

# Working for 200 euro?

## The effects of traineeship reform on labor market outcomes in Croatia

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Working for 200 euro? The effects of traineeship reform  
on labor market outcomes in Croatia

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# Working for 200 euro?

## The effects of traineeship reform on labor market outcomes in Croatia

Iva Tomić\*    Ivan Zilic†

### ABSTRACT

In this paper we evaluate the effects of active labor market policy (ALMP) reform, the so-called SOR measure (vocational training for work without commencing employment), on labor market outcomes in Croatia. In 2012 SOR was redesigned to ease the first labor market entry and promote on-the-job training, enabling a young person without relevant work experience to get a one-year contract and a net monthly remuneration of 210 euro. The measure soon became popular, especially among university graduates, accounting for around third of their unemployment exits, and absorbing two thirds of funds allocated to ALMPs. Pooling Croatian Labor Force Surveys from 2007–2016 and using difference-in-difference strategy, we estimate the causal intent-to-treat effect of the reform on labor market outcomes of the *potentially eligible* group: 18–29-year-olds. Results indicate that the reform has had, at best, neutral effects on employment and unemployment; moreover, we find some evidence that a portion of young individuals were propelled into inactivity. However, we do find an adverse effect on wages—driven mostly by wages received by females and university graduates—both at the mean and higher percentiles of the wage distribution. This research provides insights on effectiveness of ALMPs in ameliorating youth unemployment in Europe, and opens questions on the appropriateness of the use of European Union funds in new EU member states.

*Keywords:* ALMP, evaluation, labor market outcomes, traineeship reform, youth unemployment.

*JEL classification:* J08, J31, J41.

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# Raditi za 200 eura?

## Učinci reforme stručnog osposobljavanja na ishode na tržištu rada u Hrvatskoj

### SAŽETAK

U radu ocjenjujemo učinke reforme aktivne mjere zapošljavanja, takozvane SOR mjere (stručno osposobljavanje za rad bez zasnivanja radnog odnosa), na ishode tržišta rada u Hrvatskoj. SOR je 2012. godine redizajniran kako bi olakšao prvi ulazak na tržište rada i promovirao osposobljavanje na radnom mjestu, omogućujući mladoj osobi bez relevantnog radnog iskustva jednogodišnji ugovor i mjesečnu naknadu od 210 eura. Mjera je ubrzo postala popularna, osobito među visokoobrazovanima, čineći oko trećinu njihovih izlazaka iz nezaposlenosti i apsorbirajući oko dvije trećine sredstava utrošenih na aktivne mjere zapošljavanja. Koristeći Anketu o radnoj snazi za razdoblje 2007.–2016. i identifikacijsku strategiju razlike u razlikama procjenjujemo uzročni učinak reforme na ishode tržišta rada *potencijalno obuhvaćene* skupine. Rezultati pokazuju da je reforma u najboljem slučaju imala neutralan učinak na zapošljavanje i nezaposlenost; štoviše, neke procjene pokazuju da je dio mladih osoba otišao u neaktivnost. Također, pronalazimo statistički značajan negativan učinak na plaće — posebno na plaće žena i visokoobrazovanih — kako u prosjeku tako i na višim percentilima distribucije plaća. Rezultati ovog istraživanja nude uvide u učinkovitost mjera za zapošljavanja mladih u Europi i otvaraju pitanja o prikladnosti korištenja europskih fondova u novim državama članicama EU.

*Ključne riječi:* aktivne mjere zapošljavanja, evaluacija, tržište rada, stručno osposobljavanje, nezaposlenost mladih.

*JEL klasifikacija:* J08, J31, J41.

## I. INTRODUCTION<sup>1</sup>

The nexus between economic conditions of first labor market entry and subsequent outcomes is a well-established empirical fact (Kahn, 2010; Liu et al., 2016), and the long-lasting adverse effect of labor market entry during a recession is often referred to as a *scarring effect* (Scarpetta et al., 2010; Schmitten and Umkehrer, 2017). Given the immense rise of youth unemployment at the European level during the Great Recession (2007–2012), the European Union has developed a number of initiatives and policies to mitigate youth unemployment, mainly defined as measures and programs within the active labor market policies (ALMPs), the most prominent being the so-called *Youth Guarantee* and *Youth Employment Initiative* (YEI) (Escudero and Mourelo, 2015).

While active labor market programs, especially vocational training, have been often advocated as a means to alleviate the problem of youth unemployment in Europe (Biavaschi et al., 2012; Chung et al., 2012; Eichhorst et al., 2013), there is no clear consensus on their final effect. Biavaschi et al. (2012), for example, stress the importance of taking into account particular national or local starting conditions when implementing measures, including vocational education; whereas Chung et al. (2012) argue that EU member states often neglect systematic uncertainty and segmentation of the younger population; emphasizing only the supply side of the policy—education. In general, the effectiveness of ALMPs has always been questionable, especially specific programs aimed at youths (Kluve, 2010; Caliendo and Schmidl, 2016; Card et al., 2017).

In this paper we contribute to the literature by analyzing the effect of an ALMP reform—designed to ease the first labor market entry and promote on-the-job learning—on labor market outcomes in Croatia. Experiencing a particularly deep and persistent recession in the 2009–2014 period, accompanied by enormous youth unemployment rates, the Croatian government introduced the Act on the Promotion of Employment in May 2012 which established the use of ALMP as means to fight youth unemployment. The main part of the new Act was the complete redesign of the program already in place—namely, vocational training for work without commencing employment (Croatian: *stručno osposobljavanje bez zasnivanja radnog odnosa*, so-called SOR)—that enabled a young person without relevant work experience to get a one-year contract and a net monthly remuneration of 210 euro (29% of average net wage at the time). The government covered the re-

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muneration and pension contributions, while the employer should have ensured a relevant working environment and a mentor for a young person in training, without the formal obligation of offering a full-time contract to an individual who went through the program. While the program has been slightly modified since it was first introduced, most importantly increasing the remuneration to 315 euro (43% of average net wage) in 2015, the core of the measure remained the same.

Although the program has not been intended for employment of youths but for their training, it soon gained popularity and has been perceived as almost an exclusive pathway of (first) labor market entry for youths in Croatia, especially for university graduates. For example, in 2016 the measure accounted for almost a third of unemployment exits among university graduates and absorbed almost two thirds of total funds spent on ALMPs. Part of the program popularity came from Croatia's accession to the EU in 2013 and the possibility to participate in the *Youth Guarantee* and *Youth Employment Initiative*, i.e., funding ALMPs for youths through EU funds. The program is still active and its use has not decreased even when Croatia bounced out of recession and labor market showed signs of recovery in 2015.

Since its introduction, the measure has been in both public and academic spotlight. [Matić \(2014\)](#), [Levačić \(2015\)](#), and especially [HZZ \(2016\)](#) have tackled the effects of the SOR program in Croatia. However, the aforementioned studies are mainly focused on the participants of the new traineeship model, which raises concerns regarding potential bias of the estimates due to the self-selecting nature of the measure, as well as concerns regarding potential spillover effects on eligible non-users of the measure.

Therefore, in this paper we use a method that enables the estimation of the unbiased intent-to-treat effect focusing the analysis on the *potentially eligible* SOR individuals. In particular, we define a person aged between 18 and 29 as a potentially eligible individual, with the 35–44 age cohort serving as a control group, and by using pooled Croatian Labor Force Survey for 2007–2016 we compare labor market outcomes of groups based on eligibility status before and after the reform. Using this difference-in-difference strategy, we circumvent the issues of selection-into-measure and capture potential spillover-effects on eligible non-participants.

Our results indicate that the SOR program, although a flagship of ALMPs use in Croatia, has not promoted employment, decreased unemployment or inactivity for youths. Moreover, in some specifications, we find that it has propelled some of the individuals into inactivity; therefore, we conclude that the measure, at best, has had a neutral effect on employment, unemployment and inactivity. In contrast, we find a significant drop in log wages (conditional on having one), es-

timates ranging from 5% to 10%. While this effect is mechanical, since SOR remuneration is coded as wage in our data, we also find compelling evidence that the whole distribution of wages shifted, which implies that the measure decreased the offered wage for all eligible individuals. We also find that this adverse wage effect is most prominent with females and university graduates, subgroups that have been the most dominant users of the measure.

Our paper generally fits into the large literature on the evaluation of programs aimed to activate the young subpopulation (Cammeraat et al., 2017; Hämäläinen et al., 2018; Ghirelli et al., 2018) and the contribution is threefold. Firstly, we provide seminal unbiased intent-to-treat analysis of SOR reform comprehensively documenting its effects. Secondly, given the universal nature of youth unemployment issues and immense funds allocated to meliorate them, this study can be of interest to a broader academic and expert audience. Lastly, this evaluation opens a broad question of whether the use of European Union funds in new member states is well-designed, articulated, and effective.

The paper is structured as follows. The next section briefly reviews the Croatian labor market and explains in more detail the traineeship reform evaluated in the empirical part. Section 3 focuses on the description of the methodology and the data used, while Section 4 presents the main findings of the analysis. The final section summarizes the results and gives some concluding remarks.

## II. LABOR MARKET IN CROATIA AND THE TRAINEESHIP REFORM

While the Great Recession (2007–2012) had significant adverse effects on labor market developments in most European economies, Croatia, the newest EU member state, was one of the countries that experienced a particularly persistent crisis: negative growth rates were recorded for six consecutive years—from 2009 to 2014—with cumulative GDP drop of more than 12%. These developments were strongly translated onto the labor market as the unemployment rate more than doubled between 2008 and 2014 (from 8.6% to 17.3%), with more than 200,000 jobs lost in the period, mainly in the private sector. Nevertheless, the literature (Botrić, 2009; Franičević, 2011; Nestić, 2015; Nikolic et al., 2017) suggests that these developments are mainly a product of structural issues of the Croatian economy—among others, labor market rigidities, public finance concerns, low or inexistent job creation in the private sector, and skills mismatch—that were only amplified by an overall negative economic climate.

With subdued labor demand and excess labor supply, adverse labor market developments were

even more severe in the case of subpopulation of young individuals. Though even before the crisis Croatia struggled with youth unemployment issues (Kolev and Saget, 2005)—for example, youth unemployment rate of 23.7% in 2008 was the second-highest among EU member states—in 2013 youth unemployment rate skyrocketed and reached 57.8% at the beginning of the year, whereas the yearly average of 50% was more than double the EU average (23.6%) and exceeded only by the rates in Greece (58.3%) and Spain (55.5%).<sup>2</sup>

In order to mitigate these alarming trends, in mid-2012 the government introduced the Act on the Promotion of Employment.<sup>3</sup> Among other things, the law allowed the possibility of an extended use of a previously existing program: vocational training for work without commencing employment (Croatian: *stručno osposobljavanje bez zasnivanja radnog odnosa*—SOR) as a way of (first) labor market entry for youth population in Croatia. While a similar program has been operating since 2010, it was intended only for individuals specializing in occupations obliged by the law to take a professional or state exam.<sup>4</sup> Therefore, the law passed in May 2012 established the use of active labor market policy (ALMP) as means to fight youth unemployment extending the measure for all individuals without relevant working experience, regardless of occupation, sector and the employer. Keeping in mind that (secondary) education in Croatia is non-dual, i.e., young people are rarely simultaneously present in both education and work, the all-encompassing traineeship model aimed not only to ease the labor market entry but also to promote work-based learning at the early stages of the career.

In a nutshell, the government covered the remuneration in the amount of 1,600 kuna (approximately 210 euro or 29% of the average net wage at the time and well below the minimum wage which was around 300 euro in net terms in 2012) and pension contributions, while the employer was expected to ensure a relevant working environment and a mentor for a young person in training. This type of contract was generally up to one year<sup>5</sup> and the employer did not have the formal obligation to offer a full-time contract to an individual who went through the program. The eligibility condition was a lack of relevant work experience in the specific profession and being registered at the employment service for at least 90 days (what later turned into 30 days). Since its introduction in 2012, the program has been modified on several occasions—from July 2013

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<sup>2</sup>Data for unemployment rates for Croatia and EU member states are taken from the Eurostat web page, accessed in January 2018.

<sup>3</sup>Official Gazette, No. 57/2012. The law came into force on May 31, 2012. Amendments to the law can be found in Official Gazette No. 120/2012 (a person was allowed to be registered at HZZ for 30 days instead of earlier stipulation of 90 days as a condition to participate in the program) and No. 16/2017.

<sup>4</sup>Up to the age of 25 for individuals who finished upper secondary education and up to the age of 29 in the case of tertiary education and with no more than 6 months of work experience.

<sup>5</sup>The exception is craft professions that could have a contract for a period not exceeding 36 months.

the employer has to cover traveling costs, while in January 2015 the remuneration received by participants increased to 2,400 kuna (315 euro or 43% of the average net wage and approximately at the level of net minimum wage at the time)—but the core of the program remained the same. The entire program has been administrated by the Croatian Employment Service (HZZ) and is still active.

The program soon gained popularity both among employers and unemployed youths (Table 1 and Figures 1–4). The number of participants in the measure grew from below 500 in 2010 to 33,366 in 2016, which constituted 49% of unemployed among 15–29-year-olds and 47% among all ALMP participants (Figure 1).<sup>6</sup> Likewise, funds allocated to SOR also increased substantially: from less than 55 thousand euro in 2010 to more than 95 million euro in 2016 thus absorbing almost two thirds of all ALMP funds in that year (Figure 1). The possibility to finance SOR measure through EU programs aimed to mitigate youth unemployment, namely *Youth Guarantee* and *Youth Employment Initiative*, only increased its popularity.<sup>7</sup>

Table 1 shows that from 2012 SOR program accounted for around 30% of unemployment exits among the highly-educated 18–29 population. The fact that tertiary educated individuals were the most common participants of the program can also be seen from Figures 2 and 3,<sup>8</sup> while Figures 2 and 4 indicate that females were more frequent users of the measure. The most dominant sectors that used the program are those generally in the public domain (public administration and defence, health, social work, education and scientific activities: Figures 3 and 4).<sup>9</sup>

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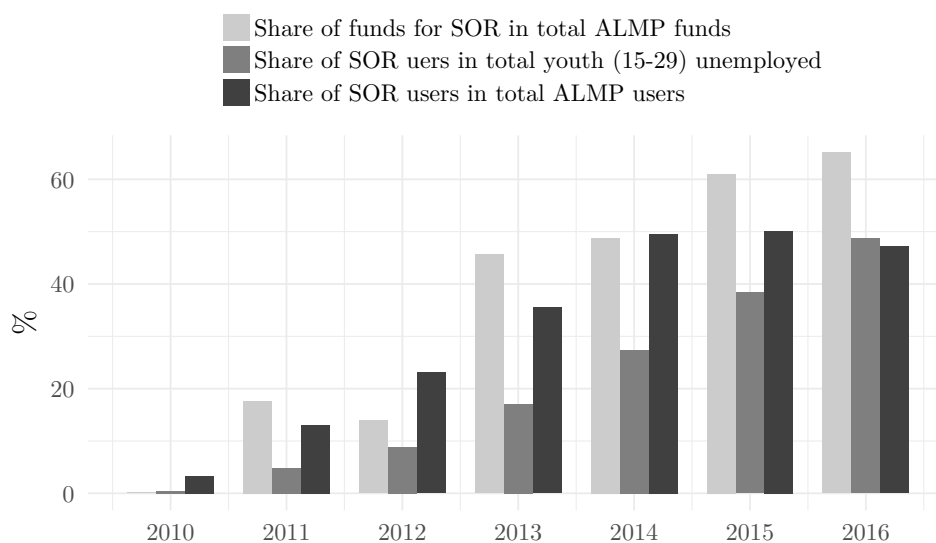
<sup>6</sup>Data obtained from the Croatian Employment Service (HZZ) web page, accessed in January 2018.

<sup>7</sup>For details about the share of EU financing see Botrić (2017).

<sup>8</sup>Interestingly, those who finished secondary education constitute more than 70% of the unemployed youth aged 15–29, while those with tertiary education comprise only about 15% (from 9% in 2007 to 21% in 2016).

<sup>9</sup>According to the HZZ (2016) study, 1/3 of the participants in the program (2010-2013) was allocated to government administration bodies and bodies of local/regional self-government, while 40% was in public institutions. The study names two main sources that could have induced people in SOR program to engage more in the public sector. They explain the first one as *administrative panic* of the public sector regarding the employment restriction and the nonexistence of noncombatant military service that brought to the substitution of opening regular work positions by the SOR program. The second one deals with the specific type of inappropriate use of the measure nominated as *continuous circulation of participants* (named *institutionalized unpleasantness* that could have long-lasting effects of career trajectory via skill formation), which has been primarily present in government administration bodies and bodies of local/regional self-government (HZZ, 2016).

FIGURE 1 — Share of SOR intensity (%)



Source: Croatian Employment Service

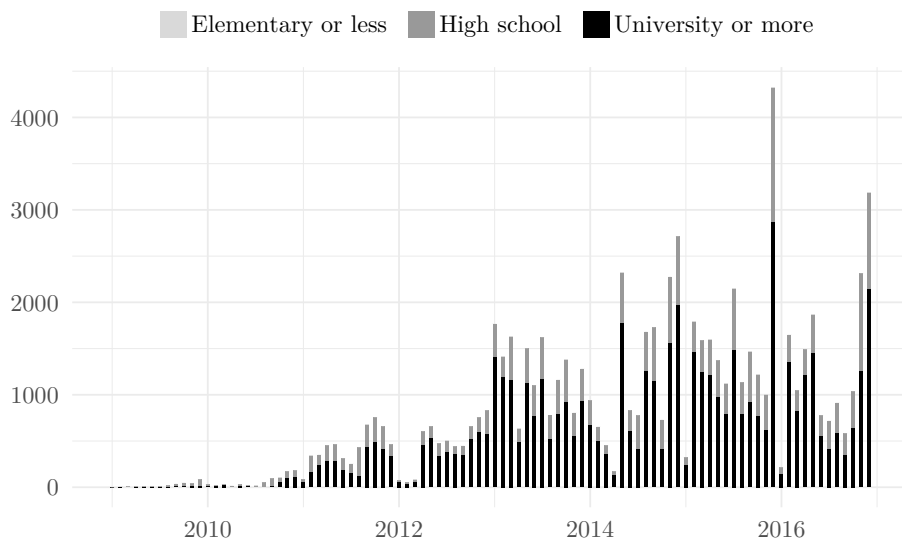
TABLE 1 — Share of unemployment exits due to SOR by education (%)

Year/Education	Elementary or less	Secondary school	University and above
2009	0.00	0.41	0.87
2010	0.01	0.47	2.06
2011	0.01	2.01	12.42
2012	0.00	1.38	15.93
2013	0.03	3.59	28.23
2014	0.01	3.68	25.85
2015	0.04	4.83	32.82
2016	0.01	4.48	29.70

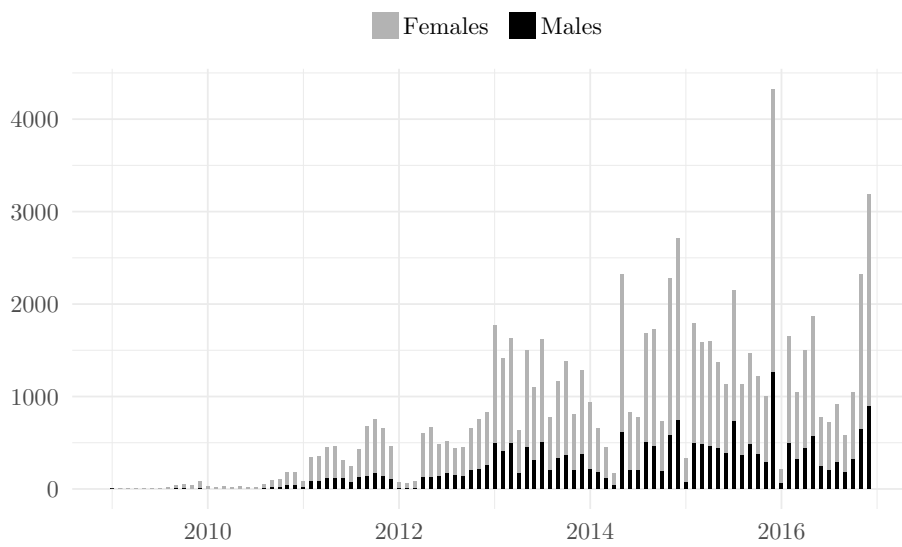
Source: Croatian Employment Service

FIGURE 2 — Unemployment exits due to SOR

(A) By education

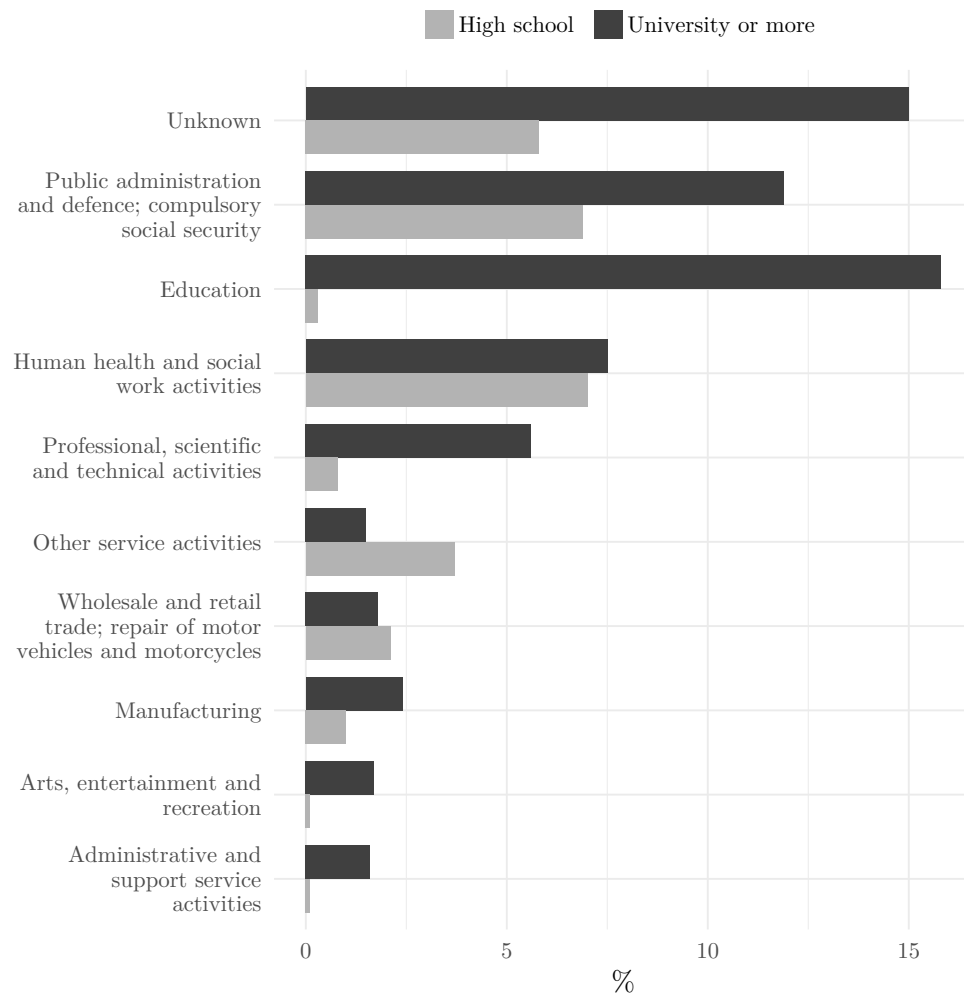


(B) By gender



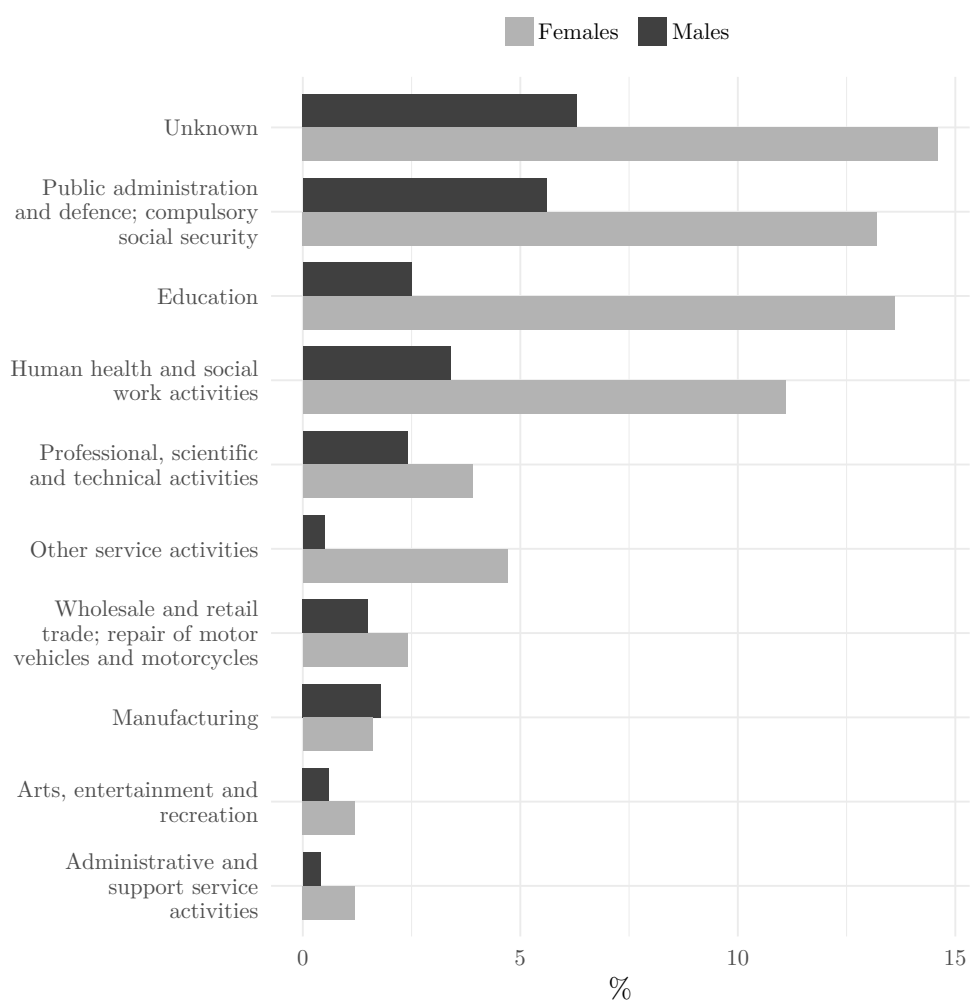
Source: Croatian Employment Service

FIGURE 3 — Share of SOR users (in total number of SOR users) by NACE and education



Source: Croatian Employment Service

FIGURE 4 — Share of SOR users (in total number of SOR users) by NACE and gender



Source: Croatian Employment Service



The measure also captured academic spotlight as scholars examined the effectiveness of the program. For example, both [Matić \(2014\)](#) and [Levačić \(2015\)](#) use qualitative research interviewing the participants in the program<sup>10</sup> with the main conclusion that participants are generally satisfied with the program. [HZZ \(2016\)](#) has conducted an econometric evaluation of its ALMPs in the 2010–2013 period, with similar conclusions.<sup>11</sup> Apart from positive effects, existing studies also emphasize some shortcomings of the new traineeship model. They argue that the measure favors highly educated individuals and those having additional income, thus overlooking financially vulnerable young people ([Matić, 2014](#); [Levačić, 2015](#); [HZZ, 2016](#)).<sup>12</sup> However, [HZZ \(2016\)](#) established that the measure has become a dominant pathway of labor market entry for a young person; given the low or inexistent job creation in the private sector, combined with employment restriction in the public sector, it is argued that the program might have been used by the employers to substitute regular employment ([Botrić, 2016](#); [HZZ, 2016](#)).

### III. METHODOLOGY AND DATA

#### *III.1. Methodology*

Existing studies that evaluate the SOR reform in Croatia ([Matić, 2014](#); [Levačić, 2015](#); [HZZ, 2016](#)) are mainly focused on the participants of the new traineeship model. This raises two important concerns. Firstly, concentrating only on participants of the measure implies issues of self-selection into the program, which complicates the identification making the estimates potentially biased. Secondly, by taking into account only those who participated in the program, potential spillover effects are neglected. For example, even if an individual was not participating in the measure, her/his eligibility might have altered her/his labor market outcomes. In particular, it could be that potential wage of an eligible individual, regardless of whether she/he participated in the measure or not, might have changed. We extend the scope of the evaluation by analyzing more broader set of labor market outcomes. In particular, given the potential crowding-out of other forms of employment, we analyze how the program affected employment, labor market participation in general and wages of youth population in Croatia.

Therefore, we propose a strategy which enables the estimation of unbiased intent-to-treat effects

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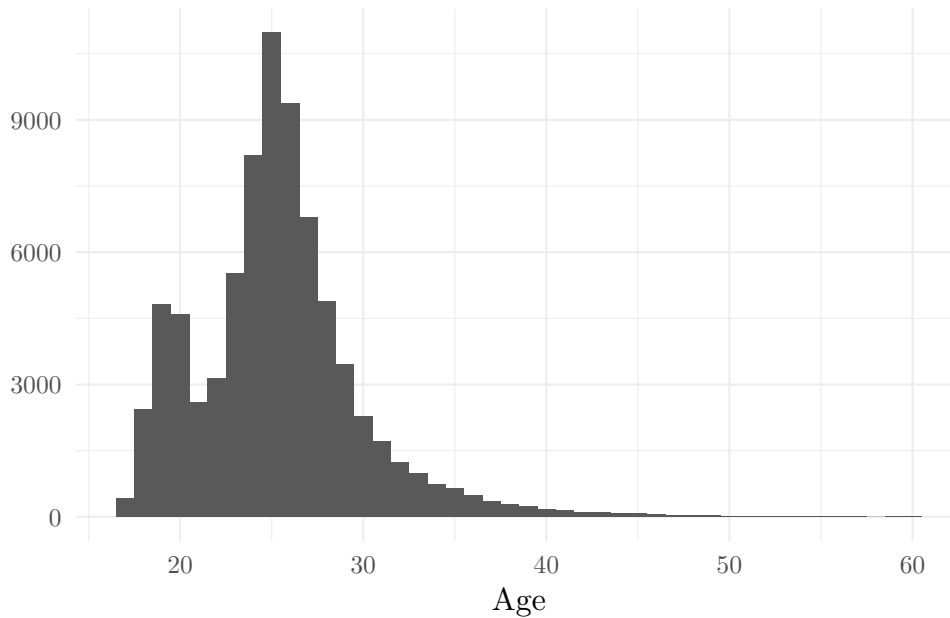
<sup>10</sup>[Levačić \(2015\)](#) also conducts a survey among (former) participants.

<sup>11</sup>Even though the study used counterfactual methods, one has to bear in mind that the control group consisted of those who were also registered at HZZ; however, youth population in Croatia does not have much incentives to be registered as they are not entitled to any unemployment benefits, which raises serious selection issues.

<sup>12</sup>Additionally, [HZZ \(2016\)](#) emphasizes a so-called *skimming effect* as participants in the program are those who have the highest employment probability anyway.

focusing our analysis on SOR eligible, or potentially eligible, individuals. In particular, we define a person aged from 18–29 as a potentially eligible individual,<sup>13</sup> and person aged 35–44 as not eligible. Figure 5 displays the participants in SOR program by age, from which we can infer that the vast majority of program participants were individuals below 30 years of age.<sup>14</sup> Therefore, by excluding overlapping age groups (30–34) we take those generally not eligible for the program as our control group. If our control group (35–44-year-olds) contains program participants, our estimates are lower bound of the true effect as they are also affected by the new program. By comparing the outcomes of groups based on eligibility status before and after the reform, we can infer unbiased causal intent-to-treat effect of the reform thus documenting the totality of its effects.

FIGURE 5 — Histogram of SOR users by age



Source: Croatian Employment Service

In particular, we estimate a following equation:

$$y_{iat} = \alpha + \beta \text{treat}_{ia} + \gamma \text{post}_{it} + \delta_{DD}(\text{treat}_{ia} \times \text{post}_{it}) + \phi' X_{it} + \epsilon_{iat} \quad (1)$$

where:

- $y_{iat}$  is a labor market outcome of a person  $i$  aged  $a$  in time  $t$ . We analyze five different labor

<sup>13</sup>According to Eurostat, dropout rates among 18–24-year-olds in Croatia are the lowest in the EU (less than 3%), which means that young population is rarely present on the labor market before the age of 18.

<sup>14</sup>HZZ (2016) reports that 80% of the participants in the program are youths between the ages 20 and 29. However, as participation in the SOR program was conditioned by the level of working experience, sometimes a person older than 29 also participated in the program.

market outcomes:

- $\log(1+\text{wage})$ ,
  - indicator of being employed,
  - indicator of being unemployed,
  - indicator of being inactive (excluding education),
  - $\log(\text{wage})$ , conditional on having a wage.
- $\text{treat}_{ia}$  is an indicator taking the value of 1 if an individual  $i$  belongs to the 18–29 age group. We define the aforementioned age group as the treated group, while the control group consists of individuals aged between 35–44.
  - $\text{post}_{it}$  is an indicator taking the value of 1 if an individual  $i$  is observed in treated period, which we define as May 2012 (excluding) onwards, since the SOR reform has been implemented on May 31, 2012.
  - $(\text{treat}_{ia} \times \text{post}_{it})$  is an interaction term which identifies the treatment. In particular, it takes the value of 1 if an individual  $i$  is aged 18–29 in the treated period (after May 2012). Hence,  $\delta_{DD}$  is the causal parameter of interest.
  - $X_{it}$  denotes individual-level covariates such as gender, years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year.

### III.2. Data

Data are obtained by pooling the 2007–2016 versions of the cross-sectional Croatian Labor Force Survey (LFS), which contains basic demographic characteristics, education variables and labor market outcomes. We use International Labour Organization (ILO) definitions for employment and unemployment so those participating in SOR program are considered as employed, even if they do not have a standard employment contract. LFS also provides the exact timing (year, month, week) of conducting the survey, thus enabling us to define the pre and postreform period. Descriptive statistics are presented in Table 2.

TABLE 2 — Descriptive statistics

	Age 18–29				Age 35–44			
	Prereform		Postreform		Prereform		Postreform	
	Mean	Std.dev.	Mean	Std.dev.	Mean	Std.dev.	Mean	Std.dev.
<b><i>Labor market outcomes</i></b>								
Log (1+wage)	2.961	3.896	2.352	3.690	4.862	4.033	4.634	4.142
Employed	0.475	0.499	0.386	0.487	0.764	0.425	0.732	0.443
Unemployed	0.131	0.338	0.198	0.399	0.074	0.262	0.119	0.324
Inactive (excluding education)	0.394	0.489	0.416	0.493	0.162	0.368	0.149	0.356
Log wage*	8.064	0.412	8.120	0.408	8.169	0.552	8.304	0.510
<b><i>Covariates</i></b>								
Female	0.466	0.499	0.466	0.499	0.522	0.500	0.511	0.500
Age	23.189	3.375	23.198	3.438	39.844	2.836	39.595	2.883
Married	0.139	0.346	0.119	0.324	0.783	0.412	0.742	0.438
Years of education	12.248	2.166	12.558	2.233	11.471	2.544	11.940	2.770
<b><i>Education level</i></b>								
Elementary or less	0.119	0.324	0.108	0.310	0.210	0.407	0.153	0.360
High school	0.772	0.419	0.732	0.443	0.638	0.481	0.639	0.480
University or more	0.109	0.311	0.160	0.367	0.152	0.359	0.208	0.406
<b><i>Education field</i></b>								
General program	0.280	0.449	0.256	0.437	0.222	0.416	0.176	0.381
Teacher training	0.017	0.129	0.025	0.156	0.030	0.171	0.036	0.186
Humanities	0.014	0.118	0.020	0.140	0.016	0.124	0.017	0.129
Social sciences	0.186	0.389	0.174	0.379	0.177	0.382	0.187	0.390
Life sciences	0.026	0.160	0.050	0.218	0.029	0.168	0.042	0.201
Engineering	0.267	0.443	0.199	0.399	0.312	0.463	0.258	0.438
Agriculture	0.032	0.177	0.033	0.178	0.041	0.199	0.032	0.177
Health care	0.038	0.191	0.077	0.267	0.041	0.197	0.080	0.272
Services	0.140	0.347	0.133	0.340	0.132	0.339	0.138	0.345
<b><i>Region</i></b>								
Central Croatia	0.263	0.440	0.237	0.425	0.275	0.447	0.244	0.430
Zagreb area	0.218	0.413	0.221	0.415	0.219	0.414	0.200	0.400
Eastern Croatia	0.222	0.415	0.196	0.397	0.205	0.404	0.203	0.403
Adriatic north	0.114	0.318	0.121	0.326	0.126	0.331	0.139	0.346
Adriatic south	0.183	0.386	0.225	0.418	0.175	0.380	0.213	0.410
<b><i>Sample size</i></b>	31,972		21,939		26,231		18,132	

Note: Prereform sample ranges from the beginning of 2007 to May of 2012 (excluding), while postreform sample includes observations from May 2012 until the end of 2016. \*Conditional on having a positive wage, respective sample sizes: 11,739; 6,354; 15,611; 10,118.

## IV. RESULTS

Figures 6 and 7 and Table 3 present the results of the difference-in-difference estimation of the effect of the ALMP traineeship reform on selected labor market outcomes. As already mentioned, these estimates should be interpreted as intent-to-treat effects as we estimate the effect on the whole eligible cohort (18–29) thus capturing potential spillover effects on individuals who did not participate in the program.

As difference-in-difference strategy relies on the parallel-trends assumption, in order to constitute a control group (Angrist and Pischke, 2008) we make an effort to evaluate whether this assumption is credible. First, we plot unconditional means of all outcomes in order to visually inspect if the evolution of outcome means of the treated and the control group are parallel prior to the reform (Figure 6). In order to explore parallel trends assumption more formally, we estimate time-specific effect of the reform, conditional on covariates (Figure 7); ideally, time-specific differences between outcomes of groups should be insignificant and centered around zero prior to the reform. Since for some outcomes we observe a potential violation of parallel trends assumption (namely, employed, unemployed and inactive), following Autor (2003), our preferred specification also includes group-specific time trends which enables more flexible identification assumption.

Table 3 presents the results of difference-in-difference estimation for the same outcomes. For each outcome we run three different specifications: without any individual covariates (column 1), with individual covariates—gender, years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year (column 2), and with individual covariates and group-specific time trends in order to account for non-parallel trends of some outcomes (column 3). Given the potential violation of non-parallel trends in some outcomes, our preferred specification is in column 3.

Despite its proclaimed aim to ease labor market entry, we find stable evidence that the traineeship reform did not increase probability of being employed for the 18–29 cohort; in fact, our preferred specification yields, although insignificant, negative coefficient, while in other specifications the effect on employment is negative and significant. Therefore, the effect of the reform on the employment is at best neutral.<sup>15</sup> Results for unemployment indicate inconclusive and mixed findings: unconditional estimation and estimation with covariates (column 1 and 2) result with positive and

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<sup>15</sup>Hirshleifer et al. (2016) find that the average impact of vocational training on employment in Turkey is vaguely positive but statistically insignificant. Hämäläinen et al. (2018) also find that the youth guarantee in Finland had a negligible effect on unemployment, while it only moderately increased unsubsidized employment.

significant coefficients, indicating that the SOR reform actually increased youth unemployment; while including group-specific time trends reverses the sign of the effect, indicating that the probability of being unemployed dropped with the reform.<sup>16</sup>

However, we do find stable and statistically significant effects throughout our specifications. In particular, the effect of the measure on inactivity is stable, positive, and significant. For example, column 3 indicates a 2.1–percentage point increase in being inactive for a potentially eligible cohort, which constitutes 19.1% of the sample mean. Also, the preferred specification reveals a 5% drop in wages (conditional on having one), an expected effect as remuneration for the individuals in the program was administratively set well below the minimum wage.<sup>17</sup>

Figure 7 displays the time-specific effects of the new traineeship model (as in Autor (2003)) on the selected labor market outcomes. This type of exercise enables three separate insights: if parallel-trends assumption is credible (by analyzing whether time-specific effects prior to the reform are centered around zero and not significant); if the effects change with the introduction of the reform (indicating that the estimated effect does come from the reform); and if the effect is changing through time (by observing magnitudes of the effects once the new program starts operating).

We do not record any positive effects on employment once the measure is introduced (Figure 7b); in fact, we observe significant negative effect, thus we argue again that the existence of SOR program was, at best, neutral for employment probability. As our estimation period expands over almost five years after the reform of the program had been introduced, the negative (neutral) effect on employment probability cannot be attributed to lock-in effects. However, it can arguably be attributed to the structure of SOR placements; as the large portion of youth participants in the SOR program ended up in the public sector (see Figures 3 and 4), which has had a restriction on new employment,<sup>18</sup> youth employment became mostly of temporary nature.<sup>19</sup> Similarly, we do not find evidence that the measure mitigated the issue of unemployment.

However, we do observe some evidence that the measure pushed a portion of individuals into in-

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<sup>16</sup>The unclear effects of the SOR reform on unemployment could be related to specific recession context as in Cammeraat et al. (2017) that attribute their result of no effect of reducing the total number of NEETs by the mandatory activation program for young welfare recipients in Netherlands to a severe economic recession.

<sup>17</sup>We additionally estimate the effect of the reform on prolonging education and find no significant effects; other definition of inactivity—that includes individuals in education—is also used and yields similar results as definition of inactivity used in baseline estimations. Since most users of the SOR measure were matched to public sector, we estimate the effect of the reform on being employed in the public sector; however, the age composition of public sector employees was profoundly altered at the time so we are not able to find credible control group which would enable unbiased estimates.

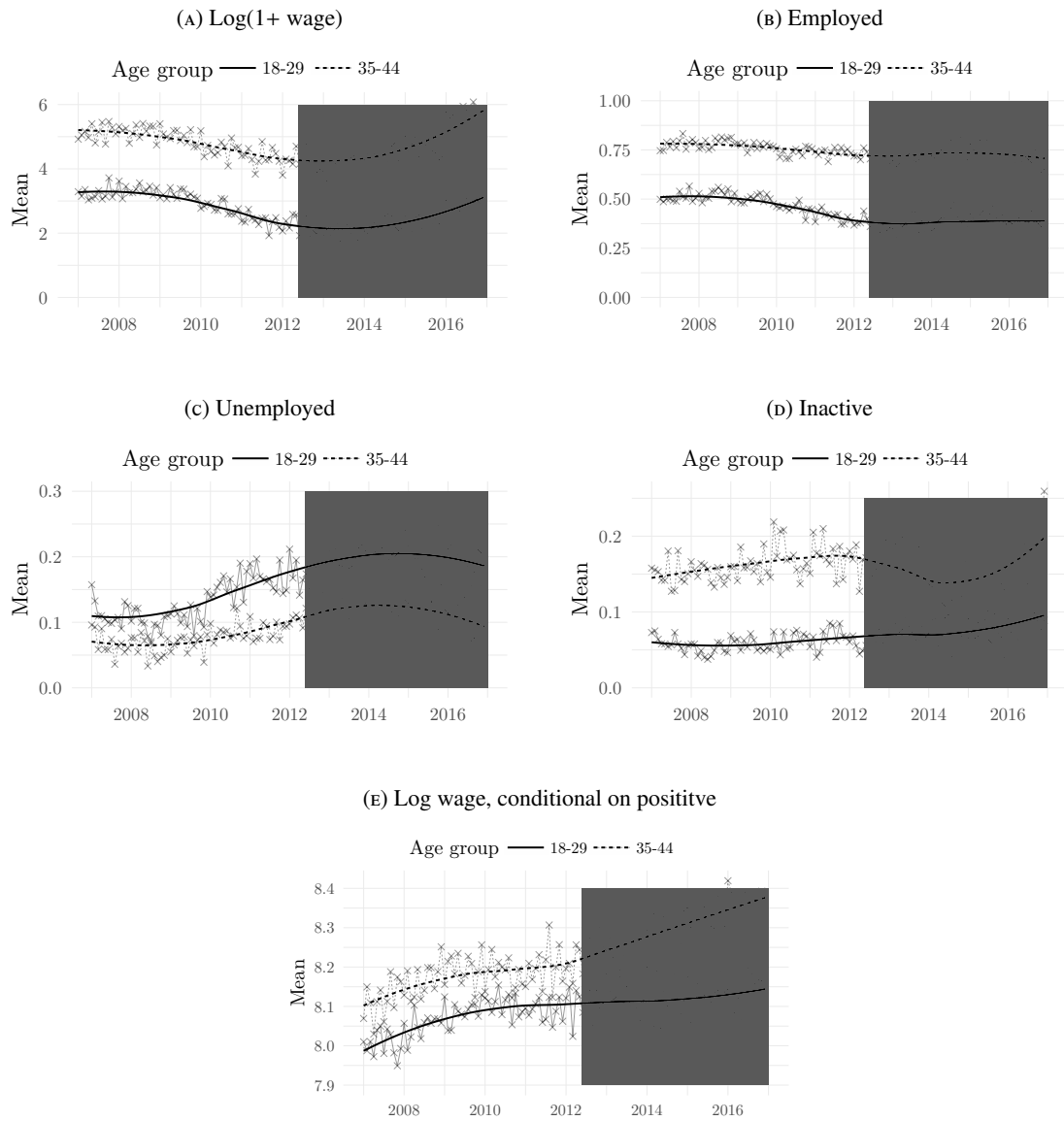
<sup>18</sup>Complete ban on new employment and/or replacement employment with those who retire.

<sup>19</sup>Interestingly, Card et al. (2017) found that ALMP programs that included public employment are mainly negligible or even exert negative effects on the participants.

activity. Since the program implied net wages for youths of around 210 and 315 euro, which at the time was 29% and 43% of the average net wage, arguably, this amount was below reservation wage for some individuals thus destimulating them from even searching for a job. Figure 7e provides convincing empirical evidence that the log wage of individuals actually decreased with the reform: the effect comes with a short lag and is stable in magnitude through time.

To sum up the insights from baseline specifications, we do not find any positive effect on employment or decreased probability of being unemployed. While significant and negative wage effect is expected as remuneration was administratively set, evidence of increased inactivity status, combined with neutral employment and unemployment effects, casts a serious shadow over the effectiveness of this active labor market policy and the way it unfolded.

FIGURE 6 — Parallel trends assumption



Note: Crosses represent a monthly mean of a particular outcome, lines represent smoothed means using local linear regression, gray area represents the reform time frame.



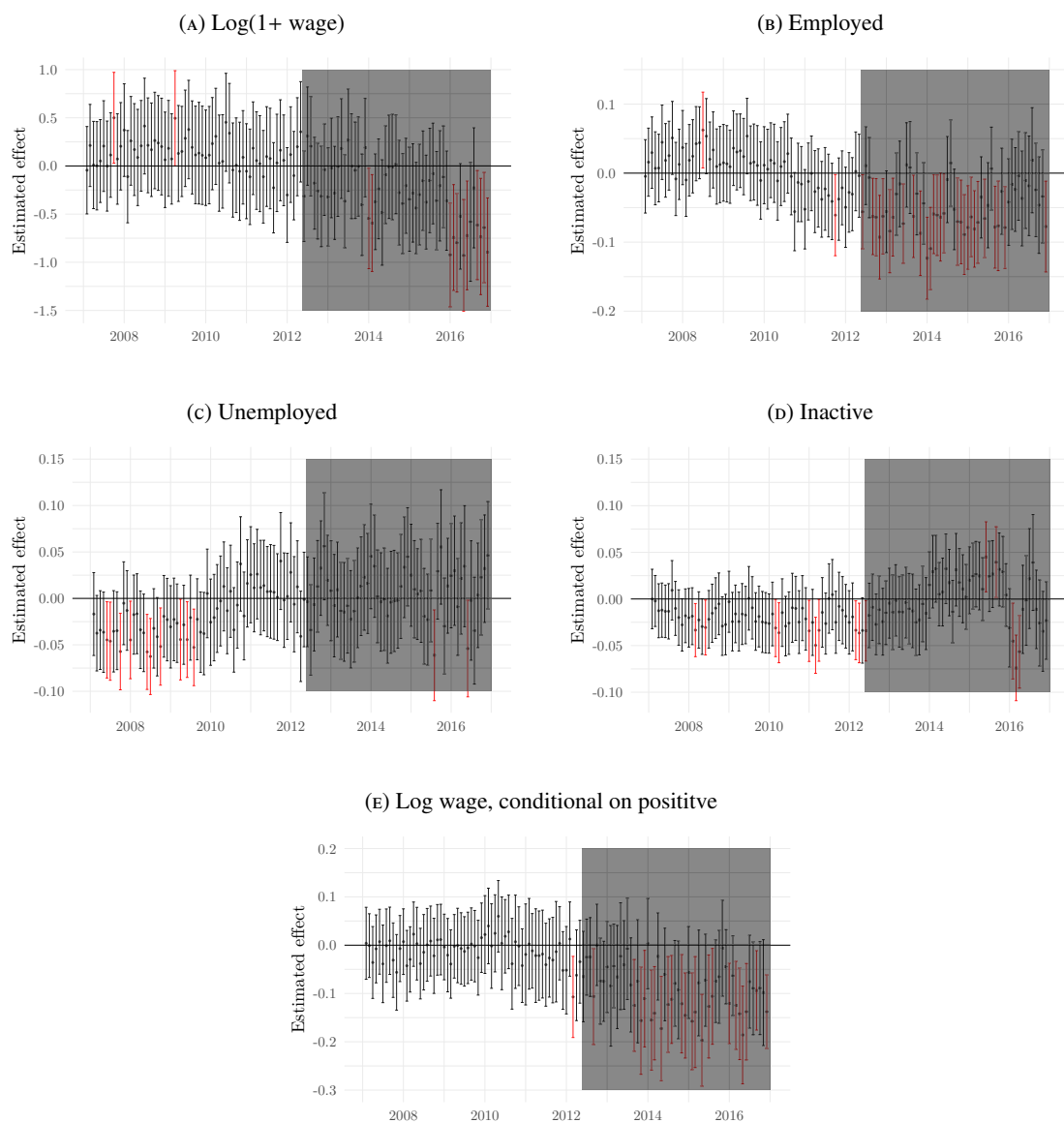
TABLE 3 — The effect of SOR on labor market outcomes (18–29 vs. 35–44)

	Variable mean	(1)	(2)	(3)
Log (1 + wage) N = 97,602	3.64	-0.381*** (0.065)	-0.417*** (0.062)	-0.162 (0.114)
Employed N = 97,602	0.58	-0.057*** (0.007)	-0.055*** (0.007)	-0.016 (0.012)
Unemployed N = 97,602	0.13	0.023*** (0.005)	0.023*** (0.005)	-0.016* (0.009)
Inactive N = 97,602	0.11	0.028*** (0.005)	0.020*** (0.005)	0.021** (0.009)
Log wage (given having one) N = 46,706	8.16	-0.079*** (0.011)	-0.081*** (0.010)	-0.050*** (0.018)
Covariates included		No	Yes	Yes
Group-specific time trends		No	No	Yes

Note: Standard errors are clustered at the household level. Each coefficient is the effect of the reform on a different outcome variable. Covariates include gender, years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

FIGURE 7 — Dynamic effects



Note: Dots represent a point estimate of time-specific difference in outcomes between a non-eligible and eligible group conditional on gender, years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year. Vertical lines represent 95% confidence interval calculated using standard errors clustered at the household level. Red lines indicate a statistically significant time-specific effect at the 5-percent level, while the gray area denotes the reform time frame.

#### *IV.1. Robustness checks*

In order to reinforce baseline results we use alternative definitions of the treated and the control group (Table 4). Firstly, as most of SOR users were university graduates (Figure 3 and 2a), in order to pinpoint the effect with more precision, we narrow down a potentially eligible group to those aged from 23 to 27 (column 1 of Table 4). Column 2 of Table 4 presents the results using baseline definition of potentially eligible group (18–29), but choosing an older age group (45–54) as a control, while column 3 displays results from placebo estimation where we define an eligible group as 35–44 and control as 45–54 year olds. All estimations use the most comprehensive set of covariates, including group-specific time trends, while standard errors are clustered at the household level.

Results shown in column 3 of Table 4 indicate that the baseline results presented in the previous section indeed capture the effects of the SOR reform. In particular, all the point estimates from this placebo exercise are insignificant thus implying that previous conclusions are not driven by some other confounder which might shift labor market perspectives of other age cohorts.

Estimates presented in the first two columns additionally support the baseline interpretation. While narrowing down the potential eligible group yields very similar conclusions as in Table 3, with the exception that now, expectedly, the wage effect is stronger as estimates show a 9.3% decrease of log wage (conditional of having one); all estimates, apart from wage effect, are insignificant in column 2. Figure 8 present the time-specific effect for wages, using this alternative specification and further corroborate our results.

Therefore, using these alternative difference-in-difference designs we find robust evidence that wages of the whole eligible cohort declined, and no positive effect of employment status, or mitigation of unemployment and inactivity; furthermore, we even find partial empirical evidence that the SOR reform propelled a portion of individuals into inactivity making this ALMP unsuccessful in its primary objective.

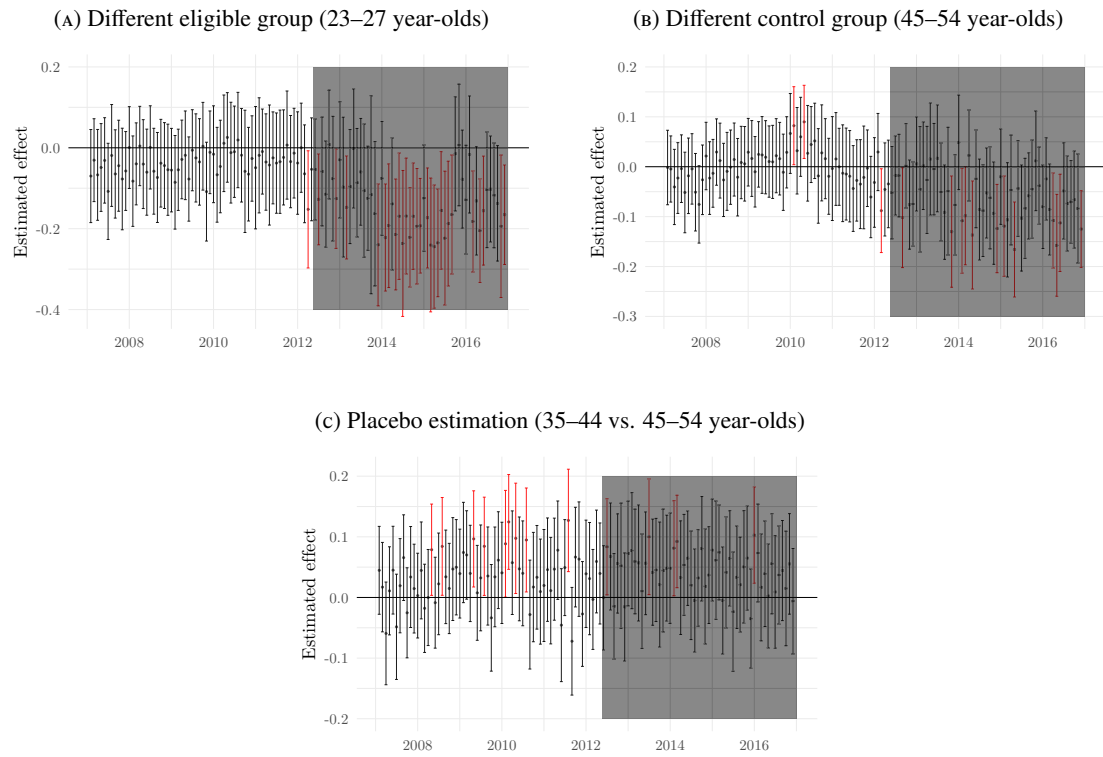
TABLE 4 — Robustness checks

	Different definition of eligible group (23–27-year-olds)	Different definition of control group (45–54-year-olds)	Placebo estimation (35– 44 vs. 45–54-year-olds)
Log (1 + wage)	–0.249 (0.157)	–0.022 (0.094)	0.117 (0.117)
Employed	0.002 (0.018)	–0.014 (0.011)	–0.002 (0.012)
Unemployed	–0.024* (0.014)	–0.005 (0.008)	0.010 (0.008)
Inactive	0.022** (0.011)	0.012 (0.009)	–0.008 (0.011)
Log wage (given having one)	–0.093*** (0.023)	–0.058*** (0.018)	–0.012 (0.018)
Covariates included	Yes	Yes	Yes
Group-specific time trends	Yes	Yes	Yes

N = 61,309; 114,197; 104,683 and in case of log wage (given having one) N = 33,787; 49,465; 57,093. Note: Standard errors are clustered at the household level. Each coefficient is the effect of the reform on a different outcome variable. Covariates include gender, years of education, dummy for being married as well as dummies for education type, education level, age, regions of residence and survey year.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

FIGURE 8 — Robustness check: log wage (conditional on having one)



Note: Dots represent a point estimate of time-specific difference in outcomes between a non-eligible and eligible group conditional on gender, years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year. Vertical lines represent 95% confidence interval calculated using standard errors clustered at the household level. Red lines indicate the statistically significant time-specific effect at the 5-percent level, while the gray area denotes the reform time frame.

#### *IV.2. Heterogeneous effects and effects beyond the mean*

In order to provide more detailed documentation of the reform impact we also present estimations of different heterogeneous effects. Table 5 presents the results of estimation of our preferred specification in the same manner as in Table 3 or Table 4 but separately for males, females, secondary-school and university graduates, while Figure 9 does the same as in Figure 8 but separately for gender and education levels.

With respect to gender heterogeneity, we observe that increased inactivity and lower wages (conditional on having one) in the baseline results are actually driven by females' inactivity and wage drop. While this is somehow expected, as Figures 2 and 4 show that females were more frequent users of the program, given that we control for both the level and the field of education, this result indicates that the Croatian labor market is somewhat gender-separated.

Additionally, our results indicate that individuals with a higher education experienced a massive drop in log wages of 16%, while other outcomes were not significantly altered by the introduction of the measure.<sup>20</sup> Although secondary-school graduates did not experience a drop in wages (conditional of having one) there is compelling evidence that some of them were pushed into inactivity.<sup>21</sup> Therefore, while there is some heterogeneity in the reform effects, one cannot characterize them as distributional: no group analyzed in this section has benefited from the reform, some of them experienced more adverse effects, namely females and university graduates.

Lastly, in order to explore the drop in log wages in more detail, in Figure 9 we plot other moments of the distribution, apart from the mean. As the drop in mean log wage (conditional of having one) can be viewed as a mechanical one given that the remuneration was administratively set, we descriptively explore whether percentiles of the wage distribution also changed due to the reform possibly indicating that the whole wage distribution has shifted. In Figure 9 we plot chosen percentiles of wage distribution across groups and time and we observe a wage drop across the entire distribution. Panels 10c and 10d indicate that 75th and 95th percentile of the wage distribution also decreased due to the reform, thus providing evidence that the adverse effect of the SOR reform on wages is not only mechanical as 95th percentile of wages should not be affected by the left-tail of wage distribution. Hence, it seems that the result obtained for the mean wage effect operates on higher parts of wage distribution as well.

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<sup>20</sup>While estimates of the effect on unemployment and inactivity are negative, they are not significant, hence we cannot reject the hypothesis that the reform did not have any effect on decreasing unemployment and inactivity.

<sup>21</sup>Hämäläinen et al. (2018) also find no evidence that the youth guarantee in Finland improved the labor market prospects of young uneducated people.

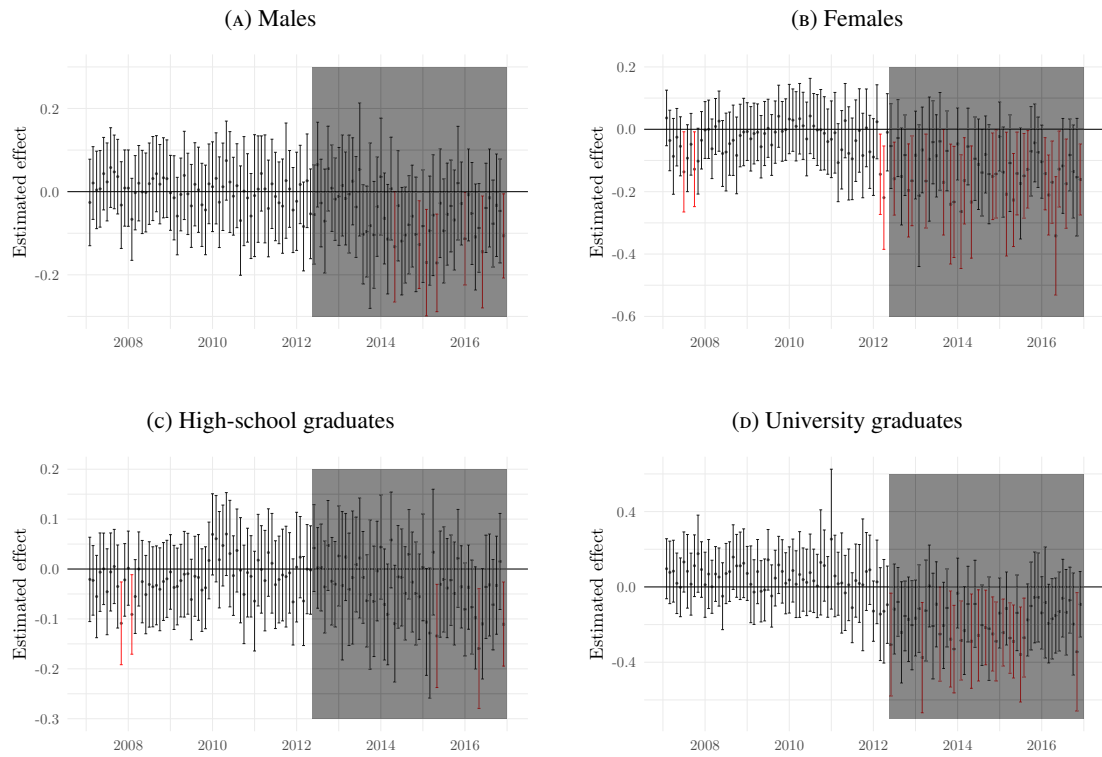
TABLE 5 — Heterogenous effects

	Males	Females	High-school graduates	University graduates
Log (1 + wage)	-0.046 (0.156)	-0.255* (0.147)	-0.270** (0.136)	0.247 (0.320)
Employed	-0.010 (0.017)	-0.022 (0.017)	-0.020 (0.015)	0.022 (0.029)
Unemployed	-0.016 (0.013)	-0.015 (0.013)	-0.012 (0.011)	-0.014 (0.023)
Inactive	0.015 (0.012)	0.027** (0.013)	0.026** (0.011)	-0.025 (0.016)
Log wage (given having one)	-0.018 (0.024)	-0.089*** (0.027)	-0.022 (0.019)	-0.160*** (0.045)
Covariates included		Yes	Yes	Yes
Group-specific time trends		Yes	Yes	Yes

N = 49,823; 47,779; 68,889; 14,704 and in case of log wage (given having one) N = 23,719; 19,987; 30,912; 8,162. Note: Standard errors are clustered at the household level. Each coefficient is the effect of the reform on a different outcome variable. Covariates include gender (if applicable), years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year.

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

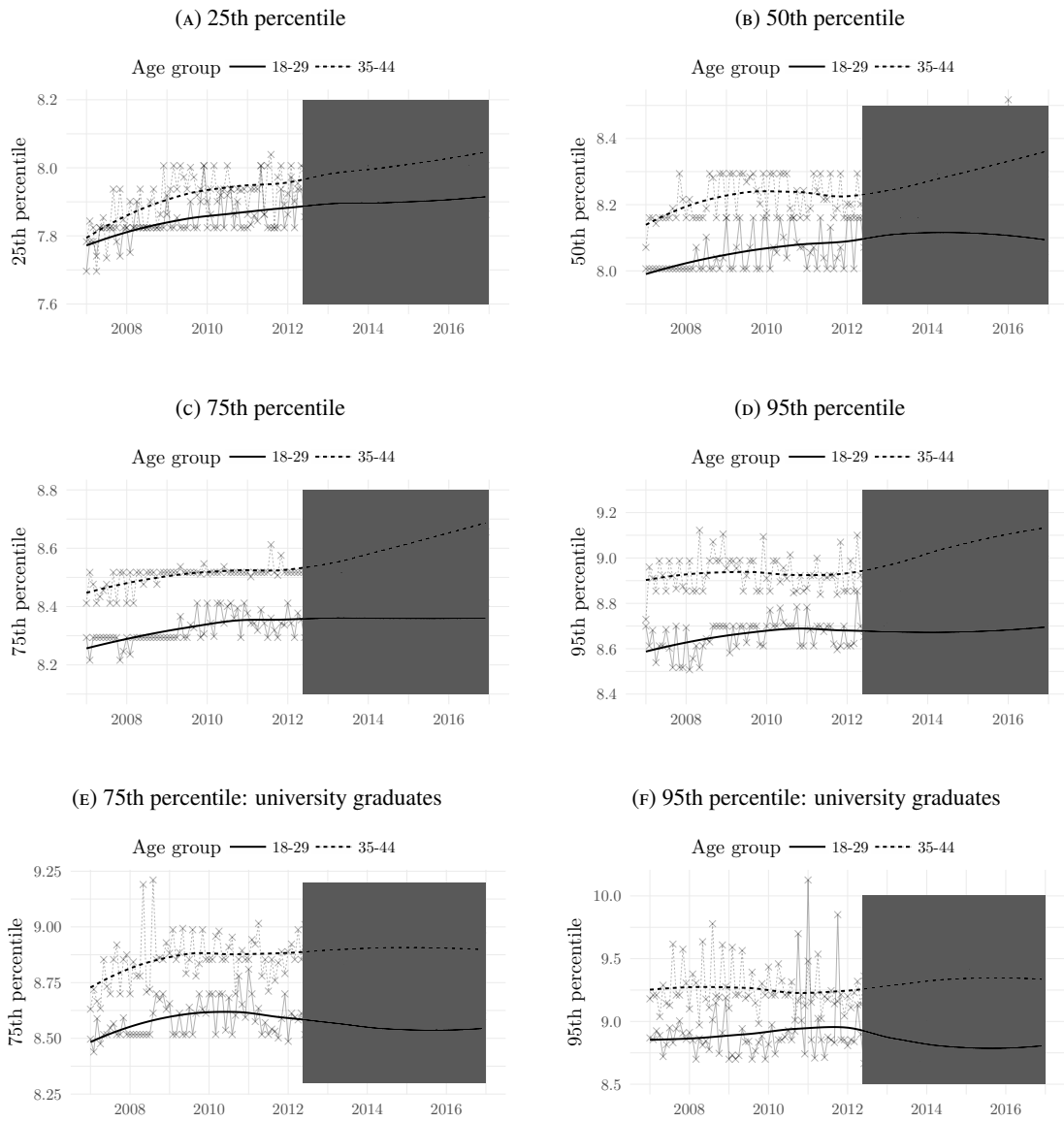
FIGURE 9 — Heterogeneous effects: log wage (conditional of having one)



Note: Dots represent a point estimate of time-specific difference in outcomes between a non-eligible and eligible group conditional on gender, years of education, age, dummy for being married as well as dummies for education type, education level, regions of residence and survey year. Vertical lines represent 95% confidence interval calculated using standard errors clustered at the household level. Red lines indicate the statistically significant time-specific effect at the 5-percent level, while the gray area denotes the reform time frame.



FIGURE 10 — Wage effect beyond the mean



Note: Crosses represent a monthly statistic of a particular outcome, lines represent smoothed means using local linear regression, gray area represents the reform time frame.

## V. CONCLUSIONS

In this paper we evaluate the effects of the ALMP reform, the so-called SOR measure on the labor market outcomes of young subpopulation in Croatia. The measure—originally not intended for employment but for training—has become almost an exclusive pathway to first labor market entry of young population, especially for university graduates, and has also become the most used active labor market policy in Croatia. Instead of focusing on participating individuals, as does the rest of the literature on the reform, we focus on the *potentially eligible* SOR individuals, which enables us to avoid bias arising from selection into measure, and to estimate the effect on eligible non-participants.

Our results provide evidence that the measure was neutral in terms of employment and unemployment probability, and we find some indications that the probability of inactivity has increased due to the reform. These results, combined with the negative effect we find across the wage distribution, reveal that this active labor market policy, and the way it unfolded, did not meet its goal.

While analyzing this measure is an important issue for the Croatian labor market and public policy, this paper has broader implications. Since youth employment is an important European issue, we offer valuable insights into effectiveness of active labor market policies in improving labor market prospects of young individuals. Though only tangentially, this research opens questions of the appropriateness of the use of EU funds in the new member states as well. Apart from fresh insights, this paper also opens new research avenues. For example, conditional on data availability, one could estimate long-run effects on labor market outcomes of beneficiaries of the measure, in terms of skill formation and subsequent employment match. Also, given the broad set of issues related to demography, one could also analyze the effect of this type of measure on homeownership, migration, and fertility decisions.

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