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Abstract

Do flexible labor market regulations reduce informal employment? This paper examines the effects of changes in labor regulations on the incidence of formal employment. Using the case of Egypt, we study the effects of the introduction of more flexible labor regulations in 2003, allowing employers to fire workers, on the incidence of formal employment. The change in the labor law provides us with a natural experiment, which can be used to evaluate the impact of such a policy. The findings show that the change in labor law had a positive impact on the incidence of contracted jobs. Thus, our findings support the hypothesis that less rigid labor market regulations increase formal employment.

JEL Classification: J2, J4

Keywords: Labor Market Regulations, Formal and Informal Employment, Labor Law, Egypt

ملخص

هل تقلل لوائح سوق العمل المرنة من العمالة غير الرسمية؟ تبحث هذه الورقة آثار التغيرات في أنظمة العمل على العمل الرسمي. وباستخدام حالة مصر، نقوم بدراسة الآثار المترتبة على تطبيق أنظمة العمل الأكثر مرونة في عام 2003، مما يسمح لاصحاب العمل بفصل العمال، وآثارها على العمل الرسمي. التغيير في قانون العمل يوفر لنا تجربة طبيعية، والتي يمكن استخدامها لتقييم أثر هذه السياسة. وأظهرت النتائج أن التغيير في قانون العمل كان له أثر إيجابي على الوظائف المتعاقد عليها. وبالتالي، النتائج التي توصلنا إليها تدعم الفرضية القائلة بأن لوائح سوق العمل الأقل صرامة تؤدى الى زيادة فرص العمل الرسمي.

1. Introduction

Labor market regulations have significant impacts on the functioning of labor markets affecting the rate of job creation and destruction, the levels of employment and unemployment in the economy, and the degree of social protection provided to workers. However, there is wide disagreement among economists on the benefits of labor market regulations. While some economists believe that regulated labor markets distort labor outcomes, others argue that regulations are needed to protect poor and vulnerable workers.[1]

Labor market regulations may affect labor market outcomes by affecting firms' choices over inputs and by influencing the allocation of resources across firms and sectors of the economy. Thus, the role played by labor regulations, and whether they help or hinder labor reallocation, is important in particular for developing countries since, as argued by Boeri et al. (2008), poorer countries have stricter labor laws compared to richer countries, even though they offer less social protection. Also, strict labor regulations are often associated with a larger informal economy, worse working conditions and poor job quality. Thus, an important issue is whether stricter regulations increase labor informality. This paper aims to examine whether strict labor regulations lead to a larger informal sector. More precisely, it studies whether the introduction of more flexible labor regulations reduces labor informality.

In order to understand better the causal impact of strict labor regulations on informality, we use the introduction of the 2003 labor law in Egypt to study the impact of a more flexible employment protection law on informal employment. The new labor law (No. 12) provided increased flexibility for firms in the hiring/firing process, which is believed to have been a major bottleneck for job creation in the Egyptian labor market.

We use this natural experiment to evaluate the impact of more flexible employment protection legislation on the formal and informal private employment. The paper examines whether flexible labor regulations, such as the 2003 Labor Law, has reduced informality (non-contracted employment) in Egypt. This is an important issue for developing countries, like Egypt, where the informal sector has been growing over the last few decades and has become the main employer in the economy. Informal employment tends to be of low quality with no job security and no social security coverage. From a policy perspective, it is essential to investigate whether a more flexible labor market would encourage the growth of the private formal sector leading to more protected jobs.

We use parametric and non-parametric techniques to estimate the effects of the policy change on the incidence of acquiring job contracts in the private sector. We exploit the temporal change in law and the variation among employers' formality status. In order to estimate the causal impact of the change in law, we adopt the difference in differences approach. To identify the effect, we argue that non-contracted workers, who work for formal or semi-formal employers, where other co-workers are contracted and covered by social security, would be directly affected by the change in law. On the other hand, non-contracted workers working for informal employers, where all other co-workers have no job contracts and no social security coverage, would not be affected by the change in law given the expected cost of formalizing workers and, thus, are used as a comparison group. We confine our analysis to the period between 1998-2008 to construct 5-year periods before and after the law. Our findings indicate that the contracted new jobs have increased compared to the non-contracted new jobs after the introduction of the new law. Thus, our findings support the hypothesis that less rigid labor market regulations increase formal employment.

The structure of the paper will be as follows. Section 2 reviews the previous literature dealing with labor market regulations, focusing on the effect of employment protection regulations.

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[1] See Freeman (1993) for a summary of this debate.

Section 3 provides a brief overview of the Egyptian labor market and describes the 2003 Labor Law. Section 4 examines trends and patterns of informality in Egypt. Section 5 presents the empirical analysis of the impact of the 2003 labor law. Section 6 concludes.

2. Theoretical Background and Literature Review

There is an extensive debate among economists on the benefits of labor market regulations. Some economists believe that unregulated labor markets are more efficient than regulated ones. They argue that labor market regulations introduce distortions that misallocate labor, waste resources through rent seeking, impede adjustments to economic shocks, discourage hiring, and favor "insiders" (such as regular workers or males) and therefore reduce growth. Freeman (1993) refers to that view as the "distortionist." On the other hand, others believe, the "institutionalist" view, that due to market failure and the ensuing injustice and inequity, regulations are needed to protect poor and vulnerable workers.[2]

There are various forms of labor regulations. In general, labor market regulations are introduced with the objective of protecting workers from uninsurable labor market risk, such as *employment* risk, or from *earnings* risk. To improve the earnings of the most disadvantaged categories of workers, governments typically set minimum wages; they might also mandate that employers provide non-wage benefits to their workers, such as healthcare, paid vacation, maternity leave, etc. To protect workers from employment risk, governments can decide to protect existing jobs by restricting the ability of firms to lay off employees at will and/or provide unemployment insurance to those who lose their job (Boeri et al. 2008).

In this paper, the focus is on employment protection rules; namely hiring and firing arrangements affecting job security. These include issues like what types of contracts are allowed, the conditions under which workers' contracts can be terminated, requirements for severance and advance notice of termination, redundancy procedures, and special rules for mass layoffs.

The degree of rigidity of employment protection rules can affect labor market outcomes such as employment levels, employment adjustment, and the composition of employment. Rigid employment rules are expected to lengthen job tenure and reduce labor turnover, protecting the jobs of incumbent employees and limiting hiring opportunities. At the same time, rigid job security rules affect the composition of employment by shifting labor to informal sectors or employment. Obviously, the effectiveness of labor regulation will depend on the extent to which those regulations are enforced.

Betcherman et al. (2001) summarize the theoretical impact of strict termination rules as follows: lower labor turnover rates (hirings plus separations); lower aggregate employment levels; greater numbers of long-tenure jobs; lower labor force participation rates; no clear impact on unemployment levels, but longer average unemployment durations; at a macro level, slower recovery from an aggregate shock; more self-employment as a share of total employment; more non-standard employment (e.g., part-time or temporary); positive employment effects for skilled prime-age males but lower employment for women, young people, and less-skilled workers. They add that the empirical findings are stronger for the dynamic effects on turnover, tenure and flows between employment and unemployment, and in terms of who benefits from employment protection rules and who does not. For example, in Latin America there is evidence of negative employment effects of job security rules.

In the last three decades or so, there has been a move toward labor market flexibility and the introduction of labor market reforms to enhance productivity, competition, and to accelerate employment generation and improve economic performance. Yet the empirical evidence on the effects of labor flexibility is mixed. The main empirical evidence on the effect of labor

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[2] See Freeman (1993) for a summary of this debate.

market flexibility has focused mainly on developed countries, but has more recently included studies on developing countries as well. As argued by Boeri et al. (2008), poorer countries tend to have stricter labor regulations compared to richer countries, even though they offer less social protection. Also, the World Bank in Doing Business (2009) shows that developing countries tend to mistakenly go to the extreme of rigid regulations, pushing employers and workers into the informal sector. Overly rigid regulations may have undesirable effects, such as lower job creation, smaller company size, less investment in research and development, and longer spells of unemployment and thus the obsolescence of skills, all of which may reduce productivity growth. Hence, excessive rigidity can be to the detriment of businesses and workers alike.

A few recent studies have examined the effects of flexible labor market regulations in developing countries. For example, Kingdon et al. (2006) show that the failure of African labor markets to create good paying jobs was the result of lack of labor market "flexibility," which kept formal sector wages above their equilibrium level and restricted job creation. This resulted in excess labor supply in the form of either open unemployment or a growing self-employment sector.

Besley and Burgess (2004) examine the link between regulation and long-term development in India by looking at state amendments to the Industrial Disputes Act of 1947. They find that labor regulation is a key factor in the pattern of manufacturing development in India. Regulating in a pro-worker direction was associated with lower levels of investment, employment, productivity and output in registered manufacturing and with increasing informal sector activity.

Kugler (2004) examines the impact of the Colombian labor market reform of 1990, which substantially reduced the costs of dismissing workers through the reduction of severance payments on unemployment. Using micro-level data from Colombia, she finds that those reforms contributed to 10% of the reduction in unemployment during the period of study.

This paper contributes to this literature by focusing on the case of Egypt and examining the impact of employment protection reforms on informal/formal employment. An earlier study by Wahba (2009) found evidence of positive effects two years after the introduction of the law. However, it is important to examine the long-term effects and the sustainability of those effects. Hence, this paper, using richer data, examines the impact of the flexible employment law in a more rigorous manner using program evaluation techniques to disentangle the impact of the law from confounding factors.

3. The 2003 Labor Law and the Egyptian Labor Market

The Egyptian labor market is highly distorted. In the 1960s, the government passed a law that guaranteed employment to all secondary, technical institutes and university graduates to encourage education and to provide a safety net. However, this led to an overstaffed and inefficient public sector. From the 1960s to the 1980s, the Egyptian public sector was the main creator of employment opportunities and typically the preferred sector by most new entrants to the labor market. The economic reforms of the 1990s curbed new employment opportunities in the public sector and initiated a privatization program of existing public enterprises, but the size of the private formal sector has continued to be small, although it has been growing fairly rapidly. As a result, unemployment rates among the new entrants to the labor market increased. At the same time, the growth of the private formal sector in job creation and absorption has been rather low limited, resulting in an increase of informal employment where jobs are not covered by social insurance or legal employment contracts, see Moktar and Wahba (2000), and Assaad (2007).

With the Egyptian government embarking on various labor market reforms, in 2003 a new labor law (No. 12) was passed with the goal of increasing flexibility in hiring/firing in the private sector. The law, which came into effect in 2004, is comprised of 257 articles that address all the legal aspects regulating the Egyptian labor market by providing comprehensive guidelines for the recruitment, hiring, compensation, and termination of employees.

Until July 2003, existing legislation had been rather stringent both for workers and for employers. It prohibited employers from terminating the contract of a worker after a probation period. The new law addressed the right of an employer to fire an employee and the conditions pertaining to that, as well as granting employees the right to carry out a peaceful strike according to controls and procedures prescribed in the new law. In particular, the 2003 law provided increased flexibility for firms in the hiring/firing process and allowed private sector employers to renew a temporary contract without transforming it automatically into a permanent employment status as was stated in the preceding law. Hence, employers can terminate a contract more easily and layoffs can be justified by difficult economic conditions. In return, workers that have been dismissed, have the right to appeal. However, this law does not apply to public servants of state agencies, including local government units and public authorities, nor to self-employed workers. In addition, a key concession was granted to workers protected by the old legislation; the new law would not diminish previously acquired worker rights obtained from laws, regulations, and internal decisions that were operative prior to the law coming into effect. This suggests that current workers will be grand-fathered under the old rules and that the new rules will only affect new contractual arrangements[3].

Thus, our hypothesis is that given the 2003 labor regulations, one would expect employers to hire and fire workers more easily and hence hire more workers formally. In the following section we test this hypothesis, namely whether the introduction of the new labor law has led to an increase in the formal private employment (i.e., higher incidence of contracted workers).

4. Informalization and Job Contract Holding

4.1 Data

The analysis in this paper is based on the Egypt Labor Market Panel Survey of 2012. The ELMPS 2012 is the third round of a periodic longitudinal survey that tracks the labor market and the demographic characteristics of households and individuals over time. The fieldwork for the ELMPS 2012 was carried out from March to June of 2012.[4] The survey was carried out by the Economic Research Forum (ERF) in cooperation with the Egyptian Central Agency for Public Mobilization and Statistics (CAPMAS)—the main statistical agency of the Egyptian government. The final sample of the ELMPS 2012 was 12,060 households and a total of 49,186 individuals.[5] We do not rely on the panel feature of the dataset but rather on the rich retrospective information. The 2012 survey has a life events calendar that includes detailed information on employment histories and changes in formality status and social insurance coverage. It also provides a rich source of information on labor market conditions of individuals and their job characteristics.

Given that the change in the labor law applies only to the private[6] non-agricultural sector and only waged workers may hold contracts (i.e., self-employed workers and employers do not

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[3] See Assaad (2004) for further details on the law.
[4] See Assaad and Krafft (2014).
[5] See Assaad and Krafft (2014) for detailed discussion of ELMPS 2012.
[6] Public sector employees are protected: they hold job contracts and have social security coverage, in addition to other benefits.

hold job contracts), the focus of this paper will be on private non-agricultural waged work. The aim of the paper is to test whether the 2003 labor law affected the incidence of job contract acquisition among the private non-agricultural waged workers. For the rest of the paper, informality refers to lack of job contract while formality refers to holding a job contract, unless otherwise mentioned.

4.2 Informality patterns and trends

First, in order to set scene and place the group of interest with respect to the labor market, we present the patterns and trends of the distribution of employment by institutional sector between 1998 and 2012. Figure 1 shows that the share of formal, private regular work has increased between 1998 and 2012 from 6 % to 10%.[7] Also, informal, private regular work has been stable between 2006 and 2012 at 14%.

Examining the incidence of informality (defined as lack of job contract and social security coverage), between 1998 and 2012, we can see a slight decline in 2006, but a larger decline by 2012 for regular waged work. As for the share of those not holding contracts, this has decreased between 1998 and 2012, but more so for the private regular waged workers. Overall, Table 1 suggests that informality has declined since 1998. In particular, there has also been an increasing trend in the job contract holding rate suggesting that the change in labor regulations might have had a positive impact.

In the analysis below we focus on the period 1998-2008, for two reasons. Firstly, we want to construct two periods of equal length before and after the change in law. The law was announced in 2003 and became effective in 2004. Thus, our pre-law period is 1998-2002 and the post-law period is 2004-2008. We do not include 2003 because of anticipation effects, though our results are robust to including 2003 in the pre-policy period. Secondly, given the political changes in Egypt in 2011, we limit our focus to well before they took place.

Table 2 shows the proportion of acquired contracts in jobs that started between the years 1998-2002 relative to those jobs that started during the period 2004-2008. There is evidence that the 2003 labor law has had an effect on the proportion of acquiring job contracts by 2 to 3 percentage points. Interestingly, this pattern is witnessed not only among workers moving jobs and changing employers but also among those who stay with the same employer. Around 13% of those who acquired contracts having previously had no contracts gained the contract with the same employer. Although two thirds have gained contracts with the same employer, between 2004-2008, only a third did so in the earlier period (1998-2002). Also, the increase in contract acquisition after the change in law is seen for new entrants to the labor market employed for the first time.

When examining existing workers in the regular waged sector regardless of when they started their jobs, a similar pattern is observed: the share of contracted regular workers increased from 33% to 35 % between those two periods – Table 3. A similar increase is also seen for all (regular and irregular) waged workers in the post-law period. Thus, overall the evidence suggests that there has been an increase in contracted jobs after the law compared to before. However, in the following sections we examine this impact controlling for workers' characteristics first and then we also estimate the causal impact of the law using treatment effects and taking into account possible time trends and/or business cycle effects.

It is important to note that acquisition of a job contract not only implies job security but also other important benefits, the most important of which is social security coverage. Almost 92% of contracted workers have social security coverage, compared to less than 9% among non-contracted workers. Furthermore, a similar proportion (92%) of contracted workers have

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[7] Regular waged work refers to permanent or temporary work, whilst irregular waged work refers to seasonal and casual work. All work includes both regular and irregular waged work.

paid vacation and sick leave, whilst amongst non-contracted workers only 5% have paid vacation and 9% have sick leave. Hence, the attraction of a job contract goes well beyond job security.

5. Simple Empirical Analysis

To examine the effect of the labor law on formal employment (jobs with contracts), we begin by three simple empirical analyses as follows.

5.1 Probability of acquiring a contract in a new job: before & after the law

To study the effect of the change in labor law, we estimate the probability of acquiring contracts in new jobs, between 1998-2008, as follows:

$$N_{it} = \alpha_{it} + \tau \ X_{it} + \lambda L_{it} \quad + \mu_{it} \tag{1}$$

We limit our sample to all new jobs that started between 1998 and 2008. N_{it} is a new job of individual i at time t and $N_{it}=1$ if the job is contracted and 0 if not. L refers to the time period and it is L=1 for the post policy period if the job started in 2004-2008 and L=0 if the job started in 1998-2002. X is a vector of the worker's characteristics such as gender, education, age and region. Table 4 shows the results for all new waged jobs. The first two columns refer to new jobs involving changing employer. Columns 3 & 4 are new jobs for new labor market entrants. The last two columns show all new jobs regardless of whether they are new jobs with a new or the same employer. As seen, the effect of the labor law captured by the dummy for 2004-2008 is positive and significant throughout.

5.2 Probability of having a contracted job: before & after the law

We then examine the probability of a worker holding a job contract before and after the law regardless of the starting date of the job. We create an unbalanced panel of those employed in the private waged sector at any point of time between 1998 and 2008, and estimate the probability of holding a contracted job using the following linear probability model:

$$J_{it} = \alpha_{it} + \rho \ X_{it} + \sigma L_{it} \quad + \eta_{it} \tag{2}$$

Where J_{it} refers to the waged job of individual i at time t and $J_{it}=1$ if the job is contracted and $J_{it}=0$ if not. L captures the effect of the labor law and L=1 if t=2004-2008 (post policy period) and L=0 if t=1998-2002 (pre-policy period). X is a vector of the worker's characteristics such as gender, education, age and region. We estimate both random effects (RE) models and fixed effects (FE) models where we control for the individual's characteristics. Table 5 shows the determinants of holding a job contract in the waged and regular waged sectors. In both sectors, and using both RE & FE suggest that the probability of holding a contracted job is higher post the law compared to before as captured by the 2004-2008 dummy which is positive and significant throughout.

To sum up, the analysis so far suggests that there has been an increase in the incidence of job holding after the change in the law. However, in order to disentangle the effect of the law from other confounding factors, such as macro and business cycle effects, we use the below policy evaluation techniques to pinpoint the causal impact of the policy change.

6. Policy Evaluation: The Effect of the Change in Labor Law

6.1 Difference in difference: set-up

To test whether the change in the labor law has increased the incidence of acquiring job contracts, we use difference in differences (DiD) estimation. We compare the two periods before and after the change in law. To be more specific, we study those who started a job but have no contract and investigate whether they have acquired a contracted job up to 5 years later. We observe two groups of workers in the private non-agriculture waged sector: the first group includes those who started a job between 1996-1998 and examine whether in the

subsequent 5 years (1998-2002) they have acquired contracts. The second group includes those who started non-contracted jobs in 2002-2004 and examine whether in the subsequent 5 years (2004-2008) they have acquired contracts. It is important to note here that we set up our DiD in a way that allows us to compare the change into a contracted job in two periods to control for confounding factors related to transition from a non-contract state to a contract state. The effect of the law is captured by the difference in those two periods' transition rates. For example, if the transition from no contract to a contract, during a five-year period, is high regardless of the change in policy, this set-up would enable us to measure the additional effect of the law on top of the usual transition rate. We construct an unbalanced panel of annual job statuses and characteristics for both groups.

6.2 Identification strategy

In order to identify the impact of the policy change, the empirical challenge here is to find a group that is unaffected by the change in the law. We identify two types of non-contracted workers based on their employer & co-workers. The first type (F) is those non-contracted workers who work for formal/semi-formal employers, where other co-workers are contracted and are covered by social security. Those workers (F) we argue, would be directly affected by the change in law since their co-workers are already formalized, which implies that the marginal cost for the employer is relatively low (the social security coverage of the new contracted worker). The second type of workers (I) is those non-contracted workers working for informal employers, where all other co-workers have no job contracts and no social security coverage. We argue that the I workers would not be affected by the change in law given the expected cost of formalizing all workers, and also the economic activity conducted by the I employer (in terms of taxes, book-keeping, etc.). Thus, we use I workers as a comparison group.

In order to check the common trend assumption, Figure A1 shows the trends in job creation in the F and I sectors. As shown, both groups have had very similar patterns, albeit the number of I jobs is much higher than the number of F jobs. It is reassuring that using I as a comparison group is justified.

6.3 Difference in differences: panel analysis

We estimate the following model:

$$C_{it} = \alpha_{it} + \beta T_{it} + \partial P_{it} + \delta T_{it} P_{it} + \phi X_{it} + \varepsilon_{it} \tag{3}$$

Where our outcome of interest C_{it} is contract holding of individual i at time t, $C_{it}=0$ when I does not hold a contract at time t and $C_{it}=1$ otherwise. T refers to Treatment. We define the treated group as those working for formal/semi-formal employers (T=1) whilst the comparison group are those employed by informal employers (T=0). P stands for Post and refers to the post policy change period, and gets the value P=1 for 2004-2008 and P=0 for 1998-2002. Our Difference-in-Differences estimate of the effect of the law is captured by the coefficient δ of the interaction term TP. X is a vector of individual characteristics, macroeconomic variables such as annual GDP growth rates and annual unemployment rates, and time trend dummies capturing the time since the job started. A fundamental assumption here is that trends are the same for the treatment and the control group. It is important to note that there might still be general equilibrium effects affecting I jobs and hence our estimate measures the average treatment effect.

Figure A2 presents graphically the impact of the law. The orange line is the number of new jobs in I, the control group. The blue line is our treatment group (the number of new contracted jobs in F). The grey line is what would have happened to the treatment group in the absence of the law if they grew at the same level as the control group (i.e., at 16%). Note that in Figure A1, we do not distinguish between contracted and non-contracted F jobs, but in

Figure A2, we do. Hence, the distance between the grey and the blue lines is δ and although it appears rather small, this is due to the scale. In absolute term, this is 44% increase for the Treatment group between before and after the law.

Table 6 presents the estimates of the difference in differences models. Panel A shows the estimates without any covariates. Panel B controls for the individual characteristics. Panel C shows the estimates controlling for individual and macroeconomic trends and Panel D the estimates controlling for time trends as well. The estimates show that the effect of the law, δ (captured in the last column by the Diff in Diff), is positive and significant. In fact controlling for individual characteristics increases the magnitude of the law, but unsurprisingly controlling for the macroeconomic trends and time trends reduces that magnitude but not substantially.

To sum up, our various tests support our hypothesis that the law had a positive impact on contract holding. This result is quite robust.

7. Further Robustness: Using Matching Difference in Difference Estimator

In order to check the robustness of our results we conduct one further exercise as follows. We use a Matching Difference in Difference (MDiD) estimator. Matching estimators evaluate the effects of a policy change by comparing outcomes for treated persons to untreated individuals of similar observed characteristics in a comparison group. First, a propensity score matching is used and is based on workers' characteristics at baseline (pre-policy). Matching of the treated (T=1 if F=1) and non-treated (T=0, if I=1) is based on their estimated propensity scores, and is estimated using a probit propensity score equation including all the controls mentioned above as regressors. Then, the difference in difference compares the change in outcomes for the treated to the change in outcomes for the comparison group where the change is measured relative to some pre-policy benchmark time. The MDiD allows for time invariant unobservable differences between treatment and comparison group individuals. The estimates are the average treatment effect on the treated (ATT).

Our focus here is on those workers who started a job without a contract between 2002-2004. This is the pre-policy benchmark time used for the matching of the treated and non-treated groups. Then, we estimate a difference in differences model, where the outcome is moving from no contracted job in 2002-2004, to a job with contract, between 2004-2008, by observing annual data on job contract holding for 2004-08.

Let t and t' be two time periods, where t=2002-2004 before the change in law and t'=2004-2008 after the change in law. Y_{0t} is the outcome observed at time t where, by design, we choose those with no contracted job at time t. The conditions needed to estimate the MDiD are:

(DID.1) $E(Y_{0t} - Y_{0t'}|P(X), D = 1) = E(Y_{0t} - Y_{0t'}|P(X), D = 0)$
(DID.2) $0 < \Pr(D = 1|X) < 1$

Under these conditions, $\Delta_{D=1}$ can be estimated by:

$$\hat{\Delta}_{D=1}^{DID} = n_{1t}^{-1} \sum_{\substack{i=1 \\ \{D_i=1\}}}^{n_{1t}} \{Y_{1ti}(X_i) - \hat{E}(Y_{0ti}|P(X_i), D_i = 0)\} -$$
$$n_{1t'}^{-1} \sum_{\substack{j=1 \\ \{D_j=1\}}}^{n_{1t'}} \{Y_{0t'j}(X_j) - \hat{E}(Y_{0t'j}|P(X_j), D_j = 0)\}$$

Where n_{1t} and $n_{1t'}$ are the number of observations in period t and t' respectively.

The propensity score estimates are shown in Table A1. Kernel matching is reported, but radius matching within 0.005 is also used as robustness (not reported). Kernel Matching defines a neighborhood for each treated observation and constructs the counterfactual using all control observations within the neighborhood, not only the closest observation. It assigns a positive weight to all observations within the neighborhood while the weight is zero otherwise. As suggested by Rosenbaum and Rubin (1985), a test based on the comparison of means for each covariate between treated and matched controls is performed. T-tests of the means of covariates across the treatment and matched controls reveal that matching achieved covariate balance since the difference in means is not too large. The tests are shown in table A2. All treatment and control observations are on the common support and used in the matching.

As a robustness check, we also estimate MDiD for the period before the policy change. In that case, the matching benchmarking is based on 1996-98 and the outcome is moving from no contract job in 1996-1998 to a job with a contract between 1998-2002. As expected, the estimated difference between the treatment and comparison group is zero (i.e. there is no effect). Thus, the MDiD provides further support that the change in the labor law has a positive impact on holding contracted jobs.

8. Conclusion

This paper examines the effect of a change in employment protection legislation on formal employment. We use the case of Egypt, where labor market informality is substantial and the introduction of a new labor law in 2003 enables us to study the impact of more flexible labor market regulations on formal employment measured as jobs with contracts.

We exploit the temporal change in the law and the variation among the employees' formality status. We use various techniques to estimate the causal impact of the change in the labor law. We confine our analysis to the period between 1998-2008 to construct 5-year periods before and after the law. Our simple probability models show an increase in the incidence of holding contracted jobs after the law was implemented compared to the previous period.

We also use difference in differences methods and matching with difference in differences to disentangle the causal impact of the law. In order to find a comparison group, we argue that non-contracted workers who worked for formal/semi-formal employers where other co-workers are contracted and covered by social security would be directly affected by the change in law. On the other hand, those non-contracted workers working for informal employers where all other co-workers have no job contracts either and no social security coverage would not be affected by the change in law given the expected cost of formalizing workers and thus are used as a comparison group.

Our findings indicate that the incidence of acquiring contracts in new jobs has increased after the introduction of this law. The findings suggest that the change in law has had a positive impact on formal employment and has reduced informal work measured as jobs without contracts. Thus, our findings support the hypothesis that less rigid labor market regulations increase formal employment.

The findings are encouraging since they indicate that labor flexibility increases formal employment. These results should encourage further labor reforms to increase flexibility in the labor market, such as reducing the social security contribution by employers and workers to attempt to reduce informalization and achieve decent employment for unprotected workers. However, policy-makers must recognize that labor regulation is only one part of the broader economic policy framework. Its interaction with the regulation of product markets, macroeconomic policy, and the business investment climate will determine the overall labor market performance.

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Figure 1: Distribution of Employment by Institutional Sector, Currently Employed, Ages 18-59, 1998-2012
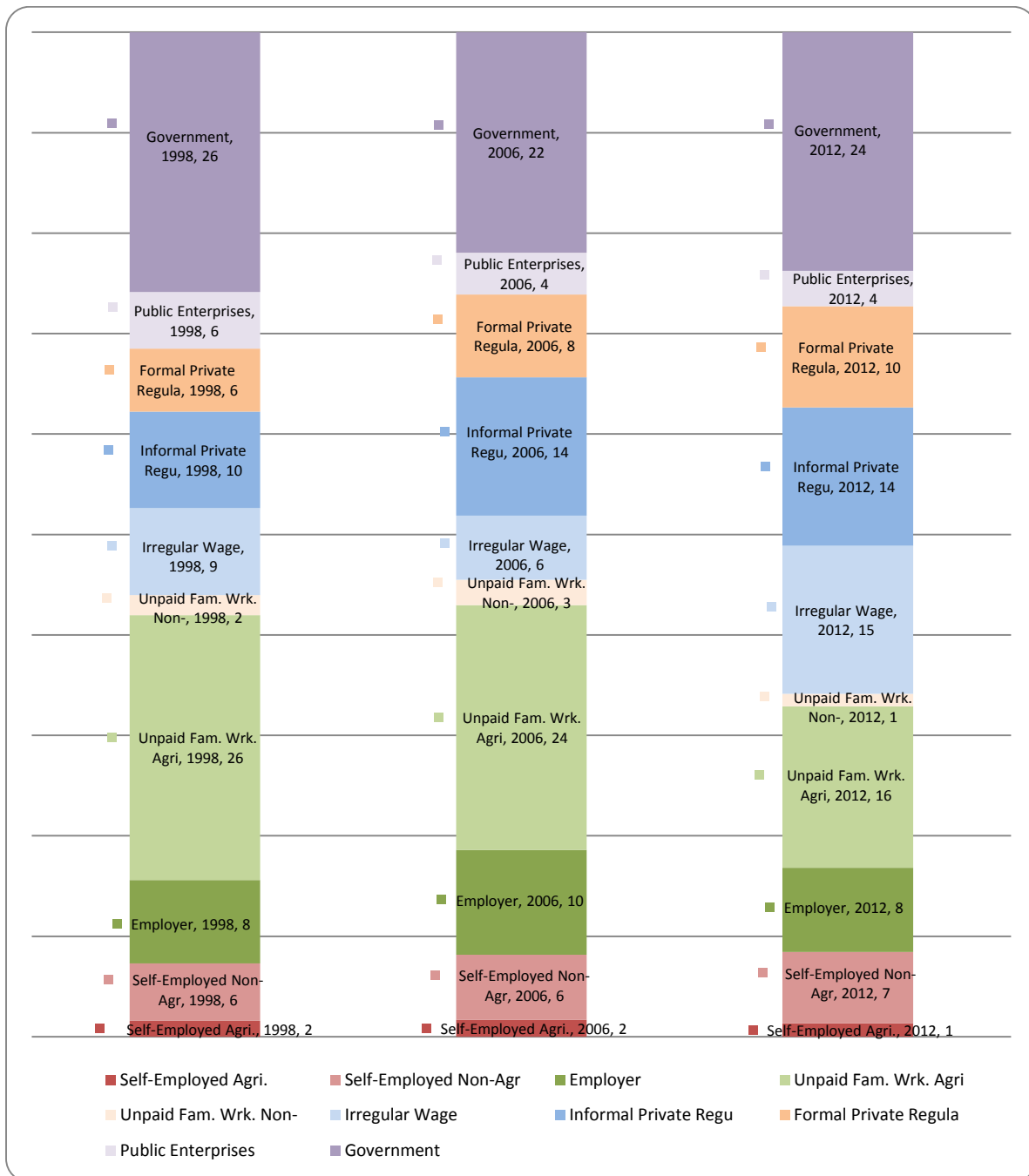
Table 1: Informality (%) 1998 – 2012 (18-59 years of age)

	Informal Employment			No Contract Holding		
	1998	**2006**	**2012**	**1998**	**2006**	**2012**
Private Non-Agriculture Waged[1]	67.9	66.1	67.7	78.5	73.9	75.3
Private Non-Agriculture Regular Waged[2]	61.5	61.6	56.1	71.5	68.7	64.0

Note: **[1]**PNAW: includes all waged work, permanent, temporary, seasonal and intermittent employment. [2]PNARW: includes regular waged work only: both permanent and temporary.

Table 2: Percentage Acquiring Contract in New Jobs: 1998-2002 vs 2004-2008

	1998-2002	**2004-2008**
New Employer		
Regular waged jobs	30.1	33.0
Regular waged jobs: First Jobs	37.4	39.5
Waged jobs	24.5	27.3
Waged jobs: First Jobs	32.8	35.0
Same or New Employer		
Regular waged jobs	31.3	34.4
Waged jobs	25.5	28.6

Note: *Contracts for jobs that started during 1998-2002 (col 1) or 2004-2008 (col 2).

Table 3: Percentage of Job Contract Holding: 1998-2002 vs 2004-2008

	1998-2002	**2004-2008**
Regular Waged Jobs	32.9	35.2
Waged Jobs	24.5	26.1

Table 4: Probability of Acquiring Contract in New Jobs 1998-2008

	New Jobs: New Employer		New Jobs: First Jobs		New Jobs: Same or New Employer			
	Regular Waged	**All Waged**	**Regular Waged**	**All Waged**	**Regular Waged**	**All Waged**	**Regular Waged**	**All Waged**
Job started 2004-08	0.034**	0.024 *	0.067**	0.075***	0.046***	0.036***	0.033**	0.0240*
	(2.17)	(1.90)	(1.98)	(2.51)	(2.95)	(2.27)	(2.17)	(1.87)
Same Employer	----	----	----	----	----	----	0.492***	0.479****
							(6.71)	(7.37)
Same Employer 2004-08	----	----	----	----	----	----	0.108	0.187**
							(1.28)	(2.44)
R[2]	0.066	0.081	0.065	0.092	0.057	0.073	0.107	0.125
No of obs	3197	4152	1163	1382	3323	4283	3323	4283

Note: Controlling for gender, education, age and region of residence. t-statistics in parentheses. Columns 1 and 2, 18-55 years of age. Columns 3 & 4, 18-45 years of age.

Table 5: Probability of Holding Job Contract: 1998- 2008

	Regular Waged Job		Waged Job	
	RE	**FE**	**RE**	**FE**
After the law: 2004-2008 dummy	0.020***	0.021 ***	0.018***	0.019 ***
	(15.17)	(14.21)	(15.17)	(15.69)
R[2]	0.102		0.127	
F-statistics		201.9		246.10
No of obs	25921	25938	37285	37317

Note: Controlling for gender, education, and age in RE model. t-statistics in parentheses. Columns 1 and 3 are random effects & Columns 2& 4 are fixed effects. 18 -55 years of age.

Table 6: Difference in Difference Estimation: Determinants of Acquiring Job Contract

| Outcome	Base Line: 1998-2002			Post : 2004-2008			Diff in Diff
	Control	Treated	Diff	Control	Treated	Diff	
Panel A: No controls							
Coef	0.075	0.091	0.017	-0.062	0.114	0.052***	0.036*
t-statistics	14.02	1.36	1.19	-2.43	3.23	2.90	1.89
Panel B: Controlling for Individual Characteristics							
Coef	0.012	0.010	-0.002	-0.008	0.029	0.037***	0.039**
t-statistics	0.72	-0.08	-0.13	-1.18	2.09	3.16	2.10
Panel C: Controlling for Individual Characteristics and Macroeconomic trends							
Coef	0.176	0.175	-0.001	0.178	0.215	0.037***	0.038**
t-statistics	2.58	0.16	-0.08	0.20	0.66	3.13	2.08
Panel D: Controlling for Individual Characteristics, Macroeconomic trends and time trend							
Coef	0.002	0.004	0.002	-0.018	0.018	0.036***	0.034*
t-statistics	0.03	0.03	0.13	-0.25	0.41	2.82	1.87

Note: *N= 6082 in Panel A, N=6060 in Panels B-D. Outcome: Acquiring contract in waged job.

Table 7: Matching Difference in Difference Estimates

	Coefficient (DiD)	Bootstrapped Std Err
Post Policy (2004-2008)	0.0330**	0.0174
Pre Policy (1998-2002)	0.0043	0.0145

Figure A1: Number of New Workers in F and I by Year of Job Start
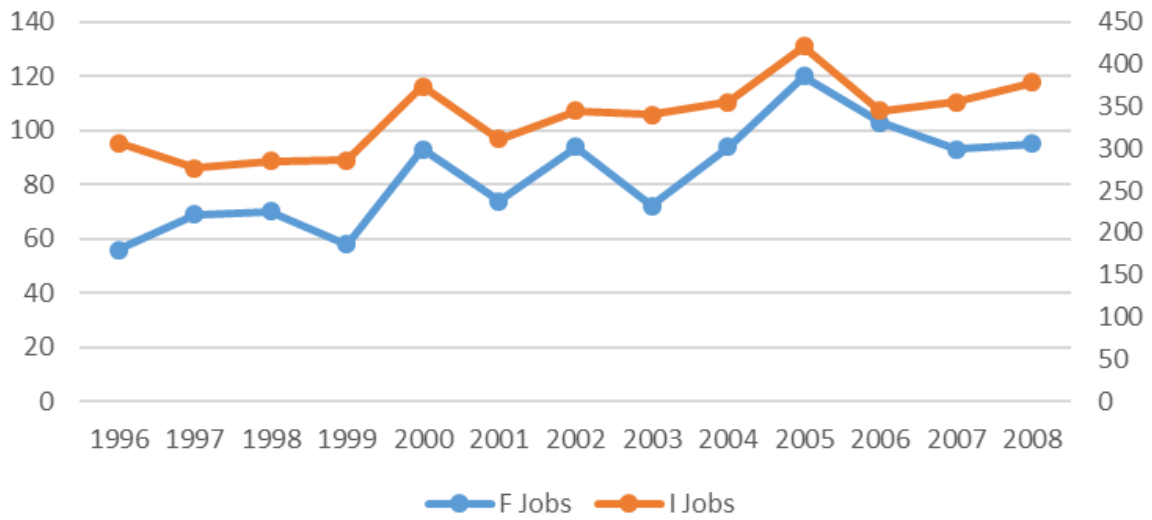
Figure A2: Number of New Workers, T & C: Before and After
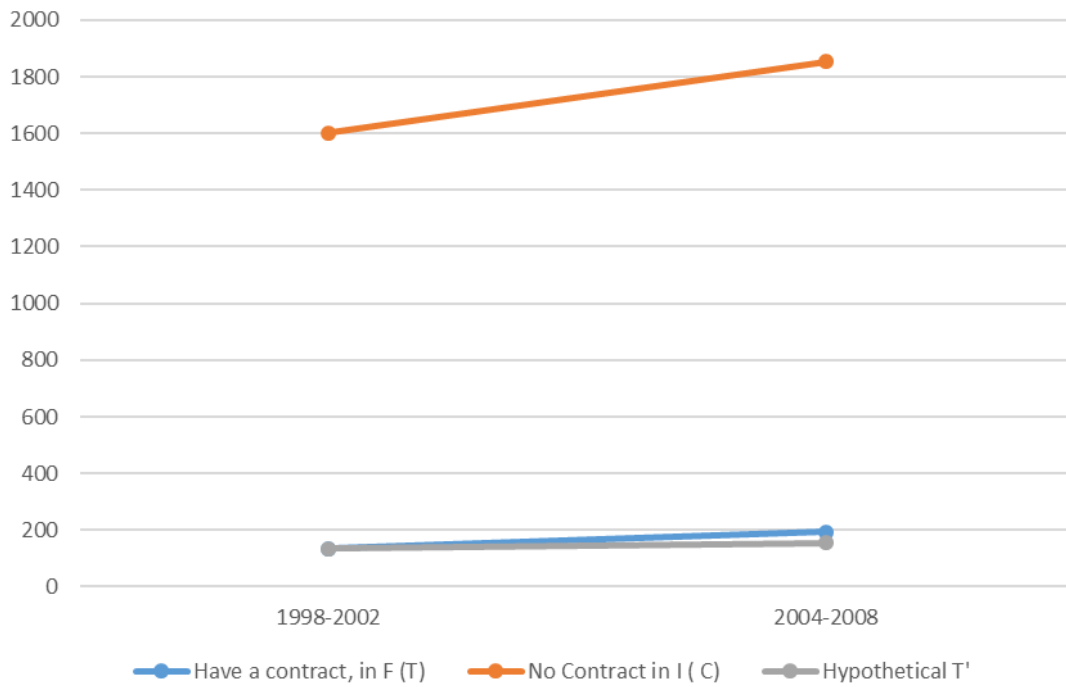
Table A1: Propensity Score Estimates

	Pre-Law		Post-Law	
	Coeff	**t-statistics**	**Coeff**	**t-statistics**
Male	-0.286	-3.39	-0.515	-7.50
Age	0.022	4.79	0.019	4.23
Education				
Less than Primary	-0.337	-1.38	-0.825	-3.25
Primary	0.574	4.1	0.073	0.64
Intermediate	0.920	6.94	0.157	1.55
Secondary School	0.561	2.71	0.110	0.63
University	0.929	6.26	0.381	3.39
Region of residence				
Alex. & Canal Cities	0.100	1.03	-0.185	-2.11
Lower Urban	-0.298	-2.93	-0.566	-5.44
Upper Urban	-0.642	-5.68	-0.428	-4.48
Lower Rural	-0.187	-2.26	-0.349	-4.66
Upper Rural	-0.584	-5.49	-0.549	-6.11

Table A2: T-tests of the Means of Covariates across the Treatment and Matched Controls

	Pre-law					Post-law				
	Mean		**%**	**t-statistics**		**Mean**		**%**	**t-statistics**	
Variable	**Treated**	**Control**	**biased**	**t**	**p>t**	**Treated**	**Control**	**biased**	**t**	**p>t**
Male	0.833	0.849	-5.0	-0.68	0.494	0.748	0.768	-5.3	-0.78	0.437
Age	23.606	23.532	1.1	0.17	0.861	24.313	24.253	1.0	0.17	0.866
Education										
Less than Primary	0.009	0.013	-2.2	-0.6	0.545	0.007	0.002	3.1	1.23	0.219
Primary	0.199	0.206	-1.7	-0.26	0.795	0.161	0.174	-3.3	-0.56	0.574
Intermediate	0.554	0.549	1.1	0.16	0.876	0.479	0.489	-1.9	-0.32	0.747
Secondary School	0.029	0.027	1.5	0.23	0.819	0.034	0.039	-2.9	-0.45	0.655
University	0.179	0.168	3.3	0.43	0.664	0.250	0.234	4.3	0.65	0.516
Region of residence										
Alex. & Canal Cities	0.186	0.172	3.9	0.54	0.592	0.157	0.170	-3.7	-0.57	0.566
Lower Urban	0.120	0.124	-1.1	-0.17	0.863	0.070	0.074	-1.5	-0.28	0.776
Upper Urban	0.068	0.067	0.3	0.05	0.96	0.100	0.102	-0.7	-0.12	0.905
Lower Rural	0.299	0.306	-1.5	-0.22	0.824	0.267	0.260	1.4	0.25	0.806
Upper Rural	0.088	0.084	1.4	0.25	0.806	0.111	0.115	-1.1	-0.21	0.833