Three Essays on Policy Evaluation

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Overview

Over the last two decades there has been a proliferation of literature on program evaluation. Many researches in economics look at the causal effect of exposure of units to programs on some outcomes through econometric and statistical analysis. The units are typically economic agents such as individuals, households, markets, firms, counties, states or countries. The programs can be job search assistance programs, educational programs, vouchers, laws or regulations, drug therapies, environmental exposure or technology shocks.
Rubin potential outcomes framework seems to be the dominant framework in which the aim is to compare the two potential outcomes for the same unit when he or she is exposed and not exposed to the program (or treatment)[1]. However, each unit can be only exposed to one levels of program: an individual may enrol or not in a training program or he (or she) may be subjected or not to policy. We can refer to this as the *fundamental problem of causal inference* (Holland, 1986; Imbens and Wooldridge, 2008).

The impossibility to compare the same individual at different treatment status induces to resolve the issue thinking in term of *counterfactual*.
We need to compare distinct units at different levels of treatment. This means to compare different physical units or the same physical unit observed at different times. But each individual or unit who chooses to enrol in a program is (by definition) different from that who chooses not to enrol. These differences may invalidate causal comparison of outcomes by treatment status. Indeed, the fear in this econometrics literature is traditionally related to endogeneity, or self-selection, issues[2].

The simplest case for analysis is when assignment to treatment is randomized, and thus independent from the covariates as well as the potential outcomes. It is straightforward to obtain attractive estimators for the average effect of treatment in randomized experiments

[1]Starting from the seventies, Rubin (1974, 1977, 1978) proposed to interpret the causal effect as comparison of so-called potential outcomes, namely pairs of outcomes define for the same unit given different levels of exposure to the treatment. This represent the dominant approach to the analysis of causal relationship in observational studies known with the label of *Rubin Causal Model*.

[2]Many of the initial theoretical studies focused on the use of traditional methods for dealing with endogeneity, such as fixed effect methods from panel data analyses and instrumental variables methods. Subsequently, the econometrics literatures has developed new approaches, requiring fewer functional form and homogeneity assumptions (Imbens and Wooldridge, 2008).

(e.g. the difference in means by treatment status). Although there have been some example of experimental evaluations, they remain relatively rare in economics.
More common is the case where economists analyse data from observational studies. Observational data generally create challenges in estimating causal effects referred to unconfoundedness, exogeneity, conditional independence, or selection on observable characteristics[3].
Estimation and inference of causal effect under unconfoundedness assumption requires that conditional on observed covariates there are no unobserved factors that are associated both with the assignment and with the potential outcomes[4]. Without unconfoundedness assumption there is no general approach to estimating treatment effects and various methods have been proposed (for a review, see Imbens and Wooldridge 2008).

Where additional data are present in the form of samples of treated and control units before and after the treatment comparisons can be made through a *difference-in-difference* approach. The simplest setting is one where outcomes are observed for units observed in one of two groups (i.e. treated and control) and in one of two time periods (i.e. pre-treatment and post-treatment). Only units in one of the two groups, in the second time period, are exposed to a treatment. There are no units exposed to the treatment in the first period, and units from control group are never observed to be exposed to the treatment.
To estimate the causal effect, the average change over time in the outcomes of control group is subtracted from the change over time in the outcomes of treated group. This double differencing removes biases in second period comparisons between the treatment and control group, that could be the result from permanent differences between those groups, as well as biases from comparisons over time in the treatment group, that could be the result of time trends unrelated to the treatment.

Where the assignment of treatment is a deterministic function of covariates, comparisons can be made exploring continuity of average outcomes as a function of covariates. This setting, known as the *regression discontinuity design*, has a long tradition in statistics though only recently it has attracted much attention in the economics literature[5].
The basic idea is that assignment to the treatment is determined, either completely or partly, by the value of a predictor (i.e. an individual's observable characteristic) being on either side of a common threshold. This generates a discontinuity in the conditional probability of receiving the treatment as a function of this particular predictor. Any other characteristic, between elected and unelected individual, is assumed to be smooth.
As a result, any discontinuity of the conditional distribution of the outcome, as a function of this covariate at the threshold, is interpreted as evidence of a causal effect of the treatment[6].

[3]For a review on this literature, see Imbens and Wooldridge (2008).

[4]Unconfoundedness implies that we have a sufficiently rich set of predictors for the treatment indicator, such that adjusting for differences in these covariates leads to valid estimates of causal effect.

[5]For recent review in the economics literature, see Van der Klaauw (2008), Imbens and Wooldridge (2008) and Lee and Lemieux (2010).

[6]It may be useful to distinguish between two general setting, the *sharp* and the *fuzzy* regression discontinuity design. In the *sharp* regression discontinuity design, the assignment to treatment is a deterministic function of one of

This thesis presents three essays of policy evaluation using the above quasi-experimental approaches. The research covers two different type of policies. On the one hand, we assess the effects on crime induced by a marijuana decriminalization policy exploiting the reforms still ongoing in the United States, on the other hand, we evaluate the impacts of the labour market reforms on labour market outcomes by using the recent changes in Italy occurred after the *law 92/2012* (the so-called *Fornero reform*) like identification tool.
Depending on the specific subject, the analysis is carried out from a specific empirical point of view.

The first essay sheds light on the relationship between Medical Marijuana Laws and crimes in United States using counties level data. The set of judicial rules on the therapeutic consumption, production and distribution of cannabis at State level — started since 1996 in the United States — is known as Medical Marijuana Law (MML). It recognises the medical value of marijuana and provides a legal defence for patients who used and possessed marijuana under recommendation of a physician.
The purpose of policy was the pain reduction for which the States allow doctors to prescribe marijuana as a pain killer also for general complaints related to pain, such as migraines, back pain and other pathologies. But, since the list of illness is quite broad, *de facto*, MML allows wide possibility for recreational use of marijuana masked like therapeutic consumptions (Chu, 2012). Hence, the assessment of policy on crime seems suitable.
The research closely examining the importance of policy dimensions and the timing of the core elements of MMLs. In the U.S. States there have been three main actions that have involved the cannabis use for medical purpose: the mere decriminalization of marijuana, the permission of home cultivation for patients and caregivers, the licence for selling marijuana in authorized dispensaries.
We interpret dimensions as design choices of policy maker on legal marijuana market by distinguishing between *demand side approach*, aimed to merely decriminalize cannabis, and *supply side approach*, directed to provide legal sources of supply for marijuana. This permits to explain the possible transmission channel trough which Medical Marijuana State Laws can affect crime.
We test three possible links between drugs liberalization reforms and crime (i.e. *pharmacological, economic, and systemic channels*) finding evidence for only one of them (i.e. *systemic channel*).
The analysis uses the *Uniform Crime Reporting Program Data* (UCR, 2013) which reports the number of arrests by type of offence from 1994 to 2014 at the U.S. county level.
Since we have data of treated and control counties before and after the implementation of MML, we employ *difference-in-difference* approach by considering several types of crime such

the observable covariates. In the *fuzzy* regression discontinuity design the probability of receiving the treatment need not change from zero to one at the threshold. The design only requires a sufficiently large discontinuity in the probability of assignment to the treatment at the threshold.

as *violent* and *property* crimes, and also felonies for *narcotic possession* (i.e. cocaine, heroine etc.).
We exploit the assessment of Medical Marijuana Law to highlight an important question in program evaluation concerning the heterogeneity of treatment effect. Even if the average treatment effect is zero, it may be important to establish whether a targeted implementation of intervention or different levels of treatment across the population could affect average outcome.
We find that a simple dichotomous indicator of Medical Marijuana Law (i.e. the average treatment effect on all the U.S. States that passed the policy) may mask crucial dynamics underlying the relationship between policy and crime. Assuming a homogeneous impact of policy on crime, regardless the action implemented, the dichotomous indicator of MML captures only the net effect of the regulatory tools put in place by the legislator. On the contrary, the policy decomposition in key dimensions allows to discover different results which suggests a heterogeneous effects on crime according to the specific regulatory actions put in place by the legislator.
In detail, for burglaries, larcenies, and cocaine drug possession, the mere application of *demand side approach* increases the crime in counties that passed the policy compared to counties without MML. While, the joint application of *demand* and *supply approach* — which establish legal sources for supply marijuana — may be able to realize a *crowding-out effect* on these offences. The findings support the idea that the licit competition on the marijuana market, triggered by the policy, could push out the illegal trade decreasing the crime. Finally, we find a net reduction in murders and a net increase in synthetic drug possession for the U.S. counties subject to the Medical Marijuana Law relatively to counties never passed the policy.

The second and the third essays assess the impact of *law 92/2012*, implemented in Italy in 2012 (the so-called *Fornero reform*), on different labour market outcomes. The *law 92/2012* introduced numerous changes regarding employment relationships amending past discipline.
First. It substantially changed the discipline concerning the dismissals in firms above 15 employees. The reform established that in case of unfair dismissal, the dismissed worker has no longer the right to be reinstated as in the pre-reform period and receives a monetary compensation that ranges between 12 and 24 months pay. Thus the reform significantly reduces the firing cost borne by large firms.
Second. Starting from January 2013, the *Fornero reform* also changed the discipline on apprenticeships concerning to the minimum duration of contract (no less than six months), the maximum number of apprentices that an employer can hire per each skilled worker (passed from 1:1 to 3:2), and the minimum number of apprentices that an employer must stabilize into permanent contracts for hiring a new apprentice (at least the 30% of apprentices hired in the last 12 months).
Third. The *Fornero reform* implemented a new incentive program in favour of employers that recruit (on fixed-term or open-ended contracts) or stabilize into permanent agreements a worker aged 50 or more years.

The second essay (carried out with *Giovanni Pica*) estimates the effect of employment protection legislation on the flow of monthly hirings on open ended contracts using the aforesaid labour market reform passed in Italy in 2012.
Much empirical research has focused on the effects of dismissal costs on labour market outcomes. The evidence suggests that EPL decreases employment inflows and outflows with little effect on employment and unemployment stocks. The reason is that firing costs act, in expected discounted value, as hiring cost reducing the willingness of the firms to both fire and hire workers (Bentolila and Bertola, 1990; Blanchard and Portugal, 2001).
The most recent studies identify the causal impact of employment protection on labour market outcome exploiting within-county variation in EPL either across firms (e.g. of different size) or workers (e.g. of different age and/or tenure). The essay presented is in the line with within-county approach which allows to better control for time-varying unobserved characteristics that may affect labour market outcomes (act as confounding factors) compared to cross-country analyses.
The presence of both treated and control firms observed before and after the policy — where the assignment of treatment depends in deterministic way from the number of workers employed — allows to implement a *difference-in-difference* approach jointly to a *regression discontinuity design*. We thus exploit the differential law change between firms with more and less than 15 workers comparing hirings in firms just above and below the 15 employee threshold before and after the reform (July 2012).
The analysis is based on monthly data drawn from Italian Social Security (INPS) record for the period 2012 and 2014. The data provide information on the number of newly hired workers by firms size, province, sector, contract type, age and gender at a monthly frequency. The findings suggest that the reform raises monthly hirings on open-ended contracts by about 5.1 percentage points. The quantification of results reveals that the reduction of dismissal costs after the reform have induced about 4000 hirings per month in firms with more than 15 workers relative to firms with less than 15 workers. The effect of the reduction in EPL is not homogeneous across workers' types. The increase seems to be more pronounced for full-time, young, and blue-collar workers. Conversely, we find no significant effect on the number of conversions of temporary contracts into permanent ones.

The third essay evaluates the impact of labour policies aimed to improve the job possibilities for workers categorized as vulnerable (particularly in labour markets with stringent employment protection)[7].
Given the increasingly complicated transition from school to works, the youth appear a group more vulnerable compared to the past. Here the apprenticeship contract performs a crucial role by improving the job possibility and the stability of young workers (Berton et al., 2007; Casale et al., 2014).

[7]Evidences suggest that labour market prospects for youth and other marginal groups seem to worsen as a consequence of stringent EPL (Allard and Lindert, 2007; Bertola et al., 2007; Skedinger, 2010).

At the same time, the low employment rates for older workers pushed most OECD countries to experiment specific employment protections with the purpose to protect them from unemployment or/and to improve their job finding rates (Chéron et al., 2011).
The *Fornero reform* intervenes by changing the discipline of apprenticeship in Italy and implementing a new incentive program for workers aged 50 or more years.
The reform asymmetrically acted on the apprenticeships by changing the discipline in firms with more than 10 employees leaving the rules for firms below 10 unchanged.
Likewise, the new incentive program for workers aged 50 or more years, passed with the *Fornero reform*, cut the hiring costs in firms that recruit workers *over-50*, leaving unaffected the costs for hiring workers *under-50*.
These discontinuities in the regulation as well as the simultaneous presence of treated and control groups observed before and after the policy allow to implement a *difference-in-deference* method jointly to a *regression discontinuity design*. This quasi-experimental method permits to evaluate the causal effect of reform on the monthly hirings of apprentices and workers *over-50*.
We thus exploit the differential law change in apprenticeships between firms with more and less than 10 employees, comparing the hirings and the conversions into open-ended contracts of apprentices in firms just below and above the 10 employees threshold before and after the reform (January 2013). Similarly, we compare the recruitments and the conversions into permanent contracts of workers with more and less 50 years before and after the reform.
Also this analysis uses monthly data draw from Italian Social Security (INPS) record for the period 2012 and 2014.
The findings suggest that the change in apprenticeships increase the stabilization of apprentices into open-ended contracts by about 3.9 percentage points in firms with more than 10 employees relative to firms with less than 10. We also find a positive association between *law 92/2012* and the new recruitments of apprentices by about 7.1 percentage points in firms with more than 10 employees relative to firms with less than 10 employees.
The employer incentives for hiring and stabilizing the workers aged 50 or more years positively affect the recruitments into open-ended contracts of workers *over-50* relative to workers *under-50* by about 1.6 percentage point. We also find a positive association between the incentive program and the hirings into fixed-term contracts of workers *over-50* relative to workers *under-50*. Conversely, we don't find effects for the conversions into open-ended contracts of workers aged 50 or more years.

The rest of thesis is organised as follow. Chapter 1 evaluates the impact of decriminalization of cannabis on crime exploiting the Medical Marijuana Laws ongoing in the U.S. States. Chapter 2 explores the relationship between employment protection legislation and the flow of monthly hirings on open-ended contracts using the *law 92/2012* in Italy. Chapter 3 assesses the impact of policies designed to improve the job possibility of workers categorised as vulnerable (i.e. youth and older) using the 2012 Italian labour market reform.
Each essay contextualizes the topic by showing the literature status, the changes occurred

in the discipline, the identification strategy, the results, the sensitivity analysis and the conclusions.

Chapter 1

The Heterogeneous Effect of Medical Marijuana Law

1.1 Introduction

In December 2012, 16 U.S. States and the District of Columbia passed a Medical Marijuana Laws (MMLs) interesting more than 90 millions of American citizen. Others 7 U.S. States approved MMLs between 2013 and December 2014.
Although possession and use of marijuana is still illegal under federal law, states are authorizing individuals to possess and use cannabis for medical or recreational use, loosening the punishments associated with this kind of felonies[1]. Moreover, states like Alaska, Colorado, Oregon, and Washington legalized possession and recreational use of marijuana by all individuals over 21 years of age and several other states are considering similar mandates[2].
Medical use of cannabis became an increasingly contentious issue, in which the forces on either side of the prohibition-legalization debate see the introduction of Medical Marijuana legislations at states level as an initial step on the road to decriminalization of the drug. Federal government vehemently opposes state-level introduction of medical cannabis laws on a number of grounds, including a fear that they have the potential to increase use among the general population (especially young people) through sending the message that cannabis use is acceptable (Gorman and Huber, 2007).
Wide acceptance and risk of consumption abuse triggered by Medical Marijuana legislations

[1] Federal agencies such Drug Enforcement Administration (2015) continue to list marijuana as a *Schedule I drug*, according to *Controlled Substance Act* (1970), like heroine. Substance are classified in *Schedule I* drugs because they have no known or accepted medical purpose and pose a risk for abuse.
Drug Enforcement Administration (2015) classify substance and certain chemicals used into 5 distinct categories or schedules depending upon the drugs acceptable medical use, abuses, or dependency potential. As schedule increase, the drug abuse potential decreases (i.e. *Schedule V* has the least abuse potential).

[2] Alaska legalized marijuana for recreational use in *Measure 2* on November 4, 2015. Colorado legalized the sale and possession of marijuana for non-medical uses on November 6, 2012, including private cultivation. Oregon legalized recreational use of marijuana for people ages 21 and older trough the *Measure 91* on November 4, 2014. Washington permits anyone over 21 to carry one once of once, with *Washington Initiative 502* in 2012.

are spurring debates over the cost and benefits of these legalizations which might pour out their effects on broad social variables like public health, economics, and criminality.
A lot of opponents of marijuana laws for medical and recreational use, for instance, argue that they may induce increases in crimes.
This essay sheds light on the impact on crimes of Medical Marijuana Laws at the U.S. county level exploiting the heterogeneous regulatory actions adopted by policy makers. There have been four main approaches for the liberalization of the cannabis for medical purpose: the mere decriminalization of marijuana, the establishment of a mandatory patient registry, the permit to cultivate cannabis at home, the release of licence for selling cannabis in authorized dispensaries. The essay explores how each above dimension might influence the crime at county level.
These dimensions of MMLs can be classified in term of homogeneous choices of *market design*. We talk about *demand side policies* whether the regulatory action aims to the mere decriminalization of cannabis and *supply side policies* whether the regulation aims to provide legal sources of supply for marijuana.
This disaggregation of reform permits also to understand the potential transmission channel between Medical Marijuana Laws and crime. A *demand side approach* might induce an increase in the demand of marijuana without providing a legal source of supply. Hence, they induce individuals to address their increased demand towards illegal market (i.e. pusher or organized crime), inflating profits and power of illegal activities. The boost of illegal trade may raise violence on drug markets through the so-called *systemic transmission channel* as defined in Goldstein (1985). In this sense, the demand side approach positively impact on crime. On the contrary, *supply side approach* could lead to a drop in crimes because they provide legal sources of supply marijuana through the cultivation at home and the selling in authorised dispensaries. This would trigger a licit competition on marijuana market which could push out the illegal trade. The result may be a reduction of crime.
According to this hypothesis, we expect that the demand side policies should be associated with an increase of crime, while the supply side policies should be able to reduce the crime. The net effect on crime of the two policies is an empirical matter.
To assess this we use a *difference-in-difference* approach considering the U.S. county level data on several types of crime among period 1994-2014.
This essay contributes to expand the previous literature regarding the impact on crime of the Medical Marijuana Laws in the United States for several reasons.
First. While the most of previous studies use crime data at State level, here we employ data at U.S. county level. This allows to catch the cross-county heterogeneity so far neglected.
Second. We observe the crime for a longer time period (1994-2012) and for a wide sample of counties (96.5% of total) than the previous works, which usually analyse a shorter interval or a restricted group of States. The interval from 1994 to 2012 allows to investigate the crime trends both before and after the policies in each U.S. county.
Third. Most of previous studies analyse only two groups of felonies, violent and property crimes, according to classification of UCR (2013). Here we evaluate effects of MMLs on

violent and property crime as well as on the felonies for narcotic possession. The offences referring to narcotic substances (different from the marijuana) helps to understand whether the *gateway-effect* happens upon the approval of Medical Marijuana Laws[3].
Finally and crucially, our approach allows for a heterogeneous impact of policies on crime. Looking at approval timing of the Medical Marijuana legislations, we propose an original empirical model aimed to evaluate how each key dimension of MMLs may affect the crime. Classifying the regulatory actions like *demand side policies* (if they merely decriminalize cannabis) or *supply side policies* (if they provide legal sources of supply for marijuana), the model permits to test also the transmission channel between marijuana's decriminalization and crime. Only few works consider the decomposition of policy (see, for instance, Pacula et al. 2015) but none of them implements a model or an interpretation similar to ours.

The remainder of the essay proceeds as follows. In Section 1.2 we summarize the limited research on Medical Marijuana Laws in United States. Section 1.3 provides background on Medical Marijuana Legislation analysing the key dimensions of reforms. In this section, we also explain our decomposition of policy and the transmission channel between MMLs and crime hypothesised. Details on empirical strategy are reported in Section 1.5. The essay goes ahead to provide a description of our data (Section 1.4) while Section 1.6 contains the results. Section 1.7 presents sensitivity analyses designed to test the robustness of results. We conclude with a summary of our finding and its implication in Section 1.8.

1.2 Literature review

Economic and social literature has discussed the potential impact of drugs liberalizations in their different regulatory forms. Medical Marijuana legislations in United States are not kept out from these debates.
Medical researches provide clinical evidence on beneficial effect of marijuana for neuropathologies, to alleviate some symptoms associated with multiple sclerosis, and to help nausea associated with HIV (Corey-Bloom et al., 2012; Riggs et al., 2012; Wilsey et al., 2013). Others support the view that the marijuana legalizations could be associated with economics improvements. On the one hand, reforms may save taxpayers' money reducing costs associated with the arrests and detentions of non-violent individuals involved in the marijuana trade. On the other hand, they may permit to tax the trade of cannabis and its derivatives[4]. Resources which could be allocated towards public schools, infrastructure, and young campaigns on conscious use of narcotic substances.

[3]With the term *gateway-effect* (also called *gateway theory* and *gateway hypothesis*) refers to the phenomenon according which the use of less deleterious drugs precedes, and can lead to, future use of more dangerous hard drugs or crime. It is often attributed to the earlier use of one of several licit substances, including tobacco or alcohol, as well as cannabis (see DeSimone 1998).

[4]During the first month of entry in force of law, the sales of recreational marijuana in Colorado produced around 3.5 million dollars in taxes (Alford, 2014).

Opponents of marijuana decriminalization argue that widely use of cannabis may lead to worse schooling and work outcomes. DeSimone (1998, 2002), for instance, finds that use of each drugs (still marijuana) reduces the likelihood of employment, while increases the probability to use of cocaine evidencing the presence of *gateway hypothesis*. Yamada et al. (1996) reveals a significant adverse effects of cannabis use on high school graduate rates, while McCaffrey et al. (2010) observed that cigarette and marijuana smoking could be associate with greater high school dropout rate.

Studies regarding the Medical Marijuana Laws can be distinguished in two branches: those which analyse the effects on consumption of marijuana and those which analyse the potential impact on crime. There are many works investigating the relationship between MMLs and consumption of marijuana, alcohol or others narcotic substances in United States while there are few researches on the relationship between MMLs and crimes[5].
Among them, only Pacula et al. (2015) employ a decomposition of MMLs in key dimensions in order to inspect the effect of policy on recreational use of marijuana. Their findings suggest a heterogeneous impact of laws depending to the specific regulatory actions put in place by the legislator[6]. This suggests that to fully understand the effect of Medical Marijuana Laws we must take into account the potential heterogeneity associated with the single regulatory actions. This might be true for the impact on marijuana's consumption as much as that on crime.
About the relationship between MMLs and crime, Morris et al. (2014) find no significant impact with the exceptions of homicides and aggravated assaults for which they reveal negative relationship. Gavrilova et al. (2015), limiting analysis for the U.S. states on border with Mexico, find a significant negative impact of MMLs on violent crime with strong effects on robberies and homicides. Moreover, they identify a not clear effect of dispensaries and home cultivation on crime. However, using also *Supplementary Homicide Reports* (SHR) data, they confirm the fall of homicides (originated from decreasing in murderers among juvenile-gang). Alford (2014) finds that selling marijuana in dispensaries may have positive effect on property crimes while she doesn't find statistically significant effect for home cultivation. Finally, Anderson et al. (2011) find that legalization in U.S. states decrease traffic fatalities, particularly those involving alcohol. They observe also sharp decreasing in alcohol consumption suggesting that alcohol and marijuana are substitute.

[5]Referring to the impact on cannabis consumption, Khatapoush and Hallfors (2004), Gorman and Huber (2007), and Harper et al. (2012) find not significant impact of MMLs on marijuana use. Instead, Anderson et al. (2011), analysing passage of laws in Rhode Island, Vermont, and Montana, finds a positive and significant relationship between policy and marijuana use among individuals aged 18 or older. Conversely, the effect seems to be negligible among the minors (aged 12 to 17). Using marijuana arrest and marijuana treatment admission like proxies for use of cannabis, Chu (2012) evidences a strong positive effect of MMLs on both outcomes, suggesting a positive association between MMLs and illegal use. Other examples are Chu (2012) and Pacula et al. (2015).

[6]The key dimensions considered in Pacula et al. (2015) are similar to ours: actions which merely decriminalize cannabis, the establishment of a mandatory patient registry, the permit to cultivate cannabis at home, and the licence for selling cannabis in authorized dispensaries. They find that the simple medical allowances and patient registration requirement have a negative impact on recreational marijuana use, whereas legally protected dispensaries positively influence recreational use.

Although the aforesaid authors analyse the relationship between MMLs and crime for the different key dimensions of policy (as in Pacula et al. 2015), they neglect both the cross-effect on crime ascribable to the combined approval of two or more dimensions and the timing in which the specific dimension is put in place by the legislator. This, jointly with the use a county level data, represents the main distinctive element of our work compared to previous literature. The essay recognizes that not all medical marijuana policies are homogeneous but policy dimensions are important and not static. We exploit the variation in the timing of the core elements of MML policy (shown in Table 1.1) to assess whether particular forms of regulation are more relevant to estimate the impact of MMLs on crime in each county.

Other studies look at the link between the marijuana market and the trade of other illicit drug in order to assess the so-called *gateway drug hypothesis*, according which marijuana is complementary to demand of others drugs. Empirical evidences don't clarify if this phenomenon is present in the context of Medical Marijuana legislations. Chu (2013) finds no significant effect on the arrests for possession of other drugs. However, using *Treatment Episode Data Set* (TEDS), he finds that MMLs may decrease heroine treatment admissions. This would contradict the complementary between the marijuana and other drugs. On the contrary, Choi (2014), examining the impact of MMLs on various risky health behaviours, finds positive association between selling marijuana in dispensaries and use of other narcotic substances (no-marijuana) suggesting the presence of *gateway hypothesis*. She also finds negative link between allowing home cultivation and driving under the influence of drugs.

1.3 Evolution of MML in the U.S.

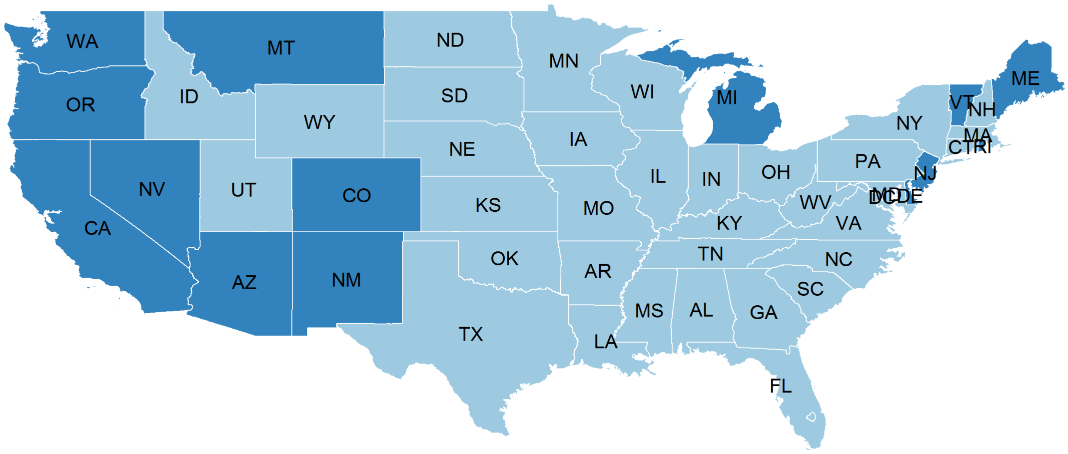
Since 1996 numerous U.S. States issued policies recognizing the medicinal value of marijuana and providing a legal defence for patient who used marijuana under recommendation of a physician. The set of juridical rules on therapeutic consumption, production, and distribution of cannabis are noted as Medical Marijuana Laws (MMLs). The purposes of MML was pain reduction for patients where the States allow doctors to prescribe marijuana as a pain killer[7]. However, the list of illness is often broad and it is so difficult to verify whether pain complaints are real. This generated wide possibilities for recreational use of marijuana masked like therapeutic consumptions (Chu, 2012).

In December 2012, 16 U.S. States and District of Columbia decided to ratify Medical Mari-

[7]Each state instituted a list of illness allowed to use marijuana for authorized patient. Among these, there are anorexia, arthritis, cachexia, neurodegenerative diseases, cancer, HIV/AIDS, chronic pain, glaucoma, migraines, persistent muscle spasms, severe nausea, seizures, and sclerosis. However, in many states is consented to use cannabis also for conditions related with stress or depression state. Patients can legally possess marijuana up to a fixed amount, which differs by U.S. states (Chu, 2012; Pros & Cons, 2015). In most cases, the law allows a written or oral recommendation by a physician to use as defence in the case the patient should be arrested on charge of cannabis possession.

juana Law involving more than 90 millions of Americans (around 30% of U.S. population). Between January 2013 and December 2014, others 7 states moved towards the same direction[8]. U.S. map permits to realize how MMLs are developing among American States (Figure 1.1). Details of legislation ratified in each U.S. State are reported in Table 1.1[9].

Figure 1.1: Medical Marijuana Law in U.S. States (1994-2012).



Note: The map omits the States of Alaska and Hawaii which passed the Medical Marijuana Law between 1994-2012. In dark blue are drawn U.S. state adopting MMLs, while in light blue ones never adopting reforms.

Voter referendum have represented the main tool to approve MMLs for many States between 1996 and 2000. However, referendum provides little specific guidance about decriminalization and acceptable source of supply for marijuana (i.e. possibility of cultivation, selling authorization, and so on). This has often flowed into legislative, administrative and judicial actions (in many cases competing one another) aimed to better specify the original reform (Pacula et al., 2015). In detail, they have established *i)* a patient registry systems for individuals who assume marijuana, *ii)* the possibility to cultivate cannabis at home, and *iii)* to sell marijuana in the authorized dispensaries. Since each State can choose the specific actions to adopt, we now observe a heterogeneous framework among the American States in term of measures in favour of marijuana (see Table 1.1).

Most of previous literature ignores this heterogeneity by evaluating the policy through a simple dichotomous distinction between States adopted MML and those that never passed the marijuana decriminalization. This could explain in part the inconsistency of past literature about the causal effect between Medical Marijuana legislations and crime. Indeed, the simple dichotomous indicator may mask important heterogeneous effects linked to the

[8]Connecticut (2013), Illinois (2014), Maryland (2014), Massachusetts (2013), Minnesota (2014), New Hampshire (2014) e New York (2014).

[9]Table 1.14 in Appendix contains the list of States interested by the Medical Marijuana Law with the legislative procedures implemented, and application methods used by policy maker.

specific actions put in place by the legislator in each State (Pacula et al., 2015).
The approach chosen in this essay neglects the dichotomous identification of MMLs, looking closely Pacula et al. (2015) for the decomposition of Mediacal Marijuana Laws. However, there are several elements of distinction in our approach. First, we are interested to the impact on crime of MMLs, while Pacula et al. (2015) inspect the relationship with marijuana consumption. Second, we remodulate the decomposition adopted by Pacula et al. (2015) classifying regulatory actions of *policy makers* into homogeneous choices of market design. Since it is reasonable to think that different regulatory actions produce different effect on markets, we need to assess them distinctly.

A. Policy Decomposition and Transmission Channel.

Pacula et al. (2015) distinguishes three specific dimensions of State MMLs that could influence the general availability and social norms surrounding marijuana use: *i)* require patient registry systems[10]; *ii)* allow home cultivation[11], and *iii)* legally permit dispensaries[12].
In this essay, we consider a further dimension of MMLs than Pacula et al. (2015): dates in which each state permits consumption of marijuana for medical practices.
Note that, it can differ from patient registry system dimension because many states implement a mandatory register after the first decriminalization. Some States either do not have a patient registry system (e.g. Washington) or have a voluntary system of registration (e.g. California).
Cutting the Medical Marijuana Law in single regulatory actions can be useful to understand how choices to design the legal marijuana market may affect social variables like crime, health, and so on.
Medical allowed (or mere decriminalization) and mandatory patient registry system may be classified like dimensions addressed at control the consumption of marijuana but without to provide ways in which new consumers can obtain the substance: *on legal or on illegal market?* Hence, we see them like a *demand side policy* regulating the marijuana market. On the contrary, dimensions referring to possibility of cultivation and selling marijuana in dispensaries may be classified like actions addressed to provide a legal sources of supply. So, they could be seen like a *supply side policy*.

[10]Patients or caregivers, affected by illnesses for which granted the use of cannabis, are obliged to register in a patient registry established by the authorities, which issue a special certificate (renewable annually). Possession of certificate is a requirement to not prosecution of crimes, like possession and consumption of marijuana.

[11]Not only patients can cultivate marijuana for personal use but laws allow caregivers (most of whom are patient as well) to grow and provide marijuana to patients on a non profit basis.

[12]Dispensaries are stores specializing in the sale of marijuana and its derivatives for authorized individuals (patients). They are typically organized as co-operative association (collectives). Members of the collective can either be producers, consumers or both. In same state producers can be a member of multiple dispensaries allowing them to scale up their production substantially, but in other states this is not allowed (Gavrilova et al., 2015).
Table 1.15 in Appendix shows when and in which U.S. States each dimension of Medical Marijuana Laws was been implemented.

Through the *demand side policy* (i.e. the mere decriminalization or mandatory patient registry) the *policy maker* increases the demand of marijuana by encouraging the entrance on marijuana market of consumers so far discouraged to use cannabis just because it was a crime[13]. Moreover, these policies may reduce both health and legal individual costs associated to consumption of marijuana, further pushing up the demand of cannabis[14].
Through the *supply side policies* the *policy maker* instead provides tools to produce and allocate cannabis on market through legal channels (thanks to possibility of home cultivation and selling in dispensaries). Also these policies may increase the consumption of marijuana since they may reduce individual research costs, rise social approbation of cannabis, and decrease the marijuana price[15].

Our codification of reforms permits to explain also the transmission channel between the Medical Marijuana Law and crime. Following Goldstein (1985), there are three main mechanisms through which narcotic drugs can affect criminal activities. *Psycho-pharmacological channel*, according to which drugs may increase aggression of users and thus induce violence[16]. *Economic channel*, according to which users may resort to crime in order to finance their drug habit[17]. *Systemic channel*, according to which disputes between drug market participants are solved with violence because drug agreements cannot be enforced in the courts[18].
Naturally, Medical Marijuana Laws may affect crime through each aforesaid transmission channel. Taking into account the specific regulatory actions approved in in each U.S. State, we can give empirical evidence also on the transmission channel.

[13]Standard economics theory of narcotic substance use postulate that consumers get utility from consuming intoxicating substance, just like other goods. Their consumption is constrained by the income available to the individual, and price of the substance (as any others goods) but also by legal and health risks associated with using the illegal intoxicating substance (Grossman, 2005; Pacula et al., 2010). The illegal nature of the drug generate also a search costs associated with trying to find and access the substance (Galenianos et al., 2012). These peculiarity costs should reduce the marginal utility of consuming marijuana and hence lower overall use of cannabis for a given price. Therefore, the decriminalization of cannabis associated with MMLs may stimulate the demand of marijuana.

[14]Legal costs because *demand side policy* permits to legally use marijuana. Health costs because this policy could bring individuals to associate lower harm to consumption of marijuana, since it is a medicine.

[15]Numerous studies reveal positive relationship between medical legalization and marijuana consumption in United States (Anderson et al., 2015; Cerdá et al., 2012; Choi, 2014; Pacula et al., 2010). Walsh et al. (2013) show the same pattern in Canada. See also Pacula and Sevigny (2014) and Pacula et al. (2015).

[16]The most relevant substances in this regard are probably alcohol, stimulants, barbiturates, and phencyclidine (PCP). Reports which sought to employ a *psycho-pharmacological channel* to attribute violent behaviour to the use opiates and marijuana have been largely discredited. Indeed, several opioid substances may have a reverse effect on users and ameliorate violent tendencies, controlling their violent impulses (Goldstein, 1985).

[17]Robberies represent typical way to support costly drug use. Heroine and cocaine are the most relevant substances in this category because they are expensive drug typified by compulsive patterns of use (Goldstein, 1985).

[18]This channel refers to traditionally aggressive patterns of interaction within the system of drug distribution and use. Examples can be: disputes over territory between rival drug dealers; assaults and homicides committed within dealing hierarchies as a means of enforcing normative code; robbery of drug dealers and the usually violent retaliation by the dealer; punishment for failing to pay one's debts, and so on. Since criminal entrepreneurs operate outside the law regardless by kind of drug traded, this channel may apply at all narcotic substances, such as marijuana, cocaine, heroine, etc. (Goldstein, 1985).

B. TIMING OF REFORMS AND POLICY INDICATORS.

U.S. States regulated the legal market of marijuana in heterogeneous way between 1994 and 2012 (see Table 1.1). Since the effective date of the legislations often occurs later than the enactment date, we follow Pacula et al. (2015) which employ the effective dates of State laws to operationalize the policy indicators used in empirical analyses.
Table 1.1 allows to rebuild the timing approval of Medical Marijuana State Laws for each aforesaid key dimension of policy.
First dimension is the so-called *Medical Allowed* (Column 3). It identify the States which passed the decriminalization of cannabis or/and established a mandatory patient registry systems between 1994 and 2012. Although they ideally represent distinct regulatory acts, decriminalization and patient registry systems actually are jointly implemented (see Table 1.15 in Appendix). Therefore, we choose to employ an unique policy indicator for both of them. Columns 4 refers to the dimension *Home Cultivation*. It identifies the States which provide legal protection for patients or caregivers to cultivate cannabis at home for medical purpose[19]. Column 5, called *Legal Dispensaries Operating*, identifies States that can be legally interpreted as providing protection for dispensaries to operate within the State. We follow Pacula et al. (2015) for identifying the actual operating dates. Table 1.1 shows both approval and operating dates of laws for dispensaries[20].

Looking at approval timing of MML dimensions in Table 1.1, we find that *demand side policies* (i.e. decriminalization or patient registry system) are systematically carried out before than *supply side policies* (i.e. home cultivation or dispensaries). This suggests that the dichotomous indicator of MMLs (which reflects the dates of first State regulatory action in favour of decriminalization of cannabis for therapeutic uses) coincides, *de facto*, with our *demand side policy* indicator. Hence, it ignores the potential heterogeneous impact of policy on crime induced by subsequent and different regulatory State acts aimed to provide legal sources of supply marijuana (i.e. *supply side policies*).
Table 1.1 represents the *guideline* to build policy indicators (treatment variables) applied in our econometrics analysis. These variables have the purpose to distinguish States adopting single dimensions of Medical Marijuana Laws from states never adopting any form of cannabis decriminalization for therapeutic uses.
Although the Medical Marijuana Law passed at State level, the UCR (2013) dataset allows to explore the impact of MMLs on crime at the county level. Consequently, also our policy

[19]Note that the laws vary across the States also referring to the number of cannabis plants that can be grown at home by patients or their designated caregiver. Table 1.14 in Appendix provides details.

[20]The clarification between approval and operating date of the dispensaries is not trivial. There are States, such as Washington and Michigan, where there are operating dispensaries in certain municipalities even though the State does not legal permit them. On the contrary, there are States which issued legal licence for dispensaries but actually they don't have operating dispensaries or they operate for a couple of years thereafter (Pacula et al., 2015). Therefore, we identify the State like allowed dispensaries if it there are operating dispensaries (whether legal or illegal way) within the State. It's reasonable to think that the dispensaries exert no effect on crime if they are not actual present within the States.

Table 1.1: Timing of Medical Marijuana State Laws (1994-2012)

U.S. State	First Reform	Medical Allowed	Home Cultivation	Legal Dispensaries operating	
Alaska	1999	1999	1999	-	-
Arizona	2010	2010	2010	2012	(2010)
California	1996	1996	2010	2003	(2003)
Colorado	2001	2001	2001	2005	(2000)
Delaware	2011	2011	-	-	(2011)
District of Columbia	2010	2010	-	-	(2010)
Hawaii	2000	2000	2000	-	-
Maine	1999	1999	1999	2011	(2009)
Michigan	2008	2008	2008	2009	-
Montana	2004	2004	2004	2009	(2009)
Nevada	2001	2001	2001	2009	-
New Jersey	2010	2010	-	2012	(2009)
New Mexico	2007	2007	2007	2009	(2007)
Oregon	1998	1998	1998	2009	-
Rhode Island	2006	2006	2006	-	(2009)
Vermount	2004	2004	2004	-	(2011)
Washington	1998	1998	1998	2011	-

Source: Pros & Cons (2015); Pacula et al. (2015); Alford (2014).

Note: The table presents MMLs and their key dimensions up to year 2012. Second column show the date when the law became active. Third column shows date of decriminalization for medical use. Fourth column shows whether there is a statewide allowance for home cultivation, with date if present, otherwise (-). Fifth column give the same information about the dispensaries. In parenthesis, there are date in which legally dispensaries are allowed. There are cases where the dispensaries are operating although there are not any act concern them, symbol (-).

indicators will refer to the U.S. Counties.

Assuming that the specific dimension j of policy carries out at time $t = k$ and each county is observed before and after the change. Let s_i be the variable identifying if county i belongs to group of counties subject to dimension j of MML. So s_i assumes value 1 if county is subjected to the dimensions j of MMLs between 1994 and 2012, otherwise it assumes zero value. Formally, the policy indicator (or *treatment variable*) is defined as:

$$d^j_{i,t} = \begin{cases} 1, & if \quad s_i = 1 \quad and \quad t > k \\ 0, & otherwise \end{cases}$$

where i identifies the county, t indexes the time, and j identifies the specific dimension of MMLs.

We consider three policy indicators corresponding to columns 3, 4, and 5 of Table 1.1. Variable d^{MML}, assuming value 1 if county i at time t has any form of *demand side policies*. Variable d^{HOME}, assuming value 1 if county i at time t allows home cultivation by approved patient. Variable d^{DISP}, assuming value 1 if county i at time t has operating dispensaries within belonging state.

Table 1.2 reports descriptive statistics of aforesaid treatment variables. Coverage of sampled counties is almost complete: on 3,144 U.S. county we have data for 3,034 (96.5%). Around of 16.3% of counties are subject to whichever form of decriminalization of cannabis for therapeutic use (as results from the mean in the Table 1.2).
All the counties adopt MMLs through a *demand side policy*. In fact, the dichotomous indicator of policy (or the first reform) is equal to indicator of *demand side*. Well we can refers to them indistinctly. Roughly the 15% of counties provides a legal supply source of marijuana to authorized patients (i.e. *supply side policies*), through the home cultivation (15.5%) or the sale in dispensaries (14.6%).

Table 1.2: Treatment Variables (descriptive analysis)

VARIABLES	N	Mean	Var.
*Dichotomous Indicator of MMLs**	3,034	0.163	0.137
Demand Side Policies	3,034	0.163	0.137
Home Cultivation (Supply Side)	3,034	0.155	0.131
Dispensaries Licence (Supply Side)	3,034	0.146	0.125

Note: *The dichotomous indicator of MMLs is built referring to the dates of the first regulatory act in favour of marijuana for therapeutic purpose. Statistics are reported for each dimension of Medical Marijuana legislation used in econometrics analysis. *Demand side policies* indicator refers to the counties which adopted or a mere decriminalization of cannabis or established a mandatory patient registry system. *Supply side policy* indicator is split in home cultivation and dispensaries dimensions. Since each county policy indicator is a dummy variable, their mean represents the percentage of counties which adopt that specific regulatory action.

1.4 Data

The analysis uses the *Uniform Crime Reporting Program Data* (UCR, 2013). The dataset contains the number of arrest by type of offence (like murder, rape, robbery, aggravated assault, burglary, larceny, auto theft, and arson) at the county level. The UCR (2013) also reports arrest for additional crimes such as forgery, fraud, vice offences, and drug possession or sale.
The data were originally collected by Federal Bureau Investigation (FBI) from reports submitted by agencies and states participating in the UCR Program. We exploit data from UCR Program provided by *Inter-university Consortium for Political and Social Research* (ICPSR, 1994-2012) for the years 1994 through 2012.
The use of arrest data as proxies of crimes is the norm in criminology researches.
Since a person may be arrested several times, each arrest count does not necessarily represent a single individual.
The UCR arrest data has a hierarchy rule, which only records arrests according to the most

serious offences[21]. Hence, minor crimes are reported only when they happen alone.
We use arrests since 1994 because there was a change in the data collection methodology after the 1993 (see codebook of data) which caused a break in series. Therefore, data from earlier years should not be compared to data from 1994 and subsequent years. This doesn't trouble present work because the first Medical Marijuana legislation happened in California in 1996.
Since participation in the UCR Program is generally voluntary, many agencies might not report data in every months for every years for one or more categories of crime. In order to distinguish true zero from missing values, FBI provides a coverage indicator in conjunction with the arrest variables. We follow the procedure illustrated in codebooks published with the data to correct for this issue.
As common in the criminology literature (see Carpenter 2007), we use observations only if the agencies report arrests for at least six months in that year.
After adjustments, we have $473,344$ observations on arrests classified by type of crime. Data refers to $3,034$ U.S. counties, that's the 96.5% of total counties, among the periods 1994-2012.

According to FBI classification, we can pool felonies in *violent*, *property*, and *narcotic* crimes (Handbook-UCR, 2004). Present analysis is based on eight crime variables, three belonging to violent crimes (murder, rape, and aggravated assault), three belonging to property crimes (robbery, burglary, and larceny), and two belonging to narcotic crimes (cocaine and synthetic narcotics possession[22]). Details on each crime variable are reported in Appendix (see *Crime Variables*). Variables are chosen in order to estimate how Medical Marijuana legislation may affect crimes at county level trying also to understand whether there was a heterogeneous impact among different kind of crimes (see Goldstein, 1985).

Finally, we consider several socio-economic variables (per-capita GDP, labour force and unemployment) for the purpose to use them like controls in our empiric strategy. They come from the U.S. Bureau of Economic Analysis (2014) and U.S. Bureau of Labor Statistics (2014).

A. Descriptive Statistics.

In this subsection we briefly exhibit the trend of crime in United States trying to underline whether there was a different pattern between the counties subject to the Medical Marijuana legislation and counties that never approved policy. This is crucial in a *difference-in-difference* approach where the *common trend* assumption among treated and control groups is a key identification condition.
Crimes in United States had a significant fall among 1994-2012 as observable for murder, rape, aggravated assault, robbery, larceny, burglary, and cocaine possession. In sharply contrast, arrests for synthetic narcotics possession strongly increase. This is true both for total crimes

[21] For instance, in the case of arrest for murder and drug trafficking UCR reports only the murder, while the felony concerning narcotic substances are not reported.

[22] In detail, *cocaine variables* includes the possession of opium, cocaine, and all their derivatives like morphine, heroine, and codeine. While, *synthetic narcotics* consists of the possession of demerol, and methadone.

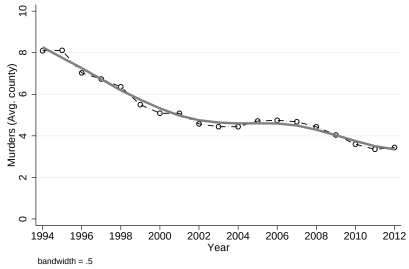
in United States and for average crime at county level (Figure 1.2).

Table 1.3 shows descriptive statistics for all crime variables at U.S. county level. We report statistics with and without zero values because, following the past literature, our *baseline* formulation of empirical model (Equation 1.4) considers crime (dependent) variable as logarithm. Thus, it refers only to non-zero observations.
Although this is not a problem for state level data, since there are not zeros, it cannot be neglected with county level data. Indeed, murder, rape, robberies, cocaine, and synthetic drug possession have significant percentages of zero observations. In Section 1.7, we test the robustness of our results also to introduction of zeros.
Almost all the mean values of crime variables are sufficiently high for ordinary least square (OLS). We stress this because with counts data (like in criminology) may happen to have a small average count (e.g. less than 5), troubling for OLS regression analysis that assume normal distribution in error around the expected average (Agresti, 2007; Piquero and Weisburd, 2010). Since most of crime variables present high average count (e.g. greater than 20), OLS represents the best estimation too (see Table 1.3).
However, felonies like murder, rape, and synthetic drugs possession have average count relatively low, making the OLS less efficient. In Section 1.7, we test the robustness of our results also estimating a non linear model for count data.

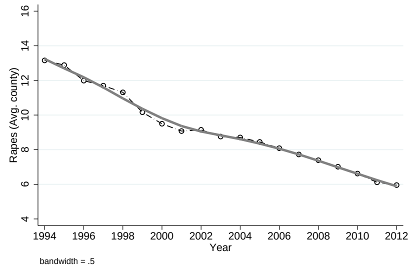
We want to verify whether the *common trend* assumption holds. For this purpose we compare, in Figure 1.3, the evolution in crimes for treated and control counties. The vertical lines in Figure 1.3 represent dates in which U.S. states endorsed the first reform towards a marijuana liberalization for therapeutic use.
Since Medical Marijuana State Laws are not passed in the same year (see vertical lines) we have a *multi-treatment* during the period 1994-2012. This doesn't consent to clearly appreciate, through the graph, the mean change in crime recorded in the treated counties relative to control counties over the period before and after the policy.
Crime trend among two groups doesn't seem to radically differ specially in a common pre-treatment period (1994-1996). Nevertheless, counties which ratified the Medical Marijuana Law are characterised by higher criminality rates than the ones which never passed decriminalization of marijuana. This evidence is true for all crimes analysed and it interests also the pre-treatment period 1994-1996 common to all counties, as showed in Table 1.4.
This might suggest that reforms may have been the consequence of periods with high criminality rate making policy endogenous. The test on mean change in crime among common pre-treatment period (i.e. 1994-1996) between treated and control counties reveals that there are not difference statistically significant at 99% of confidence level (last column, Table 1.4).

Figure 1.2: *Crime trend (average county) between 1994-2012*

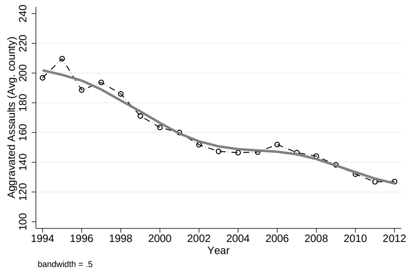
(a) Murders.



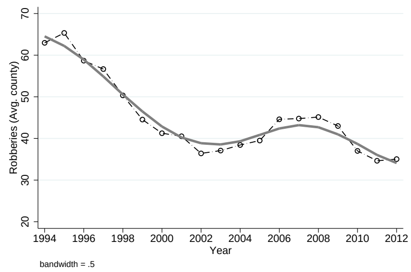
(b) Rapes.



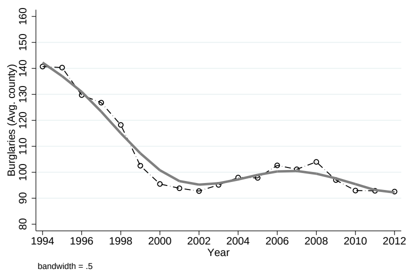
(c) Aggravated Assaults.



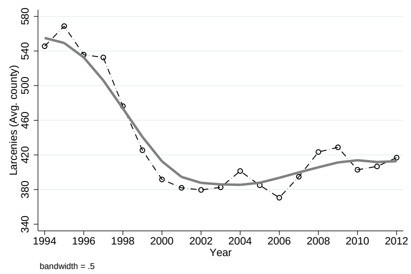
(d) Robberies.



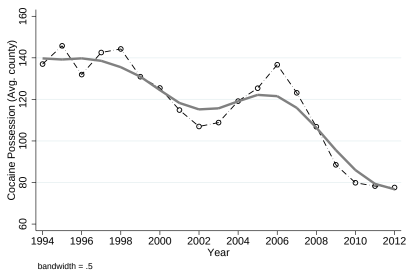
(e) Burglaries.



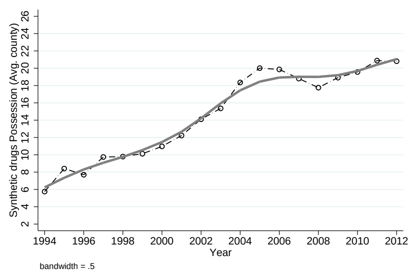
(f) Larcenies.



(g) Cocaine Possession.



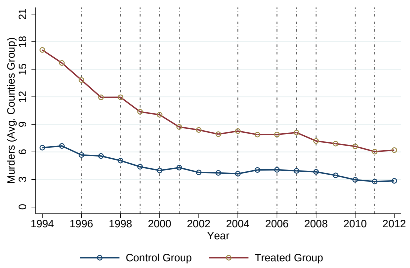
(h) Synthetic Possession.



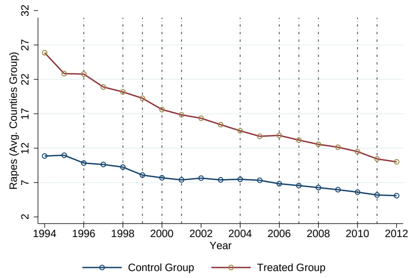
Note: Graphs report the average of felonies happened in each U.S. county.

Figure 1.3: Crime trend treated and control group (average county) between 1994-2012

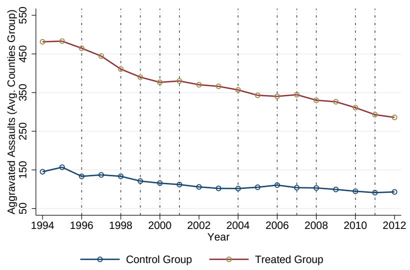
(a) Murders.



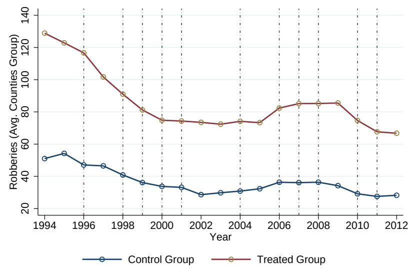
(b) Rapes.



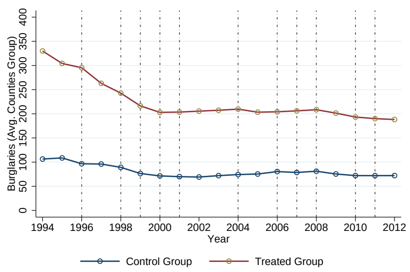
(c) Aggravated Assaults.



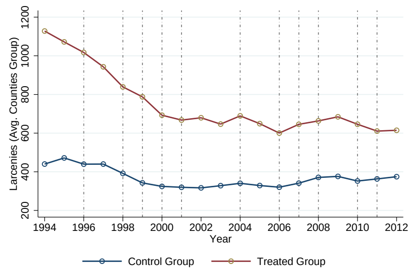
(d) Robberies.



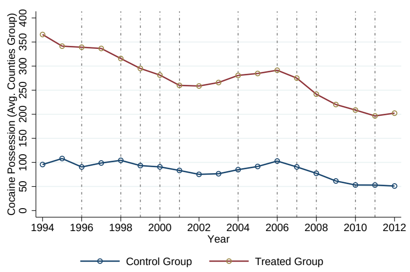
(e) Burglaries.



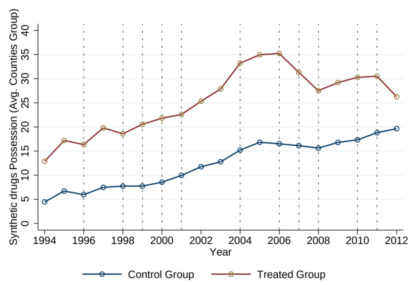
(f) Larcenies.



(g) Cocaine Possession.



(h) Synthetic Possession.



Note: Graphs report the average of felonies happened in the treated and control group of U.S. county. Vertical lines represents the dates of Medical Marijuana Laws approval for each states. For instance, the first vertical line at 1996 represents the MML passed in California, and so on.

Table 1.3: Crime Variables by U.S. County (descriptive analysis)

VARIABLES	N	Mean	Var.	Skew.	Max	Min	% Zero
Murders	3,034	4.918	663.4	17.28	764.4	0	52.10
Rapes	3,034	8.669	893.2	10.83	702.4	0	32.40
Aggravated Assaults	3,034	151.6	555,358	23.94	29,998	0	6.10
Robberies	3,034	42.28	62,918	18.63	8,464	0	35.10
Burglaries	3,034	100.9	155,160	19.64	14,488	0	6.80
Larcenies	3,034	412.4	1,670,000	9.217	26,246	0	4.60
Cocaine Possession	3,034	108.9	414,818	18.99	22,633	0	24.60
Synthetic Possession	3,034	14.02	2,842	14.84	1,277	0	38.20
				Without Zero Observations			
Murders	2,657	5.616	753.6	16.22	764.4	0.052	-
Rapes	2,826	9.307	953.1	10.49	702.4	0.052	-
Aggravated Assaults	2,985	154.1	564,094	23.76	29,998	0.052	-
Robberies	2,697	47.56	70,532	17.60	8,464	0.054	-
Burglaries	2,976	102.9	157,983	19.48	14,488	0.062	-
Larcenies	2,992	418.2	1,691,000	9.160	26,246	0.055	-
Cocaine Possession	2,839	116.4	442,450	18.39	22,633	0.052	-
Synthetic Possession	2,700	15.75	3,166	14.09	1,277	0.053	-

Note: The table shows descriptive statistics for crime variables at U.S. county level. The top of the table reports statistics including zero values reported for each county. The bottom of table shows the same statistics, keeping out the zero values. The last column reports the percentage of zeros presents in the variable at county-year level. *Cocaine* includes the possession of opium, cocaine, morphine, heroin, and codeine. *Synthetic* narcotics consists of the possession of demerol, and methadone.

1.5 Empirical Strategy

To inspect the effect on crime produced by the Medical Marijuana Laws, we use the *difference-in-difference* approach, since the policy doesn't simultaneously affect all the U.S. States at same time.
When in a dataset everybody are observed in all periods, the *difference-in-difference* design is usually based on comparing *de facto* two groups — *treated* and *control* group — with outcomes measured *before* and *after* the treatment. The bases of this empirical strategy is that if the treated and control groups are subject to the same time trend, and if the treatment has had no effect in the pre-treatment period, an estimate of the effect of the treatment in a period in which it is known to have none, can be used to remove the effect of confounding factors to which a comparison of post-treatment outcomes of treated and control groups may be subject to.
We use the mean change of the outcome variables for the treated group over time (after and before the treatment) and subtract them the mean change of the outcome variables over time for the control group to obtain the mean change of the outcome that the treated would have experienced if they had not been subjected to the treatment, that's the *average treatment effect on the treated* (Angrist and Pischke, 2009; Blundell and Dias, 2009; Imbens and Wooldridge, 2008).

Table 1.4: Crime mean by treated and control counties group (in pre-treatment 1994-1996)

VARIABLES	*Control Group* N.	Mean	*Treated Group* N.	Mean	*Control* Δ '94-'96	*Treated* Δ '94-'96	P-value Change
Murders	2,159	6.203	399	14.89	-0.461	-1.345	0.026
Rape	2,159	10.47	399	23.76	-0.579	-1.122	0.303
Aggravated Assault	2,159	146.0	399	459.9	-3.397	-1.858	0.761
Robbery	2,159	49.51	399	118.3	-2.503	-5.680	0.188
Burglary	2,159	102.8	399	300.2	-3.931	-14.10	0.047
Larceny	2,159	443.8	399	1,069	-6.596	-40.77	0.030
Cocaine Possession	2,159	95.83	399	332.1	2.234	1.651	0.904
Synthetic Possession	2,159	5.476	399	15.24	1.423	4.640	0.091

Note: Table shows the average crime, in pre-treatment period (1994-1996), at U.S. county level, classifying the counties in treated (counties with MMLs) and controls (counties without MMLs). group. It shows also the number of counties belong to each group. Columns "Δ '94-'96" report the change in crimes between 1996 and 1994, for each variable and distinguished in treated and control group. The last column reports p-value of test for the equality between mean change in crime between treated and control counties for 1994-1996 ($H_0 : \Delta^{96-94}_{treated} - \Delta^{96-94}_{control} = 0$). *Cocaine* variable includes the possession of opium, cocaine, morphine, heroin, and codeine. *Synthetic* narcotics consists of the possession of demerol, and methadone.

Now we present our empirical strategy to estimate the heterogeneous impact on crime due to Medical Marijuana Laws in the United States starting from a simple version of model (Equation 1.1, which assumes a homogeneous impact on crime of MMLs) until to present our baseline formulation (Equation 1.4) on which we base the results.
Equation (1.1) shows the OLS regression model for the *difference-in-difference* approach with county fixed effect α_i and time effect δ_t. Coefficient of interest is γ, representing the average effect on crime $y_{i,t}$ of treatment Medical Marijuana Laws in treated county i at time t (ATT). In this formulation we assume an homogeneous impact of MMLs on crime since it contemplates the simple dichotomous indicator of policy for distinguish the treated counties (those subjected to any regulatory act in favour of therapeutic use of cannabis) from the control counties (those unaffected from any cannabis decriminalization).
Imposing fixed effect allows us to explore the relationship between policy and crime within each U.S. county, ruling out the potential influences originated by all unobservable specific time-invariant characteristics (i.e political system, legal order, prison system, leniency towards crime, etc.). We also consider year dummies, δ_t, for the purpose to control unobservable factors that vary over time but are constant across counties (i.e. fluctuations in the business cycle, change in federal laws). Finally, we consider a set of time varying characteristics z_{it} refer to county i and year t (i.e. per-capita GDP, proxy of labour market). Error term is represented by $u_{i,t}$.
The dependent variable $y_{i,t}$ is the proxy of crimes measured as the number of arrests reported in county i and year t. According to past literature, we consider crime variable in term of natural logarithm.

We estimate the model (1.1) for all the crimes listed above (i.e. Murders, Rapes, Robberies, Aggravated Assaults, Burglaries, Larcenies, Cocaine possession, and Synthetic narcotic possession).

$$y_{it} = \alpha_i + \delta_t + \gamma \times d_{i,t}^{MML} + z_{it}'\theta + u_{i,t} \tag{1.1}$$

Policy indicator d^{MML} is highly serially correlated, since it is equal a string of zeroes followed by a string of ones for a counties that switches from never having the policy in place to forever after having the MMLs in place. The error $u_{i,t}$ is correlated over time for a given county if the model systematically overpredicts (or underpredicts) crime in a given county. Therefore, the default OLS standard errors are likely to be downwards-biased. In keeping with Bertrand et al. (2004) and Cameron and Miller (2013), we use *cluster-robust standard error* at U.S. State level to address this matter. In this way, standard errors are robust to serial correlation, within-state correlation, and heteroskedasticity[23].

One of a key assumption of *difference-in-difference* approach refers to *common trend*. It states that the differences in the expected potential non-treatment outcomes over time are uncorrelated to belonging to the treated or control group in the post-treatment period. It implies that both sub-populations (treated and control) would have expected the same time trend if the treated group had not been subjected to the treatment. In other words, differences between the control and treated (if untreated) are assumed time-invariant. Therefore, if the non-treatment potential outcomes share the same trend for treated and control groups, any deviation of the observed outcomes trend of the treated group from the observed outcomes trend of the control group should be directly attributed to effect of the treatment (MMLs) and non to differences in other characteristics of the treatment and control group.
Even if we cannot fully appreciate *common trend* assumption through graph tools, since MMLs didn't issue in each U.S. State at the same year, it would seem plausible for many felonies (see Figure 1.3). However, crime evolution among 1994-2012 for treated and control group may suggest that reforms would pass in counties with higher downward trend rather than countries never subject to MMLs. Hence, potential negative treatment effect, $\gamma < 0$, might not be ascribable to MMLs but (instead) to State specific unobservable time varying factors, also independent by decriminalization of cannabis. This could invalidate the *common trend* leading to a spurious underestimation of policy effect due to overestimation of the counterfactual trend for those treated.
To make *common trend* assumption more likely to hold, we re-formulate the model (1.1) adding a State specific linear trend. It's directed to capture within State time-varying heterogeneity (e.g. time-varying culture or political climate) and thus to accommodate some trend differentials across treated and control groups[24].

[23] In particular, Cameron and Miller (2013) recommend for clustering at the State level to avoid incorrect inference because, with clustering at state level rather than county level, we don't neglect the within-state cross county correlation of covariates and errors.

[24] Naturally, specific linear trend terms capture all unobserved heterogeneity that evolves linearly over time.

Equation (1.2) reports State specific time trend, $\sum_{s=1}^{S} \rho_s \times T_s \times t$, where S is the total number of U.S. States and T is a dummy which assumes value 1 for state s and zero for all the other States (Autor, 2003; Mora and Reggio, 2012). Once again, i indexes the counties, while t indexes the time.

$$y_{it} = \alpha_i + \delta_t + \sum_{s=1}^{S} \left(\rho_s \times T_s \times t \right) + \gamma \times d_{i,t}^{MML} + z_{it}'\theta + u_{i,t} \tag{1.2}$$

Another key condition required to correctly estimate the causal effect concerns the conditioning variables. It's known as exogeneity assumption. It needs that the covariates are not influenced by the treatment.
The exogeneity assumption refers also to the so-called *control variables*, namely those (time-varying) characteristics introduced in the model to better identify the coefficient of interest (i.e. ATT). Therefore, it need to control for precisely those exogenous variables that lead to differential trends and that are not influenced by the treatment[25].
Studies on the causal effect between MMLs and crime usually employ control variables as number of inmates, number of law enforcement, alcohol consumption, or consumption in other narcotic substances (different to cannabis). However, it's reasonable to think that each of these can be endogenous to treatment and enough constant over time. For instance, U.S. treated counties might anticipate the MMLs increasing the number of police officers because they suspect a rise of crime due to decriminalization of cannabis. This may introduce a potential bias in the estimation of treatment effect. For these reasons, we prefer the formulation of model with county fixed-effect, time-effects, and State specific time trend but without control variables because we believe that specific trend and fixed-effects are able to accounted for any smooth-trending variables.

The formulations so far presented contemplate the simple dichotomous indicator of MMLs which identifies the treated counties regardless of the regulatory actions put in place by the legislator in each State in favour of therapeutic use of marijuana. However, Medical Marijuana Law presents crucial elements of heterogeneity among U.S. States both referring to timing of approbation and to regulatory actions adopted (see Table 1.1).
To characterize the heterogeneous effect on crime produced by key dimensions of policy, we estimate a new version of model adding to equation (1.2) the indicators d^{HOME} (for home cultivation of cannabis) and d^{DISP} (for selling in dispensaries), plus their interactions with d^{MML}. The model is showed in equation (1.3).

Unfortunately, we cannot guarantee that all unobserved heterogeneity evolves linearly over time. Also for this, we assess our results with respect several robustness test in Section 1.7.

[25] Including control variables has also negative aspects. Every additional variables makes the common support assumption more difficult to fulfil. Concern the post-treatment period, time varying trend-confounding variables measured after the treatment are more likely to be influenced by it. Thus, the exogeneity condition might be violated. In this case, controlling for such time varying covariates leads to biased estimates. We called them *bad control* (Angrist and Pischke, 2009). However, if there are exogenous time varying variables and if anticipation effect play no rule, then using pre-treatment measurements whenever available may be the best empirical strategy.

$$
\begin{aligned}
y_{it} = \alpha_i + \delta_t + \sum_{s=1}^{S}\left(\rho_s \times T_s \times t\right) + \gamma \times d_{i,t}^{MML} + \beta \times d_{i,t}^{HOME} + \tau \times d_{i,t}^{DISP} + \\
+ \psi \times d_{i,t}^{MML} \times d_{i,t}^{HOME} + \pi \times d_{i,t}^{MML} \times d_{i,t}^{DISP} + \eta \times d_{i,t}^{HOME} \times d_{i,t}^{DISP} + \\
+ \xi \times d_{i,t}^{MML} \times d_{i,t}^{HOME} \times d_{i,t}^{DISP} + z_{it}'\theta + u_{i,t} \quad (1.3)
\end{aligned}
$$

Coefficient γ represents the impact on crime caused by decriminalization of cannabis (*demand side policy*) but now cleaned by the influences ascribable to *supply side policies*, namely the home cultivation (β) or selling marijuana in dispensaries (τ) or both of them (η). The formulation allows to identify also the effect on crime related to combination between *demand* and *supply policies* (ψ, π, and ξ).
Clearly, the sum of these coefficients gives the mean change in crime, after and before the approval of MMLs, happened in counties which adopt the policy (with simultaneous shocks on marijuana demand and supply) relative to the counties never affected by the cannabis decriminalization.
Formulation (1.3) is a fully specified econometric model, since it includes all dimensions of policy and all their possible interactions, in order to catch the heterogeneous effect (linear and no-linear) on crime due to Medical Marijuana Law.
However, it may be hardly interpreted if we look at how MMLs actually was implemented. Indeed, as we underline in Section 1.3, *demand side policies* (i.e. decriminalization of cannabis) systematically take place before of *supply side policies*: home cultivation and licence for selling cannabis don't happen alone but they are always matched with the decriminalization.
This pushes to employ a more parsimonious and realistic model (Equation 1.4) which omits the indicators $d_{i,t}^{HOME}$, $d_{i,t}^{DISP}$, and their interactions compared to Equation (1.3).
Equation (1.4) will be our baseline formulation on which we base our results. It is a new approach of matter compared to the previous literature.

$$
\begin{aligned}
y_{it} = \alpha_i + \delta_t + \sum_{s=1}^{S}\left(\rho_s \times T_s \times t\right) + \gamma \times d_{i,t}^{MML} + \psi \times d_{i,t}^{MML} \times d_{i,t}^{HOME} + \\
+ \pi \times d_{i,t}^{MML} \times d_{i,t}^{DISP} + \xi \times d_{i,t}^{MML} \times d_{i,t}^{HOME} \times d_{i,t}^{DISP} + z_{it}'\theta + u_{i,t} \quad (1.4)
\end{aligned}
$$

Model (1.4) better reflects how the Medical Marijuana Laws passed in U.S. States among 1994 and 2012. It allows to inspect the effect on crime ascribable to the: *i)* alone decriminalization, γ; *ii)* decriminalization joint to home cultivation, ψ; *iii)* decriminalization jointly to dispensaries licence, π; *iv)* decriminalization jointly to both cultivation and sell in dispensaries, ξ.

The *difference-in-difference* approach rule out the composition of the control group is affected by the treatment outcomes. Moreover, we assume that the Medical Marijuana State Laws are exogenous (Angrist and Pischke, 2009).

1.6 Results

Table 1.5 shows the ordinary least squares (OLS) estimates for the formulations (1.1) and (1.2) which assume a homogeneous impact of Medical Marijuana Laws on crime.
The estimations are run for the eight crime variables considered. The dependent variable (number of crimes in each county) is in term of natural logarithm. Panel A of Table 1.5 refers to Equation (1.1) with only county fixed effect and time dummies. Panel B refers to Equation (1.2) with county fixed effect, time dummies, and State specific linear trend but without control variables. Finally, Panel C refers to Equation (1.2) adding also control variables. Statistical tests confirm the relevance of State specific linear trend.
The results show a non significant effect of Medical Marijuana Laws on almost all crimes. Only murders and synthetic drug possession seem to have, respectively, a negative and positive relation with the decriminalization of cannabis for medical use.
The low statistical level suggests prudence. This seems to confirm the findings of Alford (2014); Gavrilova et al. (2015); Morris et al. (2014), namely the negative impact of MMLs on murders and the ambiguous effect of reforms on the other felonies.
The use of county level data jointly to the use of dichotomous indicator of policy (i.e. assumption of homogeneous impact of policy) seem to confirm the lack of impact of MML on crime[26].

As said in Section 1.3, Medical Marijuana Law is implemented in very different way within each American State. There are some States which have chosen only for decriminalizing cannabis (adopting *demand side policies*) while other States have chosen to both decriminalize and provide legal sources of supply marijuana through the home cultivation or selling in dispensaries (i.e. *demand* and *supply side policies*).
The heterogeneity of policy is neglected by previous formulation which contemplates only the dichotomous indicators. To explore the effects on crime due to this heterogeneity, we estimate the OLS model as in Equation (1.4). The results are showed in Table 1.6.
The first coefficient (*MMLs*) catches the change on crime recorded in counties that have chosen to decriminalize cannabis (*demand side policies*) relative to counties that never passed decriminalization over the period after and before the policy.
The second coefficient ($MMLs \times Home$) catches the further change on crime due to the cannabis' decriminalization jointly to the permit of home cultivation.
The third coefficient ($MMLs \times Disp$) refers to further change on crime due to the cannabis' decriminalization and selling the marijuana in dispensaries.
Finally, the forth coefficient ($MMLs \times Home \times Disp$) refers to further change on crime

[26] Aggregating the data at U.S. State level, dynamics here presented disappear. This is further demonstration that county's heterogeneity is relevant. With State aggregate data, results are in line with past studies.

Table 1.5: Impact of Marijuana Medical Law on Crime

	Violent Crime			Property Crime			Narcotic Possession	
A)	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synthetic*
MMLs	-0.063*	-0.005	0.044	0.021	-0.017	-0.136***	-0.009	-0.049
	(0.035)	(0.039)	(0.059)	(0.036)	(0.038)	(0.041)	(0.087)	(0.170)
Constant	1.408***	1.725***	3.516***	2.368***	3.554***	4.579***	2.546***	1.069***
	(0.032)	(0.028)	(0.085)	(0.030)	(0.030)	(0.026)	(0.055)	(0.080)
Obs.	22,682	32,019	44,474	30,716	44,133	45,171	35,706	29,259
R-squared	0.024	0.037	0.010	0.013	0.043	0.053	0.056	0.175
County	2,657	2,826	2,985	2,697	2,976	2,992	2,839	2,700
B)	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synthetic*
MMLs	-0.088*	0.019	-0.015	-0.032	0.039	-0.010	-0.096	0.230*
	(0.050)	(0.038)	(0.060)	(0.039)	(0.035)	(0.044)	(0.084)	(0.120)
Constant	36.001***	47.286***	17.892***	18.089***	40.112***	19.125***	-5.880	-149.508***
	(2.788)	(2.318)	(3.587)	(2.851)	(2.656)	(3.046)	(4.246)	(8.865)
Obs.	22,682	32,019	44,474	30,716	44,133	45,171	35,706	29,259
R-squared	0.032	0.056	0.076	0.028	0.069	0.095	0.100	0.244
County	2,657	2,826	2,985	2,697	2,976	2,992	2,839	2,700
C)	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synthetic*
MMLs	-0.030	-0.008	-0.008	-0.008	-0.003	-0.030	-0.014	0.148
	(0.051)	(0.041)	(0.078)	(0.032)	(0.046)	(0.062)	(0.062)	(0.104)
Constant	42.149***	50.185***	38.217***	29.578***	45.068***	30.895***	24.171*	-167.637***
	(7.992)	(7.226)	(8.638)	(9.542)	(7.222)	(7.106)	(12.582)	(19.940)
Obs.	18,836	26,914	37,904	25,970	37,479	38,404	30,613	26,062
R-squared	0.018	0.045	0.041	0.025	0.049	0.077	0.098	0.220
County	2,527	2,725	2,903	2,587	2,897	2,911	2,747	2,641

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. The dependent variables are the natural logarithm of crime. *Cocaine* includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). While, *Synthetic* narcotics consists of the possession of demerol, and methadone. All regressions include County fixed effect and time dummies. Panel B includes also State specific linear trend. Panel C includes also control variables, not shown (real per-capita GDP, unemployment rate, and labour force).

recorded in counties that ratified the decriminalization, the home cultivations, and the licence for selling marijuana.

Note that the effects of coefficients refer to the dimension of policy interacted with the *MMLs* indicator are to interpret like shifts on crime in counties that passed the specific actions for supply of marijuana (cultivation or selling) relative to the counties that passed the mere decriminalization and relative to the counties that never passed policy.

As before, Panel A refers to model (1.4) with county and time fixed-effects, Panel B adds the State specific trend, and Panel C also adds the control variables.

Results suggest that MMLs have indirect effect on crime trough the design choices regarding the American legal marijuana market.

The use of simple dichotomous indicator in policy research hides these effects, since it may mask important heterogeneous aspects of reform. Indeed, referring to the findings in Panel B of Table 1.6, we note a marked increasing in the significance levels of coefficients compared

than results in Table 1.5, specially for murders, burglaries, larcenies, cocaine, and synthetic drug possession.
In detail, for burglaries, larcenies, cocaine, and synthetic drug possession we observe that the cannabis decriminalization may induce an increase on crime in counties that passed the MMLs relative to the counties that never ratified decriminalization (the coefficient *MMLs* are always positive and significant at 1% of confidence). This suggest that, for these felonies and in average in each treated U.S. county, the decriminalization of cannabis (*demand side policies*) originates a rise in the crime: from 9.7% in larceny, 13% in burglaries, up to 15.6% in cocaine possession, and 46.3% in synthetic drug possession (interpreted as strong growth).
At the same time, the allowed to cultivate cannabis at home in combination with the decriminalization may decrease the crime.
When statistical significant the coefficient $MMLs \times Home$ is always negative. Dispensaries licence seem to have a heterogeneous impact on crime: positive for burglaries and cocaine possession, negative for larcenies (coefficient $MMLs \times Disp$).
Simultaneous implementation of cultivation and selling in dispensaries in combination with the cannabis decriminalization seem to be not statistical different from zero, maybe due to its poor variability ($MMLs \times Home \times Disp$). Interesting is the fact that the joint introduction of production and distributive tools of cannabis (sum of $MMLs \times Home$, $MMLs \times Disp$, and $MMLs \times Home \times Disp$) seems to have negative effect on crime able to countervail the rise on crime due to the mere application of *demand side policies* (i.e. the alone decriminalization). Indeed, the sum of all coefficients, excluding the constant, is always not statistical different from zero, except for burglary where prevails a positive effect (at 5% of confidential level).

The framework for these felonies (i.e. burglaries, larcenies, cocaine, and synthetic drug possession) suggests that, in principle, the *demand side policies* don't affect directly on crime because they only provide a legal protection for patients and caregivers. However, they encourage the consumption of cannabis because they reduce the individual costs linked to consumption of narcotic substance (such as social, health and search costs)[27].
The augmented demand of cannabis will be intercepted by illegal market since the *demand side policies* (i.e. the mere cannabis decriminalization to use and possess cannabis) don't provide legal sources of supply marijuana (Chu, 2012; Pacula et al., 2010)[28].
This supports and strengthens illegal activities, inflating their profits, with a positive impact on crime. Cannabis indeed represents the most widely available and commonly abused illicit drug in the United States and marijuana sales represents a relevant slice of revenues for drug trafficking organizations operating on narcotic substance markets (Drug Enforcement Administration, 2011, 2014; Gavrilova et al., 2015)[29].

[27] See Anderson et al. (2015), Cerdá et al. (2012), Choi (2014), Pacula et al. (2010), and Walsh et al. (2013).
[28] For instance, Chu (2012) finds that MMLs are associated with 10-20% increase in marijuana arrests, suggesting a positive effect on illegal use of substances. Moreover, fuzzy laws and litigious relationships between state and federal legislations referring to the cannabis decriminalization contributes to expand a huge gray area, facilitating the illegal trade.
[29] Drug Enforcement Administration (2014), in the NDIC Report, found that between 2010 and 2013 marijuana seizures by *U.S. Customs and Border Protection* remained stable at 1.3 to 1.4 million kilograms per year along the

Table 1.6: Heterogeneous impact of Marijuana Medical Law

	Violent Crime			Property Crime			Narcotic Possession	
A)	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
MMLs	0.048	-0.388***	-0.043	-0.018	0.111***	0.007	-0.018	0.551***
	(0.067)	(0.035)	(0.044)	(0.056)	(0.035)	(0.070)	(0.061)	(0.132)
MMLs x Home	-0.135*	0.415***	0.084	0.015	-0.155***	-0.103	-0.078	-0.525**
	(0.079)	(0.049)	(0.054)	(0.065)	(0.037)	(0.073)	(0.102)	(0.200)
MMLs x Disp	0.030	0.034	-0.128***	-0.060	0.064***	-0.091	0.131***	-0.104
	(0.061)	(0.029)	(0.019)	(0.049)	(0.013)	(0.056)	(0.042)	(0.097)
MMLs x Home x Disp	0.004	-0.060	0.146***	0.125**	-0.030	-0.012	0.055	-0.170
	(0.068)	(0.068)	(0.039)	(0.054)	(0.049)	(0.072)	(0.076)	(0.220)
Constant	1.408***	1.725***	3.516***	2.368***	3.554***	4.579***	2.546***	1.070***
	(0.032)	(0.028)	(0.085)	(0.030)	(0.030)	(0.026)	(0.055)	(0.079)
Obs.	22,682	32,019	44,474	30,716	44,133	45,171	35,706	29,259
R-squared	0.024	0.038	0.010	0.013	0.043	0.054	0.058	0.177
County	2,657	2,826	2,985	2,697	2,976	2,992	2,839	2,700
Sum (p-value)	0.167	0.976	0.409	0.136	0.867	0.002	0.297	0.387
B)	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
MMLs	-0.158***	0.044	0.043	-0.012	0.130***	0.097***	0.156***	0.463***
	(0.025)	(0.033)	(0.030)	(0.025)	(0.040)	(0.019)	(0.050)	(0.166)
MMLs x Home	0.079	-0.032	-0.063	-0.024	-0.107*	-0.117**	-0.289***	-0.255
	(0.060)	(0.055)	(0.068)	(0.050)	(0.054)	(0.051)	(0.084)	(0.221)
MMLs x Disp	0.049**	0.095***	-0.092***	-0.014	0.087***	-0.030**	0.177***	0.075
	(0.020)	(0.021)	(0.020)	(0.019)	(0.023)	(0.014)	(0.035)	(0.096)
MMLs x Home x Disp	0.027	-0.085	0.101***	0.067	-0.021	0.074*	0.005	-0.112
	(0.068)	(0.061)	(0.032)	(0.047)	(0.035)	(0.041)	(0.116)	(0.222)
Constant	37.470***	47.528***	17.918***	18.917***	41.087***	19.781***	-2.996	-149.969***
	(3.023)	(2.509)	(3.810)	(2.949)	(2.640)	(2.831)	(4.439)	(9.073)
Obs.	22,682	32,019	44,474	30,716	44,133	45,171	35,706	29,259
R-squared	0.032	0.056	0.076	0.028	0.069	0.095	0.101	0.244
County	2,657	2,826	2,985	2,697	2,976	2,992	2,839	2,700
Sum (p-value)	0.975	0.756	0.894	0.813	0.016	0.466	0.683	0.423
C)	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
MMLs	-0.143***	0.053	0.061**	-0.023	0.103***	0.081***	0.190***	0.427**
	(0.031)	(0.032)	(0.026)	(0.028)	(0.035)	(0.020)	(0.061)	(0.181)
MMLs x Home	0.131**	-0.071	-0.073	-0.016	-0.127**	-0.123*	-0.253***	-0.315
	(0.056)	(0.052)	(0.078)	(0.047)	(0.059)	(0.069)	(0.088)	(0.218)
MMLs x Disp	0.047*	0.088***	-0.097***	-0.021	0.065***	-0.037**	0.171***	0.114
	(0.026)	(0.017)	(0.020)	(0.020)	(0.019)	(0.015)	(0.040)	(0.095)
MMLs x Home x Disp	0.014	-0.079	0.070*	0.054	-0.017	0.064	-0.054	-0.214
	(0.061)	(0.060)	(0.040)	(0.043)	(0.031)	(0.050)	(0.103)	(0.208)
Constant	45.156***	50.526***	36.974***	30.611***	46.492***	31.720***	27.912**	-171.118***
	(8.092)	(7.252)	(9.548)	(9.760)	(7.371)	(6.913)	(12.718)	(19.806)
Obs.	18,836	26,914	37,904	25,970	37,479	38,404	30,613	26,062
R-squared	0.019	0.045	0.041	0.025	0.049	0.077	0.099	0.220
County	2,527	2,725	2,903	2,587	2,897	2,911	2,747	2,641
Sum (p-value)	0.573	0.909	0.713	0.737	0.669	0.841	0.515	0.950

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. Sum (p-value) tests if the sum of coefficients, excluding the constant, is equal to zero (under H_0). The dependent variables are the natural logarithm of crime. *Cocaine* variable includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone. All regressions include County fixed effect and time dummies. Panel B includes also State specific linear trend. Panel C includes also control variables, not shown (real per-capita GDP, unemployment rate, and labour force).

Therefore, marijuana revenues, addressed to finance other illegal activities, may bring a substantial increase in crime, justifying the positive impact on crime of *demand side policies* (sign of coefficient $MMLs$).
On the contrary, the *supply side policies* providing legal sources of supply marijuana (thanks to possibility of home cultivation and selling in dispensaries) may bring the entry on marijuana market of legal dealers, which may cut market shares to illegal sellers and decrease the price of marijuana (since they reduce the search costs).
Hence, for opposite reasons compared to *demand side policies*, the impact on crime of the *supply side policies* must be negative (coherently the sum of coefficients $MMLs \times Home$, $MMLs \times Disp$, and $MMLs \times Home \times Disp$ is negative). Indeed, legal production and distribution of cannabis may lead to a drop in violence in illicit markets and illegal trade may be pushed out by licit competition. Both the lower price and the entry of legal dealers of marijuana should weaken the black market, providing less cash resources for the crime organizations to allocate in other illegal activities.
In the line with this dynamic, we find that the *demand side policies* are associated with an increase in crime while the *supply side policies*, when ratified, are able to countervail the rise in crime ascribable to *demand side policies*, realizing a *crowding-out effect* on crime.
The findings are compatible with the *systemic transmission channel* as hypothesised by Goldstein (1985) according to which disputes between drug trafficking (such as disputes over territory between rival drug dealers, and so on) are solved with violence.
Conversely, findings seem to be inconceivable with *psycho-pharmacological* and *economic compulsive* transmission channel because according to these hypothesis both the demand and supply policies should be associated with an increase in crime. This is denied by our findings.
Possession of cocaine (and opioid substances) and synthetic drugs goes up only if the consumers get in touch with the illegal market. This likely happens when there is cannabis decriminalization without legal sources of supply marijuana.
Conversely, when the States pass both *demand* and *supply side policies* there is not affects on cocaine and synthetic drugs possession.
Hence, the *gateway hypothesis* for these drugs seems to be uniquely associated with *demand side policies*. Choi (2014), DeSimone (1998), and DeSimone (2002) confirm the *gateway hypothesis* towards the synthetic drugs in the Medical Marijuana State Law.
Finally, Murders seem different. Findings in Table 1.6 confirms the negative impact on murders induced by mere decriminalization of cannabis seen in Table 1.5 though even the test on sum of coefficients (Table 1.6) suggests that it is not statistically different from zero.
Fall of murders could be attributed to several factors. For instance, it might derive from a different distribution of law enforcement and their resources in favour of more ferocious crimes, like murders, thanks to the fact that MMLs slowed down the struggle against the use and possession of marijuana.
Following Gavrilova et al. (2015), the drop of homicides might originate by the exit of Mexican drug trafficking organizations from marijuana market induced after approval of MMLs.

South-west border.

In any case, given the low mean of occurrence of murders per unit time (see Table 1.3), we postpone the conclusion about this variable to the next section, where we estimate a *Poisson* model as robustness check (Table 1.12).

1.7 Robustness checks and placebo tests

In this section we present several robustness check of our results. We run also two different *placebo test* to support our findings.

A. LINEAR RELATIONSHIP ASSUMPTION.

In Section 1.5 we have chosen the Equation (1.4) like baseline formulation for our analysis because we believe that it represents a realist and parsimonious model able to capture the heterogeneous effect of Medical Marijuana State Laws on crime. However, Equation (1.4) omits some interaction terms between the key dimensions of MMLs assuming a linear relationship between them.
To prove that our findings are not counterfeited by this simplification, we estimate the fully specified model expressed by the Equation (1.3). The results are shown in Table 1.7 which refer to formulation with county fixed-effect, time dummies, and State specific linear trend without control variables.
As expected, it's impossible to estimate all the interaction terms due to collinearity. This is reasonable whether we keep in mind how the Medical Marijuana State Laws are implemented over the time (*demand side policies* comes first than *supply side policies*).
The findings in Table 1.7 are in line with our main results. The effect on crime induced by the mere cannabis decriminalization ($MMLs$) remains positive for burglaries, larcenies, cocaine, and synthetic drugs possession. Also the magnitude of coefficients is close to those shown in Table 1.6. Therefore, the *demand side policies* confirm of having positively impact on crime. The effect on crimes of home cultivation (to read as sum of $Home$ and $MMLs \times Home$) is still negative, while the effect of selling marijuana in dispensaries ($Disp$) remains differentiated. The findings confirmed also the *crowding-out effect* on crime induced by the *supply side policies* (see *p-value* in Table 1.7). For the murders, the coefficient of $MMLs$ is still negative as in Table 1.6. All this is robust also to introduction of control variables[30].

B. FULL COVERAGE, ZERO TESTS, AND PAST DECRIMINALIZATION.

This test refers to collection procedure of data underlying the analysis. Thus far we have considered the American counties which transmit data on crime to FBI at least for six month in one year. We repeat the estimates of Equation (1.4) narrowing the sample to the counties which reported data for at least 11 month in each year. The results are in Table 1.8.

[30]Estimates of model with control variables are available on request.

Table 1.7: Test on linear relationship assumption between MMLs dimensions

	Violent Crime			Property Crime			Narcotic Possession	
	Murder	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
MMLs	-0.158***	0.044	0.043	-0.012	0.130***	0.097***	0.156***	0.463***
	(0.025)	(0.033)	(0.030)	(0.025)	(0.040)	(0.019)	(0.050)	(0.166)
Home	-0.079**	0.099***	0.171***	0.178***	0.441***	0.149***	0.307***	-0.560***
	(0.038)	(0.027)	(0.047)	(0.028)	(0.026)	(0.034)	(0.046)	(0.082)
Dispensaries	0.049**	0.095***	-0.092***	-0.014	0.087***	-0.030**	0.177***	0.075
	(0.020)	(0.021)	(0.020)	(0.019)	(0.023)	(0.014)	(0.035)	(0.096)
MMLs x Home	0.157***	-0.130***	-0.232***	-0.201***	-0.544***	-0.265***	-0.594***	0.302
	(0.047)	(0.045)	(0.043)	(0.041)	(0.050)	(0.034)	(0.065)	(0.188)
MMLs x Disp	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-
Home x Disp	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-
MMLs x Home x Disp	0.027	-0.085	0.101***	0.067	-0.021	0.074*	0.005	-0.112
	(0.068)	(0.061)	(0.032)	(0.047)	(0.034)	(0.041)	(0.116)	(0.222)
Constant	37.457***	47.540***	17.947***	18.942***	41.164***	19.806***	-2.944	-150.032***
	(3.028)	(2.507)	(3.812)	(2.946)	(2.608)	(2.830)	(4.433)	(9.081)
Obs.	22,682	32,019	44,474	30,716	44,133	45,171	35,706	29,259
County	2,657	2,826	2,985	2,697	2,976	2,992	2,839	2,700
Sum (p-value)	0.970	0.750	0.908	0.796	0.012	0.446	0.669	0.434

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. Sum (p-value) tests if the sum of coefficients, excluding the constant, is equal to zero (under H_0). The dependent variables are the natural logarithm of crime. *Cocaine* includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone. All regressions include County fixed effect, time dummies, and State specific linear trend.

Although more than 5,000 observations and 100 counties are dropped (see at bottom of Table 1.8), all the estimations confirm our previous results.

According to previous literature, we consider the dependent variable (number of crime recorded each county) in term of natural logarithm. However, the log transformation of dependent variable causes the loss of all county-year records with zero crime. Although this would not represent a problem with the U.S. State level data, in the case of county level data the issue cannot be ignored due to the relevant presence of zeros (see Table 1.3). For this reason, we repeat the estimates of the Equation (1.4) considering the dependent variable as integer taking into account also the zeros. The results are showed in Table 1.9.
Although murders present the highest percentage of zero (52.10%) their addiction doesn't change the results: overall fall in homicides for county subject to MMLs during 1994-2012. Validation also comes from by the estimates of burglary, larceny, and synthetic drug possession. Rape estimations become coherent with our intuition, while cocaine narcotic possession doesn't hold to the robustness check since its coefficients turn the sign.

Finally, we test our results also taking into account the past decriminalization approved individually in United States. Starting with Oregon (1973), a couple of U.S. States began to partially liberalize cannabis through decriminalization laws. Hence, prior to Medical

Table 1.8: Full coverage test

	Violent Crime			Property Crime			Narcotic Possession	
	Murder	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
MMLs	-0.106***	0.004	0.035	-0.035	0.056*	0.059***	0.132**	0.370**
	(0.025)	(0.029)	(0.027)	(0.026)	(0.028)	(0.021)	(0.054)	(0.182)
MMLs x Home	0.057	0.001	-0.042	-0.015	-0.045	-0.070	-0.310***	-0.179
	(0.064)	(0.059)	(0.062)	(0.044)	(0.043)	(0.044)	(0.081)	(0.242)
MMLs x Disp	0.078***	0.084***	-0.085***	-0.012	0.088***	-0.012	0.189***	0.081
	(0.022)	(0.020)	(0.018)	(0.020)	(0.018)	(0.014)	(0.036)	(0.092)
MMLs x Home x Disp	0.009	-0.056	0.069**	0.074*	-0.028	0.042	-0.043	-0.149
	(0.062)	(0.066)	(0.034)	(0.042)	(0.028)	(0.038)	(0.109)	(0.233)
Obs.	19,474	27,524	37,635	25,732	37,325	38,205	30,095	24,685
County	2,526	2,719	2,901	2,561	2,891	2,908	2,725	2,575
Sum (p-value)	0.688	0.697	0.776	0.837	0.028	0.518	0.775	0.585
Obs. (6 months)	22,682	32,019	44,474	30,716	44,133	45,171	35,706	29,259
County (6 months)	2,657	2,826	2,985	2,697	2,976	2,992	2,839	2,700

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. Sum (p-value) tests if the sum of coefficients, excluding the constant, is equal to zero (under H_0). The dependent variables are equal to natural logarithm of crime. *Cocaine* variable includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone. All regressions include County fixed effect, time dummies, and State specific linear trend. The sample are restrict to the jurisdiction which reported data at least for 11 months on 12.

Marijuana legislations there were States in which the use of cannabis was strictly prohibited and other states where its use was tolerated[31]. In any case, before MMLs no State allowed for any form of production or distribution of the drug.
We repeat the estimation of Equation (1.4) by adding an interaction term able to isolate the effect of States subjected to the marijuana decriminalization prior of the MMLs. We estimate the model (1.4) also keeping out these States from the sample. Both formulations include county and year fixed effects, time dummies, and State time trend. The dependent variable is the logarithm of crime. The findings are robust with our previous results. In line with Pacula et al. (2010), the interaction term is not statistically significant[32].

C. Placebo Test.

We also run a *placebo experiment* to test the common trend assumption between trend of crime in pre-treatment period by treatment status of counties. In the experiment, we pretend that treatment happened earlier than the true date (so-called *fake policy*). Then we measure the outcome in correspondence of pretended treatment. A significant effect of artificial treatment might mean an anticipation of policy which would induce effects on crime before

[31]Most of decriminalization typically dates back to the 1980's. In detail, Oregon (1973), Alaska (1975), Maine (1976), California (1973), Colorado (1978), Mississippi, New York, Nebraska, North Carolina, and Ohio (1973-1978). If the drug was prohibited means that even possession and use of small quantities of marijuana lead to punishment in jail. If the drug was decriminalized means that the penalty for possession and use of small (defined) quantities was limited to a fine.
[32]Table of estimations available on request.

Table 1.9: *Zeros test*

	Violent Crime			Property Crime			Narcotic Possession	
Crime integers	*Murder*	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
Zeros (%)	52.10%	32.40%	6.10%	35.10%	6.80%	4.60%	24.60%	38.20%
MMLs	-0.539***	3.587***	7.034	0.412	53.980***	277.875***	-165.786***	24.361***
	(0.201)	(0.496)	(17.964)	(5.395)	(7.735)	(21.487)	(36.127)	(2.380)
MMLs x Home	-0.156	-2.859***	1.992	-12.222	-71.807***	-304.948***	149.752***	-18.712***
	(1.570)	(0.974)	(19.951)	(12.772)	(25.822)	(46.073)	(37.605)	(6.497)
MMLs x Disp	-0.275*	3.603***	-4.006	1.986	32.945***	16.129	69.005***	-0.425
	(0.138)	(0.259)	(10.330)	(3.161)	(4.689)	(12.428)	(20.624)	(1.372)
MMLs x Home x Disp	2.427*	-3.651***	12.955	5.007	-22.291*	10.854	-26.686	-7.463
	(1.339)	(1.033)	(11.416)	(7.569)	(12.694)	(25.341)	(35.274)	(7.476)
Obs.	47,344	47,344	47,344	47,344	47,344	47,344	47,344	47,344
County	3,034	3,034	3,034	3,034	3,034	3,034	3,034	3,034
Sum (p-value)	0.354	0.647	0.214	0.369	0.647	0.998	0.274	0.758

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. Sum (p-value) tests if the sum of coefficients, excluding the constant, is equal to zero (under H_0). The dependent variables are integer number of crime. *Cocaine* variable includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone. All regressions include County fixed effect, time dummies, and State specific linear trend.

the policy actually is approved. This may raise concerns about an anticipation effect of policy on crime. In case we can rule out anticipation, the placebo test becomes a specification test for the common trend assumption. Any estimated non-zero effect would be interpreted as *selection bias* raising serious doubts on the validity of the identifying assumptions (Angrist and Pischke, 2009).

We build experiment for crime variables which have significant coefficients in the *baseline* formulation (1.4): murders, burglaries, larcenies, cocaine, and synthetic drugs possession (see Table 1.6). We pretend three treatments moving up policy indicators of two, four and six years before the actual passage of Medical Marijuana State Laws. We pretend that the MMLs also are carried out between 1996 and 1998[33]. Results are reported in Table 1.10.

For each crime variable, the fake treatments at four and six years before, and in the two-years period (1996-1998) have either zero effect on crime or opposite sign than the results in Table 1.6. These suggest that the *common trend assumption* holds when it is evaluated at a reasonable distance from the actual Medical Marijuana Laws because there are not systematic effects on crime in pre-treatment period.

The significant impact on crimes of the pretended MMLs at two years backward could find explanation in the timing approval of Medical Marijuana State Laws. Indeed, MMLs happen in different States through different regulatory actions ratified in different times. Therefore, the pretended policies imposed two years before than the actual MML could be affected *de facto* by marijuana's decriminalization laws passed in neighbouring States.

[33]In order to preserve a pre-treatment period for each *fake scenarios*, we set at 1996 all pretended treatments which would occur in dates antecedent to 1996. Finally, we erase California from the sample because it exactly passed the decriminalization of cannabis in 1996.

Considering fake policies progressively more faraway from the actual date of MMLs these cross-border effects tend to disappear. For this, we believe that the pretended treatments imposed at four and six years before, and in the period 1996-1998 could be more representative for experiment.

To test the anticipation effect of policy, we analyse pre-treatment period estimating equation (1.5), where actual MMLs is considered together to the lead terms of policy indicators[34]. If the effect on crime is really driven by actual MMLs, the lead terms shouldn't impact on crimes, resulting non-significant and close to zero when considered together to the actual policy. Otherwise, if our results are driven by different crime trends between treated and control group, the coefficients of leads terms may be significant and have likely similar magnitude to the effects estimated on the actual treatment variable[35].

$$y_{it} = \alpha_i + \delta_t + \sum_{s=1}^{S} \Big(\rho_s \times T_s \times t\Big) + \sum_{t=-4}^{-1} \Big(\omega_t \times d_{i,t}^{MML}\Big) + \gamma \times d_{i,t}^{MML} + \psi \times d_{i,t}^{MML} \times d_{i,t}^{HOME} + \\ + \pi \times d_{i,t}^{MML} \times d_{i,t}^{DISP} + \xi \times d_{i,t}^{MML} \times d_{i,t}^{HOME} \times d_{i,t}^{DISP} + z_{it}'\theta + u_{i,t} \quad (1.5)$$

The results are showed in Figure 1.4. It confirms the absence of significant anticipation effect, since only actual policy indicators (*MMLs*) are statistical significant at 95% of confidence. Lead terms are not statistical significant and sufficiently close to zero. One exception is for the murders, whose lead term imposed 1 year before than the actual policy is statistical significant at 95% (but not significant at 99%). However, its magnitude is certainly smaller than the true impact of MMLs. This is a good news for the common trend assumption, underlying that our results are driven by treatment effect (approval of MMLs) rather than by differences in crime trend between treated and control counties.

We test our results also building a *randomized experiment* with results shown in Table 1.11. We ran a series of regressions (1000 trials) in which placebo MMLs are randomly assigned at control and treated counties[36]. Experiment is made for the variables murder, burglaries, larcenies, cocaine possession, and synthetic drug possession. The test is based on the estimation of Equation (1.4) with county fixed-effect, temporal dummies, and State specific linear trend.

[34]Specifically, we add the *MMLs* indicator for four years before the actual adoption of policies and for the year of actual adoption of MMLs. The lead terms are equal to 1 only in the relevant years, otherwise zero. Conversely, the *MMLs* indicator of actual policy is equal to 1 both in the approval date and in the subsequent years.

[35]Similar anticipation test is conducted by Autor (2003) who analyzes the contribution of *unjust dismissal* at the growth of U.S. Temporary Help Services in the last 3 decades.

[36]This approach is similar to Luallen (2006), who examined the relationship between teacher strike days and juvenile crime. Assignment of the placebo MMLs was based on random numbers drawn from the uniform distribution. This implies that each U.S. county included in our sample (true treated or not) has the same probability to be subject at placebo policy. Procedure of assignment is repeated for 1000 trials.

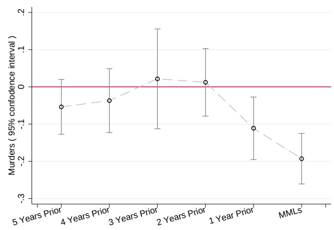
Table 1.10: *Placebo Test: fake years*

Murders	*True Policy*	*Fake 2 Years*	*Fake 4 Years*	*Fake 6 Years*	*Fake 1996-98*
MMLs	-0.156***	-0.201***	0.056	0.013	-0.013
	(0.027)	(0.029)	(0.059)	(0.031)	(0.050)
MMLs x Home	0.118**	0.162***	-0.102	-0.097*	-0.092
	(0.055)	(0.049)	(0.080)	(0.054)	(0.074)
MMLs x Disp	0.051**	-0.047**	-0.238***	-0.006	0.007
	(0.021)	(0.020)	(0.034)	(0.027)	(0.039)
MMLs x Home x Disp	0.015	0.081	0.180***	0.004	0.081
	(0.080)	(0.053)	(0.045)	(0.070)	(0.081)
Obs.	21,737	21,737	21,737	21,737	21,737
Burglaries	*True Policy*	*Fake 2 Years*	*Fake 4 Years*	*Fake 6 Years*	*Fake 1996-98*
MMLs	0.131***	0.128***	0.035	-0.022	-0.081
	(0.040)	(0.032)	(0.021)	(0.021)	(0.058)
MMLs x Home	-0.092	-0.096*	0.023	0.093	0.058
	(0.055)	(0.052)	(0.049)	(0.058)	(0.040)
MMLs x Disp	0.088***	0.114***	0.178***	0.121***	-0.175***
	(0.024)	(0.021)	(0.024)	(0.027)	(0.037)
MMLs x Home x Disp	-0.024	-0.094**	-0.141***	-0.068	0.120*
	(0.039)	(0.039)	(0.046)	(0.053)	(0.061)
Obs.	43,035	43,035	43,035	43,035	43,035
Larcenies	*True Policy*	*Fake 2 Years*	*Fake 4 Years*	*Fake 6 Years*	*Fake 1996-98*
MMLs	0.095***	0.116***	0.095**	-0.034	-0.047
	(0.020)	(0.029)	(0.036)	(0.032)	(0.039)
MMLs x Home	-0.100*	-0.091*	-0.063	0.082	0.028
	(0.055)	(0.051)	(0.059)	(0.053)	(0.051)
MMLs x Disp	-0.031**	0.024	0.078***	0.113***	-0.044
	(0.014)	(0.018)	(0.024)	(0.024)	(0.029)
MMLs x Home x Disp	0.054	0.004	-0.040	-0.105	0.004
	(0.042)	(0.039)	(0.056)	(0.066)	(0.052)
Obs.	44,072	44,072	44,072	44,072	44,072
Cocaine	*True Policy*	*Fake 2 Years*	*Fake 4 Years*	*Fake 6 Years*	*Fake 1996-98*
MMLs	0.176***	0.154***	-0.048	-0.143**	-0.051
	(0.047)	(0.037)	(0.030)	(0.056)	(0.052)
MMLs x Home	-0.275***	-0.256***	-0.077	-0.061	-0.081
	(0.096)	(0.082)	(0.087)	(0.105)	(0.065)
MMLs x Disp	0.180***	0.122***	0.240***	0.092**	-0.106**
	(0.035)	(0.031)	(0.033)	(0.035)	(0.042)
MMLs x Home x Disp	0.091	0.029	-0.151*	-0.081	-0.043
	(0.116)	(0.095)	(0.076)	(0.085)	(0.081)
Obs.	34,615	34,615	34,615	34,615	34,615
Synthetic	*True Policy*	*Fake 2 Years*	*Fake 4 Years*	*Fake 6 Years*	*Fake 1996-98*
MMLs	0.463***	0.343**	-0.026	-0.488***	0.132
	(0.166)	(0.149)	(0.090)	(0.131)	(0.095)
MMLs x Home	-0.255	-0.197	0.052	0.475**	-0.052
	(0.221)	(0.229)	(0.176)	(0.218)	(0.138)
MMLs x Disp	0.075	0.421***	0.640***	0.360***	-0.214**
	(0.096)	(0.074)	(0.074)	(0.092)	(0.105)
MMLs x Home x Disp	-0.112	-0.503**	-0.714***	-0.469***	0.154
	(0.222)	(0.227)	(0.184)	(0.114)	(0.126)
Obs.	29,258	29,258	29,258	29,258	29,258

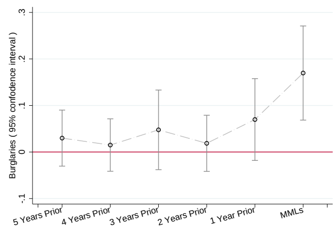
Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. The dependent variables are equal to natural logarithm of crime. All regressions include County fixed effect, time dummies, and State specific linear trend. *Cocaine* variable includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone. We deleted California from the sample because it approved MMLs in the 1996. To maintain always a pre-treatment period at least 2 years also in fake formulations, we set at 1996 the fake policies antecedent to 1996.

Figure 1.4: Anticipation effect test

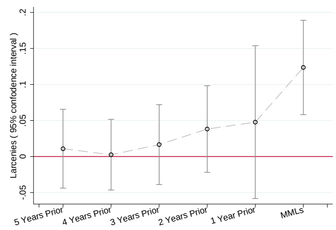
(a) Murders.



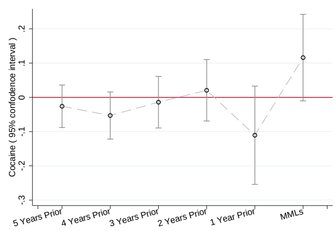
(b) Burglaries.



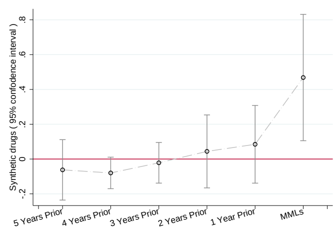
(c) Larceny.



(d) Cocaine possession.



(e) Synthetic drugs possession.



Note: Graphs report OLS estimates of formulation (1.5) for each crime variables which refer to MMLs coefficient, and its leads. Level of confidential interval is set at 95%.

Table 1.11 shows (respectively in Panel A, B, and C) the number of times (in percentage) in which policy dimension indicators have been statistically significant and coherent with findings reported in Table 1.6.
If the shift on crimes are truly ascribable at treatment, we would expect that most of the trials have coefficients close to zero. In other words, if the impact on crime is truly produced by the approval of MMLs, we would observe policy indicators not statistically significant in the most of trials. Otherwise, if we have a large number of times in which the policy indicators are different from zero, then the change on crime found in the counties subject to MMLs is not consequence of the policies but it might derive from others unobservable factors. Hence, the treatment effect estimated would be biased.

After the randomized assignment of policy, on average in 1000 trials, the 8.9% of pairs county-year truly subject to Medical Marijuana legislation are also assigned to the placebo policy. This percentage is such that, on average, the effect of placebo policy should be null (see Table 1.11 for the results of experiment).
At 90% of confidence level, the number of times for which placebo policy indicators result statistically different from zero is close to percentage to commit the *Type I Error*[37]. In the experiment the percentages vary among the 10 and 15 percentage points. At 99% of confidence level, estimations with coefficient significantly different from zero drastically fall, with

[37] We intend for *type I error* the incorrect rejection of a true null hypothesis (*false positive*), while a *type II error* is the failure to reject a false null hypothesis (*false negative*).

Table 1.11: Randomize treatment assignment (test on 1000 trials)

	Murders		*Burglaries*		*Larcenies*		*Cocaine*		*Synthetic*	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
(A)	*MMLs, mere decriminalization* (% on 1000 trials)									
Confidence level										
at 90%	13.3	7.5	12.1	7.2	12.5	6.5	13.3	7.4	13.3	8.4
at 95%	7.6	4.0	6.2	3.9	7.3	4.5	8.2	4.9	8.0	4.8
at 99%	2.4	1.6	2.3	1.3	1.9	1.2	2.6	1.7	2.4	1.3
(B)	*MMLs combined to Home Cultivation* (% on 1000 trials)									
Confidence level										
at 90%	14.0	-	12.5	7.1	13.0	7.3	13.5	7.2	13.6	-
at 95%	7.2	-	6.9	4.7	6.9	4.0	7.5	4.2	7.9	-
at 99%	2.4	-	2.0	1.1	2.0	1.4	2.5	1.6	2.6	-
(C)	*MMLs combined to Dispensaries* (% on 1000 trials)									
Confidence level										
at 90%	15.0	7.5	13.2	6.5	10.4	4.6	15.3	7.4	14.4	-
at 95%	8.7	4.8	6.8	3.2	6.7	2.6	9.0	4.7	8.3	-
at 99%	3.3	2.2	1.6	0.9	1.3	0.6	2.7	1.6	2.8	-
% of County-Year with actual policy and placebo policy								8.9		

Notes: Table presents results of randomized experiment for each crime, referring on estimation of our *baseline* formulation (equation 4). Random assignment of reform for each U.S. county is based on random numbers drawn from the uniform distribution. The procedure is repeated for 1000 trials. Column (1) shows the number of times (in percentage) in which the coefficient is significant in the 1000 trials. Column (2) shows number of times (in percentage) in which the coefficient is significant and coherent (in the 1000 trials) with results showed in Section 7. Symbol (-) indicates that coefficient are not significant in *baseline* formulation. The coefficients analysed are: merely approval of decriminalization of cannabis (Panel A), joint approval of decriminalization and home cultivation (Panel B), and joint approval of decriminalization and selling in dispensaries (Panel C). All coefficients are estimated (together) into the regression model, like equation 4, with County fixed effect, time dummies, and State specific linear trend. *Cocaine* variable includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone.

percentages between 0.9 and 2.5 percentage points, replicating the number of times in which the statistical *t-test* can produce a false positive (Table 1.11, column 1 for each crime). Interesting is also the number of times, on 1000 trials, in which placebo MMLs coefficients are coherent with findings corresponding to actual policy (Table 1.11, column 2 for each crime). For instance, placebo MMLs have been significant and coherent with our results, at 99% confidence level, only in 16 trials on 1000 for murders. The other policy indicators for all the crimes are close to this percentage and they are never greater than 22 times on 1000 trials. In summary, the numbers of times in which the randomized assignment of policy produces results coherent with our findings are really modest. This suggests that the changes in crime recorded in the counties subjected to Medical Marijuana Laws relative to counties unaffected, after and before the policy, may be interpreted as causal effect of the marijuana decriminalization for therapeutic uses.

D. Non Linear Models for Count Data.

The last robustness test concerns the estimate of a non-linear model for count data extremely popular in criminology[38].

[38] Lattimore et al. (2004), for instance, estimated a Poisson regression model to examine the number of re-arrests

The nature of criminal behaviour is such that for individuals or large aggregations (like cities or counties) the observed counts are often small (even zero) for many units and occasionally large for a few units. The consequence is that when the average counts are small (e.g. less than 5) the distribution of outcomes is skewed to the right. This is problematic for ordinary least square (OLS) regression analysis that assume normal distribution in errors around the expected average.
Poisson regression model represents the principal expression for count data, which well fits to the right skewed distribution of outcomes. This makes Poisson attractive to estimate skewed crime counts (Agresti, 2007; Piquero and Weisburd, 2010; Wooldridge, 2009)[39].
Unlike in the OLS regression model, which assumes that predicted values following a normal distribution defined by a expected average and a variance, the Poisson regression model is defined by a single parameter, such that the conditional mean and variance are supposed equal (*equi-dispersion property*)[40]. However, the *equi-dispersion property* is rarely met with observational data in criminology. Indeed, conditional variance is often greater than the conditional mean, thus we have *over-dispersion*. This means that the model fit to the observed data has more variation than is expected according to the Poisson distribution.
To face this issue, we implement robust standard errors so that the Poisson estimators could be yet consistent even though the distribution assumption is violated[41].
We employ a Poisson (*quasi-ML*) regression model with county fixed effect, time dummies, State specific time trend, and *cluster-robust standard errors* (clustered at U.S. State level)[42], considering the dichotomous indicator of Medical Marijuana Law as shown in Equation (1.2).
Most important is underline that as the numbers of counts gets larger, the outcome distribution becomes more normally distributed. In other words, OLS is fine with count outcomes when the mean gets large (e.g. $\geq$ 20). Moreover, *over-dispersion* is not an issue in ordinary regression model assuming normally distributed y, because the normal has a separate pa-

among people in parolees for the California. Osgood and Chambers (2000) use Poisson regression model to examine the count of juvenile arrests in the U.S. counties. Lattimore et al. (2005) employ a non linear model for count data for looking at the effects of substance abuse treatment on the count rearrests among drug-involved probationers in Florida.

[39]The Poisson model assumes that counts are well approximated by a Poisson distribution, which is unimodal and skewed to the right taking integers value. It is represented by a single parameters $\lambda > 0$, that's the expected probability of the count. Poisson distribution assumes identical mean and variance.

[40]*Equi-distribution assumption* implies that the model is intrinsically heteroskedastic.

[41]When the counts are not Poisson distributed, but the conditional mean function is well specified, *quasi-maximum likelihood estimator* with *robust standard errors* produces consistent estimator at cost of larger standard errors. Moreover, the Poisson regression model could be still suitable if the more (or less) variation than the expected average of the counts comes from unobserved heterogeneity between observations (i.e. omission of relevant variables) rather than from the data generating process. Hence, if there are suspects that model omits some relevant variables, the Poisson regression may be adequate (and likely the best fit) even though there are evidences of *over-dispersion* (or *under-dispersion*) (Simcoe, 2007). Alternative option is to estimate a *Negative Binomial* regression model which is the most common method to correct the *over-dispersion* phenomenon. Other variant of Poisson regression model is the *Zero Inflated* model, which takes into account of excess of zeros in data. Both *Negative Binomial* and *Zero Inflated* regression models exclude that *over-dispersion* (or *under-dispersion*) comes from by omitted heterogeneity, placing stringent assumptions on process generating the excess (dearth) of variation relative to the Poisson model. The violation of this assumption brings to biased estimators.

[42]The mass of zeros in our crime variables leads to keep out the choice of *Zero Inflated* regression model. Zeros can occur in two ways: as realization of the binary process and as realization of the count process when the binary random variable takes on a value of 1 (Cameron and Trivendi, 2009).

rameter from the mean to describe variability (i.e. the variance, σ^2). Therefore, when the average counts are large, it is better to estimate crime outcomes using OLS because the squared standard deviations from the mean are always the minimum estimator (Agresti, 2007; Piquero and Weisburd, 2010).
We have clear that the interaction effect in non-linear models has not the same interpretation than the marginal effect of the interaction term, like in a linear specifications[43]. Hence, the sign of interaction coefficient could not necessarily indicate the sign of the interaction effect (Ai and Norton, 2003). However, this issue appears not relevant when we apply a non-linear model in *difference-in-difference* approach since it aims to estimate the treatment effect rather than the simple interaction coefficient (or cross-difference).
The treatment effect is the cross difference of the conditional expectation of observed outcome minus the cross difference of conditional expectation of the potential (*conterfactual*) outcome without treatment. Since the latter cross difference is not zero in the non-linear model, the sign of treatment effect in a non-linear *difference-in-difference* with a strictly monotonic transformation function is equal to the sign of the coefficient of the interaction term of treated group.
The treatment effect is simply the incremental effect of the interaction coefficient. Whereas, the sign of cross-difference is irrelevant (Pinar et al., 2012; Puhani, 2008). Therefore, it seems correct to focus on the coefficient's sign of the interaction term, here represented by the dichotomous indicator of Medical Marijuana Law ($MMLs$).
Table 1.12 shows the results of *Poisson* regression model with fixed effects for all crime variables. On the bottom of Table 1.12 we report the estimations of Poisson model ruling out zero values from the sample.
We point out that murders, rapes, and synthetic drug possession are the main interest variables in this application with non-linear model, since they have unconditional mean lower than theoretical threshold (i.e. > 20) for which would be better to estimate an OLS model. Conversely, unconditional average of aggravated assaults, robberies, burglaries, larcenies, and cocaine possession is far and away greater than theoretical limit, thus the best fit remains the OLS estimation (see Table 1.3).
Results confirm the negative impact of Medical Marijuana Law on murders. This is coherent with our previous results and with Morris et al. (2014), and Gavrilova et al. (2015)[44]. The positive coefficient for synthetic drug possession suggests that the MMLs may encourage the *gateway effect* for these substances. Also this is in the line with our previous results. Therefore, the significantly increase in the numbers of synthetic narcotic substance laboratories and methamphetamine seizures among period 2008-2012 in the United States (as documented by the Drug Enforcement Administration 2014 in the *National Drug Threat Assessment Report*)

[43] In non-liner models the *i)* interaction effect could be non-zero, even if the interaction coefficient are non different from zero; *ii)* the statistical significance of interaction effect cannot be tested with a simple *t-test* on coefficient of interaction term; *iii)* interaction effect is conditional in independent variables, and *iv)* interaction effect may have different sign for different value of covariate (see Ai and Norton 2003).

[44] Gavrilova et al. (2015) finds a decrease in murders for Mexican border U.S. states passed Medical Marijuana legislations, due to lower juvenile-gang homicide episodes.

could derive in part from the approbation of the Medical Marijuana Laws. Finally, we abstain from the interpretation of significant coefficients of aggravated assaults and cocaine possession, since their high unconditional mean suggests to follow the OLS estimates which find a not clear effect of Medical Marijuana Law on these felonies.

Table 1.12: Poisson regression model (test)

	Violent Crime			Property Crime			Narcotic Possession	
	Murder	*Rape*	*Assault*	*Robbery*	*Burglary*	*Larceny*	*Cocaine*	*Synth.*
MMLs	-0.136***	0.025	-0.036*	0.024	0.023	0.032	-0.154***	0.229**
	(0.031)	(0.031)	(0.020)	(0.022)	(0.048)	(0.021)	(0.054)	(0.111)
Obs.	43,003	45,333	43,157	47,066	46,940	47,071	45,251	43,231
County	2,649	2,816	2,685	2,965	2,956	2,970	2,827	2,689
				Without Zeros				
MMLs	-0.140***	0.029	-0.036*	0.024	0.023	0.032	-0.143***	0.225*
	(0.034)	(0.031)	(0.020)	(0.022)	(0.048)	(0.021)	(0.048)	(0.123)
Obs.	22,383	31,880	30,530	44,431	44,095	45,129	35,578	29,099
County	2,358	2,687	2,511	2,942	2,938	2,950	2,711	2,540
Zeros (%)	52.10%	32.40%	6.10%	35.10%	6.80%	4.60%	24.60%	38.20%

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at State level. Dependent variables are integer number of crime. *Cocaine* includes the possession of opium and/or cocaine substances and all their derivatives (i.e. morphine, heroin, and codeine). *Synthetic* narcotics consists of the possession of demerol, and methadone. All regressions include County fixed effect, time dummies, and State specific linear trend.

1.8 Conclusion

Medical Marijuana Laws are created not equally between the U.S. States.
There are important nuances that can induce a differential effect of these policies on crime. We identify four key dimensions of policy: *i)* the mere cannabis decriminalization, *ii)* the establishment of mandatory patient registry systems, *iii)* the licence to cultivate cannabis at home, *iv)* the licence to sell cannabis in dispensaries.
Analyses that ignore the key elements could inaccurately represent the effects of these policies. This paper provides some additional insight into the inconsistent findings in the literature related to MMLs using U.S. county level data.
When we represent the Medical Marijuana legislations through a dichotomous indicator (neglecting the key dimensions of policy), we find a weak either no impact on crimes at the U.S. county level. Only murders and synthetic drug possession appear, respectively, negatively and positively associated with approval of reform. However, the basic dichotomous indicator for MMLs could simply capture the net effect of reform on crime obscuring the heterogeneous impact associated with the key dimensions put in place.
Our results confirm that the key dimensions make the impact of Medical Marijuana Laws on

crime heterogeneous, specially for burglaries, larcenies, cocaine, and synthetic drug possession. We find that, on average in each county, the mere cannabis decriminalization produces a rise for these felonies. At the same time, the licence of cannabis cultivation decreases these crimes. The selling of marijuana in dispensaries instead seems to have a differentiated impact: positive for burglaries and cocaine possession, negative for larcenies.

We classify the above key dimensions of MMLs in *demand side policies* (which decriminalize the cannabis and establish a patient registry system) or *supply side policies* (which provide legal sources of supply marijuana through the home cultivation or the selling in dispensaries). Looking at this classification for burglaries, larcenies, and cocaine possession, we find that the impact on crime is positive for the *demand side policies* and negative for the *supply side policies*. Therefore, the provision of legal sources of supply marijuana is able to countervail the positive impact on crime induced from *demand side policies* realizing a *crowding-out effect*. This pattern is compatible with the *systemic transmission channel* between decriminalization and crime as hypnotised by Goldstein (1985).

The mere marijuana decriminalization (i.e. *demand side policies*) induces a positive shock on demand of cannabis. However, these policies don't establish legal sources of supply marijuana pushing the augmented demand toward the illegal markets of narcotic substances. This could finance and strengthen the trafficking organizations with an increase of crime at the county level. Conversely, the *supply side policies* (providing legal sources of supply marijuana) would push out the black trades by marijuana market weakening the trafficking organizations. The result would be a fall of violence. Specifically, the fall would be such to countervail the increase of crime ascribable to *demand side policies*.

Also modelling for the key dimensions of policy, the results confirm the overall fall in Murders with positive impact for the dispensaries and none relevant effect for the home cultivation. The fall in homicides might derive by the different distribution of law enforcement and their resources in favour of more ferocious felonies, like murders, or by the exit of Mexican drug trafficking organizations from marijuana market (Gavrilova et al., 2015).

Furthermore, we find an overall increase in synthetic drug possession, supporting the *gateway hypothesis*.

Medical Marijuana Laws might provide an important lesson for policy makers. Drug markets are well-known for their violence. However, shrinking the business possibility of illegal trade, through legal sources of supply marijuana, it may reduce a significant part of violence. We would expect stronger effects on crime from full liberalization of sources for supply marijuana (which will allow for large scale production by corporations) since they could push organized crime completely out from the profitable marijuana market.

The possible reduction of violence associated with these policies could be only one aspect associated with a full marijuana liberalization. Other questions (as public health or the effect on other narcotic substances) remain open.

Medical marijuana policies continue to evolve as the Federal government increasingly tolerances state experimentation in this policy space. Given this shift in the Federal government's position, not only are more states adopting MMLs but states with existing MMLs continue to make significant changes in how they supply and regulate medical marijuana. Thus, with policy in developments in this area constantly in flux, new analysis are needed. Future policy may look very different from early MMLs and understanding the possible heterogeneous effects of these policies is important to predict the consequences of the new wave of decriminalization.

1.9 Appendix

Table 1.13: Year disribution of Medical Marijuana Laws by U.S. State

Year of First Regulation	*Relative No. of MML*	*Cumulative No. of MML*	*U.S. State that adopted the Medical Marijuana Law*
1994	0	0	-
1995	0	0	-
1996	1	1	California
1997	0	1	-
1998	2	3	Alaska, Washington
1999	2	5	Maine, Oregon
2000	0	5	-
2001	3	8	Colorado, Hawaii, Nevada
2002	0	8	-
2003	0	8	-
2004	2	10	Vermount, Montana
2005	0	10	-
2006	1	11	Rhode Island
2007	1	12	New Mexico
2008	0	12	-
2009	1	13	Michingan
2010	3	16	Arizona, District fo Columbia, New Jersey
2011	1	17	Delaware
2012	1	18	Connecticut
2013	1	19	Massachusetts
2014	5	24	Illinois, Maryland, Minnesota, New Hampshire, New York

Source: Pro.con.org - Pros & Cons of Controversial Issues; Pacula et al. (2015)

Crime Variables

Source: Handbook-UCR (2004)

- Violent Crime.
  - *Criminal Homicide: murder and nonnegligent manslaughter*.
    Definition: The willful (nonnegligent) killing of one human being by another. As a general rule, any death caused by injuries received in a fight, argument, quarrel, assault, or commission of a crime is classified as Murder and Nonnegligent Manslaughter.
  - *Forcible rape*.
    Definition: The carnal knowledge of a female forcibly and against her will. Reporting agencies classify one offense for each female raped or upon whom an assault to rape or attempt to rape has been made. Reporting agencies classify rapes or attempts accomplished by force or threat of force as forcible regardless of the age of the female victim.
  - *Aggravated Assault*.
    Definition: An unlawful attack by one person upon another for the purpose of inflicting severe or aggravated bodily injury. This type of assault usually is accompanied by the use of a weapon or by means likely to produce death or great bodily harm (e.g. firearm, knife or cutting instrument, other dangerous weapon, other assaults).

- Property Crime.
  - *Robbery*.
    Definition: The taking or attempting to take anything of value from the care, custody, or control of a person or persons by force or threat of force or violence and/or by putting the victim in fear. Robbery is a vicious type of theft in that it is committed in the presence of the victim. The victim, who usually is the owner or person having custody of the property, is directly confronted by the perpetrator and is threatened with force or is put in fear that force will be used. Robbery involves a theft or larceny but is aggravated by the element of force or threat of force.
  - *Burglary (breaking or entering)*.
    Definition: The unlawful entry of a structure to commit a felony or a theft. The UCR Program classifies offenses locally known as burglary (any degree), unlawful entry with intent to commit a larceny or felony, breaking and entering with intent to commit a larceny, housebreaking, safecracking, and all attempts at these offenses as burglary.

  - *Larceny (theft)*.
    Definition: The unlawful taking, carrying, leading, or riding away of property from the possession or constructive possession of another. Constructive possession is when one does not have physical custody or possession, but is in a position to exercise dominion or control over a thing.

- NARCOTICS CRIME.
  - *Cocaine Possession*.
    Definition: Includes possession of opium, cocaine, and all their derivatives like morphine, heroine, and codeine.
  - *Synthetic narcotics possessions*. Definition: Consists of the possession of synthetic illicit drugs, like demerol, and methadone.

Table 1.14: Medical Marijuana Laws by U.S. State

U.S. State	Pass Date	Effective Date	Pass Rate	Registration	Possession Limit
Alaska	Nov. 3, 1998	Nov. 4, 1999	58% (Ballot Measure 8)	Yes	1oz / 6 plants[a]
Arizona	Nov. 4, 2010	Nov. 4, 2010	50% (Proposition 203)	Yes	2.5oz / 12 plants
California	Nov. 5, 1996	Nov. 6, 1996	56% (Proposition 215)	Yes[b]	8oz / 6 plants[c]
Colorado	Nov. 7, 2000	Jun. 6, 2001	54% (Ballot Amendment 20)	Yes	2oz / 6 plants[a]
Delaware	May 13, 2011	Jul. 1, 2011	17-4 (Senate Bill 17)	Yes	6oz
DC	May 21, 2010	Jul. 27, 2010	13-0 (Amendment Act B18-622)	Yes	2oz
Hawaii	Jun. 14, 2000	Dec. 28, 2000	13-12 (Senate Bill 862)	Yes[d]	3oz / 7 plants[e]
Maine	Nov. 2, 1999	Dec. 22, 1999	61% (Ballot Question 2)	Yes[f]	2.5oz / 6 plants
Michigan	Nov. 4, 2008	Dec. 4, 2008	63% (Proposal 1)	Yes	2.5oz / 12 plants
Montana	Nov. 2, 2004	Nov. 3, 2004	62% (Initiative 148)	Yes	1oz / 4 plants
Nevada	Nov. 7, 2000	Oct. 1, 2001	65% (Ballot Question 9)	Yes	1oz / 7 plants[e]
New Jersey	Jan. 18, 2010	Jan. 19, 2010	25-13 (Senate Bill 119)	Yes	2oz
New Mexico	Mar. 13, 2007	Jul. 1, 2007	32-3 (Senate Bill 523)	Yes	6oz / 16 plants[g]
Oregon	Nov. 3, 1998	Dec. 3, 1998	55% (Ballot Measure 67)	Yes	24oz / 24 plants[h]
Rhode Island	Jan. 3, 2006	Jan. 4, 2006	33-1 (Senate Bill 710)	Yes	2.5oz / 12 plants
Vermount	May 26, 2004	Jul. 1, 2004	22-7 (Senate Bill 76)	Yes	2oz / 9 plants[i]
Washington	Nov. 3, 1998	Nov. 4, 1998	59% (initiative 692)	No	24oz / 15 plants

Source: Pros & Cons (2015); Pacula et al. (2015); Alford (2014).

Note: [a]6 plants: 3 mature and 3 immature. [b]Voluntary since Jan. 1, 2004. [c]6 Plants mature or 12 immature. [d] Mandatory after Dec. 31, 2010. [e] 7 plants: 3 mature and 4 immature. [f] Mandatory after Dec. 31, 2010. [g] 16 plants: 4 mature and 12 immature. [h] 24 plants: 6 mature and 18 immature. [i] 9 plants: 2 mature and 7 immature.

Table 1.15: Dimensional decomposition of Medical Marijuana Law (1994-2012)

	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012
Alaska			(a)	(b,c)													
Arizona															(a,b,c)		(d)
California	(a,b)							(d)							(c)		
Colorado						(a,b,c)				(d)							
Delaware																(a,b)	
D.C.															(a)		
Hawaii					(a,b,c)												
Maine				(b,c)										(a,d)			
Michigan													(b,c)	(d)			
Montana									(b,c)					(d)		(a)	
Nevada						(a,b,c)								(d)			
New Jersey															(a,b)		(d)
New Mexico												(a,b,c)		(d)			
Oregon			(b,c)									(a)		(d)			
Rhode Island											(a,b,c)						
Vermount									(a,c)			(b)					
Washington			(b,c)													(d)	

Source: Pros & Cons (2015); Pacula et al. (2015); Alford (2014)

Note: Table reports how the specific dimensions of Medical Marijuana Law passed in the U.S. State among 1994-2012.
The specific dimension considered are: *(a)* decriminalization of cannabis possession and consumption for therapeutic uses; *(b)* require patient registry system; *(c)* allow home cultivation of marijuana, and *(d)* legally permit dispensaries.

Chapter 2

Employment Protection Legislation and workers flows

(with *Giovanni Pica*).

2.1 Introduction

Since Lazear (1990), much empirical research has focused on the effects of dismissal costs on labour market outcomes. In line with theoretical predictions (Bentolila and Bertola, 1990), the evidence available so far suggests that stricter EPL decreases employment inflows and outflows with little effects on employment and unemployment stocks. The reason is that firing costs act, in expected present discounted value, also as hiring costs, thus reducing the willingness of the firms to both fire and hire workers.

The recent literature usually identifies the causal impact of employment protection on labour market outcomes exploiting within-country variation in EPL either across firms (e.g. of different sizes) or workers (e.g. of different age and/or tenure).[1].

Compared to cross-country studies, this approach allows to control for any time-varying unobserved country characteristics that may affect labour market outcomes and act as a confounding factor.[2].

For lack of higher frequency data, the within-country literature usually estimates the impact of firing costs on *annual* worker and job flows (with the notable exception of Marinescu 2009). This may lead to an underestimation of the allocative inefficiencies generated by EPL on job flows, as the impact of transitory shocks on high-frequency adjustments is not captured by

[1]Among others, Venn (2009), Martins (2009), Bauer et al. (2007); Verick (2004), Bauernschuster (2009), Kugler and Pica (2008), Boeri and Jimeno (2005), and Schivardi and Torrini (2008). Along similar lines, Acemoglu and Angrist (2001) and Autor et al. (2007) exploit within-US cross-states variation in EPL. See Skedinger (2010), for a recent review.

[2]An increasing number of studies have recently started focusing on the impact of EPL on other margins both at the worker and firm level, like worker effort, wages, capital deepening and total factor productivity (Cingano et al., 2015; Ichino and Riphahn, 2005; Leonardi and Pica, 2013; Prifti and Vuri, 2013; Scoppa, 2010).

annual data (Blanchard and Portugal, 2001; Wolfers, 2015). Additionally, the effect of EPL on worker flows cannot be properly identified using low-frequency (e.g. annual) data in the presence of differential high-frequency (e.g. seasonal) trends across treated and controls. This essay assesses the impact of EPL on workers' accessions exploiting *monthly* data from Italian social security records for the period 2012 and 2014.[3] The data provide information on the number of newly hired workers by firm size, province, sector, contract type, age and gender at a monthly frequency. Identification relies on a labour market reform introduced in Italy in July 2012 (*law 92/2012*, the so-called *Fornero reform*), that made the dismissal of permanent employees hired in firms with more than 15 employees less costly, while keeping the rules for firms below 15 unchanged.
We, thus, exploit the differential law change between large and small firms to implement a *difference-in-difference*, comparing hirings in firms above and below the 15 employee threshold before and after the reform (July 2012), controlling both for unobserved heterogeneity across cells defined by the intersection between firm size $\times$ province $\times$ 2-digit sector $\times$ contract type $\times$ age $\times$ gender and for the differential evolution of seasonal trends across large and small firms.

We find that monthly hirings on open-ended contracts increase in large firms relative to small firms after the reform by about 5.2 percentage points, i.e. about 20% of the pre-reform level. Interestingly, the effect of the reduction in EPL is not homogeneous across workers' types. The reform seems to favour mostly young, blue-collars, and full-time workers. Conversely, we find no significant effect on the number of conversions of temporary contracts into permanent ones. The results are also robust to the inclusion of regional and industrial specific time trends.

The rest of essay is organized as follows. Section 2.2 describes the evolution of the legislation ruling unfair dismissals in Italy. Section 2.3 discusses the identification strategy and presents the regression model. Section 2.4 describes the data. Section 2.5 presents the results and Section 2.6 provides a number of robustness checks. Section 2.7 concludes.

2.2 Regulatory background and the 2012 EPL reform

Over the years, the Italian legislation ruling unfair dismissals has changed several times. Both the magnitude of the firing cost and the coverage of the firms subject to the restrictions have gone through extensive changes. Individual dismissals were first regulated in Italy in 1966 through Law 604, which established that employers could freely dismiss workers either for economic reasons (considered as fair 'objective' motives) or in the case of misconduct (considered either as fair 'subjective' motive or as just cause). In any case, workers could take employers to court and judges would determine if the dismissals were indeed fair or unfair.

[3]The data set is based on the forms through which firms report to the Italian social security institute (INPS) information on the social security contributions paid within the month.

In the case of unfair dismissal, employers had the choice of either reinstating the worker or paying severance, which depended on firm size and – loosely – on tenure.
In 1970, the *Statuto dei Lavoratori* (Law 300) established that all firms with more than 15 employees had to reinstate workers and pay their foregone wages in cases of unfair dismissals. Firms with fewer than 15 employees remained exempt. Law 108, introduced in July 1990, restricted dismissals for permanent contracts in small firms and introduced severance payments of between 2.5 and 6 months pay for unfair dismissals in firms with 15 or fewer employees. Firms with more than 15 employees still had to reinstate workers and pay foregone wages in cases of unfair dismissals.

The *Fornero reform* passed in 2012 substantially changed the discipline concerning the dismissals in firms above 15 employees. It established that in case of unfair dismissal, the dismissed worker has no longer the right to be reinstated as in the pre-reform period and receives a monetary compensation that ranges between 12 and 24 months pay.[4] Thus the reform significantly reduces the firing cost borne by large firms.
Although the *Fornero reform* touched upon several aspects of the labour legislation[5], our empirical approach isolates the effect of the change in the strictness of EPL, since all other interventions did not affect differentially firms above and below the 15-employee threshold. The paper exploits the differential change in firing costs induced by the 2012 reform to identify the impact of EPL on the Italian labour market.

2.3 Identification

In order to identify the effect of EPL on worker flows, we set up a standard *difference-in-difference* model. Equation (2.1) formalizes our identification strategy:

$$y_{i,t} = \alpha_i + \delta_t + \tau \times Policy_{i,t} + \beta \times Size_{i,t}^{16-19} + \gamma \times Policy_{i,t} \times Size_{i,t}^{16-19} + u_{i,t} \qquad (2.1)$$

The index i identifies a cell defined by the intersection between firm size $\times$ province $\times$ 2-digit sector $\times$ contract type $\times$ age $\times$ gender. The subscript t denotes time at the monthly frequency. Additionally, $y_{i,t}$ is the (log of the) number of hirings in cell i at time t; α_i is a cell fixed effect δ_t is a time effect (month $\times$ year dummies); $Policy_{i,t}$ is a post-reform dummy which takes value 1 after the *Fornero reform* was passed, i.e. after July 2012. To ensure comparability between treated and controls we estimate the above equation restricting on hirings taking

[4]The reform also introduces the requirement of a conciliation attempt between the employer and the dismissed worker as a pre-requisite for further legal action. The conciliation procedure cannot last more than 20 days from the date in which the parties are called on to meet, unless they agree to further discuss the issue until a settlement is achieved. If the conciliation procedure is not effective, the employer can dismiss the worker. If the employer does not justify the dismissal or does not respect the obligation to seek a conciliation with the worker, the worker is entitled to receive a severance payment ranging between 6 and 12 monthly payments, even if the economic grounds for dismissal exist.

[5]For instance, the law redesigned the rules on apprenticeships and re-employment of workers over-50.

place in firms between 10 and 19 employees; $Size_{i,t}^{16-19}$ is a dummy that takes value 1 for hirings in firms above fifteen. We cluster standard errors at regional level (Bertrand et al., 2004) to account for within-region serial correlation of the shocks.
The coefficient of the interaction term (γ) between the firm size dummy and the post-reform dummy (i.e. July 2012 - March 2014) represents the *average treatment on the treated* (ATT) and identifies the differential change in permanent hirings in large firms relative to small firms after the reform.
The inclusion of cell fixed effects accounts for unobserved time-invariant specific characteristics at the cell level, while time effects control for common macro economic shocks at the monthly frequency.
We also estimate an alternative version of model (2.1) which includes sectoral and regional time trends in order to account for the potential differential evolution of employment flows by region or sector. This specification aims at capturing region- or sector-specific time-varying factors that may affect hirings. We also allow those factors to affect differently small and large firms.
Equation (**??**) shows the estimated model, where r indexes region or sector:

$$\begin{aligned} y_{i,t} = \alpha_i + \delta_t + \rho_r \times D_r \times Trend + \xi_r \times D_r \times Trend \times Size_{i,t}^{16-19} + \\ + \tau \times Policy_{i,t} + \beta \times Size_{i,t}^{16-19} + \gamma \times Policy_{i,t} \times Size_{i,t}^{16-19} + u_{i,t} \end{aligned} \quad (2.2)$$

One important issue that deserves discussion is the possibility that firms self-select into or out of the treatment group, as they can choose whether to grow above or shrink below the fifteen employee threshold.
Garibaldi et al. (2004); Schivardi and Torrini (2008) and Leonardi and Pica (2013) show that the firm size distribution displays no bunching right below the 15-employee threshold and that the probability to grow is only slightly smaller for firms at 14 employees relative to firms far away from the threshold. Thus, it is unlikely that firm sorting biases our results.
An additional threat to identification is the possibility that the parallel trend assumption – that requires the pre-reform trends of treated and controls to be parallel – does not hold. To address this issue, we estimate a *difference-in-difference-in-difference* regression model, exploiting the fact that the reform does not affect managerial hirings.

2.4 Data and descriptive statistics

The empirical analysis uses administrative panel data from the Italian Social Security Institute (INPS) for the period 2012-2014. The dataset includes information on the number of monthly hirings and conversions in permanent contracts of workers in firms with at least one employee. Since the INPS collects informations for the purpose of computing retirement benefits, whence derive the contributions charged by workers and employers, this data source is very reliable. One major shortcoming of this data set is that the unit of observation is neither the firm nor the worker: it is a cell identified by provinces (about 100), sectors (based on the *Ateco 2002* 2-digit classification), firm size categories (measured in terms employment), types of contract (i.e. apprenticeship, fixed-term, and open-ended contract), position (i.e. blue collar, white collar, apprentice, *quadro*,[6] and manager), number of hours worked (i.e. full time, part time), gender, and age categories. Table 2.1 shows the employee-firm characteristics available in the data.

Table 2.1: Employee and firm characteristics available in the data

Type of contracts	Hiring on open-ended contracts; Hiring on fixed-term contracts; Hiring on apprenticeship; Conversion in open-ended contract from fixed-term agreement; Conversion in open-ended contract from apprenticeship.
Employee age (classes)	Less than 20 years; 20-24 years; 25-29 years; 30-34 years; 35-39 years; 40-44 years; 45-49 years; 50-54 years; 55-59 years; 60-64 years; More than 65 years.
Employee gender	Man; Woman.
Employee position	Blue collar; White collar; Quadro; Apprentice; Manager.
Hours	Full-time; Vertical part-time; Horizontal part-time; Mixed part-time.
Sector (Ateco 2002 classification)	Agriculture, hunting and forestry (A); Fishing, fish farming and related services (B); Mineral processing (C); Manufacturing (D); Production and distribution of electricity, gas and water (E); Buildings (F); Wholesale and retail trade, and repair of durable goods (G); Hotels and restaurants (H); Transport, storage and communication (I); Financial assets (J); Real estate, renting, information technology, research, and business services (K); Public administration (L); Public Education (M); Public Health and social work (N); Other public, social and personal services (O); Activities of households (P); Extraterritorial organizations and bodies (Q).
Firm size (classes)	1 worker; 2-9 workers; 10-15 workers; 16-19 workers; 20-49 workers; 50-99 workers; 100-199 workers; 200-499 workers; 500-999 workers; More than 1000 workers.
Broad geographical area	North-West; North-Est; Center; South; Islands; Abroad.
Region	20 Italian regions.
Province	About 100 Italian provinces.

Since the *Fornero reform* concerns only private sector firms and employees, we drop public sector hirings. We also exclude hirings by Italian firms located abroad and the agricultural sector for which the relevant thresholds are different. Moreover, in our baseline specifications we exclude hirings in managerial positions since they are not covered by the reform. Finally,

[6]Employees in the *quadro* position are high-level white collars right below managers.

we focus on the period between January 2012 and March 2014, because in March 2014 a new labor market reform, the *Poletti decree*, was approved. We focus on hirings in firms between 10 and 19 workers: firms with 10-15 workers represent the control group, while firms with 16-19 workers represent the treatment group. As a robustness check, in Section 2.6, we experiment with different firm size windows.
The final dataset includes 362,519 observations between January 2012 and March 2014, with 255,232 observations for large firms and 107,287 for small firms.

Table 2.2 presents descriptive statistics by firm size (i.e. large and small) and reform period (i.e. before and after July 2012).
The double difference between the means of two groups across the two periods provides a first raw estimate of the impact of the reform on permanent hirings and on conversions into permanent employment (Table 2.2). Hirings on open-ended contracts decrease in the post-reform period both in small and large firms, but less so in large firms. The double difference (equal to 0.037 log points; Panel B of Table 2.2) suggests that less strict EPL might have increased hirings on open-ended contracts. The same pattern seems to show up for the conversions of fixed-term agreements into permanent contracts (0.017 log points; Panel C of Table 2.2). Conversely, apprenticeships transformed into open-ended contracts seem to be unaffected by the reform (0.001 log points; Panel D of Table 2.2).
Although the simple comparisons between means suggests that the decrease in the firing costs affected open-ended hirings and the conversions of temporary into open-ended contracts, (part of) these changes may be due to (un)observable heterogeneity. In order to account for it, Section 2.3 presents conditional estimates.

As a last description of the data, we provide a visual impression of the number of hirings (and conversions into permanent contracts) in large and small firms between January 2012 and October 2014 (Figure 2.1). The period we focus on spans between the two reforms that took place in Italy, namely the *Fornero reform* (July 2012) and the *Poletti decree* (March 2014).
Figure 2.1 helps understanding the identification strategy. Under the assumption that, absent the reform, large and small firms would have experienced the same trends in hirings, any deviation in large firms hirings relative to small firms hirings is ascribable to the Fornero reform. The triple difference strategy presented in Section 2.6 will explore the robustness of the analysis to the common trend assumption.

2.5 Results

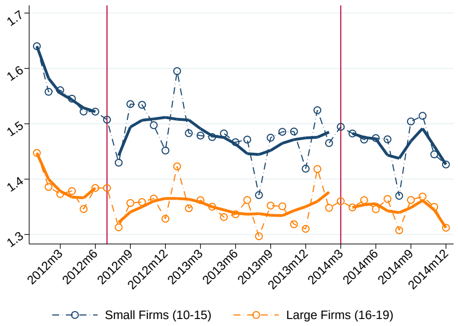
Table 2.3 shows results from the estimates of equation (2.1). The dependent variable is the (log of the) number of hirings recorded in each month and within each cell between January 2012 and March 2014.

Table 2.2: *Non-parametric impact of reform*

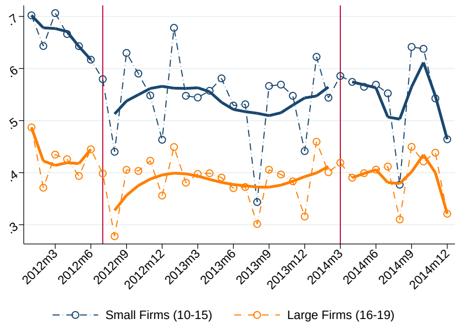
(A) *All type of contracts*	POST-REFORM	BEFORE POLICY	DIFFERENCE
LARGE FIRMS (*obs.*)	79,736	27,358	
mean	0.202	0.225	- 0.023
standard errors	0.420	0.442	
SMALL FIRMS (*obs.*)	189,756	64,659	
mean	0.286	0.337	- 0.051
standard errors	0.517	0.570	
DIFFERENCE	- 0.084	- 0.112	**0.028**
(B) *Open-ended contracts*	POST-REFORM	PRE-REFORM	DIFFERENCE
LARGE FIRMS (*obs.*)	47,647	16,771	
mean	0.247	0.270	- 0.023
standard errors	0.466	0.486	
SMALL FIRMS (*obs.*)	113,921	39,993	
mean	0.337	0.397	- 0.060
standard errors	0.564	0.621	
DIFFERENCE	- 0.090	- 0.127	**0.037**
(C) *Permanent conversions*	POST-REFORM	PRE-REFORM	DIFFERENCE
LARGE FIRMS (*obs.*)	26,829	8,802	
mean	0.143	0.159	- 0.016
standard errors	0.338	0.360	
SMALL FIRMS (*obs.*)	63,028	20,307	
mean	0.219	0.252	- 0.033
standard errors	0.433	0.471	
DIFFERENCE	- 0.076	- 0.093	**0.017**
(D) *Apprentice transformed*	POST-REFORM	PRE-REFORM	DIFFERENCE
LARGE FIRMS (*obs.*)	5,260	1,785	
mean	0.100	0.125	- 0.025
standard errors	0.277	0.307	
SMALL FIRMS (*obs.*)	12,807	4,359	
mean	0.163	0.189	- 0.026
standard errors	0.370	0.397	
DIFFERENCE	- 0.063	- 0.064	**- 0.001**

Figure 2.1: Hiring and conversions in small firms relative to large firms.

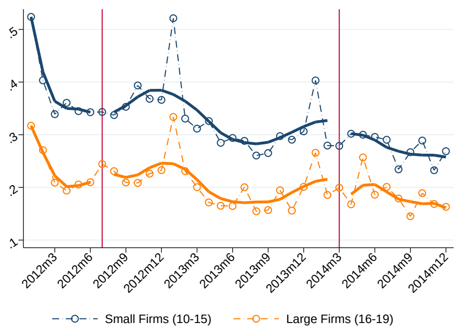
(a) All type of contracts



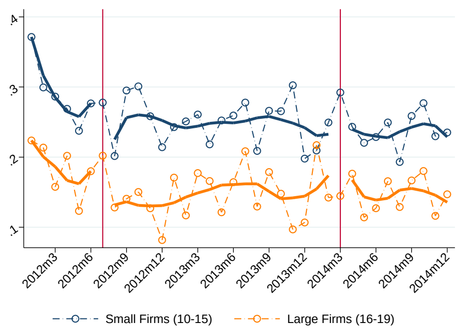
(b) New open-ended contracts



(c) Conversions of temporary into permanent contracts



(d) Conversion of apprenticeships into permanent contracts



Note: each dot represents the average (log of the) number of hirings in each cell by firm-size (10-15 and 16-19 employees) and type of contract. The cell is defined at the province × sector × gender × age × position × full-time/part-time level. Panel 2.1a includes all contracts types. Panel 2.1b restricts to permanent hirings; panels 2.1c and 2.1d focus on conversions into open-ended contracts of fixed-term contracts and apprenticeship, respectively. The first vertical red line indicates the date of approval of the *Fornero reform* (July 2012); the second one indicates the date of approval of *Poletti's Decree* (March 2014).

Our results show an increase in the total number of hirings on open-ended contracts in firms just above the 15-employee threshold relative to the firms just below after the reform (column 1, Table **??**). This effect is largely driven by the increase in new permanent hires which goes up by about 0.051 percentage points (column 2, Table **??**). Conversely, we find a negligible effect on the number of conversions into permanent contracts of temporary contracts and apprenticeships (columns 3-4, Table **??**).
According to these estimates, the reform triggered an increase in the number of permanent hires of about 4,611 workers each month.[7]

Results suggests that greater flexibility induced firms to expand permanent hirings. This

[7]The monthly increase is obtained multiplying the change for large firms relative small firms obtained in each cell, equal to 0.081, $(0.081 = e^{0.436+0.051} - e^{0.436})$, times the total number of cells $(0.81 \times 56,933)$.

Table 2.3: *Effect of reform on permanent employment in large firms relative to small firm*

VARIABLES	(1) *All type of contracts*	(2) *Open ended contracts*	(3) *Conversion from temporary to permanent*	(4) *Conversion from apprentice to permanent*
Reform Period (*July 2012 - March 2014*)	0.003	-0.018	0.054***	-0.037***
	(0.009)	(0.011)	(0.012)	(0.013)
Reform Period × Large firms	0.039***	0.051***	0.017	0.012
	(0.005)	(0.007)	(0.010)	(0.016)
Constant	0.402***	0.436***	0.371***	0.208***
	(0.013)	(0.014)	(0.015)	(0.015)
Obs.	362,519	219,329	118,966	24,224
No. Cell	99,901	56,933	35,766	7,202
R-squared	0.019	0.019	0.037	0.008
Cell FE	YES	YES	YES	YES
Month Dummies	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES

Notes: Significance levels: ***1%, **5%, *10%. Standard errors in parentheses adjusted for clustering at regional level. The sample includes only hirings in firms between 10 and 19 employees. The dependent variable is (the log of) the number of hirings in each cell given by the intersection between province, sector (Ateco2002 classification), worker age, gender, firm size, position (white-collar or blue-collar or quadro), and full-time/part-time).

result is consistent with the previous empirical (Blanchard and Portugal 2001, Bauernschuster 2009, Kugler and Pica 2008, Wolfers 2015, and Acemoglu and Angrist 2001 among others) and theoretical literature (Bentolila and Bertola, 1990; Hopenhayn and Rogerson, 1993). Instead, conversions into open-ended contracts both from fixed-term agreements and from apprenticeships seem to be unaffected by the reform.[8]

Accounting for high-frequency business cycle adding month × year dummies and for size-specific seasonal patterns adding month × $Size_i^{16-19}$ dummies, does not affect our results (Table 2.4).

2.5.1 Dynamics and heterogeneity

This section explores whether the average effect found in the previous section hides any heterogeneity either over time or across different types of workers. To explore the dynamics of the reform, we provide estimates from the baseline model (2.1), augmented with leads and lags of the policy indicator as in Autor (2003). Specifically, we add separate policy indicator dummies for the two quarters that precede the actual adoption of reform, for the quarter of the actual adoption of reform, and for the 6 subsequent quarters.

[8] Grassi (2009) instead finds that more stringent EPL has a positive effect on the conversion rates of training contracts (i.e. *contratti di formazione e lavoro*).

Table 2.4: Effect of reform on permanent employment in large firms relative to small firms

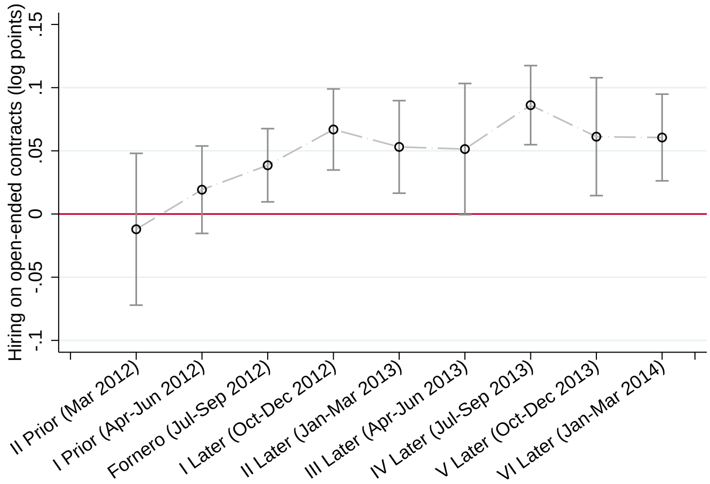
VARIABLES	(1) *Open ended contracts*	(2) *Conversion from temporary to permanent*	(3) *Conversion from apprentice to permanent*	(4) *Open ended contracts*	(5) *Conversion from temporary to permanent*	(6) *Conversion from apprentice to permanent*
Reform Period	-0.003	0.014	-0.047	-0.017	0.054***	-0.040***
	(0.010)	(0.028)	(0.034)	(0.011)	(0.012)	(0.012)
Reform Period × Large firms	0.051***	0.017	0.012	0.049***	0.018	0.021
	(0.007)	(0.010)	(0.016)	(0.009)	(0.012)	(0.013)
Constant	0.412***	0.356***	0.240***	0.450***	0.388***	0.195***
	(0.016)	(0.013)	(0.022)	(0.013)	(0.011)	(0.019)
Obs.	219,329	118,966	24,224	219,329	118,966	24,224
No. Cell	56,933	35,766	7,202	56,933	35,766	7,202
R-squared	0.020	0.038	0.009	0.020	0.038	0.009
Cell FE	YES	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES	YES
Month Dummies	YES	YES	YES	YES	YES	YES
Year × Month Dummies	YES	YES	YES	NO	NO	NO
Month × Large firms Dummies	NO	NO	NO	YES	YES	YES

Notes: Significance levels: ***1%, **5%, *10%. Standard errors in parentheses adjusted for clustering at regional level. The sample includes only hirings in firms between 10 and 19 employees. The dependent variable is (the log of) the number of hirings in each cell given by the intersection between province, sector (Ateco2002 classification), worker age, gender, firm size, position (white-collar or blue-collar or quadro), and full-time/part-time).

The dynamics of the impact of the reform on new permanent hires are reported in Figure 2.2. The coefficients on the adoption leads are close to zero, showing no evidence of anticipation. In the quarter of adoption (July - September 2012), hirings in large firms increase relative to small firms by about 3.7 log points. Subsequently, the increment fluctuates between 5.1 (April-June 2013) and 8.6 (July-September 2013) log points.
The estimated effect is, thus, reassuringly not significant before the reform and pops up positive and significant – and pretty stable – right after.

We next investigate the heterogeneous effect of the reform based on specific employee-firm characteristics. Specifically, we investigate heterogeneity along three dimensions: employee age, position, and number of hours worked (full-time/part-time). To this purpose, we estimate equation (2.3), where the variable $D_{s,i,t}$ identifies the sub-group having the specific characteristic under investigation. In detail, $D_{s,i,t}$ takes value 1 if the characteristic s is present in the cell i at time t, otherwise it is equal to zero. All the other variables have the same interpretation as in the baseline model (2.1). The triple interaction term identifies the differential effect of the reform on the relevant sub-group.

Figure 2.2: *Dynamics of the impact of the reform on open-ended contracts*



Note: Temporal pattern of the impact of the reform. Besides the actual policy indicator, we add policy dummies for the two quarters that precede the adoption of reform and for the 6 subsequent quarters. Vertical bands represent ±1.96 times the standard error of each point estimate.

Table 2.5 reports the regression results.

$$\begin{aligned} y_{i,t} \quad = \quad & \alpha_i + \delta_t + \tau \times Policy_t + \beta \times Size_i^{16-19} + \sum_{s=1}^{S-1} \left(\theta_s \times D_{s,i} \right) + \nu \times Policy_t \times Size_i^{16-19} + \\ & + \sum_{s=1}^{S-1} \left(\lambda_s \times D_{s,i} \right) \times Size_i^{16-19} + \sum_{s=1}^{S-1} \left(\mu_s \times D_{s,i} \right) \times Policy_t + \\ & + \sum_{s=1}^{S-1} \left(\gamma_s \times D_{s,i} \right) \times Policy_t \times Size_i^{16-19} + u_{i,t} \end{aligned} \tag{2.3}$$

Column 1 of Table 2.5 shows the differential effect of the reform depending on the age of the newly hired worker: teen workers (< 20 years), twenty-year-old (20-29 years), thirty-year-old (30-39), mature workers (40-54 years), and old workers (> 55 years). Teen workers are the excluded category. Results reveal a negative relationship between the effects of policy and the workers' age. Younger workers seem to to reap the greatest benefits from the *Fornero reform*. This is clearly shown in Figure 2.3 where we plot the triple interaction coefficients by age group, separately for large and small firms. These findings are consistent with Bertola et al. (2007) and Skedinger (2010) which report that the increase of stringency in the employment protection is associated with a higher incidence of involuntary employment among youths. Consistently, our results show that less strict EPL is associated with greater benefits (in terms of hirings on a permanent basis) for younger workers.
Column 2 of Table 2.5 looks at the impact of the policy across different positions, blue-collars (the excluded category), white-collars, and *quadro*. Results shows a larger positive impact of

reform for blue-collars workers. Column 3 shows the heterogeneous effect across full-time and part-time jobs. The coefficient of the triple interaction suggests that the *Fornero reform* has mainly affected full-time jobs.

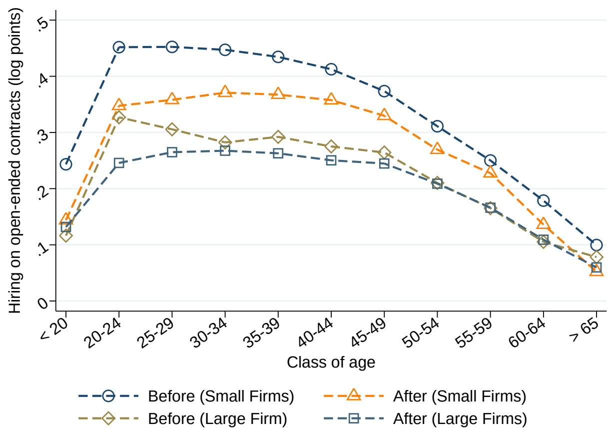
Table 2.5: Heterogeneity of policy effects (open-ended contracts)

HETEROGENEITY DEPENDENT VARIABLE: *Hiring on open-ended contracts*	(1) Employee Age	(2) Job Position	(3) Hours per week
Reform Period (July 2012 - March 2014)	0.029**	0.040**	-0.056***
	(0.014)	(0.018)	(0.010)
Reform × Large Firm	0.121***	0.061***	0.064***
	(0.023)	(0.008)	(0.009)
Reform × Large Firm × 20-29 years	-0.066**		
	(0.023)		
Reform × Large Firm × 30-39 years	-0.057*		
	(0.027)		
Reform × Large Firm × 40-54 years	-0.081**		
	(0.030)		
Reform × Large Firm × 55+ years	-0.083**		
	(0.032)		
Reform × Large Firm × White-Collar		-0.041***	
		(0.008)	
Reform × Large Firm × Quadro		0.004	
		(0.0268)	
Reform × Large Firm × Part Time			-0.027*
			(0.013)
Constant	0.435***	0.435***	0.430***
	(0.0137)	(0.014)	(0.014)
Observations	219,329	219,329	219,329
No. of Cell	56,933	56,933	56,933
R-squared	0.020	0.020	0.022
Cell FE	YES	YES	YES
Month Dummies	YES	YES	YES
Year Dummies	YES	YES	YES

Notes: Significance levels: ***1%, **5%, *10%. Standard errors in parentheses adjusted for clustering at regional level. The sample includes only hirings in firms between 10 and 19 employees. The dependent variable is (the log of) the number of hirings in each cell given by the intersection between province, sector (Ateco2002 classification), worker age, gender, firm size, position (white-collar or blue-collar or quadro), and full-time/part-time). In column (1) the excluded category is the group of teen workers (< 20 years old); in column (2) the excluded category is the group of blue-collar employed. In column (3) the excluded category is the group of full-time workers.

Overall, the results fit the interpretation that the reform favours relative disadvantaged workers, namely young blue collar workers in full-time jobs.

Figure 2.3: Heterogeneity of effects by age



Note: Permanent hirings by firm size. Each dot represents the average number of hirings by age class.

2.6 Robustness checks

In this section we test the robustness of the results presented in Table 2.3.
Our baseline approach controls for macro shocks coinciding with the reform but assumes that these shocks have similar effects across Italian regions, across sectors and across firms of different size. To check the robustness of our findings, we reformulate the model (2.1) adding a region-specific trend, a sector-specific trend and a firm size-specific trend.
The results of the exercise are reported in Table 2.6, where the first three columns (1-3) refer to the specification that includes region-specific trends adjustment and the last three (4-6) to the specification that includes sector-specific trends.

The results, even the magnitude of the coefficients, are very similar to those presented in Table 2.3.

So far the estimations are based on hirings in firms between 10 and 19 employees. In order to verify whether results are robust to this choice, we also provide estimates within the window 2-49. The results of this exercise are in Table 2.7. Again, the coefficient of interest is consistent with the previous results. The effect on all contracts and on permanent contracts (columns 1 and 2) is similar to the effect found in Table 2.3 both in terms of magnitude and significance. Interestingly, we now find a positive and statistically significant impact also on the number conversions into open-ended contracts of temporary and apprenticeship contracts (columns 3 and 4).

We finally check the plausibility of the parallel trend assumption exploiting the fact that *Fornero reform* does not apply to managers. This feature of the reform allows us to set up a

Table 2.6: Robustness check: effect of reform with regional and industrial specific time trend

VARIABLES	(1) *Open ended contracts*	(2) *Conversion from temporary to permanent*	(3) *Conversion from apprentice to permanent*	(4) *Open ended contracts*	(5) *Conversion from temporary to permanent*	(6) *Conversion from apprentice to permanent*
Reform Period	-0.014	0.063***	-0.039***	-0.012	0.063***	-0.037***
	(0.009)	(0.013)	(0.013)	(0.0098)	(0.014)	(0.013)
Reform Period × Large firms	0.034**	-0.016	0.022	0.033**	-0.018	0.020
	(0.014)	(0.015)	(0.025)	(0.014)	(0.015)	(0.025)
Constant	1.644***	4.361***	0.699**	1.793***	4.314***	0.753
	(0.293)	(0.272)	(0.254)	(0.343)	(0.371)	(0.442)
Obs.	219,329	118,966	24,224	219,329	118,966	24,224
No. Cell	56,933	35,766	7,202	56,933	35,766	7,202
R-squared	0.021	0.039	0.011	0.022	0.039	0.010
Region-specific Trend	YES	YES	YES	NO	NO	NO
Region-specific Trend × Large firms	YES	YES	YES	NO	NO	NO
Sector-specific Trend	NO	NO	NO	YES	YES	YES
Sector-specific Trend × Large firms	NO	NO	NO	YES	YES	YES
Cell FE	YES	YES	YES	YES	YES	YES
Month Dummies	YES	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES	YES

Notes: Significance levels: ***1%, **5%, *10%. Standard errors in parentheses adjusted for clustering at regional level. The sample includes only hirings in firms between 10 and 19 employees. The dependent variable is (the log of) the number of hirings in each cell given by the intersection between province, sector (Ateco2002 classification), worker age, gender, firm size, position (white-collar or blue-collar or quadro), and full-time/part-time). We assume linear specific trend.

difference-in-difference-in-difference model (DDD) using managers as a further control group within large firms. Formally, we estimate the following model:

$$\begin{aligned} y_{i,t} \quad = \quad & \alpha_i + \delta_t + \tau \times Policy_{i,t} + \beta \times Size^{16-19}_{i,t} + \theta \times NoManager_{i,t} \; + \\ & + \nu \times Policy_{i,t} \times Size^{16-19}_{i,t} + \lambda \times Size^{16-19}_{i,t} \times NoManager_{i,t} \; + \\ & + \mu \times NoManager_{i,t} \times Policy_{i,t} + \gamma \times Size^{16-19}_{i,t} \times NoManager_{i,t} \times Policy_{i,t} + \\ & + u_{i,t} \end{aligned} \tag{2.4}$$

The parameter of interest is γ that measures the differential effect of the reform on the number of hirings of non managers *vs.* managers in large *vs.* small firms after the reform. Results are in Table 2.8.

The positive coefficients of the DDD estimators are consistent with our findings: less strict EPL induces large firms to increase hirings of non-managers relative to managers in large relative to small firms after the reform. This result shows up in all columns of Table 2.8.
It is also reassuring that the number of hirings in managerial positions does not change in large firms relative to small firms after the reform (as shown by the non significant coefficient of the interaction term "Reform Period × Large Firms" in Table 2.8), given that the *Fornero reform* did not affect manager.

Table 2.7: *Effect of reform on permanent employment: different bandwidths*

VARIABLES	(1) *All type of contracts*	(2) *Open ended contracts*	(3) *Conversion from temporary to permanent*	(4) *Conversion from apprentice to permanent*
Reform Period (July 2012 - March 2014)	0.005	-0.018*	0.065***	-0.049***
	(0.008)	(0.010)	(0.009)	(0.006)
Reform Period × Large firms	0.042***	0.050***	0.021***	0.022***
	(0.006)	(0.008)	(0.004)	(0.007)
Constant	0.540***	0.582***	0.495***	0.330***
	(0.013)	(0.013)	(0.013)	(0.014)
Obs.	1,273,561	776,619	406,710	90,232
No. Cell	269,546	152,393	96,522	20,631
R-squared	0.024	0.026	0.047	0.014
Cell FE	YES	YES	YES	YES
Month Dummies	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES

Notes: Significance levels: ***1%, **5%, *10%. Standard errors in parentheses adjusted for clustering at regional level. The sample includes only hirings in firms between 10 and 19 employees. The dependent variable is (the log of) the number of hirings in each cell given by the intersection between province, sector (Ateco2002 classification), worker age, gender, firm size, position (white-collar or blue-collar or quadro), and full-time/part-time).

2.7 Conclusion

This paper provides new evidence on the impact of employment protection on the worker flows into employment. We use panel data drawn from the Italian Social Security archives to examine the impact on permanent hirings of a reform passed in 2012 (the so-called *Fornero reform*) that reduced dismissal costs for large firms (with more than 15 employees) while leaving EPL unchanged for smaller firms (with less than 15 workers).

We adopt a *difference-in-difference* strategy and find that hirings on open-ended contracts increased in large relative to small firms after the *Fornero reform* by 5.2 percentage points. Moreover, we find that the effect is stronger on full-time, young, blue collar workers. Conversely, we only find mild evidence of positive and significant impact of the reform on the number of conversions of fixed-term and apprenticeship contracts into permanent ones.

Table 2.8: Robustness check: difference-in-difference-in-difference

VARIABLES	(1) *All type of contracts*	(2) *Open ended contracts*	(3) *Conversion from temporary to permanent*
Reform Period (July 2012 - March 2014)	0.047*	0.037	0.054***
	(0.026)	(0.029)	(0.012)
Reform Period × Large Firms	0.013	0.016	-0.037**
	(0.012)	(0.012)	(0.013)
Reform Period × Non-Managers	-0.044*	-0.055**	
	(0.025)	(0.025)	
Reform × Large Firm × Non-Managers	0.026*	0.035**	0.054***
	(0.014)	(0.013)	(0.012)
Constant	0.401***	0.435***	0.371***
	(0.013)	(0.014)	(0.012)
Observations	363,776	220,519	119,033
R-squared	0.019	0.019	0.037
No. of Cell	100,716	57,685	35,829
Cell FE	YES	YES	YES
Month Dummies	YES	YES	YES
Year Dummies	YES	YES	YES

Notes: Significance levels: ***1%, **5%, *10%. Standard errors in parentheses adjusted for clustering at regional level. The sample includes only hirings in firms between 10 and 19 employees. The dependent variable is (the log of) the number of hirings in each cell given by the intersection between province, sector (Ateco2002 classification), worker age, gender, firm size, position (white-collar or blue-collar or quadro), and full-time/part-time).

Chapter 3

Apprenticeship and Older Worker Incentives

3.1 Introduction

Many countries have legislation preventing the dismissal of workers on discriminatory grounds such as gender, race, age, disability or trade union membership. Nevertheless, there are exemptions from employment protection rules for particular group of workers in order to encourage their employment. The most common are for the workers which undertaking training or for the older unemployed workers. Employment protection often affects different groups in different ways influencing the composition of the employed and unemployed on labour market (Bertola et al., 2007; Skedinger, 2010)[1]. For instance, there are several evidences suggesting that labour market prospects for youth people and other marginal groups seem to worsen as consequence of increased stringency in the employment legislation[2].
The increasingly complicated transition from school to work makes the youth still more vulnerable. Here the apprenticeship contract performs a crucial role. Literature shows that youth unemployment rates are lower exactly in the countries where the apprenticeship contract is more popular in the double type of agreement which allows to obtain a qualification and to alternate the study and work programs (Casale et al., 2014). In addition, the training and apprenticeship contracts increase the young workers' probability to move into open-ended contracts than those coming from non-work situation[3].

[1]In a contest of uncertainty concerning the potential employee's productivity, for instance, strict employment protection can damage the more vulnerable group of workers (e.g. immigrants, youth, long-term unemployment, and disable workers) for which their qualification is not easy to verify as those of other groups.

[2]For instance, Skedinger (2010) finds that the increase of stringency of regulations for regular work is associated with a higher incidence of involuntary temporary employment, particularly among the young. Bertola et al. (2007), Skedinger (2010), and Allard and Lindert (2007) confirm the adverse effects on young people and (in many cases) on women ascribable to more stringent employment protection contexts.

[3]Berton et al. (2007) find that Italian workers who enter the labour market on training or apprentice contracts are significantly more likely to move into regular contracts than those coming from non-work situation. After two year 70% of those who began working on a training contract and 29% of apprentices (whose contracts can last up to

At the same time, low employment rates for older workers pushed most OECD countries to experiment specific employment protection in the form of taxes on firing and subsidies on hiring for this group (Chéron et al., 2011)[4]. The aim is to protect older workers from unemployment or to improve their job finding rates considered *ceteris paribus* lower than younger workers. Kugler et al. (2003), for instance, find that the reduction in dismissal costs and in payroll taxes for young and older workers increases the hiring of young workers, with little impact on dismissal rates, leading to a net increase in their probability of employment. In contrast, both hiring and firing rates increased for older men, with no net change in employment [5].
Many types of hiring subsides have been introduced also in Italy since the beginning of the *20th* century. They are typically provided in the form of tax or social security rebates granted to employers recruiting workers belonging to particularity difficult-to-employ category. The bulk of them is made-up of traditional measures, such as training and recruitment incentives.

Reasons behind the introduction of incentives to hiring unemployed workers could derive by the hypothesis that employers are inclined to hire an employee who is already employed before compared to someone who is unemployed, particularly if the legislation is stringent (like in Italy) because it may be potentially more expensive. In a world with adverse selection, firing costs may redistribute new employment opportunities from unemployed to employed workers generating employment discrimination against the unemployed[6].
Mortensen and Pissarides (1999) theorise that there would be two contrasting force at work when a incentives scheme is implemented. On one hand, income support to eligible dismissed employees is likely to increase the reservation wage of the beneficiary worker extending the spell of unemployment. On the other hand, benefits given to the employers who hire older workers are likely to increase their job flow narrowing the spell of unemployment. The sign of the net effect is a priori uncertain and depend on which of the two effect prevails. It must be assessed empirically. Paggiaro and Trivellato (2002) and Rettore et al. (2008)

five years and are likely to still be completing apprenticeship training) have moved into open-ended employment, the majority of them within the same firm. Moreover, working on a training contracts has no significant impact on the likelihood of moving into an open-ended contract in a different firm suggesting that employers value training contracts as a screening device rather than for providing general purpose training which can increase the workers' value to another employer.

[4]These kind of employment protection can consist in penalties for firm that lay off older workers, either in the form of a tax or higher social security contributions (e.g. Austria, Finland, France, and Spain) or in the form of paying part or all of the costs of outplacement services to help workers find new jobs (e.g. Belgium and Korea). For instance, workers with longer-tenure (more likely to be older workers) are often required to be given longer notice period in the case of dismissals and higher severance payments. The hiring of older workers lead to permanent reduction or exonerations in social security contribution in Australia, Belgium, Netherlands, Norway and Spain (Chéron et al., 2011).

[5]The authors assess the *1997 Spanish reform*. The reform also targeted the long-term unemployed who are underrepresented in their occupations and disable workers, but the impact of the reform on employment for these group was not evaluated by the author.

[6]Evidence of this hypothesis comes from by Kugler and Saint-Paul (2004), which indicate that in the United States unemployed individuals are disadvantage in states with stronger employment protection than in states with feeble legislation. To test aforesaid hypothesis, the authors look at the effect of state unjust-dismissal provision on the re-employment in US over the 1980s.

find that additional support to older unemployed workers doesn't necessarily reduce the unemployment period. Moreover, the provision of benefit for a longer period has a negative effect on re-employment probabilities suggesting that it might be advisable to phase out cuts in social security contributions and to reduce the length of time older worker are favoured by the incentives[7].
Notwithstanding the numerous legislative activities, the best practice is yet to be found and there is substantial uncertainty about the best way to proceed (perhaps because of the lack of clear-cut evidence from empirical research). Therefore, the evaluation of these interventions might be a useful tool to design appropriate policies which can avoid large dead-weight losses.

To face the growing youth employment rate and solve the chronic problem in Italy of poor integration between theoretical and practical training, the *law 92/2012* (on July *28th* 2012) reformed the apprenticeship contracts with the aim to favour this kind of employment agreement. The reform put in practice also a set of incentives to hire workers categorised as disadvantaged such the older unemployed workers (i.e. *over-50*). The legislator also tries to face the increase job-insecurity affecting the Italian labour market through incentives to conversions in open-ended contracts for both apprentices and older workers[8]. These measures are in force starting from January 2013.

In this essay, we exploit the changes in Italian apprenticeship regulation and in employer incentives to hire and convert older workers (carried out with the *law 92/2012*) to assess how these labour market policies affect the younger and older possibilities to be hired. We focus on the hiring and the conversion of the Italian labour market.
For the apprenticeships, the *reform 92/2012* induced a change only for the firms with more than 9 employees, while for the smaller firms nothing changed. Similarly, the rebate of employer's social contributions who hires the older workers are in force only for individuals *over-50* years, leaving unchanged the payments referring to workers with less than 50 years. These conditions of applying the reform allow to employ a quasi-experimental approach. We use a *difference-in-difference* approach according to a *regression discontinuity design* by comparing the hiring and conversion in firms with 2-9 employees (i.e. firms just below the threshold of 9 workers) and in firms with 10-15 employees (i.e. firms just above the threshold) before and after the reform (January 2013). Similar reasoning can be applied to assess the effect of employer incentives to hire the workers *over-50*. In this case, the workers with 50

[7] Authors assess the impact of *Mobility Lists Programme* ratified in Italy with the *law 223/1991*. Under this programme, eligible redundant workers are enrolled in mobility lists managed by regional employment agencies and have access to income support and a set of incentives are granted to the employers who hire them. Employers hiring a worker from the lists are entitled both to a temporary social security rebate and to a bonus equal to part of the unemployment benefit still to be paid to the worker. Both the analysis are for Veneto region for the period January 1995 - March 1999. In detail, Rettore et al. (2008) estimate the average casual effect of extending by one year the eligibility to the provision of the *Mobility Lists Programme* finding a tangible effect only for the workers aged 50 years which use the programme as a bridge to retirement.

[8] The *reform 92/2012* increases also the employer's social contributions referring to atypical form of employment contracts in order to discourage their usage.

years represent the threshold while the age classes just below and just above are respectively represented by the workers age 45-49 and 50-54 years[9].
We use the administrative panel data of the Italian Social Security Institute (INPS) for our empirical analysis. The data provide information on the number of newly hired workers by firm size, province, sector, contract type, age and gender at a monthly frequency between 2012 and 2014[10].
Our results show an increase in the hiring on apprenticeships and conversion rates from apprenticeship to open-ended contracts in larger firms (10-15 employees) relative to smaller firms (2-9 employees) after the reform. Results fit to interpretation that the reform favours the younger apprentices (aged 15-24) relative to fewer young (aged 25-29), the recruitments of female apprentices, and the transformation in permanent contracts of male apprentices. The rebate of employer's social contributions who hires or transforms the workers *over-50* positively affects their recruitment into temporary and permanent contracts. Conversely, we don't find effect for the transformations in open-ended contracts. We notice that the INPS's instructions on effective fruition of incentives could have played an important role. The blue-collars seem to benefit of reform more than the other job categories. All the results are robust to the inclusion to regional and industrial specific time trends.

The rest of chapter is organized as follows. Section 3.2 describes the main changes induced by the *reform 92/2012* regarding the apprenticeship contracts and the employer incentives for workers *over-50*. Section 3.3 explain the identification strategy used to evaluate the impact of policy on the recruitments and conversions. Section 3.4 describes the Social Security data. Section 3.5 presents the model. Section 3.6 presents the results, while Section 3.7 shows sensitivity analysis. Finally, Section 3.8 concludes.

3.2 Changes in legislation for youth and older workers

Law 92/2012 represents a systematic reform approved with the aim to introduce a 'good flexibility' as the result of a balanced mix of restricted entry-flexibility and major exit-flexibility. Aside from the loosening in the employment protection regarding the unfair dismissal procedures in open-ended contracts (see Chapter 2), the *reform 92/2012* introduced measures in order to support the hiring of youth, women, and older worker[11]. This section summarizes the changes on the apprenticeships and employer incentives for workers aged

[9] The use of *difference-in-difference* approach in this context is common in the literature. Rettore et al. (2008) is an example in which the authors exploiting within-county variation across groups and over time to examine the effect of labour policy on job reallocation.

[10] The data set is based on the forms through which firms report to the Italian social security institute (INPS) information on the social security contributions paid within the month.

[11] Starting from January 2013 employers could benefit from a 50% reduction of their social contributions for employment contracts regarding women of any age who have been unemployed for the previous 24 months. The same rebate applies to contracts with female workers who have been unemployed for the previous 6 months and live in a disadvantage area.

50 or older after the reform.

Since 2011 the number of reforms has been increasing in order to face the growing young unemployment rate and solve the problem of the lack integration between theoretical and practical training that makes the transition from school to work harder[12]. The *reform 92/2012* looks the apprenticeship contracts as a privileged channel for helping young people to enter in the labour market (also) in the light of the elimination of access-to-work contracts. However, these agreements seem to be far from being the main first-contract for young[13].
The *reform 92/2012* introduced changes with reference to duration, maximum number of appendices per skilled employer and confirmation of the apprenticeship contracts.
I) The law sets a minimum duration of the contract (at least 6 months), except for seasonal activity, while the maximum duration for each agreement remains unchanged at 5 years.
II) Starting from January 2013, law provides that it is possible to hire a new apprentices only on condition that at least 30% of the apprenticeship contracts signed in the previous 36 months were confirmed into open-ended contracts. The percentage passes to 50% after the 2015. The mandatory transformation is in force only for the firms with at least 10 employees. Firms with less than 10 employees don't have commitments.
III) Innovation regards also the increase of number of apprentices recruited for each skilled worker in employment. Starting form January 2013, the firms with at least 10 employees cannot exceed the proportion of 3 to 2, compared with the skilled workers number. Therefore, the firms with more than 9 employees can recruit at most 3 apprentices for each 2 qualified workers. Conversely, the *reform 92/2012* doesn't change the ratio for the firms with less than 10 employees, which remains 1 to 1 as before the policy.
IV) The legislator have also envisaged an increase of employers' social contributions upon all businesses which hire a apprentice (regardless the firm size). The rise, equal to 1.31% plus 0.30%, has the aim to finance, respectively, the reform of social security cushion (so called ASpI, namely, *Assicurazione Sociale per l'Impiego*) and the institution of a training fund (so called *Fondo Interprofessionali*)[14].

[12]In 2006, the *law 296/2006* established reductions of social contributions for the recruitment of young people through an apprenticeship contract. In 2011 the *legislative decree 167/2011* reorganised the apprenticeship contract describing it as an open-ended contract aimed to training and employment youth through: *i)* apprenticeship for qualification for young people aged 15 and 25; *ii)* professional training apprenticeship for young people aged 18 and 29; *iii)* high training and research apprenticeship for young people aged 18 and 29.

[13]Nearly one year after the enforcement of legislation regulating apprenticeship the devising of system (different among productive sectors and regions) has not yet been fully envisaged. Moreover, one of its crucial aspects - i.e. the training content - still remains unsolved (Tiraboschi, 2012).

[14]Summarize the scheme of social contributions in Italy is not an easier challenge. The rule of reference on the apprenticeship contracts was the *law 296/2003*. It established an employer's social contribution depending by firm size. In businesses with more than 9 workers there was only one contribution rate equal to 10%, while in businesses with less than 10 workers there was a progressive rate based on the duration of contracts (equal to 1.5% for the first year, 3% for the second year, and 10% for the following years). The social contribution upon the employee (apprentice) was equal to 5.84%. In November 2011, the *law 183/2011* (*Legge di Stabilità 2012*) introduced a three-years total rebate of employer's social security contributions for each apprenticeship contract signed into enterprises with less than 10 employees, starting from January 2012 and up to December 2016. Conversely, the *law 183/2011* left unchanged the employer's social contribution for firm with more than 10 employees (i.e. 10%). On this framework, the *reform 92/2012* established the aforesaid increase equal to 1.61% (1.31% + 0.30%) for all firms. After the *reform 92/2012*, the employer's social contributions in the first year of contract in firms with less than 10 workers is equal to

The *reform 92/2012* put in practice also a set of employer incentives to hire workers categorised as vulnerable or disadvantaged (e.g. women living in disadvantaged areas, long-term unemployed, and worker *over-50*). In detail, the law established a rebate of employer social contributions which hires (with fixed-term or open-ended contracts) worker *over-50* unemployed in the previous 12 months (*co. 8, art. 4, law 92/2012*). Subsidized contributions are also applied to the transformations of fixed-term into open-ended contracts (*co. 9, art. 4, law 92/2012*)[15]. The incentives are in force for all private firms and for all the type of workers aged 50 or older (managers, white collars, and blue collars), while they doesn't applies on the public administrations.
We stress the fact that the employer incentives for older workers envisaged by the *reform 92/2012* are *de facto* a new incentives scheme. They are not a replacing of old facilitations[16]. The same applies regarding the regulation on the apprenticeship contracts.
In order to clarify the changes induced by *reform 92/2012*, Table 3.1 summarizes how the policy has reformed the set of rules before in force.

Table 3.1: *Reform's intervention: changes in apprenticeships and for older workers*

	Actions	*Individuals*	*Before*	*After*
APPRENTICE	Min. duration for the apprenticeship contracts.	All firms	No restriction	6 months
	Min. percentage of apprentices to be transformed into open-ended contracts.	Firms > 9 workers[a]	No restriction	50%[b]
	New ratio "apprentices *vs* skilled workers".	Firms > 9 workers[c]	3:2	1:1
	Rise of employer's social contributions.	All firms	—	+ 1.61%
OVER 50	Rebate of employer's social contributions[d].	Over-50	10%	50%

Notes: [a]For the firms with 9 or less employees the policy doesn't envisage any restriction; [b]Starting from January 2013 the percentage is equal to 30%, while starting form January 2016 the percentage passes to 50%; [c]For the firms with 9 or less employees the ratio remains to 1:1 as before the policy; [d]The cut is valid for 18 months whether there is either a new hiring of older workers (over-50) into open-ended contract or a conversion of older workers into open-ended contract. Conversely, the rebate is valid for 12 months in the event of the new hiring into temporary contract of older worker.

We exploit the sharp discontinuity in the regulation of apprenticeship contracts in force

1.61% (i.e. 1.31% + 0.30%) for the apprentices hired between January 2012 and December 2016 or equal to 3.11% (i.e. 1.5% + 1.31% + 0.30%) for the apprentices hired before the 2012. Whereas, in firms with more than 9 workers the employer's social contribution is equal to 11.61% (i.e. 10% + 1.31% + 0.30%). The cut of social contribution remains even if the firms cross the threshold of 9 workers.

[15]The duration of benefit varies with the type of arrangements. For the new hiring on fixed-term contract of older workers *(over-50)* unemployed from the least 12 months, the *reform 92/2012* established a cut equal to 50% of employer's social contributions for 12 months. Whereas, for conversion from temporary to permanent contract or for a new hiring on open-ended contract, the law extended the above incentives (i.e. 50%) until to 18 months.

[16]Note that, until December 2012 the only facilitation in force for older workers was the *law 191/2009* (*art. 2*) which envisaged a cut of employer's social contribution equal to 10%.

after the *law 92/2012*, namely the change in the ratio 'apprentice on skilled workers' and the mandatory conversion from apprenticeships into open-ended contracts, to assess the impact of reform. Indeed, it acts only upon the firms with more than 9 workers leaving the firms with less than 10 workers unaffected. All other interventions did not affect differentially firms above and below the 9-employee threshold.
Similarly, to assess the impact of employer incentives for workers *over-50*, we exploit the discontinuity in the employer's social contributions in force after the *reform 92/2012* referring to threshold of 50 years old.

3.3 Empirical Strategy

To assess the effect of *reform 92/2012* on apprenticeship contracts and on the hiring of older workers, we set up a standard *difference-in-difference* model in a *regression discontinuity* design. The model will be estimated with the *ordinary least squares* (OLS) method[17].
Regression discontinuity analysis is a rigorous non-experimental approach that can be used to estimate program impacts in situations in which there is a discontinuity in the probability of treatment receipt (at *cut-point*). Underlying assumption is that in the absence of treatment the relationship between outcomes and rating variable passes continuously through the *cut-point*. Conversely, in presence of treatment we expect a sharp upward (downward) jump in the relationship at the *cut-point* determining a discontinuity. The direction and magnitude of the jump is a direct measure of the causal effect of the treatment on the outcome for candidates near the *cut-point*. The premise underlying the *regression discontinuity* approach is that differences between candidates who are just below and just above the threshold are random. Any difference in subsequent mean outcomes observed between candidates either subject or not to treatment would be caused by treatment.
Since *regression discontinuity* approach is a non-experimental method, it must meet a set of conditions to provide unbiased impact estimates. First, the rating variable cannot be caused or influenced by the treatment. Second, the *cut-point* is determined independently of the rating variable (i.e. it is exogenous), and assignment is entirely based on candidate's rating and on threshold. Third, nothing other than the treatment status should be discontinuous in the analysis.
We choose to follow a *local strategy*. It views the estimation of treatment effect as local randomization and limits the analysis to the observations lying within the closely vicinity of the *cut-point* (called bandwidth). The method involves to choose a small neighbourhood to the left and right of *cut-point* and using data within that range to estimate the discontinuity in outcomes across the threshold. The jump observed at the *cut-point* will be an approximation of treatment effect[18].

[17]The regression discontinuity design was first introduced in the evaluation literature by Thistlethwaite and Campbell (1960). See also Imbens and Wooldridge (2008) for a practical and theoretical issues. For an example see Lee (2008) which studies the effect of party affiliation of a congressman on congressional voting outcome.
[18]Local strategy is counterpoised to *parametric global strategy*, in which we use every observations in the sample to model the outcome as a function of rating variable and treatment status. Since the global approach uses all available

In this essay, we want to assess the impact of *reform 92/2012* on the hiring and conversion rates of apprentices and workers with more than 50 years. As said, reform of apprenticeships affects only the firms with more than 9 employees, while the change in the hiring incentives for older workers is in force only for individuals with more than 50 years. In both cases, the *outcome variable* is represented by the frequencies of hiring (or conversion) disaggregated according to firm and worker's homogeneous characteristics. The *rating variables* are the firm size for the apprentices and the age for workers.
In the apprenticeship evaluation the *cut-point* is given by the firms with 9 employees (dimension above which there is a sharp discontinuity in the Italian rules on the apprentices). The bandwidth includes the classes of firms which employ just more or just less 9 workers (precisely from 2 to 15 employees). At same way, the *cut-point* for the hiring incentives for older workers is given by individuals with 50 years (above which the rebate of employer's social contributions is in force). The bandwidth is the age classes just above and just below the threshold of 50 years (i.e. 45-54).
The logic behind these approaches is straightforward. In the case of apprenticeship, it's reasonable to think that firms with employees surrounding the 9 workers have roughly the same characteristics (i.e. similar warehouses, equipments, company structure, production organization, etc.). Although they have numerous common characteristics, these businesses radically differ regarding apprenticeships regulation, namely for the apprentices that can be hired for each skilled worker (3:2 *vs* 1:1) and for the minimum number of apprentices to be stabilized. The net discontinuity at 9 employees allows to firms placed just below the threshold to be exempted by the *law 92/2012* compared to the firms placed just above. This should represent the only relevant difference among these businesses.
The changes arisen from the comparison between hiring and conversion trends of apprentices in larger firms relative to smaller firms, before and after the reform, should reflect the policy's effects on the apprenticeship contracts. Underlying this logic there is the key assumption of *common trend* between treated and control groups (see Angrist and Pischke 2009). It implies that in absence of treatment (*reform 92/2012*) both the classes of businesses (larger and smaller) would have expected the same trends.
Further key assumption says that the group effect does not change over time, that is the group composition, on average, is constant.
Aforesaid logic can be extend also at the case of the hiring and conversion incentives for the workers with more than 50 years. However, the identification of impact on the hiring and conversion rates of older workers lacks of a proper identification of group made up of eligible individuals. Although the *regression discontinuity design* suggests to consider the workers just below the threshold of 50 years like counterfactual group and those just above the threshold

data in the estimation of treatment effect, it can potentially offer grater precision than the *local approach*. However, is often difficult to ensure that the functional form of the relationship between the conditional mean of outcome and the rating variable is correctly specified over large range of data, increasing the potential bias. The *local strategy* substantially reduces the *chances* that bias will be introduced by using a mush smaller portion of data, but it can have more limited statistical power due to the smaller sample size used in the analysis.

like treated group, we should keep in mind that in the treated group (i.e. workers aged 50-54 years) there are some individuals which are actually not eligible for the treatment, namely the older individuals unemployed for a period lesser than the 12 months. With our data, we are not able to exclude such individuals from the treated group. Even if this issue doesn't allow the precise identification of the reform's effect, we can equally interpret results like a weakened treatment effect since the treated group contains *de facto* also individuals untreated. The effect arisen by the estimation would be interpreted like the lower bound of the actual effect of reform. Therefore, we cannot infer about the magnitude of impact but we can assess the direction of policy's impact.

3.4 Data and descriptive statistics

The empirical analysis uses administrative panel data of the Italian Social Security Institute (INPS) for years 2012-2014, which is grounded on the monthly UniEMens communications. The dataset includes informations on hiring and conversions recorded in the public and private Italian businesses.
Since the INPS collects informations for the purpose of computing retirement benefits, whence derive the contributions charged by workers and employers, this data is very reliable.
They are not micro data employee-employer. The statistical unit (cell) observed in the dataset represents the number of workers employed in businesses disaggregated according to time (month-year), and to employee-firm characteristics. In detail, the cell identifies the province (about 1000), sectors (based on the *Ateco 2002* 2-digit classification), firm size categories (measured in terms employment), type of contract (i.e. apprenticeships, fixed-term, and open-ended contract), position (i.e. full time, part time), gender, and age categories. The statistical unit is present in the data only if there has been at least one hiring or conversion in that given cell. Table 3.2 collects the employee-firm characteristics available in the data. Of course, the cell should be designed in accordance with the specific dynamics under investigation, in our case: apprenticeship contracts and hiring of older workers.

Since the *reform 92/2012* affects only the firms and employees belong to private sector, we drop public sectors hirings and conversions. We exclude hirings and conversions by Italian firms located abroad and the agricultural sector for which the relevant thresholds are different. Finally, we focus on the period between January 2012 and March 2014, because in March 2014 a new labour market reform, the *Poletti's Decree*, was approved.

According to *discontinuity design* implemented through a *local strategy* (see Section 3.3) we focus, in case of apprenticeships, on the observations referring to the apprentices (hired or transformed) aged 16-29 years recorded in firms placed just above and just below the *cut-point* of the 9 employees. In detail, we focus on firms with 2-9 workers (control group) and firms with 10-15 workers (treated group), namely the two size classes surrounding the threshold of the 9 employees.

Table 3.2: Employee-firm characteristics

Employee - Firm Characteristics	
Type of contracts	Hiring on open-ended contracts; Hiring on fixed-term contracts; Hiring on apprenticeship; Conversion in open-ended contract from fixed-term agreement; Conversion in open-ended contract from apprenticeship.
Employee age (classes)	Less than 20 years; 20-24 years; 25-29 years; 30-34 years; 35-39 years; 40-44 years; 45-49 years; 50-54 years; 55-59 years; 60-64 years; More than 65 years.
Employee gender	Man; Woman.
Employee position	Blue collar; White collar; Quadro; Apprentice; Manager.
Hours per week	Full-time; Vertical part-time; Horizontal part-time; Mixed part-time.
Industrial sector[a]	Agriculture, hunting and forestry (A); Fishing, fish farming and related services (B); Mineral processing (C); Manufacturing (D); Production and distribution of electricity, gas and water (E); Buildings (F); Wholesale and retail trade, and repair of durable goods (G); Hotels and restaurants (H); Transport, storage and communication (I); Financial assets (J); Real estate, renting, information technology, research, and business services (K); Public administration (L); Public Education (M); Public Health and social work (N); Other public, social and personal services (O); Activities of households (P); Extraterritorial organizations and bodies (Q).
Firm size (classes)	1 worker; 2-9 workers; 10-15 workers; 16-19 workers; 20-49 workers; 50-99 workers; 100-199 workers; 200-499 workers; 500-999 workers; More than 1000 workers.
Geographic zone	North-West; North-Est; Center; South; Islands; Abroad.
Region	Italian regions.
Province	Italian provinces.

Note: [a]The industrial sector are identified according to *Ateco 2002* classification which provides a classification of the Italian production units of goods and services developed by the Italian Statistical Institute (ISTAT).

We follow the same approach to assess the impact of *reform 92/2012* on hiring and conversions of older workers. In this case, the sample is composed by the two age classes of workers just above and just below the threshold of 50 years, namely the workers aged 45-49 years and ones aged 50-54 years.
The final dataset includes, in the case of apprenticeships, 158,115 observations between January 2012 and March 2014, where 115,936 of them concern the smaller firms (unaffected by the policy) while 42,179 concern the larger firms (subject to policy). To assess the impact of incentives to hire or convert older worker we have 775,350 observations, where 439,336 refer to individuals aged 45-49 years (unaffected by the policy) while 336,014 refer to individuals aged 50-54 years.

Table 3.3 and 3.4 present descriptive statistics separately for firm size (i.e. *larger* and *smaller*) and for individual age (*under-* and *over-50*) before and after the policy (January 2013). The double differences between the mean of two group (treated and untreated) across the two period (before and after the policy) provides a first raw estimate of the impact of reform on hiring and conversions of apprentices and older workers. Apprentices' recruitments decrease in the post-reform period in both classes of firms but less in the firms with more

than 9 employees relative to firms with less than 9 employees. The double differences (equal to 0.052 log points; Panel B of Table 3.3) suggests that the *reform 92/2012* might have increased the hiring of apprentices in the larger firms (subjected to the policy) relative to smaller firms (unaffected). Although with lesser magnitude, the apprentices' transformations into open-ended contracts seem to follow the same pattern (0.029 log points; Panel C of Table 3.3).
The differences between *over-50* and *under-50*, before and after the policy, are smaller than those observed for apprentices (see Table 3.4). The double differences for older workers in Table 3.4 suggest that the *reform 92/2012* positively affected the conversions (0.005 log points; Panel B), the hiring into fixed-term contracts (0.009 log points; Panel C), and the hiring into open-ended contracts (0.011 log points; Panel D).
Though the simple comparison between means suggests that the *reform 92/2012* may have positively affected the employments and conversions of apprentices and workers aged 50 years or older, (part of) these changes could be ascribed to (un)observed heterogeneity. In order to account for it, Section 3.5 presents conditional estimates.

As last description of data, we provide a visual impression of the number of hirings (and conversions) in treated group (i.e. firms with 10-15 employees or workers aged 50-54 years) and control group (firms with 2-9 employees or workers aged 45-49 years) between January 2012 and October 2014 (Figure 3.1 and 3.2). Figures help understanding the identification strategy.

3.5 The regression model

In order to infer about the treatment effect on apprentices and older workers induced by the *reform 92/2012*, we set up a standard *difference-in-difference* model analysing the only observations lying within the close vicinity of *cut-point*. Equation (3.1) formalizes our identification strategy. The estimation method is the *ordinary least squares* (OLS). The index i identifies a cell defined by the intersection between firm size × province × 2-digit sector × contract type × age × gender. The subscript t denotes time at the monthly frequency.

$$y_{i,t} = \alpha_i + \delta_t + \tau \times Policy_{i,t} + \beta \times D_{i,t} + \gamma \times Policy_{i,t} \times D_{i,t} + u_{i,t} \tag{3.1}$$

Where:

$y_{i,t}$	is the logarithm of hiring (conversion) measured for the unit i at time t;
α_i	is the cell fixed effect;
δ_t	is the time dummy;
$Policy_{i,t}$	is the policy indicator, whose value is equal to 1 when the *reform 92/2012* was in force (otherwise zero);
$D_{i,t}$	is a dummy variable which identifies the treated group[19].

[19]In the apprenticeship's evaluation, $D_{i,t}$ represents the firm size indicator, whose value is equal to 1 for the firms

Table 3.3: *Apprenticeships: non-parametrics impact of reform (mean differences)*

(A) *All type of apprentices*	After policy	Before policy	Difference
Larger firms (*obs.*)	21,798	20,368	
mean	0.295	0.321	- 0.026
sd.	0.521	0.541	
Smaller firms (*obs.*)	61,295	54,602	
mean	0.514	0.585	- 0.071
sd.	0.697	0.747	
Difference	- 0.219	- 0.264	**0.045**
(B) *Apprenticeships*	After policy	Before policy	Difference
Larger firms (*obs.*)	14,566	13,775	
mean	0.351	0.379	- 0.028
sd.	0.567	0.586	
Smaller firms (*obs.*)	40,301	36,231	
mean	0.610	0.690	- 0.080
sd.	0.747	0.795	
Difference	- 0.259	- 0.311	**0.052**
(C) *Apprentice transformed*	After policy	Before policy	Difference
Larger firms (*obs.*)	7,232	6,593	
mean	0.183	0.201	- 0.018
sd.	0.392	0.407	
Smaller firms (*obs.*)	20,994	18,371	
mean	0.331	0.378	- 0.047
sd.	0.542	0.591	
Difference	- 0.148	- 0.177	**0.029**

Notes: We define larger the firm with more than 9 workers (class 10-15) and smaller the firm with less than 10 workers (class 2-9). Before and after policy refers to the approval date of *reform 92/2012* (January 2013). The values in table represents mean, standard deviation, and observations of natural logarithm of hiring (or conversion) frequencies refer to larger (smaller) firms and before (after) policy approval. The differences are computed on mean values. Numbers in bold represents the (non-parametric) *difference-in-difference* values.

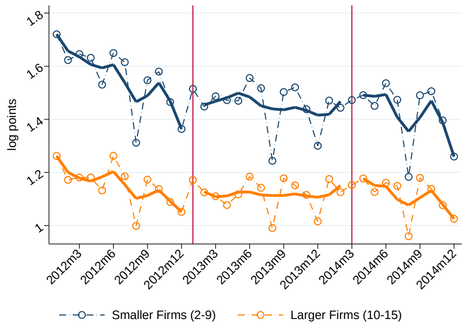
Table 3.4: *Over-50: non-parametrics impact of reform (mean differences)*

(A) *All type of apprentices*	AFTER POLICY	BEFORE POLICY	DIFFERENCE
OVER-50 (*obs.*)	180,966	150,473	
mean	0.421	0.429	- 0.008
sd.	0.690	0.693	
UNDER-50 (*obs.*)	233,495	198,968	
mean	0.473	0.488	- 0.015
sd.	0.738	0.744	
DIFFERENCE	- 0.052	- 0.059	**0.007**
(B) *Conversions open-ended contracts*	AFTER POLICY	BEFORE POLICY	DIFFERENCE
OVER-50 (*obs.*)	29,375	25,708	
mean	0.217	0.231	- 0.014
sd.	0.448	0.461	
UNDER-50 (*obs.*)	39,300	35,595	
mean	0.253	0.272	- 0.019
sd.	0.488	0.508	
DIFFERENCE	- 0.036	- 0.041	**0.005**
(C) *Fixed-term contracts*	AFTER POLICY	BEFORE POLICY	DIFFERENCE
OVER-50 (*obs.*)	90,527	75,563	
mean	0.514	0.530	- 0.016
sd.	0.755	0.763	
UNDER-50 (*obs.*)	115,594	97,687	
mean	0.578	0.603	- 0.025
sd.	0.815	0.823	
DIFFERENCE	- 0.064	- 0.073	**0.009**
(D) *Open-ended contracts*	AFTER POLICY	BEFORE POLICY	DIFFERENCE
OVER-50 (*obs.*)	61,064	49,202	
mean	0.380	0.376	- 0.004
sd.	0.660	0.651	
UNDER-50 (*obs.*)	78,601	65,686	
mean	0.427	0.434	- 0.007
sd.	0.691	0.696	
DIFFERENCE	- 0.047	- 0.058	**0.011**

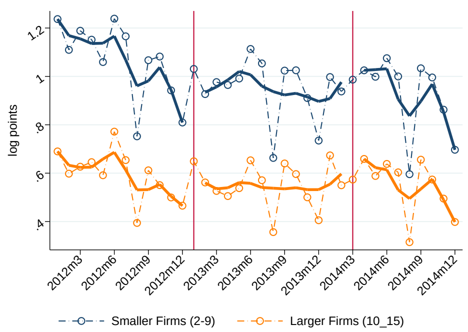
Notes: We define over-50 the individuals aged 50-54 years and under-50 the individuals aged 45-49 years. Before and after policy refers to the approval date of *reform 92/2012* (January 2013). The values in table represents mean, standard deviation, and observations of natural logarithm of hiring (or conversion) frequencies refer to larger (smaller) firms and before (after) policy approval. The differences are computed on mean values. Numbers in bold represents the (non-parametric) *difference-in-difference* values.

Figure 3.1: Hiring and conversions of apprentices in smaller firms relative to larger firms.

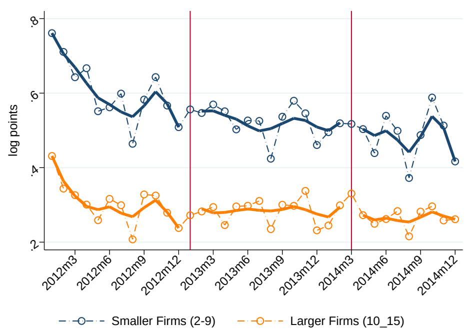
(a) Apprentices & Transformed Apprentices.



(b) Hiring of apprentices.



(c) Transformation in open-ended contracts.

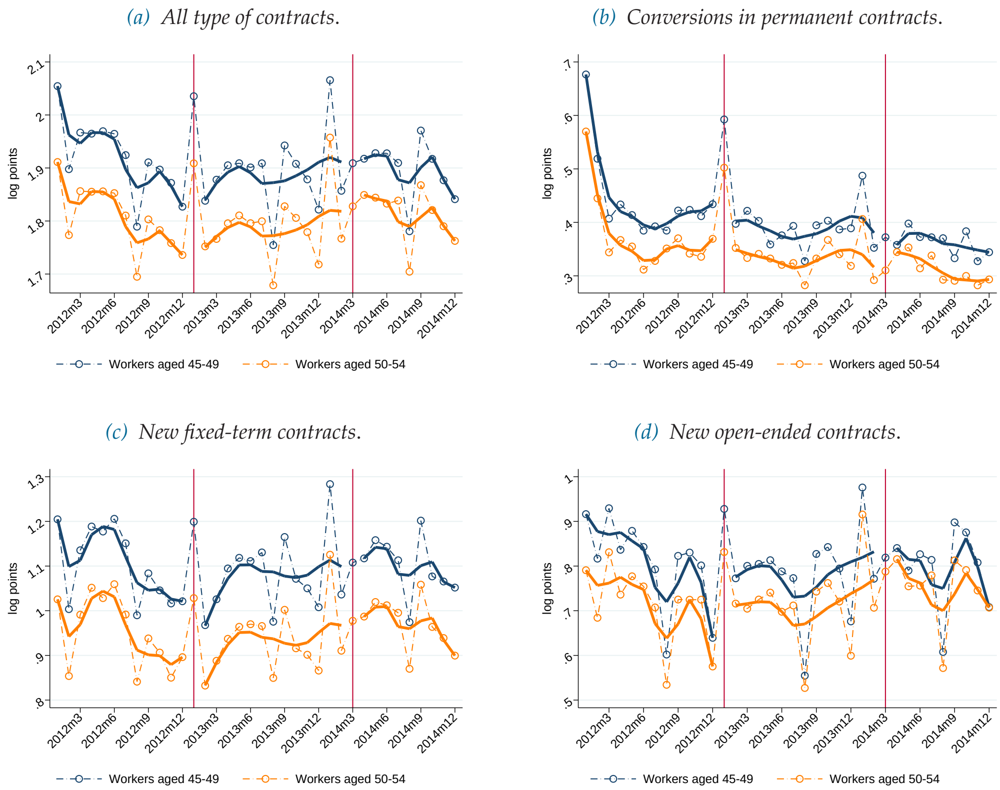


Note: Graphs report the hiring and conversions regarding the apprentices recorded in larger (10-15) and smaller (2-9) firms. Each dot represents the average of natural logarithm of frequencies reported at cell level, distinguished for firm-size (smaller and larger) and type of contracts. The cell is built taking into account employee-firm characteristics such as province, industrial sector, gender of employee, age, placement, and hours per week. Graph (b) represents the employments of apprentices, while graph (c) represent the apprentices transformed into open-ended contract. The first vertical (red) line represents the date in which the apprentices' reform is in force (January 2013), while the second one represents the approval of *Poletti's Decree* (March 2014).

The coefficient of interaction term (γ) between firm (or worker) indicator (i.e. firms with 10-15 employees or workers aged 50-54 years) and the policy indicator (i.e. January 2013 - March 2014) represents the *average treatment on the treated* (ATT). In the apprenticeships' evaluation, it identifies the differential change in hirings (or conversions) of apprentices in larger firms relative to small firms after the reform. Similarly, it identifies the differential change in hirings (or conversion) of workers aged 50-54 years (treated) relative to workers aged 45-49 years (untreated).
The inclusion of cell fixed effect accounts for unobserved time-invariant specific characteristics at the cell level, while time effect control for common macro economic shocks at the

belong to size class with 10-15 employees, otherwise zero. Whereas, in the analysis of older workers, $D_{i,t}$ represents the age indicator, whose value is 1 for the workers aged 50-54 years, otherwise zero. Remember that the sample has been narrowed to the only classes of firms (or workers) adjacent to *cut-point*.

Figure 3.2: Hiring and conversions of over-50 relative to under-50



Note: Graphs report the hiring and conversions regarding the workers aged 50-54 years relative to workers aged 45-49 years. Each dot represents the average of natural logarithm of frequencies reported at cell level, distinguished for age class of workers (*over-50* and *under-50*) and type of contracts. The cell is built taking into account employee-firm characteristics such as province, industrial sector, gender of employee, firm size, placement, and hours per week. Graph (b) represents the conversions in open-ended contracts, while graphs (c) and (d) represent the hiring, respectively, into fixed-term contracts (c) and open-ended contracts (d). The first vertical (red) line represents the date in which the incentives on older workers are in force (January 2013), while the second one represents the approval of *Poletti's Decree* (March 2014).

monthly frequency.
We also estimate an alternative version of model (3.1) which includes sector and region time trend (see Equation 3.2). This specification aims at capturing region- or sector-specific time varying factors that may affect hirings (or conversions).

Since policy indicator is equal to string of zero followed by a string of 1 for the individuals affected by the reform's changes it is highly serially correlated. The error term is correlated over time for a given cell if the model systematically overpredicts or underpredicts the hiring (or conversion) frequencies recorded in the cell. Hence, the default OLS standard errors are likely to be biased. To address this matter, we use *cluster-robust standard errors* at Italian region level (see Bertrand et al., 2004 and Cameron and Miller, 2013). In this way, standard errors are robust to serial correlation, within-region correlation, and to heteroskedasticity.

Regression discontinuity design can be considered as a strict tool for estimating the causal effect if at *cut-point* there exists a clear discontinuity in the probability of receiving the treatment. Furthermore, the rating variable (i.e. firm size or workers age) and *cut-point* should be determined independently of each other. If not so, the validity of approach is called into question. In our application, the discontinuity condition is easily satisfied since the changes in the apprenticeship regulation are in force only for the firms with more than 9 employees. Likewise, the incentives for older workers are in force only for the individuals aged 50 years or older. Also the exogeneity condition of *cut-point* seems to be reasonable. On the one hand, the businesses cannot influence the setting of the threshold (9 employees) and, on the other hand, it's hard to believe that the set of rules in force on apprenticeships is able to manipulate the choice of firms regarding their size growth[20]. Referring to incentives to hire or convert workers aged 50 or older it's clear the exogeneity of the corresponding *cut-point*, namely the age of individuals.

3.6 Results

Table 3.5 shows the OLS estimates of Equation (3.1). The dependent variable represents the (log of) hirings or conversions recorded in each month and within each cell between January 2012 and March 2014. The columns 1-3 refer to apprentices, while the columns 4-7 refer to older workers.

Our results show that *reform 92/2012* positively affects the hiring and conversions of apprentices in firms just above the threshold of 9 workers (i.e. with 10-15 employees) relative to firms just below (i.e. with 2-9 employees) after the approval of policy (column 2 and 3, Table

[20]Studying the effect of employment protection on the size choices of Italian firms, Garibaldi et al. (2004) find that the small firms close to the 15 employees (relevant dimension to apply strict EPL) are characterized by an increase in inaction and by a reluctance to grow. In this sense, employment protection contingent to the firm size affects the businesses behaviour. Conversely, we believe that the regulation on apprenticeships in force in Italy (in terms of cost-benefit) could hardly affect the firm's behaviour about the choice on their size.

Table 3.5: Effect of reform on apprentices and on older workers

	Apprenticeship (1-3)			Older workers (4-7)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	*All apprentices*	*Hiring apprentices*	*Transformed apprentices*	*All type contracts*	*Open-ended conversions*	*Fixed-term contracts*	*Open-ended contracts*
Reform Period	-0.104***	-0.122***	-0.113***	-0.0260***	-0.044***	-0.137***	-0.038***
	(0.007)	(0.009)	(0.012)	(0.006)	(0.004)	(0.008)	(0.008)
Reform × Larger firms	0.058***	0.069***	0.038***				
	(0.007)	(0.009)	(0.008)				
Reform × Over-50				0.021***	0.013***	0.028***	0.015***
				(0.002)	(0.004)	(0.004)	(0.004)
Constant	0.598***	0.706***	0.383***	0.577***	0.443***	0.649***	0.521***
	(0.010)	(0.011)	(0.015)	(0.015)	(0.014)	(0.015)	(0.014)
Obs.	158,115	104,873	53,242	775,350	129,978	389,087	256,285
No. Cell	24,208	14,038	10,170	162,371	35,972	65,348	61,051
R-squared	0.036	0.049	0.017	0.020	0.037	0.028	0.020
Cell FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. In the apprenticeships estimation (columns 1-3) the analysis refers to the only apprentices (recruited or transformed) reported in firms with 2-9 employees (defined like smaller) and in firms with 10-15 employees (defined like larger). We also keep out the manager. Instead, in the older workers estimation (columns 4-7) we refers to the individuals aged 45-54 years (distinguished in two class: *over-50* '50-54' and *under-50* '45-49') hired into open-ended or fixed-term contracts or converted into open-ended contracts. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (Ateco2002 classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time).

3.5). After the reform, the larger firms employ apprentices roughly the 7.1% (0.069 log points) more than the smaller firms. They also convert apprentices in open-ended contracts roughly the 3.9% (0.038 log points) more than the smaller firms. The results are consistent with the idea according which, in an information asymmetry context, incentives to conversion satisfy the preference of employers to sign a permanent contracts with workers that have already been screened serving to stepping-stone towards open-ended contracts[21]. Overall, the results suggest that the *reform 92/2012* positively affects apprenticeship contracts.

Referring to the incentives on the older workers established with the *reform 92/2012*, the results show a widespread positive effect of policy on workers aged 50-54 years relative to workers aged 45-49 years. According our results, the greater hiring of workers *over-50* compared to *under-50* is equal to 1.5% (0.015 log points) for the open-ended contracts and 2,8% (0.028 log points) for the fixed-term contracts (columns 6 and 7 of Table 3.5). Also the conversions in open-ended contracts are greater (+1.3%) for workers aged 50-54 years relative to workers aged 45-49 years after the reform (column 6 of Table 3.5). Anyway, the effects of these incentives seem to be more lightweight than ones linked to the apprenticeship reform.

[21]The effectiveness of the conversion incentives is appreciable also relative to other incentive schemes. Cipollone and Guelfi (2003), for instance, examining the effects of a generous tax credit in Italy to firms choosing to hire workers under open-ended rather than fixed-term contracts find that most of the financial support was wasted as dead-weight loss. The reason is because firms mainly used this tax credit provision to hire under open-ended contracts workers who, on average, turn out to have the highest probability of being permanently hired even without the subsides, perhaps after a short transition into temporary employment.

Although the *reform 92/2012* has interested several aspect of employment relationship, our empirical approach is able to well capture the effect of changes on apprenticeship regulation and incentives program on older workers. Indeed, all the other contemporaneous changes referring to the reform don't differently affect the firms (or workers) surrounding the *cut-point* of 9 employees (or 50 years old). Furthermore, the high number of observations (158,115 for the apprentices and 775,350 for older workers) reassures about the capability of local linear regression approach to well approximate the relationship between outcome and rating variable.
However, the estimation on older workers deserves some considerations. First, as we said in Section 3.3, among the workers aged 50-54 years there are actually untreated individuals (since the rebate of employer's social contributions is in force for the only people unemployed from at least 12 months). Hence, we should interpret the estimations in Table 3.5 (columns 4-7) like a lower bound of actual impact of reform.
Second, as said in the Section 3.2, the *reform 92/2012* envisages several incentives to hire workers categorised as vulnerable or disadvantaged. Therefore, there might be circumstances in which the same individuals are affected to different incentives scheme ascribable to *reform 92/2012*. For instance, an employer who hired or converted into open-ended contracts a female unemployed worker aged 50-54 years is entitled to the cut of social contributions, according to regulatory setting in force. However, also an employer who hired or converted a female unemployed aged 45-49 years is entitled to the same rebate because the incentive program for the unemployed women is in force regardless the worker's age. In other words, there could be some individuals (i.e. unemployed women) lacking of discontinuity in the rating variable (i.e. age) fundamental condition for obtaining a consistent estimation of treatment effect[22].
In order to verify whether our results (in Table 3.5) are strayed due to this issue, we repeat the estimation of model (3.1) narrowing the sample at the only male workers aged between 45 and 54 years. The results of this exercise are showed in Table 3.6.
Although the female workers aged 45-54 years represents roughly the 40% of sample they don't falsify our results. The findings in Table 3.6 seem to be consistent in the sign and magnitude with the results showed in Table 3.5.
Our results are in step with Paggiaro and Trivellato (2002) and Rettore et al. (2008), according which a proper incentives scheme for older workers should provide for an phase out cuts for limited time length. Coherently, the *reform 92/2012* envisages that the rebate of employer's social contributions varies between 12 to 18 months.

3.7 Sensitivity Analisis

In this part we test the results showed in Section 3.6 through a set of sensitivity analysis. We also verify whether there is an heterogeneous impact of *reform 92/2012* among firms and

[22]The *reform 92/2012* established that the cut of the employer's social contributions happens also when a unemployed female worker is hired regardless from her age.

Table 3.6: *Effect of reform on male older workers*

VARIABLES	(1) *All type contracts*	(2) *Open-ended conversions*	(3) *Fixed-term contracts*	(4) *Open-ended contracts*
Reform Period	-0.005	-0.129***	-0.098***	-0.034***
	(0.009)	(0.007)	(0.006)	(0.010)
Reform Period × Over-50	0.018***	0.012***	0.022***	0.016***
	(0.002)	(0.004)	(0.004)	(0.004)
Constant	0.622***	0.517***	0.686***	0.569***
	(0.013)	(0.015)	(0.012)	(0.012)
Obs.	446,963	78,516	216,140	152,307
No. Cell	86,140	19,208	33,891	33,041
R-squared	0.021	0.040	0.025	0.025
Cell FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. We refers to the individuals aged 45-54 years (distinguished in two class: *over-50* '50-54' and *under-50* '45-49') hired into open-ended or fixed-term contracts or converted into open-ended contracts. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (Ateco2002 classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time).

employees.

Our baseline approach controls for macro shocks coinciding with the reform but assumes that these shocks have similar effects on treated and untreated group. To check the robustness of our findings, we reformulate the model (3.1) adding a linear trend of outcome variable (hiring or conversions) specific for Italian region and sector (according to 2-digit *Ateco 2002*) between January 2012 and March 2014. These formulations are directed to capture time-varying within region and industry heterogeneity, which could differently affected the hiring and the conversions of apprentices and older male workers, making the treated and control group (i.e. firms or workers) more comparable[23]. Equation (3.2) shows the model estimated, where i indexes cells, t indexes the time (month), and r indexes the Italian region (or industrial sector). The results of the exercise are reported in Table 3.7 for the apprentices and in Table 3.8 for the older male workers. Specifically, the first two columns (1-2 in Table 3.7 and 1-3 in Table 3.8) refer to regional trend adjustment, while the last two columns (3-4 in Table 3.7 and 4-6 in Table 3.8) refer to industrial trend adjustment.

$$y_{i,t} = \alpha_i + \delta_t + \sum_{r=1}^{R} \Big(\rho_r \times G_r\Big) \times Time + \tau \times Policy_{i,t} + \beta \times D_{i,t} + \gamma \times Policy_{i,t} \times D_{i,t} + u_{i,t} \tag{3.2}$$

[23]In detail, we assume linear and quadratic form for the regional and industrial specific trend. These are able to capture the unobservable heterogeneity which evolves linearly or in quadratic form over time. Unfortunately nothing guarantees that all unobserved heterogeneity evolves according these functional forms.

The robustness check is consistent with our results being robust to the introduction of both regional and industrial trends. Once again, all the coefficients corresponding to apprentices and older male workers are positively and statistically significant. Also the magnitude appears to be close to the baseline results in Table 3.5 and 3.6. This suggests that the effect found wouldn't be ascribable to regional or industrial factors (unobservable and time-varying). These findings confirm the greater impact (in absolute term) of the *reform 92/2012* on the apprentices (around to +4-6%) rather than one on the older workers (around to +1-2%).

So far the estimations are based on the bandwidth composed by the firms or workers neighbouring to *cut-point*. In the case of apprentices, the bandwidth involves the classes of firms just below (i.e. firms with 2-9 employees) and just above the 9-employees threshold (i.e. firms with 10-15 employees). In the case of older male workers, the bandwidth involves the classes of workers placed just below (i.e. aged 45-49) and just above (i.e. workers aged 50-54) the 50-years threshold.
In order to verify whether our results may be addressed from the bandwidth choice, we follow Imbens and Wooldridge (2008) and we repeat the estimations considering a bandwidth size twice the originally chosen bandwidth. The sample now includes all the recruitments and conversions recorded in firms with 1-19 employees (i.e. classes of businesses with 1, 2-9, 10-15, and 16-19 employees) referring to apprenticeships, while all the recruitments and conversions of workers aged 40-59 years (i.e. classes of workers aged 40-44, 45-49, 50-54, and 55-59 years). The results of exercise are in Table 3.9.
All the coefficients are positive, statistically significant, and consistent with previous results. Furthermore, both the magnitude and the standard errors are not far away form those of Tables 3.5 and 3.6. According to these findings, the impact of *reform 92/2012* on the apprentices and on the workers aged 50 or older may be not dependent by the bandwidth size chosen.

Equation 3.1 doesn't provide informations about the dynamics of *reform 92/2012* on the hiring and conversions. We explore how quickly employment of apprentices and older workers grows after adoption of policy and whether the policy impact accelerate, stabilizes or mean reverts. Looking the dynamic of policy's impact before the data of approval, we can also infer whether the common trend assumption held. This assessment cannot ignore that the changes on the apprenticeships and older workers following *reform 92/2012* were ratified in July 2012 while the measures are in force starting from January 2013.
To explore these dynamics, Tables 3.10 and 3.11 provide OLS estimations of the baseline model (3.1) augmented with lead and lag terms of policy indicator referring to apprenticeships and older male workers. Specifically, we add policy indicators for three quarters before the actual adoption of reform, for the quarter of actual adoption of reform, and for 1-4 quarters after adoption of reform. These eight policy indicators are equal to 1 only in the relevant quarter, otherwise zero[24].

[24]We choose quarterly indicators of policy because quarter interval is commonly used in the literature. The whole procedure is inspired by Autor (2003).

Table 3.7: *Apprentices: effects of reform with regional and industrial specific time trend*

VARIABLES	(1) *Hiring apprentices*	(2) *Transformed apprentices*	(3) *Hiring apprentices*	(4) *Transformed contracts*
Reform Period × Larger firms	0.0660***	0.0395***	0.0683***	0.0443***
	(0.00796)	(0.00790)	(0.00907)	(0.00865)
Constant	5.173***	3.281***	5.140***	3.272***
	(0.204)	(0.0932)	(0.473)	(0.352)
Obs.	104,873	53,242	104,873	53,242
No. Cell	14,038	10,170	14,038	10,170
R-squared	0.051	0.018	0.051	0.023
Linear Trend (region)	YES	YES	NO	NO
Linear Trend (sector)	NO	NO	YES	YES
Cell FE	YES	YES	YES	YES
Time FE	YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. The analysis refers to the only apprentices (recruited or transformed) reported in firms with 2-9 employees (defined like smaller) and in firms with 10-15 employees (defined like larger). We also keep out the manager. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (Ateco2002 classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time).

Table 3.8: *Older male workers: effects of reform with regional and industrial specific time trend*

VARIABLES	(1) *Open-ended conversions*	(2) *Fixed-term contracts*	(3) *Open-ended contracts*	(4) *Open-ended conversions*	(5) *Fixed-term contracts*	(6) *Open-ended contracts*
Reform Period × Over-50	0.012***	0.022***	0.017***	0.014***	0.021***	0.017***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Constant	3.837***	-0.470***	0.985***	3.757***	-0.352	0.992**
	(0.139)	(0.157)	(0.119)	(0.173)	(0.256)	(0.355)
Obs.	78,516	216,140	152,307	78,516	216,140	152,307
No. Cell	19,208	33,891	33,041	19,208	33,891	33,041
R-squared	0.041	0.026	0.026	0.042	0.030	0.026
Linear Trend (region)	YES	YES	YES	NO	NO	NO
Linear Trend (sector)	NO	NO	NO	YES	YES	YES
Cell FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. We refers to the male workers aged 45-54 years (distinguished in two class: *over-50* '50-54' and *under-50* '45-49') hired into open-ended or fixed-term contracts or converted into open-ended contracts. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (*Ateco 2002* classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time).

Table 3.9: Effect of reform on apprentices and older workers: sensitivity of the bandwidth choice

	APPRENTICESHIP (1-3)			OLDER MALE WORKERS (4-7)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	*All apprentices*	*Hiring apprentices*	*Transformed apprentices*	*All type contracts*	*Open-ended conversions*	*Fixed-term contracts*	*Open-ended contracts*
Reform Period	-0.096***	-0.108***	-0.102***	-0.015**	-0.047***	0.029***	-0.020
	(0.007)	(0.008)	(0.010)	(0.007)	(0.006)	(0.008)	(0.015)
Reform × Larger firms	0.056***	0.063***	0.038***				
	(0.006)	(0.007)	(0.007)				
Reform × Over-50				0.025***	0.018***	0.035***	0.018***
				(0.003)	(0.005)	(0.006)	(0.005)
Constant	0.512***	0.592***	0.333***	0.625***	0.517***	0.691***	0.573***
	(0.008)	(0.009)	(0.012)	(0.012)	(0.014)	(0.012)	(0.011)
Obs.	235,031	162,437	72,594	881,104	155,408	425,067	300,629
No. Cell	40,528	24,689	15,839	170,101	38,283	67,037	64,781
R-squared	0.032	0.042	0.015	0.021	0.039	0.026	0.025
Cell FE	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. In the apprenticeships estimation (columns 1-3) the analysis refers to the only apprentices (recruited or transformed) reported in firms with 1-9 employees (defined like smaller) and in firms with 10-19 employees (defined like larger). Note that the bandwidth size in this analysis is twice than the original size (2-9 and 10-15) since it involves also the firms with 1 employee and the firms with 16-19 employees. We also keep out the manager. Instead, in the older workers estimation (columns 4-7) we refers to the individuals aged 40-59 years (distinguished in two class: *over-50* '50-59' and *under-50* '40-49') hired into open-ended or fixed-term contracts or converted into open-ended contracts. Again, the bandwidth size in this analysis is twice than the original size (45-49 and 50-54) since it involves also the workers aged 40-44 years and the workers aged 55-59 years. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (*Ateco 2002* classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time).

Column 2 of Table 3.10 presents the results for the hiring of apprentices. The coefficients on adoption leads are all statistically different from zero suggesting evidence of anticipatory response within larger firms (with 10-15 employees). The statistical significance of coefficients referring to quarters before the approval date of *reform 92/2012* (January 2013) appears unjustified and it might suggest a diverging trend between treated and control group. The weakening of the common trend assumption reduces the power of our results which should be interpreted very cautiously. The fear is that the effect caught in our analysis might be actually ascribable no to the *reform 92/2012* but to an underlying dynamic already existing among larger and smaller firms. Anyway, starting from January 2013 (date in which the changes are in force) there would be a sharp rise in the recruitments of apprentices for larger firms (with 10-15 employees) relative to smaller firms (with 2-9 employees) passing form 7.5% (0.073 log points) to 11.7% (0.111 log points).

More consistent are instead the findings referring to the transformation of apprentices into open-ended contracts. As we can see in column 3 of Table 3.10, the coefficients on the adoption leads are close to zero, showing no evidence of an anticipatory response within large firms. In the quarter of adoption (January-March 2013), the transformations in the larger firms relative to smaller firms increase substantially by 0.024 log points, after which this increment fluctuates until the 0.087 in the quarter July-September 2013. In this sense, the mandatory minimum percentage of apprentices to be transformed in permanent contract imposed with the *reform 92/2012* seem to produce a significant effect.

Table 3.10: Apprentices: dynamic of policy (quartly analysis)

VARIABLES	*Quarter of reference*	(1) *All apprentices*	(2) *Hiring apprentices*	(3) *Transformed apprentices*
Reform Period × Larger Firms$_{(t_{-3})}$	*Apr-Jun 2012*	0.037**	0.043*	0.016
		(0.015)	(0.024)	(0.017)
Reform Period × Larger Firms$_{(t_{-2})}$	*Jul-Sep 2012*	0.047***	0.054**	0.031
		(0.012)	(0.020)	(0.021)
Reform Period × Larger Firms$_{(t_{-1})}$	*Oct-Dec 2012*	0.052***	0.073***	0.012
		(0.013)	(0.021)	(0.026)
Reform Period × Larger Firms$_{(t_{0})}$	*Jan-Mar 2013*	0.089***	0.115***	0.036**
		(0.014)	(0.019)	(0.015)
Reform Period × Larger Firms$_{(t_{+1})}$	*Apr-Jun 2013*	0.057***	0.066***	0.041**
		(0.014)	(0.021)	(0.017)
Reform Period × Larger Firms$_{(t_{+2})}$	*Jul-Sep 2013*	0.104***	0.111***	0.087***
		(0.015)	(0.025)	(0.021)
Reform Period × Larger Firms$_{(t_{+3})}$	*Oct-Dic 2013*	0.086***	0.111***	0.037
		(0.014)	(0.018)	(0.030)
Reform Period × Larger Firms$_{(t_{+4})}$	*Jan-Mar 2014*	0.124***	0.153***	0.063***
		(0.016)	(0.021)	(0.022)
Constant		0.624***	0.735***	0.402***
		(0.011)	(0.012)	(0.019)
Observations		158,115	104,873	53,242
R-squared		0.038	0.052	0.018
No. of Cell		24,208	14,038	10,170
Cell FE		YES	YES	YES
Time FE		YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. The analysis refers to the only apprentices (recruited or transformed) reported in firms with 2-9 employees (defined like smaller) and in firms with 10-15 employees (defined like larger). We also keep out the manager. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (Ateco2002 classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time). Law change dummies $t_{-3} - t_{+4}$ are equal to 1 in only one quarter (before of after the actual approval of policy) each per larger firms.

Interesting are also the dynamics arise for the older male workers (Table 3.11). The column 2 reveals a substantial lack of effect for the conversions in open-ended contracts recorded among male workers aged 50-54 compared to male workers aged 45-49 after the *reform 92/2012*. Conversely, there would be a strong impact on the hiring into fixed-term contracts (column 3). The no significance of lead terms regarding the quarters before the ratification of policy (January 2013) provides evidence of a little anticipatory response supporting the common trend assumption between treated and control group. However, the significance of lead term regarding to quarter October-December 2012, namely before that incentives were in force, appears puzzling. Indeed, the rebate of employer's social contributions triggers only for the hiring or conversions of workers aged 50 or older occurred after January *1st* 2013. Therefore, it appears senseless to recruit or transform an older worker just before the effective

Table 3.11: *Older male workers: dynamic of policy (quartly analysis)*

VARIABLES	*Quarter of reference*	(1) *All type contracts*	(2) *Open-ended conversion*	(3) *Fixed-effect contract*	(4) *Open-ended contracts*
Reform Period × Over-50$_{(t_{-3})}$	*Apr-Jun 2012*	0.004	0.007	0.002	0.003
		(0.007)	(0.014)	(0.008)	(0.012)
Reform Period × Over-50$_{(t_{-2})}$	*Jul-Sep 2012*	0.011	0.009	0.015	0.005
		(0.007)	(0.018)	(0.009)	(0.010)
Reform Period × Over-50$_{(t_{-1})}$	*Oct-Dec 2012*	0.015*	0.003	0.022***	0.009
		(0.008)	(0.014)	(0.007)	(0.015)
Reform Period × Over-50$_{(t_0)}$	*Jan-Mar 2013*	0.015**	0.007	0.030***	0.004
		(0.006)	(0.009)	(0.007)	(0.010)
Reform Period × Over-50$_{(t_{+1})}$	*Apr-Jun 2013*	0.020***	0.022*	0.027***	0.009
		(0.006)	(0.011)	(0.009)	(0.009)
Reform Period × Over-50$_{(t_{+2})}$	*Jul-Sep 2013*	0.032***	0.006	0.040***	0.030***
		(0.006)	(0.013)	(0.008)	(0.009)
Reform Period × Over-50$_{(t_{+3})}$	*Oct-Dec 2013*	0.030***	0.026	0.028***	0.037**
		(0.007)	(0.018)	(0.009)	(0.013)
Reform Period × Over-50$_{(t_{+4})}$	*Jan-Mar 2014*	0.030***	0.024*	0.032***	0.027*
		(0.006)	(0.013)	(0.007)	(0.014)
Constant		0.633***	0.511***	0.712***	0.573***
		(0.013)	(0.014)	(0.012)	(0.012)
Observations		446,963	78,516	216,140	152,307
R-squared		0.021	0.041	0.027	0.025
No. of Cell		86,140	19,208	33,891	33,041
Cell FE		YES	YES	YES	YES
Time FE		YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. We refers to the male workers aged 45-54 years (distinguished in two class: *over-50* '50-54' and *under-50* '45-49') hired into open-ended or fixed-term contracts or converted into open-ended contracts. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (Ateco2002 classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time). Law change dummies $t_{-3} - t_{+4}$ are equal to 1 in only one quarter (before of after the actual approval of policy) each per larger firms.

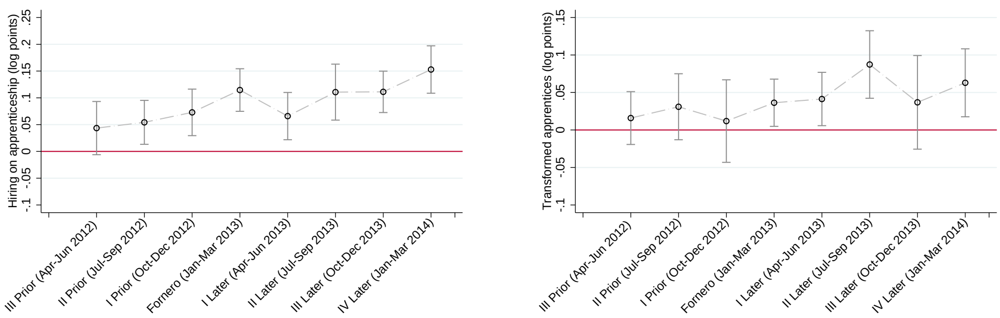
date of incentives program (wasting the possible cut in the social contributions). It might endanger the common trend assumption and the robustness of our findings referring to the recruitments on fixed-term contracts. Anyway, when the rebate of employer's social contributions goes in force (January 2013) we find an increase of coefficients until to maximum of 0.040 log points in the quarter July-September 2013, namely when the INPS clarified the payment method of incentives[25]. Finally, column 4 of Table 3.11 shows the policy's impact for the open-ended contracts. Also in this case, there isn't evidence of anticipatory effect since all the lead terms are close to zero. The impact on permanent contracts seems to be deferred than the date in which the incentives are in force. The first statistically significant impact of *reform 92/2012* starts from the quarter July-September 2013. The result seems to suggest that the measure on older workers takes root only following the issuing of INPS's instructions

[25]On July *24th* 2013, the Italian Social Security Institute (INPS) issued an important document (*Circolare n. 111* provided detailed instructions about the effective fruition of incentives, establishing timing, extension, and individuals involved).

about the effective fruition of the incentives (i.e. document *n. 111*, July *24th* 2013). Indeed, the correspondent quarter (Jul-Sep 2013) records the higher coefficient for the fixed-term contracts and the first significant coefficient for the open-ended contracts.
We well know that in the same period the Italian labour market was also interested by another regulatory intervention (*decree n. 76/2013*) having the aim to incentive the employment (especially of youth and unemployed)[26]. However, we believe that the effect found in Table 3.11 are not ascribable to *decree n. 76/2013* since it doesn't present discontinuity among the workers aged 45-54 years. The Figures 3.3 and 3.4 give a graphical illustration of dynamics so far described.

Figure 3.3: Timing of impact for apprentices

(a) Hiring of apprentices. *(b) Transformed apprentices.*



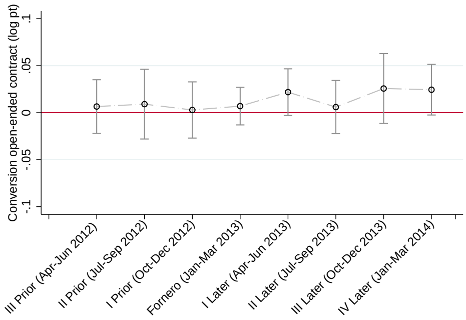
Note: The pattern of impact relative to quarters of adoption of policy derives from the estimation on the baseline model (3.1) augmented with lead and lag terms of policy indicator. Specifically, besides to actual policy indicator (January-March 2013), we add policy variable fro I, II, and III quarters before the adoption of reform and for I, II, III, and IV quarters after the adoption of reform. Vertical bands represents ± 1.96 times the standard error of each point estimated.

So far we have assumed an uniform impact of policy on the apprentices and older workers in treated group relative to control group. However, it could be reasonable suppose an heterogeneous effect of the *reform 92/2012* based on specific employee characteristics. Specifically, in the case of apprentices, we investigate along two dimensions: age and gender. In the case of older male workers we inspect along the positions.
To this purpose, we estimate Equation (3.3), where the variable $H_{s,i,t}$ identifies the sub-group having the specific characteristic under investigation. In detail, $H_{s,i,t}$ assumes value 1 if the characteristics s is present in the cell i at time t, otherwise it is equal to zero. All the other variable have the same interpretation as in the baseline model (3.1). The triple interaction term identifies the potential heterogeneity, namely, the differential effect of the reform on the sub-group having the above mentioned characteristics. Table 3.12 reports regression results.

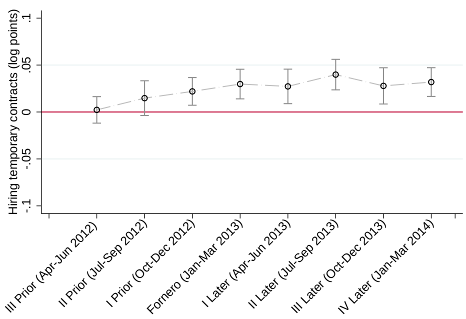
[26]The *decree n. 76/2013* was issued on June *28th* 2013 and converted in the *law n. 99* on August *9th* 2013.

Figure 3.4: Timing of impact for older male workers

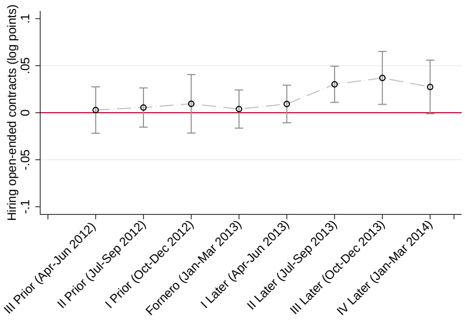
(a) Conversion into open-ended contracts.



(b) Hiring on fixed-term contracts.



(c) Hiring on open-ended contracts.



Note: The pattern of impact relative to quarters of adoption of policy derives from the estimation on the baseline model (3.1) augmented with lead and lag terms of policy indicator. Specifically, besides to actual policy indicator (January-March 2013), we add policy variable fro I, II, and III quarters before the adoption of reform and for I, II, III, and IV quarters after the adoption of reform. Vertical bands represents ±1.96 times the standard error of each point estimated.

$$
\begin{aligned}
y_{i,t} = \alpha_i + \delta_t + \tau \times Policy_{i,t} + \beta \times D_{i,t} + \sum_{s=1}^{S-1} \left(\theta_s \times H_{s,i,t}\right) + \\
+ \nu \times Policy_{i,t} \times D_{i,t} + \sum_{s=1}^{S-1} \left(\lambda_s \times H_{s,i,t}\right) \times D_{i,t} + \sum_{s=1}^{S-1} \left(\mu_s \times H_{s,i,t}\right) \times Policy_{i,t} + \\
+ \sum_{s=1}^{S-1} \left(\gamma_s \times H_{s,i,t}\right) \times Policy_{i,t} \times D_{i,t} + u_{i,t}
\end{aligned}
\tag{3.3}
$$

Columns 1 and 2 of Table 3.12 describe how the reform affects the hiring and the transformation in open-ended contracts at different age of apprentices: 15-19 years old, 20-24 years old, and 25-29 years old. Teen apprentices are the excluded category.

Results suggest that mostly of the hiring are ascribable at the apprentices aged 15-24 years, while (on average) the recruitments of apprentices aged 25-29 years have been significantly less. Conversely, there isn't an heterogeneous impact of policy along the age variable referring to the transformations from apprenticeships to open-ended contracts.
Columns 3 and 4 of Table 3.12 show the potential heterogeneous effect between male and female apprentices. We find that even though the female apprentices seem to be hired more than the men, the male apprentices have more possibility to be transformed into open-ended contracts.
Columns 5 and 6 of Table 3.12 look at how the impact of the policy changes for different job positions for older male workers. In detail, we consider five classes: blue-collars, white-collars, *quadro*, manages and others. The coefficients of triple interaction should be compared to blue-collars. Results reveal a greater positive impact of reform for the blue-collars relative to white-collars both for the permanent and temporary recruitments.
Overall the results fit the interpretation that reform favours the younger apprentices (aged 15-24 years) relative to the older (aged 25-29 years), the recruitments of female apprentices, and the transformations of male apprentices. Among the male older workers, the blue-collars seem to benefit of reform more than the others job categories.

3.8 Conclusion

The paper gives new evidences on the impact of active labour market policies targeting to workers categorized like vulnerable. We focus on apprentices and workers aged 50 or older. There are not many examples in literature relating to evaluation of these kinds of programs. We use a panel data drawn from the Italian Social Security to examine the impact of *reform 92/2012* on employments and conversions of apprentices and workers *over-50*. The period under investigation goes from January 2012 to March 2014. Although the law passed In July 2012, the measures here inspected are in force on January *1st* 2013.
Referring to apprenticeship contracts, reform induces changes for the large firms (with more than 9 employees) leaving small firms (with at least 9 employees) unaffected. Taking advantage of this sharp discontinuity, we use a *difference-in-difference* model to overcome identification problems typical in these kind of applications. Likewise, we assess the effect of employer incentives on hiring and conversions of workers aged 50 or older. In this case, we exploit the sharp discontinuity arisen between workers *over-50* (recipients of incentives) and *under-50* (unaffected).
Our findings suggest an increase in the hiring on apprenticeship contracts and in the conversion rates from apprenticeship to open-ended contracts in large firms (10-15 employees) relative to small firms (2-9 employees) after the policy. After the reform, the large firms employ apprentices roughly the 7.1% more than the small firms and convert apprentices (in open-ended contracts) roughly the 3.9% more than the small firms. Furthermore, our results fit the interpretation that the policy favours the younger apprentices (aged 15-24) relative to older (aged 25-29), the recruitments of female apprentices, and the transformation

Table 3.12: Heterogeneity of policy effects on apprentices and older male workers

	APPRENTICESHIPS (1-4)				OLDER MALE WORKERS (5-6)	
HETEROGENEITY	(1) Hiring apprentices	(2) Transformed apprentices	(3) Hiring apprentices	(4) Transformed apprentices	(5) Fixed-term contracts	(6) Open-ended contracts
Reform Period (R.)	-0.057***	-0.159*	-0.119***	-0.095***	0.760***	-0.090***
	(0.010)	(0.086)	(0.011)	(0.015)	(0.174)	(0.029)
R. × Treated Group	0.074***	0.102	0.052***	0.053***	0.191***	0.127***
	(0.016)	(0.124)	(0.013)	(0.014)	(0.037)	(0.044)
R. x Larger Firm x 20-24 years	0.015	-0.037				
	(0.016)	(0.121)				
R. x Larger Firm x 25-29 years	-0.034**	-0.090				
	(0.016)	(0.125)				
R. × Larger Firm × Women			0.040**	-0.037**		
			(0.016)	(0.015)		
R. × Over-50 × White-collar					-0.208***	-0.126*
					(0.043)	(0.067)
R. × Over-50 × Quadro					-0.163**	-0.048
					(0.064)	(0.085)
R. × Over-50 × Manager					-0.108	-0.086
					(0.129)	(0.052)
R. × Over-50 × Others					-0.120	0.453
					(0.110)	(0.301)
Constant	0.705***	0.382***	0.706***	0.382***	3.641***	2.825***
	(0.010)	(0.015)	(0.011)	(0.015)	(0.157)	(0.081)
Observations	104,873	53,242	104,873	53,242	216,140	152,307
No. of Cell	14,038	10,170	14,038	10,170	33,891	33,041
R-squared	0.051	0.017	0.050	0.017	0.016	0.018
Cell FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES

Notes: Significant levels: ***1%, **5%, *10%. Standard errors in parenthesis adjusted for clustering at Italian region level. We exclude the hiring recorded in public sector, extraterritorial organizations, agricultural sector. In the apprenticeships estimation (columns 1-3) the analysis refers to the only apprentices (recruited or transformed) reported in firms with 2-9 employees (defined like smaller) and in firms with 10-15 employees (defined like larger). We also keep out the manager. Instead, in the older workers estimation (columns 4-7) we refers to the individuals aged 45-54 years (distinguished in two class: *over-50* '50-54' and *under-50* '45-49') hired into open-ended or fixed-term contracts or converted into open-ended contracts. The period under investigation is January 2012 - March 2014. The reform period is January 2013 - March 2014. The dependent variable is the natural logarithm of hiring or conversions recorded in each cell. The cells are built taking into account Italian province, industrial sector (Ateco2002 classification), worker characteristics (i.e. gender and age), business size, and type of employment contract (permanent or temporary or apprenticeship, white-collar or blue-collar or quadro, full-time or part-time). Table omits the interaction between the policy indicator and heterogeneity elements. In columns (1) and (2) the heterogeneity is relative to group of teen apprentices ($<$ 20 years old). In columns (3) and (4) the heterogeneity is relative to male apprentices. In columns (5) and (6) the heterogeneity is relative to group of blue-collar employed.

in permanent contracts of male apprentices.

The rebate of employer's social contributions who hires (or transforms) workers *over-50* positively affects hirings into temporary and permanent contracts. Conversely, we don't find effect for the transformations in open-ended contracts. The hirings in open-ended and fixed-term contracts for male workers aged 50-54 are greater than ones aged 45-49 years, respectively, for the 1.6% and 2.2% after the policy. We notice that the INPS's instructions on effective fruition of incentives have played an important role for the effectiveness of measure. Among the older workers, the blue-collars seem to benefit of reform more than the other job categories.

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