

Worker Mobility in a Global Labor Market: Evidence from the United Arab Emirates*

Suresh Naidu
Columbia

Yaw Nyarko
NYU

Shing-Yi Wang
Wharton

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Abstract

Globally, migrant workers are often in visa arrangements that tie them to a particular employer, restricting job-to-job transitions and potentially lowering earnings and efficiency. In 2011, the United Arab Emirates reformed their labor market system to allow any employer to renew a migrant's visa upon contract expiration without written permission from the initial employer. To examine the impact of the reform on worker outcomes, we combine monthly administrative worker payroll data with the universe of Ministry of Labor contracts data. We find that monthly earnings increase by over 10% following a contract expiration after the reform relative to before. In addition, we observe an increase in employer transitions and a fall in exiting to the home country. Our results are similar when correcting for potential bias from selective exits from the data, and remain present in various subsamples and with the inclusion of various controls. Labor market competition can be restrained by contracts, and the long-term visa restrictions on migrant worker contracts may be a barrier to realizing the full wage gains of international migration.

Keywords: Migration, Job Mobility, Labor Markets, Labor Contracts, Middle East.

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1 Introduction

Global labor market mobility has the possibility of dramatically raising world welfare and equalizing the global distribution of income (Clemens 2011). However, many countries that admit large quantities of foreign workers also impose a variety of legal restrictions on migrants. As a result, long-term migrant workers are commonly not only excluded from political rights and public goods, but also agree to labor market contracts that tie them to particular employers. Laws stipulate that employers must agree to and arrange the transfer of visas in order for an immigrant worker to transition between employers. These visas substantially reduce the ex-post mobility of migrants, even as they incentivize firms to recruit migrants ex-ante. The resulting labor market restrictions may also reduce ex-post competition for low-skilled migrant labor, lowering worker mobility and earnings and reducing the overall efficiency of the labor market.

This paper provides estimates of the impact of such visa restrictions in the context of a reform in the United Arab Emirates (UAE). Prior to this reform, migrant workers in the UAE were under the *kafala* (sponsorship) system that tied each of them to one employer for the duration of their multi-year contracts. When the employment contract expired, the employer was given the option of renewing the contract or giving a No-Objection Certificate (NOC) to allow the worker to transition to another firm. If the employer did not renew the contract and did not provide the NOC, workers had to return to their host countries for at least 6 months. In January 2011, the UAE government implemented a policy reform that allowed migrant workers to transition to new employers after their contracts expired without approval from their previous employer. This translates into more competitive labor markets for both workers and the incumbent employers when contracts are renegotiated.

The UAE, with an 89% migrant share of population, is an ideal context to study migration. Migration into the Gulf region in general increased substantially in the past decades. In the UAE specifically, the number of migrants jumped from 1.3 million in 1990 to 7.8 million in 2013 (UN 2013). Accompanying the surge in migrant flows to the area, there has been a great deal of international concern about the power that employers have over migrant workers. Human Rights Watch (2013) illustrates this concern in writing, based on anecdotal evidence, “Migrant workers in these countries typically have their passports confiscated and are forced to work under the highly exploitative kafala system of sponsorship-based employment, which prevents them from leaving employers. Employers are rarely, if ever, prosecuted for violations

of labor law. As a result, migrant workers in the Gulf frequently experience hazardous working conditions, long hours, unpaid wages, and cramped and unsanitary housing.” However, there is little quantitative evidence on migrant labor market conditions in these countries, nor any attempts to evaluate the impact of policy reforms that have been undertaken in Gulf countries in recent years.

The policy debates over the implications of employer-specific visas extend well beyond the Gulf. In the United States, H2-B visas, for example, also tie low-skilled seasonal workers to particular employers, and do not allow immediate job-to-job transitions after a contract expires. Immigration reformers have asked for a clause allowing a 6-month interval whereby workers can search for a new employer without having to return to their source country. These types of visas are often criticized by advocates as restraining labor market competition and lowering migrant wages.¹

In addition to the policy importance of these issues, the structure of these non-competitive labor markets is also of interest to economists. While there exists a large theoretical and empirical literature studying imperfect competition and agreements in restraint of trade in the industrial organization of product markets (Bresnahan 1989, Porter 2005), there is little direct evidence on imperfect competition in labor markets and the ways in which government policies contribute to or respond to these labor market imperfections (Manning 2003). The existing literature on labor market contracts focuses on how they may be able to solve problems of insurance and incentive provision.² However, there is much less work on the potential that labor market contracts serve as a means of restraining competition (Aghion and Bolton 1987) or limiting search (Naidu 2010, Stevens 2004).

To our knowledge, our paper is the first to provide the causal estimates of how workers’ outcomes change following the reform of a system that tied migrant workers to their initial employers. Our empirical strategy uses the timing of the reform together with individual-level variation in the expiration dates of labor contracts to estimate the impact of the easing of mobility restrictions on employer transitions, wages and country exits. To implement this strategy, we match two high quality administrative data sets. The first data set is UAE Ministry of Labor data on the terms of the contracts signed between workers and firms. The second data set is from a large, private payroll processing firm that provides monthly payroll disbursement for migrant workers employed at thousands of firms in the UAE.

¹e.g. <http://www.epi.org/publication/2b-employers-congressional-allies-fighting>

²See Gibbons (1998) for a review.

Our results indicate that worker outcomes improve substantially following the reform. Real earnings following a contract expiration increase by over 10 percent. The rate of employer transition at the end of a contract more than doubles. The probability of leaving the UAE at the end of a contract also falls by about 4 percentage points.

2 Other Related Literature

This paper contributes to the growing literature on the outcomes of international migrants.³ Clemens (2012) studies migrant labor to the Gulf, and focuses on the effects of migration on Indian workers. Using Indian workers whose contracts were cancelled due to the economic downturn as a control group, Clemens finds that migrants and their families are substantially better off on a number of margins. Relatedly, Clemens (2013) studies the effects of randomized visa lotteries on software developers, and finds that ex-ante identical workers have substantial earnings differences depending on their migration destinations. McKenzie, Theoharides, and Yang (2013) examine the effects of host country GDP on migrant labor supply, and argue that binding wage regulation on migrants amplifies the effect of output shocks on migrant employment.

Our work also contributes to a large literature in labor and macro economics that has emphasized the role of search frictions and job-to-job transitions (Burdett and Mortensen 1998) in explaining wages and employment.⁴ Having the option to change jobs is potentially important for wage growth. Hornstein, Krusell, and Violante (2011), Manning (2003), and Cahuc, Postel-Vinay and Robin (2006), while methodologically very diverse, all suggest that offers received while on the job are important for explaining wage variation. However, precise estimates of the effect of job offers and labor mobility on individual wages are lacking, as are estimates of the consequences of changes in the availability of outside job offers.

In addition, the bulk of the literature on job mobility examines formal labor markets in advanced economies, yet their importance in developing countries and immigrant labor markets is likely even greater, given lack of formal information sharing or institutionalized wage setting. In developing countries, bonded labor arrangements, where workers are tied to particular employers for long periods of time, have been studied both theoretically and

³Much of the literature on international migration has focused on remittances. Nyarko, Wang and Thomas (2014) use the same data sets as we use in this paper to estimate the impact of earnings fluctuations on remittances in the UAE. See Yang (2011) for a review of the remittances literature.

⁴See Rogerson and Shimer (2005) for a review.

empirically (Bardhan 1983, von Lilienfeld-Toal and Mookherjee 2010).⁵

3 Institutional Background

Migrant workers make up 96% of the workforce in the United Arab Emirates (Forstenlechner and Rutledge 2011). Employers in the UAE recruit workers from around the world, although the bulk of the workers are from South Asia. Migrants are recruited through source country labor brokers, specialized UAE-based recruiting firms and by UAE firms directly. A signed contract and a passport (a non-trivial requirement in some source countries) are required to obtain a visa. Formally, employers and their contractors are forbidden from charging recruitment fees to workers, but it is unclear if this is enforced. Employers generally cover lodging, health insurance, and travel costs (conditional on contract fulfillment). Workers are entitled to 1 month of leave per year, and many wait several years to take 2-3 months contiguously. Workers are housed in large labor camps, which are often span multiple employers. Employers pay fixed fees to the government for labor cards for each migrant worker under contract, which cover the cost of catching and deporting workers should they abscond from their job. Fees depend on the composition of the workforce of the firm, with skill-intensive and high local-employment firms able to purchase cheaper labor cards. The government regulates contract lengths by the types of visas granted. Before 2011, contracts tended to be for three years; since 2011, the typical contract is two years.

The contracts and visas are regulated under the *kafala* system, which is widely used in the Gulf countries (Longva 1999). Traditionally under this system, guarantors were used to enforce contracts where the individual guarantor (*kafeel*) was liable for the credit, safety, and good conduct of the debtor (*kafila*). In modern Gulf countries, this has become an elaborate set of regulations on migrant labor, tethering workers to their employers via contracts and visas, and giving employers a substantial amount of power.⁶

Under the *kafala* system, workers fired by their employers promptly lose their visa status and are required by law to leave the country soon after the employer terminates the contract.⁷

⁵A recent literature on non-compete clauses in labor contracts in the U.S. is also related (Marx 2011). Non-compete clauses restrict the ability of employees to work for firms that compete in the same sector, and have become more frequently used in recent years, particularly in high-tech, high-skill sectors with substantial firm-specific knowledge.

⁶One ethnography suggests that “Migrants ..assume that any Emirati is capable of deporting any migrant should they choose.” (Bristol-Rhys 2012, pg 68).

⁷While the numbers of migrants in the UAE without a valid visa is unknown, it is thought to be quite small as police will regularly stop workers and ask them for their papers.

Workers have the right, however, to appeal the firm's decision to fire them to the government under certain circumstances, such as if wages are owed. If workers wish to end their contract early, they must leave immediately and bear the travel costs, which would otherwise be borne by the company.

Most importantly, under the pre-2011 system, workers needed a No-Objection Certificate (NOC) from their existing employer in order to change employers either during an existing contract or after the contract expired. Anecdotal evidence suggests that some employers required workers to pay substantial fees in exchange for the NOC. Without an NOC at contract expiration, workers had to either return to their source country for at least 6 months (a visa ban) before re-entering or renew with their current employer. This feature of the *kafala* system has fallen under widespread criticism. In Salem (2010), a worker's statement illustrates some issues related to the NOC requirement: "At the beginning, when I gave my one-month notice to move to another job, my boss said OK, but at the end of the month he said no, he needs me, it is not his problem I didn't want to continue in that job."

Evidence that these restrictions are binding can also be seen from online forums where expatriate workers trade advice for dealing with visa issues in the UAE.⁸ Numerous posts are from workers asking for legal advice and complaining about the 6-month and other bans imposed if a worker leaves a contract without an NOC. While we have not done a systematic analysis, some quotes are revealing. For example, one user with the screen name "Exchange job" wrote in January 2011, "I am working in an exchange for three months. My salary is very low. Now I want to switch the job but my contract period is of three years. I also want to pay the ban charges if there is a ban. kindly guide me if it will be possible for me to change the job and as well as to pay the ban fee." Similarly, "Jahangir" wrote (typos in original) "Respected Sir, I am very new in uae - dubai my company head office is in dubai and having one branch in ksa [Saudi Arabia] and i was appointed for ksa but company want to stay in dubai on same salary and i already resign my past job, and write now my company makes my work permit but i don't want to work with this on same salary in dubai so let me know what is the way to change the job in uae."⁹ While these workers are obviously among the more educated, and it is difficult to validate the impressionistic evidence from the Internet, it does suggest that the legal contract restrictions are enforced and are seen as a constraint by workers.

These kinds of restrictions are not new. Historically, legal and social impediments to

⁸For example: <http://www.dubaiforums.com/dubai-visa/> or <http://www.desertspeak.com/>

⁹<http://www.desertspeak.com/viewtopic.php?t=1911> Accessed Feb. 4 2014.

migrant worker mobility have been commonplace, particularly in non-democratic environments. British Master and Servant law¹⁰ governed migrant indentured labor contracts throughout the Empire.¹¹ The Gulf countries, then known as the Trucial states, were recipients of Indian migrant labor under this system beginning in the early 20th century. No-objection cards were issued by the British Political Agent to merchants in the Gulf as early as the 1930s (Secombe and Lawless 1986). While the increase in migrant labor has been recent, the institutional foundation for the *kafala* system was laid well before formal codification in the 1970s.

3.1 Labor Mobility Reform

Discussions of reforming the NOC requirements in the UAE followed when neighboring Bahrain’s reformed a similar requirement in August 2009. The UAE government formally announced the reform in December 2010 and it took effect in January 2011 (Cabinet Resolution number 25 of 2010). The UAE Minister of Labour, Saqr Ghobash, stated that the change was intended to “improve the labour market and... protect the rights and benefits of the labourers as well as their employers” (Salem, 2010).¹²

The reform had a number of components. Most important for this paper is the reform that abolished the NOC requirement when a contract expired. Starting in January 2011, workers could switch employers without the NOC from their previous employers *after* their current contracts had expired. Other components of the reform included some changes to visa fees for skilled workers and shortening the duration of contracts from 3 years to 2 years, as well as a lowering of the age of eligible workers from 65 to 60. The change in the duration of contracts only applied to new contracts beginning on or after January 2011 and did not shorten existing 3 year contracts.

Officials acknowledged the implications of the reform for labor market competition in the UAE, with Minister Ghobash saying, “Giving the private sector more freedom of movement will have automatic impact on employers by the way of preserving their interests through creating many options for recruiting skillful workers as per the supply-demand equation... These measures [are] expected to play a major role in advancing efforts towards creating an efficient labour market and sharpening competitiveness and transformation towards a knowledge-driven

¹⁰Naidu (2010) and Naidu and Yuchtman (2013) study the effects of Master and Servant law, which criminalized employer poaching and unauthorized quits, in the U.S. South and England, respectively.

¹¹See Galenson (1984), for a historical account of indentured migration in the British empire.

¹²Discussion of reform in the region began in 2008 with the Abu Dhabi Dialogue, which brought major recipient countries together with major source countries together to discuss contract migrant issues.

economy” (WAM, 2010).

4 Conceptual Framework

Given the large wage differences between the UAE and many other countries, it is not surprising that there is a large supply of foreign workers who are willing to migrate to and work in the UAE. One possible implication of the large supply of workers with very low reservation wages is that firms do not need to respond to labor market regulations that govern within-country employer transitions; firms may simply replace workers with new entrants rather than respond to the within-country labor market competition that may increase worker mobility and drive up wages to retain or attract workers.

Despite the important role of the global labor market in the UAE, we find evidence that decreasing the barriers to worker mobility within the country has a large positive effect on job transitions and earnings, and a negative effect on exiting the UAE. A simple explanation for this is that firms face substantial costs in recruiting labor in source countries. In Appendix B, we provide a simple theoretical framework with two firms where, in equilibrium, one firm invests in international recruitment while the other firm tries to poach the first firm’s workers after contracts have expired. In this equilibrium, reducing the cost of employer transitions increases the set of options available to the worker, resulting in higher wages, higher probabilities of transition, and a strict preference for remaining in the host country.

5 Data

5.1 Salary Disbursal Data

The data on wage disbursements of migrant workers are from a company in the UAE called UAE Exchange. The company provides payroll disbursement services to other firms in addition to offering other financial transactions such as remittances. This firm pays wages to approximately 10-15% of the total migrant workforce in the country. Many firms, including quite large and small ones, use a payroll processing firm in order to adhere to the wage protection system, which was implemented by the government in 2009 and 2010 (with larger firms required to implement the system earlier) to protect workers by creating an electronic records of wage payments.

We have monthly payroll disbursements for the period from January 2009 to October 2012. Recall that the reform went into effect in January 2011, so the data span both sides of the

reform. The entire sample of earnings disbursements includes 427,265 unique individuals working in 20,366 firms. In the UAE, salaries are paid out on a monthly basis.¹³ There are on average 17.6 monthly salary observations per worker. The key advantage of the data is that they are high-frequency administrative records of actual earnings transferred to workers, and should not suffer from issues of recall error that are common in survey based questions on earnings.

The observed earnings may differ from total compensation for several reasons. First, many migrant workers are compensated with several in-kind benefits, including housing and food. Second, workers may supplement their earnings in their primary jobs with informal work. This is unlikely to be as common in the UAE as in other contexts because it is illegal for migrant workers to receive compensation for work outside of the one employer associated with their visas.

Because the data are from administrative payroll processing records, the other information available for each worker is limited. The data include firm identifiers and some demographic characteristics of workers, including their country of origin, age and gender. It is important to note that the data set does not include any information on actual hours worked in each month.

5.2 Ministry of Labor Administrative Contracts Data

In addition to the salary disbursement data, we also received data on migrant workers' labor contracts from the UAE Ministry of Labor (MOL). Two key variables in this data set are the start and end dates of the labor contract signed between a migrant worker and a firm. This allows us to identify the exact month that a worker's labor contract will expire. Furthermore, the MOL data allows us to link individuals in the UAE Exchange payroll data as they move across firms. Not all firms in the UAE use UAE Exchange for payroll processing so we do not observe all firm-to-firm transitions of workers in the UAE Exchange data alone.

Thus, a key benefit of the MOL data is that it allows us to identify whether a worker that disappears from the UAE Exchange dataset switches to another firm that does not use UAE Exchange for payroll processing or leaves the MOL data entirely. We characterize those migrants who leave the MOL data as having exited the UAE, and this is true in the vast majority cases. However, a fraction of migrant workers who leave the MOL data remain in the UAE. This reflects the fact that the MOL data only includes migrant labor contracts that fall under the jurisdiction of the Ministry of Labor. Domestic workers fall under the jurisdiction

¹³In the less than 5% of observations for which multiple payments are made to an individual in a single month, we aggregate those into the total earned in that month.

of the Ministry of the Interior, as do any workers that work in freezone areas of the UAE.¹⁴ A comparison of the MOL data to UN population figures for migrant workers in the UAE in 2012 suggests that the MOL data covers approximately 80% of all migrant workers in the country.

In addition to the start and end dates of contracts, the MOL data also includes other details of each labor contract, including contracted hours, earnings, and total compensation.¹⁵ It would be inaccurate to assume that contracted earnings are equivalent to actual earnings; a comparison of the MOL data and the UAE Exchange data suggests that the contracted earnings are a lower-bound on workers' earnings. Most workers earn more than what is stated in their contract and the amount fluctuates considerably from month to month. The data set also contains all of the demographic characteristics included in the UAE Exchange data as well as some additional ones such as religion and educational attainment.

Both the MOL contracts and the UAE Exchange payroll data sets include a unique government-issued identifier that is called the labor card ID number. Thus, the matching between the two data sets is fairly straightforward and outlined in more detail in Appendix C.1. We are able to match 81% of the observations in the payroll data with their corresponding contracts in the MOL data, and Appendix Figure A.1 shows that the earnings densities between the matched and unmatched payroll observations are virtually identical. Appendix Figure A.2 shows the densities in the contract salary for individuals that match with the payroll data and the rest of the migrant sample that is not in the UAE Exchange payroll data. The distribution is extremely similar for the lower end of the distribution and the comparison suggests that the payroll data is more oriented towards the median and lower end of the salary distribution of migrants and under-samples migrant workers at the top end of the salary distribution.

6 Descriptive Statistics

6.1 Administrative Contracts Data

We begin by showing summary statistics from the MOL contracts data, which provides the universe of labor contracts under the jurisdiction of the MOL. Figure 1 shows the total number of employer transitions that occur at the end of a contract by the expiration date of the

¹⁴Freezones are industrial parks throughout the UAE that provide special incentives for foreign investments, such as tax breaks and less restrictions on foreign ownership. The bulk of the freezones are in the vicinity of the cities of Dubai and Sharjah.

¹⁵Total compensation includes the value specified in the contract for housing, food and transportation.

contract.¹⁶ The vertical line indicates December 2010, the date that the reform was announced, which is also the month immediately prior to the implementation of the reform. This figure provides immediate evidence that employer transitions increased following the reform.

Figure 2 shows the real change in the compensation stipulated in the new contract compared to the previous contract by the expiration date of the previous contract. Compensation includes both earnings and the value of employer-provided housing and transportation. This includes both employer transitions and individuals who re-sign contracts with their previous employers. We see a substantial increase the growth rate of compensation following the reform.¹⁷

Both of the figures provide suggestive evidence that the reform had an immediate effect on mobility and earnings for individuals whose contracts were expiring around the time of the reform. Furthermore, the magnitude of the effects are quite large.

6.2 Salary Disbursal and Administrative Contracts Merged Data

Table 1 presents the summary statistics for the sample used in our estimation. The first three column shows the mean, standard deviation and number of observations for the person-month observed present in the data in the months prior to the reform over the period December 2008 to December 2010. The last three columns display the same statistics for the post-reform months over the period January 2011 to October 2012.

The first three rows present our main outcomes of interest. Log monthly earnings is the logarithm of the real monthly earnings disbursal that the worker received.¹⁸ The average log earnings is a little over 7; this corresponds to about 1100 dirham or USD\$300. This is the actual earnings disbursal reported by the payroll-processing firm and does not include the value of in-kind benefits. A simple pre-post comparison shows a small increase in average real earnings following the reform.

Employer change is equals 100 if the individual changed firms within the past 3 months, and zero otherwise.¹⁹ Prior to the reform, the rates of employer change are quite low at 0.2% per month (or 2.4% per year), which translates to only 2 workers per thousand who changed employers each month. This low rate should not be that surprising in the pre-reform period

¹⁶This includes only employer transitions that occur within three months of the contract expiration.

¹⁷Note that the negative gains in compensation that are observed prior to the reform are driven by the adjustment for inflation. The nominal changes in compensation over the full period shown are positive.

¹⁸We convert nominal earnings to real earnings using the monthly consumer price index published by the UAE National Bureau of Statistics. Earnings are in 2007 dirham.

¹⁹See the Data Appendix for more information on the construction of this and other variables.

given that workers could not freely change employers either during or at the end of a contract. The average rate of employer change more than triples after the reform.

Exiting the UAE is an indicator variable that equals 100 if the individual leaves the sample for at least 6 months, and zero otherwise. There is some noise in this measure as individuals may move within the UAE but out of the jurisdiction of the MOL to a freezone area and would be counted as exiting the UAE. The rate of exiting prior to the reform was 4.8% per month; after the reform, this falls to 3.3% per month.

Stayer is an indicator that is defined as an individual that does not change employers at all during the sample period. The vast majority of workers do not change employers at all during the sample period. The majority of the workers in our sample work in construction.²⁰ The average age of workers is in the mid-thirties and the vast majority of them are male. Educated is an indicator variable that equals one if the person has higher than intermediate education. The pool of educated workers increases fairly substantially after the reform. About 40% of the workers are Muslim. Over 60% of the migrant workers in our sample work in the neighboring cities of Dubai and Sharjah. Indians represent the largest nationality among migrants in the UAE and are about half of our sample.

We do not directly observe hours worked per month in either of the data sets. However, we do observe actual earnings disbursements each month and the earnings and hours stipulated in the contract. We construct two measures of hours worked each month based on the assumption that variation in earnings month-to-month is a reflection of variation in hours.²¹ The upper bound of hours worked per month is constructed based on the assumption that overtime pay equals 1.25 times the standard hourly wage and the lower bound calculation of hours worked assumes that overtime is paid at a rate of 1.5 times the standard hourly wage.²² The average number of hours worked per month falls from around 260 in the pre-reform period to 240 in the post-reform period.

²⁰Each firm is coded by at least two research assistants. The coding is based on the name of the firm if it contains explicit industry information or by researching the firm. If the two research assistants coded the firm differently, then another coding was done by a third research assistant. We thank Marton Pono, Mengxing Lin, Zhiwen Xie and Cheng Xu for their assistance in industry coding.

²¹That hours was the primary source of earnings variation was confirmed by conversations with UAE Exchange officials.

²²UAE law stipulates rates of overtime between 1.25 to 1.5 depending on the time of the day and of the week that the extra hours takes place.

7 Estimation Strategy and Results

7.1 Overview of Strategy

The estimation strategy is analogous to a differences-in-differences framework. We examine worker outcomes before and after the implementation of the reform in January 2011. The other comparison that we exploit is looking at outcomes before and after the worker's contract expires.

Given that we have less than four years of data on salary disbursement and that the standard contract length for contracts beginning prior to 2011 was 3 years, we have very few individuals who have contracts expiring both before and after the implementation of the reform. Thus, we might think of individuals whose contracts expire after the reform as our treatment group and individuals whose contracts expire before the reform as our comparison group.

One concern is that the types of individuals entering the UAE changes over time, and the pool of individuals with contracts expiring prior to the reform is different from the pool of individuals with contracts expiring after the reform. However, it is important to note that any changes in the selection of individuals cannot be driven by an endogenous response to the reform itself. This is because individuals whose contracts expired in 2010 versus in 2011 have contracts that began in 2007 and 2008, respectively, and this precedes serious discussion of such reforms in the UAE. Our specification also includes individual fixed effects which should remove any time-invariant differences across the groups.

Our analysis focuses on 7 periods per individual. We look at the three months leading up to an individual's contract expiration, the period of the contract expiration, and the three months following the initial contract expiration. It can vary by individual whether the month of contract expiration itself can be considered post-contract expiration because a person's contract may expire at the beginning or end of a month and they may or may not have the opportunity to transition within the expiration month itself. There are a few reasons that we adopt a strategy of using 3 leads and lags around the time of the contract expiration. First, it allows us to examine whether there are pre-expiration trends that suggest that the date of contract expiration is not a clean event. Second, the three lags following the contract expiration can be important as many individuals return to their home country for a vacation for 1 to 2 months following a contract expiration.²³ Thus, any post-contract expiration effects may not show up in just one month.

²³This is true regardless of whether they change employers or not.

7.2 Specification and Results

Corresponding to the strategy described above, we begin our analysis with the following specification:

$$y_{it} = \sum_{k=-3}^3 \gamma_k^{Post2011} D_{it+k} + \sum_{k=-3}^3 \gamma_k^{Pre2011} D_{it+k} + \delta_i + \delta_t + \epsilon_{it} \quad (1)$$

where y_{it} denotes the outcomes of interest: log earnings, employer transitions and exiting the UAE. The variable D is a dummy variable that indicates the period relative to the contract expiration date. The sample is restricted to the 7 contiguous months centered around a contract expiration, so $k = -3$ corresponds to 3 periods before the contract expires and $k = 3$ corresponds to 3 periods after the previous contract expired. Thus, the coefficients $\gamma_k^{Pre2011}$ provide the effect of the contract expiration prior to the 2011 reform, and the coefficients $\gamma_k^{Post2011}$ provide the effect of the contract expiration after the 2011 reform. We are most interested in whether the effects of contracts expirations after the reform are different from the effects prior to the reform, and that is given by the estimates of $\gamma^{Post2011} - \gamma^{Pre2011}$. We also include year-month fixed effects and individual fixed effects. The standard errors are robust and clustered by individual.

Given the large number of coefficients, we show graphical plots of $\gamma_k^{Post2011}$ and $\gamma_k^{Pre2011}$ from estimates of equation 1. Figure 3 displays the coefficients together with 95% confidence intervals when the dependent variable is log earnings. The omitted category is the month immediately prior to the contract expiration. The figure shows that prior to the reform, log earnings did not increase following a contract expiration. This may not be surprising given that in this period, employers had the right to retain workers by not providing a NOC. In contrast, after the reform, we see a significant increase in log earnings that begins immediately after the contract expires. In addition, there are no significant post-reform effects in the periods prior to the expiration.

Figure 4 shows the coefficients from equation 1 where the dependent variable is employer transitions. In both the pre-reform and post-reform period, the pre-contract trends show no employer transitions in the three months prior to the contract expiration. There is a significant increase in the probability of employer transitions in the pre-reform period. In the post-reform period, there is a significantly larger probability of employer transitions relative to the pre-reform period that occurs immediately in the month of expiration but then declines 3 months after the expiration.

Finally, Figure 5 shows the same estimates where the dependent variable is whether the individual exits from the UAE. Consistent with the options available to individuals prior to the reform, we see an increase in exits following a contract expiration on average, but this effect is significantly attenuated following the reform. This suggests that workers were less likely to return to their home countries following the expiration of their contracts after the reform than before. These results are consistent with the idea that workers are better off in the UAE with the presence of additional work opportunities and/or higher wages.

While the figures provide compelling evidence, we formally present these results in a regression framework and estimate the following specification:

$$y_{it} = \sum_{k=0}^3 \gamma_k^{Post2011} D_{it+k} + \sum_{k=0}^3 \gamma_k^{Pre2011} D_{it+k} + \delta_i + \delta_t + \epsilon_{it} \quad (2)$$

The key difference from equation 1 is that we omit the leads to contract expiration ($-3 \leq k < 0$), so the coefficient estimates are relative to all three months prior to expiration. Given that the estimates of other leads were generally not significantly different from the period prior to expiration, these estimates are quantitatively very similar but parsimonious enough to display in tables. The main hypothesis to be tested is whether $\sum_{k=1}^3 \gamma_k^{Post2011} - \sum_{k=1}^3 \gamma_k^{Pre2011} = 0$ for earnings, employer transitions, and exits from the UAE. This is the test of whether outcomes are different following a contract expiration after the reform as compared with before the reform. Given the variation in exactly when in the month contracts expire, we are also interested in testing $\gamma_0^{Post2011} - \gamma_0^{Pre2011} = 0$ which we show in the third row of the bottom panel, although this is not our main parameter of interest.

In addition to the basic specification given in equation 2, we also include a number of controls in order to eliminate possible confounds in our identification strategy. We include polynomials in the time between the date that the current contract expires and the reform, separately for before and after the reform. This is in order to control for possible changes in the wage-contract tenure profile owing to the reform, or, albeit less likely, endogenous transitions and exits as a function of contract tenure.

We next include a vector of worker characteristics (age, Indian citizenship, education) interacted with year-quarter fixed effects. This allows for time-varying effects of observable differences in the characteristics of individuals whose contracts are expiring at different times. We also include an indicator for whether the initial job was in construction interacted with year-quarter fixed effects, as Table 1 suggested substantial changes over time in the share of

workers in construction.

Finally, we include three months of lagged earnings, as well as their interactions with *Post2011*, in order to control for autocorrelation in earnings, as well as endogenous transitions owing to past earnings.²⁴ For example, employers may try to terminate worker contracts prior to the expiration date following an unusually low period of labor demand, which would show up as low earnings immediately prior to a separation.

In addition to the various control variables, we also examine the results when we restrict the sample to workers with earnings observations both before and after the reform. This ensures that the estimates are not driven by changes in the composition of new entrants over time. Finally, we examine a sub-sample where we discard the data in the first and last calendar quarter of the sample, together with the quarter immediately preceding and immediately following the reform (quarter 4 of 2010 and quarter 1 of 2011). Dropping the first and last quarter addresses the concern that there is a selection problem for these periods; for example, not all firms may have paid out their wages for October 2012 when the data were obtained for us. Dropping the quarters immediately around the reform addresses potential issues that the timing of announcement and implementation were in response to labor market conditions in those particular months.

Table 2 shows the estimates $\gamma_k^{Post2011}$ and $\gamma_k^{Pre2011}$ from equation 2. Column 1 shows results from the specification with no controls. The first 4 coefficients are $\gamma_3^{Post2011} \dots \gamma_0^{Post2011}$, while the next four are $\gamma_3^{Pre2011} \dots \gamma_0^{Pre2011}$. Clearly, there is a differential increase in earnings following a contract expiration after the labor reform. This occurs both in the month of expiration as well as in the 3 months afterwards. The top row of the bottom panel of the table shows the pooled effect of all three months after the contract expiration interacted with the post-reform dummy, minus the pooled effect of all three months after the contract expiration interacted with the pre-reform indicator.²⁵ The standard error is reported in the row below. The third row in the bottom panel shows the differential effect on the month of expiration, and this is always positive and significant, as well as always smaller than the average effect in the subsequent 3 months, consistent with substantial heterogeneity in exactly when in the month contracts expire.

The magnitudes of the effects are substantial. We find an 11% effect on real earnings, that is, monthly earnings grow by about 11% in the 3 months following a contract expiration

²⁴Appendix Table A.1 shows the earnings results are also robust to the inclusion of firm fixed effects.

²⁵Note the pooled average effect is given by $\sum_{k=1}^3 \frac{\gamma_k^{Post2011} - \gamma_k^{Pre2011}}{3}$.

after the reform, with an increase of 1.4% on the month of contract expiration. Column 2 includes polynomials in time to reform, and column 3 includes both the time to reform polynomials as well as the time-varying effects of individual characteristics. The coefficients are very similar across specifications, with a 1.3 to 1.5% additional increase in earnings in the month of a contract expiration following the reform, and a mean increase of 11% in the 3 months following a contract expiration post 2011. The coefficient magnitudes change somewhat when autocorrelation in earnings is accounted for, but there is still a 3% increase in the month of a contract expiration post-reform, and an additional 5.3% increase in earnings over the next 3 months.²⁶ These estimates are all significant at the 1% level.

Columns 5 and 6 correspond with columns 1 and 3, respectively, except the sample is restricted to workers with at least one earnings observation before and after the reform. The coefficients on the month of contract expiration post-reform increase to around 5%, while the mean earnings increase over the subsequent 3 months is roughly 9%. Columns 7 and 8 estimate the same specifications, but now the sample omits the first and last quarter of the sample and the two quarters surrounding the reform. The coefficients are very similar to columns 1 and 3, but slightly larger in magnitude. The estimates with the sub-samples remain significant at the 1% level.

One possible concern is that we are only observing earnings, and not wages. Thus it could be that the estimated earnings increase is coming from an increase in hours worked, rather than an increase in wages. While we do not observe actual hours worked each month, we construct upper and lower bounds on hours worked using data from the MOL on the contracted hours and contracted wages. Table 3 presents these results and confirms that hours did not increase following the reform. If anything, there is some evidence of a decline in hours, but this is imprecise and sensitive to specification.

Table 4 shows the same specifications and samples for the employer transition variable. Recall that from Table 1 the level of employer transitions is quite low, and so the magnitude of the coefficients is substantial relative to the base rate of transitions. Without controls, we see a 0.48 percentage point increase in the probability of an employer transition during the month of a contract expiration, with an extra 0.67 percentage point increase per month on average over the next 3 months. These estimates are significant at the 1% level. Looking at the individual

²⁶There is low autocorrelation in earnings (with the first and largest lag having a coefficient of 0.027 pre-reform, and 0.03 following the reform). The low autocorrelation and relatively long panel suggests that Nickell bias is not likely to be a problem in this specification, despite a lagged dependent variable.

lags, we can see that the postreform coefficient is smallest in the last lag, consistent with the relatively short window workers have to find a new employer.

When the tenure polynomials are included, the coefficients are virtually identical. When both time to reform polynomials and the time-varying effects of worker characteristics are included, the effect during the month of expiration increases and the probability of a transition over the next 3 months are both lower by roughly 50%, but still significant at the 1% level. The lower magnitude of the effect, where the additional post-reform probability of a transition is 0.26 percentage points, persists with the inclusion of lagged earnings.

In columns 5 and 6, where the sample is restricted to observations with earnings observed both before and after the reform, the immediate effect of a contract expiration after the reform is a small fall in the probability of a transition, but this is offset by a increased probability of a transition 1 and 2 months after the contract expired. In columns 7 and 8, where the sample excludes months at the beginning and end of the sample as well as near the reform period, we obtain coefficients quite similar to columns 1 and 3. To put these coefficients into perspective, even the smaller coefficients represents a doubling of the base rate of employer transitions.

Finally Table 5 shows estimates of $\gamma^{Post2011}$ and $\gamma^{Pre2011}$ for exits from the UAE and has the same structure as Tables 2 and 4. The results show both consistent positive effects of contract expirations on the probability of exit on average (rows 5-8), but significant reductions in this probability following the reform (rows 1-4). These estimates are all significant at the 1% level. The effects are apparent in the month of the contract expiration, but become larger in the subsequent months. The one set of results with substantially different coefficients is in the trimmed sample with controls, where the negative effect of the reform on the probability of leaving the UAE following a contract expiration falls by about 50%, but is still significant.

Overall, the results suggest that the labor reform led to an improvement in the outcomes of migrant workers in the UAE. Granting them the ability to switch jobs at the end of a multi-year contract without needing approval from their previous employers increased employer transitions and earnings and decreased the likelihood of leaving the UAE for at least 6 months.

7.3 Accounting for Selection: Imputed Outcomes

One important concern is that the selection induced by exits from the formal UAE labor market could be significantly biasing the results on earnings and transitions. We implement two bounds, one wider and thus more conservative than the other, more “naive” bounds. Extending

Manski (1990) to our differences-in-differences setting, we deal with this by imputing earnings and employer transitions for observations that exit the UAE. The aim is to produce upper and lower bounds on our main results. For both log earnings and employer transitions, we first recover residuals of each outcome \tilde{y} in the 7-period window around a contract expiration, conditional on worker and year-month fixed effects. We calculate the 90th and 10th percentile values of the distribution of residuals separately for before and after a contract expiration, and separately pre and post the reform: $\tilde{y}_{90}^{Post1}, \tilde{y}_{10}^{Post1}, \tilde{y}_{90}^{Pre1}, \tilde{y}_{10}^{Pre1}, \tilde{y}_{90}^{Post0}, \tilde{y}_{10}^{Post0}, \tilde{y}_{90}^{Pre0}, \tilde{y}_{10}^{Pre0}$.

To impute an upper bound on our coefficients, we assume that all exits following a contract expiration after the reform have the 90th percentile value, \tilde{y}_{90}^{Post1} , and all exits prior to a contract expiration, but after the reform, have the 10th percentile value, \tilde{y}_{10}^{Post0} . Similarly, we impute \tilde{y}_{90}^{Pre0} for all pre contract expiration exits prior to the reform, and \tilde{y}_{10}^{Pre1} for all the post contract expiration exits prior to the reform. For the lower bound, we impute \tilde{y}_{90}^{Post0} to all exits prior to a contract expiration but following the reform, and \tilde{y}_{10}^{Post1} to all exits following a contract expiration after the reform. The parallel assignment is done with \tilde{y}_{90}^{Pre1} and \tilde{y}_{10}^{Pre0} to exits prior to the reform. This strategy maximizes the impact of selection on the coefficients estimated by our differences-in-differences framework. The intuition of this approach is that the reform alters the types of individuals that choose to leave the country in the way that will shift our estimates the most.

The naive bound just assigns \tilde{y}_{90} to all exits for the lower bound and \tilde{y}_{10} to all exits for the upper bound. These are tighter bounds than the ones above, as we show in Appendix A. Table 6 shows the coefficients of equation 2 using the imputed values of earnings and transitions. Column 1 shows the coarse bounds and column 2 shows the naive bound estimate of the baseline specification without controls for the imputation of log earnings that recovers an upper bound on the coefficients of interest, while columns 3 and 4 show the coarse and tight lower bound on the same coefficients. The upper bound of the impact of the reform on earnings over the 3 months following a contract expiration is 17 – 18% , while the lower bound is 3-5%. Thus, both remain positive and statistically significant at the 1% level. While the coarse bounds are wide, it is reassuring that the estimated earnings effects remain positive even when the pattern of selection on imputed wages is chosen to minimize the estimated effect. In contrast, the tighter naive bounds are very close to the main results, with the upper bound at 12% and the lower bound at 9%, both very close to the 11% estimate from the main sample.

Columns 5-8 report the same results with employer changes as the dependent variable. The

overall impact is the same across the various bounds. Contract expirations result in increased likelihood of employer transitions following the reform, and the estimates are significant at the standard levels. One feature to note is that even the upper bounds for the specifications for this variable are slightly lower than the coefficients from Table ??.

7.4 Time-shifted placebos

In order to rule out further sources of unobserved heterogeneity, we conduct a falsification exercise where we assume that contract expiration dates are uniformly shifted backwards by multiples of 3 months from 0 to 24. Formally, we re-estimate equation 2 replacing D_{it} with $\tilde{D}_{it}^j \equiv D_{i,t-3j}$, where j runs from 0 to 8.

Figure 6 shows the resulting plots of $\sum_{k=0}^3 \tilde{D}_{i,t+k}^j$ for log earnings. Consistent with our previous results, the only positive and significant coefficient is where $j = 0$, which corresponds to our main specification. The confidence intervals become large as j increases as the sample size falls. Figure 7 shows the same plot for employment changes, while Figure 8 shows the same plot for UAE exits. In all cases, we obtain the result from our main specification at $j = 0$, a much smaller result at $j = 3$, and then no or little effect from $j = 6$ to $j = 24$, with standard errors increasing substantially for $j \geq 21$ as the sample size becomes smaller. Overall, this suggests that our main effects are not driven by other changes in the wage-tenure profile that are not due to contract expirations.

7.5 Heterogeneity

We first explore heterogeneous effects by quarter. This provides an additional check that our results are not merely picking up a time trend of increased wages upon contract expiry. We attempt to ameliorate the unbalanced nature of the panel somewhat by restricting attention to observations of individuals with data both before and after the reform.

We estimate the following equation:

$$y_{it} = \gamma_q \sum_{k=0}^3 D_{it+k} + \delta_i + \delta_q + \beta_q X_{it} + \epsilon_{it} \quad (3)$$

where q refers to the quarter. X_{it} are the controls for worker characteristics. The estimates of γ_q provide the quarter-by-quarter impact of a contract expiration in the month of expiration and the three months afterwards.

Figure 9 shows how the effect of contract expiration changes on log earnings quarter by quarter. Specifically, it shows the sum $\sum_{k=1}^3 \frac{\gamma_k}{3}$ for each calendar quarter. There is a dip in the post-contract expiration evolution of earnings in 2009 but other than that there are little pre-reform trends.²⁷ There is an immediate shift in earnings following contract expirations after the reform, and the earnings effects seem to grow over time.

As shown in Figure 10, the post-reform increase in labor market competition can be seen equally starkly in the results on transitions. The standard errors are quite large given that there are a fairly small number of individuals changing employers in any given quarter. However, the pattern shows a fairly flat trend in employer transitions following a contract expiration prior to the reform, and a shift up in this outcome following the reform. The coefficients are only significantly different from zero after the reform occurs (in most of the periods).

Figure 11 shows the effect of contract expiration changes on country exits by quarter. There is a drop in migrants exiting the UAE following their contract expirations after 2009 but it drops dramatically again around the time of the labor market reform. The drop occurs within a couple of quarters of the reform and remains at the new lower level.

We next examine heterogeneity by worker characteristics. Table 7 shows heterogeneity by various sub-populations. We begin by examining whether the reform benefited individuals who changed employers differently from stayers, who are defined as workers who never changed employers during our sample frame. The impact of the reform on the earnings of stayers is very close to our main sample estimates. This indicates that employers responded to the increased labor market competition by altering wages paid to workers without equilibrium transitions as suggested in Cahuc, Postel-Vinay and Robin (2006).

Next we look at workers from India, who are by far the largest source country in our sample. Column 2 of Table 7 and column 1 of Table 8 show that Indians both experience a larger wage effect, as well as a larger mobility effect. Column 3 of Table 7 shows that educated workers experienced a smaller wage gain from the reform, perhaps consistent with this group already having good outside options, but experienced a larger than average probability of an employer transition, as shown in column 2 of Table 7.

When we look at workers who began in the construction sector, we see large effects of the reform on wages, at roughly 15%, and small increases in mobility, around 0.2 percentage points. We also look separately at workers in the Dubai and Sharjah cities, as these workers

²⁷The coefficients tend to be negative prior to the reform. This suggests that real earnings actually falls after contract renewals prior to the reform, even though nominal earnings increase.

potentially have more access to the free enterprise zones near these cities, which are governed by a different labor regulation system. We see a somewhat smaller (7%) wage effect, and a much larger mobility effect of 1.2 percentage points.

Interestingly, as shown in Table 9, there does not seem to be significant heterogeneity in the probability of exiting from the UAE across the same sub-samples. The fact that this is true even in the Dubai-Sharjah subsample suggests that differential exits to the freezones is unlikely to be a significant explanation of our results.

Finally we look at young workers defined as those under the age of 35 at the start of the sample. While these workers do not experience wage effects that are very different from the overall sample (Table 7, column 6), they do seem to experience a somewhat larger than average increase in mobility following the reform (Table 8, column 5). Perhaps consistent with the value of future work opportunities, we see a slightly larger fall in exits from the UAE for younger workers.

8 Alternative Explanations

In this section, we explore other possible changes that could be driving our results. We focus on the possibility that changes in contract duration (from 3 to 2 years) that were announced and implemented at the same time as the NOC reform could be driving the estimated results. While there were other changes to the minimum and maximum ages as well as labor card costs for skilled workers in the reform, they do not affect the vast majority of workers in our sample. Thus, we focus exclusively on the changes in contract duration.

Using the data on all contracts in the MOL database, Figure 12 shows the average length of worker contracts by the start date of the contract. Contract lengths fell substantially at the time the reforms on contract length and on the NOC requirements were implemented.²⁸ How the change in contract duration might affect the main estimates depends on whether employers and workers prefer longer or shorter contracts. Shorter contracts correspond to shorter durations of tied labor for employers and may lead employers to offer lower wages; this would suggest the bias from the change in contract length would bias our results downwards.

In contrast, if workers prefer long contracts, as they provide more employment stability, the change in contract length may lead to workers needing higher wages to compensate for the

²⁸Note that the full MOL contracts data includes special short term contracts in addition to the standard multiple year contracts.

lower job stability. However, if workers are dissatisfied with contracts of a shorter duration, we would expect to see a higher rate of exiting the UAE, the opposite of what we see in the data.

Another potential interpretation is that workers are not in fact exiting to their source country but in fact are leaving for the illegal labor market. While this is almost certainly likely going on, it does not affect our wage or mobility results so long as the wage in the informal sector sits between the wage in the source country and the formal sector wage. However, another reason to doubt that this is a major component of the exit response is that the informal labor market is relatively small in the UAE, as the government is able to locate and deport most illegal workers, and sentences for illegal activity are potentially quite harsh. Thus the informal labor market of the UAE is relatively small.

9 Conclusion

Transnational labor markets, where migrants from poor countries are employed for fixed-length contracts in richer countries, are growing in importance and are a potentially powerful way to raise world welfare and reduce across-country income inequality. While the ex-ante international mobility allows workers in poor countries an opportunity to earn much higher income, the institutions surrounding these labor markets restrict the ability of workers to take advantage of ex-post labor market opportunities in the destination country. These institutions are present, in some form or another, in virtually every country with an extensive migrant worker population.

The reform of the *kafala* system in the UAE provides an opportunity to study the impact of ex-post labor market mobility on migrants' labor market outcomes. Previously, visa requirements tied migrant workers to employers for the duration of their contract and restricted the extent of mobility in the labor market. The 2011 labor reform, which allowed workers to change employers after the expiration of a contract without explicit approval from their existing employers was quite effective: worker mobility doubled, wages increased by over 10%, and exits from the UAE fell.

Our results suggest that ex-ante international mobility is not enough to allow workers to capture their full marginal productivity. Restrictions on ex-post mobility play an important role in depressing wages. This has important consequences for many other types of labor markets where employment-at-will does not prevent workers from tying themselves to employers,

be they bonded labor in agricultural settings or non-compete clauses in Silicon Valley.

Additional research is needed to fully understand the implications of the labor reform on international migration. While we demonstrate that migrant workers already in the UAE benefited from the reform, the reform may potentially affect the decisions of firms regarding recruiting and wage-setting for new migrant workers. While the model in the Appendix suggests some margins of firm behavior that could be altered by the reform, we leave models and tests of the potentially rich general equilibrium effects of the labor reform to future work.

A Difference-in-Difference Bounds

In this section, we describe the extension of Manski (1990) to our differences in differences setup. For simplicity, we consider only two periods B and A , corresponding to before and after the reform, and two states 1 (After contract expiration) and 0 (before contract expiration). Without any attrition, our estimator should implement:

$$\hat{T} \equiv E[y_{A1}|i, t] - E[y_{A0}|i, t] - (E[y_{B1}|i, t] - E[y_{B0}|i, t]) \quad (4)$$

With attrition, or exits to the UAE denoted Z , we have (suppressing i and t in the conditional expectations and probabilities):

$$\begin{aligned} \hat{T} = & E[y_{A1}|Z=0]P(Z=0|A1) - E[y_{A0}|Z=0]P(Z=0|A0) - (E[y_{B1}|Z=0]P(Z=0|B1) - E[y_{B0}|Z=0]P(Z=0|B0)) \\ & + E[y_{A1}|Z=1]P(Z=1|A1) - E[y_{A0}|Z=1]P(Z=1|A0) - (E[y_{B1}|Z=1]P(Z=1|B1) - E[y_{B0}|Z=1]P(Z=1|B0)) \end{aligned}$$

If we know that $y_{A1}, y_{A0} \in [y_{min}^A, y_{max}^A]$ and that $y_{B1}, y_{B0} \in [y_{min}^B, y_{max}^B]$, then we can bound 5 from above with the following:

$$\begin{aligned} \hat{T} \leq & E[y_{A1}|Z=0]P(Z=0|A1) - E[y_{A0}|Z=0]P(Z=0|A0) - (E[y_{B1}|Z=0]P(Z=0|B1) - E[y_{B0}|Z=0]P(Z=0|B0)) \\ & + y_{max}^A P(Z=1|A1) - y_{min}^A P(Z=1|A0) - (y_{min}^B P(Z=1|B1) - y_{max}^B P(Z=1|B0)) \\ \equiv & \hat{T}^U \end{aligned}$$

The expression on the second line is what the difference-in-differences estimator recovers without adjusting for selection. The third line is the ‘‘imputed’’ difference in difference estimate, where y is imputed so that the exitters have the largest possible diff-in-diff estimate of y . We define the sum of these two terms as T^U , indicating that this is the upper bound on \hat{T} .

Similarly, we define \hat{T}^L as a lower bound on \hat{T} , by imputing values of y to exitters so that the smallest possible diff-in-diff estimate is assumed:

$$\begin{aligned} \hat{T} \geq & E[y_{A1}|Z=0]P(Z=0|A1) - E[y_{A0}|Z=0]P(Z=0|A0) - (E[y_{B1}|Z=0]P(Z=0|B1) - E[y_{B0}|Z=0]P(Z=0|B0)) \\ & + y_{min}^A P(Z=1|A1) - y_{max}^A P(Z=1|A0) - (y_{max}^B P(Z=1|B1) - y_{min}^B P(Z=1|B0)) \\ \equiv & \hat{T}^L \end{aligned}$$

So we have that $\hat{T}^L < \hat{T} < \hat{T}^H$. Now, we show that these bounds are coarser than ‘‘naive’’ bounds. Define the naive lower bound as $\hat{T}_{Naive}^L = \hat{T} + \hat{P}y_{max}$, where $y_{max} = \max(y_{max}^A, y_{max}^B)$ and:

$$\hat{P} = P(Z=1|A1) - P(Z=1|A0) - (P(Z=1|B1) - P(Z=1|B0)) < 0$$

Where the inequality is assumed, and implies that attrition is monotonic in treatment.

Note that $\hat{T}_{Naive}^L > \hat{T}^L$ follows from:

$$\hat{P}y_{max} > y_{min}^A P(Z = 1|A1) - y_{max}^A P(Z = 1|A0) - (y_{max}^B P(Z = 1|B1) - y_{min}^B P(Z = 1|B0))$$

A parallel exercise with $y_{min} = \min(y_{min}^A, y_{min}^B)$, under the same assumption that $\hat{P} < 0$, shows that $\hat{T}_{Naive}^H < \hat{T}^H$. In the paper we use the 90th and 10th percentiles of the residualized (orthogonal to worker and year X month fixed effects) earnings distributions, separately before and after the reform, to obtain analogues of $y_{max}^B, y_{min}^B, y_{max}^A, y_{min}^A$.

B Model

We provide a simple model to show how contracts relate to labor market competition and wages in our context. The model is an extension of Aghion and Bolton (1987), who suggest that contracts (enforced with endogenous payments for breach) can be a barrier to entry. In our case, contracts (enforced with exogenous payments for breach) act as a barrier to employment offers.

Our model shows simply the effect of the reform on labor market competition for workers. By eliminating payments for breach, firms that did not invest in recruitment can attempt to poach workers recruited by firms who did. This increases the set of options available to the worker, resulting in higher wages, higher probabilities of transition, and higher welfare from remaining in the host country.

This model also helps us understand why some firms recruit directly and others instead wait and attempt to poach workers recruited by other firms: the cost of recruitment is high. Anecdotal evidence from the UAE confirms this, as firms report substantial difficulties in setting up recruitment in source countries. Difficulties include screening, as well as paying fees to the UAE government to insure against the costs of enforcing regulations on migrants (e.g. catching and deporting absconding workers).

Consider a 2 period model with measure 1 workers with reservation wages equal to 0 and two firms with constant marginal productivity. Each firm is initially uncertain about their productivity, which is distributed on $U(0,1)$. Prior to learning their types, firms can choose either to invest F and obtain a recruitment network in a poor country, or instead wait, forgo 1 period of profit, and try to recruit from the labor pool already in the country.

Formally, the timing is:

1. Firms choose whether or not to invest in recruitment in source country at cost F .

2. Firms learn their own productivity.
3. Firms with recruitment networks make offer to workers (Bertrand or monopoly).
4. Period 1 production occurs and payments are made.
5. Firms without recruitment networks can make offers to workers (Bertrand).
6. Workers all choose whether or not to switch (and if pre-reform, bear costs).
7. Period 2 production occurs and payments are made.

We solve by backwards induction, first focusing on the case where one firm (say Firm 1) forms a recruitment network in a poor country, paying the cost F , while the other, say Firm 2, does not. Firm 1, after realizing productivity p , gets to make an initial offer to all workers (who have reservation wage $b = 0$). Firm 2, however, has productivity q , but can only make offers to workers already in the country. Therefore, in the first period, Firm 1 offers wage 0.

To capture the institutional environment in the UAE, suppose that there is a law that imposes a restriction on worker transitions in period 2. Formally, suppose that the worker has to leave the country for a fixed fraction $d > 0$ prior to taking Firm 2's offer. In other words, d is the fraction of the second period that the worker must spend out of the labor market, so Firm 2 also cannot benefit from the worker's services for that fraction of the period.

Transitions will only occur if Firm 2 can make positive profits while making a wage offer that workers will accept. Since workers are homogeneous and firms have constant marginal productivity, if one worker leaves, all workers will leave. If Firm 2 has productivity q , it will offer a wage up to $q(1-d)$ as workers are not working for fraction d of the second period. Firm 1 will respond to the offer with an identical offer so long as $p > q(1-d)$, and so the probability of a transition conditional on a given p is $1 - \frac{p}{1-d}$ if $p < 1-d$ and 0 otherwise. The ex-ante probability of a transition is $\frac{1-d}{2}$.

The period 2 wage depends on the relative values of p and q . If $p < 1-d$, then there is a chance that the poaching firm will draw a q such that $1 > q > \frac{p}{1-d}$; in this case, a transition will occur and the wage paid will be $\frac{p}{1-d}$. Otherwise if $q < \frac{p}{1-d}$, then no transition will occur, but wages will still be bid up to $(1-d)q$. If $p > 1-d$ then transitions will never occur for any value of $q \in (0, 1)$, but again wages will be bid up to $(1-d)q$. Thus we get an expression for expected period 2 wages w_2 as:

$w_2 =$

$$\begin{aligned} & \overbrace{\int_0^{1-d} \left(\int_{\frac{p}{1-d}}^1 \frac{p}{1-d} dq \right) dp}^{\text{Transition}} + \overbrace{\int_0^{1-d} \left(\int_0^{\frac{p}{1-d}} (1-d)q dq \right) dp + \int_{1-d}^1 \left(\int_0^1 (1-d)q dq \right) dp}^{\text{No Transition}} = \\ & \int_0^1 \frac{p^2(1-d^2) + 2p + (1-d)}{2(1-d)^2} dp = \frac{1-d^2}{3} \end{aligned}$$

Note that this is also the average change in wages, as the period 1 wage is always 0. Clearly, as d falls, the ex-ante probabilities of transitions increase, while ex-ante wages (and wage changes) increase.

Now suppose the law is reformed, so that $d = 0$, and Firm 2 can more easily poach all the workers in period 2. Workers will now accept any offer that is greater than p , which will occur with probability $\frac{1}{2}$ as Firm 2 will now make an offer whenever $q > p$. Thus, expected second period wages are now $E[\min(p, q)] = \frac{1}{3}$ which are greater than before. Workers are better off than they were before the reform.²⁹

Workers in period 2 will now more frequently be better off than the reservation position in their source country, and so exits from the UAE should also decline.

Note also that the model suggests that recruitment may not increase following the reform. The reason is that it makes it more profitable to try and poach workers that have already been brought to the UAE by other firms. This suggests one reason why wages may respond to the reform despite the presence of a large supply of low reservation wage workers: fixed costs of recruitment networks imply that some firms may choose to only poach workers already in the country, rather than trying to recruit their own from abroad.

What about profits? Firm 1 (the recruiting firm) gets profits p in the first period and $p - (1-d)q$ if there are no transitions in the second period. Firm 2, the poaching firm, gets 0 in the first period and $(1-d)(q - \frac{p}{1-d})$ in the second period if there is a transition. Integrating over the probability of a transition we get that expected profits for the first firm (the recruiting one), $E[\pi_1]$ are given by:

$$E[\pi_1] = \frac{1}{2} + \int_0^{1-d} \left(\int_0^{\frac{p}{1-d}} p - (1-d)q dq \right) dp + \int_{1-d}^1 \left(\int_0^1 p - (1-d)q dq \right) dp - F = \frac{1}{2} + \frac{1+d+d^2}{6} - F \quad (5)$$

²⁹To the extent that workers are better off after the reform, they should be less likely to exit the UAE, but this is outside of the context of this simple two-period model.

While expected profits for firm 2 (the poaching firm) are:

$$E[\pi_2] = \int_0^{1-d} \left(\int_{\frac{p}{1-d}}^1 (1-d) \left(q - \frac{p}{1-d} \right) dq \right) dp = \frac{(1-d)^2}{6} \quad (6)$$

Suppose $F < \frac{2}{3}$, then $E[\pi_1] > 0$ for all values of $d \in [0, 1]$. Then we have that $E[\pi_1] > 0$ and $E[\pi_2] > 0$. Note that both firms make positive expected profits in this equilibrium, so neither will deviate from their decision regarding investing in a recruitment network. We now turn to examining the conditions under which this is a unique Nash equilibrium.

B.1 Alternative Recruitment Equilibria

Consider the several alternative possible equilibria. One possible equilibrium is that neither firm sets up a recruitment network, in which case both firms get 0. Another possibility, which we now characterize, is the equilibrium where both firms set up recruitment networks. Firm 1 and 2 anticipate productivity draws p and q , respectively. Without loss of generality suppose $p > q$. Then they engage in Bertrand competition for the workers in the source country.³⁰

We solve by backwards induction. Suppose that Firm 1 has all the workers at the end of period 1. The maximum Firm 2 can offer to attract workers is q . Since $q < p$ there are no transitions in period 2 and the second period wage is $\frac{q}{1-d}$. Now consider the case where Firm 2 has all the workers at the end of period 1. If $p > \frac{q}{1-d}$ then Firm 1 will offer $\frac{q}{1-d}$ and poach all the workers. If $p < \frac{q}{1-d}$, then Firm 1 will bid p for workers but there will be no transitions and wages will be $\frac{p}{1-d}$.

Now consider period 1 where both firms are recruiting (and competing) in the source country. There are two cases. First, since $p > q$, if $p > \frac{q}{1-d}$ then Firm 1 bids q and attracts all the workers in period 1 and retains them in period 2. In this case, across periods, Firm 1 gets $p - q + p - (1-d)q - F$ and Firm 2 gets $-F$.

However, if $q > (1-d)p$ it could also be that Firm 2 pays a wage $w' > q$, taking a loss $w' - q$, in the first period in order to keep workers at $p(1-d)$ in period 2, in which case Firm 2's profits would be $q - w' + q - (1-d)p - F$ and Firm 1 would get $-F$. This would only be profitable if $p < w' \leq 2q - p(1-d) - F$. But then Firm 1 could offer the same w' plus ϵ , make positive profits and get all the workers in period 1. This will drive first period wages up to $2q - p(1-d)$, and second period wages will be $q(1-d)$, so the profits of firm 1 will be $(p - q)(3 - d) - F$.

³⁰The case in which they do not need to engage in Bertrand competition for source country workers, perhaps because they enter into different source countries, is also a Nash equilibrium that requires no further discussion.

Summarizing, in the equilibrium where both firms set up recruitment networks, there are 4 possible outcomes. Either Firm 1 has higher productivity than Firm 2, or vice versa. And then for each of these cases there are parameter values in which case initial wages are bid up higher than per-period productivity because the value of locked workers in period 2 is high. There are also parameter values for which the first period wage is equal to productivity of the lower firm. In all cases however, the more productive firm obtains all the workers in both periods, second period wages are $\frac{\min(p,q)}{1-d}$ and there are no transitions.

Averaging over all these cases yields the following expected profit:

$$\int_0^1 \left(\int_0^{p(1-d)} 2p - q - (1-d)q dq + \int_{p(1-d)}^p (p-q)(3-d) dq \right) dp - F = \frac{2+d-d^2}{6} - F \quad (7)$$

So expected profit for both firms is given by $\frac{2+d-d^2}{6} - F$. If $F > \frac{3}{8}$, then expected profits are negative for all values of d in $(0, 1)$. Thus under the assumption that $\frac{3}{8} < F < \frac{2}{3}$ the only pure strategy equilibria will be ones in which one firm invests in a recruiting network, and the other firm poaches. Only in this equilibrium will both firms make positive profits, which dominates the 0-payoffs in the no recruitment case and the negative payoffs in the simultaneous recruitment case.

C Data Appendix

C.1 Matching Ministry of Labor Data with Payroll Processing Data

Both the Ministry of Labor data (MOL) on labor contracts of migrant workers and the payroll processing records with earnings disbursements contain a government issued identifier called the labor card id number. This numeric identifier is associated with each individual-firm match. When workers change employer, they receive a new labor card with a new labor card id number.

We use this identifier to match the two data sets. We lose 107,698 individuals in the payroll processing data set that have missing, non-numeric or incomplete identifiers. This is partially driven by the fact that some individuals in the payroll processing data set do not provide their labor card id. Some individuals provide the company with their passport or a driver's license, but the labor card id is used in the vast majority of cases. We are able to match 553,375 individuals in the payroll processing data with their contract information in the MOL data set. There are 25,883 individuals present in the payroll processing data that are not matched into the MOL data set. This reflects the fact that some migrant workers, including domestic

workers and those working in the freezone areas of the UAE, fall under the jurisdiction of the Ministry of the Interior rather than the Ministry of Labor. In A.1 we show the earnings density of the matched and unmatched workers in the UAE Exchange data. The figure clearly shows that there is little difference in the densities, suggesting that the matching was not biased towards higher wages.

C.2 Variable Construction

Combining the MOL data with the payroll disbursement data allows us to construct several key variables in our analysis. Not all firms use the payroll services of UAE Exchange for earnings disbursement at all or for the entire period for which we have earnings data. The payroll data alone cannot allow us to distinguish whether a person that leaves the payroll data has changed employer or left the UAE. Fortunately, we are able to use the MOL data to construct these variables.

C.2.1 Exiting the UAE

We create a variable for exiting the UAE that takes on a value of one in the month in which an individual leaves the UAE, and zero otherwise. Because we have all of the labor contracts in the MOL, we identify country exits if one of two conditions holds. The first condition identifies whether the person leaves the country permanently. This is defined by whether the person's labor card is no longer active, and there are no subsequent contracts associated with that individual. This includes both individuals that depart before their contract expires and those that leave when it expires. The second condition identifies whether the individual leaves the country for at least six months before returning to the UAE. If workers break their labor contracts with consent from their employers or if employers terminate the labor contracts without providing a NOC, then workers can only start jobs with new employers after leaving the UAE for six months. This is defined using whether the gap between the start of the next contract and the end of the previous contract is greater than six months.

C.2.2 Employer Change

This variable equals one if the person switches firm and zero otherwise. The MOL data contains both labor card id numbers, which change for each contract that an individual has with a firm and an individual identifier that links a person across all of his contracts both within an employer and across employers. If an individual changes firms but there is at least 6 months

between the end of one contract and the start of the other, this is classified as exiting the UAE and is missing for this variable. Thus, this variable captures a direct firm-to-firm switch rather than an individual who must leave the country for six months before he is allowed to take a new job.

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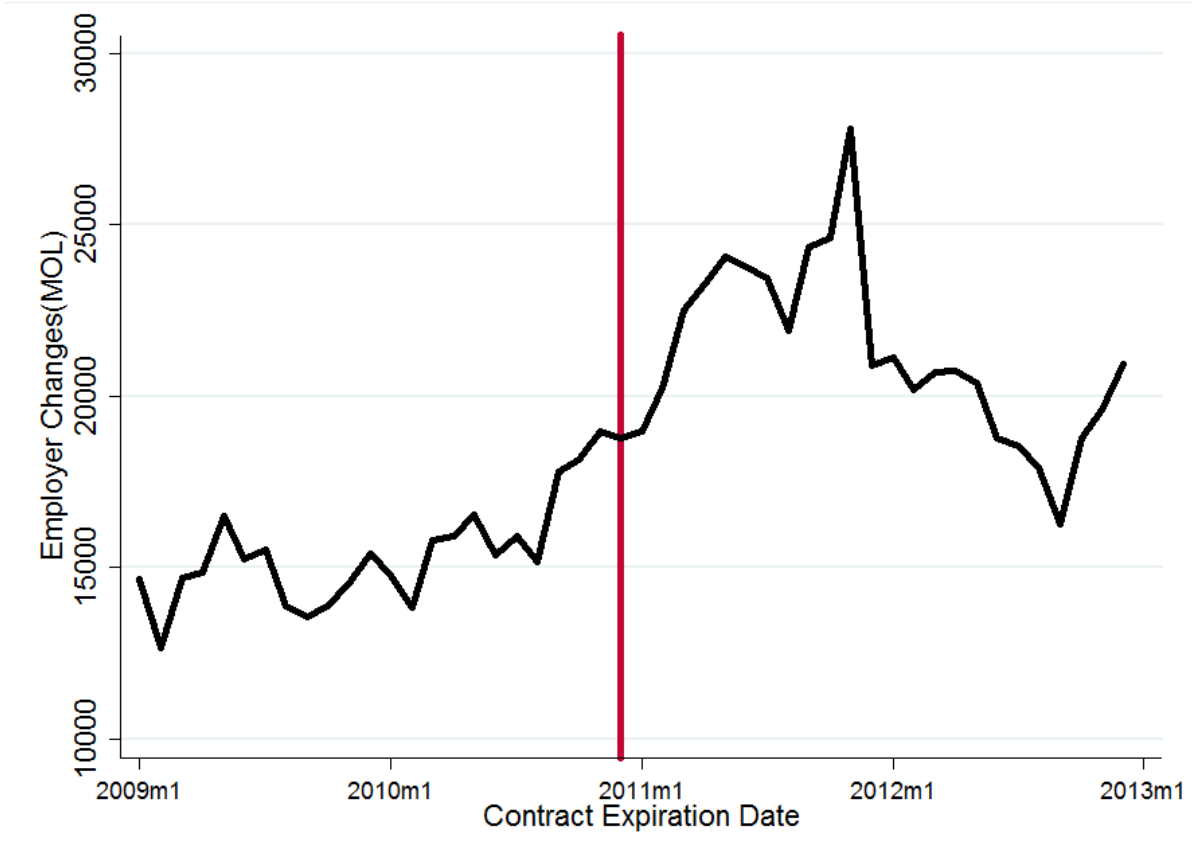


Figure 1: Total Change in Employer by Contract Expiration Date (Sample: MOL data).

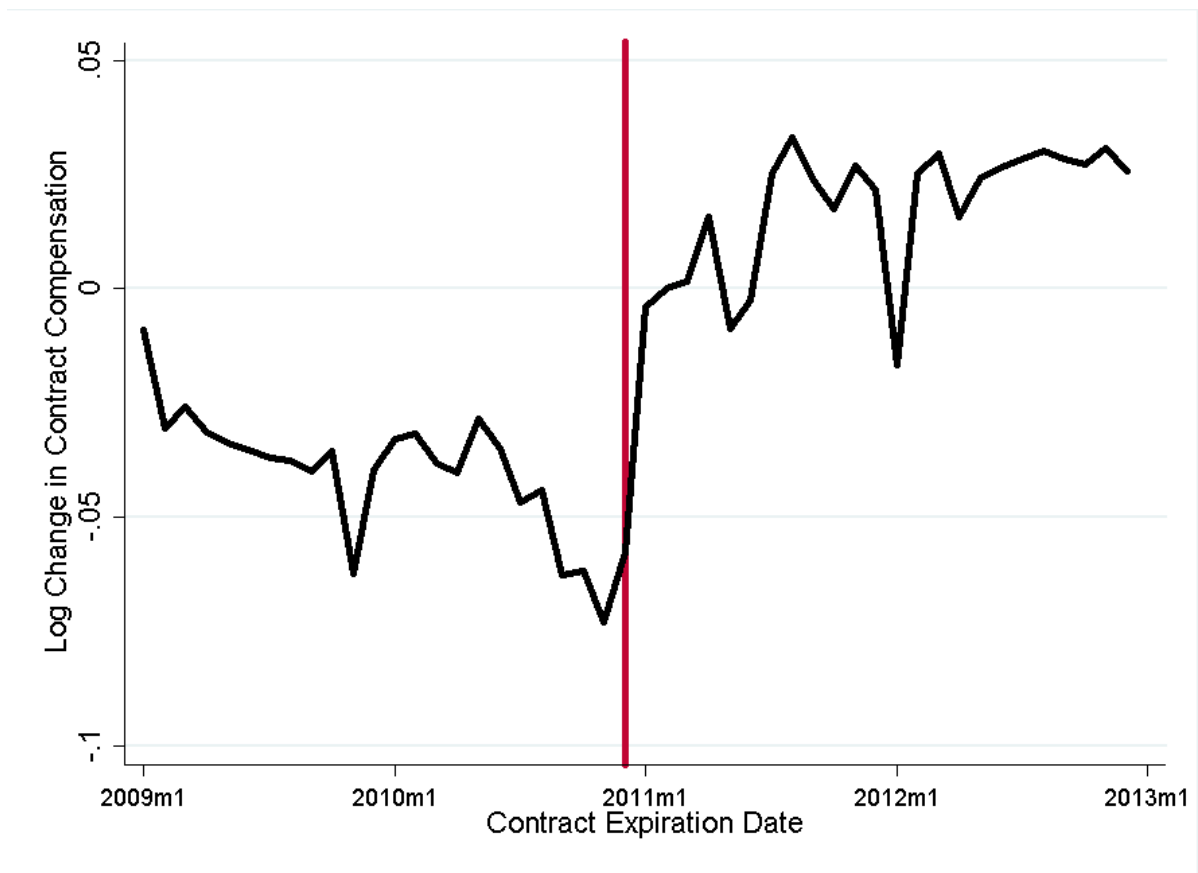


Figure 2: Average Change in Log Real Compensation (Earnings + Benefits) by Contract Expiration Date (Sample: MOL data).

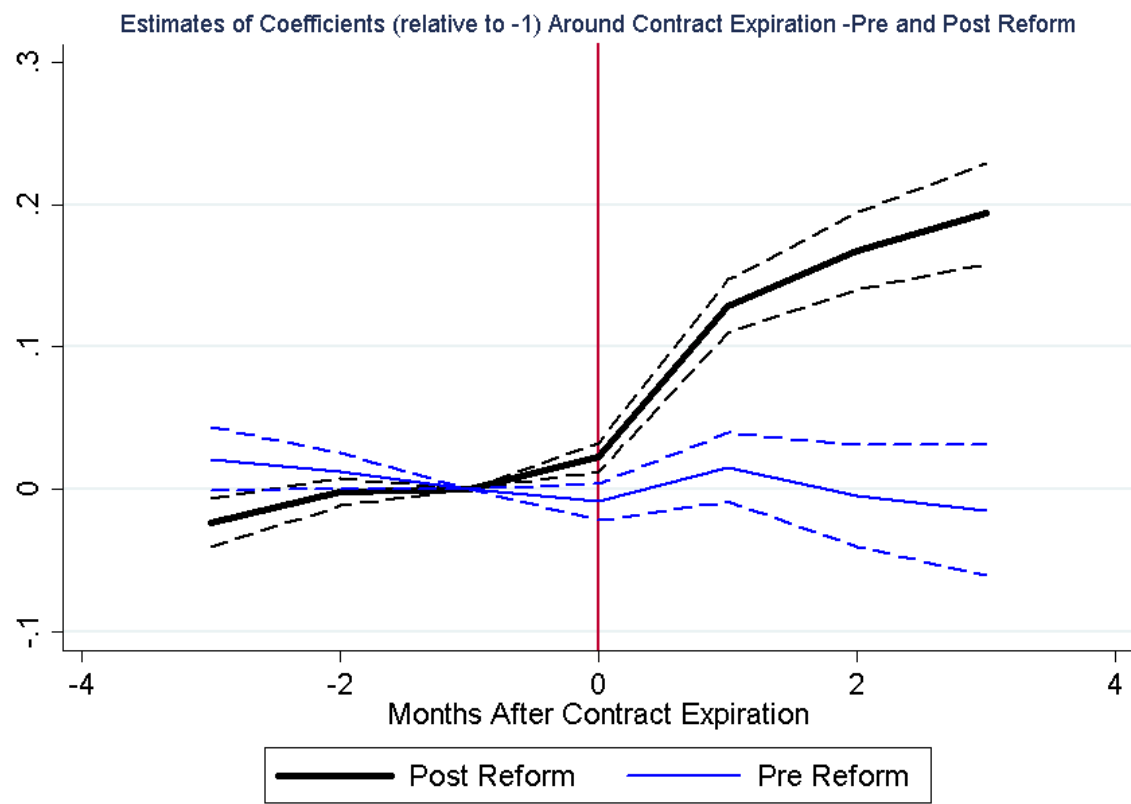


Figure 3: Log Earnings: Event-study pre- and post-reform

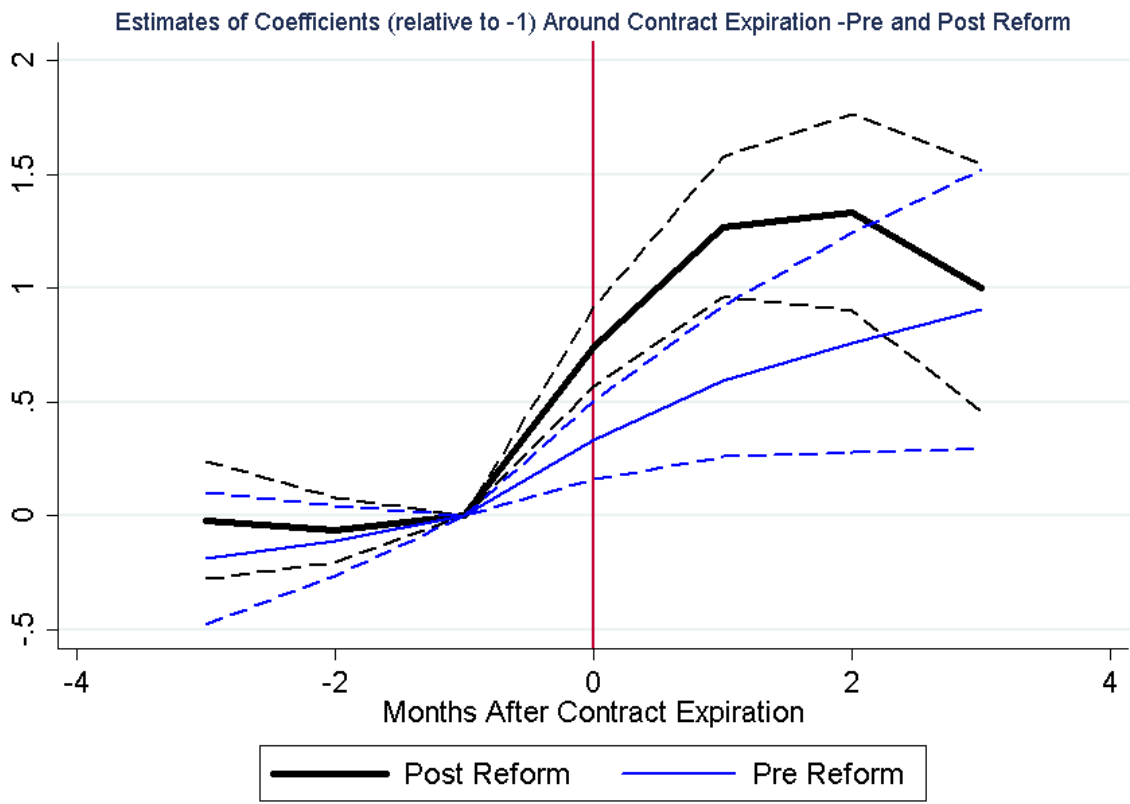


Figure 4: Employer Changes: Event-study pre- and post-reform

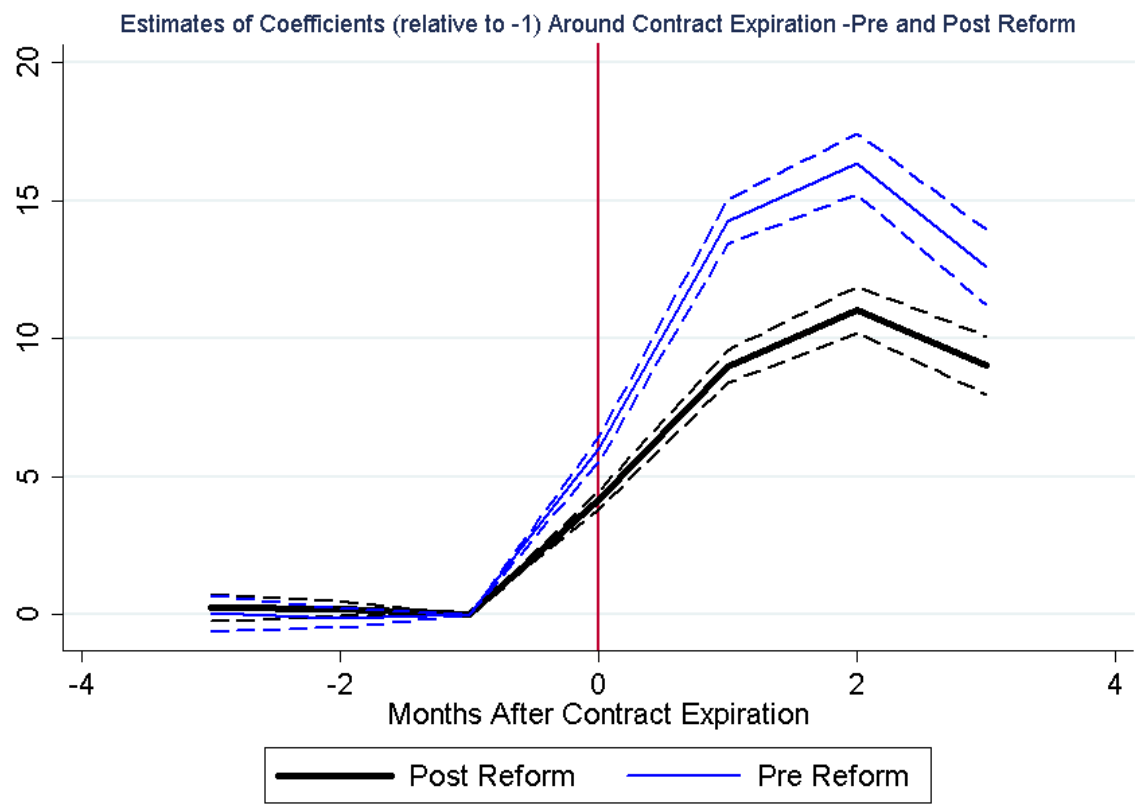


Figure 5: Exit UAE: Event-study pre- and post-reform

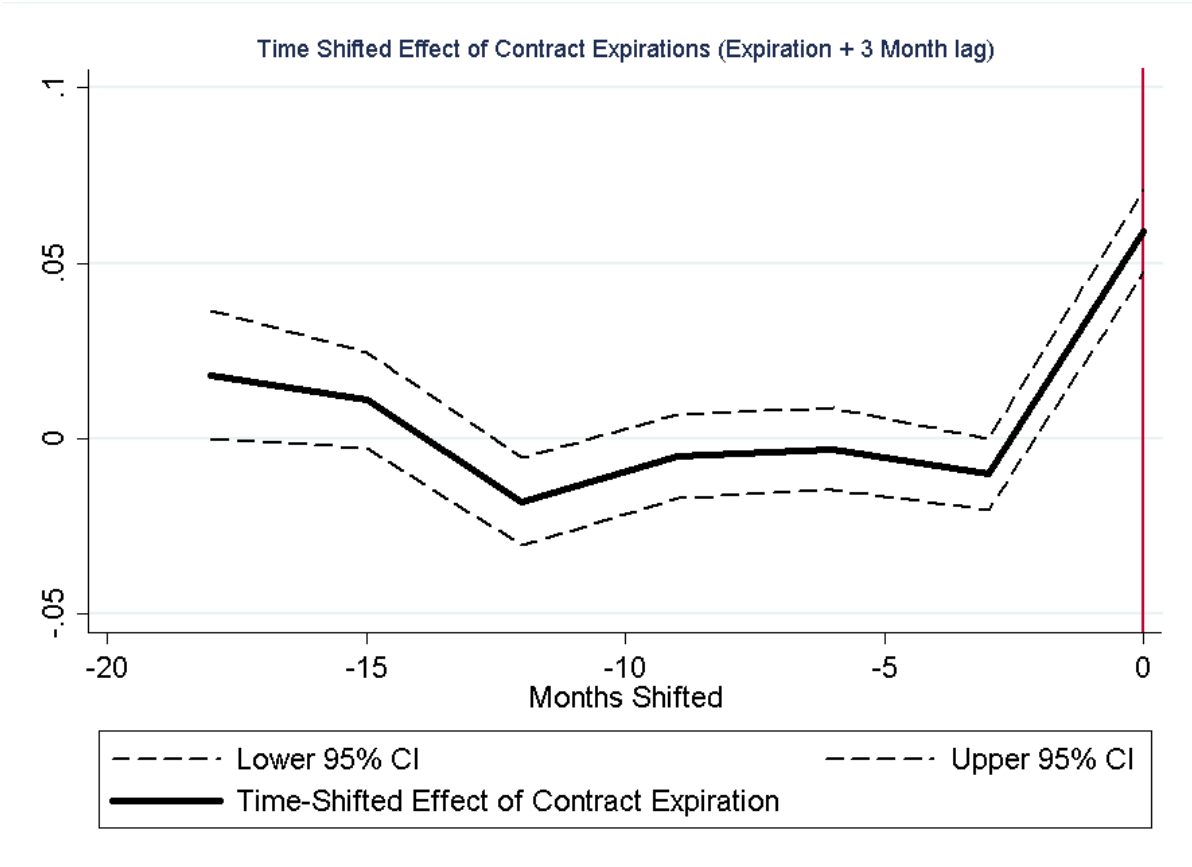


Figure 6: Log Earnings: Time-Shifted Placebos

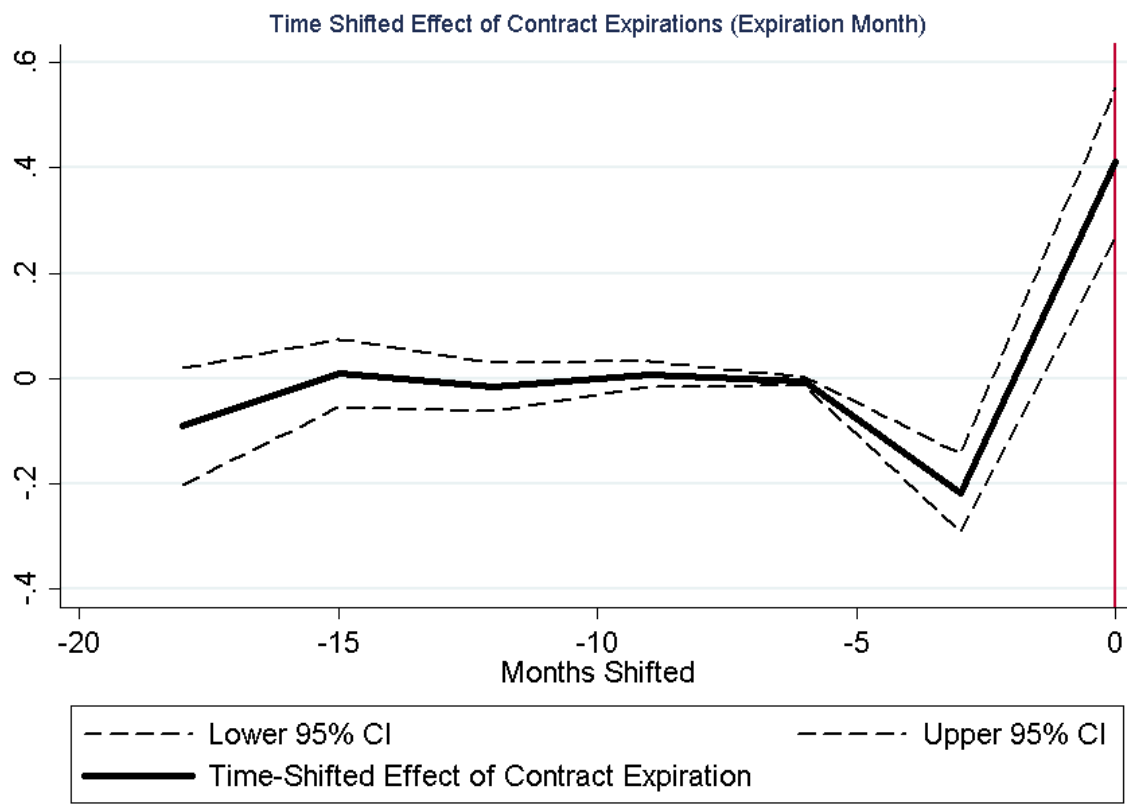


Figure 7: Employment Changes: Time-Shifted Placebos

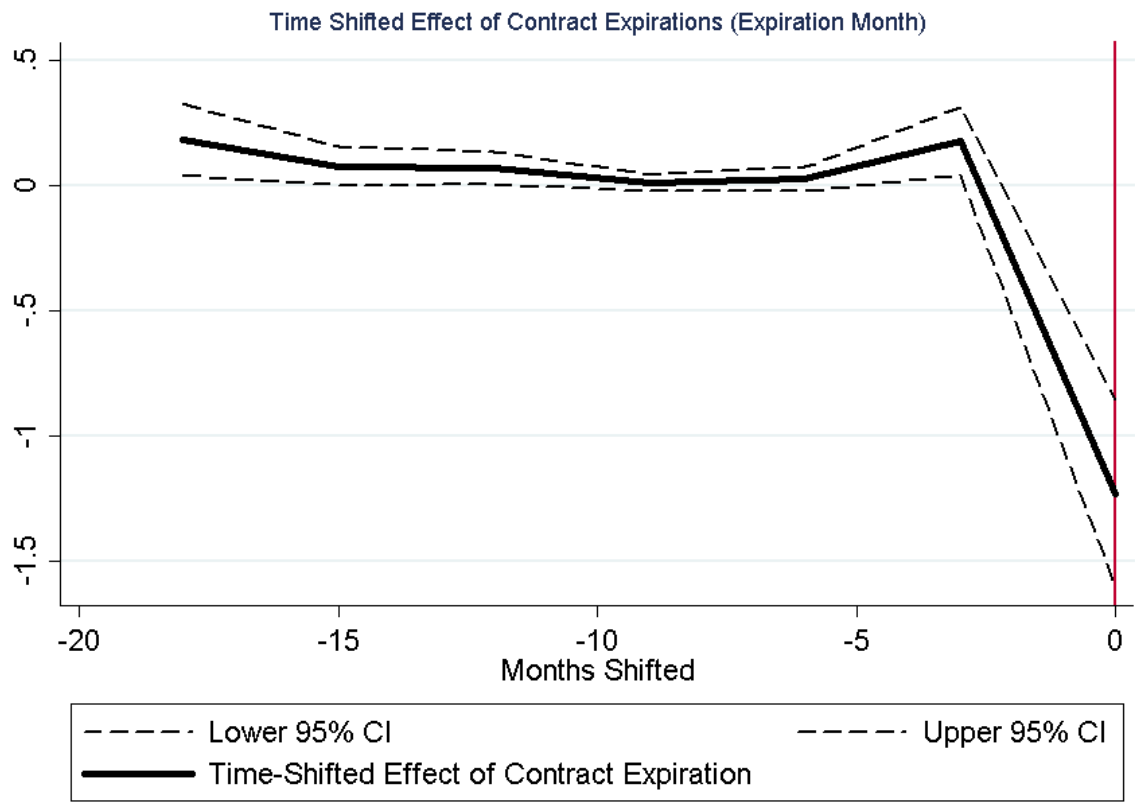


Figure 8: Exits from the UAE: Time-Shifted Placebos

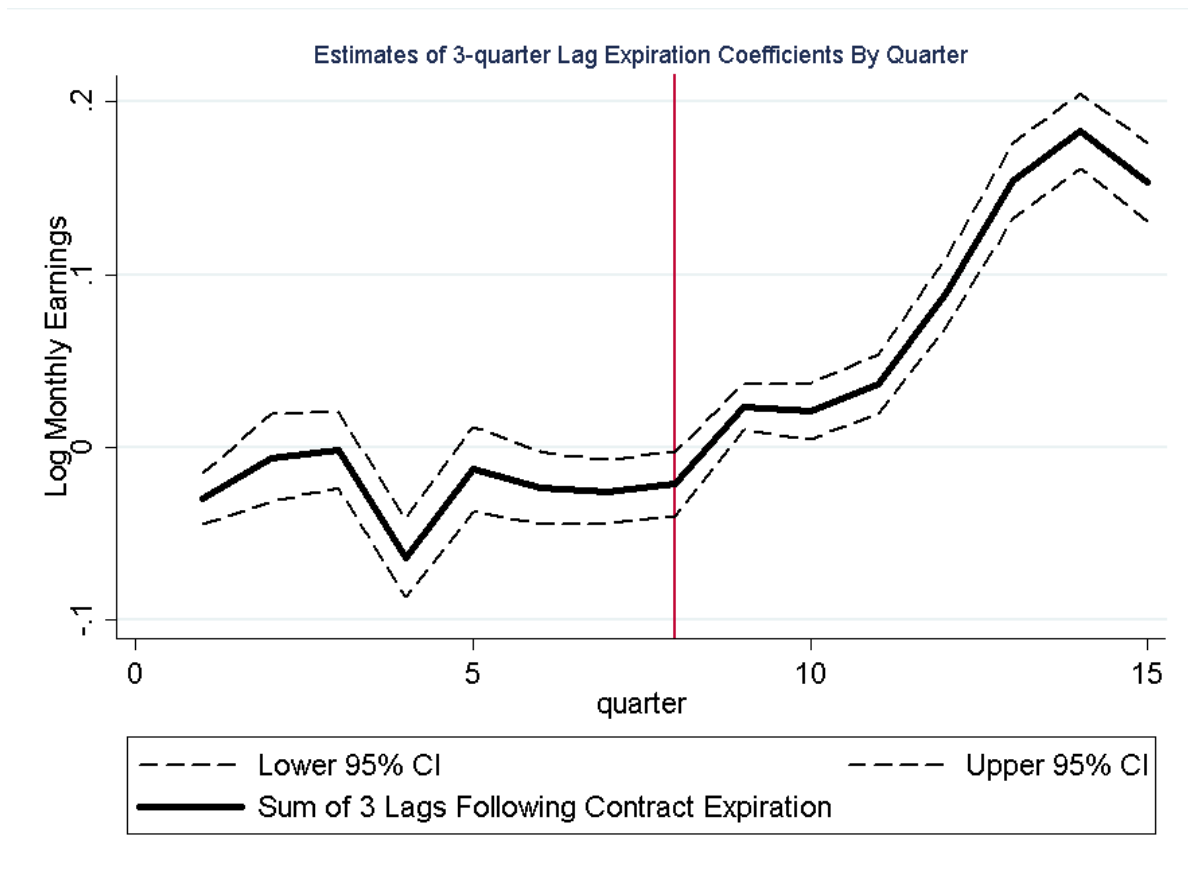


Figure 9: Quarter-specific effects of contract expiration on wages (3 lags).

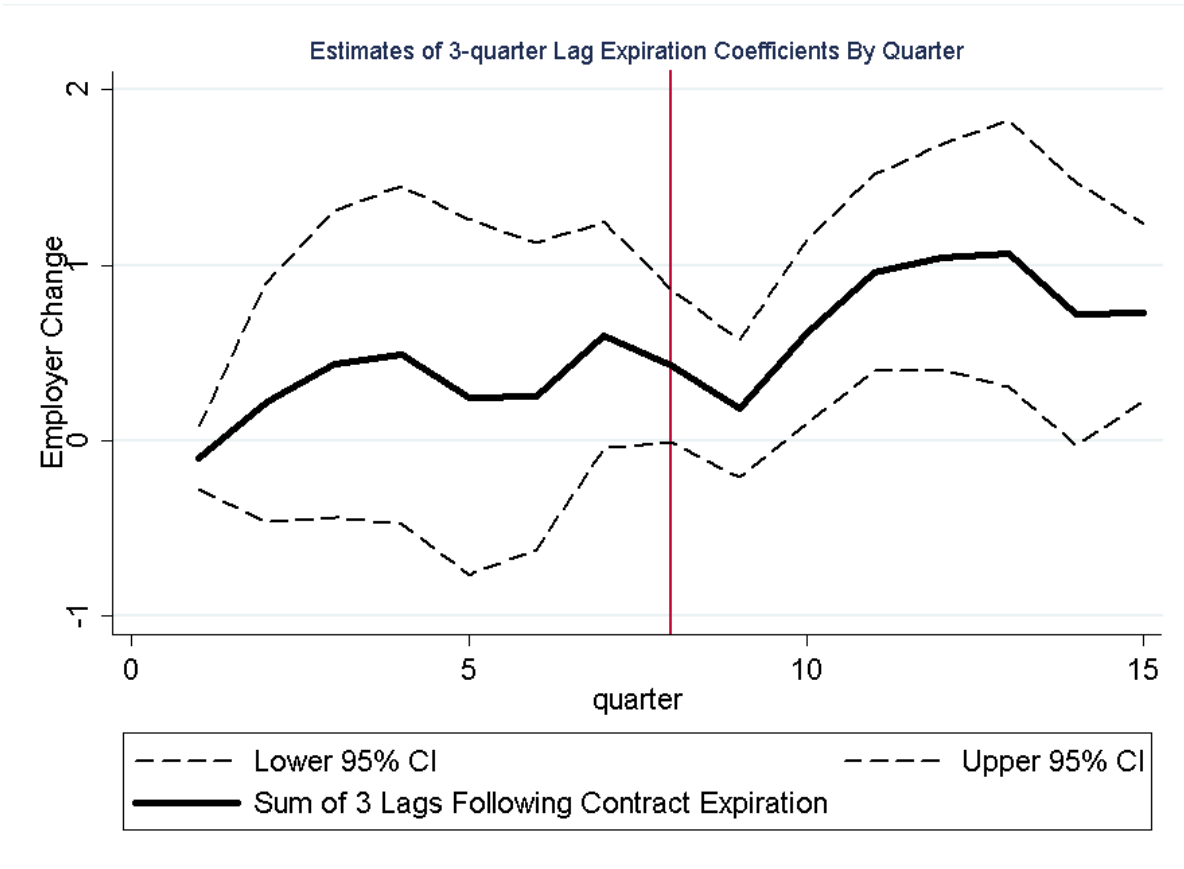


Figure 10: Quarter-specific effects of contract expiration on employer changes (3 lags).

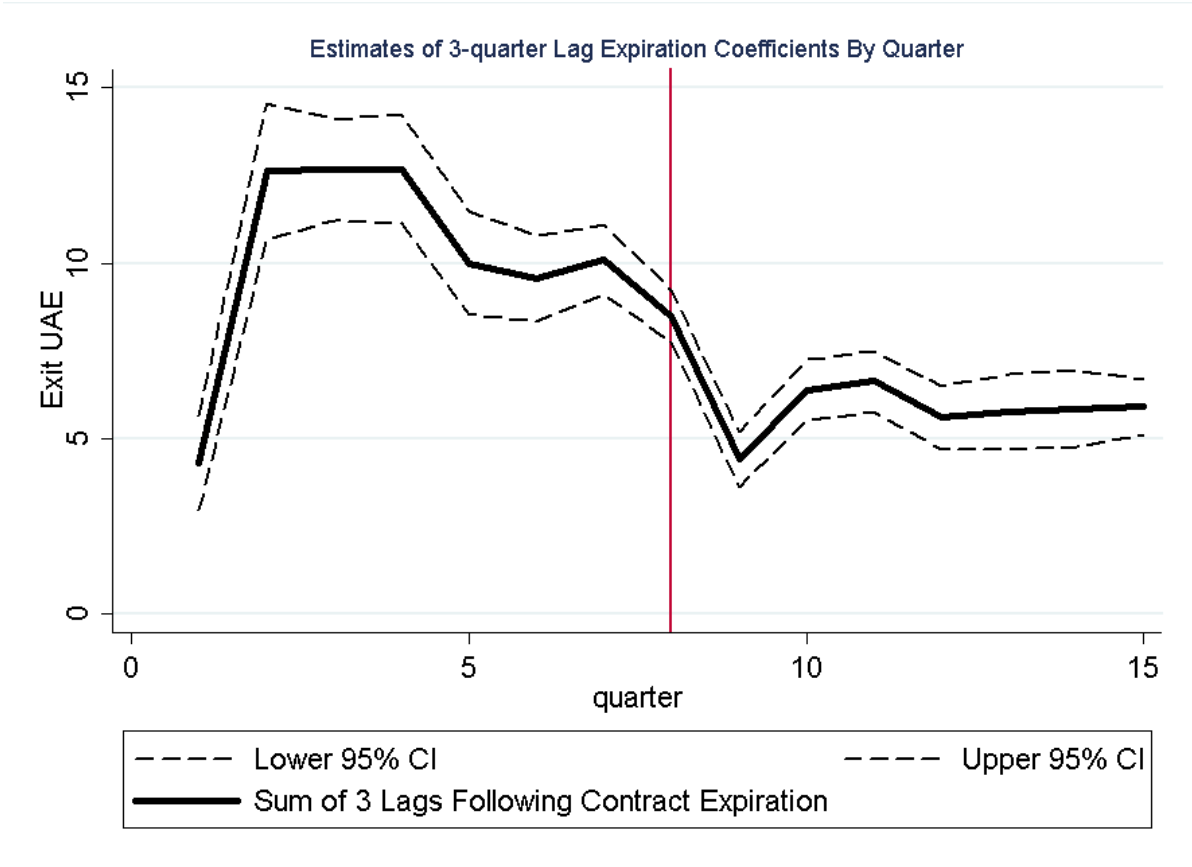


Figure 11: Quarter-specific effects of contract expiration on exits from the UAE (3 lags).

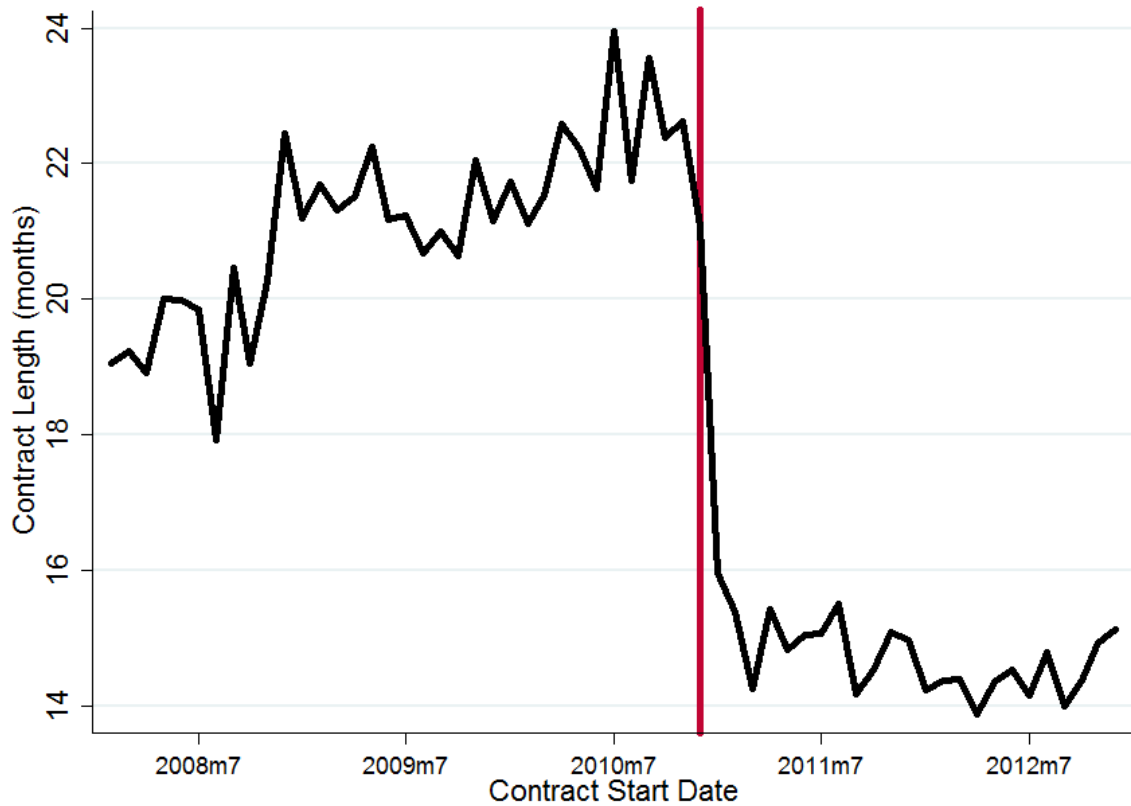


Figure 12: Average Contract Length by Contract Start Date (Sample: MOL data)

Table 1: Summary Statistics By Person-Month

	Pre-Reform			Post-Reform		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Log Monthly Earnings	7.013	0.560	183530	7.039	0.652	346009
Employer Change (x100)	0.192	4.375	177843	0.694	8.303	336812
Exit UAE (x100)	4.863	21.51	192916	3.370	18.05	358088
Stayer	0.973	0.162	193977	0.962	0.192	361481
Construction	0.705	0.456	144520	0.565	0.496	217704
Age	36.68	8.303	193977	35.89	8.415	361481
Muslim	0.400	0.490	184293	0.434	0.496	347479
Male	0.998	0.0390	193977	0.994	0.0794	361466
Educated	0.276	0.447	181579	0.388	0.487	355062
Dubai-Sharjah	0.652	0.476	193965	0.604	0.489	361481
Indian	0.507	0.500	190627	0.446	0.497	358971
Hours (Lowerbound)	254.4	50.66	100246	238.2	54.62	202269
Hours (Upperbound)	264.3	60.67	100246	244.9	65.46	202269

Table 2: Effect on Log Earnings

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform	0.157*** (0.00724)	0.160*** (0.00785)	0.135*** (0.00999)	0.0715*** (0.00824)	0.110*** (0.00745)	0.0965*** (0.00915)	0.185*** (0.00913)	0.166*** (0.0116)
Lag 2 Contract Expire X Post Reform	0.138*** (0.00587)	0.141*** (0.00635)	0.125*** (0.00808)	0.0584*** (0.00661)	0.0994*** (0.00602)	0.0903*** (0.00738)	0.162*** (0.00740)	0.152*** (0.00938)
Lag 1 Contract Expire X Post Reform	0.109*** (0.00449)	0.111*** (0.00487)	0.101*** (0.00621)	0.0487*** (0.00510)	0.0804*** (0.00458)	0.0746*** (0.00564)	0.127*** (0.00567)	0.122*** (0.00721)
Contract Expire X Post Reform	0.0118*** (0.00325)	0.0131*** (0.00357)	0.0111** (0.00459)	0.0254*** (0.00390)	0.0681*** (0.00321)	0.0675*** (0.00403)	0.0162*** (0.00415)	0.0175*** (0.00536)
Lag 3 Contract Expire X Pre Reform	0.0175* (0.00943)	0.0228** (0.00894)	0.00412 (0.0103)	0.00503 (0.00897)	0.00268 (0.00973)	-0.0114 (0.00936)	0.0143 (0.0115)	0.00178 (0.0115)
Lag 2 Contract Expire X Pre Reform	0.0199** (0.00773)	0.0242*** (0.00732)	0.00719 (0.00844)	-0.00471 (0.00738)	0.00270 (0.00798)	-0.00930 (0.00772)	0.0149 (0.00933)	0.00189 (0.00937)
Lag 1 Contract Expire X Pre Reform	0.0316*** (0.00586)	0.0347*** (0.00558)	0.0218*** (0.00641)	0.00805 (0.00560)	0.0150** (0.00605)	0.00704 (0.00583)	0.0311*** (0.00716)	0.0183** (0.00717)
Contract Expire X Pre Reform	-0.00214 (0.00403)	-0.000116 (0.00396)	-0.00459 (0.00455)	-0.00535 (0.00402)	0.0193*** (0.00408)	0.0143*** (0.00403)	-0.000775 (0.00494)	-0.00213 (0.00509)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_0^{Pre}}{3}$)	0.112*** (0.00938)	0.110*** (0.00939)	0.109*** (0.0113)	0.0567*** (0.00956)	0.0899*** (0.00963)	0.0917*** (0.0103)	0.138*** (0.0116)	0.139*** (0.0129)
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.0140*** (0.00516)	0.0132** (0.00533)	0.0156** (0.00646)	0.0307*** (0.00560)	0.0488*** (0.00517)	0.0531*** (0.00569)	0.0170*** (0.00646)	0.0197*** (0.00739)
N	529544	529544	342584	342584	463308	306015	447427	288225
Number of Clusters	111019	111019	69253	69253	88290	57023	105613	66210
R-squared	0.017	0.017	0.023	0.087	0.010	0.015	0.019	0.024

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. ***, **, * denotes significance at the 1, 5, and 10% levels, respectively. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table 3: Effect on Hours Variables

	Hours Upper Estimate		Hours Lower Estimate	
	(1)	(2)	(3)	(4)
Lag 3 Contract Expire X Post Reform	-1.001 (0.911)	0.266 (1.410)	-0.834 (0.759)	0.221 (1.175)
Lag 2 Contract Expire X Post Reform	-1.108 (0.729)	0.162 (1.128)	-0.923 (0.607)	0.135 (0.940)
Lag 1 Contract Expire X Post Reform	-0.374 (0.551)	0.446 (0.851)	-0.312 (0.459)	0.372 (0.709)
Contract Expire X Post Reform	-0.0486 (0.386)	-0.470 (0.595)	-0.0405 (0.322)	-0.392 (0.496)
Lag 3 Contract Expire X Pre Reform	0.0524 (1.248)	-0.189 (1.637)	0.0437 (1.040)	-0.158 (1.364)
Lag 2 Contract Expire X Pre Reform	0.311 (1.018)	-0.530 (1.331)	0.259 (0.849)	-0.442 (1.109)
Lag 1 Contract Expire X Pre Reform	1.565** (0.764)	1.685* (1.012)	1.304** (0.636)	1.404* (0.843)
Contract Expire X Pre Reform	0.331 (0.525)	0.314 (0.689)	0.276 (0.437)	0.262 (0.574)
Polynomials in Time to Reform	No	Yes	No	Yes
Worker Characteristics	No	Yes	No	Yes
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	-1.470 (1.193)	-0.0306 (1.678)	-1.225 (0.994)	-0.0255 (1.399)
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	-0.380 (0.649)	-0.785 (0.910)	-0.317 (0.541)	-0.654 (0.759)
N	302524	186848	302524	186848
Number of Clusters	72916	44309	72916	44309
R-squared	0.005	0.013	0.005	0.013

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses.

Table 4: Effect on Employer Change

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform	0.996*** (0.104)	0.904*** (0.114)	0.525*** (0.116)	0.491*** (0.0997)	0.510*** (0.101)	0.338*** (0.0946)	0.832*** (0.129)	0.444*** (0.150)
Lag 2 Contract Expire X Post Reform	1.341*** (0.0933)	1.266*** (0.102)	0.678*** (0.106)	0.678*** (0.0923)	0.966*** (0.0922)	0.562*** (0.0879)	1.196*** (0.116)	0.579*** (0.143)
Lag 1 Contract Expire X Post Reform	1.275*** (0.0801)	1.220*** (0.0884)	0.642*** (0.0879)	0.685*** (0.0766)	0.921*** (0.0790)	0.520*** (0.0719)	1.112*** (0.0988)	0.544*** (0.117)
Contract Expire X Post Reform	0.754*** (0.0599)	0.718*** (0.0666)	0.449*** (0.0612)	0.433*** (0.0535)	-0.279*** (0.0412)	-0.105*** (0.0364)	0.713*** (0.0762)	0.416*** (0.0755)
Lag 3 Contract Expire X Pre Reform	0.627*** (0.112)	0.526*** (0.0731)	0.473*** (0.0877)	0.423*** (0.0762)	0.533*** (0.116)	0.381*** (0.0761)	0.516*** (0.112)	0.333*** (0.0996)
Lag 2 Contract Expire X Pre Reform	0.546*** (0.0928)	0.463*** (0.0628)	0.350*** (0.0674)	0.343*** (0.0596)	0.472*** (0.0961)	0.286*** (0.0585)	0.400*** (0.0894)	0.215*** (0.0724)
Lag 1 Contract Expire X Pre Reform	0.439*** (0.0722)	0.378*** (0.0527)	0.301*** (0.0571)	0.302*** (0.0499)	0.378*** (0.0742)	0.225*** (0.0488)	0.335*** (0.0730)	0.199*** (0.0657)
Contract Expire X Pre Reform	0.270*** (0.0494)	0.231*** (0.0406)	0.154*** (0.0398)	0.146*** (0.0351)	0.0312 (0.0423)	-0.0348 (0.0224)	0.174*** (0.0496)	0.0817*** (0.0405)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No

Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)

Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)

N

Number of Clusters

R-squared

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table 5: Effect on Exits from UAE

	Full Sample			Both Sides			Trimmed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag 3 Contract Expire X Post Reform	9.447*** (0.190)	9.605*** (0.198)	5.500*** (0.272)	5.551*** (0.250)	7.392*** (0.180)	4.591*** (0.234)	9.052*** (0.242)	5.299*** (0.307)
Lag 2 Contract Expire X Post Reform	11.34*** (0.174)	11.47*** (0.184)	6.954*** (0.293)	6.951*** (0.271)	9.746*** (0.170)	6.214*** (0.265)	10.76*** (0.218)	6.653*** (0.325)
Lag 1 Contract Expire X Post Reform	9.171*** (0.144)	9.265*** (0.155)	5.561*** (0.227)	5.570*** (0.211)	7.297*** (0.142)	4.495*** (0.194)	8.580*** (0.180)	5.129*** (0.250)
Contract Expire X Post Reform	4.242*** (0.101)	4.302*** (0.110)	2.438*** (0.137)	2.458*** (0.121)	-0.0921* (0.0514)	-0.0919 (0.0569)	3.958*** (0.129)	2.172*** (0.154)
Lag 3 Contract Expire X Pre Reform	12.59*** (0.292)	13.14*** (0.300)	11.81*** (0.350)	11.39*** (0.301)	9.268*** (0.279)	8.952*** (0.298)	12.74*** (0.391)	7.538*** (0.457)
Lag 2 Contract Expire X Pre Reform	16.31*** (0.281)	16.77*** (0.297)	14.32*** (0.317)	14.09*** (0.276)	13.97*** (0.281)	12.33*** (0.280)	15.73*** (0.366)	9.220*** (0.492)
Lag 1 Contract Expire X Pre Reform	14.27*** (0.236)	14.60*** (0.252)	12.32*** (0.267)	12.25*** (0.235)	11.93*** (0.236)	10.49*** (0.237)	14.32*** (0.313)	8.333*** (0.426)
Contract Expire X Pre Reform	6.030*** (0.156)	6.241*** (0.167)	5.611*** (0.178)	5.507*** (0.157)	0.111 (0.0780)	0.463*** (0.0804)	6.010*** (0.205)	3.882*** (0.228)
Polynomials in Time to Reform	No	Yes	Yes	Yes	No	Yes	No	Yes
Worker Characteristics	No	No	Yes	Yes	No	Yes	No	Yes
Lagged Earnings	No	No	No	Yes	No	No	No	No
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_0^{Pre}}{3}$)	-4.405*** (0.272)	-4.725*** (0.287)	-6.812*** (0.370)	-6.552*** (0.330)	-3.577*** (0.256)	-5.491*** (0.315)	-4.798*** (0.362)	-2.670*** (0.502)
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	-1.788*** (0.184)	-1.939*** (0.200)	-3.173*** (0.224)	-3.049*** (0.198)	-0.204** (0.0903)	-0.555*** (0.0984)	-2.052*** (0.241)	-1.709*** (0.271)
N	550989	550989	356248	356248	477741	315506	465384	299737
Number of Clusters	111332	111332	69457	69457	88290	57023	106800	66998
R-squared	0.084	0.084	0.351	0.351	0.092	0.357	0.083	0.448

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses. The full sample includes all months from January 2009 to October 2012. The both sides sub-sample restricts attention to workers with wage observations both before and after the reform. The trimmed sub-sample excludes the last quarters of 2010 and 2012 and the first quarter of 2009 and 2011.

Table 6: Effect on Imputed Outcome Variables

	Earnings Imp. High			Earnings Imp. Low			Emp. Change High			Emp. Change Low		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)				
Lag 3 Contract Expire X Post Reform	0.187*** (0.00715)	0.129*** (0.00714)	0.126*** (0.00713)	0.189*** (0.00714)	0.947*** (0.101)	0.909*** (0.101)	0.903*** (0.101)	0.958*** (0.101)				
Lag 2 Contract Expire X Post Reform	0.173*** (0.00578)	0.103*** (0.00577)	0.0998*** (0.00577)	0.175*** (0.00578)	1.264*** (0.0898)	1.220*** (0.0898)	1.212*** (0.0898)	1.276*** (0.0898)				
Lag 1 Contract Expire X Post Reform	0.136*** (0.00441)	0.0791*** (0.00441)	0.0765*** (0.00440)	0.138*** (0.00441)	1.204*** (0.0765)	1.169*** (0.0765)	1.163*** (0.0765)	1.213*** (0.0765)				
Contract Expire X Post Reform	0.0274*** (0.00319)	0.00130 (0.00320)	0.0000456 (0.00320)	0.0283*** (0.00319)	0.726*** (0.0578)	0.711*** (0.0578)	0.708*** (0.0578)	0.731*** (0.0578)				
Lag 3 Contract Expire X Pre Reform	-0.0180* (0.00931)	-0.0173* (0.00925)	0.0618*** (0.00927)	0.0630*** (0.00935)	0.588*** (0.109)	0.558*** (0.109)	0.625*** (0.109)	0.621*** (0.109)				
Lag 2 Contract Expire X Pre Reform	-0.0271*** (0.00759)	-0.0269*** (0.00754)	0.0748*** (0.00757)	0.0771*** (0.00764)	0.494*** (0.0892)	0.457*** (0.0892)	0.541*** (0.0892)	0.536*** (0.0892)				
Lag 1 Contract Expire X Pre Reform	-0.0117** (0.00573)	-0.0117** (0.00570)	0.0771*** (0.00571)	0.0793*** (0.00577)	0.396*** (0.0687)	0.364*** (0.0688)	0.435*** (0.0688)	0.432*** (0.0687)				
Contract Expire X Pre Reform	-0.0184*** (0.00395)	-0.0182*** (0.00393)	0.0196*** (0.00394)	0.0203*** (0.00397)	0.253*** (0.0475)	0.239*** (0.0475)	0.271*** (0.0475)	0.269*** (0.0475)				
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_{k, k}^{Post} - \gamma_{k, k}^{Pre}}{3}$)	0.184*** (0.00926)	0.122*** (0.00920)	0.0294*** (0.00922)	0.0941*** (0.00929)	0.646*** (0.102)	0.640*** (0.102)	0.559*** (0.102)	0.619*** (0.102)				
Post Reform X Contract Expire ($\gamma_{10}^{Post} - \gamma_{10}^{Pre}$)	0.0458*** (0.00507)	0.0195*** (0.00505)	-0.0195*** (0.00506)	0.00807 (0.00509)	0.473*** (0.0684)	0.472*** (0.0684)	0.438*** (0.0684)	0.462*** (0.0684)				
N	550965	550965	550965	550965	536093	536093	536093	536093				
Number of Clusters	111331	111331	111331	111331	110349	110349	110349	110349				
R-squared	0.016	0.007	0.011	0.022	0.005	0.005	0.005	0.005				

All specifications include individual, year-month fixed effects and a constant term. Columns 1, 3, 5, and 7 are the coarse bounds described in the text, while Columns 2, 4, 6, and 8 and the finer bounds. Standard errors clustered by individual in parentheses.

Table 7: Heterogeneous Effect on Log Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Stayer	Indian	Educated	Construction	Dubai-Sharjah	Age < 35
Lag 3 Contract Expire X Post Reform	0.155*** (0.00723)	0.199*** (0.0112)	0.145*** (0.0118)	0.180*** (0.0123)	0.0957*** (0.00867)	0.167*** (0.00992)
Lag 2 Contract Expire X Post Reform	0.137*** (0.00585)	0.172*** (0.00909)	0.130*** (0.00954)	0.167*** (0.0101)	0.0785*** (0.00702)	0.148*** (0.00808)
Lag 1 Contract Expire X Post Reform	0.108*** (0.00449)	0.134*** (0.00690)	0.105*** (0.00730)	0.128*** (0.00772)	0.0608*** (0.00535)	0.117*** (0.00620)
Contract Expire X Post Reform	0.0103*** (0.00328)	0.0221*** (0.00500)	0.0174*** (0.00525)	0.0168*** (0.00565)	0.000570 (0.00388)	0.00373 (0.00448)
Lag 3 Contract Expire X Pre Reform	0.0138 (0.00905)	-0.0131 (0.0132)	0.0711*** (0.0190)	-0.00367 (0.0125)	0.000553 (0.0103)	0.0344** (0.0139)
Lag 2 Contract Expire X Pre Reform	0.0170** (0.00742)	-0.00312 (0.0107)	0.0542*** (0.0156)	0.00565 (0.0103)	0.00192 (0.00843)	0.0349*** (0.0114)
Lag 1 Contract Expire X Pre Reform	0.0297*** (0.00564)	0.0148* (0.00816)	0.0716*** (0.0118)	0.0194** (0.00777)	0.0135** (0.00636)	0.0454*** (0.00869)
Contract Expire X Pre Reform	-0.00308 (0.00391)	-0.00850 (0.00561)	0.0214*** (0.00812)	-0.0137** (0.00540)	0.00178 (0.00444)	0.00178 (0.00598)
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	0.1113*** (0.00927)	0.169*** (0.0137)	0.0612*** (0.0175)	0.151*** (0.0140)	0.0730*** (0.0106)	0.106*** (0.0134)
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.0134*** (0.00515)	0.0306*** (0.00752)	-0.00409 (0.00959)	0.0305*** (0.00783)	-0.00121 (0.00594)	0.00194 (0.00739)
N	513828	242938	178712	223779	327785	272745
Number of Clusters	105664	51744	38777	45319	67970	57868
R-squared	0.017	0.023	0.015	0.027	0.011	0.021

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses in parentheses.

Table 8: Heterogeneous Effect on Employment Transitions

	(1)	(2)	(3)	(4)	(5)
	Indian	Educated	Construction	Dubai-Sharjah	Age < 35
Lag 3 Contract Expire X Post Reform	1.193*** (0.139)	2.355*** (0.188)	0.383*** (0.120)	1.432*** (0.112)	1.235*** (0.145)
Lag 2 Contract Expire X Post Reform	1.392*** (0.124)	2.810*** (0.172)	0.540*** (0.108)	1.754*** (0.105)	1.563*** (0.130)
Lag 1 Contract Expire X Post Reform	1.350*** (0.109)	2.460*** (0.145)	0.605*** (0.0958)	1.594*** (0.0906)	1.497*** (0.112)
Contract Expire X Post Reform	0.831*** (0.0820)	1.295*** (0.108)	0.577*** (0.0758)	0.902*** (0.0665)	0.965*** (0.0844)
Lag 3 Contract Expire X Pre Reform	0.398*** (0.103)	1.491*** (0.287)	0.397*** (0.106)	0.456*** (0.0853)	0.696*** (0.192)
Lag 2 Contract Expire X Pre Reform	0.388*** (0.0904)	1.483*** (0.246)	0.240*** (0.0815)	0.451*** (0.0725)	0.631*** (0.161)
Lag 1 Contract Expire X Pre Reform	0.273*** (0.0725)	1.114*** (0.193)	0.187*** (0.0620)	0.369*** (0.0616)	0.540*** (0.124)
Contract Expire X Pre Reform	0.0909* (0.0496)	0.636*** (0.130)	0.135*** (0.0457)	0.203*** (0.0415)	0.270*** (0.0833)
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	0.959*** (0.133)	1.179*** (0.250)	0.235** (0.117)	1.168*** (0.113)	0.809*** (0.169)
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.740*** (0.0941)	0.658*** (0.161)	0.443*** (0.0838)	0.699*** (0.0774)	0.695*** (0.108)
N	236394	173048	219246	321884	263483
Number of Clusters	50932	38230	45050	67595	56831
R-squared	0.007	0.012	0.003	0.008	0.007

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses in parentheses.

Table 9: Heterogeneous Effect on Exits from the UAE

	(1)	(2)	(3)	(4)	(5)	(6)
	Stayer	Indian	Educated	Construction	Dubai-Sharjah	Age < 35
Lag 3 Contract Expire X Post Reform	9.329*** (0.190)	10.69*** (0.331)	11.35*** (0.333)	9.491*** (0.337)	11.06*** (0.258)	9.763*** (0.254)
Lag 2 Contract Expire X Post Reform	11.30*** (0.175)	13.28*** (0.302)	12.86*** (0.299)	11.45*** (0.307)	12.85*** (0.236)	11.76*** (0.232)
Lag 1 Contract Expire X Post Reform	8.896*** (0.145)	10.33*** (0.240)	10.16*** (0.245)	8.649*** (0.248)	9.776*** (0.191)	9.459*** (0.193)
Contract Expire X Post Reform	4.272*** (0.0986)	4.403*** (0.166)	4.992*** (0.172)	4.419*** (0.174)	4.845*** (0.133)	4.349*** (0.135)
Lag 3 Contract Expire X Pre Reform	12.79*** (0.297)	13.23*** (0.449)	13.85*** (0.675)	13.03*** (0.382)	13.70*** (0.379)	14.49*** (0.508)
Lag 2 Contract Expire X Pre Reform	16.61*** (0.286)	18.45*** (0.432)	17.01*** (0.622)	17.60*** (0.375)	18.37*** (0.369)	18.84*** (0.484)
Lag 1 Contract Expire X Pre Reform	14.20*** (0.240)	16.05*** (0.362)	14.77*** (0.508)	15.03*** (0.319)	16.35*** (0.313)	15.50*** (0.390)
Contract Expire X Pre Reform	6.304*** (0.157)	6.933*** (0.236)	6.116*** (0.315)	6.708*** (0.213)	6.986*** (0.199)	6.155*** (0.253)
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	-4.692*** (0.275)	-4.477*** (0.432)	-3.751*** (0.591)	-5.356*** (0.397)	-4.910*** (0.358)	-5.949*** (0.444)
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	-2.032*** (0.183)	-2.530*** (0.284)	-1.125*** (0.357)	-2.289*** (0.271)	-2.141*** (0.236)	-1.806*** (0.284)
N	534351	254562	185953	233381	342067	283987
Number of Clusters	105959	51881	38903	45421	68153	58029
R-squared	0.084	0.101	0.082	0.094	0.093	0.089

All specifications include individual, year-month fixed effects and a constant term. Standard errors clustered by individual in parentheses in parentheses.

Table A.1: Firm Fixed Effects

	Earnings			Earnings (Imp. High)			Earnings (Imp. Low)		
	(1)	(2)	(3)	(4)	(5)	(6)			
Lag 3 Contract Expire X Post Reform	0.161*** (0.00613)	0.133*** (0.00785)	0.170*** (0.00598)	0.149*** (0.00771)	0.146*** (0.00598)	0.121*** (0.00772)			
Lag 2 Contract Expire X Post Reform	0.142*** (0.00505)	0.124*** (0.00647)	0.153*** (0.00492)	0.142*** (0.00635)	0.124*** (0.00493)	0.107*** (0.00636)			
Lag 1 Contract Expire X Post Reform	0.111*** (0.00391)	0.100*** (0.00498)	0.119*** (0.00380)	0.113*** (0.00489)	0.0972*** (0.00380)	0.0870*** (0.00489)			
Contract Expire X Post Reform	0.0132*** (0.00279)	0.0105*** (0.00358)	0.0190*** (0.00272)	0.0192*** (0.00351)	0.00792*** (0.00272)	0.00555 (0.00351)			
Lag 3 Contract Expire X Pre Reform	0.0119 (0.00811)	0.00484 (0.00962)	-0.00336 (0.00784)	-0.0114 (0.00944)	0.0371*** (0.00785)	0.0292*** (0.00945)			
Lag 2 Contract Expire X Pre Reform	0.0153** (0.00665)	0.00812 (0.00789)	-0.00664 (0.00641)	-0.0137* (0.00772)	0.0465*** (0.00642)	0.0382*** (0.00773)			
Lag 1 Contract Expire X Pre Reform	0.0282*** (0.00512)	0.0224*** (0.00607)	0.00460 (0.00493)	-0.000986 (0.00592)	0.0551*** (0.00493)	0.0492*** (0.00593)			
Contract Expire X Pre Reform	-0.00429 (0.00364)	-0.00465 (0.00432)	-0.0127*** (0.00351)	-0.0131*** (0.00421)	0.00872*** (0.00351)	0.00814* (0.00421)			
Polynomials in Time to Reform	No	Yes	No	Yes	No	Yes			
Worker Characteristics	No	Yes	No	Yes	No	Yes			
Firm Fixed-Effects	Yes	Yes	Yes	Yes	Yes	Yes			
Post Reform X Post Contract Expire ($\frac{\sum_{k=1}^3 \gamma_k^{Post} - \gamma_k^{Pre}}{3}$)	0.119*** (0.00797)	0.107*** (0.00969)	0.149*** (0.00774)	0.143*** (0.00951)	0.0763*** (0.00775)	0.0663*** (0.00952)			
Post Reform X Contract Expire ($\gamma_0^{Post} - \gamma_0^{Pre}$)	0.0175*** (0.00461)	0.0151*** (0.00561)	0.0317*** (0.00446)	0.0323*** (0.00547)	-0.000799 (0.00446)	-0.00259 (0.00548)			
N	529544	342584	550965	356234	550965	356234			
Number of Clusters									
R-squared	0.748	0.723	0.259	0.259	0.258	0.259			

All specifications include individual, month, and firm fixed effects. Standard errors clustered by individual in parentheses.

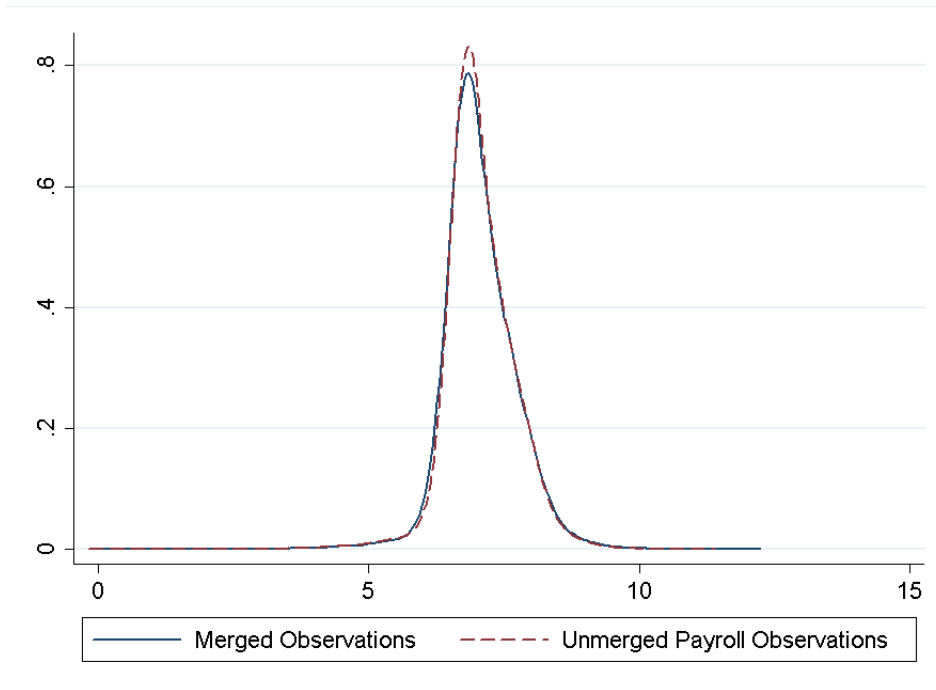


Figure A.1: Density of Matched Payroll-MOL Log Earnings and Unmerged Payroll Log Earnings

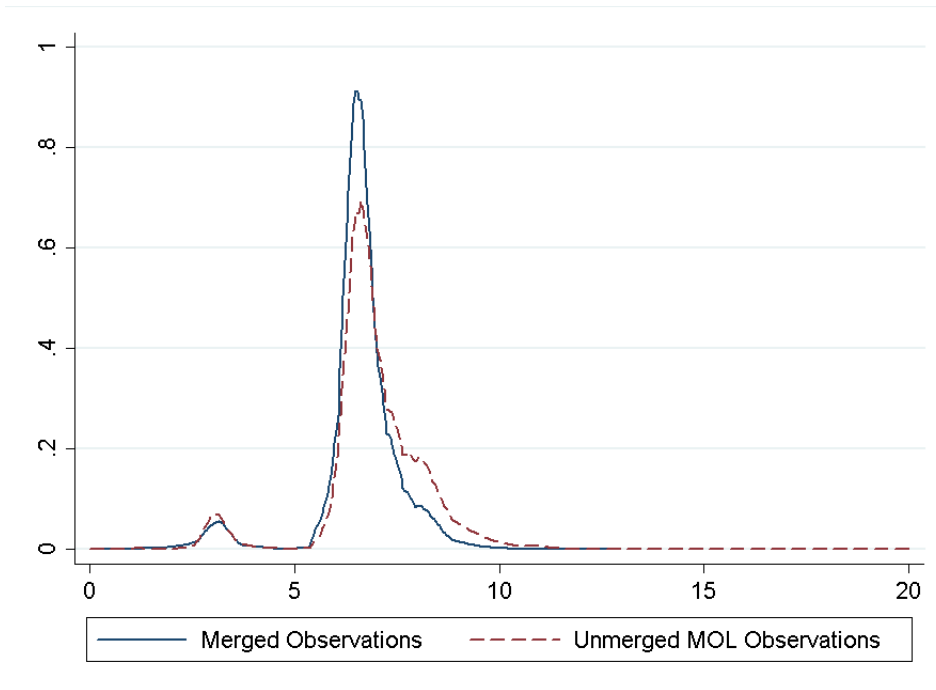


Figure A.2: Density of Matched Payroll-MOL Log Contract Salary and Unmerged MOL Log Contract Salary