Fixed exchange-rate policy and real wage growth: Quasi-experimental evidence

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Abstract

Using Difference-in-Differences estimation and data from the European Community Household Panel, this paper suggests that the fixed exchange-rate policy adopted by Italy in the 1997-2000 period has reduced the real hourly wage growth of Italian full-time workers with permanent contracts, on average, by at least 5.4% in the private sector. The evidence is consistent with a model by Andersen and Sørensen (1988) in which the private-sector unions ask for a lower average exchange-rate risk premium under fixed exchange rates.

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1. Introduction

The persistence of the economic crisis and the high levels of unemployment in Greece, Spain, Portugal and Italy have given more visibility to the critics of the euro. Even in France, a growing share of the population is nowadays looking at the euro with suspicion. The defenders of the single currency warn that the difficulties of the peripheral economies are due to structural problems slowing productivity growth. The critics react suggesting that the observed productivity dynamics may be a consequence of the euro and of the exchange-rate policies implemented before the adoption of the euro.

The case of Italy is emblematic. Figure 1 depicts the evolution of the real GDP per hour worked in Italy from 1970 to 2013. Many have noted that labour productivity kept growing slower since around 1996. This observation has inspired an intensive search for explanations (Daveri and Jona-Lasinio, 2005; Daveri and Parisi, 2010; Hassan and Ottaviano, 2013; Manasse, 2013; Nannicini, 2013a; Nannicini, 2013b; Lippi and Schivardi, 2014; Pellegrino and Zingales, 2014; to cite a few). Among these, one that has attracted the attention of the media and the public in general is the possible association between this productivity slowdown and the exchange-rate policy adopted by Italy after 1996 (Bagnai, 2013).

In November 1996, Italy adopted a fixed exchange-rate policy. It was not the first time the country adopted such a policy, but it was the first time this policy was adopted in an environment characterized by free capital mobility, central bank independence, and a new system of industrial relations (the so-called *protocollo d'intesa* of July 1993). Despite the relevance of the change occurred, causal evidence on the effects of the policy choice made by Italy in the winter of 1996 is scant. This partly explains why the debate has gravitated so far around an unanswered question: Has the fixed exchange-rate policy caused - or partly caused - the slowdown of the real hourly productivity growth that we observe in the data?

Of course, it is difficult to say "yes" or "no". To the best of our knowledge, the above question can only be truly answered if a credible quasi-experimental study could be set up. The ideal design should employ internationally comparable micro data on labour productivity and its determinants. With these data at hands, a Difference-in-

Differences (DiD) approach could, in principle, be used to estimate the impact of the policy on productivity growth.

Unfortunately, data with the above mentioned characteristics are not currently available, and a quasi-experiment based on a DiD model cannot be designed in absence of appropriate data. Nevertheless, the available data allow us to answer a question related to labour productivity, which is the focus of this paper: Has the fixed exchange-rate policy adopted by Italy after 1996 caused - or partly caused - a decline of the real hourly wage growth of Italian private-sector workers?[1]

The question addressed in this paper is interesting for three main reasons. First, real hourly wage and real hourly productivity are widely seen by economists as strongly associated[2]. If the exchange-rate policy adopted by Italy had an effect on real wages, the possibility that the policy had an effect on real labour productivity as well turns out to be more realistic (the whole efficiency wage theory is based on the idea that the worker effort responds to pay incentives). Second, our research question is important because it is related to the classical question of whether a change in the policy management of a nominal variable - the nominal exchange rate here - has real effects. A positive answer would be in line with the standard Keynesian view that policies changing the behavior of nominal variables can, in principle, affect the dynamics of real ones. Finally, as we shall see in the next section, a union model by Andersen and Sørensen (1988) provides us with a clear theoretical prediction about the labour-market effects of a fixed exchange-rate policy *in the private sector*, which we find worth testing.

Yet, in order to answer our main research question, it is crucial to set up a credible quasi-experiment allowing us to predict what would have been the trajectory of real wages had Italy not chosen to adopt a fixed exchange-rate policy. The research design proposed here is the main contribution of this paper and can be used to answer other causal questions related to the effects of the policy choice made by the Italian government in November 1996.

This paper belongs to the small set of studies that provide quasi-experimental evidence in macroeconomics (Fuchs-Schuendeln and Hassan, 2015). The additional difficulty in *international* macroeconomics is not only that one typically lacks the

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[1] By "Italian" we mean individuals working in Italy, not necessarily Italian citizens.

[2] The direction of causality can be twofold: for instance, productivity is seen to affect wages in the human capital theory, and wages are seen to affect productivity in the efficiency wage theory.

counterfactual - what would have happened if Italy had not adopted a fixed exchange-rate policy - but also that a counterfactual can only be artificially built using data from at least one different country. While examples of quasi-experimental studies exploiting differences across states, regions, municipalities, firms and individuals of the same country are common, these approaches are not useful to evaluate the effects of an exchange-rate policy because all units of the same country are treated by the policy. To the best of our knowledge, this paper innovates by using *individual-level* data from a different country as counterfactual. In particular, we select the United Kingdom (UK) as counterfactual for Italy and use a DiD approach to estimate the causal effect of interest. The advantage of this approach is that we do not need the treated and the controls to be "equal at the baseline". We mainly need them to belong to "parallel worlds".

2. Theory, methodology and data

The link between exchange rates and labour-market outcomes is a classical topic in international macroeconomics. The works authored by Goldstein (1974), Sachs (1980), Dornbusch (1987), Djajić (1988), Andersen and Sørensen (1988), Collins and Park (1989), Burgess and Knetter (1998), Lawler (2000), Goldberg and Tracy (2000, 2001), Campa and Goldberg (2001), Mishra and Spilimbergo (2011), Wright and Bastos (2012), Schmitt-Grohé and Uribe (2012, 2013), Galí and Monacelli (2013) testify for the interest in the topic.

We build on earlier research along two lines. First, while several studies have looked at the impact of exchange-rate movements (appreciation or depreciation) on wages, we focus on the impact of the exchange-rate risk on wages. In particular, we test the hypothesis of Andersen and Sørensen (1988) that unions have a rational incentive to ask for a lower exchange-rate risk premium under fixed exchange rates. Second, we follow a recent contribution by Manasse et al. (2013) and use a counterfactual approach.

Theory

Andersen and Sørensen (1988) considered an economy where workers are organized in one single union. The economy consumes two goods: one domestic and one foreign, which are imperfect substitutes. The union sets the nominal wage unilaterally to maximize the expected utility of the total number of employed, subject to the labour-demand constraint. This expected utility is increasing in the number of employed, so that the union not only cares about the wage but also about the employment level. The firms respond by determining employment according to their labour-demand function, which is downward sloping. In this economy, they showed that the union sets $ln\, w = \alpha + E(ln\, p) + \beta \cdot V(ln\, e)$ where $ln\, w$ is the log nominal wage, α is a constant (a function of the utility from being unemployed and other factors), $E(ln\, p)$ is the expected log consumer price index, and $V(ln\, e)$ is the variance of the log nominal exchange rate. In addition, they argued that β is a strictly positive coefficient in a small open economy. As a consequence, the expected log real wage is given by $ln\, w - E(ln\, p) = \alpha + \beta \cdot V(ln\, e)$.

The above result has four implications for the private sector[3]. First, if there is no uncertainty, i.e. $V(ln\, e) = 0$ and $E(ln\, p) = ln\, p$, then the level of the log real wage is equal to α with certainty. Second, $\beta \cdot V(ln\, e)$ is the exchange-rate risk premium that the union requires because of the uncertainty induced by the exchange-rate variability. Third, this premium is higher under flexible exchange rates because the exchange-rate variability is typically higher under flexible exchange rates. Fourth, by reducing or eliminating the exchange-rate risk premium, the transition from a flexible to a fixed exchange-rate regime implies a lower expected log real wage. The latter is the theory tested in this paper: *the transition from a flexible to a fixed exchange rate induces the unions to ask for a lower average exchange-rate risk premium, reducing the average expected log real wage in the private sector and, eventually, the average effective log real wage in the private sector.*

More formally[4], let $Y_{i1} = \tilde{Y}_i + P_{i1}$ be the expected[5] log real wage for the treated if treated ($Treat_i = 1$), which is observable, and $Y_{i0} = \tilde{Y}_i + P_{i0}$ be the expected log real

[3] Note that the Andersen-Sørensen model is based on the assumption of profit-maximizing firms, which does not necessarily apply to the public sector. In addition, in the public sector, the union is likely to take employment as given and to only care about the wage. This implies no incentive to set a lower wage.

[4] We use the following notation: $Y \cong ln\, w - E(ln\, p)$, $\tilde{Y} \cong \alpha$ and $P \cong \beta \cdot VAR(ln\, e)$.

[5] The term "expected" refers to the fact that the union sets the log nominal wage today and makes a prediction today about the log price level tomorrow. This prediction is the same for every individual covered by the collective bargaining agreement. Thus, the term "expected" should not be misleading: both Y_{i1} and Y_{i0} are random variables because they are both defined as a random variable (the log nominal wage) minus a constant (the predicted log price level).

wage for the treated if not treated, which is not observable. P_{i1} is the exchange-rate risk premium for the treated when treated, i.e. when the fixed exchange-rate policy is in place, and P_{i0} is the exchange-rate risk premium for the treated when not treated. $\tilde{Y}_i$ is the expected log real wage for the treated, independent of the treatment. We will estimate the following Average Treatment Effect on the Treated (ATET):

$$E[Y_{i1} - Y_{i0} | Treat_i = 1] = E[Y_{i1} | Treat_i = 1] - E[Y_{i0} | Treat_i = 1] = E[P_{i1} | Treat_i = 1] - E[P_{i0} | Treat_i = 1]$$

We expect the ATET to be negative because $E[P_{i1} | Treat_i = 1] < E[P_{i0} | Treat_i = 1]$ is likely to hold. $E[Y_{i0} | Treat_i = 1]$ is not observable, but a DiD model is able to provide us with a consistent estimate of this mean value.

As we will see, under a mild assumption about the log price process, the effect of the treatment on the average *expected* log real wage for the treated is informative about the effect of the treatment on the average *effective* log real wage for the treated.

Methodology and data

Manasse et al. (2013) studied the effects of the euro on the Italian economy and came up with an original idea to create a reliable counterfactual. They used a "synthetic-control" approach and exploited the information from aggregate time-series data for each country in their sample. They considered that the treatment took place since January 1999 and analyzed what would have happened to the Italian economy, if the country had not joined the euro. The authors reported that most indicators of the Italian performance would not have changed, thus suggesting that the euro should not be seen as the cause of the Italian recent problems. However, they did find that the dynamics of real productivity has been negatively affected by the adoption of the euro.

The study by Manasse et al. (2013) is particularly relevant because, to the best of our knowledge, it is the first attempt to analyze the effects of an exchange-rate policy choice, such as the adoption of the euro, in a quasi-experimental setting. However, a lot of heterogeneity is likely to be disguised by the use of aggregate time-series data.

We depart from Manasse et al. (2013) along two directions. On the one hand, we try to do a step onwards by using individual-level data. On the other hand, we do a step back by focusing on the years immediately prior to the birth of the euro. The latter choice is justified on the ground that, as already seen, the Italian productivity slowdown started before the birth of the euro. Most importantly, the structural change in the management of the nominal exchange rate in Italy happened in November 1996, not in January 1999.

The construction of a DiD exercise requires several steps. First, we must define the treatment variable, the treated group, the control group, the pre-treatment period and the treatment period. Then, it is important to check whether the so-called "parallel trends assumption" is satisfied.

Let us start with the treatment variable and the treatment period. We use wage data from the European Community Household Panel (ECHP). This is a well known individual-level longitudinal dataset which reports micro data for 15 countries. The waves go from 1994 to 2001. However, the data reported in the dataset in each wave sometimes refer to the previous year. This is the case with individual wages. For example, wage data reported in the 1994 wave refer to wages earned in 1993 and so on. This means that, in principle, we have access to actual data on individual wages from 1993 to 2000. Since the data are annual, our treatment variable will be defined annually.

Figure 2 shows that, at the peak, in April 1995, the exchange rate between the Italian lira and the ecu (European currency unit) was at the level of 2,296.16 liras per ecu. After a revaluation started in May 1995, which led to an exchange rate of 1,932.35 liras per ecu in November 1996, Italy re-joined the European Monetary System (EMS). Since then, the exchange rate history of the country has been simple. The rate was kept around the value of 1,936.27 liras per ecu, which will become the official rate of conversion when, in January 1999, the euro experience started. The euro entered in circulation in January 2002.

Since we use annual wage data from 1993 to 2000 in our DiD exercise, our treatment variable is the fixed exchange-rate policy adopted by Italy in the 1997-2000 period. Given that we have data only from 1993 onwards, the pre-treatment period will be from 1993 to 1996. Of course, the treated group is formed by individuals working in Italy during the 1997-2000 period.

As a control group, we use individuals working in the UK. This is for one basic reason. Among the ECHP countries, the only two that had a flexible exchange-rate policy in the pre-treatment period and did not abandon it in the treatment period were the UK and Sweden (Denmark did not join the euro but had a fixed exchange-rate policy similar to that of Italy since around October 1998). We could have chosen Sweden. However, for Sweden, the ECHP does not provide data for the whole pre-treatment period (the waves start in 1997, reporting wages from 1996 onwards). Thus, the only country that can actually be used as a control group is the UK. Interestingly, both Italy and the UK exited the EMS in September 1992.

In particular, the wage data for the UK in the ECHP dataset are of two types: those collected by the European Commission (Eurostat), which are only available for the waves from 1994 to 1996, and those borrowed from British Household Panel Survey (BHPS), which cover all relevant years. The BHPS data are adapted to the ECHP variables and thus perfectly comparable with the data for Italy, which are available for the entire period of interest.

It is worth noting that, in DiD exercises, we do not need to compare groups with similar characteristics. We need that the mean outcome variables in the two groups - the treated and the controls - are parallel in the pre-treatment period, conditional on the set of explanatory variables we control for. In this study, we do not just assume the latter, as sometimes done in DiD studies. We test for the parallel trends assumption by following the approach suggested by Centeno and Novo (2014). Provided that we actually have parallelism in the pre-treatment period, then a break of parallelism in the treatment period can be attributed to the treatment.

As a matter of curiosity, comparing the lines of the real hourly wage in Italy and the UK in Figure 3, we can see that they followed roughly parallel patterns until around 1996, with Italian workers earning more. The relative decline of the Italian real wage started since around 1997. From 1997 to 2000, the ratio of Italy to UK real hourly wage[6] fell from 112.09% to 95.49%, i.e. the Italian real hourly wage declined 14.80% relative to the British.

[6] Our measure of real hourly wage in Figure 3 is a measure of real total labour compensations per hour worked. It is obtained by multiplying real GDP per hour worked (Eurostat) times labour share (OECD.Stats). The latter is defined as total labour compensations over GDP. The evolution over time of our measure of real hourly wage for the whole economy compares favorably with the real hourly wage index in the manufacturing industry obtained dividing the nominal hourly wage index in manufacturing (OECD.Stats) by the consumer price index (OECD.Stats). The graph is available from the author upon request.

Of course, we cannot draw any causal implication from the above graph because a lot of individual and firm heterogeneity is hidden behind aggregate data, and we do not. However, we can get an idea of the numbers and the dynamics in place.

3. Empirical strategy

We estimate the following DiD model:

(1) $$ln\, w_{it} - E_t(ln\, p_{t+1}) = \psi_0 + \psi_1 Treat_i + \psi_2 After_t + \psi_3 (Treat_i \times After_t) + X_{it}\delta + \varepsilon_{it}$$

The variable $ln\, w_{it}$ is the logarithm of the nominal gross hourly wage. The matrix X_{it} is a set of individual characteristics including education levels (primary, secondary and tertiary), individual age, age squared, gender, job status (whether the individual is supervisor, intermediate, and lower-intermediate), marital status (whether the individual is married or not), health (presence of chronic health problems), sector of production (agriculture[7], industry[8], and services[9]), migration status (whether the individual is an immigrant or not), sector of activity (whether the individual works in the public sector or not), and finally information on occupations[10].

The above control set includes a long list of individual productivity determinants. As a result, model (1) allows us to estimate if and how the treatment had a *direct* effect on the outcome variable (by inducing unions to ask for a lower exchange-rate risk premium), independently of any potential indirect effect that the treatment might have had on individual productivity determinants.

The variable $Treat_i$ equals to one for the treated group - individuals working in Italy - and zero for the control group - individuals working in the UK. The variable

[7] This category includes: 1) agriculture, hunting, and forestry; and 2) fishing.

[8] This category includes: 1) manufacturing; 2) mining; 3) electricity, gas and water supply; and 4) construction.

[9] This category includes: 1) wholesale and retail commerce and repair activities; 2) hotels and restaurants; 3) transportation, storage and communication; 4) financial intermediation; 5) real estate, renting and business activities; 6) public administration, defense and compulsory social security; 7) education; 8) health and social work; 9) other services.

[10] The occupation categories are nine: 1) legislators, senior officials and managers; 2) professionals; 3) technicians and associate professionals; 4) clerks; 5) service workers and shop and market sales workers; 6) skilled agricultural and fishery workers; 7) craft and related trades workers; 8) plant and machine operators and assemblers; and 9) elementary occupations.

$After_t$ equals to one from 1997 onwards - when the fixed exchange-rate policy is officially in place in Italy. Thus, the interaction term $Treat_i \times After_t$ equals to one for individuals working in Italy from 1997 onwards, and its coefficient ψ_3 identifies the impact of the policy change - the adoption of a fixed exchange-rate policy in Italy.

Without loss of generality, the log price index can be modeled as a first order autoregressive process, i.e. $ln\, p_{t+1} = \rho_0 + \rho_1 \, ln\, p_t + \varepsilon_t$. This immediately implies that $E_t(ln\, p_{t+1}) = \rho_0 + \rho_1 \, ln\, p_t$ and allows us to obtain two versions of model (1) which can be, in turn, used to estimate ψ_3 : one general[11] and one restricted. The latter assumes $\rho_1 = 1$ and, as a result, uses $ln\, w_{it} - ln\, p_t$ as dependent variable[12].

Before estimating model (1), we need to check whether the parallel trends assumption holds. As stressed before, we follow Centeno and Novo (2014) who estimate a model of the following type in the pre-treatment period:

(2) $$ln\, w_{it} - E_t(ln\, p_{t+1}) = \lambda_0 + \lambda_1 Treat_i + \lambda_2 Time_t + \lambda_3 (Treat_i \times Time_t) + X_{it}\delta + \varepsilon_{it}$$

where $Time_t$ is a linear trend between 1993 and 1996 (this variable takes the value of the corresponding year, i.e. 1993 in 1993, and so on).

If the trends are parallel, then the coefficient λ_3 is not statistically different from zero. This means that there is not a specific time trend of wages in Italy, conditional on the model covariates. Note, further, that it is not important whether the level of the mean log wage is different in Italy and the UK, or whether the levels of the covariates (say education) are different. All we need is parallelism in the pre-treatment period, and a reasonable argument to impute any change in parallelism to a single treatment: Italy's decision to adopt a fixed exchange-rate policy in an unprecedented economic environment (see above).

[11] We estimate $ln\, w_{it} = \psi_0 + \rho_0 + \psi_1 Treat_i + \psi_2 After_t + \psi_3 (Treat_i \times After_t) + \rho_1 \, ln\, p_t + X_{it}\delta + \varepsilon_{it}$. The data for the log price index refer to Italy for the treated group and to the UK for the control group. In this case, ψ_3 gives the effect of the treatment on the average log nominal wage for the treated, holding the log price level fixed - which is a proxy for the effect of the treatment on the average log real wage for the treated.

[12] We estimate $ln\, w_{it} - ln\, p_t = \psi_0 + \rho_0 + \psi_1 Treat_i + \psi_2 After_t + \psi_3 (Treat_i \times After_t) + X_{it}\delta + \varepsilon_{it}$. In this case, ψ_3 gives the effect of the treatment on the average log real wage for the treated.

4. Tricky points

DiD exercises can be criticized by arguing that the treatment may be multiple, i.e. there might be other things changing in Italy after 1996. This is a limitation of most DiD studies, particularly when the treatment period extends over several years. Our study is not exempted. Nevertheless, we try to address this issue by dealing with three potentially confounding factors: the Treu reform, competition from China, and the Information-Technology (IT) revolution.

The Treu reform

To keep into account that an important labour-market reform (*legge 24 giugno 1997, n. 196*) occurred in Italy in June 1997 - the so called Treu reform - which changed the legislation on temporary job contracts increasing their use, we focus our analysis on full-time wage earners with permanent contracts (p.c.) since 1993.

Specifically, we focus on individuals in paid employment with complete 8-year work histories and we exclude individuals working with an employer in paid apprenticeship or training, self-employed, and unpaid workers in family enterprises. In addition, we exclude all part-time jobs. Finally, we exclude individuals with fixed-term or short-term contracts, causal work with no contract, and some other working arrangements. Summary sample statistics for treated and controls are reported in Appendix (Table A1 and Table A2).

Arguably, the wages of the workers in the sample (both Italian and British) have not been affected by the Treu reform. This argument is also consistent with the fact that the Treu law explicitly prohibited to pay lower wages to workers on temporary contracts performing the same jobs as workers on permanent contracts.

One peculiar aspect of the Treu reform was that the sectors of agriculture and construction were excluded by the application of the law, unless an explicit agreement between unions and employer organizations were achieved at a later stage. Yet, to the best of our knowledge, the agreement was not reached. This fact probably induced the organizations of private employers to put pressure on the legislator with an eye at obtaining the abrogation of this norm - the conditional exclusion of agriculture and construction - which partially occurred only in December 1999 (*legge 23 dicembre*

1999, n. 488). The abrogation was partial because it only interested a specific category of workers in the two sectors, the clerks, meaning that all the other categories of workers were still excluded. Hence, the permanent workers in the sectors of agriculture and construction can be considered as not affected by the Treu reform during the 1997-1999 period and only marginally affected in 2000 (by the potential competition of part-time clerks), thus providing a cleaner quasi-experimental setup, useful to make a robustness check.

As an additional robustness check, we will also test whether our results are robust to the inclusion of temporary and part-time workers in the sample.

<u>China and IT revolution</u>

Regarding the other two confounding factors mentioned before, a recent study by Pellegrino and Zingales (2014) has stressed that - besides the euro - the Italian economy has been exposed to two additional important shocks in the last 25 years: one is competition from China and one is the IT revolution. In particular, the authors attribute the "Italian disease" to the fact that the country was not able to react to the challenge posed by the Chinese competition and to take full advantage of the IT revolution.

We believe that both the "China" and "IT" shocks do not seriously affect our estimate of the treatment effect for following two reasons. First, China only joined the World Trade Organization (WTO) in 2001. Hence, the Chinese competition may have reduced real wage growth in Italy most likely after our treatment period. Second, the IT revolution occurred in the 1990s. It affected both treated and controls, both in the pre-treatment period and in the treatment period, thus being implicitly accounted for in our estimation strategy.

As a matter of fact, trade barriers started to decrease well before China joined the WTO. Yet, the liberalization process was limited to few specific industries. For instance, one important change, which affected the textile and clothing industries worldwide, was the progressive elimination of the quantitative restrictions to imports of textile and clothing products, enforced by the Agreement on Textiles and Clothing (ATC) of December 1993.

Before the ATC signature, the European Economic Community (EEC) market was protected by the import quotas imposed under the Multi Fibre Agreement (MFA) of December 1973. The ATC was inspired by a liberalization principle. It designed a transition period during which a progressive phase-out on quotas had to be implemented. Four key dates and shares were agreed (Spinanger, 1999): 16% of 1990 imports had to be liberalized since January 1995, 17% since January 1998, 18% since January 2002, and the remaining 49% since January 2005.

The Italian textile and clothing industry may have been affected by this shock. However, the magnitude of the shock was very similar in the pre-treatment period (16%) and during the treatment period (17%), thus being unlikely to bias our estimate of the policy treatment. In addition, most of the liberalization happened after the treatment period. Finally, the shock was not specific to Italy and its amplitude was limited to one specific industry.

These arguments suggest that the "China" effects are unlikely to bias the estimation of the parameter of interest. Nevertheless, if the combination of lower trade barriers and Chinese competition really reduced real wage growth in Italy during the treatment period and the policy did not, then the "China" effects should be concentrated among good-producing industries. We will make a robustness check along these lines.

A similar argument can be used for the IT revolution. If the IT revolution really had an impact on real wage growth during the treatment period and the policy did not, then the "IT" effects should be concentrated among service-producing industries. However, it is difficult to make a robustness check in this case because we do not have a prior on the potential effect of the IT revolution. On the one hand, it may have increased real wage growth by increasing real productivity growth. On the other hand, it may have decreased real wage growth by replacing workers with technology.

<u>Control group, endogeneity and anticipation issues</u>

Another possible criticism to our DiD approach is that treated and controls live in two different countries. Thus, the control group may have deviated from trend after 1996 due to some specific shock occurred in the British economy. Despite such an event cannot be excluded, it is worth noting that the British real wage has been roughly parallel to the Italian one until 1996 (included). From 1997 onwards, while the British real wage recovered the historically increasing trend, the Italian real wage did not (Figure 3).

A further criticism is that the exchange-rate policy adopted by Italy after November 1996 not only has treated the Italian workers but also the British ones, through bilateral trade and capital movements, or even migration. Of course, such externalities cannot be ignored. Nevertheless, they are present in most of the existing DiD studies. For instance, if a reform affects the workers of one sector, they can move to other sectors used as control group, thus corrupting the quasi-experimental setup. On this specific point, an argument supporting our analysis is that, as a matter of fact, the economic links between Italy and the UK are not as tight as those between Italy and Germany, or Italy and France. Thus, the assumption we make is not "heroic" when compared to similar assumptions usually made in other DiD studies.

Finally, the identification of model (1) can be criticized because it requires that any influence of the treatment on the set of covariates, which includes the log price index, does not affect the potential outcomes (Lechner, 2008) and that the treated do not change their behaviour - affecting the pre-treatment outcome - in anticipation of future treatment. These assumptions - unlike the one on parallel trends - cannot be directly tested. However, Section 5 will show that our main results are robust to the use of $ln\, w_{it} - ln\, p_t$ as dependent variable. In addition, it will discuss why a significant effect of the treatment on the pre-treatment outcome is unlikely.

5. Results

Models (1) and (2) are estimated by Ordinary Least Squares. Following Eissa and Leibman (1996, p. 632), among others, we estimate model (1) as a two-period model (pre-treatment and treatment), i.e. we treat the observations from 1993 to 1996 as if they were referring to one single period, and the observations from 1997 to 2000 as if they were referring to another single period. Our empirical approach is clearly summarized in Figure 4(a).

Main results

Let us consider Table 1. The main coefficient of interest, ψ_3, is equal to -0.0546, and statistically significant at 1% level, for the private sector of the economy. This means that the fixed exchange-rate policy adopted by Italy after 1996 caused a reduction of the real hourly wage growth of Italian full-time permanent workers by 5.4% on average in the 1997-2000 period, in the private sector.

The parallel trends assumption, tested in Table 2, is not rejected, which supports our identification approach. Indeed, the coefficient λ_3 in the private sector is found to be not statistically significant and very close to zero (-0.003).

Using the same empirical strategy, it is also interesting to investigate to what extent the policy affected other groups of the working population in Table 1. In particular, we find that public-sector workers have not been affected by the policy. In addition, within the private sector, we do not find evidence of relevant heterogeneity between industry (good-producing sectors) and services. Yet, since we find that services workers have been a bit more penalized by the policy on average, the estimates do not support the criticism of the "China" bias.

As expected, the coefficient of the log price index is not necessarily equal to 1. However, there are cases in which ρ_1 is very close to 1 (all sectors, private sector, and private industry). In general, we have a trade-off between the potential endogeneity bias due to the use of the log price index as covariate and the potential distortion created by assuming that ρ_1 is equal to 1. We prefer the first of the two options because it is more flexible and allows us to capture the variability of the coefficient of the log price index as we move from aggregated to more disaggregated data (see further below).

Like in the case of the private sector, the parallel trends assumption is satisfied in all the other cases reported in Table 2, though less clearly in the case of the public sector (the null is rejected at 10% level).

When further investigating the existence of heterogeneity within the private sector, we find some evidence of differentiated responses across industries in Table 3 (see also Table A3 in Appendix). Seven out of eleven industries have negative coefficients above the average for the private sector (-0.0546), but the four industries with coefficients below the average (manufacturing, mining and utilities, commerce, and transportation) account for 62% of the observations in the private sector.

For private manufacturing, the evidence is in line with the one obtained for the whole good-producing sector (private manufacturing accounts for 10993 observations out of 13644). The impact coefficient is equal to -0.0487 and significant at 1% level (Table 3).

A finding supporting the idea that our main result is not driven by the potentially confounding effect of the Treu reform is that the real wage growth in the construction industry strongly decreased as a result of the policy (-12%, see Table 3). A similar result is found for the agricultural sector (-17%), though it should be handled with caution because the number of observations is low.

The finding for the construction industry is particularly interesting for two reasons. First, it is more robust to criticisms because the quasi-experimental setting is cleaner. Not only the Treu reform but also the "China" and the "IT" biases are less likely to apply to this industry. In addition, once we make treated and controls more similar at the baseline by means of kernel matching, the estimation of model (1) on the common support provides an estimate of the impact of the policy in the construction industry which is even bigger (-16%). Second, the construction-industry finding suggests that our main result for the private sector (-5.4%) may be, if anything, underestimated. This reinforces our conclusion of a negative causal effect of the treatment.

As expected, we find a lot of variability in the coefficient of the log price index across industries (Table 3), supporting the general specification of the DiD model vs. the restricted one.

Again, the parallel trends assumption is satisfied for all industries in Table 3, as suggested by the estimates of λ_3 reported in Table 4.

Overall, we find that the fixed exchange-rate policy adopted by Italy in the 1997-2000 period reduced the average real hourly wage growth in Italy for the "strongest" category of workers in the private sector, i.e. the full-time wage earners with permanent contracts. In contrast, the wage trajectories of public-sector workers have not been affected by the policy.

Robustness checks

The main result of this paper - the treatment effect in the private sector - is robust to the inclusion in the sample of other categories of Italian workers in paid employment, namely part-time workers and workers on temporary contracts (t.c.; see results in Table A4 and Table A5 in Appendix). However, it should be noted that the number of observations increases only by 9% (roughly 4000 obs.).

There are two complementary explanations for this small increase. First, we focus on workers in paid employment with complete 8-year work histories. Obviously, part-time workers and workers on temporary contracts are less likely to fit within this type of sample restriction. Second, these workers are not a large share of the labour force. For instance, data from the Organization for Economic Cooperation and Development (2002) suggest that the percentage of dependent employees in temporary jobs was around 5% in both Italy and the UK in 1990, and it increased to around 7% in the UK and 10% in Italy in 2000.

In addition, our main finding is not driven the potential endogeneity of the log price index. Indeed, the results for the whole economy (all sectors) and the macro-sectors in Table 1 are all robust to the use of $ln\, w_{it} - ln\, p_t$ as dependent variable (see Table A6 in Appendix).

Finally, our main result is robust to a panel-data specification that controls for individual and year fixed effects[13], clustering the standard errors at individual level, as suggested by Bertrand et al. (2004). In particular, we find that the estimate of the impact of the policy in the private increases in size (-9.5%). This result, likewise the one for the construction-industry discussed before, suggests that the impact of the treatment in the private sector reported in Table 1 is, if anything, underestimated. Another interesting point is that the treatment effect for public-sector workers turns out to be significant in the panel-data specification (see Table A7 in Appendix), though the size of the effect is not big (-2.7%).

In sum, the evidence supports the prediction of the Andersen-Sørensen model according to which the private-sector unions ask for a lower exchange-rate risk premium under fixed exchange rates. However, the effect of the policy should not be

[13] We estimate $ln\, w_{it} - ln\, p_t = \psi_0 + \rho_0 + \psi_i + \psi_t + \psi_3 (Treat_i \times After_t) + X_{it}\delta + \varepsilon_{it}$.

seen as independent of the unique economic environment in which the policy was implemented. Indeed, several factors have likely contributed to the treatment effect.

First, the fixed exchange-rate policy was implemented in a new institutional environment in which the unions had a more active role in real-wage determination than in the past. Indeed, the wage indexation mechanism in place in Italy during the 1970s (the so-called *scala mobile*), which automatically adjusted nominal wage growth to actual inflation, was gradually weakened in the 1980s and completely abolished in July 1992. This mechanism was replaced by a new type of collective agreement system (the so-called *protocollo d'intesa*) in July 1993, based on expected inflation and sector-level bargaining between unions and employer organizations (when not involved as the employer, the government played a role of intermediation).

Second, the fixed exchange-rate policy was adopted in a new environment characterized by central bank independence, which reduced the expected inflation. This likely provided the private-sector employers with an argument to ask for lower nominal wage growth, and the unions with a reason to agree.

Third, the fixed exchange-rate policy was adopted in a new economic environment where capital could freely circulate. This increased the average wage-bargaining strength of Italian employers, who could relocate their production units in countries where business conditions were more favorable, and benefit from lower exchange-rate uncertainty.

Fourth, almost all the sector-level contracts were renewed between 1997 and 1998 (Centro di Studi Economici e Sindacali, 2000). This fact not only suggests that the policy may have affected real wage growth by 1997 onwards, but also that a significant effect of the treatment on the pre-treatment outcome is unlikely. Indeed, allowing for heterogeneous treatment effects across years[14] in the private sector, we find that the negative effects of the policy have been gradually increasing over time, being not significant in 1997 and marginally significant in 1998 (see Figure 4(b)). They became large in magnitude and statistically relevant only in 1999 (-7.1%) and 2000 (-11.7%).

[14] We estimate $ln\, w_{it} - ln\, p_t = \psi_0 + \rho_0 + \psi_1 Treat_i + \sum_{t=1997}^{2000} \psi_{2t} Year_t + \sum_{t=1997}^{2000} \psi_{3t} \left(Treat_i \times Year_t \right) + X_{it}\delta + \varepsilon_{it}$ where $Year_{1997}$ is equal to one in 1997 and zero otherwise (and so on).

Thus, the impact of -5.4% discussed above must be seen as an average of the annual differences from the counterfactual.

6. Falsification

A standard practice in DiD studies is to present one or more falsification exercises. Unfortunately, producing a falsification exercise is not an easy task in international macroeconomics. Its construction is even trickier than the one of the main DiD exercise proposed in this paper. Yet, we make an attempt to present at least one reliable falsification in this section.

A falsification exercise implements the main DiD exercise in a context where one should expect no effect of the policy. One way to do it is to use a group of "controls" affected by the policy as counterfactual. If both the treated and the "controls" have been affected by the same policy, we should expect no effect of the policy on the outcome variable. If an effect is indeed found, then our main DiD exercise is unlikely to be well-designed. Thus, the estimated treatment effect in the main DiD exercise is likely to be misleading. Again, for the "treatment effect" in the falsification exercise to be reliable, the parallel trends assumption must be satisfied in the pre-treatment period.

In short, we have to proceed in two steps. First, among those listed in the ECHP dataset, we need to select a country which has adopted an exchange-rate policy very similar to that of Italy, ideally during both the pre-treatment period and the treatment period. Second, we have to test whether the parallel trends assumption holds in the pre-treatment period.

As for the first step, the only ECHP country that started to implement a fixed exchange-rate policy around November 1996 is Finland. As shown in Figure A1, there are clear differences in the exchange-rate dynamics between Italy and Finland in the 1993-1996 period, but the timing of the switch to the fixed exchange-rate policy in Finland is pretty in line with that observed in Italy. The fact that the 1993-1996 dynamics of the exchange rates in Finland and Italy have been different is not really an issue to the extent that the parallel trends assumption is not rejected.

Table A8 and Table A9 in Appendix present the estimation results from model (1) and model (2) when using Finnish full-time permanent workers as "controls". Overall, the results look encouraging.

The parallel trends assumption is satisfied (again, less clearly for the public sector). This means that the estimated "treatment effect" in the falsification exercise is reliable. We find no effect of the policy in both the public sector and the private sector. However, ψ_3 is significant at 5% for all sectors.

The main point of this section is that the falsification exercise fully supports the treatment effect estimated in the main DiD exercise *for the private sector*, which is the key result of this paper. The drawback is that the treatment effect estimated in the main DiD exercise *for all sectors* is not supported by the falsification.

Though the latter result is unexpected, it should not be seen as problematic for three reasons. First, the statistical significance of the coefficient for all sectors is not consistent with the evidence for the two main sectors of the economy, public sector and private sector. Second, the coefficient of -0.0178 is an estimate of the magnitude of the overall effect of confounding factors on real wage growth in all sectors: the low magnitude of the estimate is reassuring. Third, the focus of this paper is on the private sector. The way in which we have constructed the quasi-experiment may be not suitable to estimate the causal effect of the policy in the public sector - and thus in the economy as a whole - because the hypotheses of the underlying theoretical model are unlikely to hold for the public sector (see footnote 3).

7. Conclusions

This paper has used a Difference-in-Differences estimation approach and individual data from the European Community Household Panel to provide causal evidence on the link between exchange-rate policies and labour-market outcomes. The results suggest that the fixed exchange-rate policy adopted by Italy in the 1997-2000 period has reduced the real hourly wage growth of Italian full-time workers with permanent contracts by at least 5.4%, in the private sector. This is consistent with the theoretical mechanism put forward by Andersen and Sørensen (1998): in the private sector, the unions ask for a lower average exchange-rate risk premium under fixed exchange rates, and thus set a lower average expected log real wage which, eventually, causes a lower average effective log real wage.

The main strength of our analysis is that, to the best of our knowledge, we are the first to evaluate an exchange-rate policy choice in a quasi-experimental setting using individual data. The main limitation is that we do not *additionally* control for firm characteristics. However, to our knowledge, this is something that cannot be done with the existing available data.

In the last two decades, the Italian economy has suffered. It is difficult to explain what caused the labour-productivity slowdown observed in the data, and this paper has not tackled that specific issue. Yet, this research has highlighted that the fixed exchange-rate policy adopted by Italy since November 1996 - despite being related to a monetary variable (the nominal exchange rate) - was not neutral: it had real effects. By inducing private-sector unions to ask for lower real wage growth, that policy *caused* lower real wage growth in the private sector. This does not necessarily imply that the Italian productivity slowdown resulted from the fact that Italy joined the EMS and the euro zone. Nevertheless, our results suggest that more attention should be paid to the potential real effects of the policy change occurred in November 1996.

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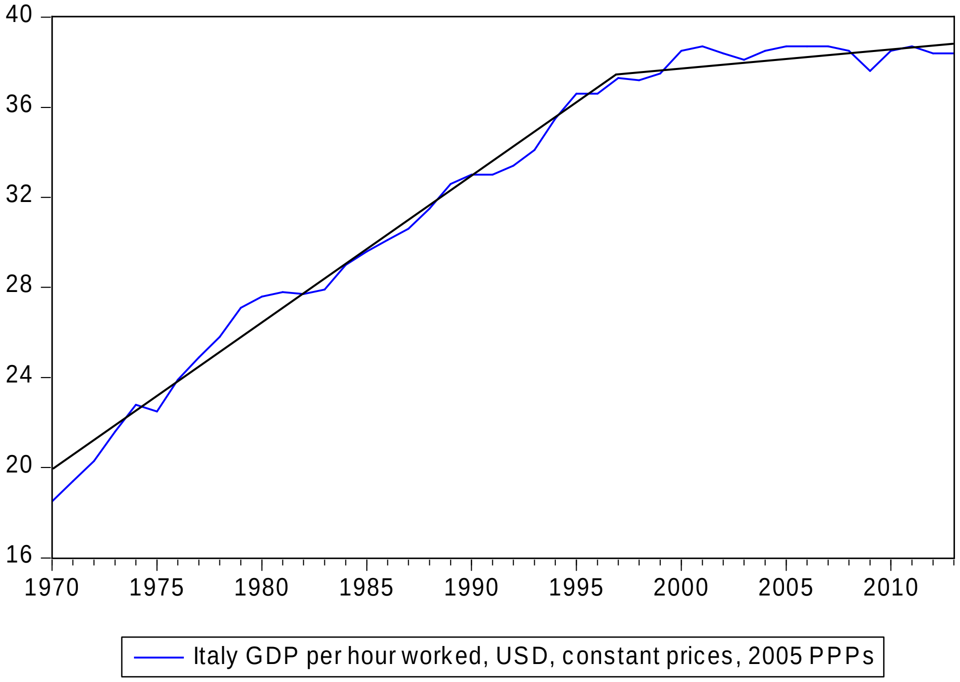
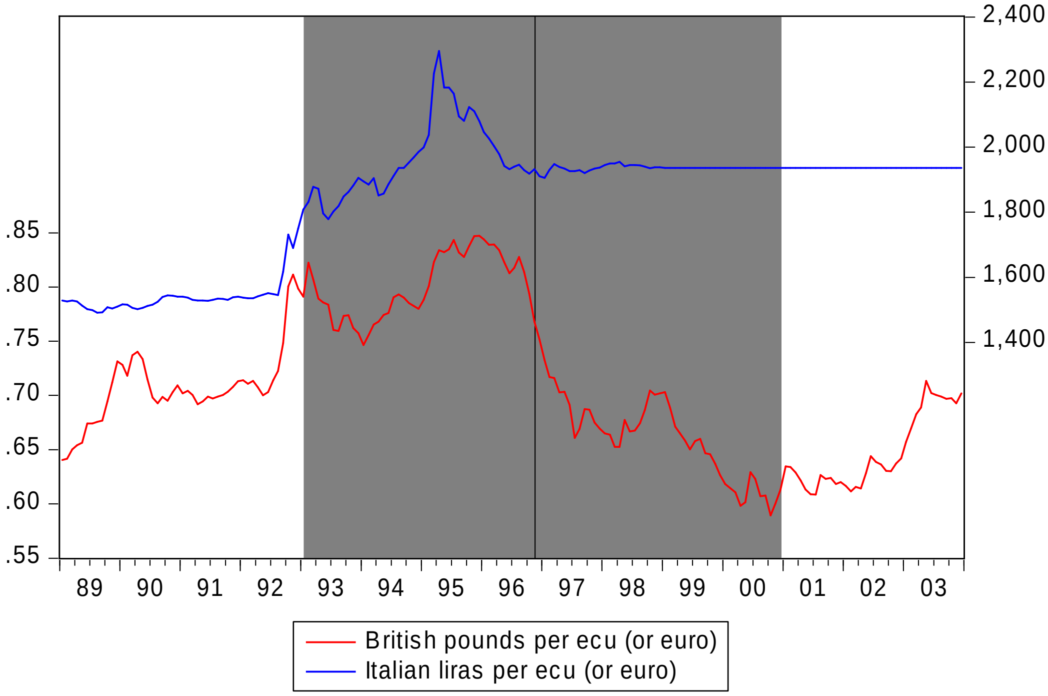
Fig. 1 - The Italian productivity slowdown



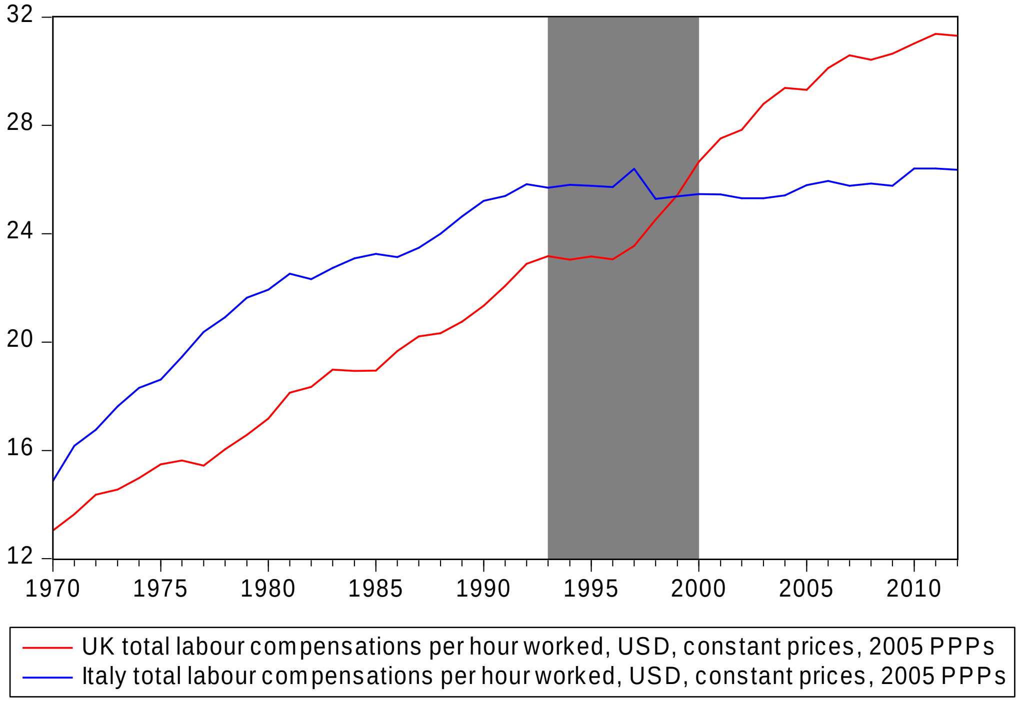
Source: OECD.Stat (http://stats.oecd.org/)

Fig. 2 - Nominal exchange rates, Italy vs. United Kingdom



Source: Eurostat (http://epp.eurostat.ec.europa.eu/portal/page/portal/statistics/)

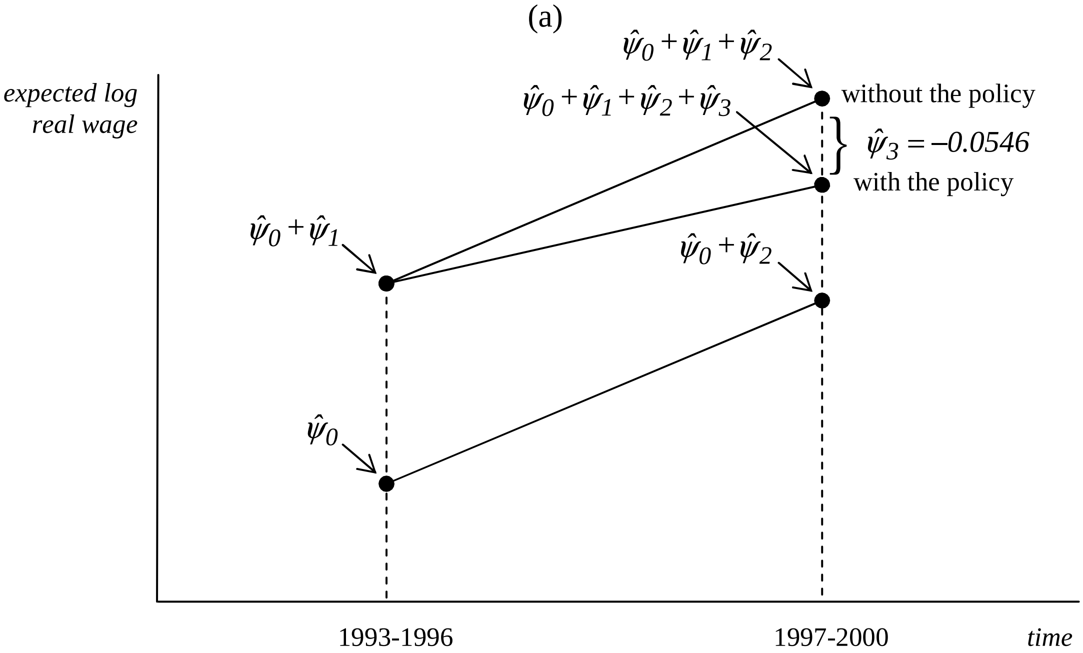
Fig. 3 - Italy vs. UK real hourly wage



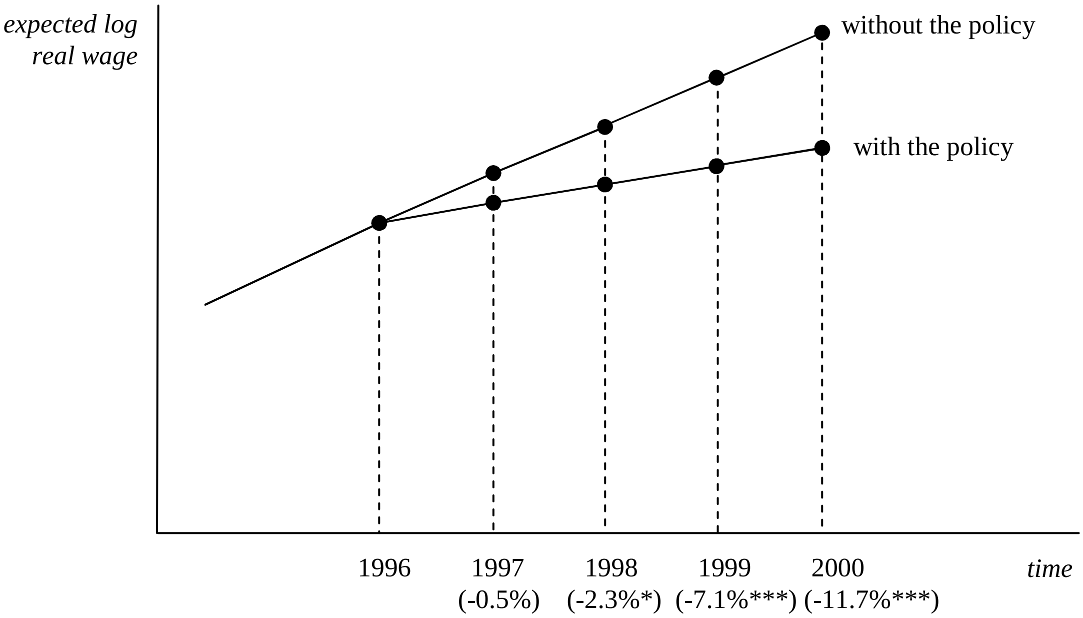
Source: Author elaboration from OECD.Stat and Eurostat data

Fig. 4 - Difference-in-differences, private sector

(a)



(b)



*** p<0.01, ** p<0.05, * p<0.1
See Table B2 for the estimates

Table 1. Treatment effect, 1997-2000, full-time workers on p.c.
(Dependent variable: lnw)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	0.905***	0.927***	0.901***	0.834***	0.963***
	(0.00646)	(0.0106)	(0.00803)	(0.0102)	(0.0125)
after	0.0518***	0.0222*	0.0633***	0.0773***	0.0566***
	(0.00699)	(0.0115)	(0.00858)	(0.0114)	(0.0125)
treat×after	-0.0305***	0.0170	-0.0546***	-0.0537***	-0.0656***
	(0.00718)	(0.0118)	(0.00883)	(0.0117)	(0.0130)
lnp	0.979***	0.861***	1.038***	0.970***	1.107***
	(0.0557)	(0.0842)	(0.0722)	(0.0863)	(0.118)
age	0.0472***	0.0263***	0.0538***	0.0417***	0.0641***
	(0.00125)	(0.00236)	(0.00156)	(0.00197)	(0.00241)
age2	-0.000499***	-0.000243***	-0.000591***	-0.000441***	-0.000725***
	(1.53e-05)	(2.73e-05)	(1.95e-05)	(2.45e-05)	(3.06e-05)
educ2	0.0740***	0.0726***	0.0701***	0.0787***	0.0500***
	(0.00394)	(0.00652)	(0.00487)	(0.00581)	(0.00793)
educ3	0.187***	0.220***	0.164***	0.178***	0.149***
	(0.00565)	(0.00902)	(0.00715)	(0.0101)	(0.00987)
married	0.0416***	0.0325***	0.0453***	0.0584***	0.0372***
	(0.00392)	(0.00619)	(0.00496)	(0.00631)	(0.00744)
female	-0.147***	-0.0941***	-0.175***	-0.166***	-0.180***
	(0.00354)	(0.00532)	(0.00471)	(0.00627)	(0.00684)
supervisor	0.168***	0.160***	0.175***	0.185***	0.186***
	(0.00617)	(0.00950)	(0.00794)	(0.00996)	(0.0118)
intermediate	0.0565***	0.0631***	0.0536***	0.0645***	0.0477***
	(0.00420)	(0.00640)	(0.00544)	(0.00685)	(0.00830)
public	0.106***				
	(0.00404)				
disability	-0.0388***	-0.0285***	-0.0431***	-0.0259***	-0.0617***
	(0.00523)	(0.00749)	(0.00682)	(0.00890)	(0.00997)
industry	0.165***	0.129***	0.161***		
	(0.0156)	(0.0342)	(0.0179)		
services	0.121***	0.116***	0.121***		
	(0.0155)	(0.0324)	(0.0180)		
immigrant	-0.0253**	-0.0345*	-0.0169	-0.0841***	0.0823***
	(0.0127)	(0.0189)	(0.0163)	(0.0148)	(0.0314)
_Iocc1	0.376***	0.429***	0.358***	0.326***	0.388***
	(0.0108)	(0.0178)	(0.0133)	(0.0186)	(0.0190)
_Iocc2	0.437***	0.404***	0.444***	0.348***	0.511***
	(0.00850)	(0.0110)	(0.0131)	(0.0185)	(0.0184)
_Iocc3	0.278***	0.259***	0.284***	0.212***	0.338***
	(0.00740)	(0.00995)	(0.0101)	(0.0137)	(0.0145)
_Iocc4	0.186***	0.120***	0.231***	0.165***	0.282***
	(0.00651)	(0.00861)	(0.00873)	(0.0119)	(0.0128)
_Iocc5	0.0231***	0.124***	-0.0214**	0.0716**	0.00513
	(0.00771)	(0.0111)	(0.0100)	(0.0282)	(0.0127)
_Iocc6	0.000144	0.0308	-0.00997	-0.182***	-0.0177
	(0.0189)	(0.0299)	(0.0237)	(0.0484)	(0.0562)
_Iocc7	0.0778***	0.101***	0.0777***	0.0564***	0.0940***
	(0.00655)	(0.0121)	(0.00799)	(0.0103)	(0.0147)
_Iocc8	0.0910***	0.138***	0.0850***	0.0589***	0.0932***
	(0.00710)	(0.0126)	(0.00866)	(0.0111)	(0.0145)
Constant	-3.840***	-2.850***	-4.192***	-3.463***	-4.593***
	(0.242)	(0.367)	(0.313)	(0.374)	(0.512)
Obs.	43,291	14,239	29,052	13,644	14,900
R-squared	0.679	0.699	0.644	0.654	0.643

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2. Parallel trends, 1993-1996, full-time workers on p.c.
(Dependent variable: lnw)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	-37.49	-93.08*	8.103	-12.29	20.40
	(34.87)	(54.27)	(44.63)	(53.84)	(74.86)
time	0.0471**	0.0708**	0.0235	0.0371	0.0198
	(0.0214)	(0.0331)	(0.0274)	(0.0327)	(0.0465)
treat×time	0.0192	0.0470*	-0.00363	0.00655	-0.00976
	(0.0175)	(0.0272)	(0.0223)	(0.0270)	(0.0375)
Constant	-90.15**	-131.6**	-47.65	-72.59	-40.93
	(39.33)	(60.95)	(50.42)	(60.25)	(85.40)
Obs.	19,846	6,696	13,150	6,522	6,409
R-squared	0.674	0.683	0.641	0.658	0.640

Regressions control for all model covariates
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Treatment effect, 1997-2000, private sector full-time workers on p.c.
(Dependent variable: lnw)

	agf	mut	con	mnf	com	hor	tra	fin	rer	edu	hlt
treat	1.154***	0.758***	0.875***	0.838***	0.994***	1.236***	0.903***	0.903***	0.820***	1.067***	1.083***
	(0.0767)	(0.0504)	(0.0311)	(0.0110)	(0.0206)	(0.0471)	(0.0328)	(0.0351)	(0.0329)	(0.0881)	(0.0400)
after	0.193**	0.0693	0.133***	0.0722***	0.0458**	0.0834*	0.0154	0.0265	0.0674**	0.0885	0.104***
	(0.0914)	(0.0505)	(0.0323)	(0.0124)	(0.0217)	(0.0465)	(0.0324)	(0.0320)	(0.0295)	(0.0695)	(0.0381)
treat×after	-0.174*	-0.0492	-0.120***	-0.0487***	-0.0414*	-0.110**	-0.0455	-0.0616*	-0.0882***	-0.109	-0.124***
	(0.0905)	(0.0524)	(0.0339)	(0.0127)	(0.0222)	(0.0482)	(0.0345)	(0.0328)	(0.0313)	(0.0776)	(0.0431)
lnp	0.598	0.722	0.933***	1.020***	1.051***	1.432***	1.310***	1.433***	1.171***	1.136	0.757**
	(0.444)	(0.456)	(0.219)	(0.0951)	(0.204)	(0.467)	(0.323)	(0.309)	(0.313)	(0.800)	(0.334)
Constant	-1.837	-3.042	-3.651***	-3.573***	-4.356***	-5.392***	-4.902***	-5.332***	-5.395***	-5.197	-2.962**
	(1.922)	(1.984)	(0.953)	(0.411)	(0.879)	(1.997)	(1.399)	(1.335)	(1.361)	(3.522)	(1.451)
Obs.	508	722	1,929	10,993	4,254	987	1,947	1,908	2,493	411	1,144
R-squared	0.623	0.684	0.632	0.663	0.680	0.756	0.632	0.667	0.560	0.659	0.759

Regressions control for all model covariates
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
See Table A3 for industry labels

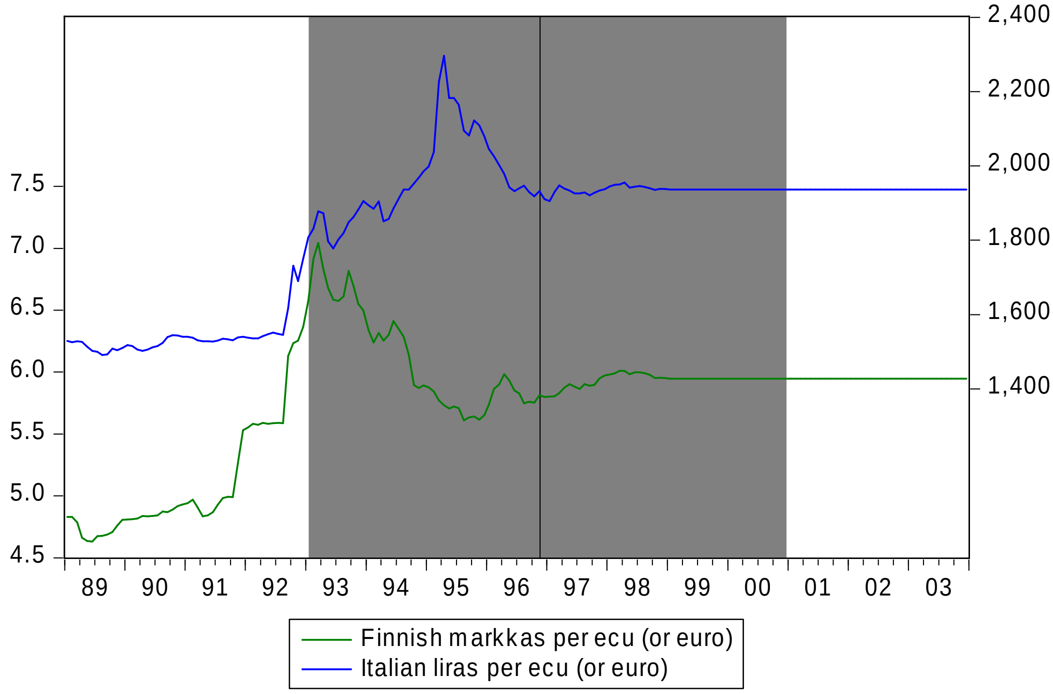
Table 4. Parallel trends, 1993-1996, private sector full-time workers on p.c.
(Dependent variable: lnw)

	agf	mut	con	mnf	com	hor	tra	fin	rer	edu	htl
treat	486.8	-176.9	-94.06	21.73	-5.881	-127.8	230.5	45.82	42.75	-63.05	-123.7
	(353.8)	(284.5)	(141.0)	(58.84)	(122.5)	(319.6)	(205.9)	(199.6)	(216.8)	(657.5)	(203.9)
time	-0.148	0.118	0.148*	0.0193	0.0194	0.0870	-0.104	-0.0185	0.0773	0.0433	0.224*
	(0.212)	(0.179)	(0.0843)	(0.0358)	(0.0776)	(0.195)	(0.128)	(0.124)	(0.136)	(0.418)	(0.121)
treat×time	-0.243	0.0899	0.0475	-0.0105	0.00342	0.0645	-0.115	-0.0225	-0.0211	0.0322	0.0623
	(0.177)	(0.142)	(0.0706)	(0.0295)	(0.0613)	(0.160)	(0.103)	(0.0999)	(0.109)	(0.329)	(0.102)
Constant	260.9	-220.3	-278.5*	-40.39	-39.98	-163.4	186.1	30.12	-149.1	-85.68	-419.9*
	(397.4)	(328.9)	(155.8)	(65.90)	(142.4)	(358.1)	(234.6)	(226.7)	(249.9)	(763.8)	(222.1)
Obs.	219	370	873	5,279	1,917	390	782	892	1,000	154	496
R-squared	0.585	0.706	0.643	0.668	0.675	0.725	0.646	0.678	0.535	0.703	0.783

Regressions control for all model covariates
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
See Table A3 for industry labels

Appendix

Fig. A1 - Nominal exchange rates, Italy vs. Finland



Source: Eurostat (http://epp.eurostat.ec.europa.eu/portal/page/portal/statistics/)

Table A1. Summary statistics for British full-time workers on p.c. (controls)

	mean	sd	min	max
lnw	1.95	0.51	-2.1	5
age	38.41	11.38	17.0	65
educ1	0.36	0.48	0.0	1
educ2	0.14	0.34	0.0	1
educ3	0.50	0.50	0.0	1
married	0.59	0.49	0.0	1
female	0.41	0.49	0.0	1
supervisor	0.26	0.44	0.0	1
intermediate	0.18	0.38	0.0	1
nonsupervisor	0.56	0.50	0.0	1
public	0.25	0.43	0.0	1
disability	0.26	0.44	0.0	1
agriculture	0.01	0.08	0.0	1
industry	0.30	0.46	0.0	1
services	0.69	0.46	0.0	1
immigrant	0.00	0.07	0.0	1
_Iocc1	0.18	0.38	0.0	1
_Iocc2	0.13	0.34	0.0	1
_Iocc3	0.14	0.34	0.0	1
_Iocc4	0.17	0.37	0.0	1
_Iocc5	0.11	0.32	0.0	1
_Iocc6	0.01	0.08	0.0	1
_Iocc7	0.12	0.32	0.0	1
_Iocc8	0.10	0.29	0.0	1
_Iocc9	0.05	0.22	0.0	1
Observations	17355			

Nominal wages are measured in British pounds

Table A2. Summary statistics for Italian full-time workers on p.c. (treated)

	mean	sd	min	max
lnw	2.72	0.38	-0.2	5
age	39.59	10.23	17.0	65
educ1	0.40	0.49	0.0	1
educ2	0.48	0.50	0.0	1
educ3	0.12	0.33	0.0	1
married	0.71	0.46	0.0	1
female	0.37	0.48	0.0	1
supervisor	0.09	0.29	0.0	1
intermediate	0.17	0.38	0.0	1
nonsupervisor	0.74	0.44	0.0	1
public	0.38	0.49	0.0	1
disability	0.06	0.24	0.0	1
agriculture	0.02	0.14	0.0	1
industry	0.35	0.48	0.0	1
services	0.63	0.48	0.0	1
immigrant	0.02	0.15	0.0	1
_Iocc1	0.02	0.15	0.0	1
_Iocc2	0.11	0.31	0.0	1
_Iocc3	0.13	0.33	0.0	1
_Iocc4	0.26	0.44	0.0	1
_Iocc5	0.09	0.29	0.0	1
_Iocc6	0.01	0.10	0.0	1
_Iocc7	0.19	0.39	0.0	1
_Iocc8	0.09	0.29	0.0	1
_Iocc9	0.09	0.29	0.0	1
Observations	25936			

Nominal wages are measured in Italian liras

Table A3. Industry labels

Abbreviation	Industry denomination
agf	Agriculture, hunting and forestry + Fishing
mut	Mining + Electricity, gas and water supply
con	Construction
mnf	Manufacturing
com	Wholesale and retail commerce and repair activities
hor	Hotels and restaurants
tra	Transportation, storage and communication
fin	Financial intermediation
rer	Real estate, renting and business activities
edu	Education
htl	Health and social work

Table A4. Treatment effect, 1997-2000, including part-time workers and workers on t.c.
(Dependent variable: lnw)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	0.917***	0.946***	0.907***	0.837***	0.971***
	(0.00629)	(0.0103)	(0.00783)	(0.0101)	(0.0120)
after	0.0487***	0.0151	0.0626***	0.0758***	0.0574***
	(0.00687)	(0.0114)	(0.00842)	(0.0113)	(0.0122)
treat×after	-0.0346***	0.00206	-0.0512***	-0.0565***	-0.0594***
	(0.00704)	(0.0117)	(0.00864)	(0.0115)	(0.0126)
lnp	1.004***	0.951***	1.025***	0.980***	1.084***
	(0.0536)	(0.0828)	(0.0688)	(0.0833)	(0.111)
age	0.0464***	0.0297***	0.0525***	0.0418***	0.0618***
	(0.00119)	(0.00226)	(0.00147)	(0.00190)	(0.00226)
age2	-0.000490***	-0.000277***	-0.000575***	-0.000443***	-0.000697***
	(1.45e-05)	(2.63e-05)	(1.85e-05)	(2.36e-05)	(2.88e-05)
educ2	0.0747***	0.0773***	0.0696***	0.0792***	0.0495***
	(0.00381)	(0.00644)	(0.00467)	(0.00567)	(0.00745)
educ3	0.187***	0.223***	0.164***	0.180***	0.148***
	(0.00550)	(0.00880)	(0.00698)	(0.00999)	(0.00956)
married	0.0438***	0.0356***	0.0474***	0.0607***	0.0410***
	(0.00378)	(0.00601)	(0.00479)	(0.00620)	(0.00710)
female	-0.136***	-0.0873***	-0.162***	-0.156***	-0.163***
	(0.00338)	(0.00521)	(0.00445)	(0.00601)	(0.00641)
supervisor	0.166***	0.160***	0.173***	0.183***	0.185***
	(0.00609)	(0.00935)	(0.00786)	(0.00995)	(0.0116)
intermediate	0.0546***	0.0600***	0.0518***	0.0621***	0.0446***
	(0.00410)	(0.00630)	(0.00530)	(0.00675)	(0.00800)
public	0.103***				
	(0.00389)				
disability	-0.0350***	-0.0203***	-0.0407***	-0.0258***	-0.0589***
	(0.00513)	(0.00739)	(0.00670)	(0.00876)	(0.00974)
industry	0.202***	0.115***	0.210***		
	(0.0135)	(0.0273)	(0.0157)		
services	0.157***	0.101***	0.170***		
	(0.0134)	(0.0252)	(0.0158)		
immigrant	-0.0262**	-0.0389**	-0.0171	-0.0790***	0.0552**
	(0.0121)	(0.0195)	(0.0151)	(0.0153)	(0.0270)
_Iocc1	0.378***	0.443***	0.353***	0.330***	0.365***
	(0.0105)	(0.0176)	(0.0130)	(0.0183)	(0.0183)
_Iocc2	0.435***	0.418***	0.430***	0.348***	0.475***
	(0.00808)	(0.0107)	(0.0126)	(0.0180)	(0.0174)
_Iocc3	0.276***	0.274***	0.271***	0.215***	0.305***
	(0.00706)	(0.00981)	(0.00958)	(0.0133)	(0.0134)
_Iocc4	0.182***	0.129***	0.219***	0.167***	0.249***
	(0.00611)	(0.00846)	(0.00807)	(0.0113)	(0.0115)
_Iocc5	0.0247***	0.132***	-0.0174*	0.0712***	-0.0107
	(0.00720)	(0.0110)	(0.00920)	(0.0252)	(0.0115)
_Iocc6	-0.00494	0.0478*	-0.0254	-0.146***	-0.0860
	(0.0163)	(0.0245)	(0.0205)	(0.0393)	(0.0536)
_Iocc7	0.0706***	0.102***	0.0639***	0.0548***	0.0619***
	(0.00621)	(0.0126)	(0.00744)	(0.00967)	(0.0138)
_Iocc8	0.0882***	0.147***	0.0752***	0.0608***	0.0655***
	(0.00673)	(0.0125)	(0.00810)	(0.0105)	(0.0137)
Constant	-3.977***	-3.328***	-4.153***	-3.509***	-4.431***
	(0.233)	(0.360)	(0.298)	(0.361)	(0.480)
Obs.	47,442	15,517	31,925	14,595	16,547
R-squared	0.671	0.690	0.633	0.645	0.635

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5. Parallel trends, 1993-1996, including part-time workers and workers on t.c.
(Dependent variable: lnw)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	12.58	-8.928	33.59	11.91	48.50
	(33.68)	(53.20)	(42.83)	(52.34)	(69.71)
time	0.0155	0.0137	0.00961	0.0313	-0.00311
	(0.0207)	(0.0326)	(0.0263)	(0.0318)	(0.0433)
treat×time	-0.00586	0.00492	-0.0164	-0.00557	-0.0238
	(0.0169)	(0.0266)	(0.0214)	(0.0262)	(0.0349)
Constant	-32.60	-27.72	-22.37	-62.49	1.291
	(38.04)	(60.01)	(48.40)	(58.53)	(79.46)
Obs.	21,860	7,352	14,508	6,988	7,179
R-squared	0.666	0.681	0.627	0.649	0.627

Regressions control for all model covariates
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A6. Treatment effect, 1997-2000, full-time workers on p.c., real wage spec.
(Dependent variable: lnw – lnp)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	0.906***	0.935***	0.898***	0.835***	0.956***
	(0.00560)	(0.00940)	(0.00686)	(0.00880)	(0.0105)
after	0.0504***	0.0135	0.0657***	0.0755***	0.0632***
	(0.00610)	(0.0103)	(0.00735)	(0.0100)	(0.0103)
treat×after	-0.0311***	0.0127	-0.0534***	-0.0546***	-0.0623***
	(0.00700)	(0.0115)	(0.00859)	(0.0114)	(0.0126)
age	0.0472***	0.0263***	0.0538***	0.0417***	0.0642***
	(0.00125)	(0.00236)	(0.00156)	(0.00197)	(0.00241)
age2	-0.000499***	-0.000243***	-0.000591***	-0.000441***	-0.000726***
	(1.53e-05)	(2.73e-05)	(1.95e-05)	(2.45e-05)	(3.06e-05)
educ2	0.0740***	0.0725***	0.0701***	0.0786***	0.0502***
	(0.00394)	(0.00652)	(0.00487)	(0.00582)	(0.00793)
educ3	0.187***	0.220***	0.164***	0.178***	0.149***
	(0.00565)	(0.00902)	(0.00715)	(0.0101)	(0.00987)
married	0.0417***	0.0327***	0.0452***	0.0585***	0.0371***
	(0.00392)	(0.00618)	(0.00496)	(0.00631)	(0.00744)
female	-0.147***	-0.0942***	-0.175***	-0.166***	-0.180***
	(0.00354)	(0.00532)	(0.00471)	(0.00627)	(0.00684)
super	0.168***	0.160***	0.175***	0.185***	0.186***
	(0.00617)	(0.00949)	(0.00794)	(0.00996)	(0.0118)
inter	0.0565***	0.0631***	0.0536***	0.0645***	0.0477***
	(0.00420)	(0.00640)	(0.00544)	(0.00685)	(0.00830)
public	0.106***				
	(0.00404)				
disability	-0.0388***	-0.0286***	-0.0431***	-0.0259***	-0.0616***
	(0.00522)	(0.00749)	(0.00682)	(0.00890)	(0.00997)
indus	0.165***	0.129***	0.161***		
	(0.0156)	(0.0342)	(0.0179)		
serv	0.121***	0.116***	0.121***		
	(0.0155)	(0.0324)	(0.0180)		
immigrant	-0.0253**	-0.0349*	-0.0169	-0.0841***	0.0821***
	(0.0127)	(0.0190)	(0.0163)	(0.0148)	(0.0314)
_Iocc1	0.376***	0.429***	0.359***	0.326***	0.388***
	(0.0108)	(0.0178)	(0.0133)	(0.0186)	(0.0190)
_Iocc2	0.437***	0.403***	0.444***	0.348***	0.511***
	(0.00850)	(0.0110)	(0.0131)	(0.0184)	(0.0184)
_Iocc3	0.278***	0.259***	0.285***	0.212***	0.339***
	(0.00740)	(0.00995)	(0.0101)	(0.0137)	(0.0145)
_Iocc4	0.186***	0.120***	0.231***	0.165***	0.282***
	(0.00651)	(0.00861)	(0.00873)	(0.0119)	(0.0128)
_Iocc5	0.0231***	0.124***	-0.0213**	0.0715**	0.00531
	(0.00771)	(0.0111)	(0.0100)	(0.0282)	(0.0127)
_Iocc6	0.000106	0.0305	-0.00990	-0.183***	-0.0176
	(0.0189)	(0.0298)	(0.0237)	(0.0484)	(0.0563)
_Iocc7	0.0777***	0.100***	0.0777***	0.0563***	0.0942***
	(0.00655)	(0.0121)	(0.00799)	(0.0103)	(0.0147)
_Iocc8	0.0910***	0.138***	0.0851***	0.0587***	0.0935***
	(0.00709)	(0.0126)	(0.00865)	(0.0111)	(0.0145)
Constant	-3.932***	-3.450***	-4.028***	-3.591***	-4.131***
	(0.0282)	(0.0573)	(0.0337)	(0.0389)	(0.0441)
Observations	43,291	14,239	29,052	13,644	14,900
R-squared	0.692	0.711	0.657	0.668	0.655

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
See Table B1 for the unconditional estimates

Table A7. Treatment effect, 1997-2000, full-time workers on p.c., real wage spec., panel
(Dependent variable: lnw – lnp)

	All sectors	Public sector	Private sector	Private industry	Private services
treat×after	-0.0728***	-0.0274***	-0.0956***	-0.0936***	-0.0851***
	(0.00571)	(0.00832)	(0.00731)	(0.0103)	(0.0105)
age	0.0320	0.134***	0.0215	0.0128	0.0367
	(0.0336)	(0.0378)	(0.0395)	(0.0364)	(0.0626)
age2	-0.000688***	-0.000364***	-0.000797***	-0.000563***	-0.000962***
	(4.02e-05)	(6.52e-05)	(5.16e-05)	(6.53e-05)	(7.79e-05)
educ2	0.00491	0.00672	0.00217	-0.00662	0.00338
	(0.00653)	(0.0110)	(0.00790)	(0.00867)	(0.0126)
educ3	0.00272	0.0149	-0.00621	-0.0167	-0.00311
	(0.00921)	(0.0146)	(0.0109)	(0.0151)	(0.0146)
married	0.00554	-0.00251	0.00374	0.00407	-0.00332
	(0.00785)	(0.0133)	(0.00959)	(0.0131)	(0.0135)
super	0.0431***	0.0396***	0.0453***	0.0446***	0.0439***
	(0.00572)	(0.00944)	(0.00714)	(0.0100)	(0.0105)
inter	0.0180***	0.0101	0.0206***	0.0136**	0.0224***
	(0.00410)	(0.00667)	(0.00516)	(0.00673)	(0.00782)
public	0.0162*				
	(0.00874)				
disability	-0.00710	-0.00493	-0.00670	0.00118	-0.0111
	(0.00466)	(0.00668)	(0.00617)	(0.00810)	(0.00876)
indus	0.0472**	0.0416	0.0448*		
	(0.0220)	(0.0345)	(0.0268)		
serv	0.0197	0.0322	0.0139		
	(0.0223)	(0.0321)	(0.0276)		
immigrant	0.0800***		0.106***	0.0527***	
	(0.00898)		(0.0109)	(0.0141)	
_Iocc1	0.0389***	0.0485**	0.0286*	0.0138	0.0295
	(0.0124)	(0.0210)	(0.0156)	(0.0217)	(0.0250)
_Iocc2	0.0320***	0.0235	0.0292*	-0.00827	0.0350
	(0.0124)	(0.0166)	(0.0172)	(0.0202)	(0.0288)
_Iocc3	0.0182*	0.00728	0.0196	-0.0187	0.0349
	(0.0104)	(0.0144)	(0.0141)	(0.0168)	(0.0239)
_Iocc4	0.00532	0.00577	-0.00350	-0.0350*	0.0136
	(0.0102)	(0.0124)	(0.0140)	(0.0183)	(0.0232)
_Iocc5	-0.0296***	0.00106	-0.0479***	-0.0108	-0.0378*
	(0.0114)	(0.0160)	(0.0149)	(0.0332)	(0.0212)
_Iocc6	-0.0240	-0.0204	-0.0339	0.0140	0.00641
	(0.0220)	(0.0181)	(0.0315)	(0.0694)	(0.129)
_Iocc7	0.00561	0.0259	0.000323	-0.0109	-0.0224
	(0.00913)	(0.0192)	(0.0111)	(0.0122)	(0.0239)
_Iocc8	0.00710	0.0251	0.00765	-0.0199	0.0185
	(0.0102)	(0.0235)	(0.0122)	(0.0126)	(0.0287)
Constant	-2.223*	-6.536***	-1.862	-1.717	-2.239
	(1.215)	(1.499)	(1.354)	(1.254)	(2.115)
Observations	43,291	14,239	29,052	13,644	14,900
R-squared	0.124	0.087	0.144	0.124	0.154

Regressions control for individual and year effects
Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A8. Treatment effect, 1997-2000, full-time workers on p.c., falsification
(Dependent variable: lnw)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	-1.239***	-1.142***	-1.306***	-1.341***	-1.267***
	(0.00764)	(0.0117)	(0.00992)	(0.0132)	(0.0154)
after	0.0521***	0.0486***	0.0424***	0.0593***	0.0309**
	(0.00626)	(0.00926)	(0.00829)	(0.0112)	(0.0122)
treat×after	-0.0178**	-0.00294	-0.0149	-0.0231*	-0.0141
	(0.00754)	(0.0115)	(0.00979)	(0.0130)	(0.0148)
Constant	-0.681***	-0.347	-0.776**	-0.404	-0.829*
	(0.236)	(0.369)	(0.302)	(0.370)	(0.499)
Obs.	36,871	14,119	22,752	11,522	10,661
R-squared	0.857	0.847	0.863	0.884	0.845

Regressions control for all model covariates
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A9. Parallel trends, 1993-1996, full-time workers on p.c., falsification
(Dependent variable: lnw)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	-76.29	-164.6*	-9.186	-36.39	-18.40
	(59.16)	(92.50)	(75.52)	(92.86)	(126.8)
time	0.0295**	0.0545***	0.00782	0.0179	0.00199
	(0.0115)	(0.0172)	(0.0152)	(0.0202)	(0.0237)
treat×time	0.0375	0.0818*	0.00393	0.0175	0.00857
	(0.0296)	(0.0463)	(0.0378)	(0.0465)	(0.0635)
Constant	-52.41**	-95.27***	-14.64	-32.47	-3.054
	(20.74)	(30.64)	(27.66)	(37.39)	(42.06)
Obs.	17,672	7,067	10,605	5,577	4,747
R-squared	0.868	0.858	0.873	0.888	0.861

Regressions control for all model covariates
Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table B1. Treatment effect, 1997-2000, full-time workers on p.c., real wage spec., unconditional
(Dependent variable: lnw – lnp)

	All sectors	Public sector	Private sector	Private industry	Private services
treat	0.845***	0.845***	0.802***	0.729***	0.875***
	(0.00672)	(0.0105)	(0.00814)	(0.0106)	(0.0123)
time	0.0801***	0.0517***	0.0927***	0.116***	0.0889***
	(0.00775)	(0.0125)	(0.00933)	(0.0127)	(0.0131)
treat×after	-0.0452***	-0.00196	-0.0618***	-0.0806***	-0.0674***
	(0.00901)	(0.0143)	(0.0108)	(0.0145)	(0.0159)
Constant	-2.450***	-2.298***	-2.504***	-2.449***	-2.542***
	(0.00586)	(0.00931)	(0.00708)	(0.00939)	(0.0101)
Obs.	43,291	14,239	29,052	13,644	14,900
R-squared	0.467	0.529	0.434	0.443	0.428

Regressions do not control for model covariates
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2. Treatment effect, 1997-2000, full-time workers on p.c., real wage spec., year heterogeneity
(Dependent variable: lnw – lnp)

	All sectors	Public sector	Private sector	Private industry	Private services
treat×year1997	0.0156	0.0587***	-0.00505	-0.0226	-0.00115
	(0.00997)	(0.0165)	(0.0122)	(0.0164)	(0.0176)
treat×year1998	-0.00503	0.0323*	-0.0236*	-0.0282	-0.0282
	(0.0106)	(0.0181)	(0.0128)	(0.0171)	(0.0187)
treat×year1999	-0.0479***	-0.00326	-0.0715***	-0.0523***	-0.0966***
	(0.0107)	(0.0178)	(0.0130)	(0.0180)	(0.0188)
treat×year2000	-0.0906***	-0.0407**	-0.117***	-0.120***	-0.124***
	(0.0110)	(0.0181)	(0.0134)	(0.0179)	(0.0194)
Constant	-3.928***	-3.447***	-4.023***	-3.592***	-4.124***
	(0.0282)	(0.0572)	(0.0336)	(0.0389)	(0.0440)
Obs.	43,291	14,239	29,052	13,644	14,900
R-squared	0.693	0.712	0.658	0.669	0.657

Regressions control for all model covariates
Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$