construction, hotels and restaurants and education services.[40] This finding suggests that in these sectors native employment

[38]Here the average region is defined as a region that experienced average migration flows during the peak of the Arab Spring migration. For more details see the footnotes for Table 2.4.

[39]At lower frequencies the number of observations declines and the underlying variation changes. This is reflected in weaker instruments and noisier second stage estimates. With this caveat, Figure 2.5 in the Appendix shows the evolution of the effects up to 24 months later. Based on this estimate, the effects converge toward zero in all sectors after two years. Table 2.15 in the Appendix shows first stage estimates at all frequencies between 3 and 24 months.

[40]Educational services remains marginally significant in the specification in which I only consider legal immigrants.

shifted back to the pre- Arab Spring levels (Table 2.5). The effects are more persistent in wholesale trade, where they remain significant at annual frequency. The average ratio between the coefficients estimated at frequencies lower than 3 months and the quarterly coefficients, indicates that the magnitude of the short-term effect declines by around 15% within the first 6 months and by around 50% within one year.

While the main focus of this analysis is on employment, Table 2.14 in the online Appendix shows the estimated effects of the Arab Spring migration on average earnings. These were obtained from the same estimation strategy discussed in Section 2.4, only here changes in average earnings rather than employment were used as the dependent variable in equation (2.4.6). Overall I find the effects of the Arab Spring migration on earnings to be small. In particular, I only find negative effects on average earnings in the construction sector. The evidence of little effect on earnings is consistent with the institutionalized nature of the wage-setting mechanism in Italy. Existing studies show that reduced flexibility in the labor market can amplify the negative effects of migration on employment while reducing the impact on wages (Angrist and Kugler 2003; Glitz 2012).[41]

[41] In particular, Glitz 2012 finds evidence of employment being more reactive than wages to migration in Germany, a country characterized by institutionalized labor markets.

2.5.1 Discussion of the results

The results of the previous section indicate sizeable short-term effects of migration on native employment. To better measure the relative size of the effects, I use the labor market participation of legal immigrants in the post-Arab Spring period (i.e. 2011 and 2012) and their distribution across negatively affected sectors to obtain a measure of the quarterly displacement effect of migration.[42] I base this calculation on two sets of coefficients. The first one is from the model that only accounts for legal flows (i.e. equation (2.4.6)). The second one considers both legal and (estimated) illegal flows (i.e. equation (2.4.5)). When I consider only legal immigrants, I estimate that on a quarterly basis, in those sectors negatively affected by the Arab Spring migration, on average around 0.8 natives are displaced for each immigrant who is hired.[43] When I consider both legal and illegal flows, the estimated displacement effect goes down to 0.68 natives displaced for each immigrant who finds a job.[44] Among the studies that find a displacement effect of

[42]The average employment rate of immigrants from the Arab Spring countries over the period 2011-2012 is 57.29%. The distribution of employed immigrants across the industries is reported in Table 2.2.

[43]Using LFS data I obtain that 764 working-age immigrants entered the average region over the period 2010Q4-2011Q2. Using the employment rate of legal immigrants, I estimate that 438 were employed (0.5729*764). The employed immigrants are then distributed across industries following Table 2.2. Namely 0.13% of them are employed in mining, 2.76% in wholesale trade and 17.56% in hotels and restaurants. The resulting total inflow into those 3 sectors is thus around 90 immigrants. Based on Table 2.4, this inflow displaced a total of around 72 immigrants. This is the sum of the negative effects in mining (0.12), wholesale trade (41.43), hotels and restaurants (29.97). The ratio of displaced natives over hired immigrants is thus 0.8.

[44]I derive the number of working age illegal immigrants multiplying illegal flows by the share of working age legal immigrants (relative to all legal immigrants) at the peak of the Arab Spring migration (i.e. 2010Q4-2011Q2). I then sum the flow of working-age legal and illegal immigrants to obtain that around 1180 working-age immigrants entered the average region over the period 2010Q4-2011Q2. Using the employment rate of legal immigrants, I estimate that 676 of these immigrants were employed in that period (i.e. 0.5729*1180). The 676 employed immigrants are

migration, Glitz 2012 estimates an annual displacement rate of about 0.3 to 1 in Germany. Using establishment-level data on German workers, Campos Vazquez 2008 finds a similar displacement of 0.3 displaced natives to 1 immigrant hired over a 1-2 year horizon. Federman, Harrington, and Krynski 2006 focusing on Californian manicurists, estimates that, depending on the model specification, 10 new Vietnamese displace 4 to 5 non-Vietnamese manicurists on a yearly basis. Consistent with my findings that capture the short-term effects of migration, the quarterly displacement rate that I obtain is significantly larger than those estimated in the literature. In particular, short-term displacements are between 2.2 and 2.6 times as large as the annual displacement estimated by Campos Vazquez 2008 and Glitz 2012 in Germany, and between 1.3 and 1.6 times larger than the displacement estimated in California by Federman, Harrington, and Krynski 2006.[45] In the specific case of the Arab Spring migration, the significant displacement effects are countered by significant positive effects on employment. Later on in this paragraph I discuss these positive effects in more details.

The results from the previous section also indicate that on average around 50% of the short-term effects dissipate within one year. This corroborates the existing evidence on the dynamic effects of migration that finds shrinking effects of

then distributed across industries following Table 2.2. The resulting total inflow in the 3 sectors negatively affected by migration is of about 138 immigrants. Based on Table 2.4, this inflow displaced a total of 94 immigrants (0.15 in mining, 54.46 in wholesale trade and 39.9 in hotels and restaurants). The ratio of displaced natives over hired immigrants is thus 0.68.

[45] I obtain 2.3 and 2.6 as the ratio of 0.68 and 0.8 over 0.3 (i.e. the displacement in Campos Vazquez 2008 and Glitz 2012). Similarly, I obtain 1.3 and 1.6 from the ratio of 0.68 and 0.8 over 0.5 (i.e. the displacement in Federman, Harrington, and Krynski 2006).

permanent migration waves over time (Campos Vazquez 2008; Cohen-Goldner and Paserman 2011; Monras 2014). The convergence towards zero however, is faster in the case of the Arab Spring migration.[46] This is likely due to the temporary nature of the shock. Since most of the immigrants left Italy within one year in fact, the adjustment to the pre-migration levels happened within a few quarters. While the temporary nature of the migration that I analyze makes it difficult to compare the dynamics that I obtain with those of other studies based on more permanent events, the size of the adjustments that I estimate (i.e. 50%) is generally in line with previous studies that find responses to migration to absorb between 40 and 80% of the effects (Lewis 2003; Borjas 2006; Peri and Sparber 2009).[47]

The comparison between quarterly effects from the Arab Spring migration and annual effects from other studies, as well as the analysis of the dynamics that followed the temporary migration, indicate that migration can lead to substantial action in local labor markets in the short-run. The short-term effects can have both positive and negative sign and, in most sectors, they disappear within one year. The existence of sizable and short-lived effects better links the existing evidence of little long-term effects with the standard labor market model that would predict migration to impact native employment.

I find that migration has positive effects on employment in construction

[46] Campos Vazquez 2008 and Monras 2014 find the effects of migration to vanish within 2 to 5 years.

[47] Borjas 2006 estimates that native migration attenuates the effect of immigration on wages in the United States by 40 to 60 percent. Peri and Sparber 2009 estimate that specialization reduced the wage loss of migration in the United States by around 80%. Lewis 2003 shows that firm adjustments can absorb as much as 80% of the labor supply change associated with migration.

and negative effects in mining, wholesale trade, hotels and restaurants (Table 2.3). A breakdown of the effects by education and type of occupation, shows that the negative and the positive effects in these sectors are concentrated among similar workers (Table 2.20 and 2.19 in the Appendix).[48] In light of this finding, one possible interpretation of the results is that migration caused a shift of workers out of mining, hotels and restaurants and wholesale trade into construction. A fact consistent with this interpretation, is that in the post-Arab Spring period a greater share of immigrants from the Arab Spring countries was employed in the sectors where I find significant displacement effects, compared to the years prior to the uprisings (Table 2.2). In contrast, the share of immigrants employed in construction went slightly down from the pre- to the post-Arab Spring period.

To further investigate the possibility of a sectoral shift, I exploit the instrumental variable approach described in Section 2.4 to estimate the effect of migration on the inflow of workers in construction from mining, wholesale trade, hotels and restaurants.[49] Due to the cross-sectional nature of the LFS data, this requires assumptions about the timing of the shift from one sector to the other.[50] I find that migration causes a larger fraction of workers to switch into construction (Table 2.6).

[48]I classify 1 digit occupations into 4 broader categories following Cattaneo, Fiorio, and Peri 2013 (see Table 2.18 in the Appendix). Recent studies find that migration is associated with natives moving to different types of occupations (Cattaneo, Fiorio, and Peri 2013; Foged and Peri 2013). Those studies also show that the process of job upgrading for low skilled workers, happens over the medium or tong-term. Consistent with this evidence, in the short-run I find that natives do not switch occupation while moving from one sector to the other.

[49]In particular, I use the relative change in the regional flows of natives between the sectors negatively affected and construction as a dependent variable in equation (2.4.6). I deal with the endogenous settlement of legal immigrants using the IV approach described in Section 2.4.

[50]In particular, I assume that the individuals who shifted to construction, did it in the same quarter of the year prior to the interview. See the online Appendix 2.10 for more details.

In particular, I estimate that the arrival of 1,000 new immigrants induced around 50 native workers to move from mining, wholesale trade, hotels and restaurants into construction (Panel B). The magnitude of these effects is in line with the average effects presented in Table 2.4 where an inflow of around 900 immigrants is estimated to cause around 48 more workers to be hired in construction. These findings are generally consistent with recent studies based on individual longitudinal data that find that workers respond to local negative shocks by moving to less affected sectors and/or firms (see Dix-Carneiro and Kovak 2015 and Foged and Peri 2013). The transition of native workers to construction is also in line with the findings of negative effects on earnings in this sector (Table 2.14 in the Appendix). In fact, the workers who switched to construction possibly needed some initial training to work in that sector and thus were more likely to accept lower wages in exchange for training in the short-run.[51]

The discussion so far suggests that fewer immigrants entered the construction industry. This might have favoured the expansion of native employment in this sector. There are multiple reasons why this scenario is plausible. First, based on the short-term nature of the migration event, immigrants were probably more interested in finding short-term jobs. This can explain why they preferred hotels

[51]Other explanations may be consistent with negative effects that I find on earnings in construction. It might be for instance, that the increased supply of labor by immigrants lowered the bargaining power of natives and thus their wages. While the institutionalized nature of the Italian labor markets makes it difficult to believe that wages of tenured workers in construction were reduced, I can not rule out the possibility that this happened for those workers who switched to construction from other sectors. I do not find significant effects of migration on hours worked in construction (Table 2.17 in the online Appendix). I thus interpret the effects on earnings as a result of changes in hourly wages.

and restaurants to construction as the jobs offered in the former industry are more likely to be temporary (see "Share of short-term contracts" in Table 2.7). Second, construction is more institutionalized than other sectors, making it more difficult for illegal workers to be hired. Third, construction work is also more dangerous than work in any other industry and as a result it is much riskier for employers to hire illegals.[52] The smaller presentage of illegal workers in construction than in hotels and restaurants is consistent with this view (Table 2.7). Finally, working in construction might require more technical training and thus a good knowledge of Italian. This might favor native workers over immigrants.[53] A fact consistent with this line of argument is that the fraction of Italian workers who participate in training activities is similar in hotels, restaurants and in construction but the corresponding fraction of foreigners is much lower in construction than in hotels and restaurants (Table 2.7).[54] A similar type of reasoning can be applied to the comparison between wholesale trade and construction.

The positive effect of migration on employment in construction also suggests a possible link between migration and the local housing markets. Existing studies show that migration can cause the local demand for housing to go up (Saiz 2003; Saiz

[52]Based the latest available data, between 1998 and 2007 on average 6,767 workers per 100,000 employees in the construction industry were involved in work-related accidents. This compares to an average of 2,334 in retail and wholesale trade and 3,283 in hotels and restaurants (Source: *INAIL - Infortuni sul lavoro (ESAW) fino al 2007 - Tav. 15*).

[53]If we are willing to assume that jobs in the construction industry involve more interactive tasks (training in Italian can be seen as one of those), then these findings are generally consistent with those of Peri and Sparber 2009 in the United States and D'Amuri and Peri 2011 in Europe.

[54]This hold also when I group workers based on their educational attainments. However, given the descriptive nature of this analysis we can not rule out the possibility that the results are driven by differences in the composition of the Italian and foreigner population across sectors.

2007). A greater demand can result in higher rents and house prices. Higher prices might be expected to lead to a supply expansion either through the construction of new housing units or through the renovation of existing ones. If that is the case, employment in construction is expected to grow faster in regions experiencing larger migration flows. Due to the fact that most of the immigrants from the Arab Spring likely were refugees or asylum seekers who stayed in Italy for a short time period, the rise in the housing demand associated with the Arab Spring can be thought of as primarily induced by the publicly funded programs that provided housing to asylum seekers and refugees. In the years 2011 to 2013 in fact, the Italian government financed assistance programs for asylum seekers from North Africa for about 1 billion euros, on average 1,200 euros per beneficiary each month.[55] The primary aim of most of these programs was to provide new arrivals with a place to stay.

Along this lines, the available semiannual data show a positive association between migration flows, house prices and rents of inexpensive housing units in the most central zone of the regional capital cities (see Table 2.21 in the Appendix).[56] In contrast, the magnitude of the association between migration flows and prices of more expensive housing units is much closer to zero as we would expect given

[55]For more details see *Ordinanza del Presidente del Consiglio dei Ministri* n. 3924 February 18 2011, *Decreto-Legge*, n. 95 articolo 23 July 6 2012, "Fine emergenza Nord Africa, il 28 febbraio chiusi i centri. Il governo ha speso più di un miliardo" *Il Sole 24 Ore*, March 3, 2013.

[56]Appendix 2.10 describes the housing market data in detail. The IV regressions shown in Table 2.21 use the change in log prices and log rents between two consecutive semesters as the dependent variable in equation (2.4.6). I deal with the endogenous settlement of immigrants using the IV approach described in Section 2.4.2. Following Jud, Benjamin, and Sirmans 1996, I augment the set of controls ΔX_{rt} including lagged values of the regional employment rate and average income.

the type of housing experiencing increased demand from the new immigrants. Turning to the housing supply, while data are only available at annual frequency, I find a positive association between the annual number of building permits issued and migration flows to a region (Table 2.21). While limited by the quality and the frequency of the available data, this evidence is suggestive of a link between migration and the demand for construction labor.

I also find positive effect on employment in educational services resulting from the Arab Spring migration (Table 2.3). As in the case of the construction industry, a rise in the demand of educational services might be related to the wave of migration. In the first six month of 2011 in fact, 9000 legal Arab Spring immigrants younger than 15 entered the country. Many of the refugee camps that hosted the immigrants soon after their arrival, offered Italian classes. This might have increased the demand for teacher in the regions experiencing more migration.[57] The positive coefficient might also capture the growth of employment in cultural organizations. These are classified as part of the educational services industry and they were likely involved in assisting refugees and asylum seekers at the time of the uprisings.[58]

[57]See also "Emergenza migranti: le attivitá di accoglienza della Croce Rossa Italiana sul territorio nazionale" (Italian Red Cross website). Comparing the inflow of immigrants with the estimated employment effects from Table 2.4, I obtain a 1 to 13 ratio between natives employed and immigrants arriving. The ratio is obtained by dividing average regional inflow (i.e. 887) by the estimated effects on employment (i.e. 67). These figure are shown in Table 2.4.

[58]I do not exclude the possibility that other factors played a role in determining the positive effects on employment in educational services. In fact, I find the employment effect to be driven by female workers (Appendix Table 2.22). Based on this, one concurrent explanation might be that migration had positive effects on the labor supply of female workers, as Cortes and Tessada 2011 argue in the case of the United States. One other possibility is that female workers were induced to enter the educational services sector to compensate for the loss of income at the household

So far I have presented explanations for the positive effects on employment that move away from the more canonical hypothesis of complementarity in production. There are several reasons for this. First, I do not find evidence of the Arab Spring immigrants entering those sectors in which I estimate positive employment effects.[59] Second, I do not find significant effects of migration on earnings in educational services while I find significant and negative effects on earnings in construction. These results are difficult to reconcile with the standard complementarity arguments.[60]

Most of the sectors that I find to be affected by the Arab Spring migration produce nontraded goods or services. This suggests that the IV approach that I use does not capture the effects of the uprisings on trade flows between Italy and the Arab Spring countries. Mining can be seen as an exception to this. The uprisings in Libya did, in fact, significantly impact the supply of oil and, probably, the employment of Italians in mining. However, these effects appear to be very small and play only a minor role in my findings (Table 2.4). Finally, I do not find significant effects on employment in finance, insurance, real estate or manufacturing.

level deriving from the displacement of male workers in the other industries (Mincer 1960; Mincer 1962). In fact, I find that most of the displacement effects are concentrated in the male native population (Appendix Table 2.22). While those channels can explain an increase in the supply of labor by female workers, they can-not explain why there was an excess demand for labor in the educational services sector at the time of the uprisings.

[59] On average, I observe around 21,900 immigrants employed in construction in the pre-Arab Spring period and 21,700 in the post-Arab Spring period. Similarly in educational services, there were around 1,600 immigrants employed prior to the uprisings and 900 after.

[60] In the standard labor market model, if natives and immigrants are complements in production wages and employment of native workers are positively linked to the supply of labor from immigrants. The existing literature on Italy finds no evidence of negative effects and some evidence of positive effects on employment (Venturini and Villosio 2006; Giuntella 2012) and wages (Gavosto, Venturini, and Villosio 1999; Staffolani and Valentini 2010). Those are generally interpreted as evidence of complementary in production.

Since these industries were heavily hit by the European debt crisis, I also exclude the possibility that the instrument captures the effects of the financial crisis on employment.

2.6 Robustness checks

In this section I present the results of a set of robustness checks performed on equation (2.4.6). The same robustness analysis was performed on equation (2.4.5) and on the reduced form model. The results obtained from these other specifications are similar to those presented here, but they are available upon request. To control for region-specific effects that change over-time, I add to the empirical model year times region fixed effects and regional trends.[61] When equation (2.4.6) is estimated separately for each industry, the year-region fixed effects and the regional trends capture time varying factors specific to a given industry in each region. Controlling for these factors is particularly important in my setting, where the instrument is available only at the regional level rather than at the sector-region level. This creates concerns about the existence of persistent industry-specific factors that might have influenced the past settlement of immigrants. Table 2.8 shows that the baseline results are robust to these controls. This suggests that the instrument isolates the effects of migration from other region or sector specific confounding factors.

[61] I obtain similar estimates using non linear trends.

I then proceed to include quarter times year fixed effects to control for unobserved factors specific to a given quarter-year. I also control for the stock of immigrants in the previous period and I instrument for it using 2003 shares of immigrants from the Arab Spring countries. This is done to control for lagged effects of migration that might change the interpretation of the coefficients estimated in the baseline model. Finally, I control for the change in migration flows from countries not involved in the Arab Spring. The existence of contemporaneous migration flows to Italian regions, in fact, might bias the estimated effects of the Arab Spring migration.[62] The baseline findings are robust to these checks (Table 2.8).

Table 2.9 shows the results of a falsification test in which I restrict the sample used for the analysis to the pre Arab spring period (2009Q1-2010Q3). If the instrument only captures the flows of immigrants from the Arab Spring, I do not expect to find significant effects in the pre-Arab Spring period. The instrument remains relevant despite the lower number of observations (Panel A). Turning to the second stage regressions, I find no evidence of significant effects on total or industry specific employment (Panel B).

In the baseline specifications the standard errors are clustered at the regional level to account for serial correlation. The limited number of regions, however, can cause the estimated standard errors to be biased toward zero (Bertrand, Duflo,

[62] I use the interaction between 2003 shares of immigrants from other origin countries and the national change in legal immigrants from other origin countries as an instrument for the change in immigrants from other origin countries.

and Mullainathan 2004).[63] To check that the significance of the estimated effects does not entirely depend on a limited number of clusters, I use the wild bootstrap procedure to derive new p-values on the estimated coefficients (Cameron, Gelbach, and Miller 2008a; Davidson and MacKinnon 2010a). The results are reported in Table 2.10. As expected, the p-values obtained from the wild bootstrap are greater than those from the baseline specification. With the exception of the effects on employment in mining however, the coefficients that are significant in the baseline specification remain significant in the wild bootstrap.

Other studies in the literature use 1995 shares of resident permits as an instrument for the endogenous settlement of immigrants across regions (e.g. Barone and Mocetti 2011; Giuntella 2012; Bratti and Conti 2014). Table 2.24 in the Appendix reports the results obtained using those earlier shares. Panel A shows that the instrument obtained from the 1995 shares is weak. The estimated coefficients of the second stage regression are however, very similar to those estimated in the baseline model. Overall, I take this as evidence of the results being robust to the use of the 1995 shares in place of the 2003 shares. I thus prefer to use the 2003 shares in the baseline model because they have greater predictive power.

In deriving the baseline estimates, I do not distinguish between working age and nonworking-age immigrants. Old or very young immigrants, however, are arguably less likely to participate in the labor market. Table 2.25 in the Appendix shows the results obtained when using the flow of working-age immigrants as a

[63] In Italy there are 20 regions.

dependent variable in the first stage regressions. As expected, the greater magnitude of the effects on employment reflects the larger effects associated with smaller flows. The overall results however, are very similar to those obtained from the baseline model.

The existing literature provides evidence of negative effects of migration on labor market outcomes of older immigrants (e.g. Cortes 2008). To investigate whether the effects are driven by older immigrants, I redefine native workers as Italian born. The results obtained while using this alternative definition are reported in Table 2.26 in the Appendix. The effects are very similar to those of the baseline model. I thus infer that most of the effects that I find are on Italian-born workers.

Mobility can equalize labor market outcomes across regions (Borjas 2006). In Table 2.27 and 2.28 in the Appendix, I use the instrumental variable approach described in Section 2.4 to estimate the effects of the Arab Spring migration on the regional outflow and inflow of natives and of other immigrant groups.[64] I also estimate the effect of migration on inflow rates in those industries in which I find significant employment effects.[65] I fail to find significant effects of the Arab Spring migration on inflows or outflows of natives or different immigrant groups. Migrating from one region to the other might involve sunk costs, it is thus less likely to happen within a quarter. The absence of particular migration flows argues in favor of the

[64]The results are obtained using changes in inflows/outflows as dependent variables in (2.4.6). Other immigrants include all immigrants with the exception of those born in the Arab Spring countries or in any other African country.

[65]Due to a change of the industry classification used in the LFS across time, I can not derive regional outflow rates at the industry level that are consistent over time. For more details see Appendix 2.10.

effects discussed in Section 2.5 as capturing the more direct effects of migration. The derivation of quarterly outflow/inflow rates from LFS data however, requires assumptions on the timing of the migration from one region to the other (Appendix 2.10).

Finally, Table 2.29 in Appendix shows the results obtained while using the average population and the population in the first quarter of 2009 as the denominator in equation (2.4.6). The baseline results are robust to these changes.

2.7 Conclusions

The temporary, unanticipated and exogenous migration caused by the Arab Spring provides an ideal setting in which to estimate the short-term effects of migration. In the short-term, migration is found to have considerable and offsetting effects on employment of native workers across sectors. I estimate a quarterly displacement that is 2.2 to 2.6 times larger than the annual displacement estimated by other studies in the European literature. I also show that the employment effects disappear within one year in most of the sectors that were affected on a quarterly basis. This evidence in favor of substantial and short-lived effects suggests that a significant share of the effects of migration is concentrated in the very short run.

Focusing on the very short run allows us to uncover labor market mechanisms that would otherwise be neglected. I find, in fact, that migration has both positive and negative effects on employment of native workers. In particular, I find evidence of displacement effects in mining, hotels, restaurants and wholesale trade but positive effects on employment in construction and educational services. I present evidence that suggests that migration led to a short-term shift of displaced workers to construction.

Finally, I discuss the possibility that the positive employment effects in construction and educational services were driven by the increased demand for goods and services from this sectors from immigrants, rather than by complementarity between immigrants and native workers in production. The effects that immigrants as consumers exert on the demand for labor have so far received little attention in the literature and therefore warrant more rigorous analysis in the future.

2.8 Acknowledgments

Chapter 2 is currently being prepared for submission for publication of the material. Labanca, Claudio. "The Effects of a Temporary Migration Shock: Evidence from the Arab Spring Migration through Italy". The dissertation author was the principal researcher and author of this paper.

2.9 Figures and Tables

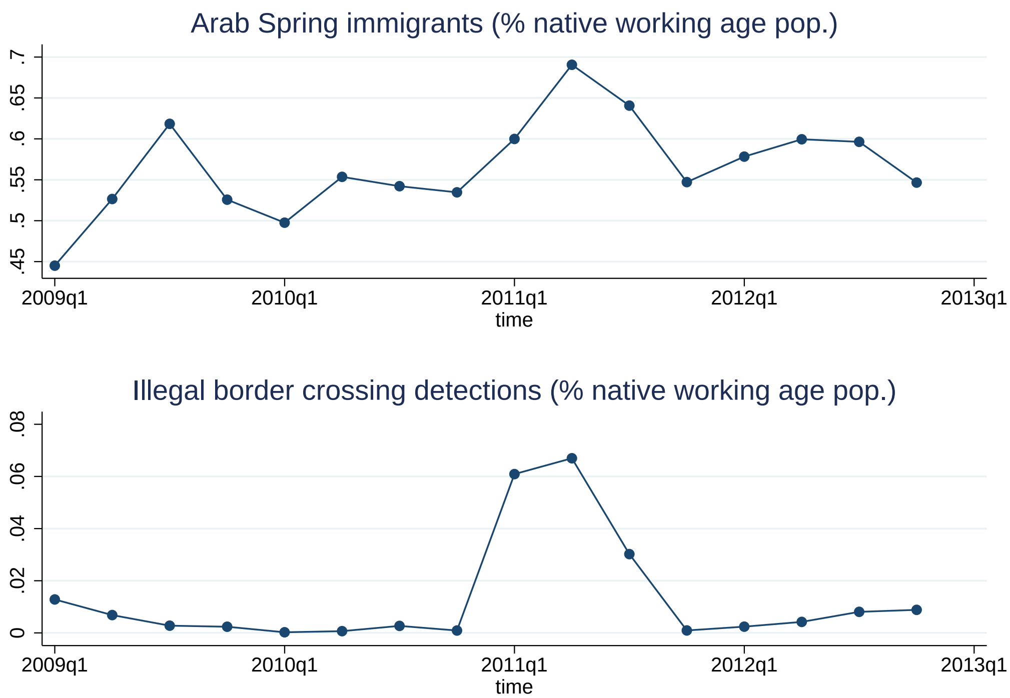


Figure 2.1: The Arab Spring Migration

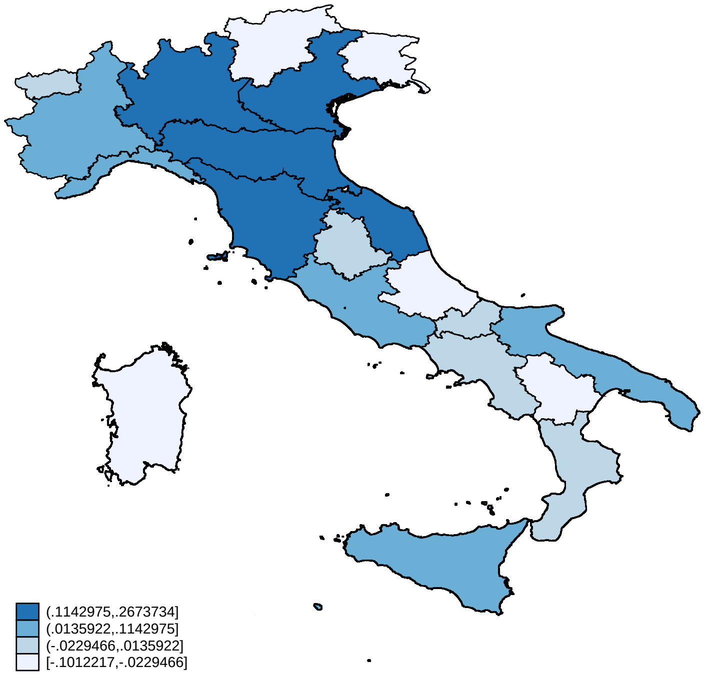


Figure 2.2: Average change in immigrants from the Arab Spring countries (% native working age population) - 2011Q1-2011Q2

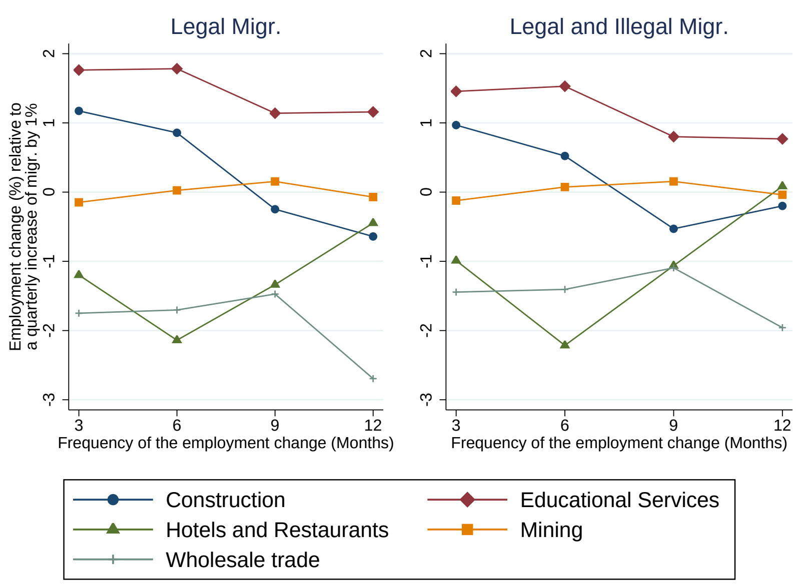


Figure 2.3: The dynamic effects of the of the Arab Spring migration

Table 2.1: Descriptive Statistics - Explanatory Variables

	Obs.	**Mean**	**Std. Dev.**	**Min**	**Max**
Change immig. from Arab Spring countries (% native pop.)	300	0.006	0.185	-0.761	0.722
Change immig. from Arab Spring countries Q410-Q211 (% native pop.)	60	0.050	0.187	-0.402	0.534
Change Arab Spring immigrants employed Q410-Q211 (%)	53	24.55051	83.15233	-100	400.296
2003 share of AS imm. (fraction of national AS imm.)	20	0.167	0.129	0.018	0.383
Illegal border crossing dectections (number of people)	16	4783	7635	87	24193
Illegal border crossing dectections (number of people) Q110-Q311	3	19029	7129.936	10894	24193
Males (% working age pop.)	320	45.694	1.768	42.934	49.842
Average Age	320	41.012	1.098	38.758	42.994
Elementary school or less (% working age pop.)	320	47.467	6.237	33.935	60.924
High school diploma (% working age pop.)	320	32.422	2.839	24.781	39.793
College degree or more (% working age pop.)	320	11.415	1.706	7.128	15.725
Tenured workers (% native workers)	320	64.900	3.733	53.883	71.762
Full-Time workers (% native workers)	320	85.599	1.870	78.631	91.426
White collars (% native workers)	320	43.063	4.534	30.897	53.412
Working age population	320	1977665	1603703	82801.4	6512507
Native working age population	320	1807053	1444980	75273	5775852
Total Population	320	3008085	2424482	125980.6	9967758

Sources: Italian Labor Force Survey Data - Istat, I.Stat - Istat, Frontex and FrontexWatch Malta
Regional observations are weighted by the corresponding population shares. AS stands for Arab Spring.
Elementary school is defined as primary (grade 1 to 5) and middle school (grade 6 to 8).
High school follows middle school. It can consist of 3 or 5 years of schooling depending on the field of study.
College or more is defined as any type of degree issued by a university, independently of its length.

Table 2.2: Immigrants versus Natives - Descriptive Statistics

	Immigrants from Arab Spirng Countries		**Natives**
	Pre-Arab Spring	**Post-Arab Spring**	
Demographics			
Males (%total)	61.52	62.66	48.59
Mean Age	45	44	43
Number of individuals (thousand)	192.86	215.50	60215.19
Observations (unweighted)	3416	3786	2424758
Educational attainments (%total)			
Elementary school or less	52.29	56.84	54.99
High School	33.67	33.00	33.95
College or more	14.04	10.16	11.06
Observations (unweighted)	3209	3563	2113357
Country of Origin (% total)			
Egypt	40.37	38.80	
Tunisia	40.76	44.07	
Lybia	18.38	16.92	
Yemen	0.48	0.21	
Observations (unweighted)	3416	3786	
Distribution of workers across sectors (% total)			
Agriculture, Forestry and Fishing	6.51	6.85	3.66
Mining	0.05	0.13	0.16
Construction	25.59	22.03	7.43
Manufacturing	17.36	15.67	18.75
Transportation, Communications, Electric, Gas and Sanitary Services	7.47	5.43	8.86
Wholesale Trade	2.38	2.77	4.29
Retail Trade	5.43	7.43	8.88
Finance, Insurance and Real Estate	2.20	1.19	3.75
Hotels and Restaurants	12.72	17.57	4.93
Public Administration	2.06	1.98	6.76
Educational Services	1.88	0.92	7.29
Other Services	16.34	18.03	25.25
Observations (unweighted)	1386	1548	822282

Source: Italian Labor Force Survey-Istat. Each observation is weighted by its relative population weight. "Number of individuals" refers to the average number per quarter. Elementary school is defined as primary (grade 1 to 5) and middle school (grade 6 to 8). High school follows middle school. It can consist of 3 or 5 years of schooling depending on the field of study. College or more is defined as any type of degree issued by a university, independently of its length. Natives are defined as Italian citizens. The pre-Arab Spring period consists of the years 2009 and 2010. The years 2011 and 2012 belong to the post-Arab Spring period.

Table 2.3: The effects of the Arab Spring migration on employment

Panel A: First stage regressions			
	Legal Migration change (%) (1)	**Legal and Illegal Migration change (%) (3)**	**Obs.**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.590***	1.926***	300
	(0.202)	(0.235)	
F-stat excl. instr.	62.00	67.08	
R-squared	0.136	0.165	

Panel B: Second stage regressions
Dep. Variable: Native employment change (fraction of native working age pop.)

	IV Legal Migr. (1)	**OLS Legal Migr. (2)**	**IV Legal and Illegal Migr. (3)**	**Reduced Form (4)**	**Obs.**
All sectors	-0.0034	0.0031	-0.0027	-0.0534*	300
	(0.0202)	(0.0030)	(0.0172)	(0.0262)	
Agriculture, Forestry and Fishing	0.0074	0.0016	0.0061	0.0118	300
	(0.0066)	(0.0013)	(0.0056)	(0.0105)	
Mining	-0.0015*	-0.0002	-0.0012*	-0.0023*	300
	(0.0007)	(0.0004)	(0.0006)	(0.0013)	
Construction	0.0117**	0.0009	0.0097**	0.0187**	300
	(0.0041)	(0.0025)	(0.0037)	(0.0065)	
Manufacturing	-0.0003	-0.0041	-0.0003	-0.0006	300
	(0.0113)	(0.0054)	(0.0098)	(0.0189)	
Transportation, Communications,	-0.0004	0.0011	-0.0003	-0.0007	300
Electric, Gas and Sanitary Services	(0.0099)	(0.0020)	(0.0085)	(0.0164)	
Wholesale Trade	-0.0174***	0.0020*	-0.0144***	-0.0278***	300
	(0.0043)	(0.0010)	(0.0038)	(0.0051)	
Retail Trade	-0.0008	-0.0023	-0.0004	-0.0008	300
	(0.0090)	(0.0023)	(0.0076)	(0.0146)	
Finance, Insurance and Real Estate	-0.0067	-0.0021**	-0.0055	-0.0106	300
	(0.0055)	(0.0009)	(0.0047)	(0.0084)	
Hotels and Restaurants	-0.0121***	0.0023	-0.0099***	-0.0190***	300
	(0.0034)	(0.0022)	(0.0029)	(0.0051)	
Public Administration	-0.0048	0.0026	-0.0039	-0.0076	300
	(0.0043)	(0.0017)	(0.0036)	(0.0068)	
Educational Services	0.0176***	0.0004	0.0146**	0.0280***	300
	(0.0059)	(0.0010)	(0.0052)	(0.0085)	
Other Services	0.0039	0.0008	0.0030	0.0058	300
	(0.0138)	(0.0034)	(0.0117)	(0.0221)	
Quarter and year fixed effects	YES	YES	YES	YES	
Region fixed effects	YES	YES	YES	YES	

The dependent variable in Panel A column (1) is $\Delta Legal^{AS}_{rt+1}$ from eq. (2.4.6)
The dependent variable in Panel A column (3) is $\Delta Legal^{AS}_{rt+1} + \Delta Illegal_{rt+1}$ from eq. (2.4.5).
Each entry in Panel B column (1) and (2) is the coefficient on the variable "legal migration flows" ($\Delta Legal^{AS}_{rt+1}$) in equation (2.4.6).
Each entry in Panel B column (3) is the coefficient on the variable "legal and illegal migration flows" ($\Delta Legal^{AS}_{rt+1} + \Delta Illegal_{rt+1}$) in equation (2.4.5).
Each entry in Panel B column (4) is the coefficient on the (instrumental) variable $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$ from the reduced form regression.
Each regression contains the following controls (relative to the native population): average age, the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high school and college graduates.
Standard errors in parentheses. Observations are weighted by quarter specific population shares.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.4: The employment effects of the Arab Spring migration: number of workers

Average inflow of legal immigrants Q42010-Q22011 (number of individuals)	887				
Average inflow of legal and illegal immigrants Q42010-Q22011 (number of individuals)	1413				
Average regional working age population Q42010-Q22011 (number of individuals)	1806878				
	Mining	**Construction**	**Wholesale Trade**	**Hotels and Restaurants**	**Educational Services**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Estimated change in native employment due to legal migration (number of workers)	-0.12*	47.53**	-41.43***	-29.97****	67.62***
	(0.056)	(17.96)	(10.84)	(8.887)	(23.39)
Estimated change in native employment due to legal and illegal migr. (number of workers)	-0.15*	62.46**	-54.46***	-39.39***	88.87**
	(0.076)	(23.58)	(14.35)	(11.73)	(31.52)
Etimated change in native employment from the reduced form model (number of workers)	-0.08*	32.44**	-28.28***	-20.46***	46.15***
	(0.042)	(11.38)	(5.155)	(5.513)	(14.04)
Average native employment 2009-Q32010 (number of individuals)	1548	80335	46969	49620	76044
Quarter, year and region fixed effects	YES	YES	YES	YES	YES

Standard errors in parentheses. The period Q42010-Q22011 is here considered as the peak of the Arab Spring.
The effects of legal migration are based on the coefficients from Table 2.3 Panel B column (1).
The effects of legal and illegal migration are based on the coefficients from Table 2.3 Panel B column (3).
The effects from the reduced form model are based on the coefficients from Table 2.3 Panel B column (4).
The estimated change in native employment is derived in two steps. First, by multiplying the coefficients from Table 2.3 column (1), (3) and (4) by, respectively, the average $\Delta Legal^{AS}_{rt+1}$, the average $\Delta Legal^{AS}_{rt+1} + \Delta Illegal_{rt+1}$ and the average $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$ at the peak of the Arab Spring (Q42011-Q22011).
The resulting number is then multiplied by the average number of workers employed in each industry prior to the uprisings (this reported in the Table). At the peak of the Arab Spring $\Delta Legal^{AS}_{rt+1}$ was on average around 0.05%, $\Delta Legal^{AS}_{rt+1} + \Delta Illegal_{rt+1}$ was about 0.08% and $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$ was about 0.02%.

Table 2.5: The dynamic effects of temporary migration

Dep Var: Native Employment change over 3,6,9 and 12 months (fraction of native work. age pop.)				
	3 months	**6 months**	**9 months**	**12 months**
Mining				
Quarterly change legal immig. (%)	-0.0015*	0.0003	0.0015	-0.0007
	(0.0007)	(0.0021)	(0.0021)	(0.0014)
Quarterly change legal and illegal	-0.0012*	0.0007	0.0015	-0.0004
immig. (%)	(0.0006)	(0.0018)	(0.0015)	(0.0011)
Construction				
Quarterly change legal immig. (%)	0.0117**	0.0086	-0.0025	-0.0064
	(0.0044)	(0.0061)	(0.0058)	(0.0076)
Quarterly change legal and illegal	0.0097**	0.0052	-0.0053	-0.0020
immig. (%)	(0.0037)	(0.0062)	(0.0050)	(0.0071)
Wholesale trade				
Quarterly change legal immig. (%)	-0.0175***	-0.0170***	-0.0147***	-0.0269***
	(0.0046)	(0.0051)	(0.0045)	(0.0092)
Quarterly change legal and illegal	-0.0144***	-0.0141***	-0.0109**	-0.0196*
immig. (%)	(0.0038)	(0.0042)	(0.0042)	(0.0103)
Hotels and Restaurants				
Quarterly change legal immig. (%)	-0.0120***	-0.0214**	-0.0134**	-0.0045
	(0.0036)	(0.0078)	(0.0053)	(0.0055)
Quarterly change legal and illegal	-0.0099***	-0.0221***	-0.0106	0.0009
immig. (%)	(0.0029)	(0.0045)	(0.0063)	(0.0056)
Educational Services				
Quarterly change legal immig. (%)	0.0176***	0.0178***	0.0114**	0.0116*
	(0.0061)	(0.0052)	(0.0047)	(0.0066)
Quarterly change legal and illegal	0.0146**	0.0153***	0.0080	0.0077
immig. (%)	(0.0052)	(0.0043)	(0.0063)	(0.0069)
Legal migration effect as a share of the quarterly effect (mean across sectors)	1.000	0.860	0.278	0.498
Legal and illegal migration effect as a share of the quarterly effect (mean across sectors)	1.000	0.842	0.116	0.498
F stat. excluded instrument legal immig.	62	59.69	12.41	9.93
F stat. excluded instrument legal and illegal immig.	67.08	74.52	17.88	14.18
Obs.	300	280	260	240

Each entry corresponding to the variable "Quarterly change legal immig." is the coefficients on the variable ($\Delta Legal^{AS}_{rt+1}$) in equation (2.4.6).
Each entry corresponding to the variable "Quarterly change legal and legal immig." is the coefficients on the variable ($\Delta Legal^{AS}_{rt+1} + \Delta Illegal^{AS}_{rt+1}$) from equation (2.4.5). The first stage regressions are in Table 2.15 and 2.16 in the Appendix.
Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates, region, year and quarter fixed effects. Observations are weighted by quarter specific population shares.
Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.6: Sectoral shift of employment

Panel A: First stage regression Change in Arab Spring immigrants (%native working age pop.)					
	(1)	**Obs.**	**(2)**		**Obs.**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.8533***	220	1.8690***		220
	(0.3866)		(0.4177)		
F-stat excl. instr.	22.98		20.02		
R-squared	0.178		0.178		

Panel B: Second stage regression Inflow into construction from Mining, Wholesale trade and Hotels and Restaurants (fract. native working age pop.)					
	(1)	**Obs.**	**(2)**		**Obs.**
$\Delta Legal^{AS}_{rt+1}$	0.0487*	220	0.0491*		220
	(0.0254)		(0.0273)		
Quarter and year fixed effects	YES		YES		
Region fixed effects	NO		YES		

Panel A and B: Each cell is a different regression. Each regression contains the following controls (native pop.): average age, the fraction of males, the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates. Observations are weighted by quarter specific population shares. Standard errors in parentheses.
The variable "Instrument" in Panel A indicates the product term described in expression (2.4.3).
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.7: Descriptive Statistics on Sectors

	Average earning pre-Arab Spring period Elementary School at most	**Illegal workers (% total) Av. 2005-2009**	**Training**		**Share of short term contracts (less than 6 months) (% total emp.)**
			Italians (% total Italians)	**Foreigners (% total foreigners)**	
Agriculture, Forestry and Fishing	992.04	23.55	1.54	0.79	11.68
Mining	1304.13	4.87	3.98	0.35	2.66
Construction	1181.15	10.63	2.21	0.97	2.24
Manufacturing	1162.57	3.94	3.10	1.15	2.21
Transportation, Communications, Electric, Gas and Sanitary Services	1303.04	28.26	4.85	2.18	2.34
Wholesale Trade	1140.03	7.25	3.79	2.60	1.56
Retail Trade	984.48	7.25	2.44	0.76	2.48
Finance, Insurance and Real Estate	1326.57	9.04	9.87	7.10	0.90
Hotels and Restaurants	886.90	31.26	2.17	1.86	6.85
Public Administration	1346.72		5.37	0.66	1.14
Educational Services	972.88	6.74	8.61	5.55	2.24
Other Services	938.37	12.59	7.25	2.09	2.29

Source: Data on illegal workers from *La misura dell'occupazione non regolare nelle stime di contabilitá nazionale* - Istat.
All the remaining statistics are from the Italian Labor Force survey data - Istat.
The training variable is constructed looking at the number of workers who have attended at least one job training activity in the 4 weeks prior to the interview.
Elementary school is defined as primary (grade 1 to 5) and middle school (grade 6 to 8).

Table 2.8: Employment effects - Robustness checks

Second stage regression **Employment change (fraction of native working age pop.)**					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
All sectors	-0.0042	-0.0049	-0.0114	-0.0039	-0.0087
	(0.0222)	(0.0230)	(0.0243)	(0.0186)	(0.0212)
Agriculture, Forestry and Fishing	0.0073	0.0070	0.0164	0.0073	0.0049
	(0.0074)	(0.0076)	(0.0099)	(0.0063)	(0.0067)
Mining	-0.0015*	-0.0015	-0.0024*	-0.0015**	-0.0015**
	(0.0008)	(0.0009)	(0.0013)	(0.0007)	(0.0007)
Construction	0.0118**	0.0120**	0.0143*	0.0118***	0.0090*
	(0.0046)	(0.0050)	(0.0079)	(0.0039)	(0.0054)
Manufacturing	-0.0007	-0.0008	-0.0096	-0.0006	0.0011
	(0.0128)	(0.0135)	(0.0146)	(0.0104)	(0.0079)
Transportation, Communications,	-0.0004	-0.0006	-0.0079	-0.0004	-0.0012
Electric, Gas and Sanitary Services	(0.0113)	(0.0117)	(0.0132)	(0.0094)	(0.0099)
Wholesale Trade	-0.0176***	-0.0177***	-0.0141**	-0.0172***	-0.0173***
	(0.0050)	(0.0053)	(0.0065)	(0.0040)	(0.0054)
Retail Trade	-0.0008	-0.0010	-0.0009	-0.0009	-0.0021
	(0.0100)	(0.0104)	(0.0110)	(0.0086)	(0.0071)
Finance, Insurance and Real Estate	-0.0063	-0.0065	-0.0028	-0.0066	-0.0074
	(0.0062)	(0.0065)	(0.0059)	(0.0052)	(0.0048)
Hotels and Restaurants	-0.0116***	-0.0115**	-0.0235**	-0.0118***	-0.0122***
	(0.0039)	(0.0043)	(0.0090)	(0.0031)	(0.0038)
Public Administration	-0.0052	-0.0053	0.0039	-0.0047	-0.0030
	(0.0051)	(0.0052)	(0.0075)	(0.0040)	(0.0047)
Educational Services	0.0173**	0.0174**	0.0223**	0.0173***	0.0160**
	(0.0064)	(0.0067)	(0.0102)	(0.0054)	(0.0076)
Other Services	0.0034	0.0036	-0.0071	0.0036	0.0050
	(0.0154)	(0.0162)	(0.0204)	(0.0130)	(0.0119)
Observations	300	300	300	300	300
Year f.e.	YES	YES	YES	YES	YES
Quarter f.e.	YES	YES	NO	YES	YES
Region times year f.e.	YES	YES	YES	NO	NO
Regional time trends	NO	YES	YES	NO	NO
Quarter times year f.e.	NO	NO	YES	NO	NO
Region f.e.	NO	NO	NO	NO	NO
Stock of AS immigrants at t-1	NO	NO	NO	YES	NO
Change Immig. from other countries	NO	NO	NO	NO	YES

Each entry in the table is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Change Immig. from other countries is instrumented using the interaction between 2003 regional shares
and the national quarterly change in non-As immigrants.
Stock of AS immigrants at t-1 is instrumented using the 2003 regional shares of immigrants from the
AS countries relative to the native population. Table 2.23 in the online Appendix shows the first stage regressions.
Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males,
the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high
school and college graduates. Observations are weighted by quarter specific population shares.
Standard errors in parentheses. Standard errors are clustered at the regional level.
*, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.9: Placebo Test - Pre-Arab Spring period only (09Q1-10Q3)

Panel A: First stage regressions		
Change in Arab Spring immigrants (% native working age pop.)		
		Observations
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	-19.279***	120
	(3.459)	
F-stat excl. instr.	31.07	
R-squared	0.285	
Panel B: Second stage regressions		
Native employment change (fract. native working age pop.)		
		Observations
All sectors	-0.0103	120
	(0.019)	
Agriculture, Forestry and Fishing	0.0020	120
	(0.005)	
Mining	-0.0004	120
	(0.001)	
Construction	0.0104	120
	(0.007)	
Manufacturing	-0.0045	120
	(0.010)	
Transportation, Communications,	-0.0022	120
Electric, Gas and Sanitary Services	(0.005)	
Wholesale Trade	0.0069	120
	(0.009)	
Retail Trade	-0.0092	120
	(0.008)	
Finance, Insurance and Real Estate	-0.0028	120
	(0.003)	
Hotels and Restaurants	0.0024	120
	(0.004)	
Public Administration	0.0073	120
	(0.005)	
Educational Services	-0.0089	120
	(0.007)	
Other Services	-0.0112	120
	(0.013)	
Quarter and year fixed effects	YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high school and college graduates. Observations are weighted by quarter specific population shares.
The variable "Instrument" in Panel A indicates the product term described in expression (2.4.3).
Each entry in Panel B is the coefficient on the explanatory variable of interest (migration flows) in equation (2.4.6).
Standard errors in parentheses.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.10: Wild bootstrap versus clustered standard errors

Second stage regressions Native employment change (share of native working age pop.)			
	(1) **Clustered s.e.**	**(2)** **Wild bootstrap**	**Observations**
Mining	-0.0015*	-0.0015	300
(p-value)	(0.0546)	(0.3442)	
Construction	0.0117**	0.0117**	300
(p-value)	(0.0107)	(0.0374)	
Wholesale Trade	-0.0174***	-0.0174**	300
(p-value)	(0.0007)	(0.0226)	
Hotels and Restaurants	-0.0121***	-0.0121***	300
(p-value)	(0.0019)	(0.0026)	
Educational Services	0.0176***	0.0176*	300
(p-value)	(0.0076)	(0.063)	
Quarter, year f.e.	YES	YES	

Each cell is a different regression. Each regression contains the following controls:
average age, the fraction of males, the regional population, the fraction of full-time workers,
the fraction of white collar and tenured workers, the fraction of high school and college graduates.
Observations are weighted by quarter specific population shares.
Each entry in Panel B is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Wild bootstrap performed as in Davidson and MacKinnon (2010). Results obtained from 999 repetitions.
Standard errors in parentheses. *, ** and *** are 10, 5 and 1 percent significance levels.

2.10 Additional information on the dataset

The industry classification- The classification of industry that we use follows the division classification of the US department of labor[66]. However, we consider Hotels and Restaurants and Educational Services separately from Services because the Arab Spring migration more strongly affected employment in those two industries. The two digit classification in the Italian Lfs doesn't allow us to distinguish between Wholesale and Retail trade of automotive dealer and mechanics. The distinction between retail and wholesale trade will turn out to be important for the analysis. Thus we exclude the Italian automotive dealer and mechanic sector from our analysis.

Illegal border crossing detections- The total number of illegal border crossing detections through the Central Mediterranean route was made available by Frontex upon request. To this we subtract the number illegal entries in Malta. Apart from Italy, Malta is the only other country on the Central Mediterranean route. Data on illegal entries in Malta can be found online from FrontexWatch Malta. We only consider the number of individuals alive upon arrival on Maltese lands.

Housing market data- In analyzing the effects of migration on the housing market, we use data on the number of residential property sales from *Agenzia delle Entrate- Osservatorio del mercato immobiliare*. The data are collected on a

[66] *www.osha.gov/pls/imis/*

quarterly basis and they cover all major Italian cities (*capoluoghi di provincia*) and surrounding counties (*province*). We use data on house prices and rents in regional capital cities from the database *Quotazioni Immobiliari*[67]. These are available on a semiannual basis. We derive data on house prices and rents in 4 zones in each regional capital city[68]. We differentiate between prices and rents of more expensive versus less expensive housing units in the most central zone (zone A)[69].

The database *Quotazioni Immobiliari* provided by *Agenzia delle Entrate-Osservatorio del mercato immobiliare* provides 95% confidence intervals on prices and rents of a given type of building (called *tipologia*) in a given micro-area (called *zona OMI*) for each town in Italy. We restrict our sample to the set of regional capital cities. The 95% confidence interval is based on the mean and the standard deviation that result from a representative sample[70] of housing units and it is defined using a t-student distribution. The type of building is defined by looking at the reported characteristics and the scope of each given building. We only focus on residential units. Conditional on being assigned to a given typology, the location of the unit sampled determines the micro-area. Micro-areas can be aggregated into larger areas. Those are referred as *fasce* in the original database and here labeled as Zones A to D. For each micro-area and typology of building we derive the average price defined as the center of the 95% interval described above. We

[67]The data are made available by *Agenzia delle Entrate- Osservatorio del mercato immobiliare.*

[68]Figure 2.4 in the online Appendix provides a graphical example of the 4 zones.

[69]The Appendix Table 2.13 shows descriptive statistics on house sales, house prices and rents. The online Appendix 2.10 describes the construction of house prices and rents into more details.

[70]This sample must contain a minimum of 5 housing units.

then take the average of those prices across micro-areas and type of building within the each zone (A to D). From this we obtain a price and a rental value for each zone, in each semester and in each regional capital city. In in Panel C and D of Table 2.21, we differentiate between more and less expensive types of buildings bases on the reported typology (*tipologia*). In particular we define as less expensive the residential units classified as *Abitazioni di tipo economico* and *Abitazioni tipiche dei luoghi*. We classified as more expensive *Ville e Villini, abitazioni civili and abitazioni signorili*. This classification is based on the description of each typology that can be found on the users guide (i.e. *Manuale della Banca dati dell'osservatorio del marcato immobiliare*) available on line from *Agenzia delle Entrate*.

Throughout the housing market analysis, we do not consider data from *Abruzzo* because this region was heavily hit by an earthquake in 2010. This caused huge damage to properties. We also exclude *Trentino Alto Adige* from our analysis because data on house sales are not available. Finally, data on cheaper housing units in Rome are not available[71].

Inflows into construction from mining, wholesale trade and hotels and restaurants- For each region and in each quarter we count the weighted number of workers who were employed in mining, hotels and restaurants or wholesale trade the year prior to the survey and that are employed in construction at the time of the survey. We thus derive the change in the number of native workers

[71] This explains why the analysis on cheap housing units is based on fewer observations (Table 2.21 Panel C and D).

who moved to construction from wave $t-1$ to wave t of the data. We restrict the possible timing of migration into construction to be either in between t and $t-1$ or $t-4$ and $t-5$. We assume that workers move into construction in between $t-4$ and $t-5$. Accordingly we make such a change relative to the population at $t-5$.

Inflows of natives/immigrants - For each region we count the weighted number of workers who lived in a different region one year prior to the interview. In each quarter,we then take the first difference in inflows to each region between consecutive waves of data. As for the *Inflows into construction from mining, wholesale trade and hotels and restaurants*, we assume that the workers who moved into a given region did so between $t-4$ and $t-5$. The change is reported relative to the population at $t-5$.

Outflows of natives/immigrants - For each region we count the weighted number of workers who lived in that region one year prior to the interview and who lived in a different region at the time of the interview. In each quarter, we then take the first difference in outflows to each region between consecutive waves of data. We then assume that the workers who moved out of the region did so in between $t-4$ and $t-5$. The change is reported relative to the population at $t-5$. The change in the industry classification that occurred in 2011, makes it impossible to construct series of outflow rates for each industry that are consistent over time. This is because there is no available bridge between old and and new industry classification on past employment[72]. For this reason, we only report total

[72]The industry classification changed in 2011. A bridge between the two classification was

outflows.

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made available by Istat for the current industry of employment. Such a bridge does not exist for past (or lagged) industry of employment (i.e. The variable ”ate2de” in the Italian Lfs data).

2.11 Supplmentary Tables and Figures

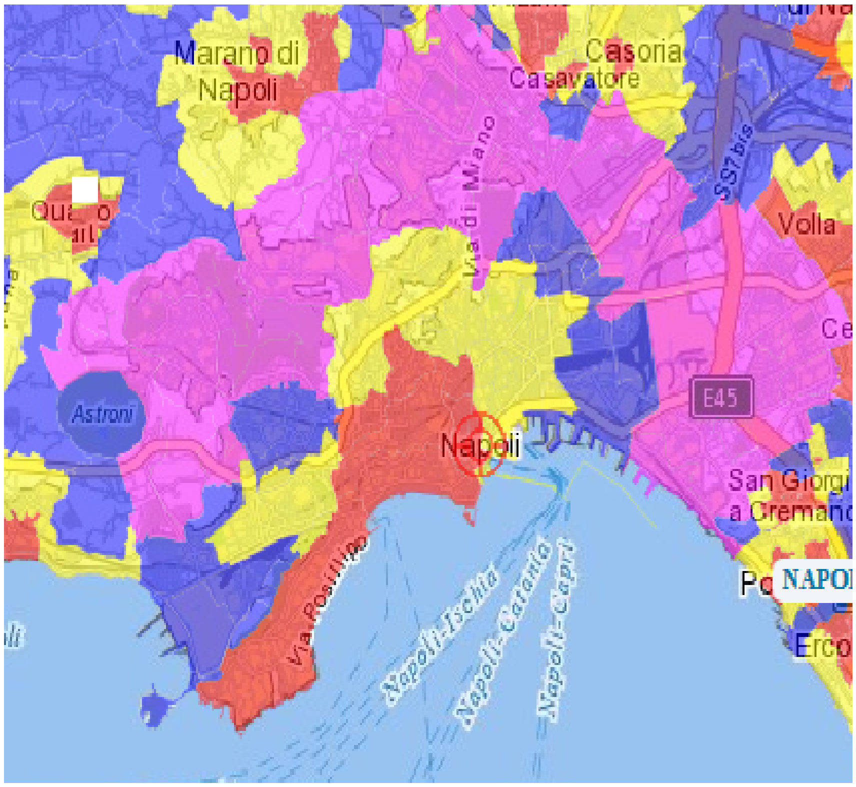


Figure 2.4: Example of zones within a city - Naples

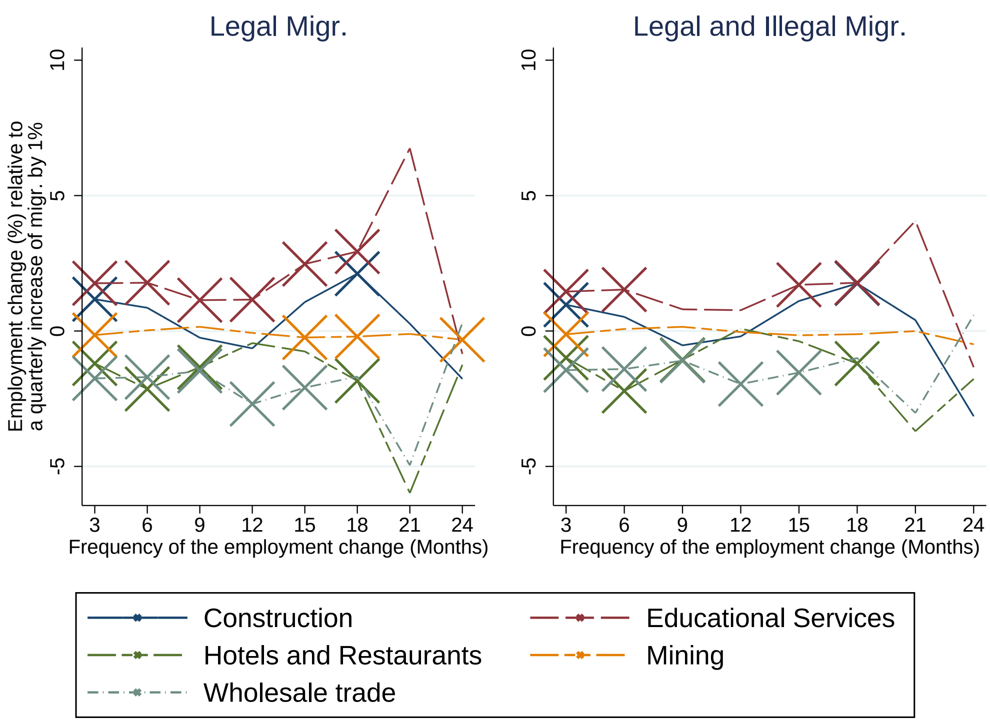


Figure 2.5: The evolution of the short term effects of migration: 3 to 24 months

Table 2.11: Descriptive Statistics: Employment of natives by sector

Native Employment (% native pop.)					
	Obs.	**Mean**	**Std. Dev.**	**Min**	**Max**
All sectors	320	57.07	10.47	38.39	70.29
Agriculture, Forestry and Fishing	320	1.93	1.01	0.59	5.94
Mining	320	0.09	0.06	0.00	0.33
Construction	320	4.21	0.65	2.62	8.42
Manufacturing	320	10.92	5.81	2.56	20.91
Transportation, Communications, Electric, Gas and Sanitary Services	320	5.11	1.49	2.61	8.80
Wholesale Trade	320	2.45	0.88	0.58	4.47
Retail Trade	320	5.01	0.67	3.44	7.88
Finance, Insurance and Real Estate	320	2.16	0.96	0.41	3.96
Hotels and Restaurants	320	2.81	0.72	1.15	7.18
Public Administration	320	3.85	1.28	1.98	8.69
Educational Services	320	4.15	0.51	2.67	7.24
Other Services	320	14.37	2.99	8.52	19.82

Sources: Italian Labor Force Survey Data

Regional observations are weighted by the corresponding population shares

Table 2.12: Descriptive Statistics: Average Monthly Earnings

Average monthly earnings (euros)					
	Obs.	**Mean**	**Std. Dev.**	**Min**	**Max**
All sectors	320	1256.16	71.21	1103.06	1387.96
Agriculture, Forestry and Fishing	319	1014.43	183.71	576.40	2000.00
Mining	286	1424.43	356.14	541.44	3000.00
Construction	320	1219.41	101.00	968.35	1485.97
Manufacturing	320	1262.90	100.47	966.23	1489.86
Transportation, Communications, Electric, Gas and Sanitary Services	320	1382.85	91.68	1168.33	1614.83
Wholesale Trade	320	1224.46	133.58	893.50	1876.35
Retail Trade	320	1009.14	76.73	810.21	1197.85
Finance, Insurance and Real Estate	320	1639.39	127.46	1245.44	1970.78
Hotels and Restaurants	320	918.21	84.87	662.16	1183.16
Public Administration	320	1474.24	75.89	1245.16	1669.46
Educational Services	320	1358.40	59.02	1218.90	1603.06
Other Services	320	1179.98	75.18	964.08	1361.75

Sources: Italian Labor Force Survey Data - Istat

Regional observations are weighted by the corresponding population shares

Table 2.13: Descriptive Statistics - Housing market variables

	Obs.	Mean	Std. Dev.	Min	Max
			House sales		
Entire Region	288	7678.36	6965.20	335	35869
Provinces	288	1036.37	975.48	0	4311
Capital City County	288	3226.52	3571.68	335	15243
Capital City	288	1356.37	1900.08	60	9636
			House Prices (euros/square meter)		
Zone A	152	3116.51	1493.69	1293.75	7024.24
Zone B	136	2499.13	858.55	1183.75	4219.92
Zone C	144	2084.24	735.03	969.17	3813.80
Zone D	128	1834.81	714.18	658.75	3143.75
Less expensive housing units	138	2359.42	1082.75	1204.64	4917.50
More expensive housing units	152	3349.04	1541.96	1362.50	7024.24
			Rents (euros/square meter)		
Zone A	152	10.30	5.16	4.36	27.29
Zone B	136	8.16	2.88	4.40	16.69
Zone C	144	6.61	2.75	0.00	13.82
Zone D	128	6.00	3.10	0.00	11.70

We label the most central zone "zone A" (the red zone in Figure 2.4). Moving away from the city center the remaining 3 zones (the yellow, blue and violet zone in Figure 2.4) are labeled respectively as zone B, C and D. Provinces refers to the main cities (*Province*) of each Italian region.

Table 2.14: The effects of the Arab Spring migration on average earnings

	Second stage regressions (Log) Average monthly earnings change				
	(1) IV	**(1) OLS**	**(2) IV**	**(2) OLS**	**Number of Observations**
All sectors	-0.0041	-0.0053	-0.0037	-0.0053	300
	(0.0156)	(0.0037)	(0.0162)	(0.0039)	
Agriculture, Forestry and Fishing	-0.0281	-0.0061	-0.0293	-0.0059	298
	(0.1041)	(0.0304)	(0.1061)	(0.0313)	
Mining	0.2380	0.1636	0.2099	0.1695	247
	(0.4400)	(0.1086)	(0.4589)	(0.1150)	
Construction	-0.1491***	-0.0365**	-0.1479***	-0.0370**	300
	(0.0413)	(0.0136)	(0.0422)	(0.0142)	
Manufacturing	0.0675	0.0052	0.0676	0.0051	300
	(0.0459)	(0.0117)	(0.0476)	(0.0121)	
Transportation, Communications,	0.0547	-0.0151*	0.0548	-0.0152*	300
Electric, Gas and Sanitary Services	(0.0559)	(0.0085)	(0.0582)	(0.0087)	
Wholesale Trade	0.0058	-0.0210	0.0040	-0.0210	300
	(0.0507)	(0.0170)	(0.0523)	(0.0178)	
Retail Trade	0.0075	0.0124	0.0084	0.0121	300
	(0.0503)	(0.0146)	(0.0518)	(0.0151)	
Finance, Insurance and Real Estate	0.0227	0.0183	0.0231	0.0184	300
	(0.0794)	(0.0243)	(0.0823)	(0.0252)	
Hotels and Restaurants	-0.0086	-0.0504**	-0.0119	-0.0506**	300
	(0.0831)	(0.0222)	(0.0868)	(0.0233)	
Public Administration	0.0146	-0.0097	0.0147	-0.0095	300
	(0.0774)	(0.0136)	(0.0802)	(0.0142)	
Educational Services	-0.0416	-0.0063	-0.0412	-0.0064	300
	(0.0283)	(0.0127)	(0.0295)	(0.0130)	
Other Services	0.0066	0.0109	0.0073	0.0111	300
	(0.0394)	(0.0155)	(0.0411)	(0.0161)	
Quarter and year fixed effects	YES	YES	YES	YES	
Region fixed effects	NO	NO	YES	YES	
Estimated change	-9.046				
in monthly earnings (euros)	(2.504)				

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males,the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates.
The Table only shows the coefficients relative to migration flows. Observations are weighted by quarter specific population shares.
Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.
The estimated change in monthly earnings in construction is obtained as the product of the estimated coefficient (-0.1491), the average percentage change in immigrants from the Arab Spring countries in the fist six months of 2011 (0.05%) and the average Pre-Arab Spring earnings in construction (1202.832 euros)

Table 2.15: The dynamic effects of Legal migration: First Stage Regressions

	3 months	6 months		
	Quarterly change Imm. from AS countries (%)	**Quarterly change Imm. from AS countries (%)**	**Contemporaneous change Imm. from AS countries (%)**	
Quarterly instrument	1.590***	1.413***	-0.498	
	(0.202)	(0.251)	(0.793)	
Contemporaneous instrument		1.227	2.144***	
		(1.242)	(0.345)	
F (exclud. inst.)	62.00	59.69	28.33	
Obs.	300	280	280	
R-squared	0.136	0.159	0.146	
Region, year and quarter f.e.	YES	YES	YES	

	9 months		12 Months	
	Quarterly change Imm. from AS countries (%)	**Contemporaneous change Imm. from AS countries (%)**	**Quarterly change Imm. from AS countries (%)**	**Contemporaneous change Imm. from AS countries (%)**
Quarterly instrument	1.785***	-0.487	1.610***	0.017
	(0.473)	(0.902)	(0.440)	(1.277)
Contemporaneous instrument	0.145	2.096***	-0.051	1.702***
	(0.689)	(0.479)	(0.397)	(0.417)
F (exclud. inst.)	12.41	9.83	9.935	12.88
Obs.	260	260	240	240
R-squared	0.137	0.198	0.139	0.242
Region, year and quarter f.e.	YES	YES	YES	YES

	18 Months		24 Months	
	Quarterly change Imm. from AS countries (%)	**Contemporaneous change Imm. from AS countries (%)**	**Quarterly change Imm. from AS countries (%)**	**Contemporaneous change Imm. from AS countries (%)**
Quarterly instrument	1.255*	1.287	5.229**	-5.194
	(0.707)	(1.211)	(2.286)	(3.765)
Contemporaneous instrument	-0.749	2.435***	2.539**	-1.445
	(0.737)	(0.439)	(1.304)	(1.790)
F (exclud. inst.)	10.80	27.71	2.95	3.35
Obs.	200	200	160	160
R-squared	0.250	0.443	0.220	0.370
Region, year and quarter f.e.	YES	YES	YES	YES

Quarterly instrument here refers to $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{Illegal_{t+1} - Illegal_{t}}{pop_{rt}} \times 100\right)$

Contemporaneous instrument here refers to $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{Illegal_{t+i} - Illegal_{t+1}}{pop_{rt+1}} \times 100\right)$

The dep. variable "Quarterly change Imm. from AS countries (%)" indicates $\frac{(Im^{AS}_{rt+1} - Im^{AS}_{rt})}{pop_{rt}} \times 100$ in eq. (2.4.2)

The dep. variable "Contemporaneous change Imm. from AS countries (%)" indicates $\frac{(Im^{AS}_{rt+i} - Im^{AS}_{rt+1})}{pop_{rt+1}} \times 100$ in eq. (2.4.2)

Migration flows account for legal migration only. Observations are weighted by quarter specific population shares.

Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.16: The dynamic effects of Legal and Illegal migration: First Stage Regressions

	3 months	6 months		
	Quarterly change Imm. from AS countries (%)	**Quarterly change Imm. from AS countries (%)**	**Contemporaneous change Imm. from AS countries (%)**	
Quarterly instrument	1.926***	1.801***	-0.123	
	(0.235)	(0.235)	(0.816)	
Contemporaneous instrument		0.939	2.513***	
		(1.247)	(0.381)	
F (exclud. inst.)	67.08	74.52	39.74	
Obs.	300	280	280	
R-squared	0.165	0.181	0.178	
Region, year and quarter f.e.	YES	YES	YES	
	9 months		**12 Months**	
	Quarterly change Imm. from AS countries (%)	**Contemporaneous change Imm. from AS countries (%)**	**Quarterly change Imm. from AS countries (%)**	**Contemporaneous change Imm. from AS countries (%)**
Quarterly instrument	2.092***	0.285	1.859***	1.124
	(0.498)	(0.901)	(0.463)	(1.224)
Contemporaneous instrument	-0.131	2.530***	-0.345	2.038***
	(0.687)	(0.501)	(0.404)	(0.473)
F (exclud. inst.)	17.88	14.37	14.18	19.62
Obs.	260	260	240	240
R-squared	0.168	0.242	0.176	0.311
Region, year and quarter f.e.	YES	YES	YES	YES
	18 Months		**2 years**	
	Quarterly change Imm. from AS countries (%)	**Contemporaneous change Imm. from AS countries (%)**	**Quarterly change Imm. from AS countries (%)**	**Contemporaneous change Imm. from AS countries (%)**
Quarterly instrument	1.367*	2.319*	5.988**	-6.411
	(0.687)	(1.250)	(2.384)	(3.880)
Contemporaneous instrument	-0.957	2.890***	2.638**	-2.127
	(0.740)	(0.464)	(1.319)	(1.819)
F (exclud. inst.)	13.48	32.17	4.143	3.097
Obs.	200	200	160	160
R-squared	0.297	0.517	0.253	0.490
Region, year and quarter f.e.	YES	YES	YES	YES

Quarterly instrument here refers to $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{Illegal_{t+1} - Illegal_t}{pop_{rt}} \times 100\right)$

Contemporaneous instrument here refers to $\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{Illegal_{t+i} - Illegal_{t+1}}{pop_{rt+1}} \times 100\right)$

The dep. variable "Quarterly change Imm. from AS countries (%)" indicates $\frac{(Im^{AS}_{rt+1} - Im^{AS}_{rt})}{pop_{rt}} \times 100$ in eq. (2.4.2)

The dep. variable "Contemporaneous change Imm. from AS countries (%)" indicates $\frac{(Im^{AS}_{rt+i} - Im^{AS}_{rt+1})}{pop_{rt+1}} \times 100$ in eq. (2.4.2)

Migration flows (Im^{AS}_{rt}) account for legal and illegal migration. Observations are weighted by quarter specific population shares.

Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.17: The effects of the Arab Spring migration on hours worked

	Second stage regressions (Log) Average hours worked change		
	(1) IV	**(2) IV**	**Obs.**
All sectors	0.0049	0.0050	300
	(0.0093)	(0.0096)	
Agriculture, Forestry and Fishing	0.2417**	0.2403**	300
	(0.1018)	(0.1043)	
Mining	-0.0202	-0.0132	262
	(0.1229)	(0.1237)	
Construction	-0.0515	-0.0516	300
	(0.0325)	(0.0331)	
Manufacturing	0.0340	0.0346	300
	(0.0228)	(0.0240)	
Transportation, Communications,	0.0281	0.0278	300
Electric, Gas and Sanitary Services	(0.0276)	(0.0282)	
Wholesale Trade	0.0039	0.0036	300
	(0.0480)	(0.0495)	
Retail Trade	-0.0331	-0.0321	300
	(0.0210)	(0.0215)	
Finance, Insurance and Real Estate	0.0202	0.0205	300
	(0.0438)	(0.0456)	
Hotels and Restaurants	-0.0294	-0.0298	300
	(0.1386)	(0.1436)	
Public Administration	-0.0197	-0.0198	300
	(0.0331)	(0.0344)	
Educational Services	0.0714	0.0716	300
	(0.0462)	(0.0474)	
Other Services	0.0478*	0.0477*	300
	(0.0231)	(0.0241)	
Quarter and year fixed effects	YES	YES	
Region fixed effects	NO	YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high school and college graduates. The table only shows the coefficients relative to migration flows.
Standard errors in parentheses. Observations are weighted by quarter specific population shares.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.18: Classification of Occupations in types of occupations

Occupation Type	Occupation
Elementary occupations	Elementary occupations
Clerical and Craft occup.	Clerks Service and market sales workers Skilled agricultural and fishery workers Craft and related trades workers Plant and machine operators and assemblers
Technical and Associate professionals	Technicians and associate professionals
Professional and managers	Legislators, senior officials and managers Professionals

Table 2.19: Employment effects by occupation and education

Panel A: Second stage Regressions Employment change by type of occupation				
	Mining	**Construction**	**Wholesale trade**	**Hotels and Restaurants**
Elementary occup.	-0.0004	0.0011	0.0010	-0.0027***
	(0.0003)	(0.0016)	(0.0010)	(0.0008)
Clerical and Craft occup.	-0.0016**	0.0072**	-0.0103***	-0.0088***
	(0.0006)	(0.0032)	(0.0019)	(0.0022)
Technical and Associate professionals	-0.0002	0.0030	-0.0053*	0.0012
	(0.0002)	(0.0018)	(0.0026)	(0.0011)
Professional and managers	0.0007***	0.0005	-0.0028***	-0.0017
	(0.0002)	(0.0015)	(0.0009)	(0.0012)
Obs.	300	300	300	300
Quarter, year and region fixed effects	YES	YES	YES	YES
Panel B: Second stage Regressions Employment change by education				
	Mining	**Construction**	**Wholesale trade**	**Hotels and Restaurants**
Elementary school	-0.0010	0.0042	-0.0076**	-0.0094***
	(0.0006)	(0.0041)	(0.0028)	(0.0019)
High school	-0.0010**	0.0072	-0.0080***	-0.0005
	(0.0003)	(0.0046)	(0.0021)	(0.0025)
Colege or more	0.0005*	0.0003	-0.0019	-0.0020**
	(0.0002)	(0.0007)	(0.0025)	(0.0008)
Obs.	300	300	300	300
Quarter, year and region fixed effects	YES	YES	YES	YES

Each cell is a different regression. Each regression contains the following controls: average age,
the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers,
the fraction of high school and college graduates. Observations are weighted by quarter specific population shares.
Each entry in the table is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Elementary school is defined as primary (grade 1 to 5) and middle school (grade 6 to 8). High school follows middle school.
It can consist of 3 or 5 years of schooling depending on the field of study.
Standard errors in parentheses are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.20: Employment effects by occupation and education - Number of workers

Panel A: Second stage Regressions Employment change by occupation type (number of workers)				
	Mining	**Construction**	**Wholesale trade**	**Hotels and Restaurants**
Elementary occup.	-0.00157	0.267	0.110	-0.397***
	(0.00128)	(0.394)	(0.117)	(0.122)
Clerical and Craft occup.	-0.0815**	22.48**	-12.68***	-18.41***
	(0.0304)	(9.855)	(2.283)	(4.579)
Technical and Associate professionals	-0.00197	1.071	-4.535*	0.0641
	(0.00251)	(0.631)	(2.214)	(0.0606)
Professional and managers	0.00865***	0.151	-0.600***	-0.371
	(0.00220)	(0.503)	(0.186)	(0.252)
Obs.	300	300	300	300
Quarter, year and region fixed effects	YES	YES	YES	YES
Panel B: Second stage Regressions Employment change by education (number of workers)				
	Mining	**Construction**	**Wholesale trade**	**Hotels and Restaurants**
Elementary school	-0.0392	12.03	-7.515**	-14.48***
	(0.0238)	(11.51)	(2.796)	(2.986)
High school	-0.0252**	7.784	-9.661***	-0.481
	(0.00889)	(4.995)	(2.525)	(2.278)
Colege or more	0.00620*	0.0410	-0.392	-0.176**
	(0.00308)	(0.0922)	(0.523)	(0.0701)
Obs.	300	300	300	300
Quarter, year and region fixed effects	YES	YES	YES	YES

Standard errors in parentheses. The estimated change in native employment is derived by multiplying the coefficients of Table 2.20 by the average change in migration flows (0.05%) and the average pre Arab Spring number of workers employed in . each industry-education or industry-occupation cell. Elementary school is defined as primary (grade 1 to 5) and middle school (grade 6 to 8). High school follows middle school. It can consist of 3 or 5 years of schooling depending on the field of study.

Table 2.21: Migration and the housing market

	Panel A: Correlation Change in the number of building permits (%)	
Predicted change Arab Spring immigrants (%)	0.126	
	Panel B: Second stage regressions Change in the number of building permits (%)	
Arab Spring immigrants change (%)	2.6411	0.9833
	(13.0318)	(17.5134)
Year fixed effects	YES	YES
Region fixed effects	NO	YES
Regional Controls	YES	YES
Obs.	60	60
	Panel C: Second stage regressions Regional capital city only	
	Log Price change inexpensive hous. units Zone A	**Log Price change expensive hous. units Zone A**
Arab Spring immigrants change (%)	0.0482* (0.0253)	0.0133 (0.0145)
Obs.	120	133
Year and Semester f.e.	YES	YES
	Panel D: Second stage regressions Regional capital city only	
Arab Spring immigrants	**Log Rent change**	**Log Rent change**
change (%)	**inexpensive hous. units**	**expensive hous. units**
	Zone A	**Zone A**
Obs.		
Year and Semester f.e.	0.0896	-0.0266
	(0.0535)	(0.0182)
		133
	120	
	YES	YES

Each cell is a different regression. Regression in Panel B contain the same controls as Panel B in Table 2.3. Regressions in Panel C contain the following controls: average age, one period lagged employment, one period lagged (log)average earnings, the regional population, the fraction of white collars and tenured workers,the fraction of college graduates. Observations are weighted by quarter specific population shares. Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.22: Breakdown of the employment effects by gender

Native employment change (fract. native working age pop.)			
	Baseline	**Males**	**Females**
Mining	-0.0015*	-0.0016*	
	(0.0007)	(0.0009)	
Construction	0.0117**	0.0046	0.0071*
	(0.0041)	(0.0037)	(0.0034)
Wholesale trade	-0.0174***	-0.0095***	-0.0080**
	(0.0043)	(0.0030)	(0.0033)
Hotels and Restaurants	-0.0121***	-0.0064	-0.0056*
	(0.0034)	(0.0048)	(0.0030)
Educational Services	0.0176***	0.0045*	0.0132**
	(0.0059)	(0.0024)	(0.0050)
Quarter, year and region fixed effects	YES	YES	YES

Each cell is a different regression. Each regression contains the following controls:
average age, the fraction of males, the regional population,fraction of full-time workers,
the white collar and tenured workers, the fraction of high school and college graduates.
Observations are weighted by quarter specific population shares.
Each entry in the table is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Standard errors in parentheses
are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.23: Robustness checks - First stage regressions

	Immigrants from AS countries change (%)	**Immigrants from AS countries change (%)**	**Immigrants from AS countries change (%)**	**Immigrants from AS countries change (%)**	**Stock of immigr. from AS countries at t-1**
	(1)	**(2)**	**(3)**	**(4)**	**(4)**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.588***	1.590***	1.961***	1.5866***	-0.0075*
	(0.228)	(0.239)	(0.402)	(0.1954)	(0.0038)
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right)$				0.0468*	0.0269***
				(0.0232)	(0.0039)
F (exclud. inst.)	48.63	44.45	23.80	39.34	38.74
Obs.	300	300	300	280	280
R-squared	0.173	0.182	0.216	0.136	0.741
Year f.e.	YES	YES	YES	YES	YES
Quarter f.e.	YES	YES	NO	YES	YES
Region times year f.e.	YES	YES	YES	NO	NO
Regional time trends	NO	YES	YES	NO	NO
Quarter times year f.e.	NO	NO	YES	NO	NO
Region f.e.	NO	NO	NO	NO	NO
Stock of immigrants at t-1	NO	NO	NO	YES	YES
Change Immig. from other countries	NO	NO	NO	NO	NO

	Immigrants from AS countries change (%)	**Immigrants from other countries change (%)**
	(5)	**(5)**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.6186***	-0.8598
	(0.2188)	(0.8519)
$\left(\frac{Im^{OC}_{r2003}}{Im^{OC}_{2003}}\right) \times \left(\frac{\Delta Im^{OC}_{t+1} \times 100}{pop_{rt}}\right)$	0.0777	0.5897***
	(0.0877)	(0.1892)
F (exclud. inst.)	36.99	8.752
Obs.	300	300
R-squared	0.140	0.320
Year f.e.	YES	YES
Quarter f.e.	YES	YES
Region times year f.e.	NO	NO
Regional time trends	NO	NO
Quarter times year f.e.	NO	NO
Region f.e.	NO	NO
Stock of immigrants at t-1	NO	NO
Change Immig. from other countries	YES	YES

Each cell is a different regression. Each regression contains the following controls (native population): average age, the fraction of males, the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates.
Im^{OC} stands for immigrants from countries others than those involved in the Arab Spring.
The 2003 regional shares of immigrants from the Arab Spring countries relative to the native population are used as instrument for the regional stock of immigrants at t-1.
Observations are weighted by quarter specific population shares. Standard errors in parentheses. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.24: The effect of the Arab Spring migration using 1995 shares

	First stage regressions Change in Arab Spring immigrants (% native working age pop.)		
	(1) **2003 shares**	**(2)** **1995 shares**	**Observations in (2)**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.587***	1.227**	300
	(0.195)	(0.505)	
F-stat excl. instr.	65.98	5.90	
R-squared	0.133	0.120	

	Second stage regressions					
	Native employment change (fract. native working age pop.)			**Av. monthly earnings change (%)**		
	(1) **2003 shares**	**(2)** **1995 shares**	**Observations (2)**	**(1)** **2003 shares**	**(2)** **1995 shares**	**Observations (2)**
All sectors	-0.0034	0.0142	300	-0.0041	-0.0172	300
	(0.0202)	(0.0342)		(0.0156)	(0.0279)	
Agriculture, Forestry and Fishing	0.0074	0.0107	300	-0.0281	-0.0502	300
	(0.0066)	(0.0108)		(0.1041)	(0.1338)	
Mining	-0.0015*	-0.0015	300	0.2380	0.1175	300
	(0.0007)	(0.0009)		(0.4400)	(0.5314)	
Construction	0.0117**	0.0113*	300	-0.1491***	-0.0986	300
	(0.0041)	(0.0065)		(0.0413)	(0.1391)	
Manufacturing	-0.0003	0.0136	300	0.0675	0.0723	300
	(0.0113)	(0.0209)		(0.0459)	(0.0886)	
Transportation, Communications,	-0.0004	0.0049	300	0.0547	0.0560	300
Electric, Gas and Sanitary Services	(0.0099)	(0.0162)		(0.0559)	(0.0809)	
Wholesale Trade	-0.0174***	-0.0191**	300	0.0058	-0.0849	300
	(0.0043)	(0.0078)		(0.0507)	(0.1450)	
Retail Trade	-0.0008	-0.0051	300	0.0075	0.0661	300
	(0.0090)	(0.0134)		(0.0503)	(0.1293)	
Finance, Insurance and Real Estate	-0.0067	-0.0085	300	0.0227	0.0671	300
	(0.0055)	(0.0076)		(0.0794)	(0.1181)	
Hotels and Restaurants	-0.0121***	-0.0151**	300	-0.0086	-0.0653	300
	(0.0034)	(0.0062)		(0.0831)	(0.1301)	
Public Administration	-0.0048	-0.0139	300	0.0146	0.0076	300
	(0.0043)	(0.0115)		(0.0774)	(0.0917)	
Educational Services	0.0176***	0.0249**	300	-0.0416	-0.0454	300
	(0.0059)	(0.0111)		(0.0283)	(0.0446)	
Other Services	0.0039	0.0119	300	0.0066	-0.0294	300
	(0.0138)	(0.0183)		(0.0394)	(0.0872)	
Quarter, year fixed effects	YES	YES		YES	YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates.
Each entry in Panel B is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Observations are weighted by quarter specific population shares.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.25: The labor market effects of working age immigrants

Panel A: First stage regressions Change in Arab Spring immigrants (% native working age pop.)			
	Baseline	**Work. Age**	**Obs.**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.590***	1.312***	300
	(0.202)	(0.164)	
R-squared	0.136	0.113	

Panel B: Second stage regressions

	Native employment change (fract. of native working age pop.)			Av. monthly earnings change (%)		
	(1)	**(2)**		**(1)**	**(2)**	
	Baseline	**Work. Age**	**Obs.**	**Baseline**	**Work. Age**	**Obs.**
All sectors	-0.0032	-0.0039	300	-0.0037	-0.0045	300
	(0.0209)	(0.0255)		(0.0162)	(0.0194)	
Agriculture, Forestry and Fishing	0.0074	0.0090	300	-0.0293	-0.0355	298
	(0.0068)	(0.0083)		(0.1061)	(0.1309)	
Mining	-0.0015*	-0.0018*	300	0.2099	0.2550	247
	(0.0007)	(0.0010)		(0.4589)	(0.5709)	
Construction	0.0117**	0.0142**	300	-0.1479***	-0.1792***	300
	(0.0044)	(0.0050)		(0.0422)	(0.0587)	
Manufacturing	-0.0003	-0.0004	300	0.0676	0.0819	300
	(0.0119)	(0.0144)		(0.0476)	(0.0573)	
Transportation, Communications,	-0.0004	-0.0005	300	0.0548	0.0664	300
Electric, Gas and Sanitary Services	(0.0103)	(0.0125)		(0.0582)	(0.0722)	
Wholesale Trade	-0.0175***	-0.0212***	300	0.0040	0.0048	300
	(0.0046)	(0.0047)		(0.0523)	(0.0637)	
Retail Trade	-0.0005	-0.0006	300	0.0084	0.0102	300
	(0.0092)	(0.0111)		(0.0518)	(0.0624)	
Finance, Insurance and Real Estate	-0.0067	-0.0081	300	0.0231	0.0279	300
	(0.0055)	(0.0067)		(0.0823)	(0.0998)	
Hotels and Restaurants	-0.0120***	-0.0145***	300	-0.0119	-0.0145	300
	(0.0036)	(0.0043)		(0.0868)	(0.1046)	
Public Administration	-0.0048	-0.0058	300	0.0147	0.0179	300
	(0.0044)	(0.0051)		(0.0802)	(0.0968)	
Educational Services	0.0176***	0.0214***	300	-0.0412	-0.0500	300
	(0.0061)	(0.0074)		(0.0295)	(0.0351)	
Other Services	0.0036	0.0044	300	0.0073	0.0088	300
	(0.0142)	(0.0170)		(0.0411)	(0.0502)	
Quarter, year and region f. e.	YES	YES		YES	YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates.
Each entry in Panel B is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Observations are weighted by quarter specific population shares.
Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.26: The effect of the Arab Spring migration on employment and earnings of Italian born

	Second stage regressions					
	Native employment change (fract. native working age pop.)			**Av. monthly earnings change (%)**		
	(1)	**(2)**		**(1)**	**(2)**	
	Baseline	**Italian born**	**Observations (2)**	**Baseline**	**Italian born**	**Observations (2)**
All sectors	-0.0032	-0.0032	300	-0.0037	-0.0042	300
	(0.0209)	(0.0209)		(0.0162)	(0.0183)	
Agriculture, Forestry and Fishing	0.0074	0.0081	300	-0.0293	-0.0258	298
	(0.0068)	(0.0068)		(0.1061)	(0.0939)	
Mining	-0.0015*	-0.0015*	300	0.2099	0.3707	245
	(0.0007)	(0.0007)		(0.4589)	(0.3348)	
Construction	0.0117**	0.0094**	300	-0.1479***	-0.1266***	300
	(0.0044)	(0.0043)		(0.0422)	(0.0431)	
Manufacturing	-0.0003	-0.0028	300	0.0676	0.0551	300
	(0.0119)	(0.0122)		(0.0476)	(0.0435)	
Transportation, Communications,	-0.0004	0.0004	300	0.0548	0.0475	300
Electric, Gas and Sanitary Services	(0.0103)	(0.0106)		(0.0582)	(0.0581)	
Wholesale Trade	-0.0175***	-0.0176***	300	0.0040	0.0152	300
	(0.0046)	(0.0045)		(0.0523)	(0.0497)	
Retail Trade	-0.0005	-0.0009	300	0.0084	-0.0131	300
	(0.0092)	(0.0101)		(0.0518)	(0.0522)	
Finance, Insurance and Real Estate	-0.0067	-0.0062	300	0.0231	0.0287	300
	(0.0055)	(0.0057)		(0.0823)	(0.0908)	
Hotels and Restaurants	-0.0120***	-0.0132***	300	-0.0119	-0.0206	300
	(0.0036)	(0.0033)		(0.0868)	(0.0971)	
Public Administration	-0.0048	-0.0057	300	0.0147	0.0167	300
	(0.0044)	(0.0047)		(0.0802)	(0.0838)	
Educational Services	0.0176***	0.0169**	300	-0.0412	-0.0423	300
	(0.0061)	(0.0060)		(0.0295)	(0.0298)	
Other Services	0.0036	0.0010	300	0.0073	0.0131	300
	(0.0142)	(0.0116)		(0.0411)	(0.0430)	
Quarter, year and region f. e.	YES	YES		YES	YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, the fraction of white collar and tenured workers, the fraction of high school and college graduates. Observations are weighted by quarter specific population shares.
Each entry in the table is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Observations are weighted by quarter specific population shares.
Standard errors in parentheses. Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.27: The effect of migration on mobility - Outflow rates

Panel A: First stage regression				
Change in Arab Spring immigrants (% native working age pop.)				
	(1)	**Observations**	**(2)**	**Observations**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.7553***	200	1.6820***	200
	(0.4426)		(0.4542)	
R-squared	0.235		0.240	
Panel B: Second stage regression				
Regional outflow of Italians (fraction of native working age pop.)				
	(1)	**Observations**	**(2)**	**Observations**
Arab Spring immigrants	-0.1090	200	-0.1094	200
change (%)	(0.1384)		(0.1494)	
Panel C: Second stage regression				
Regional outflow of other immigrants (fraction of native working age pop.)				
	(1)	**Observations**	**(2)**	**Observations**
Arab Spring immigrants	-0.0644	200	-0.0723	200
change (%)	(0.0443)		(0.0523)	
Quarter and year f.e.	YES		YES	
Region f. e.	NO		YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high school and college graduates, current, one and two periods lagged employment and average earnings.
Observations are weighted by quarter specific population shares. Standard errors in parentheses.
Other immigrants include all the individuals who are born abroad except those who are born in Africa.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.28: The effect of migration on mobility - Inflow rates

Panel A: First stage regression				
Change in Arab Spring immigrants (% native working age pop.)				
	(1)	**Observations**	**(2)**	**Observations**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.6820***	200	1.6733***	200
	(0.4542)		(0.5074)	
R-squared	0.240		0.248	
Panel B: Second stage regression				
Regional inflows of Italians (fraction of native working age pop.)				
	(1)	**Observations**	**(2)**	**Observations**
All sectors	-0.3159	200	-0.3558	200
	(0.3688)		(0.3893)	
Construction	-0.0040	200	-0.0031	200
	(0.0238)		(0.0246)	
Wholesale trade	0.0400**	200	0.0371*	200
	(0.0156)		(0.0183)	
Hotels and Restaurants	0.0233	200	0.0240	200
	(0.0232)		(0.0250)	
Educational Services	-0.0256	200	-0.0259	200
	(0.0939)		(0.0990)	
Quarter and year f.e.	YES		YES	
Region f. e.	NO		YES	
Panel C: Second stage regression				
Regional inflows of other immigrants (fraction of native working age pop.)				
	(1)	**Observations**	**(2)**	**Observations**
All sectors	-0.0045	200	-0.0083	200
	(0.0472)		(0.0503)	
Construction	-0.0079	200	-0.0083	200
	(0.0169)		(0.0177)	
Wholesale trade	-0.0000	200	0.0000	200
	(0.0004)		(0.0004)	
Hotels and Restaurants	-0.0299**	200	-0.0286**	200
	(0.0114)		(0.0124)	
Educational Services	0.0008	200	0.0009	200
	(0.0010)		(0.0012)	
Quarter and year f.e.	YES		YES	
Region f. e.	NO		YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high school and college graduates, current, one and two periods lagged employment and average earnings.
Each entry in Panel B and C is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Observations are weighted by quarter specific population shares.Standard errors in parentheses.
Other immigrants include all the individuals who are born abroad except those who are born in Africa.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Table 2.29: Migration and employment - Robustness checks on population

Panel A: First stage regressions				
Change in Arab Spring immigrants (% native working age pop.)				
	Baseline	**Average population**	**2009Q1 opulation**	**Obs.**
$\left(\frac{Im^{AS}_{r2003}}{Im^{AS}_{2003}}\right) \times \left(\frac{\Delta Illegal_{t+1} \times 100}{pop_{rt}}\right)$	1.590***	1.588***	1.586***	300
	(0.202)	(0.203)	(0.205)	
R-squared	0.136	0.136	0.136	
Panel B: Second stage regressions				
Native employment change (fraction of native working age pop.)				
	Baseline	**Average population**	**2009Q1 opulation**	**Obs.**
All sectors	-0.0032	-0.0032	-0.0032	300
	(0.0209)	(0.0209)	(0.0209)	
Agriculture, Forestry and Fishing	0.0074	0.0075	0.0076	300
	(0.0068)	(0.0068)	(0.0068)	
Mining	-0.0015*	-0.0015*	-0.0015*	300
	(0.0007)	(0.0007)	(0.0007)	
Construction	0.0117**	0.0118**	0.0118**	300
	(0.0044)	(0.0044)	(0.0044)	
Manufacturing	-0.0003	-0.0004	-0.0004	300
	(0.0119)	(0.0118)	(0.0118)	
Transportation, Communications,	-0.0004	-0.0005	-0.0005	300
Electric, Gas and Sanitary Services	(0.0103)	(0.0103)	(0.0102)	
Wholesale Trade	-0.0175***	-0.0175***	-0.0176***	300
	(0.0046)	(0.0046)	(0.0047)	
Retail Trade	-0.0005	-0.0004	-0.0005	300
	(0.0092)	(0.0092)	(0.0092)	
Finance, Insurance and Real Estate	-0.0067	-0.0067	-0.0066	300
	(0.0055)	(0.0057)	(0.0057)	
Hotels and Restaurants	-0.0120***	-0.0119***	-0.0119***	300
	(0.0036)	(0.0036)	(0.0036)	
Public Administration	-0.0048	-0.0048	-0.0048	300
	(0.0044)	(0.0044)	(0.0044)	
Educational Services	0.0176***	0.0178***	0.0178***	300
	(0.0061)	(0.0061)	(0.0062)	
Other Services	0.0036	0.0035	0.0034	300
	(0.0142)	(0.0142)	(0.0142)	
Quarter, year and region f. e.	YES	YES	YES	

Each cell is a different regression. Each regression contains the following controls: average age, the fraction of males, the regional population, the fraction of full-time workers, white collar and tenured workers, the fraction of high school and college graduates, current, one and two periods lagged employment and average earnings.
Each entry in the table is the coefficients on the explanatory variable of interest (migration flows) in equation (2.4.6).
Observations are weighted by quarter specific population shares.Standard errors in parentheses.
Other immigrants include all the individuals who are born abroad except those who are born in Africa.
Standard errors are clustered at the regional level. *, ** and *** are 10, 5 and 1 percent significance levels.

Chapter 3

Preparing to Export

Abstract

Exporters differ from each other in size and export-market participation over time. This diversity, however, is not strongly reflected in their observed workforce composition regarding skills and occupations. Using Brazilian linked employer-employee data, we turn to a typically unknown worker characteristic: a worker's prior experience at other exporters. We show that expected export status, predicted with current destination-country trade instruments, leads firms to prepare their workforce by hiring workers from other exporters. Hiring away exporter workers is associated with both a wider subsequent reach of destinations and a deeper market penetration at the poaching firm, but only with reduced market penetration at the firm losing the worker. This evidence is consistent with the hypothesis that expected export-market access exerts a labor demand shock, for which exporters

actively prepare with selective hiring, and with the idea that a few key workers affect a firm's competitive advantage.

3.1 Introduction

A large body of empirical evidence and recent trade theory suggest that exporters substantively differ from non-exporters regarding size, productivity and workforce composition.[1] To learn more about export success, this paper compares Brazilian exporters among themselves regarding export dynamics and workforce characteristics. We document that firms actively prepare for expected exporting by hiring a few key workers from other exporters, and we provide evidence that hiring exporter workers is a strong predictor of various aspects of export-market success at the poaching firm.

There is considerable heterogeneity in performance and size among exporters. When we rank Brazilian exporters by their export-market participation over three consecutive years, this performance ranking is almost perfectly mirrored in a monotonic size ranking from about 80 workers at "marginal" in-out switching exporters to 550 workers at "successful" exporters with a sustained OECD-market presence. Surprisingly, the substantive heterogeneity in export participation and size is not reflected in observable worker characteristics. The workforce composition

[1] The literature following Bernard and Jensen (1995) documents exporter premia for many countries (see for example Bernard and Wagner 1997; Isgut 2001; Álvarez and López 2005). Exporter wage premia persist after controlling for unobserved worker and spell effects in linked employer-employee data (for example Schank, Schnabel, and Wagner 2007; Krishna, Poole, and Senses 2011).

regarding skills and occupations is similar among otherwise diverse exporters and in some cases statistically indistinguishable. Comparable to our evidence for Brazil, Bernard and Jensen (1997; 1999), Trefler (2004) and Harrigan and Reshef (2011) also find negligible differences in educational composition among U.S., Canadian and Chilean exporters in the cross section.[2] This leads us to hypothesize that typically unobserved worker characteristics can be important determinants of export-market performance.

We use comprehensive linked employer-employee data for the universe of formal Brazilian manufacturing firms and their export behavior between 1990-2001 to extract an otherwise unobserved worker characteristic: a worker's prior experience at other exporting firms. We define *hires from exporters* as the head count of hired workers whose immediately preceding formal employment was at an exporter. We propose that expected favorable export conditions in the future, predicted by current product market conditions abroad, exert a labor demand shock that leads firms to prepare workforces. To provide evidence on the hypothesis, we implement a new identification strategy for export preparations in economically stable times:

[2]Results for exporter responses to large-scale trade shocks are more mixed. Trefler (2004) detects no response of the educational workforce composition at Canadian exporters under the Canada-U.S. Free Trade Agreement, whereas Bustos (2011) finds that Argentine firms employ more educated workers when Mercosur reduces import duties in Argentina's neighboring export markets. Results are also mixed for major exchange rate shocks. Verhoogen (2008) argues that Mexican exporters upgraded workforce skills as reflected in wages around the Peso devaluation in 1995, whereas Frías, Kaplan, and Verhoogen (2009) favor the interpretation that increases in wage premia at Mexican exporters after the Peso devaluation are largely shared rents not associated with skill upgrading. Brambilla, Lederman, and Porto (2012) find that the workforce skill composition at Argentine exporters responded to the revaluation of the Peso against the Brazilian Real in 1999 only among the exporters that ship to high-income countries. Those studies rely on large-scale macroeconomic shocks for identification, whereas our instrumentation method isolates exporter responses also during tranquil times.

we use current sector-level imports into destinations outside Latin America from source countries other than Brazil as instruments to predict a Brazilian firm's future export status. Our panel data allow us to simultaneously condition on a rich set of worker and firm characteristics, including a firm's overall employment change, as well as firm, sector and year effects, domestic sector-level absorption and sector-year trends. A firm's instrumented future export status in turn predicts significantly more hires of former exporter workers in the current year.

Firms in Brazilian regions with many exporters, large firms, and firms with lasting export-market participation react most responsively in hiring away other exporters' workers. The poached workers share in the current employer's wage premium. A corollary of our hypothesis is that firms for which foreign product market conditions predicted a high probability of export-market participation but which subsequently fail to become exporters should let go again the recently poached hires from exporters. Our results show that unexpectedly unsuccessful exporters indeed separate again from most of their recently hired former exporter workers.

Former-exporter hires predict both a wider reach of destinations at the extensive margin and a deeper export-market penetration at the intensive margin. These effects are the strongest when there is an overlap of export destinations between the former and the current employer. Poaching exporter workers in marketing-related occupations predicts a wider destination reach, whereas poaching skilled production workers predicts a deeper market penetration. These findings are consistent with the idea that exporters actively build up workforce expertise for expected export-market access. Results also suggest that worker mobility may be a crucial mechanism by which knowledge spreads through an economy: we find that firms losing workers to other exporters do not suffer a significant decline in the number of export destinations, only a decline in market penetration, whereas hiring firms experience improvement at both margins.

Our paper is related to several strands of the existing literature. Recent trade theory for heterogeneous firms explains how the sorting of workers to employers interacts with exporting. One line of research considers competitive labor markets and generates assortative matching of more able workers to more capable firms, but workers with the same characteristics are paid the same wage (see for example Manasse and Turrini 2001; Yeaple 2005; Verhoogen 2008; Bustos 2011; Monte 2011; Burstein and Vogel 2012). Another line of research introduces labor market frictions so that workers with the same ability can be paid different wages by different firms, and higher wages by exporters. Search and matching frictions and the resulting bargaining over surplus from production can induce wages to vary across firms (see for example Helpman, Itskhoki, and Redding 2010; Davidson, Matusz, and Shevchenko 2008; Coşar, Guner, and Tybout 2016). Alternatively, efficiency wages that induce effort or fair wages can vary with revenue between firms (see for example Egger and Kreickemeier 2009; Amiti and Davis 2012; Davis and Harrigan 2011). In a dynamic setting, Fajgelbaum (2013) studies employment growth under search frictions with job-to-job mobility and shows that job-to-job transitions generate diverse outcomes across workers. Our data show that similarly able workers receive different wages depending on their employer (see also Helpman, Itskhoki, Muendler, and Redding 2017). While broadly consistent with our empirical work and gradual employment responses to anticipated export-market opportunities, the theoretical models do not discern potentially export-specific skills.

Worker skills relevant for exporting have been shown to be portable from firm to firm in case studies and firm surveys (Rhee 1990; Gershenberg 1987; Görg and Strobl 2005). Using linked employer-employee data, Balsvik (2011) and Poole (2013) provide systematic evidence that domestic employers exhibit higher productivity and pay higher wages after they hire workers from foreign-owned firms. Parrotta and Pozzoli (2012) document that poached recruits with specific knowledge from a prior employer significantly raise value added at the hiring firm. Poole (2013) uses linked employer-employee data from the same Brazilian source as we do and documents a statistically significant pay increase of incumbent workers at domestic firms after workers from foreign-owned firms join, but the pay raise is small. For export-market participation, in contrast, we find the hiring of a few former exporter workers to be an economically important variable, predicting a probability increase in export-market participation of about 3 percentage points. This is a considerable probability shift, given an overall exporting frequency of only 5 percent in manufacturing, and is similar in magnitude to what only substantive changes in observed workforce characteristics would predict.

In recent research closely related to ours, Minondo (2011), Sala and Yalcin (2015) and Mion and Opromolla (2014) investigate how the presence of managers with prior exporter experience changes a firm's outcomes. Minondo (2011) and Sala and Yalcin (2015) use linear and probit probability models for a firm's export status and show for Spanish and Danish firms that the presence of a manager with a previous job spell at an exporter predicts a higher probability of export participation at the current employer. Mion and Opromolla (2014) estimate Mincer wage regressions for Portuguese linked employer-employe data and document that managers with prior exporter experience receive sizeable wage premia, especially if the preceding and the current employer export to common destinations. Those empirical strategies lend themselves to the interpretation that a favorable labor supply condition (the treatment with managers assigned to firms) facilitates export performance at the manager's current employer. Our paper broadens the perspective to workers in any occupation and with any skill, and poses the complementary question: How do favorable product market conditions translate into a firm's labor demand for skills pertinent to exporting? Related to the specific literature on demand for observed skill and product-market conditions (see for example Guadalupe 2007 and the survey by Fortin and Lemieux 1997), we provide evidence that typically unobserved ability, inferrable from a worker's career history, influences employment and pay. Reminiscent of findings in the literature on knowledge spillovers and agglomeration (for example Jaffe, Trajtenberg, and Henderson 1993; Moretti 2004), the targeted hiring of exporter workers is statistically most significant in locations with a concentration in manufacturing.

Much empirical research has established evidence that firms with a competitive advantage self-select into exporting (see for example Clerides, Lach, and Tybout 1998; Bernard and Jensen 1999).[3] A more recent branch of the literature explores preparations for export-market entry.[4] López (2009) documents for a Chilean plant sample that productivity and investment increase prior to export-market entry. Identification rests on the notion of Granger causality that subsequent realizations of firm-level variables should not cause current realizations. Aw, Roberts, and Xu (2011) structurally estimate a model of innovation and exporting choices, and find that allowing for both endogenous exporting and innovation contributes to large estimated productivity gains at Taiwanese electronics plants. Lacovone and Smarzynska Javorcik (2012) study unit prices of products at Mexican plants. They use anticipated cuts in U.S. tariffs, which offer large-scale exogenous variation, and show that a product variety receives a domestic price premium one year before its first export, consistent with advance quality upgrading. Our paper explores preparations in workforce choice and uses current foreign product market conditions as instruments for identification, so our findings equally apply to exporter behavior under ordinary economic conditions.

The remainder of the paper is structured as follows. We describe our data in Section 3.2, and we document substantial differences among exporters in

[3] Most evidence suggests that a firm-level competitive advantage leads to exporting, and typically not the reverse, with some exceptions (for example Van Biesebroeck 2005; Crespi, Criscuolo, and Haskel 2008).

[4] This economic literature adds systematic evidence to case study and survey findings from related research in strategic management (see for example Gomez-Mejia 1988 and the survey by Leonidou, Katsikeas, and Coudounaris 2010) as well as organizational change (for a survey see Helfat and Lieberman 2002).

Section 3.3. In Section 3.4 we turn to our main analysis of workforce choice in response to foreign product market conditions, present the identification strategy, and empirically document active workforce preparations for subsequent exporting. Section 3.5 highlights worker and job characteristics that are closely associated with subsequent exporter success. Section 3.6 concludes.

3.2 Data

We combine data from three main sources. Our first data source is the universe of Brazilian exporters: a three-dimensional panel data set by firm, destination country and year between 1990 and 2001. Second, we match those exporter data to the universe of formal firms and all their formally employed workers. This second data source is a three-dimensional linked employer-employee panel data set by firm, worker and year between 1990 and 2001. The matched employer-employee-exports data provide us with information on the workforce at exporters as well as on transitions of workers from firm to firm, and complement the exporter data with the universe of formal non-exporting firms. Third, we combine the former two data sources with worldwide trade flow data by sector at distant destinations for Brazilian exporters to construct instrumental variables (IVs) for export status.

Exporter data. Exporter data derive from the universe of Brazilian customs declarations for merchandise exports by any firm collected at Secex (*Secretaria de Comércio Exterior*). For comparability to other studies, we remove agricultural and mining firms as well as commercial intermediaries from the exporter data and only keep manufacturing firms that report their direct export shipments. See Appendix 3.8.1 for more detail on the Secex data and their deflation.

Linked employer-employee data. Our source for linked employer-employee data is Rais (*Relação Anual de Informações Sociais*), a comprehensive administrative register of workers formally employed in any sector of Brazil's economy. This register contains the universe of formal Brazilian firms, including non-exporters. Rais offers information on worker characteristics such as education, a detailed occupational classification of the job, the firm's industry, and the legal form of the company including its foreign ownership, as well as the worker's earnings. We keep observations for the years 1990 through 2001, again drop all firms outside manufacturing, and then construct workforce and firm characteristics from employment on December 31st, and we track recent hires back to their last preceding employer's export status. These Rais records consist of 49 million formal workers employed at 449,390 manufacturing firms (1,767,491 firm-year observations). See Appendix 3.8.2 for more detail on Rais.

Combined with the Secex exporter data 1990-2001, we find that 23,518 manufacturing firms are exporters in at least one sample year (87,050 exporter-year observations). These manufacturing exporters account for only around 5 percent of formal manufacturing firms, similar to the around 5 percent exporter share in the U.S. universe of manufacturing firms (Bernard, Jensen, and Schott 2009). Single-employee firms enter the Rais records, explaining the apparently low share of exporter firms compared to data from many other developing countries, which censor their samples at a minimum employment level. In terms of employment, manufacturing exporters account for 24 million jobs or roughly half of Brazilian formal employment during the sample period.

Including both non-exporters and exporters, there is a total of 1,767,491 firm-year observations in our manufacturing data (after restricting the sample period to the years 1992-2001 in order to measure export status with two lags). In regression analysis, we use one lead year and our basic regression sample shrinks to 1,557,474 firm-year observations for 1992-2000. When we include employment change at the firm level as a covariate in regressions, only firms with observations for two consecutive years remain in the sample, and sample size drops to 1,199,490 firm-year observations for 1992-2000.

Given those large sample sizes, we report statistical significance only at the 1-percent significance level throughout this paper.

Tracing workers to prior and future employers. We track a firm's hires back to their prior employer. We define a relevant hire at a manufacturing firm as a worker accession that is not classified as a transfer between the firm's plants and that lasts at least until December 31st of the calendar year. We then trace the worker back to the last preceding formal-sector employment for up to three prior years and obtain the former employer's export status.[5] This allows us to identify *hires from exporters* as acceding workers whose immediately preceding

[5]For hires from exporters in 1990 or 1991 we use the exporter category in 1992 (see Table 3.1).

formal-sector employment during up to three past years was at an exporter. For predictions of exporter performance, we obtain in addition the share of common export destination markets (overlap) between the prior and the current employer, an indicator whether the former employer was a continuous exporter for three years, an occupational indicator whether the worker's prior employment was in sales (CBO 3-digit classification codes 400 to 499), and another occupational indicator whether the worker's prior employment was in an ISCO-88 skilled blue-collar occupation.[6]

We also track workers into the future. First, we follow recent hires from exporters into the next calendar year and identify subsequent separations. We define *separations of recent exporter hires* as hires from exporters whose new employment terminates before December 31st of the following year. Second, we track any worker who separates from a firm to the immediately following formal-sector employment for up to three subsequent years and obtain the future employer's export status (mirroring the definition for hires from exporters). This allows us to define *departures to exporters* as separating workers whose immediately following formal-sector employment during up to three future years will be at an exporter.

Worldwide trade flows by sector. Our IVs for expected export status are imports into destinations outside Latin America from source countries other than Brazil, by subsector IBGE. We use WTF data on bilateral trade (Feenstra,

[6]We also constructed a common-sector indicator whether the prior and the current employer are in the same subsector IBGE industry, an indicator whether the worker is employed in the same occupation at the current employer as at the prior employer, and the worker's tenure at the prior employer. We found none of those variables to be statistically significant predictors of exporter performance (Table 3.12), conditional on our common covariate set, and omit them.

Lipsey, Deng, Ma, and Mo 2005) from 1991 to 2000 to construct the IVs by subsector IBGE, year and six world destinations. The six world destinations are Asia-Pacific Developing countries (APD), Central and Eastern European countries (CEE), North American countries (NAM excluding Mexico), Other Developing countries (ODV), Other Industrialized countries (OIN), and Western European countries (WEU). We remove Latin American and Caribbean countries (LAC) from our set of IVs. We concord the SITC (Rev. 2) sectors at the four-digit level in WTF to subsector IBGE.[7] We then calculate aggregate imports into each foreign destination region, excepting imports from Brazil, by subsector IBGE. The IVs are plausibly unrelated to labor-market outcomes in Brazil other than through export-market shocks.

3.3 Exporter Types and Workforce Characteristics

Exporter categories. To document export success over time, we adopt a lexicographic ranking of export-market participation. We consider the current year and two preceding years and record in which of the three years a firm was an exporter with at least one reported shipment (8 possible combinations). We first order firms by current-year export status (t), within current-year status by past-year status ($t-1$), and within those by two-years past status ($t-2$). Beyond

[7] Our concordance is available at URL *econ.ucsd.edu/muendler/brazil*.

this basic time-pattern ranking, we separate non-exporting firms into those that are permanent non-exporters (non-exporters in every sample year) and current non-exporters (with foreign sales in at least one sample year). We also separate continuous-exporting firms into non-sustained exporters that do not serve one common destination in all three years, into sustained non-OECD exporters that serve at least one non-OECD country for three years, and into sustained OECD exporters that serve at least one OECD country for three years (resulting in a total of 11 possible combinations). Table 3.1 shows our resulting ranking of export success, with the category in the upper-most row showing the least successful exporters (permanent non-exporters) and the lower-most row containing the most successful exporters (sustained OECD exporters).[8]

We choose these export-status categories to clarify beyond a two-period categorization that there is considerable heterogeneity among exporters, both in terms of workforce sizes and export values. As displayed in Table 3.1, our time-pattern and destination-market ranking of export-market success is a refinement of a simpler two-period grouping of exporters into *non-exporters* for three consecutive years, exporters that *quit exporting* (including past quitters), firms that *start exporting* (including past starters), and exporters with *continuous exporting*.[9]

[8]In an alternative ordering, Álvarez and López 2008 classify firms as permanent exporters if they export in all sample years, as sporadic exporters if they export in at least one sample year, and as non-exporters if they do not export during the sample period. Except for permanent non-exporting, our lexicographic ordering does not depend on the number of sample periods. When we adopt the Álvarez and López classification, we obtain similar results.

[9]About 39 percent of manufacturing exporters are starters; they account for employment of four million workers out of a total of 49 million in manufacturing and command 6 percent of export sales.

Table 3.1: Export Status Ordering

Export status	Export period $t-2$	$t-1$	t	Firm-year observations (1)	Workers per firm (2)	Annual exports (3)
Non-Exporter						
Permanent non-exporter	0	0	0	1,596,947	12	
Current non-exporter [a]	0	0	0	60,198	66	
Export Quitters						
Past quitter	1	0	0	9,101	79	
In-out switcher	0	1	0	7,626	76	
Recent quitter	1	1	0	6,569	102	
Export Starters						
Recent starter	0	0	1	18,420	104	310.7
Re-entrant	1	0	1	3,181	137	231.0
Past starter	0	1	1	12,252	149	923.1
Continuous Exporters						
Non-sustained continuous exporter	1	1	1	6,047	178	561.1
Sustained non-OECD exporter[b]	1	1	1	21,916	232	889.2
Sustained OECD exporter[b]	1	1	1	25,234	552	10,803.7

Source: SECEX 1990 through 2001 (t: 1992-2001), manufacturing firms (subsectors IBGE 2-13).
Notes: Universe of 1,767,491 manufacturing firm-year observations. Exports (fob) in thousands of August-1994 USD. Permanent non-exporters do not export in any sample year; current non-exporters export in at least one sample year. Non-sustained continuous exporters export in three consecutive years but serve no single destination in all three years; sustained non-OECD exporters serve at least one destination (but no 1990-OECD member country) in three consecutive years; sustained OECD exporters serve at least one 1990-OECD member country in all three years.

Curiously, our refined export-status ranking is almost perfectly mirrored in the firms' ranking by workforce size (column 2). For example, permanent non-exporters have an average size of twelve workers, in-out switchers who recently quit exporting employ 76 workers, recent export starters employ 104 workers, while sustained OECD exporters employ 552 workers on average. This surprising employment size monotonicity is preserved for all but one pair of neighboring rows.[10] Our refined export-status ranking is also positively related to annual sales (column 3,

[10] A two-period classification would have lumped past quitters with non-exporters, but their workforce size turns out to be more similar to other quit-exporting firms under the refinement. Similarly, a two-period classification would have lumped past starters with continuous-exporting firms, but their workforce is more similar to other start-exporting firms under the refinement.

correlation coefficient of .11 at the firm level).

The vast majority of formal-sector manufacturing firms (over 90 percent) never exports in any year between 1990 and 2001. The 57,149 firms that quit or start exporting make up more than half of all firms that export in at least one year between 1990 and 2001 but account for only 6 percent of all export sales. Even among the continuous exporters, it is the select group of sustained OECD exporters that dominates. The 25,234 sustained OECD exporters are fewer than one-third of all current exporters, but they ship close to 90 percent of Brazilian exports and employ more than half of all exporters workers (and one-third of all Brazilian manufacturing workers). For a breakdown of export-market participation and employment by sector, see Table 3.14 in the Appendix.

Workforce composition. Table 3.2 reports summary statistics for the universe of manufacturing firms, restricting the sample to 1992-2000 to account for one lead in addition to two lags in export status. There are substantive differences in export-market participation among exporters. Compared to firms that start exporting, continuous exporters serve 2.7 times (one log unit) more destinations and have 4.6 times (one-and-a-half log units) larger sales per destination. Continuous exporters exhibit less than a one-in-twelve frequency of quitting exports, while firms that recently started exporting (within the past two years) quit exporting with almost a one-in-three frequency.

Surprisingly, workforce characteristics do not reflect exporters' performance and size differences. The most prevalent occupation in manufacturing, skilled blue-collar work, is performed by 63 percent of workers at the average manufacturing firm and by around 57 percent of workers at exporters, almost independent of the exporters' export status. Similarly, white-collar occupations are preformed to a similar degree across exporters, varying only between 28 and 31 percent. The most prevalent schooling level in manufacturing is primary education. There are more primary schooled workers at the average manufacturing firm with a share of 76 percent than at exporters with a share of 67 percent, but there is only minor variation among exporters for primary school educated workers (between 66 and 69 percent) or highly educated workers (between 8 and 10 percent).[11]

Firm heterogeneity is often described with log premia regressions, which show that non-exporters significantly differ from exporters along several dimensions including workforce characteristics. Arguably less attention has been paid to differences among exporters. In our exporter-premia regressions, we condition on sector and year effects, as well as on the firm's log employment to control for the part of the exporter premium that is predictable with size differences. The omitted firm category is non-exporters for at least three years.

Table 3.3 shows that workers at continuous exporters earn a wage premium of 55 percent (.44 log units) over workers at non-exporters, and even workers at recent export-market quitters earn 38 percent (.32 log units) more than workers at firms with no exports for three years. Only a small part of this wage premium is

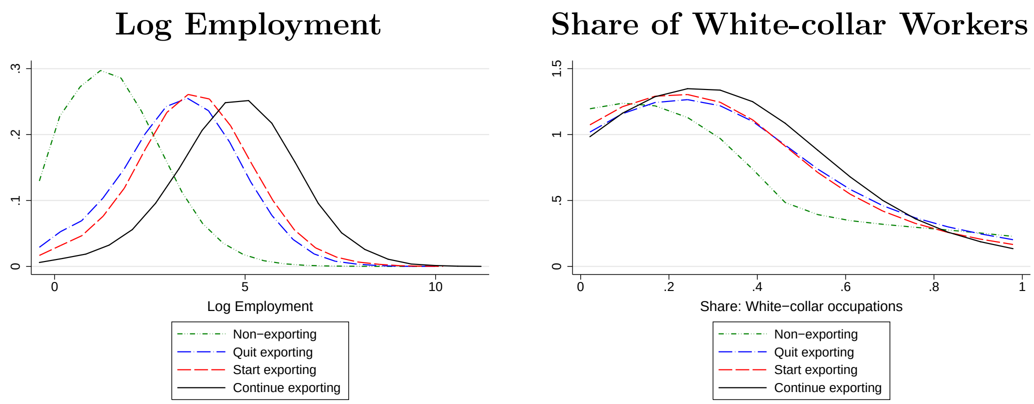
[11] Exporters, and especially continuous exporters, exhibit net employment reductions, a phenomenon beyond the scope of this paper. For related evidence and explanations see Helpman, Itskhoki, Muendler, and Redding (2017) and Bazzi, Menezes-Filho, and Muendler (2016).

due to different workforce compositions, as the log wage residual (from a regression on educational and occupational workforce variables) shows. The residual log wage still exhibits a premium between 28 and 42 percent (.25 and .35 log units) over non-exporters, suggesting that much wage variation remains to be explained by other firm or workforce characteristics. These findings are consistent with the hypothesis that mostly unobserved worker characteristics are associated with a firm's export status and that there is sharing of an exporter's profits with the exporter employees.[12]

Workforce composition differences in Table 3.3 are economically small and not generally statistically significant (at the one-percent significance level in the universe of firms). Employment premia for white-collar occupations, for instance, are statistically significantly different between exporters but economically roughly similar at a 7 to 10 percent premium for exporters of any status over non-exporters. For college (tertiary) educated educated workers there are statistically significant differences among continuous and other exporters but these differences are economically small (just as the raw mean differences in Table 3.2 show no marked variation among exporters of different status).

One typically unobserved worker characteristic is the worker's prior work experience at an exporter. Continuous exporters hire 43 percent (.36 log units) more workers from other exporters than export starters, conditional on firm size,

[12]For structural evidence on rent sharing in the cross section of firms see for example Helpman, Itskhoki, Muendler, and Redding (2017).



Sources: SECEX and RAIS 1992-2001, manufacturing firms (subsectors IBGE 2-13).
Note: Export status as defined in Table 3.1. Workforces on December 31st. Epanechnikov kernels with bandwidths .4 (employment) and .2 (white-collar occupations).

Figure 3.1: Density Estimates of Sizes and White-collar Shares

and export starters hire 33 percent (.29 log points) more workers with prior exporter experience than export quitters. Compared to these substantive differences in gross hires of workers with typically unobserved prior exporter experience, the observed workforce composition differences in Table 3.3 appear small.[13]

In Figure 3.1, we look beyond mean comparisons and plot nonparametric estimates of densities for firm characteristics. In the left graph of the Figure, the kernel estimates for log employment reflect the marked size rankings from Table 3.1 before, with continuous exporters' sizes exhibiting a clearly right-shifted probability mass over firms that start exporting, firms that quit exporting, and non-exporters in this order. The ranking becomes less clear-cut for shares of white-collar occupations in the right graph of Figure 3.1. While there is still a pronounced difference between non-exporters and exporters, the density functions for exporters with different status

[13]The differences in pay and gross hires of former exporter workers are even more pronounced in premia regressions that do not condition on size, and workforce characteristics premia are economically more similar among exporters (see Online Supplement).

exhibit multiple crossings and do not suggest as clear a ranking as there appears to be for sizes. The minor economic differences of workforce characteristics among exporters in Table 3.1 and the right graph of Figure 3.1 suggest that more successful and larger exporters employ scaled-up workforces with similar compositions as their less successful and smaller competitors.

To summarize, existing research documents that workforce characteristics differ between non-exporters and exporters. Our descriptive evidence shows in addition that export-market performance and sizes also differ markedly among exporters of different status. Commonly observed workforce characteristics such as educational attainment and occupations, however, are quite similar among exporters despite substantive diversity in export performance and size. However, the typically unobserved worker characteristic of a worker's prior experience at another exporting firm is markedly different between different types of exporters. We now query to what extent the hiring of former exporter workers occurs in preparation for export-market participation.

3.4 Preparing to Export

In trade models with endogenous technology adoption such as Yeaple (2005) and Costantini and Melitz (2008), falling variable trade costs induce more firms in differentiated-goods industries to adopt innovative technology and raise their employment, hiring away from differentiated-goods producers with lower productivity (in Costantini and Melitz 2008) or hiring away the top-skilled workers from firms with inferior technology (in Yeaple 2005). The timing of hiring and technology-adoption decisions is explicitly modelled by Costantini and Melitz who show in simulations that anticipated future drops in variable trade costs lead firms to adopt innovation before the anticipated favorable trade shock manifests itself.

Estimation model. Motivated by these theories, we adopt a straightforward empirical model of the firm's employment and export decision in two parts. First, a firm i observes export-market conditions $\mathbf{z}_{it}$ abroad at time t and uses them to linearly estimate the probability of its own future export-market participation next year $x_{i,t+1}$, conditional on its current firm characteristics and domestic market conditions $\mathbf{y}_{it}$:

$$x_{i,t+1} = \mathbf{y}'_{it}\boldsymbol{\gamma}_y + \mathbf{z}'_{it}\boldsymbol{\gamma}_z + \eta_{it}, \tag{3.4.1}$$

where η_{it} is a mean independent error term and $\boldsymbol{\gamma}_y$ and $\boldsymbol{\gamma}_z$ are vectors of regression coefficients. The measures of export-market conditions $\mathbf{z}_{it}$ are sector-level imports into foreign destinations (outside Latin America) from source countries other than Brazil. The idea for these foreign-demand IVs is that, prior to exporting, firms use the foreign market information available in the media, through trade fairs, or from specialized trade journals on their product markets to infer the future market conditions of their own expected residual demand.

Second, firm i uses the prediction of its future export status $\hat{x}_{i,t+1} = \mathbf{y}'_{it}\hat{\boldsymbol{\gamma}}_y +$

$\mathbf{z}'_{it}\hat{\boldsymbol{\gamma}}_z$ to choose the number of its hires from exporters h_{it}:

$$\ln(1 + h_{it}) = \mathbf{y}'_{it}\boldsymbol{\beta}_y + \hat{x}_{i,t+1}\beta_x + \epsilon_{it}, \tag{3.4.2}$$

where ϵ_{it} is a mean independent error term that is uncorrelated with $\mathbf{z}_{it}$, conditional on the set of covariates $\mathbf{y}_{it}$. The measure $\ln(1 + h_{it})$ of log gross hiring from exporters is zero for zero hires and increases monotonically at a decreasing rate in the number of hires so that regression coefficient reflect semi-elasticities.[14] In robustness analysis, we also use exports two and three periods in advance, $x_{i,t+2}$ and $x_{i,t+3}$, for otherwise the same right-hand side variables in equations (3.4.1) and (3.4.2).

The control variables $\mathbf{y}_{it}$ include firm fixed effects, sector fixed effects, year fixed effects and domestic sector-level absorption (to control for a potentially co-integrated sector-level business cycle abroad and in Brazil), three indicators for the firm's current export status (to capture different degrees of persistence in export market participation), the firm's employment change between $t-1$ and t relative to employment at t (to control for total net hiring that coincides with the hiring of exporter workers), employment (as a basic size measure), and an indicator whether the firm is directly foreign owned. This is our baseline specification. In a variant under firm-level IVs, we also include sector-year trends.

[14] We experimented with three more specifications of the left-hand side outcome in equation (3.4.2): $\ln h_{it}$ (which is only defined for non-zero hires), h_{it}, and an indicator $\mathbf{1}(h_{it} > 0)$. Those specifications result in the same significance and sign patterns as the specifications reported below (see Online Supplement).

A concern with the baseline specification is that omitted workforce characteristics and concomitant workforce changes, in addition to hiring-away exporter workers, may bias the estimates. However, the changing workforce composition is itself a potentially endogenous outcome of anticipated future export participation. To facilitate interpretation of our estimates we therefore adhere to the baseline regression and adopt longer regressions in robustness checks, including the workforce composition shares of worker education and occupation categories, an indicator whether the firm is high-skill intensive (its current share of technical/supervisory and professional/managerial occupations falling into the top quartile of firm-year observations), and observed changes in the workforce composition. We will find the main coefficient estimates to be similar in sign and magnitude to those from the shorter baseline regression.

While a large swing in the real exchange rate or dismantling trade barriers offers substantive variation beyond a firm's control, findings from such large-scale experiments, which might have considerable concomitant macroeconomic consequences, are arguably less instructive about exporter behavior in ordinary times. We therefore adopt an instrumentation strategy that relates a firm's export-market participation next year to current destination-market shocks. Our main identifying assumption is that a sector's current foreign market conditions $\mathbf{z}_{it}$ in destinations outside Latin America affect the hiring of exporter workers h_{it} only through expected export-market participation—conditional on the firm's sectoral affiliation, its current export status, its other observed characteristics and domestic market conditions at the sector level. The sectoral variation in the instruments allows us to remove concomitant economy-wide shocks, responses to country-level trade flows, and macroeconomic shocks through year effects and common sectoral responses through sector effects.

Our main hypothesis is that β_x is strictly positive. When firms observe a favorable foreign import-demand shock so that they can expect a higher chance of exporting $\hat{x}_{i,t+1}$ next year, they prepare their workforces similar to technology upgrading in Costantini and Melitz (2008) and top-skill hiring in Yeaple (2005). Our empirical design allows us to interpret a positive β_x coefficient as evidence of preparing to export because market conditions $\mathbf{z}_{it}$ at distant destinations abroad plausibly isolate how favorable product demand translates into a firm's labor demand $(1 + h_{it})$ for exporting skills.

Export-market shocks. There is limited econometric guidance to date for the selection among multiple valid IVs when some IVs are potentially weak but others strong. If the F statistic for the hypothesis that the instrumental-variable coefficient is non-zero on the first stage surpasses a value of 10, an instrument is commonly considered a strong one (Stock, Wright, and Yogo 2002). We have six potential IVs but our export status classification requires three IVs for our regressions to be just identified. To select the strongest possible set of IVs, we use the F statistic like an information criterion. We first regress the binary future exporting indicator on all six IVs and other exogenous variables, conditioning on firm, sector and year effects. From this initial regression we select the three IVs with the highest t statistics. We then set out to add IVs in the order of their t statistics, from next highest to lowest, and observe the evolution of the F statistic as we include IVs, with the intent to stop including IVs as soon as the F statistic starts falling. We find the import-demand IVs of OIN, WEU and NAM to have similarly high t statistics (between 3.9 and 3.4 in absolute value) and then add CEE to the regression, which has the next highest t statistic (1.7 in absolute value). With this addition, the F statistic for joint significance of the IVs drops, however. We therefore use no IVs other than import demand in OIN, WEU and NAM.

The upper panel (specification A) in Table 3.4 shows the results from linear regressions of future exporting on these pure demand IVs, conditional on our set of control variables.[15] There is no a priori expected sign for coefficients on our foreign import-demand measures. A positive sign is consistent with favorable consumer demand conditions at the foreign destination both for Brazilian and non-Brazilian exporters. A negative sign is consistent with unfavorable residual demand at the foreign destination for Brazilian exporters in the wake of large competing shipments by non-Brazilian export countries. By this interpretation of coefficients in Table 3.4, shipments from non-Brazilian export countries to North America and other industrialized countries tend to substitute Brazilian exports whereas others' shipments to Western Europe tend to complement Brazilian exports (columns 1 through 3). Expectedly, signs of significant coefficients are reversed for

[15]Firms are not nested within sectors in our data so sector fixed effects are separately identified but common clustering of standard errors in the two-stage least squares regression becomes inviable.

Brazilian firms that quit exporting (column 4).

Foreign market conditions $\mathbf{z}_{it}$ vary by sector and year and capture pure demand effects, which are common to all firms within a sector. While instrument validity is unaffected by this limited variation, predictive power of the IVs can be a concern. The F statistic clearly exceeds 10 for the binary future exporter indicator and for export starters, but the F statistic falls below the threshold of 10 for continuous exporting status and for firms that quit exporting. We will therefore interpret second-stage results for continuous exporters and export quitters with caution. For export status two or three periods in advance, in contrast, the same IVs clearly exceed the F statistic threshold of 10 for all exporter categories in our robustness regressions (see Tables 3.21 and 3.21 in the Appendix).

In the presence of sunk entry costs the firms' responses to changing foreign market conditions depend on the firm's current export status (Dixit 1989). Among the control variables $\mathbf{y}_{it}$ we include the firm's current export status, thus capturing the direct effect of current exporting on hiring exporter workers on the second stage (3.4.2). As a result, the interaction of worldwide market conditions and the vector of current export status indicators $z_{it}^{ww} \cdot \mathbf{x}_{it}$ is a valid instrument as long as persistent firm-level export-supply shocks are summarized by the current export status and hence do not confound second-stage estimation, conditional on a large set of firm-level controls. We exclude imports into any Latin American economy from the measure of worldwide imports z_{it}^{ww} and interact worldwide import demand with indicators for the three export status categories other than non-exporters

(Table 3.1).

The middle panel (B) in Table 3.4 shows the results for the first-stage of the according interacted instrumental variable regression. Expectedly, the F statistics now far exceed the threshold of 10. In the lower panel (C) in Table 3.4, we introduce sector-year trends in addition and the F statistics remain above the threshold of 10. The identifying assumption for the new set of instruments (B and C) is more restrictive. So we will check second-stage estimates from the alternative sets of instruments (A-C) against each other to assess robustness and query their implied validity.

Hiring away exporter workers. We now consider the hiring of former exporter workers at time t as a preparation for anticipated export-market participation in the next year. For this purpose, we use expected export-market participation at $t+1$, predicted by the above-mentioned observed foreign import-demand shocks at t.

Results in Table 3.5 show that expected future exporting is significantly positively associated with advance hiring of former exporter workers across all four specifications, irrespective of instrumentation. In magnitude, coefficient estimates are strictly larger when future exporting is instrumented (columns 2 through 4) than in ordinary regression (column 1). Note that our IV regressions measure the effect of expected future export-market participation (the treatment) on responding firms that are susceptible to favorable foreign demand conditions (treatment responders). In contrast, the ordinary regression (column 1) measures the covariation of observed future export-market participation on the universe of firms, including the bulk of never-exporting firms that are not susceptible to favorable foreign demand (never responders). Coefficients in IV regressions therefore expectedly exceed those from ordinary regression. We provide evidence on the most responsive firms by region and size below, consistent with this interpretation (Table 3.7).

Using pure foreign-demand IVs (specification A in column 2) predicts that firms prepare for an expected 10 percentage-point increase in the probability of export-market participation next year with one gross hire of a former exporter worker at the sample mean.[16] This is a plausible number. The average firm in the sample exports with a probability of 4.9 percent (Table 3.2). The average exporter contracts twelve former exporter workers per year during the sample period, while recent export quitters just hire three former exporter workers on average and the mean manufacturing firm just hires one (Table 3.2). Using foreign-demand IVs interacted with the firm's present export status (columns 3 and 4) leads to a smaller magnitude: by this measure, an expected 10 percentage-point increase in the exporting probability next year results in advance gross hiring of only .4 former exporter workers.

We condition on a firm's relative employment change, so employment expansions at hiring firms cannot confound our finding. We control for affiliates of foreign multinational enterprises, so our specification separates the effect of

[16]By the coefficient estimate in column 2, implied gross hiring of former exporter workers is $.1 \cdot 4.679 \cdot (1 + \bar{h}) = .98$ workers for a 10 percentage-point increase in the exporting probability and mean former exporter hires $\bar{h} = 1.1$ (Table 3.2). It is $.1 \cdot 2.034 \cdot (1 + \bar{h}) = .43$ by column 3 and $.1 \cdot 1.766 \cdot (1 + \bar{h}) = .37$ by column 4.

exports from foreign ownership. Our finding is also unaffected by common sectoral business cycles between Brazil and foreign markets because we control for domestic absorption at the sector level.

Numerous coefficients on current firm characteristics are consistent with the interpretation that strong firm-side performance up to the current year is not typically associated with hiring former exporter workers. Continuous exporting firms up to the current period, and export starters in the current period, hire strictly fewer former exporter workers than non-exporters, whereas firms that just quit exporting in the current period contract more former exporter workers, arguably in anticipation of a reversion in their export participation. Similarly, firms with more tertiary educated workers and a higher skill intensity hire strictly fewer former exporter workers (with a minor coefficient alteration for the fraction of exporters among high skill intensive firms). The overall pattern broadly supports the interpretation that currently less successful exporters and less well staffed firms pursue the strongest advance hiring of former exporter workers.

A comparison of results from the three different sets of instruments (A-C) shows that signs and significance patterns are highly robust across specifications (columns 2 through 4), with signs identical when significant for thirteen out of fifteen covariates.

Hiring away exporter workers by expected export status. Theory implies that firms with the largest anticipated gains from exporting have the strongest incentive to engage in preparatory hiring (Yeaple 2005; Costantini and

Melitz 2008). One proxy to returns from export-market participation is the expected exporter category, with expected continuous exporters arguably commanding larger gains than expected export starters. We accordingly estimate equation (3.4.1) for a vector of anticipated exporter status over three categories and look up to three years into the future.

Table 3.6 reports the results for one year in advance ($t+1$, with first stage results in Table 3.4, columns 2 through 4) and for two and three years in advance ($t+2$ and $t+3$, with first-stage results in Tables 3.21 and 3.22 in the Appendix). We disregard results from specification A in column 2, which produce a poor fit on the second stage under relatively weak instruments.[17]

As theory suggests, expected continuous exporters generally exhibit the strongest response. Results for $t+1$ show directly that expected continuous exporters hire more former exporter workers than any other firm. The coefficient on continuous exporting monotonically drops for later years $t+2$ and $t+3$, consistent with a declining incremental response of continuous exporters. To interpret results for $t+2$ in levels, note that expected continuous exporters at $t+2$ must be at least export starters at $t+1$, so the coefficients in the lower panels of Table 3.6 cumulate with those in the panel above (for export status definitions see Table 3.1). When it comes to coefficient magnitudes, expected continuous exporters at $t+2$ also hire more former exporter workers than any other firm. However, at $t+2$, only

[17]We consider the coefficient on expected export quitters at $t+1$ in specification A spuriously significant, given weak instruments with an F statistic of only 4.03 (Table 3.4 column 4). When instruments are sufficiently strong with F statistics of 66.32 for $t+2$ and 81.95 for $t+3$ (Tables 3.21 and 3.22 column 4), then expected export quitters exhibit no significant hiring.

the response of continuous exporters is statistically significant at the 1-percent significance level. At $t+3$, no response is statistically significant at the 1-percent level, and the response of continuous exporters would be the only statistically significant one at the 5-percent significance level.

Export starters show a statistically significant coefficient for exporter-worker poaching at the t+1 horizon, but smaller in magnitude than at continuous exporters. Export quitting at $t+1$, $t+2$ or $t+3$ is generally not associated with hiring of former exporter workers, except for specification B in column 3 at the $t+1$ horizon. One consistent explanation of that single finding might be that current exporters threatened by immediate export-market exit have a stronger incentive to poach former exporter workers than non-exporters, perhaps because a current exporter's expected returns from catching up to well staffed exporters are larger than for never-exporters, similar to a gamble for resurrection. In summary, continuous exporters with arguably the largest gains from exporting also exhibit the strongest response in hiring away exporter worker.

We now return to the difference in coefficient magnitudes between the instrumented and non-instrumented specifications. To assess our explanation that non-exporting firms (never responders) bias ordinary regression coefficients downwards, we query which firms most responsively hire former exporter workers.

Hiring away exporter workers by region and firm size. We interact the indicator of exporting one year in advance with the firm's location in one of three broad regions in Brazil, having three instruments at hand. São Paulo state is Brazil's manufacturing center, hosting about half of Brazil's manufacturing value added during the 1990s. The South and South East of Brazil (excluding São Paulo state) exhibit higher per-capita incomes than the North, North East and Center West, but neither the South nor the remaining South East (Rio de Janeiro, Minas Gerais and Espírito Santo) can match São Paulo state's concentration of manufacturing industries.

Results in the upper panel of Table 3.7 corroborate our interpretation that instrumented regressions reflect the responses of firms that are susceptible to favorable foreign demand conditions (treatment responders). We ignore results from weak instruments (specification A in column 2). Only firms in São Paulo state significantly respond to favorable foreign demand by hiring away exporter workers (columns 3 and 4). Arguably only the industry agglomeration in São Paulo state offers a sufficiently thick labor market to permit effective worker poaching.

We also interact the indicator of exporting one year in advance with the firm's current log size. Results in the lower panel of Table 3.7 for this interaction provide further evidence in favor of our interpretation of firm responsiveness (columns 3 and 4).[18] Only relatively large firms with an arguably strong competitive advantage respond to favorable foreign demand conditions by hiring former exporter workers. Overall, these findings on regional and size-specific responses are consistent with our hypothesis that non-exporting firms (never responders) bias ordinary regression

[18]Note that current mean log employment at exporters is 4.329 (Table 3.2), so an estimate of the net effect of future export status in specification A (column 2) is $.820 = -12.046 + 2.972 \cdot 4.329$. The net effect is therefore positive as expected but hard to interpret.

coefficients downwards.

Workforce composition and concomitant changes in observed skill demand. To assess the robustness of our baseline specification to omitted workforce characteristics, we adopt a long regression specification that includes the workforce composition shares of worker education and occupation categories as well as an indicator whether the firm is high-skill intensive (its current share of technical/supervisory and professional/managerial occupations falling into the top quartile of firm-year observations). For our main predictor of interest, Table 3.8 restates the earlier results from the short baseline specification (Table 3.5) in the upper-most panel and shows the results from the long specification with workforce controls in the middle panel. (For the full robustness regression results see Appendix 3.8.4.) The main coefficient estimates on the anticipated export status predictors remain closely similar, compared to our baseline specification in Table 3.5, and are not statistically significantly different.

A remaining concern with our findings could be that a firm may hire former exporter workers away not because of their prior exporter experience but because of their other skills. Those other skills could correlate with employment at an exporter. To isolate a hiring firm's changes to observed skill demand, we therefore augment the specifications of Table 3.5 not only with workforce characteristics in the current year but also with a set of concomitant relative employment changes by education and occupation group at the firm. Table 3.8 reports the results in the lower-most panel. A comparison to coefficient estimates in Table 3.5 shows that the inclusion of controls for observed workforce skill changes hardly affects our main estimates. None of the slight coefficient changes are statistically significant. In summary, our evidence suggests that a firm with expected export-market participation prepares its workforce by hiring away workers from other exporters because of those workers' exporter experience.

Alternative clustering assumptions. We assess the statistical significance of our results under alternative clustering assumptions, testing the hypothesis that a coefficient estimate is zero in three different ways. In the IV regressions reported in Table 3.5, we cluster the standard errors at the firm level. In doing so, we account for the existence of serial correlation at the firm level in unobserved factors that may affect hiring away from exporters. Panel I in Table 3.9 restates the IV regression results from Table 3.5, now reporting the p-value for each coefficient estimate instead of the standard error, for comparison to alternative tests. Reported p-values less than $1\,e{-}06$ indicate that estimates are zeroes at machine-size precision.

Panel II of Table 3.9 shows the results when clustering the standard errors at the sector level. Sector-level clustering allows for both serial correlation at the firm level and correlation across firms or over time within the same sector. We classify firms into twelve manufacturing sectors using the RAIS industry classification subsector IBGE. We need to assign firms to a single sector (cluster) over time and choose the mode sector for this assignment. We therefore lose observations of firms whose mode sector is indeterminate because of ties, resulting in a small reduction of our sample. Coefficient estimates, however, remain closely similar to those in Panel I. Under sector-level clustering the p-value increases compared to firm-level clustering, but we continue to reject the null of a zero coefficient at the 1-percent significance level. However, the small number of sectors raises the concern of lacking consistency.

A finer industry classification is not available for the full sample period. In Panel III we therefore show p-values from a wild bootstrap. Simulations have shown the wild bootstrap to produce a better test size than the standard Wald test under clustering when the number of clusters is small (Cameron, Gelbach, and Miller 2008b). We follow Davidson and MacKinnon (2010b) in applying the wild bootstrap procedure to our IV model. Expectedly, the p-values from the wild bootstrap are higher than those under clustering at the sector level (Panel II). The IV result without export status interactions (column 1) is now only statistically significant at the 5-percent level. However, we continue to reject the null of a zero coefficient at the 1-percent significance level for the interacted IVs (columns 2 and 3). In summary, we find no evidence that our baseline firm-level clustering assumption would lead to erroneous statistical significance judgments.

Wage changes and their components for hires from exporters. The value of a former exporter worker to the current employer should be reflected in the wage payment. For every hired worker j from an exporter, we compute the difference in the log wage between the current job and the immediately preceding job ($\ln w_{jt} - \ln w_{j,t-\tau}$). We then use the mean log difference in wages of hired former exporter workers at a given firm i as the left-hand side variable ($\Delta_i \ln w_{it} \equiv (1/J_i) \sum_j \ln w_{jt}/w_{j,t-\tau}$), and run our main regression equation (3.4.2) with the mean former exporter workers' log wage difference between jobs as dependent variable ($\Delta_i \ln w_{it} = \mathbf{y}'_{it}\boldsymbol{\delta}_y + \hat{x}_{i,t+1}\delta_x + \epsilon_{it}$). Table 3.10 reports the important coefficient estimate for expected future exporter status in the upper panel. Former exporter workers receive a sizeable log wage premium upon being hired away in response to the new employer's favorable export market demand shock. The pay increase is statistically significant at the one-percent level only in specification B (column 3) but would be significant at the five-percent level in all IV specifications.

To determine the source of the wage increase, we resort to a Mincer log wage regression $\ln w_{jt} = \boldsymbol{z_{jt}}'\boldsymbol{\vartheta}_t + \psi_{i(j)t} + \nu_{jt}$ in the cross section of workers j year by year to isolate three log wage components for every worker (as in Menezes-Filho, Muendler, and Ramey 2008):[19] first the log wage component $\boldsymbol{z_{jt}}'\hat{\boldsymbol{\vartheta}}_t$ that is predicted by observed worker characteristics (education, occupation, labor force experience, gender, age); second the plant-specific component $\psi_{i(j)t}$ predicted by a plant fixed effect in the annual cross section of employers (reflecting both a pure plant component and unobserved differences in workforce composition such as the plant average match effects); and third an individual worker residual component (ν_{jt}). We then use the mean difference in those wage components for hired former exporter workers at a given firm i as the left-hand side variable in our main

[19] To narrow the data to a single job per worker and year, we retain the last recorded and highest-paid job spell (randomly dropping ties) in a given year.

regression equation (3.4.2). Table 3.10 reports the coefficient estimates for expected future exporter status in the lower three panels.

Only the firm-average plant fixed effect is statistically significantly associated with the new employer's favorable export demand shock, whereas neither a hired former exporter worker's individual residual wage component nor the worker's observable characteristics are associated with the wage premium at the new employer. The decomposition exercise therefore shows that the pay increase for former exporter workers between jobs stems from the new employer's firm-wide pay, in which all employees share.

Firing recent exporter hires upon unexpected export failure. Regression specifications so far offer evidence for our main hypothesis that a firm hires away exporter workers when it can expect to realize export-market gains. A corollary of our hypothesis is that a firm with favorable foreign-demand conditions, which currently predict a high probability of export-market participation next year, should lay off again its currently poached hires from exporters if it fails to become an exporter by next year.[20] To pursue this placebo-like treatment, we follow recent hires from exporters in the current year into the next calendar year and identify separations that occur before the end of the next calendar year. We define *separations of recent exporter hires* as hires from exporters in the current year whose new employment terminates before December 31st of the next year. We then restrict the firm sample in two ways. First, we keep only those firm observations

[20]We thank Don Davis for this suggestion.

whose predicted export indicator for next year from Table 3.5 is above the sample median, consistent with a favorable expectation of export-market participation. Of those firm observations, we only keep the ones that turn out to be non-exporters next year. Second, we keep only firm observations with predicted exporting next year above the 75th percentile, and of those only the non-exporters next year.

For each restricted sample of unexpectedly failing exporters, we replicate equation (3.4.2) and regress separations from current exporter hires $\ln(1 + s_{i,t+1})$ on the prediction of the firm's future export status $\hat{x}_{i,t+1}$ and the control variables. Note that separations in this exercise are only for those workers recently hired from exporters. We know from estimates of equation (3.4.2) that a higher propensity of exporting next year leads to more hires of exporter workers in the current year. If those hires mainly serve for export-market entry, and little else, then we should expect in the restricted sample of unexpectedly failing exporters that a higher propensity of exporting next year leads to more firings of these recently hired former exporter workers over the next year. Results in Table 3.11 corroborate this implication.

The coefficient estimate on the exporting predictor for next year is strictly positive. Unexpectedly failing exporters fire more recent exporter hires if the exporting predictor induced them to poach more exporter workers in the current year. Given our endogenous sample restriction based on first-stage estimates, we bootstrap the standard errors over both estimation stages. The coefficients are statistically significant at the one-percent level in the larger sample with the median export indicator as the cutoff for a firm's predicted export indicator (and at the five-percent level in the smaller sample for specification B). Comparing estimates in the upper panel of Table 3.11 to the hiring estimates (Table 3.5) suggests that unexpectedly failing exporters let go again of between one-third to 90 percent of the recently poached hires from exporters.[21]

In summary, firms hire former exporter workers in advance of expected favorable export conditions, and especially firms in regions with thick manufacturing labor markets contract exporter workers in response to expected export-market participation. Large firms and firms that anticipate to become continuous exporters pursue relatively more such advance hires. The hired former exporter workers share in the firm-wide wage premium upon their employment change. Conversely, unexpectedly failing exporters lay off a significant fraction of their recently hired former exporter workers. We now return to a descriptive investigation into the importance of advance hiring of exporter workers for a firm's performance in foreign markets.

3.5 Predictors of Exporter Performance

Performance after hiring away exporter workers. We now restrict the sample to exporters only and seek additional evidence on two aspects of exporter performance. We decompose the log of a firm's exports into the log

[21] The coefficient ratios range from .37 and .36 under specifications (B) and (C) to .89 under specification (A).

number of its export destinations (market reach) and its log exports per destination (market penetration). We relate these two outcomes next year to the firm's present characteristics, including its hires of former exporter workers.

Table 3.12 shows two sets of three regressions for exporting firms, one set with the log number of destinations as dependent variable (columns 1 through 3) and one set with the log exports per destination as dependent variable (columns 4 through 6). Each regression conditions on the other outcome variable to isolate the covariation of predictors.

In a short regression, neither the indicator for hiring former exporter workers nor the log number of hired exporter workers are significant predictors of market reach at the one-percent significance level (column 1). The log number of hired exporter workers, however, is a significant predictor of export-market penetration in a short regression (column 4). We next bring to bear exporter categories in our data to discern between hires from continuous exporters and hires from recent export starters. For both outcomes at the hiring firm, market reach (column 2) and market penetration (column 5), now the log number of workers hired from continuous exporters is a significant predictor of better export performance, but not the number of hires from export starters. This finding is consistent with the idea that workers with a background at continuous exporters have unobserved characteristics that are more important for reaching more destinations and deeper into a destination than workers just with experience at export starters.

Finally, we bring to bear both additional worker-level and exporter information in our data to gain more detailed insight from long regressions. Among the hires from exporters, mostly workers in marketing occupations at the prior employer predict a wider market reach at the hiring firm (column 3) but not a deeper export-market penetration (column 6). Mostly workers in skilled blue-collar occupations at the prior employer predict a deeper market penetration by the hiring firm (column 6) but not a wider export-market reach (column 3). A larger overlap of export destinations between the prior employer and the current employer predicts a higher success for both market reach and penetration at the hiring firm. These findings are consistent with the idea that workers bring with them destination-specific knowledge. The findings also invite speculation that salespersons may be more important to reach additional destinations (perhaps because they know market characteristics and clients), whereas production skills (perhaps for high quality and timely delivery) are more relevant for deeper penetration of a market with additional sales.

Performance after departures of workers to exporters. For a final investigation as to how knowledge may move with workers, we consider the effect of departing workers on an exporter's success. For this purpose, we track a worker who separates from a firm to the immediately following formal-sector employment for up to three subsequent years and obtain the future employer's export status (mirroring the definition for hires from exporters). This allows us to define *departures to exporters* as separating workers whose following formal-sector employment is at an exporter.

We include an indicator for such worker departures to exporters and the log number of departures to exporters as additional regressors into the specifications of market reach and market penetration before. Table 3.13 reports the results for the two new variables. Remarkably, the log number of departures is a significant predictor only for market penetration (in the specification of column 6). A consistent interpretation is that current exporters might only suffer a significant loss in market penetration but not in market reach, once they know how to access a given set of foreign markets.

This result is interesting in at least two regards. First, the result offers a potential explanation why worker poaching can be successful. While the hiring firm may expect to improve export outcomes in two dimensions, both regarding market reach and market penetration, the losing firm may expect to suffer only in the dimension of market penetration. This difference in product-market outcomes potentially raises the marginal product of the poached worker for the hiring firm above the value for the losing firm. Second, the result suggests that worker mobility may be an efficient mechanism by which knowledge spreads through an economy. If the moving worker's marginal product increases with the move, the spread of knowledge is welfare improving.

3.6 Concluding Remarks

Using rich linked employer-employee data that track Brazilian manufacturing firms, their exports and individual workers over more than a decade, we document substantive size and performance differences among exporters, not just between exporters and non-exporters. Despite this diversity in export-market performance and employment, the workforce composition varies little among exporters. Looking into typically unobserved aspects of workers' job histories, we find that hiring a small number of former exporter workers is an important predictor of a firm's export-market success. To measure the extent of active workforce preparations for future exporting, we use import demands for non-Brazilian goods outside Latin America as instruments. We find that firms hire former exporter workers in response to favorable demand conditions abroad and in advance of expected export-market entry. Especially firms in regions with thick manufacturing labor markets, large firms, and firms that can anticipate to become continuous exporters contract exporter workers in response to expected export-market participation.

Hiring workers from marketing-related occupations at former exporters predicts a wider reach of destinations, and hiring skilled blue-collar workers from exporters predicts a deeper penetration of destinations. Yet the exact origins of former exporter workers' skills remain a matter for future research. Former exporter workers may have special skills from passive learning or active training at former exporters, they may know individual clients or have broad insight into destination-market characteristics, or their prior exporter employment may simply signal a screened but unobserved ability.

Our results are consistent with the idea that firms, especially firms with long-term export potential, actively contract a competitive workforce to add to their initial advantage, and then select to export. So firms prepare for expected export-market participation through prior workforce upgrading. These workforce preparations are consistent with recent trade models where firms can both choose export-market participation and engage in innovation, while each activity raises the return to the other. A firm's competitive advantage is therefore partly under its own control, and firms share in an economy's knowledge pool through mobile workers.

3.7 Acknowledgements

Chapter 3 is currently being prepared for submission for publication of the material. Labanca, Claudio; Molina, Danielken; Muendler, Marc-Andreas."Preparing to Export". The dissertation author was the principal researcher and author of this paper.

Table 3.2: Summary Statistics

Variable	All firms (1)	Exporters (2)	Export Status (t) Continuous (3)	Start (4)	Quit (5)
Foreign-market participation					
Indic.: Exporter (t)	.049	1.000	1.000	1.000	
Indic.: Affiliate of foreign MNE (t)	.0001	.0005	.0007	.0002	.0002
Log # Destinations (t)	.986	.986	1.375	.376	
Log Exports/Destination (t)	3.832	3.832	4.423	2.906	
Anticip. Continuous Exporting (t+1)	.031	.619	.854	.252	
Anticip. Start Exporting (t+1)	.017	.136		.350	.192
Anticip. Quit Exporting (t+1)	.013	.163	.076	.298	.398
Anticip. Non-exporter for three years (t+1)	.741				.287
Size					
Employment (t)	28.2	285.4	386.1	127.9	87.2
Log Employment (t)	1.756	4.329	4.758	3.658	3.311
Net Employment Change ($t-1$ to t)	-0.2	-5.5	-13.0	7.2	-6.1
Workforce characteristics					
Share: Unskilled blue-collar occupation (t)	.130	.127	.120	.137	.132
Share: Skilled blue-collar occupation (t)	.631	.576	.573	.580	.560
Share: White-collar occupation (t)	.239	.297	.306	.283	.309
Share: Primary school education (t)	.756	.673	.662	.690	.690
Share: High school education (t)	.207	.232	.234	.229	.228
Share: Tertiary education (t)	.037	.095	.104	.081	.081
Workforce background					
Indic.: Hires from Exporters (in t)	.205	.741	.786	.671	.529
Gross Hires from Exporters (in t)	1.1	12.1	15.2	7.3	3.5

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: 1,557,474 regression sample observations (employment change based on 1,277,201 observations of firms with consecutive-year presence). Export status as defined in Table 3.1. Current exporters (column 2) include firms with continuous exporting (column 3) or that start exporting (column 4) but not firms that recently quit exporting (column 5). Workforces on December 31st. Exports (fob) and annualized December wages in thousands of August-1994 USD.

Table 3.3: Exporter Premia conditional on Log Firm Size

	Export Status			t-tests of null-hypothesis	
	Continuous	Start	Quit		
Firm characteristic	(1)	(2)	(3)	(1)==(2)	(2)==(3)
Earnings					
Log Annual Wage	.440	.307	.316	$\neq$	
	(.006)*	(.004)*	(.005)*		
Residual Log Annual Wage	.351	.248	.256	$\neq$	
	(.005)*	(.004)*	(.005)*		
Observed workforce composition					
Share: Unsk. blue-collar occ.	-.021	-.003	-.001	$\neq$	
	(.002)*	(.002)	(.002)		
Share: Skilled Blue-Collar Occ.	-.081	-.070	-.085	$\neq$	$\neq$
	(.003)*	(.002)*	(.003)*		
Share: White-collar occ.	.102	.073	.086	$\neq$	$\neq$
	(.002)*	(.002)*	(.002)*		
Share: High School Education	.047	.034	.021	$\neq$	$\neq$
	(.002)*	(.002)*	(.002)*		
Share: Tertiary Education	.064	.042	.040	$\neq$	
	(.001)*	(.001)*	(.001)*		
Typically unobserved background					
Log Gross Hires from Exporters	.834	.475	.185	$\neq$	$\neq$
	(.011)*	(.007)*	(.007)*		

Sources: SECEX and RAIS 1992-2001, manufacturing firms (subsectors IBGE 2-13).
Notes: Premia are coefficients from linear regressions of the firm characteristic on export status dummies, controlling for the firms' log employment, sector and year effects in the universe of 1,767,491 manufacturing firm-year observations. Export status as defined in Table 3.1. The omitted baseline category is non-exporters for three years. Workforces on December 31st. Annualized December wages in thousands of August-1994 USD. The residual log annual wage is from a linear regression on educational and occupational workforce composition variables. The log number of gross hires from exporters is set to missing if zero. Robust standard errors in parentheses, clustering at the firm level, in parentheses. In columns 4 and 5, rejections of the null hypothesis of equality are reported for t tests at 1-percent significance.

Table 3.4: Foreign Demand and Future Export-Market Participation

Instrument (t)	Exporter (t+1) (1)	Export Status (t+1) Continuous (2)	Start (3)	Quit (4)
A: Sectoral Foreign Imports by Region, no trend (IV)				
Non-Brazil Imports in NAM (t)	-.037	.013	-.050	-.002
	(.017)	(.011)	(.015)*	(.013)
Non-Brazil Imports in OIN (t)	-.193	-.123	-.070	.070
	(.059)*	(.039)*	(.052)	(.044)
Non-Brazil Imports in WEU (t)	.031	.005	.026	-.026
	(.013)	(.009)	(.012)	(.010)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic	7.76	3.50	6.23	4.03
B: Sectoral Foreign Imports × Exporter Status, no trend (IV × Exp.)				
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.087	-.039	-.049	.039
	(.006)*	(.006)*	(.004)*	(.005)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.069	-.011	-.058	.035
	(.008)*	(.005)	(.006)*	(.007)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.020	-.007	-.013	-.019
	(.007)*	(.003)*	(.006)	(.006)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic	64.24	14.47	55.36	33.01
C: Sectoral Foreign Imports × Exporter Status, with sector trend (IV × Exp.)				
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.089	-.040	-.049	.042
	(.006)*	(.006)*	(.004)*	(.005)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.070	-.012	-.058	.037
	(.008)*	(.005)	(.006)*	(.007)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.022	-.008	-.014	-.017
	(.007)*	(.003)*	(.006)	(.006)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic	67.62	16.10	55.65	35.31

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13).
Notes: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption, panel C also controlling for linear sector trends. Binary future exporter indicator represents firms that start exporting at $t+1$ or that continue exporting at $t+1$; future and current export status as defined in Table 3.1. Non-Brazilian imports in Other Industrialized countries (OIN), Western European countries (WEU), North American countries (NAM excluding Mexico), and worldwide (WW excluding Latin America and Caribbean). Additional regressors: current export status, workforce characteristics, MNE indicator and absorption as in Table 3.5. F statistics for the joint zero effect of the IVs. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.5: Hires from Exporters

Predictor (t unless noted otherwise)	FE (1)	IV FE (A) (2)	IV × Exp. FE (B) (3)	IV × Exp. FE, trend (C) (4)
	Log [1 + Hires from Exporters] (t)			
Indic.: Anticip. Exporter ($t+1$)	.121	4.679	2.034	1.766
instr. in (2)-(4)	(.005)*	(1.076)*	(.194)*	(.175)*
Indic.: Continue Exporting	.014	-.564	-.229	-.188
	(.009)	(.140)*	(.028)*	(.025)*
Indic.: Start Exporting	.051	-.704	-.266	-.219
	(.006)*	(.180)*	(.034)*	(.031)*
Indic.: Quit Exporting	-.030	.285	.103	.088
	(.006)*	(.076)*	(.017)*	(.015)*
Rel. Employment Chg. ($t-1$ to t per t)	.002	-.0001	.001	.001
	(.0005)*	(.0005)	(.0004)*	(.0004)*
Log Employment	.231	.139	.192	.197
	(.002)*	(.022)*	(.004)*	(.004)*
Indic.: Affiliate of foreign MNE	.037	.102	.064	.061
	(.066)	(.115)	(.068)	(.065)
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic (excluded IVs first stage)		7.76	64.24	67.61

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator (column 1 of Table 3.4). Binary future exporter indicator represents firms that start exporting at $t+1$ or that continue exporting at $t+1$; current export status as defined in Table 3.1. Workforces on December 31st. F statistics for the joint zero effect of IVs on the first stage from Table 3.4. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.6: Hires from Exporters and Anticipated Future Export Status

	Log [1 + Hires from Exporters] (t)			
		IV	IV × Exp.	
	FE	FE (A)	FE (B)	FE, trend (C)
Predictor	(1)	(2)	(3)	(4)
		Export Status t+1		
Anticip. Continue Exporting (t+1)	.179	5.276	3.861	3.313
	(.009)*	(5.804)	(.750)*	(.675)*
Anticip. Start Exporting (t+1)	.113	3.141	2.311	1.956
	(.006)*	(3.036)	(.557)*	(.528)*
Anticip. Quit Exporting (t+1)	.034	16.822	1.603	1.236
	(.007)*	(4.606)*	(.583)*	(.554)
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistics (IVs first stage)		3.50, 6.23, 4.03	14.47, 55.36, 33.01	16.10, 55.65, 35.10
		Export Status t+2		
Anticip. Continue Exporting (t+2)	.235	3.006	.774	.747
	(.008)*	(7.986)	(.093)*	(.098)*
Anticip. Start Exporting (t+2)	.119	-5.293	.481	.358
	(.007)*	(11.685)	(.464)	(.442)
Anticip. Quit Exporting (t+2)	.095	1.666	.693	.739
	(.007)*	(8.855)	(.511)	(.503)
Observations	1,035,386	1,035,386	1,035,386	1,035,386
F statistics (IVs first stage)		191.27, 41.43, 66.32	402.51, 39.04, 22.08	373.90, 31.37, 20.28
		Export Status t+3		
Anticip. Continue Exporting (t+3)	.250	-16.713	.303	.253
	(.008)*	(40.050)	(.209)	(.242)
Anticip. Start Exporting (t+3)	.127	22.319	3.119	3.488
	(.007)*	(44.734)	(4.570)	(5.207)
Anticip. Quit Exporting (t+3)	.140	20.821	-1.605	-1.913
	(.007)*	(52.140)	(3.168)	(3.636)
Observations	872,537	872,537	872,537	872,537
F statistics (IVs first stage)		209.71, 87.31, 81.95	512.73, 49.53, 67.15	478.46, 43.81, 59.24

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. For the first panel of export status at t+1, specifications 2, 3 and 4 use instrumented future export status indicator (columns 2 through 4 of Table 3.4; instrumented future export status indicators for the second and third panel of export status at t+2 and t+3 reported in columns 1 through 3 of Tables 3.21 and 3.22 in the Appendix). Future and current export status as defined in Table 3.1. Workforces on December 31st. Additional workforce and MNE control variables as in Table 3.5. F statistics for the joint zero effect of IVs on the first stage are reported (from Tables 3.4, 3.21 and 3.22) in the order shown for the three predicted effects (Continue Exporting first, Start Exporting next, Quit Exporting last). Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.7: Hires from Exporters and Region and Size Interactions

	Log [1 + Hires from Exporters] (t)			
		IV	IV × Exp.	
	FE	FE (A)	FE (B)	FE, trend (C)
Predictor (t unless noted otherwise)	(1)	(2)	(3)	(4)
Regional Interactions				
Indic.: Antic. Exp. $(t+1)$ in São Paulo state	.105 (.007)*	-27.187 (236.744)	2.936 (.539)*	2.544 (.457)*
Indic.: Antic. Exp. $(t+1)$ in South/SouthEast	.130 (.008)*	-141.581 (1412.930)	2.064 (2.117)	1.552 (1.983)
Indic.: Antic. Exp. $(t+1)$ in North/NorthEast/CenterWest	.165 (.020)*	-787.961 (6646.776)	.830 (7.195)	1.691 (6.651)
Observations	1,199,227	1,199,227	1,199,227	1,199,227
F statistics (IVs first stage)		9.83, 1.55, 0.58	42.87, 59.63, 26.69	44.40, 60.10, 26.98
Log Size Interaction				
Indic.: Anticip. Exporter $(t+1)$	-.612 (.015)*	-12.046 (2.316)*	.060 (1.083)	.179 (1.034)
Log Employment	.221 (.002)*	.095 (.026)*	.185 (.005)*	.191 (.005)*
Indic.: Antic. Exp. $(t+1)$ × Log Employment	.191 (.004)*	2.972 (.394)*	.368 (.201)	.300 (.195)
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistics (IVs first stage)		7.76, 14.62	64.24, 102.57	67.61, 105.54

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator (column 1 of Table 3.4). Future and current export status as defined in Table 3.1. Additional variables as in Table 3.5. F statistics for the joint zero effect of IVs on the first stage (from Tables 3.19 and 3.20) are reported in the order shown for the three predicted regional effects (São Paulo state first, South/SouthEast next, North/NorthEast/CenterWest last) in the upper panel and the two predictions in the lower panel (Exporter indicator first, interaction of indicator with Log Employment second). Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.8: Hires from Exporters, Conditional on Workforce Variables

	Log [1 + Hires from Exporters] (t)			
		IV	IV × Exp.	
	FE	FE (A)	FE (B)	FE, trend (C)
Predictor	(1)	(2)	(3)	(4)
Baseline Specification (Table 3.5)				
Indic.: Anticip. Exporter ($t+1$)	.121	4.679	2.034	1.766
instr. in (2)-(4)	(.005)*	(1.076)*	(.194)*	(.175)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic (excluded IVs first stage)		7.76	64.24	67.61
Controlling for Workforce Characteristics				
Indic.: Anticip. Exporter ($t+1$)	.119	4.772	2.048	1.748
instr. in (2)-(4)	(.005)*	(1.094)*	(.208)*	(.186)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic (excluded IVs first stage)		7.75	57.78	60.80
Controlling for Workforce Characteristics and Skill Changes				
Indic.: Anticip. Exporter ($t+1$)	.112	4.701	1.982	1.687
instr. in (2)-(4)	(.005)*	(1.137)*	(.207)*	(.186)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic (excluded IVs first stage)		7.00	56.12	59.27

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator. Workforces on December 31st. Additional regressors: current export status, workforce characteristics, MNE indicator and absorption as in Table 3.5. Additional regressors in middle panel: those in upper-most panel and workforce characteristics as in Table 3.17. Additional regressors in lower-most panel: those in middle panel and workforce skill changes as in Table 3.18. F statistics for the joint zero effect of IVs on the respective first stage. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.9: Alternative Clustering Assumptions

	Log [1 + Hires from Exporters] (t)		
	IV	IV × Exp.	
	FE (A)	FE (B)	FE, trend (C)
Predictor	(1)	(2)	(3)
I: Baseline Firm-level Clustering (Table 3.5)			
Indic.: Anticip. Exporter $(t+1)$	4.679	2.034	1.766
p-value	$1.00\,e{-05}^*$	$< 1\,e{-06}^*$	$< 1\,e{-06}^*$
Observations	1,199,490	1,199,490	1,199,490
II: Sector-level Clustering			
Indic.: Anticip. Exporter $(t+1)$	4.936	2.074	1.769
p-value	$.008^*$	$< 1\,e{-06}^*$	$< 1\,e{-06}^*$
Observations	1,190,402	1,190,402	1,190,402
III: Sector-level Clustering, Wild Bootstrap			
Indic.: Anticip. Exporter $(t+1)$	4.936	2.074	1.769
p-value	.038	$< 1\,e{-06}^*$	$< 1\,e{-06}^*$
Observations	1,190,402	1,190,402	1,190,402

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator (column 1 of Table 3.4). Binary future exporter indicator represents firms that start exporting at $t{+}1$ or that continue exporting at $t{+}1$; current export status as defined in Table 3.1. Controls as in Table 3.5. p-values reported below coefficient estimates. Wild bootstrap based on 999 repetitions (Davidson and MacKinnon 2010b); asterisk marks significance at 1-percent level. Reports of p-values less than $1\,e{-06}$ indicate that estimates are zeroes at machine-size precision.

Table 3.10: Log Wage Changes for Hires from Exporters

Predictor	Change in mean Log Wage Component ($t-1$ to t)			
		IV	IV × Exp.	
	FE	FE (A)	FE (B)	FE, trend (C)
	(1)	(2)	(3)	(4)
	Change in mean **Log Wage**			
Indic.: Anticip. Exporter ($t+1$)	-.001	1.670	.795	.296
	(.004)	(.672)	(.127)*	(.120)
R^2 (overall)	.106	.030	.062	.100
	Change in mean **Worker Observable Log Wage Component**			
Indic.: Anticip. Exporter ($t+1$)	-.002	.309	-.065	-.085
	(.001)	(.203)	(.041)	(.041)
R^2 (overall)	.165	.090	.166	.165
	Change in mean **Plant-fixed Log Wage Component**			
Indic.: Anticip. Exporter ($t+1$)	.0006	1.564	.947	.576
	(.003)	(.516)*	(.095)*	(.086)*
R^2 (overall)	.155	.070	.106	.141
	Change in mean **Individual Worker Log Wage Residual Component**			
Indic.: Anticip. Exporter ($t+1$)	.0001	-.203	-.087	-.194
	(.003)	(.443)	(.095)	(.093)
R^2 (overall)	.002	.004	.004	.004

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). 658,077 observations.

Notes: Log wage change is difference between the current log wage (component) and the log wage (component) at the preceding exporter. Log wage components from Mincer (1974) regressions by year for the cross section of plants, decomposing the log wage into a worker observable component, a plant-fixed component, and an individual worker residual, and then summing up over current employer's hires from exporters. Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator (similar to Table 3.4 for subsample with wage information). Future and current export status as defined in Table 3.1. Additional workforce and MNE control variables as in Table 3.17. Standard errors in parentheses (no correction for generated regressors); asterisk marks significance at 1-percent level.

Table 3.11: Separations of Recent Exporter Hires at Unexpectedly Unsuccessful Exporters

	Log [1 + Separations of Recent Exp. Hires] (t+1)		
	IV	IV × Exp.	
	FE (A)	FE (B)	FE, trend (C)
Predictor (predictors at t not reported)	(1)	(2)	(3)
Unsuccessful Exporters with Pred. Export Indic. above Median			
Pred. Indic. Anticip. Exporter (t+1)	4.248	.754	.630
	(1.152)*	(.234)*	(.195)*
Observations	576,340	576,226	576,186
R^2 overall (subsample)	.257	.256	.257
Unsuccessful Exporters with Pred. Export Indic. above 75th Percentile			
Pred. Indic. Anticip. Exporter (t+1)	4.314	.565	.492
	(2.131)	(.307)	(.303)
Observations	257,766	257,623	257,592
R^2 overall (subsample)	.260	.262	.261

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 3. Additional workforce and MNE control variables as in Table 3.17. Standard errors from 50 bootstraps over both stages in parentheses; asterisk marks significance at 1-percent level.

Table 3.12: Predictions of Future Exporter Performance

Predictor (t unless noted)	Log # Destinations $_{(t+1)}$ (1)	(2)	(3)	Log Exports/Dest. $_{(t+1)}$ (4)	(5)	(6)
Log # Destinations (t+1)				.114	.114	.104
				(.013)*	(.013)*	(.013)*
Log Exports/Destination (t+1)	.029	.029	.026			
	(.003)*	(.003)*	(.003)*			
Log Employment	.204	.198	.186	.275	.276	.261
	(.010)*	(.010)*	(.010)*	(.019)*	(.018)*	(.019)*
Rel. Employment Chg. ($t-1$ to t per t)	-.001	-.001	-.001	-.002	-.002	-.002
	(.0008)	(.0008)	(.0008)	(.001)	(.001)	(.001)
Indic.: Hires from Exporters	-.010			-.002		
	(.008)			(.016)		
Log Gross Hires from Exp.	.009			.039		
	(.004)			(.007)*		
Indic.: Hires from Start Exp.		-.009	.010		.023	.030
		(.005)	(.006)		(.011)	(.012)*
Log Gross Hires from Start Exp.		.006	.016		.014	.015
		(.005)	(.005)*		(.009)	(.010)
Indic.: Hires from Cont. Exp.		.007	-.0009		.010	.010
		(.007)	(.009)		(.014)	(.020)
Log Gross Hires from Cont. Exp.		.011	-.003		.029	.007
		(.004)*	(.005)		(.008)*	(.009)
Indic.: Skld. Bl. Hires fr. Exp.			.009			-.035
			(.009)			(.020)
Log Gr. Skld. Bl. Hires fr. Exp.			-.005			.029
			(.004)			(.009)*
Indic.: Mkt. Occ. Hires fr. Exp.			-.0007			.008
			(.006)			(.012)
Log Gr. Mkt. Occ. Hires fr. Exp.			.014			-.006
			(.005)*			(.010)
Mean # Overlapping Dest.			.048			.026
			(.002)*			(.003)*
Indic.: High-skill firm	.022	.023	.016	.004	.004	.004
	(.011)	(.011)	(.010)	(.022)	(.022)	(.022)
Indic.: High-sk. frm. $\times$ Ind.: Hires fr. Exp.			-.070			-.044
			(.010)*			(.022)
Observations	56,141	56,141	56,141	56,141	56,141	56,141
R^2 (overall)	.292	.293	.410	.279	.28	.288

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), current and future manufacturing exporters (subsectors IBGE 2-13).
Notes: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption. Workforces on December 31st. Exports (fob) in thousands of August-1994 USD. Log number of gross hires from exporters set to zero if zero hires. High-skill firms are firms with share of technical/supervisory and professional/managerial occupations in top quartile of firm-year observations. Additional control variables as in Table 3.5. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.13: Predictions of Future Exporter Performance, controlling for Departing Workers to Exporters

Predictor (t)	Log # Destinations (t+1) (1)	(2)	(3)	Log Exports/Dest. (t+1) (4)	(5)	(6)
Indic.: Departures to Exporters	.011	.011	.017	.0007	.002	-.004
	(.006)	(.006)	(.007)	(.013)	(.013)	(.014)
Log Gross Departures to Exp.	.001	-.0004	-.008	-.017	-.019	-.023
	(.004)	(.004)	(.004)	(.008)	(.008)	(.008)*
Observations	56,141	56,141	44,463	56,141	56,141	44,463
R^2 (overall)	.292	.293	.411	.278	.277	.268

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), current and future manufacturing exporters (subsectors IBGE 2-13).
Notes: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption. Additional workforce and MNE control variables as in Table 3.12. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

3.8 Data Construction and Additional Results

3.8.1 SECEX exports data

All export values in the Secex exports data are reported in current U.S. dollars (USD), free on board (fob). We have observations on exporting plants, declared export values and export destinations for the years 1990 through 2001. We aggregate monthly plant-level export information to years and firms. We deflate export sales to their August-1994 equivalents using the monthly U.S. consumer price index (from Global Financial Data). The choice of August 1994 is motivated by the timing of Brazil's last major currency reform in July 1994, which put the Brazilian Real (BRL) value at an initial exchange rate of one with the U.S. dollar (USD).

Exporting is transitory for most Brazilian exporters. Similar to evidence in Brooks (2006) for Colombian plants between 1981 and 1991, only a fraction of any cohort of first-time exporters continues to export after a year. Of the 1993 cohort, for instance, less than a quarter of firms is still an exporter by 1998, five years later. Of the 1996 cohort, only slightly more than a quarter of firms is still an exporter by 2001.[22]

[22]An empirical supplement with according tabulations is available at URL *econ.ucsd.edu/muendler*.

3.8.2 RAIS linked employer-employee data

Brazilian law requires every Brazilian plant to submit detailed annual reports with individual information on its employees to the ministry of labor (*Ministério de Trabalho*, MTE). The collection of the reports is called *Relação Anual de Informações Sociais*, or Rais, and typically concluded at the parent firm by March for the preceding year of observation. By design, Rais covers all formally employed workers in any sector (including the public sector) and tracks workers nationwide over time between formal jobs. Workers with no current formal employment, however, are not in Rais. Our version of the data provides monthly spell information on individually identified workers at individually identified plants. Similar to our treatment of the Secex data, we aggregate the monthly worker-plant information to years and firms for most of our analysis. (For Mincer log wage regressions at the worker level we retain the last recorded and highest-paid job spell, randomly dropping ties, in a given year and estimate cross-sectional employer fixed effects at the plant level.) Annual aggregation removes seasonal fluctuations in worker accession and separation rates from the data.

Rais primarily provides information to a federal wage supplement program (*Abono Salarial*), by which every worker with formal employment during the calendar year receives the equivalent of a monthly minimum wage. A strong incentive for compliance is that workers' benefits depend on Rais so that workers follow up on their records. The ministry of labor estimates that currently 97 percent of all formally employed workers in Brazil are covered in RAIS, and that coverage exceeded 90 percent throughout the 1990s.

We keep observations for the years 1990 through 2001, drop all firms outside manufacturing, and then use the data for the construction of several sets of variables. First, we use employment on December 31st to obtain information on the firm's workforce size and composition across all its plants. We pay attention mainly to the education and occupation categories and construct according shares and changes over time (see Appendix 3.8.2 for definitions). Second, we use worker IDs to trace recent hires at potential exporting firms back to their preceding employer and count the number of gross hires who were employed at an exporter in their immediately preceding job. For the purpose of worker tracking, we restrict the worker sample to all proper worker IDs (11-digit *PIS*).

Third, we obtain industry information for every firm. RAIS reports industries at the subsector IBGE classification (roughly comparable to the *NAICS 2007* three-digit level) over the full sample period. Subsector IBGE industries are recorded by plant, however. There are multi-plant firms in our sample, and we assign the industry associated with most employees in a given year to multi-plant firms. At the subsector IBGE level, there are twelve manufacturing industries in RAIS. The main sector affiliation of firms varies over time. There are 36,599 observations of firms that change sector so that firm effects are not nested within sector effects in later empirical analysis. While RAIS offers comprehensive workforce information, data on domestic sales are neither available from SECEX nor RAIS.

Table 3.14: Firm Characteristics by Industry

Subsector IBGE	Firm-year observ.	Workers per firm	Share (%) exporters	Workers per exp.	Exports per exp.
Non-metallic mineral products	137,091	18.8	.026	212.5	1,574.7
Metallic products	201,093	24.8	.046	288.4	5,974.8
Machinery, equipment and instruments	73,976	39.4	.152	167.9	1,962.3
Electrical and telecomm. equipment	40,603	51.9	.123	285.8	2,618.3
Transport equipment	39,169	80.9	.103	622.4	13,010.7
Wood products and furniture	234,913	15.2	.042	120.1	1,064.9
Paper and paperboard, and publishing	132,108	23.0	.023	349.9	5,118.3
Rubber, tobacco, leather, and prod. nec.	96,152	25.3	.082	173.1	2,805.6
Chemical and pharmaceutical products	131,110	37.2	.099	206.4	2,100.9
Apparel and textiles	332,926	20.6	.025	314.1	1,290.1
Footwear	48,881	46.5	.099	335.2	2,630.4
Food, beverages, and ethyl alcohol	299,469	34.1	.024	637.2	9,372.6
Total	1,767,491	27.7	.049	278.9	3,598.7

Sources: SECEX and RAIS 1990-2001, manufacturing firms (subsectors IBGE 2-13).
Notes: Employment on December 31st. Exports (fob) in thousands of August-1994 USD.

Table 3.14 reports firm counts, the share of exporters (from the link to SECEX exporter information) and select firm characteristics by subsector IBGE.[23] On average, only about 5 percent of Brazilian formal-sector manufacturing firms are exporters, a considerably smaller share than in Chile (21 percent of manufacturing plants export in 1990-96, see Álvarez and López 2005), or Colombia (18 percent of plants in 1991, see Brooks 2006) and Mexico (36 percent of plants in 1996, see Lacovone and Smarzynska Javorcik 2012). Our data are more closely comparable to the U.S. universe of manufacturing firms (a 5 percent exporter share in the U.S. universe of manufacturing firms, see Bernard, Jensen, and Schott 2009).

Exporting is most frequent in machinery and equipment manufacturing

[23] We consider as industrialized countries the 24 OECD member countries in 1990: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal (including Madeira Islands), Spain (including Alborán, Parsley Island, and Canary Islands), Switzerland, Turkey, United Kingdom (including Channel Islands), and the United States. We exclude the following types of exports and destinations: immediate reexports of imports, on-board aircraft consumption, and non-declared destinations.

Table 3.15: Education Categories

	RAIS category	Education Level
1.	8.-9.	Some College or College Graduate
2.	6.-7.	Some High School or High School Graduate
3.	1.-5.	Illiterate, or Primary or Middle School Educated (*reference category*)

industries, where workforce sizes per firm also tend to be large. Except for transportation equipment, the industries with most frequent exporting are populated by firms with below-average sizes and below-average exports per firm. We account for sector differences with industry-fixed effects in all regressions.

3.8.3 Education and occupation categories in RAIS

We group education information from nine RAIS education categories into three categories as shown in Table 3.15.

Occupation indicators derive from the 3-digit CBO classification codes in our nationwide RAIS data base, and are reclassified to conform to ISCO-88.[24] We map RAIS occupations into ISCO-88 occupations and regroup them into five categories as shown in Table 3.16.

Earnings. We use the monthly December wage paid to workers with employment on December 31st of a given year. RAIS reports the December wage in multiples of the current minimum wage. We use the log of annualized December wages as our earnings measure, defined as the reported monthly wage times the December U.S. dollar equivalent of the current minimum wage times 12. Similar to

[24]See online documentation at URL *econ.ucsd.edu/muendler/brazil.*

export values, we deflate this earning measure to its August-1994 equivalent using the monthly U.S. consumer price index (from Global Financial Data).

Legal form. RAIS reports a firm's legal form, including its direct foreign ownership by a foreign company (the according legal form code is "branch or office of foreign company"). Indirect foreign ownership, minority foreign ownership, or portfolio holdings do not fall under this category. We use the annual mode of legal form across the firms' workers to deal with occasional coding errors of legal form. The self-reported foreign-ownership category in RAIS potentially differs from foreign ownership in Poole (2013), who uses independent information on direct and indirect foreign ownership from the Central Bank of Brazil for a shorter sample period.

3.8.4 Additional Robustness Checks

To assess the robustness of our baseline specification to omitted workforce characteristics and concomitant workforce changes, we adopt long regression speci-

Table 3.16: Occupation Categories

	ISCO-88 occupation category	Occupation Level
1.	Legislators, senior officials, and managers	Professional or Managerial
	Professionals	Professional or Managerial
2.	Technicians and associate professionals	Technical or Supervisory
3.	Clerks	Other White Collar
	Service workers and sales workers	Other White Collar
4.	Skilled agricultural and fishery workers	Skilled Blue Collar
	Craft and related workers	Skilled Blue Collar
	Plant and machine operators and assemblers	Skilled Blue Collar
5.	Elementary occupations	Unskilled Blue Collar (*reference category*)

fications. We include the workforce composition shares of worker education and occupation categories as well as an indicator whether the firm is high-skill intensive (its current share of technical/supervisory and professional/managerial occupations falling into the top quartile of firm-year observations), and report the results in Table 3.17. In Table 3.18 we show results when we also control for concomitant observed changes in the workforce composition. The main coefficient estimates on the anticipated export status predictors remain closely similar, compared to our baseline specification in Table 3.5.

3.8.5 Additional First-stage IV Results

Tables 3.19 through 3.23 present the first-stage IV regressions that accompany Tables 3.18, 3.7 and 3.6 in the text as well as Table 3.17 in the Appendix.

Table 3.17: Hires from Exporters, Conditional on Workforce Composition

	Log [1 + Hires from Exporters] (t)			
		IV	IV × Exp.	
	FE	FE (A)	FE (B)	FE, trend (C)
Predictor (t unless noted otherwise)	(1)	(2)	(3)	(4)
Indic.: Anticip. Exporter ($t+1$)	.119	4.772	2.048	1.748
instr. in (2)-(4)	(.005)*	(1.094)*	(.208)*	(.186)*
Indic.: Continue Exporting	.044	-.611	-.227	-.178
	(.010)*	(.157)*	(.033)*	(.029)*
Indic.: Start Exporting	.081	-.753	-.265	-.209
	(.007)*	(.198)*	(.039)*	(.035)*
Indic.: Quit Exporting	-.032	.294	.103	.086
	(.006)*	(.078)*	(.018)*	(.016)*
Rel. Employment Chg. ($t-1$ to t per t)	.002	-.0002	.0009	.001
	(.0005)*	(.0005)	(.0004)*	(.0004)*
Log Employment	.230	.137	.191	.197
	(.002)*	(.022)*	(.004)*	(.004)*
Share: High school education	.005	-.006	.0005	.002
	(.002)*	(.004)	(.002)	(.002)
Share: Tertiary education	-.025	-.027	-.026	-.025
	(.003)*	(.007)*	(.004)*	(.004)*
Share: Skilled blue-collar occ.	-.006	-.020	-.012	-.011
	(.002)*	(.006)*	(.003)*	(.003)*
Share: Other white-collar occ.	-.064	-.052	-.059	-.061
	(.004)*	(.008)*	(.005)*	(.005)*
Share: Techn. or supervis. occ.	.036	.041	.038	.038
	(.004)*	(.008)*	(.005)*	(.005)*
Share: Profess. or manag'l. occ.	.009	.049	.026	.023
	(.006)	(.016)*	(.008)*	(.007)*
Indic.: Affiliate of foreign MNE	.035	.104	.064	.060
	(.067)	(.117)	(.068)	(.064)
Indic.: High-skill firm	-.052	-.072	-.060	-.059
	(.002)*	(.006)*	(.003)*	(.002)*
Indic.: High-skill firm × Exporter	-.103	.116	-.012	-.027
	(.009)*	(.058)	(.017)	(.015)
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic (excluded IVs first stage)		7.75	57.78	60.80

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator (column 1 of Table 3.23). Binary future exporter indicator represents firms that start exporting at $t+1$ or that continue exporting at $t+1$; current export status as defined in Table 3.1. Workforces on December 31st. F statistics for the joint zero effect of IVs on the first stage from Table 3.23. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.18: Hires from Exporters, Conditional on Workforce Skill Changes

	Log [1 + Hires from Exporters] (t)			
		IV	IV × Exp.	
	FE	FE (A)	FE (B)	FE, trend (C)
Predictor ($t-1$ to t per t unless noted)	(1)	(2)	(3)	(4)
Indic.: Anticip. Exporter ($t+1$)	.112	4.701	1.982	1.687
instr. in (2)-(4)	(.005)*	(1.137)*	(.207)*	(.186)*
Rel. empl. chg.: High school educ.	.002	-.001	.0006	.0008
	(.002)	(.001)	(.001)	(.001)
Rel. empl. chg.: Tertiary educ.	.004	-.0003	.003	.003
	(.001)*	(.002)	(.001)	(.001)
Rel. empl. chg.: Skilled blue-collar occ.	.005	.001	.003	.004
	(.0006)*	(.001)	(.0005)*	(.0005)*
Rel. empl. chg.: Other white-collar occ.	.007	.003	.005	.006
	(.003)	(.002)	(.003)	(.003)
Rel. empl. chg.: Techn. or supervis. occ.	.007	.004	.006	.006
	(.002)*	(.002)	(.002)*	(.002)*
Rel. empl. chg.: Prof. or manag'l. occ.	.020	.003	.013	.014
	(.002)*	(.005)	(.002)*	(.002)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic (excluded IVs first stage)		7.00	56.12	59.27

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption as well as workforce and MNE control variables as in Table 3.5; for linear sector trends in specification 4. Specifications 2, 3 and 4 use instrumented binary future exporter indicator. Workforce changes between December 31st of two consecutive years. Omitted workforce categories: Primary school education and Unskilled blue-collar occupations. Additional workforce and MNE control variables as in Table 3.17. Binary future exporter indicator represents firms that start exporting at t+1 or that continue exporting at t+1. F statistics for the joint zero effect of IVs on the first stage (full first-stage results available upon request). Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.19: Foreign Demand, Future Export-Market Participation by Region

	Export Status by Region (t+1)		
	São Paulo state	South/ SouthEast	North/NorthEast CenterWest
Instrument (t)	(1)	(2)	(3)
A: Sectoral Foreign Imports by Region, no trend (IV)			
Non-Brazil Imports in OIN (t)	-.223	.027	.002
	(.051)*	(.036)	(.017)
Non-Brazil Imports in WEU (t)	.032	-.001	-.0005
	(.010)*	(.009)	(.004)
Non-Brazil Imports in NAM (t)	-.022	-.018	.004
	(.013)	(.011)	(.005)
Observations	1,199,227	1,199,227	1,199,227
F statistic	9.83	1.55	0.58
B: Sectoral Foreign Imports × Exporter Status, no trend (IV × Exp.)			
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.037	-.040	-.010
	(.005)*	(.005)*	(.002)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	.003	-.057	-.015
	(.006)	(.005)*	(.002)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.042	.013	.008
	(.005)*	(.005)*	(.002)*
Observations	1,199,227	1,199,227	1,199,227
F statistic	42.88	59.63	26.69
C: Sectoral Foreign Imports × Exporter Status, with sector trend (IV × Exp.)			
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.037	-.041	-.011
	(.005)*	(.005)*	(.002)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	.003	-.058	-.016
	(.006)	(.005)*	(.002)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.042	.013	.008
	(.005)*	(.005)*	(.002)*
Observations	1,199,227	1,199,227	1,199,227
F statistic	44.40	60.10	26.98

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption, panel C also controlling for linear sector trends. Binary future exporter indicator represents firms in a given region that start exporting at t+1 or that continue exporting at t+1; future and current export status as defined in Table 3.1. Non-Brazilian imports in Other Industrialized countries (OIN), Western European countries (WEU), North American countries (NAM excluding Mexico), and worldwide (WW excluding Latin America and Caribbean). Additional regressors: current export status, workforce characteristics and MNE indicator as in Table 3.5. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.20: Foreign Demand and Future Export-Market Participation, Size

Instrument (t)	Export Status (t+1) (1)	Employment (t+1) (2)
A: Sectoral Foreign Imports by Region, no trend (IV)		
Non-Brazil Imports in OIN (t)	-.193	-.693
	(.059)*	(.263)*
Non-Brazil Imports in WEU (t)	.031	.007
	(.013)	(.056)
Non-Brazil Imports in NAM (t)	-.037	-.185
	(.017)	(.073)
Observations	1,199,490	1,199,490
F statistic	7.76	14.62
B: Sectoral Foreign Imports × Exporter Status, no trend (IV × Exp.)		
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.087	-.501
	(.006)*	(.029)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.069	-.321
	(.008)*	(.030)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.020	-.088
	(.007)*	(.028)*
Observations	1,199,490	1,199,490
F statistic	64.24	102.58
C: Sectoral Foreign Imports × Exporter Status, with sector trend (IV × Exp.)		
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.089	-.504
	(.006)*	(.029)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.070	-.323
	(.008)*	(.030)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.022	-.090
	(.007)*	(.027)*
Observations	1,199,490	1,199,490
F statistic	67.62	105.54

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption, panel C also controlling for linear sector trends. Binary future exporter indicator represents firms that start exporting at t+1 or that continue exporting at t+1; future and current export status as defined in Table 3.1. Non-Brazilian imports in Other Industrialized countries (OIN), Western European countries (WEU), North American countries (NAM excluding Mexico), and worldwide (WW excluding Latin America and Caribbean). Additional regressors: current export status, workforce characteristics and MNE indicator as in Table 3.5. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.21: Foreign Demand and Future Export-Market Participation at $t+2$

	Export Status (t+2)		
	Continuous	Start	Quit
Instrument (t)	(1)	(2)	(3)
A: Sectoral Foreign Imports by Region, no trend (IV)			
Non-Brazil Imports in OIN (t)	.166	.032	.066
	(.056)*	(.054)	(.052)
Non-Brazil Imports in WEU (t)	-.090	-.003	-.075
	(.012)*	(.013)	(.012)*
Non-Brazil Imports in NAM (t)	-.239	-.122	-.056
	(.017)*	(.017)*	(.015)*
Observations	1,035,386	1,035,386	1,035,386
F statistic	191.28	41.44	66.32
B: Sectoral Foreign Imports $\times$ Exporter Status, no trend (IV $\times$ Exp.)			
Non-Brazil Imports WW (t) $\times$ Cont. Exp. (t)	-.251	-.038	.0007
	(.007)*	(.005)*	(.006)
Non-Brazil Imports WW (t) $\times$ Start. Exp. (t)	-.094	-.044	-.042
	(.007)*	(.006)*	(.008)*
Non-Brazil Imports WW (t) $\times$ Quit. Exp. (t)	-.053	-.059	-.032
	(.004)*	(.007)*	(.006)*
Observations	1,035,386	1,035,386	1,035,386
F statistic	402.52	39.04	22.09
C: Sectoral Foreign Imports $\times$ Exporter Status, with sector trend (IV $\times$ Exp.)			
Non-Brazil Imports WW (t) $\times$ Cont. Exp. (t)	-.242	-.032	.010
	(.007)*	(.005)*	(.006)
Non-Brazil Imports WW (t) $\times$ Start. Exp. (t)	-.089	-.040	-.036
	(.007)*	(.006)*	(.008)*
Non-Brazil Imports WW (t) $\times$ Quit. Exp. (t)	-.047	-.055	-.025
	(.004)*	(.007)*	(.006)*
Observations	1,035,386	1,035,386	1,035,386
F statistic	373.91	31.37	20.29

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption, panel C also controlling for linear sector trends. Future exporter indicators represents firms that start exporting at t+2, continue exporting at t+2, or quit exporting at t+2; future and current export status as defined in Table 3.1. Non-Brazilian imports in Other Industrialized countries (OIN), Western European countries (WEU), North American countries (NAM excluding Mexico), and worldwide (WW excluding Latin America and Caribbean). Additional regressors: current export status, workforce characteristics and MNE indicator as in Table 3.5. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.22: Foreign Demand and Future Export-Market Participation at $t+3$

	Export Status (t+3)		
	Continuous	Start	Quit
Instrument (t)	(1)	(2)	(3)
A: Sectoral Foreign Imports by Region, no trend (IV)			
Non-Brazil Imports in OIN (t)	.527	.277	.135
	(.064)*	(.050)*	(.055)
Non-Brazil Imports in WEU (t)	-.186	-.117	-.044
	(.014)*	(.014)*	(.014)*
Non-Brazil Imports in NAM (t)	-.258	-.040	-.161
	(.022)*	(.021)	(.021)*
Observations	872,537	872,537	872,537
F statistic	209.71	87.31	81.96
B: Sectoral Foreign Imports × Exporter Status, no trend (IV × Exp.)			
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.317	-.031	-.025
	(.008)*	(.006)*	(.006)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.098	-.069	-.089
	(.007)*	(.006)*	(.008)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.064	-.057	-.085
	(.006)*	(.007)*	(.007)*
Observations	872,537	872,537	872,537
F statistic	512.75	49.53	67.15
C: Sectoral Foreign Imports × Exporter Status, with sector trend (IV × Exp.)			
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.304	-.024	-.014
	(.008)*	(.006)*	(.006)
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.091	-.065	-.083
	(.007)*	(.006)*	(.008)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.056	-.053	-.078
	(.006)*	(.007)*	(.007)*
Observations	872,537	872,537	872,537
F statistic	478.49	43.81	59.25

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption, panel C also controlling for linear sector trends. Future exporter indicators represents firms that start exporting at t+3, continue exporting at t+3, or quit exporting at t+3; future and current export status as defined in Table 3.1. Non-Brazilian imports in Other Industrialized countries (OIN), Western European countries (WEU), North American countries (NAM excluding Mexico), and worldwide (WW excluding Latin America and Caribbean). Additional regressors: current export status, workforce characteristics and MNE indicator as in Table 3.5. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

Table 3.23: Foreign Demand and Future Export-Market Participation, Conditional on Workforce Composition

	Exporter	Export Status (t+1)		
	(t+1)	Continuous	Start	Quit
Instrument (t)	(1)	(2)	(3)	(4)
A: Sectoral Foreign Imports by Region, no trend (IV)				
Non-Brazil Imports in NAM (t)	-.038	.013	-.051	-.002
	(.017)	(.011)	(.015)*	(.013)
Non-Brazil Imports in OIN (t)	-.189	-.122	-.068	.068
	(.060)*	(.039)*	(.052)	(.044)
Non-Brazil Imports in WEU (t)	.031	.005	.026	-.026
	(.013)	(.009)	(.012)	(.010)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic	7.75	3.43	6.33	3.95
B: Sectoral Foreign Imports × Exporter Status, no trend (IV × Exp.)				
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.084	-.038	-.046	.037
	(.006)*	(.006)*	(.004)*	(.005)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.063	-.009	-.054	.032
	(.008)*	(.005)	(.006)*	(.007)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.022	-.008	-.015	-.018
	(.007)*	(.003)*	(.006)	(.006)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic	57.78	13.60	49.33	29.53
C: Sectoral Foreign Imports × Exporter Status, with sector trend (IV × Exp.)				
Non-Brazil Imports WW (t) × Cont. Exp. (t)	-.085	-.039	-.047	.041
	(.006)*	(.006)*	(.004)*	(.005)*
Non-Brazil Imports WW (t) × Start. Exp. (t)	-.064	-.010	-.054	.034
	(.008)*	(.005)	(.006)*	(.007)*
Non-Brazil Imports WW (t) × Quit. Exp. (t)	-.023	-.009	-.015	-.016
	(.007)*	(.003)*	(.006)	(.006)*
Observations	1,199,490	1,199,490	1,199,490	1,199,490
F statistic	60.8	15.24	49.39	31.69

Sources: SECEX and RAIS 1990-2001 (t: 1992-2000), manufacturing firms (subsectors IBGE 2-13). *Notes*: Linear regressions, controlling for firm fixed effects, sector and year effects, and sectoral absorption, panel C also controlling for linear sector trends. Binary future exporter indicator represents firms that start exporting at t+1 or that continue exporting at t+1; future and current export status as defined in Table 3.1. Non-Brazilian imports in Other Industrialized countries (OIN), Western European countries (WEU), North American countries (NAM excluding Mexico), and worldwide (WW excluding Latin America and Caribbean). Additional regressors: current export status, workforce characteristics, MNE indicator and absorption as in Table 3.17. F statistics for the joint zero effect of the IVs. Robust standard errors, clustered at the firm level, in parentheses; asterisk marks significance at 1-percent level.

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