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The Unintended Consequences on Crime of "Pennies from Heaven"

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Abstract[*]

This paper examines how an infrastructure investment policy, implemented nationwide at the local level, has affected local crime rates. This policy, developed in the wake of the global recession of 2008–09, was designed to boost local economies through job creation. Using monthly figures from the Spanish region of Catalonia's more than 900 municipalities, the paper exploits geographic and time variation in the Spanish Ministry of Public Administration's random approvals of local investment policies, to estimate their impact on both (un)employment and crime. The combination of difference-in-differences and IV estimates makes it possible to precisely assess both the size and timing of the policy's impact on the local labor market and on municipal-level crime rates. While the policy apparently did not tackle the economic recession over the long run, local public finances did experience a boost over the short term, resulting in a temporary reduction in local unemployment rates (as legally required by the policy), as well as a significant drop in crime rates.

JEL classifications: K42, R53, H54, J40
Keywords: Crime, Unemployment, Local investment Policies, Local economic development

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1. Introduction

The impact of local development on crime rates is contingent on time and space. Many factors related to local economic activity can exert counterbalancing forces, leading to either a positive or negative impact on crime rates. Going back to the beginning, and taking Becker's (1968) seminal model in the economics of crime literature, we have learned that rational individuals divide their time between legal and illegal activities (contingent on many factors, such as rewards from these activities, deterrence variables, severity of punishment, and personal traits). Thus, in principle, other things being equal, all public policies designed to increase labor market opportunities should reduce crime.

The empirical evidence is mixed and contradictory, precisely because it is very difficult to account for all factors that could be in play. Using various mechanisms (both direct and indirect), it is easy to figure out why economic activity in general, and employment in particular, may have a decisive impact on criminal activity. As shown by Freedman and Owens (2015) in the San Antonio (Texas) case, as long as employment opportunities are not equally distributed among individuals, criminal activity can increase when the earnings of those individuals benefiting from an employment program go up (i.e., an increase in the supply of criminal opportunities) vis-à-vis the earnings of those not benefiting from the program. However, other forces could be at work; for instance, better employment opportunities could result in more people working, hence spending less time at home, increasing the chances of being burglarized. Besides, all sorts of possible forces between labor market opportunities and crime could be simultaneously acting, thus counterbalancing one another. Therefore, it is crucial in this literature to find adequate, empirical setups that allow us to properly address the issue at stake, and to find proper, causal relationships among employment opportunities, local development in general, and criminal activity.

In December 2008, as a result of the intensifying financial crisis that began three months earlier with the Lehman Brothers bankruptcy, and which was characterized at the time by a credit crunch, growing uncertainty about the economic outlook, and the ensuing severe contraction in private demand, the Spanish Central Government decided to implement various urgent and extraordinary measures to boost local economic activity and local employment. The measure that captured the spotlight was the creation of an €8 billion nationwide public fund to finance local

public works infrastructure. This fund, also known as *Plan E* or *Plan Zapatero*,[1] was called the *Fondo Estatal de Inversión Local* or FEIL (State Fund for Local Investment).

In this paper, using difference-in-differences and an instrumental variable methodology, I take a look at the Spanish Ministry of Public Administration's random approval dates of these local investment programs to examine how they vary across time and municipalities. I use this variability to assess how the upsurge in local economic activity—as a result of higher local employment—affects crime. I start by using monthly data from the Catalan municipalities to assess the local investment programs' impact on employment and unemployment rates. Once I have demonstrated that the FEIL fund indeed affected short-run labor outcomes, I then measure this fund's impact on local crime. I do this by studying all recorded crime incidents taken from a geocoded dataset provided by the Catalan Police Department.

The results show that, in the short run, the FEIL fund did successfully reduce unemployment rates—especially among unemployed male construction workers—thus significantly reducing crime. A closer look at results reveals some interesting features. For instance, I find that some types of crime were significantly reduced during working hours and also that the probability of repeated offenders was lower as a result of the decrease in the unemployment rates experienced in the municipalities. Moreover, the occurrence of crime incidents matches what I identify as the time profile for the impact of different types of local infrastructure projects.

Understanding the effects of local public investment programs on crime is interesting in its own right, but the findings of this paper may also lead to a better understanding of similar programs being carried out in other countries. For instance, many developed—and especially developing—countries are implementing these types of local programs. The International Labour Organization (ILO) issued a guide for labor-intensive infrastructure programs (see Bentall, Beusch, and de Veen, 1999), recognizing that "(w)*ell-designed and well-implemented labour-based infrastructure programmes offer specific advantages to the social partners (governments, employers and workers) in developing countries in terms of improved access to public markets, increased employment and better returns to investment. Moreover, they provide a good opportunity to each of these partners to incorporate social policy objectives into infrastructure*

[1] Nowadays, the FEIL fund is remembered as one of the Socialist government's worst economic decisions of that time; see http://listas.eleconomista.es/economia/364-los-errores-econmicos-ms-graves-de-zapatero.

investment policies."[2] The present paper aims to provide tools to better define how social policy objectives benefit public investment programs.

The paper is organized as follows. Section 2 briefly reviews the extensive literature on the issue under discussion. Section 3 provides background on the FEIL local investment fund, which will be used as an exogenous source of how municipal-level unemployment rates vary. Section 4 gives a detailed description of the dataset used. Section 5 explains the identification strategy and the methodology employed. Section 6 presents the main results of the paper. Finally, Section 7 concludes.

2. A Glance at the Literature

The relationship between labor outcomes and crime has a long tradition in both the empirical and theoretical economic literature. From an empirical point of view, unemployment was the focus of researchers' attention, especially in the mid-1980s, when unemployment was believed to be the key determinant of crime.[3] Those "initial" studies in the mid-1980s revealed that high unemployment was associated with a rise in crime, although the relationship between crime and unemployment was less statistically significant than, for instance, that between deterrence variables and crime. In addition, the empirical evidence was far from conclusive, and the relationship between crime and unemployment ambiguous (in both its nature and robustness), hence leaving the topic open for further research (see, for example, Cameron, 1988, and Freeman, 1996, for surveys of those initial studies).

Such ambiguity was linked to various factors, such as the level of data aggregation, the measures of unemployment and criminality used, and the econometric specification used. If

[2] Specific examples of programs can be found in many countries, such as Mexico, in 2014 (http://goo.gl/7AU3C7). Another example is a program to be implemented by the Australian embassy in Chile, Colombia, and Ecuador in 2015–16 (http://goo.gl/OPxXE0). In fact, many types of foreign aid to developing countries take the form of public investment programs. See, for instance, USAID (https://goo.gl/Lkf8TS) or the 2014 United Nations Development Programme (UNDP) program in the Central African Republic (http://goo.gl/cmbie3) to promote social cohesion, rebuild local infrastructure, and create short-term employment opportunities in communities that have seen homes and businesses destroyed by ongoing violence. These local investment programs are attractive to donors and governments alike, as they meet employment and poverty objectives, improve income and living standards in rural and urban areas, and strengthen the domestic construction industry.

[3] Theoretical models include, among others, a structural model with time allocated among criminal activities, the labor market, and nonmarket activities (Grogger, 1998). In his model, Grogger finds evidence that higher wages deter crime. Job search models, such as the one constructed by Burdett, Lagos, and Wright (2003, 2004), enabled crime and labor decisions to be endogenized, allowing for multiple equilibria to occur. This opened the door to explaining the high dispersion in crime rates across urban areas, for instance (see Glaeser, Sacerdote, and Scheinkman, 1996).

studies using aggregate time-series and cross-sectional data found a causal relationship between unemployment and crime, panel data studies—such as Papps and Winkelmann's (2000) article on crime in New Zealand, and Entorf's and Spengler's (2000) article on crime in Germany—found little effect. Using state-level data for the United States, Raphael and Winter-Ebmer (2001) found that a significant reduction in the (state aggregate) proportion of property crimes in the United States during the 1990s was due to the reduction in unemployment. This latter result is consistent with the findings of Machin and Meghir (2004) and Mocan and Rees (2005). Using an instrumental variable approach, Gould, Weinberg, and Mustard (2002) established a causal relationship between changes in labor market prospects (especially wages) of young, unskilled men in U.S. counties from 1979 to 1997, and crime rates. They showed that "although *crime rates are found to be significantly determined by both the wages and unemployment rates of less educated males, our results indicate that a sustained long-term decrease in crime rates will depend on whether the wages of less skilled men continue to improve*" (Gould, Weinberg, and Mustard, 2002).

More recent publications have focused on individuals' chances of engaging in illegal activities, depending on their employment status or prospects. This development makes up for the tendency of earlier publications to focus mainly on unemployment while overlooking other potential job opportunities in the labor market. This strand of literature usually finds that the beneficiaries of improved economic conditions commit fewer crimes (see, for instance, Harbaugh, Mocan, and Visser 2013 for evidence on an economic experiment).

Another strand of the literature relevant to the present analysis seeks to disentangle the relationship—and the multiple forces that could be at work—between economic development (broadly defined) and illegal behaviors. In this regard, the centrally planned economic measure I shall focus on was designed to favor the types of investments that help stimulate short-term economic activity (i.e., through job creation) while also strengthening the financing of municipalities.

In this context, improved employment opportunities are perhaps the key issue for local development, at least for small and medium-sized municipalities. However, the FEIL fund was also intended to improve the overall economic standing of municipalities, thereby boosting local economic growth and maybe even reducing poverty and income inequality. In this context, Kelly (2000) showed that, for urban counties in the United States, inequality has no effect on property

crime, but it does have a strong and robust impact on violent crime. In this sense, inequality may be associated with a lack of social capital and upward mobility, and social disorganization, all of which may lead to higher levels of crime. On the other hand, Kelly (2000) also showed that, while poverty has a significant effect on property crime, it has little effect on violent crime.

Furthermore, the FEIL fund was geared towards municipal infrastructure improvement projects with a productive and socially useful purpose. In this respect, a meticulous review of approved local projects reveals that many projects were designed to improve municipalities' social capital. It is now well established in the literature that social capital (broadly defined) affects crime (see, for instance, Buonanno, Montolio, and Vanin, 2009). Several theories, developed by sociologists and criminologists, hold that social capital has a negative effect on crime. Rosenfeld, Messner, and Baumer (2001) argue that the social disorganization, anomie, and strain theories all predict that civic engagement and social trust (to which they refer as social capital) should reduce crime, because they increase formal and informal social control, strengthen the effectiveness of social norms, and provide resources for individual goal attainment. Indeed, the purpose of many local investment projects presented was to improve local conditions.

In any case, both the empirical strategy and the Catalan Police Department's detailed database will help us determine the factor at work regarding the change in local crime rates.

3. Institutional Setting: The 2008–09 FEIL Fund

As a result of the crisis that began in early September 2008, the unemployment rate in Spain rose from a record low of 7.95 percent in the second quarter of 2007 to 11.34 percent in 2008. In 2009, it shot up to 18.01 percent, double the average unemployment rate of the Eurozone countries. This increase in the unemployment rate was especially accentuated in the construction industry, a phenomenon that would come to be known as the "bursting of the housing bubble" (in which Spain had lived since the early 2000s).

In this environment, the Spanish Central Government—led by Socialist Prime Minister José Luis Rodríguez Zapatero—created a public fund to finance local investment projects whose main purpose was to create jobs (or reduce unemployment) at the local (municipal) level. The fund, formally called *Fondo Estatal de Inversión Local* (FEIL), was popularly known as *Plan E* or *Plan Zapatero*. Using this public investment fund—established on November 28, 2008 under

Royal Decree-Law 9/2008—the Spanish government approved a series of loans worth €8 billion, an initiative representing 0.76 percent of GDP in 2009. FEIL's objective was to maintain and create jobs (avoid job destruction), especially in the construction industry, and shore up those businesses (especially SMEs) that were tied to the construction industry.

Between December 10, 2008 and January 24, 2009, a total of 8,108 Spanish municipalities (99.8 percent) electronically proposed 30,903 projects, of which 30,698 were approved (99.6 percent). To ensure that funds were distributed equally across municipalities, and contingent upon the project's approval, funds per municipality were allocated in accordance with its population, with approximately €177 per inhabitant. The maximum amount allocated to each project could not exceed €5 million, and most public works had to be undertaken in 2009.[4] Almost 80 percent of the investment was dedicated to rehabilitation projects and improving public spaces, facilities, basic services and cultural infrastructure, schools, and sports arenas. Municipalities were paid 70 percent of the project amount at the beginning of the project, and 30 percent upon its (certified) completion.

Some features of the FEIL fund itself, and the way it was handled, make it an ideal example of a source of exogenous variation in local unemployment rates. Analyzing FEIL's potential impact on local crime is equally rewarding. First, the fund was a totally unanticipated shock for local public finances. Indeed, the Royal Decree-Law was issued at the end of November[5] (when all local public budgets for the next fiscal year were already drawn up and many of them approved), clearly establishing that the fund would be dedicated to local public investment not entered in the 2009 budget.[6] Naturally, this meant an unanticipated increase in local public budgets.

Second, the FEIL fund was pushed through urgently, meaning that, for the Central Government, timing was crucial for having an immediate impact on the labor market. This meant that project proposals to be funded had to include public works that could be implemented immediately (i.e., work tenders were to begin within a month after the FEIL funding-

[4] In principle, all public works were to be finished by the end of 2009, and the work completion certificate submitted to the Ministry by March 2010. The only information available is from a 2010 follow-up report, which states that in July 2010, 99.78 percent of municipalities had received the first payment (70 percent) and that 93.60 percent of municipalities had already received the second payment. As a result, the projects were liquidated (see MPT, 2010).

[5] Although the Royal Decree-Law was issued on Friday, November 28, it was not published in Spain's Official State Bulletin (BOE) until December 2, 2008.

[6] In fact, as part of the application process, the municipalities were required to certify that the investment had not previously been factored into the 2009 budget.

authorization resolution had been published on the Ministry of Public Administration's Web site). Indeed, this point is key to my identification strategy, given that it defines when treatment will start for a given municipality. Consequently, it deserves a bit more attention.

3.1 FEIL Application, Approval, and Public Procurement Rules in Spain

In its eagerness to positively affect labor outcomes as quickly as possible, Spain's Central Government took some drastic measures. First, it fast-tracked the approval and implementation of proposed public works projects by accelerating the tender and award process. Second, it conducted its operations almost entirely online. Third, its territorial delegations (usually one per province) had to verify that projects met the requirements established in the Royal Decree-Law. Once the verification was made, the territorial delegations sent an electronic notification to the Secretary of State for Regional Cooperation, who then issued the resolution authorizing the projects' financing. Finally, the Secretary of State for Regional Cooperation had the resolution published on the Ministry of Public Administration's Web site. In this way, the Central Government could decentralize and accelerate the process without overwhelming the Secretary of State for Regional Cooperation.

3.1.1 Submission and Approval of Project Applications

The process of submitting project applications opened up between December 10, 2008 and January 24, 2009. As soon as the applications were received, the Central Government's territorial delegations had up to 10 business days to review them. Once reviewed, and once the delegations had informed the Secretary of State for Regional Cooperation that the projects qualified for the FEIL fund, the Secretary of State for Regional Cooperation had up to 10 business days to issue the authorizing resolution and get it published on the Ministry's Web site.

The first resolution of approved projects was published on December 20, 2008. In principle, a project submitted on the last day of the submission period (January 24, 2009) should have been approved by February 20, 2009. Nevertheless, I noticed that some resolutions were being published as late as March 24, 2009. In other words, there was a four-month window (from December 2008 to March 2009) during which projects could be approved.

3.1.2 Tender and Implementation Process

Once the project had been approved, and as explicitly stated in the Royal Decree-Law, the tender procedure should have strictly adhered to public procurement rules, which in Spain varied depending on the amount allocated to the project and its nature. Tendering of public works could have been performed according to any of the procedures under the law governing public sector contracts. That is, tenders could have been open, restricted, negotiated with or without publicity, or processed as a smaller contract.

The urgent way in which this fund was being handled, caused all projects to be placed into the immediate implementation category; therefore, the tender procedure had to begin within one month after the Ministry of Public Administration had published the resolution authorizing the FEIL fund's financing of the project on its Web site. The length of the tender process, though, varies depending on the amount allocated to the project (see Table 1).

Table 1. Summary of Public Procurement Procedures for Investment Projects in Spain

Type of procedure	Amount	Expected length of time under normal procedure	Expected length of time under urgent procedure
Minor contract	< €50,000	< 1 month	< 1 month
Negotiated with no publicity	>€50,000 and <€200,000	2–3 months	1–2 months
Negotiated with publicity	>€200,000 and <€1,000,000	3–4 months	2–3 months
Open or restricted procedure (not subject to EU harmonized regulation)	>€1,000,000 and <€5,000,000	3–4 months	2–3 months
Open or restricted procedure (subject to EU harmonized regulation)	>€5,000,000	5–6 months	3–4 months

Source: Author's calculation based on http://www.boe.es/buscar/act.php?id=BOE-A-2011-17887 and http://ec.europa.eu/growth/single-market/public-procurement/rules-implementation/index_en.htm.

Projects with allocated amounts of less than €50,000 were considered minor works contracts, and it was possible that the project would be assigned directly to a contractor without any type of public competition. Indeed, the Royal Decree-Law established that in the case of such contracts, not only did the tender process have to take place within a month, but the project had to be awarded to a private firm as well.

Public procurement somehow became more complicated for project amounts above €50,000. For example, public investment projects with amounts between €50,000 and €200,000 were subjected to the no-publicity procedure, wherein it was expected to take between one and two months for the project to be awarded. For projects with amounts greater than €200,000 and less than €5,000,000, (the upper limit being established by the Royal Decree-Law precisely to limit tender procedures)[7] there were three tender options: a procedure negotiated with publicity, an open procedure, and a restricted procedure. The latter two options applied to cases not subjected to EU harmonization regulation. For all three procedures, and taking into account that all three could be issued urgently, the expected length of time until the project could be adjudicated was between two and three months.

Timing is important for analyzing when local investment projects started being implemented and, consequently, when the potential impact on labor outcomes and crime rates was expected to take effect. In this context, it is also reasonable to expect a different time frame for implementation, depending on the size and nature of the projects. Small projects were undoubtedly carried out within a shorter time frame than larger ones.[8]

3.2 Defining the Treatment

My identification strategy relies on the nature of the FEIL program because, as explained above, it materialized out of nowhere and was urgently undertaken and implemented at the local level. As a result of the way the fund was planned, organized, and managed during the submission and approval stages—and, as shown in Figure 1 (for projects submitted by the Catalan

[7] To keep from exceeding this amount, local governments were not permitted to split the project into two or more projects. However, it was possible for municipalities to contribute their own funds to some projects. In any case, the tender procedure of FEIL-funded projects had to follow tender rules for projects with amounts below the €5,000,000 threshold.

[8] I believe, for instance, that the €14,210 project entitled "Adapting Plots of Municipal Property to the Terms of Rule That Prevent Forest Fires"—which essentially involved clearing a municipal plot of weeds in Bescanó (Girona)—implied a shorter implementation time frame and less substantial impact on labor outcomes, than the €2,999,927 project entitled "Works of Reform and Extension of the Francesc Calvet Sports Center" in Sant Joan Despí (Barcelona).

municipalities), projects were approved on a daily basis. In addition, resolutions with lists of approved projects throughout Spain were published almost daily.

I noted with interest that the timing in which local investment projects were approved varied from month to month and by municipality (as my labor and crime data). Project approvals took place from December 2008 through March 2009.[9] However, as shown in Figure 1, a mere total of seven projects from four Catalan municipalities were approved in March 2009. Similarly, 211 projects from 30 Catalan municipalities (nine with population above 500 inhabitants) were approved in December 2008. Hence, in order to simplify the empirical exercise, I omitted these two approval dates from the rest of the analysis.[10] Consequently, I define my treatment status by the month in which a municipality's projects were approved; that is, in January 2009 or February 2009.

Before elaborating further on my treatment definition, I shall briefly present the main figures of the FEIL fund in Catalonia. In the 948 Catalan municipalities (11.7 percent of the Spanish total) where the average project amount was €324,281.97,[11] 3,932 projects were approved (12.8 percent of the Spanish total) for a total of €1.276 billion (16.0 percent). The amount injected into the Catalan municipalities was equivalent to 0.64 percent of total regional GDP.

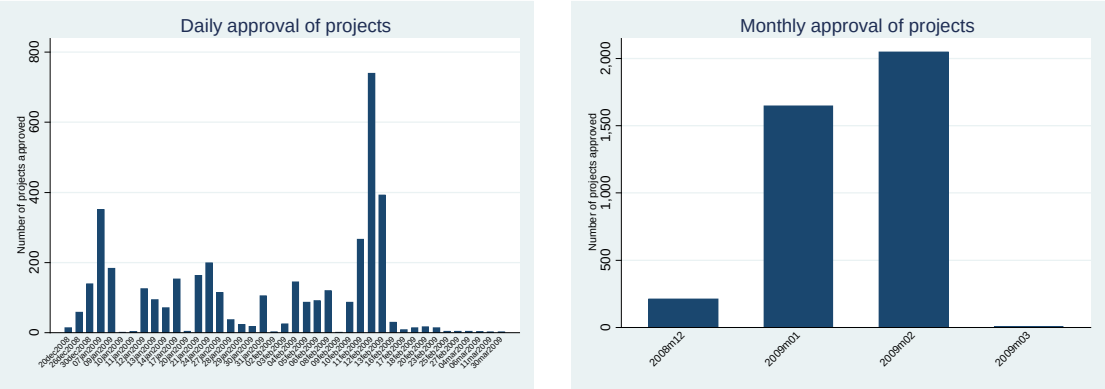
Interestingly—for us, and for my research strategy—of the 948 Catalan municipalities, 872 (92.3 percent) had projects approved within a single month, while the remaining 76 had projects approved over a period of several months. I omitted those 76 municipalities from my analysis, since their populations are larger on average, and they submitted an average of 25.3 projects (ranging from a low of two projects to a high of 302, in the city of Barcelona). For these municipalities, I do not have a clearly defined temporal treatment definition.

[9] Final, extraordinary FEIL approval was granted in December 2009. Regarding Catalan municipalities, that approval only included projects from the city of Barcelona, which, as discussed on the next page, were not factored into our analysis.

[10] From the sample of municipalities with more than 500 inhabitants and with all presented project approved in one month only nine municipalities had all projects approved in December 2008. In Appendix D I present, as a robustness exercise, the main results of the paper when I also include December 2008 as the approval month.

[11] From a regional perspective—and consistent with the government's criterion of distributing FEIL funds according to municipalities' populations—the Catalonian region was the second-largest recipient of total FEIL program funds, behind Andalucía (whose share of those funds was 17.8 percent, compared with its 9.5 percent share of all Spanish municipalities). The Madrid region came in third, receiving 13.5 percent of the FEIL funds for 179 municipalities (2.2 percent of the total). It received an average amount of €867,292.96 per project, by far the highest amount per project among the Spanish regions.

Figure 1. Daily and Monthly Approval of Projects by the Ministry of Public Administration



Note: Projects submitted by Catalan municipalities.

In addition to omitting from the study the highly populated 76 municipalities (since the various projects they received had been approved over a period of several months), I further restricted my sample to municipalities with more than 500 inhabitants. The reasons for doing this are manifold. Small municipalities in Catalonia, and in Spain as a whole, have long faced an organizational problem: they lack an auditor/treasurer (*secretario-interventor*) on their municipal councils. This implies that, on many occasions, there is no one authorized to validate all agreements approved by the council.[12] The most common solution that small municipalities have come up with is to share auditors who periodically rotate among different municipalities. This approach, however, affects the municipality's ability to apply and manage funds, as in the case of the FEIL fund. Moreover, procurement procedures for these small municipalities are not that strict and can be changed due to their nature. In fact, in the FEIL Royal-Decree Law, explicit mention was made of this issue, stating that the Ministry of Public Administration could, in exceptional cases, authorize direct implementation of public works in municipalities with fewer than 200 inhabitants. The special nature of those municipalities uncertainty with respect to tender procedures and project implementation in those municipalities prevent us from including them in my study. Indeed, I believe that my final sample is rather homogeneous in the type of

[12] This phenomenon occurs for two reasons. On the one hand, there are municipalities that cannot afford to hire a *secretario-interventor* (i.e., a high-ranking civil servant who acquires his or her position by passing a very competitive nationwide examination). On the other hand, not all auditors/treasurers want to live in municipalities with fewer than 500 inhabitants.

municipality analyzed, which protects it from distortions that could potentially arise from using very small or very large municipalities.

The remaining 539 municipalities submitted, on average, 3.4 projects, with a low of 1 and a high of 37, with population figures ranging from 501 inhabitants to 107,770 inhabitants.[13] However this is not still my final sample. As explained previously, not only does the month in which approval was granted matter to us, but also the tender and implementation periods. In this context, and according to the information in Table 1, I computed a tender and award period for each project approved by the Ministry of Public Administration, depending on the amount. For projects with amounts below €50,000, I assume a tender and award period of one month following the month in which the project was approved. For projects with amounts between €50,000 and €200,000, I assume a two-month period, while for projects with amounts above €200,000 I assume a three-month period before project implementation.[14]

I further restrict my sample to municipalities with approved projects of the same size in terms of tender time. Although nearly 50 percent of the 539 municipalities presented a single project (and up to 70 percent three projects which in general are quite similar in size), I make use a restricted sample of 348 municipalities to perform my main estimates. Table 2 presents the summary statistics by treatment status. Appendix D, however, also presents the full set of results when I use the three treatment statuses (that is, also including approved projects in December 2008) and restrict sample to municipalities having more than 500 inhabitants.

[13] As the descriptive statistics show, municipalities submitted, on average, two projects to the Ministry, many of which were approved the same day. However, there were cases where municipalities had various projects approved on several different days within the same month. For instance, La Garriga, a municipality in the province of Barcelona with 15,000 inhabitants, submitted nine projects: one was approved on February 2, 2009; six were approved on November 2, 2009; and two were approved on December 2, 2009. Similarly, Sant Feliu de Llobregat, a municipality in the province of Barcelona with 43,000 inhabitants, had five projects approved on three different days: February 3, 2009; February 10, 2009; and February 11, 2009. Vila-seca, a municipality in the province of Tarragona with 21,000 inhabitants, is an example of a municipality that had projects approved on January 14, 2009; January 17, 2009; and January 20, 2009.

[14] Defining the tender and award periods for projects with amounts between €50,000 and €200,000 as 1.5 months and 2.5 months for those projects with amounts above €200,000 does not significantly change the results obtained and presented in the rest of the paper.

Table 2. Municipal Summary Statistics by Treatment Status

Variable	Mean	Std. Dev.	Min.	Max.	Observations
Treatment = 2009m01					
Population	4,581.12	8,313.17	510	45,994	91
Number of projects	2	2	1	10	91
Total amount	751,263.90	1,303,149.00	86,051.91	7,907,482	91
Amount per project	390,604.80	576,053.80	36,459.67	4,574,085	91
Tender period	2.5	0.5	1	3	91
Treatment = 2009m02					
Population	2,775.04	4,216.49	501	33,761	257
Number of projects	2	1	1	9	257
Total amount	438,132.50	565,566.60	68,816.99	3,870,910	257
Amount per project	294,645.70	394,378.70	23,207.63	3,870,910	257
Tender period	2.3	0.6	1	3	257

In short, my identification strategy relies on two facts. First, as I prove below, a different month of approval by the Ministry is something random, or at least independent of my main variables of interest. Second, I take advantage of public procurement rules to assign a tender and award period for each project, depending on its amount. This will allow me to properly match the succession of events between project approval, tender, implementation, and the impact on labor outcomes and crime.

Note that a positive aspect of the narrow time window I use to identify FEIL's impact on labor outcomes and crime rates is that the timing of approval, tender, and implementation phases of each municipality's FEIL projects was very unlikely to coincide with another simultaneous shock, thereby influencing my outcome variables and invalidating my results. Moreover, I do not have the potential drawback of the time distance between the announcement (approval) of projects and their implementation, as did Freedman and Owens (2015). Those authors had to rely on potential purchasing power increases of potential workers hired by the project to explain the observed results.[15]

[15] It is very unlikely that firms anticipated the announcement of the FEIL program, since they did not have extra personnel in place ahead of time to undertake the potential projects being designed and submitted by local governments: first, because the Royal Decree-Law was issued rather quickly (and unpredictably) and through urgent procedures, and, second, because the projects were not required to be budgeted for previously, firms and workers could not have anticipated them by looking at the 2009 budget.

4. Data

4.1 Labor Market Data

I use monthly employment and unemployment figures from the Spanish Ministry of Employment and Social Security. At the municipal level, employment data only take into account registered workers in the various existing regimes; they do not reveal the particular industry or personal attributes of those individuals. By contrast, registered unemployment data at the municipal level reveal a great deal more; I have information on the industry, gender, and age of registered unemployed individuals. These details will be very useful for ascertaining who, if anyone, benefited from the FEIL fund. They will also be useful for figuring out the potential dynamics at play if crime rates are found to have been affected as well.[16]

Given the FEIL fund's objectives, its impact should, in principle, be greater on construction workers (or unemployed construction workers) due to the nature of public works financed by the fund and the Royal Decree-Law's explicit mention of the construction industry and related industries.

The present study runs from January 2007 to December 2009. The main reason for ending it in December 2009 is that, by the end of 2009, the Spanish Central Government had set up a second fund. This €5 billion fund, known as FEES (*Fondo Estatal para el Empleo y la Sostenibilidad Local*, in English State Fund for Employment and Local Sustainability), was established on October 26, 2008 by Royal Decree-Law 13/2009. Under FEES, potential projects to be funded had to be submitted between November 2009 and January 2010. FEES funding for municipal projects was approved between January 2010 and May 2010. For this reason, I use year-end 2009 as the cutoff point for this study to prevent my estimates from capturing any possible impact from this second fund.

[16] It is well known that registered unemployment figures are lower than those obtained from the Spanish Labor Force Survey (EPA). Registered unemployment is a recount of the unemployed who apply for benefits at regional public employment offices. It does not, for instance, reflect students looking for a job, or people who gave up looking for jobs through the national employment system and sought employment on their own. In any case, Figure E.1 in Appendix E shows that quarterly, provincial-level unemployment data from both sources show the same trends. Moreover, it is required to be registered to apply for unemployment benefits in order to be entitled to receive them. In late 2008 and early 2009, when the economic crisis was in its early stage and unemployment benefits started being drawn, it is reasonable to assume that workers who lost their jobs applied for unemployment benefits (hence registering in the public employment offices). For that reason, one can also assume that registered unemployment statistics were more reliable during that time than during other periods, when there could be fewer incentives to register at the public employment offices.

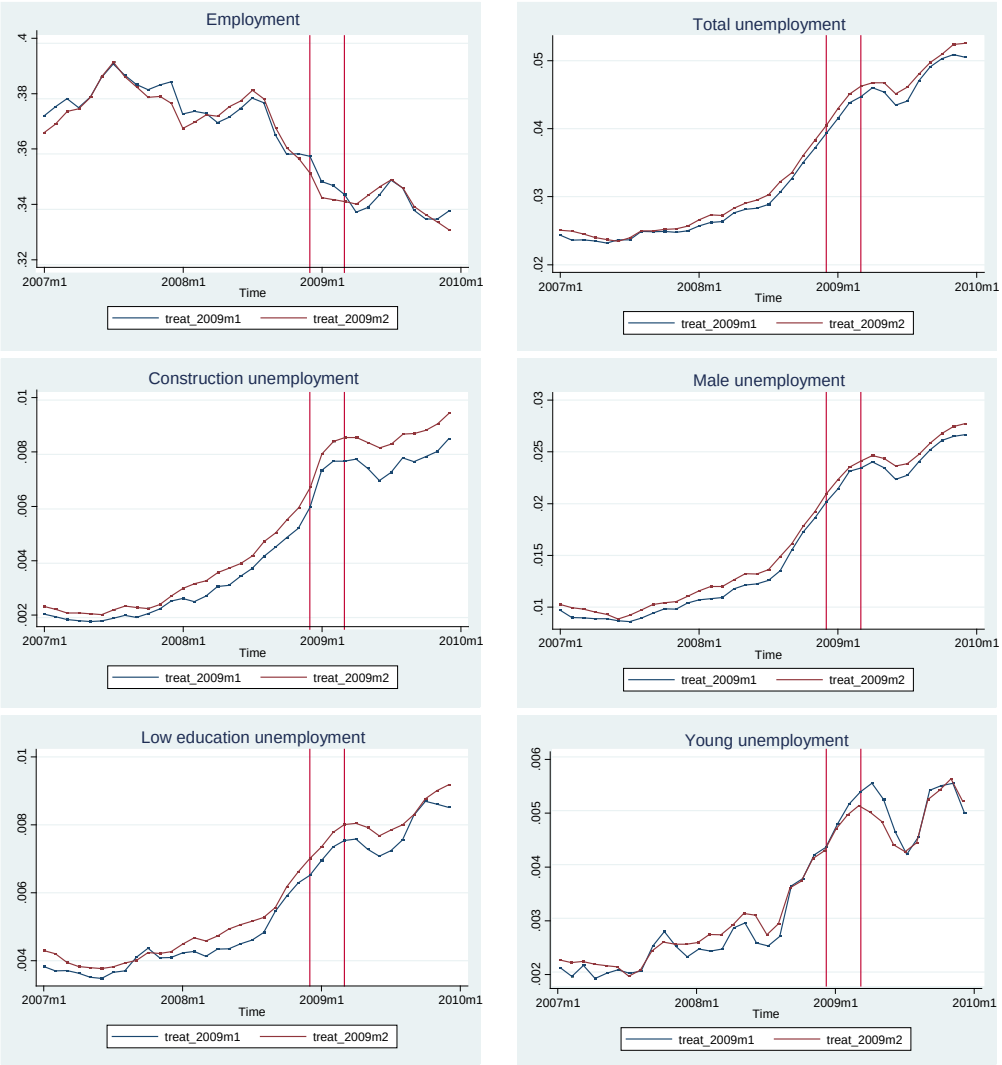
Figure 2 presents aggregate data for both employment and unemployment in Catalan municipalities, together with the (full) temporal window in which FEIL projects were approved. Starting in mid-2008, both data series show consistent change as the crisis starts and deepens, with a sharp acceleration in unemployment rates (a steep decline in employment rates). Interestingly, during the period when the FEIL projects were being approved, employment rates keep decreasing as unemployment rates increase. However, it can be clearly seen that immediately following the program approval period (i.e., the period when the projects were likely being implemented), both data series experienced a substantial change. My objective is to determine if that change is a meaningful result of the FEIL fund. If it is, my goal is then to determine if this change in labor outcomes also affected crime at the local level.

Figure 2. Labor Market Outcomes and the FEIL Approval Period

With respect to labor outcomes, in the *Ministerio de Política Territorial* (Ministry of Territorial Policy, 2010) reported that FEIL projects provided employment to a total of 426,195 people in Spain, of whom 59,693 (14.0 percent) were located in Catalonia. In 2009, this figure represented approximately 12 percent of the overall unemployed population in that region, a remarkable impact.

Figure 3 presents further on labor outcomes, this time by selected types of unemployment and treatment status.[17] Note that all types of unemployment (by industry, gender, education, and age) increased starting in mid-2008, particularly registered unemployment in the construction industry.[18] More importantly for us, the main variables of interest do not seem to differ with respect to treatment status, which is important for my identification strategy.

Figure 3. Employment and Unemployment Rates, and Treatment Status



[17] In Figure 3 I only report one type of unemployment in the gender (male), sectoral (construction), education (low) and age (young) categories that I consider as most relevant for the discussion below.

[18] Regarding unemployment figures by age I have constructed three categories: young (less than 25 years old); middle-age (25-40 years old) and mature (more than 40 years old).

In both Figures 2 and 3, unemployment rates are shown to be increasing more slowly (and even to decrease temporarily). This suggests that the FEIL fund may have buffered employment (and reduced registered unemployment) against the sharp economic downturn that began in mid-2008. Against this backdrop, the empirical strategy will assess if this relationship is statistically robust and can be seen as causal.

4.2 Crime Data

I use a non-public dataset containing all crimes recorded by the *Mossos d'Esquadra* (the autonomous police agency in Catalonia), which is responsible for preventing and solving crimes, and specialized crime investigation in the Catalan region. This dataset holds reports filed by both citizens and the *Mossos d'Esquadra*, as well as local police forces, who are primarily responsible for urban traffic and upholding municipal laws and ordinances.

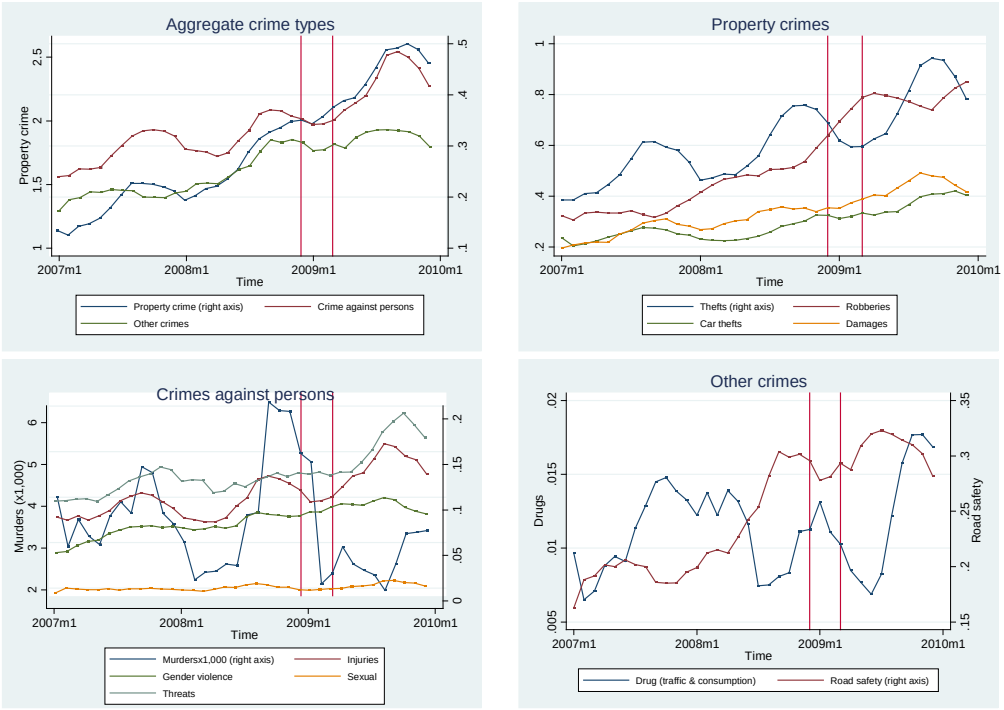
The dataset records at what time the crime takes place (if known), where it takes place, and the type of crime committed. The dataset extends from January 2007 to December 31, 2009.[19] Illegal activities are classified in accordance with the roughly 190 articles of the Spanish penal code. However, to reduce the number of categories without causing an aggregation bias that might undermine my estimates (Cherry and List, 2002), I combined some of these articles, taking care not to aggregate crimes with different offender motivations. I ended up with three main categories: property crimes (with a clear economic return), crimes against persons, and other types of crimes.

For property crimes (84 percent of all recorded crimes in Catalonia during the 2007–09 period) I calculated the number of "Thefts", "Robberies", "Car thefts", and "Damages". Thefts, the misappropriation of others' belongings without resorting to any type of violence, are by and large the most common type of recorded crime, with approximately 43.7 percent of all recorded felonies. Robberies (14.3 percent of the total) entail some sort of violent behavior by offenders, hence they would be classified as a mix between property crimes and crimes against persons, although the original definition of a robbery is to take property unlawfully.

[19] The use of monthly data is a great advantage in the present setup. First, because criminal behavior varies greatly depending on when it takes place, as opposed to where it takes place. Secondly, as noted by Felson and Poulsen (2003), monthly crime cycles are well-known periodicities among criminologists (see, for example, Harries, 1980). They make it possible to analyze how quickly delinquency responds to changes in the environment, changes that are usually evened out in yearly data.

The main crimes involving interpersonal violence, which I have called crimes against persons (11 percent of all recorded crimes in Catalonia in the 2007–09 period) include "Murders" (0.1 percent); "Injuries" (3.8 percent); "Gender violence" (3 percent); "Sexual assaults" (0.4 percent); and "Threats" (3.7 percent). A final aggregate category of other crimes (5 percent of all recorded crime in Catalonia in the 2007–09 period) include "Drug" consumption or trafficking (0.6 percent), and crimes against "Road safety" (4.4 percent).

Figure 4. Trends in Local Crime Rates



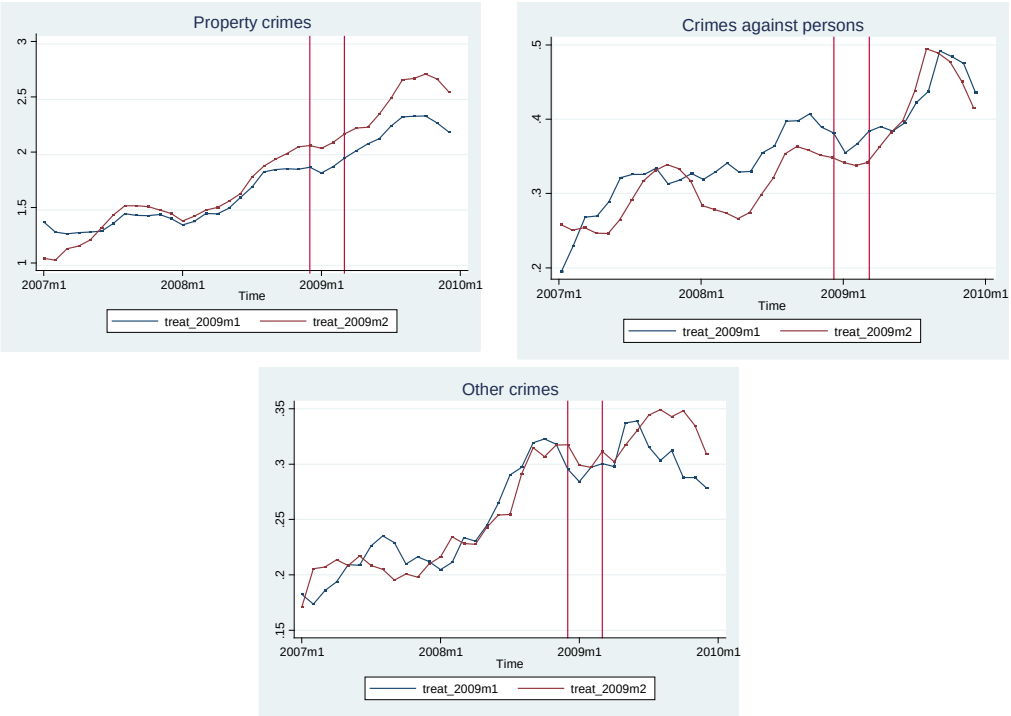
Note: Crime series data have been smoothed.

Figures 4 and 5 provide graphical evidence on the development of each type of criminal behavior and how it changes according to the type of municipality treated. The general upward trend of recorded crime is clear for nearly all types of crime. (Obviously, murders are somewhat more random than the other types of crime, given that they are quite rare in Catalonia). Therefore, during the crisis there was the perception—perhaps fueled by the media—that crimes with a clear economic return had increased. For instance, copper wire and machinery in rural areas suddenly became very attractive to thieves, as did businesses—many of which were located outside town centers. There was also the perception that robberies and thefts had increased at

private properties in remote areas or in small municipalities. Consequently, the overall spike in thefts and robberies could have had an important impact on the economy.[20]

The data for the period under study seem to confirm this upward trend in crime rates in Catalonia. Note that, again, it seems that there is no differential trend for municipalities with respect to their treatment status, and this is good news for my identification strategy. Similar to employment and unemployment figures, the crime data can be seen on a downward trend following the approval period of the FEIL fund; this reverses the recent trend observed in the data series. In this respect, when looking at the various types of crime in a disaggregated way, each crime type experiences turning points at different times, compared with the others (see, for instance, Thefts and Robberies in Figure 4). However, when the various types of crime are analyzed in the aggregate, these differences even out.

Figure 5. Crime Rates and Treatment Status



Note: Crime series data have been smoothed.

[20] http://www.lavanguardia.com/vida/20120124/54245296947/se-disparan-robos-con-fuerza-en-empresas-y-tirones-en-catalunya-en-2011.html

In order to ease the reading of the paper, I report in the main results section the results obtained for property crimes, that is, those which in principle are expected to be more affected by the change in the economic perspectives (in terms of increase in labor opportunities) as a result of the FEIL funds. The full set of results for crimes against persons and other types of crimes is presented in Appendix B.

5. Identification Strategy and Econometric Specifications

The empirical specification aims to unveil the causal relationship between the unemployment rate (key variables of local development) and crime, using the FEIL funds as an exogenous shock to labor outcomes and, hence, to crime. The structural equation is

$$Crime_{it} = \beta(Labor\ Outcome)_{it} + \eta_t + \mu_i + \varepsilon_{it}, \quad (1)$$

which, of course, can suffer from all the known problems that make OLS estimates inadequate: from measurement errors (or omitted variables affecting both variables of interest) to reverse-causality problems. In this equation, *i* indexes municipalities and *t* indexes months. *Labor Outcome*$_{it}$ is the outcome of interest: either employment rates or unemployment rates (by industry, gender, education level, and age). In this setup, there is a set of first-stage regressions that estimate the effects of the FEIL fund on the labor outcomes of interest.

$$Labor\ Outcome_{it} = \delta FEIL_{it} + \eta_t + \mu_i + u_{it}, \quad (2)$$

FEIL$_{it}$ is an indicator for the municipality receiving the funds and implementing the project's investment at time *t*.[21] As explained previously, the main variable of interest takes into account not only the month of approval, but also the tender period by project type (size), as explained in Section 3. The sample is restricted to those municipalities that experienced project approvals in only one of the two-month windows of project approvals selected, and with projects of the same size. As a result, identifying the parameter of interest, δ, requires only that the timing of approvals be uncorrelated with time-varying, unobserved factors that themselves generate outcomes of interest. If the sample included municipalities that were not treated under the FEIL program, the identifying assumption would be more restrictive and require that both when and if

[21] Given how the *FEIL*$_{it}$ indicator is constructed, equation (2) can be easily understood as a diff-in-diff setup.

a municipality receives the public investment to be uncorrelated with trends in these unobserved factors that could also generate outcomes.

An important aspect of my setup is the timing of events. On the one hand (as explained previously in Section 3.2), I deal with varying tender procedures depending on the size of the project, as defined by my main variable of interest, $FEIL_{it}$. However, there could be some uncertainty regarding when the projects were implemented, or at least when they began to have an impact on labor outcomes and crime rates. Projects can be characterized by different time profiles when hiring personnel, i.e., a project can be more labor intensive at a particular stage of its development. To properly address this issue, I shall use $FEIL_{it}$ variable leads to account for the projects' varying time impacts. As previously explained, the number of leads used is conditioned by the fact that, in late 2009, there was a second call for projects: the FEES fund.[22] Therefore, a five-month-forward period, coupled with a three-month tender period (for projects with amounts above €200,000), for a FEIL project approved in February 2009 leads to FEIL having an estimated impact up to October 2009.

In order for the abovementioned identification strategy to work out, several conditions need to be met. First, the FEIL variable must not be correlated with the error term of the structural equation; that is, it must be uncorrelated with other (omitted) variables that could also affect crime rates. If there is little doubt that my instrument (the FEIL fund) affects the endogenous variable given that it was a legal requirement of the program, what else could the FEIL fund have affected in those municipalities and with such deterministic timing? Or, more generally, what other potentially invalidating factors could have changed during that particular period in those specific municipalities (in response to the local public investment plan) and in such a way that also could have impacted crime significantly?

It is difficult to find one socioeconomic factor that changes so much at the municipal level and on a monthly basis. Two factors represent potential threats to my identification strategy: population dynamics and the outcomes of the projects themselves. Regarding population dynamics, it is quite unlikely for people to change residences simply because the FEIL fund has been approved and implemented, especially in such a short time. For one thing, construction workers—those most prone to be hired under the FEIL fund—and the population in

[22] In this sense, workers and firms with prior FEIL experience could anticipate the impact of the new FEES funds and, hence, this could introduce some noise into our estimates.

general, did not frequently check out the Spanish government's Web site (or, for that matter, the Web site of the Ministry responsible for managing the funds) to find out when projects were approved. Nor did they know what tender procedures to follow in order to be able to estimate when a project would be implemented in the municipality. Although there was news coverage of the program's launch,[23] it is implausible to assume that individuals were aware of the projects submitted by each municipality, when these projects were approved, and how tender procedures worked. Consequently, it is also implausible to assume that these individuals would decide to move in order to increase their chances of being hired to work on a project. Moreover, I rule out any possible effect induced by population inflows of unofficially registered individuals ("*call-effect*") in the municipality, given that they could not be registered as unemployed (people without a work permit). Hence, the municipality could not justify hiring them in the liquidation of the FEIL projects before the Ministry of Public Administration (legal requirement of the call).

Second, one could argue that the type of projects themselves could also reduce crime rates once they were built. This is so if I assume that local development projects were aimed at improving municipal infrastructure provision, both productive and socially useful, and as explained in the literature review section, there is evidence linking a better "local environment" (broadly defined) and crime. In this sense, even if I admit that this channel could be possible, it would be expected some time for "social capital" to react to FEIL projects being implemented. Moreover, I believe those "social capital" effects would be very difficult to find in the (short-run) time profile I propose in my estimations. For instance, the construction of a sports or recreational center can improve the amenities of a given municipality and improve social conditions with an expected positive (reducing) impact in crime rates once it is fully operative; therefore, it is reasonable to expect some time before such effects are observed while the impacts of such project on labor outcomes are expected to be observed from the very beginning of the construction of the local infrastructure.

[23] News coverage of the FEIL plan includes the following articles:
27 November 2008 (http://elpais.com/diario/2008/11/28/economia/1227826801_850215.html),
12 January 2009 (http://elpais.com/diario/2009/01/13/economia/1231801205_850215.html)
10 February 2009 (http://www.expansion.com/2009/02/09/economia-politica/1234218553.html).

6. Results

6.1 First-Stage Results: The Impact of FEIL Fund on Labor Outcomes

I start this results section by looking in more detail at my FEIL variable which is meant to provide exogenous variation in time and municipality and, hence, which will allow me to identify causal effects across the main variables of interest: labor outcomes and crime rates. Table 3 reports the balancing t-tests of municipal characteristics depending on month of approval. The results presented in Table 3 are completed with the evidence provided in Appendix A, where I present further evidence on the determinants of the probability of a project being approved in a specific month. Both pieces of evidence point in the same direction: it seems that the approval month of the projects was random or, at least, the Central Government did not take into account relevant municipal characteristics, especially regarding labor outcomes and crime rates, in the approval procedure of projects.

Table 3. T-tests for Balancing Characteristics on Treatment Status

VARIABLES	**2009m01**	**2009m2**	**t-test**
Average project characteristics			
Amount per project	387,228,9	292,338.7	1.73*
	(576,832)	(393,723.8)	[0.083]
Number of projects	1.69	1.77	-0.45
	(1.51)	(1.48)	[0.652]
Labor market conditions			
Employment rate	0.376	0.373	0.09
	(0.185)	(0.218)	[0.925]
Unemployment rate	0.0346	0.0346	-0.024
	(0.013)	(0.012)	[0.980]
Male unemployment rate	0.0155	0.0161	-0.797
	(0.006)	(0.005)	[0.425]
Female unemployment rate	0.0190	0.0185	0.581
	(0.007)	(0.007)	[0.561]
Construction unemp. rate	0.0045	0.0048	-0.944
	(0.002)	(0.03)	[0.345]
Industry unemp. rate	0.008	0.009	-0.678
	(0.005)	(0.006)	[0.497]
Services unemp. rate	0.019	0.018	0.783
	(0.008)	(0.007)	[0.461]
Agriculture unemp. rate	0.001	0.001	0.972
	(0.001)	(0.001)	[0.331]
Low education unemp. rate	0.005	0.006	-0.813
	(0.003)	(0.003)	[0.416]
High education unemp. rate	0.028	0.028	0.244
	(0.011)	(0.010)	[0.807]
Young unemp. rate	0.003	0.003	-0.504
	(0.001)	(0.001)	[0.614]
Middle age unemp. rate	0.014	0.013	0.684
	(0.006)	(0.005)	[0.493]
Mature age unemp. rate	0.017	0.017	-0.455
	(0.007)	(0.007)	[0.649]

Table 3., continued

VARIABLES	2009m01	2009m2	t-test
Crime rates			
Car thefts	0.253	0.315	-0.604
	(0.276)	(0.948)	[0.546]
Thefts	0.539	0.593	-0.610
	(0.469)	(0.786)	[0.541]
Robberies	0.587	0.571	0.261
	(0.471)	(0.516)	[0.794]
Damages	0.362	0.308	1.455
	(0.267)	(0.321)	[0.146]
Murders	0.003	0.003	-0.169
	(0.014)	(0.017)	[0.844]
Injuries	0.108	0.099	0.617
	(0.100)	(0.124)	[0.537]
Gender violence	0.095	0.084	1.019
	(0.095)	(0.084)	[0.308]
Sexual offenses	0.012	0.011	0.298
	(0.023)	(0.025)	[0.765]
Threats	0.151	0.136	0.983
	(0.116)	(0.127)	[0.326]
Drugs	0.010	0.013	-0.658
	(0.025)	(0.042)	[0.510]
Road safety	0.258	0.265	-0.141
	(0.292)	(0.386)	[0.887]
Census data			
2001 Degradation rate	0.150	0.135	0.048
	(0.153)	(0.132)	[0.373]
2001 Old population rate	0.210	0.218	-1.070
	(0.067)	(0.058)	[0.284]
2001 Young population rate	0.196	0.192	0.475
	(0.067)	(0.001)	[0.634]
2001 Illiteracy rate	0.008	0.008	-0.063
	(0.001)	(0.001)	[0.949]
2001 Employment rate	0.443	0.436	1.146
	(0.048)	(0.046)	[0.252]
2001 Unemployment rate	0.033	0.032	0.291
	(0.016)	(0.013)	[0.771]
Budget data			
Deficit per capita	-12.24	-8.55	-0.091
	(217.79)	(360.57)	[0.926]
Public investment (%)	0.304	0.311	-0.358
	(0.155)	(0.168)	[0.720]
Security expenditures (%)	0.075	0.073	0.248
	(0.073)	(0.068)	[0.804]

Notes: Standard deviations in brackets; p-values in square brackets. T-test computed for the sample of 348 municipalities with more than 500 inhabitants, all projects approved in one month and all projects of the same size. For this subsample there are 91 municipalities with projects approved in January 2009 and 257 municipalities with projects approved in February 2009. All variables are measured in year 2008 except average project variables and Census variables which are measured in 2001. The results are robust when using the sample of 539 municipalities with more than 500 inhabitants and all projects approved in one month (with 142 municipalities with projects approved in January 2009 and 397 municipalities with projects approved in February 2009).

Once I have determined that the FEIL variable is uncorrelated with labor outcomes and crime rates of the affected municipalities and, hence, that is indeed exogenous, I present the first-stage results of the IV estimation strategy; those results are drawn from the estimation of equation (2).

Results reported in Table 4 show that the FEIL variable is a very strong and precise predictor of the evolution of the unemployment rate at the municipal level. Regarding the results for employment rates, these are less robust than those obtained for unemployment for the specific sub-sample of municipalities used in Table 4 but show the expected sign in the first two months of the FEIL execution. Further results, not reported but available upon request, show positive estimates of the FEIL variable (in the initial periods of the execution of the projects) on employment rates for all municipalities with projects of the same size and for municipalities with more than 500 inhabitants regardless of the size of the project approved.

The results on unemployment rates are by far more robust and give us more insights on the type of unemployment that the FEIL funds reduced and when that reduction occurred. First, it can be seen that the first month after the approval and tender procedure, and hence with the expected execution of the project, nearly all types of unemployment were reduced, with a higher point estimate for male, construction, low-educated and middle-age registered unemployed workers.

Table 4. First Stage Estimates: Impact of FEIL Projects on Labor Outcomes

VARI-ABLES	*Employ.*	*Total Unemp.*	*Male Unemp.*	*Female Unemp.*	*Constr. Unemp.*	*Serv. Unemp.*	*Indust. Unemp.*	*Agric. Unemp*	*Low Edu. Unemp.*	*High Edu. Unemp.*	*Young Unemp.*	*Mid-age Unemp.*	*Mature Unemp.*
$FEIL_t$	0.0172	**-0.1141*** **	**-0.1345*** **	**-0.0700* **	**-0.1656*** **	**-0.0772** **	-0.0556	**-0.1009* **	**-0.1077** **	**-0.1001*** **	-0.0724	**-0.1289*** **	**-0.0754** **
	(0.0131)	**(0.0348)**	**(0.0421)**	**(0.0359)**	**(0.0571)**	**(0.0372)**	(0.0432)	**(0.0536)**	**(0.0494)**	**(0.0356)**	(0.0542)	**(0.0416)**	**(0.0367)**
$FEIL_{t+1}$	0.0067	**-0.1023*** **	**-0.1249*** **	-0.0581	**-0.1528*** **	**-0.0781** **	-0.0409	-0.0840	**-0.1078** **	**-0.0860** **	-0.0475	**-0.1040** **	**-0.0835** **
	(0.0139)	**(0.0346)**	**(0.0427)**	(0.0355)	**(0.0564)**	**(0.0390)**	(0.0446)	(0.0571)	**(0.0499)**	**(0.0352)**	(0.0556)	**(0.0426)**	**(0.0395)**
$FEIL_{t+2}$	-0.0009	**-0.1098*** **	**-0.1502*** **	-0.0451	**-0.1741*** **	**-0.0645* **	-0.0452	-0.0386	**-0.1323*** **	**-0.0869** **	-0.0608	**-0.0903** **	**-0.1059*** **
	(0.0166)	**(0.0341)**	**(0.0424)**	(0.0343)	**(0.0549)**	**(0.0378)**	(0.0436)	(0.0633)	**(0.0469)**	**(0.0355)**	(0.0537)	**(0.0428)**	**(0.0396)**
$FEIL_{t+3}$	0.0005	**-0.1089*** **	**-0.1456*** **	-0.0484	**-0.1872*** **	**-0.0673* **	-0.0299	0.0101	**-0.0946** **	**-0.0983*** **	-0.0831	**-0.0887** **	**-0.0985** **
	(0.0195)	**(0.0355)**	**(0.0412)**	(0.0363)	**(0.0531)**	**(0.0385)**	(0.0465)	(0.0706)	**(0.0481)**	**(0.0370)**	(0.0529)	**(0.0408)**	**(0.0417)**
$FEIL_{t+4}$	-0.0004	**-0.1094*** **	**-0.1343*** **	-0.0614	**-0.1885*** **	-0.0679	-0.0128	0.0224	**-0.1152** **	**-0.0920** **	-0.0511	**-0.1171*** **	**-0.0839* **
	(0.0200)	**(0.0392)**	**(0.0441)**	(0.0402)	**(0.0541)**	(0.0414)	(0.0529)	(0.0596)	**(0.0538)**	**(0.0411)**	(0.0580)	**(0.0429)**	**(0.0456)**
$FEIL_{t+5}$	0.0139	**-0.0996** **	**-0.1178** **	-0.0606	**-0.1765*** **	-0.0586	-0.0327	0.1179	-0.0939	**-0.0874** **	-0.0218	**-0.1072** **	**-0.0836* **
	(0.0179)	**(0.0399)**	**(0.0470)**	(0.0395)	**(0.0560)**	(0.0404)	(0.0539)	(0.0946)	(0.0623)	**(0.0434)**	(0.0596)	**(0.0430)**	**(0.0489)**
Municip. FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
F-test	1,400.5***	137.5***	68.52***	179.1***	62.0***	131.0***	30.9***	28.5***	80.9***	143.6***	35,8***	95.8***	162.3***
# Obs.	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municip.	348	348	348	348	348	348	348	348	348	348	348	348	348

Notes: Standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

This profile of unemployed people, who in principle benefited from FEIL-funded projects at the local level, is persistent through time and seems to be coherent with the type of projects approved at a municipal level, many in the construction industry (conservation and maintenance of buildings, construction of new buildings and facilities for social, sport and cultural activities, rehabilitation of public spaces, etc.). Hence, a priori, the FEIL program hit the target and reduced local unemployment rates, especially in the construction industry, by about 10-12 percent. In sum, the first stage results are very robust and coherent with the story behind the FEIL fund.

6.2 Using the FEIL Fund to Identify Causal Effects: IV Results

This section discusses the IV results using the FEIL program to instrument total unemployment rates.[24] Table 5 presents detailed results for property crimes, while Appendix B contains the full set of results for crimes against persons and other types of crime.

The results generally show a significant and positive impact of unemployment on property crimes. In particular, robberies and serious car theft offenses (including some sort of violence) are the crimes that most promptly react to a decrease in unemployment rates. The higher estimated impact is obtained after three periods from the expected beginning of the projects' execution, with a sizable and significant reduction in nearly all types of property crimes rates. "Damages" is the only type of property crime that is not affected by the decrease occurring in unemployment rates as a result of the FEIL funds.

The timing observed for the estimated impacts of unemployment rates on crime rates is consistent with different size projects being executed and with some periods needed to observe a reaction on criminal rates. In Appendix B the results for crimes against persons and other types of crime show a picture consistent with what is expected and usually found in the literature (see Freedman and Owens, 2015) for these types of crimes, characterized in general by lower economic motivations: a less significant pattern of the impact of unemployment on crime. Quantitatively, and taking into account that in the first stage I estimate that the FEIL fund reduced total unemployment by 11 percent on average, a one percent increase in unemployment rates also increases total property crime by approximately 13 percentage points.[25]

[24] In Appendix C, Table C.1 shows the IV results on crime rates obtained when instrumenting the male unemployment rate with the FEIL projects.

[25] It is worth mentioning that this broad picture is also obtained when using the three month approval window (December 2008 – February 2009) and only restricting municipalities to have more than 500 inhabitants in the IV framework (see Appendix D).

Table 5. IV Estimates for the Impact of Total Unemployment Rate on Property Crime Rates

Second-stage	*Total property*	*Robberies*	*Total thefts*	*Serious thefts*	*Minor thefts*	*Total car*	*Serious car*	*Minor car*	*Damages*
(*Total_Unemp.* = $FEIL_t$)	0.7625	0.9691	0.1425	-0.2515	0.4196	0.5004	0.6571	-0.3767	0.3189
	(0.4828)	(0.5899)	(0.4362)	(0.4336)	(0.4687)	(0.4319)	(0.4316)	(0.5373)	(0.4747)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.8812	**1.6744****	0.0259	0.2423	-0.0317	**1.0855****	**1.0263****	0.3991	0.5218
	(0.5529)	**(0.7567)**	(0.4836)	(0.4532)	(0.4689)	**(0.5158)**	**(0.4945)**	(0.5591)	(0.5052)
(*Total_Unemp.* = $FEIL_{t+2}$)	**0.9080***	0.6844	0.1134	0.5444	**1.1527****	**1.2020****	**1.0360****	0.7482	-0.0862
	(0.5181)	(0.5893)	(0.4523)	(0.4775)	**(0.5775)**	**(0.5383)**	**(0.5001)**	(0.5893)	(0.4912)
(*Total_Unemp.* = $FEIL_{t+3}$)	**1.4381****	**1.0878***	**1.0781****	**1.2541****	**2.2347*****	0.0871	-0.2598	**1.0742***	-0.5180
	(0.6013)	**(0.6305)**	**(0.5499)**	**(0.5684)**	**(0.7803)**	(0.4324)	(0.4243)	**(0.6224)**	(0.5090)
(*Total_Unemp.* = $FEIL_{t+4}$)	0.7452	-0.6524	**2.2005*****	0.6363	0.7200	0.6172	0.6856	-0.0837	0.7930
	(0.5018)	(0.6114)	**(0.7611)**	(0.5319)	(0.5676)	(0.5057)	(0.4978)	(0.6058)	(0.5840)
(*Total_Unemp.* = $FEIL_{t+5}$)	0.8909	-0.0305	0.8380	-0.0158	-0.7144	**0.9073***	**0.9309****	0.1114	0.7520
	(0.5691)	(0.6313)	(0.5641)	(0.4410)	(0.4933)	**(0.4846)**	**(0.4747)**	(0.5424)	(0.5184)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348	348	348

Notes: Standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The results presented so far suggest that FEIL funds reduced unemployment rates and, as consequence, crime rates were significantly lower. Moreover, the first-stage results (see Table 4) suggest that a particular type of unemployed workers benefited from the program. In this sense, the impact of FEIL funds in the first month after the execution of the works is significant for many types of unemployment analyzed; however, it can be seen that the point estimates are higher for unemployed workers being male, in the construction industry, with low education and mostly being middle-age. This profile remains consistent when I look at the temporal impacts of FEIL funds on unemployment rates. As expected, the impact on unemployment rates is not only restricted to one period and extends in accordance with the nature of the FEIL projects, mainly local public infrastructures. When analyzing the time impacts of the FEIL funds on unemployment rates the impact remains strong for male unemployed in the construction industry being middle-age and mature; the results regarding education and age are less distinctive.

The second-stage results show that as a consequence of such reduction on unemployment rates crime rates were also significantly reduced. There was an immediate impact for property crimes such as robberies and serious car thefts, but I also find an impact on crime rates when I analyze the (instrumented) leads of my variable of interest. To interpret these results more precisely I need to figure out the various channels that could be at work to observe such results.

On the one hand, it could be case that the FEIL funds reduced unemployment rates and, hence, improved general economic conditions in the municipality, reducing overall incentives to commit crimes for both those who obtained a job and those who did not. Otherwise, and following the reasoning and evidence of Freedman and Owens (2015), if unemployment reduction only benefited a specific group of individuals and not the overall municipal population this could increase the supply of criminal opportunities for those who have not benefited from the FEIL project and, as a result, I should observe as a consequence of the reduction in unemployment rates an increase in crime rates. Given that I observe just the opposite it means that, at least, there is a counterbalancing force that reduces crime rates when unemployment is reduced (even if only a fraction of the total population receives direct benefits from FEIL funds).

On the other hand, this counterbalancing force could be that those individuals facing worse *ex ante* economic conditions (unemployed) and possibly more prone to be involved in illegal activities were precisely those who received the positive shock in their economic expectations and, as a result, decided not to engage in illegal activities. If this is the case, the

results, more in line with the traditional economics of crime literature, would point to a sort of incapacitation effect.

It is very difficult to assess the precise mechanism behind the observed results; however, the detailed crime dataset can be used to explore further the relation at stake. In this sense, if the incapacitation effect is at work, and the reduction of crime is due to the employment of those individuals with a higher probability to commit a crime, I should observe at least two things. First, that some types of crimes are significantly reduced during working hours given that now potential offenders spend most of their time in legal activities. Second, that the probability of recidivism is also reduced with a decrease in unemployment rates, or in other words, that those individuals who previously committed a crime, if they now obtain a job with FEIL funds, have a lower probability of committing an illegal activity again.

Table 6 presents the second-stage results when dividing crimes depending on the time of the day when they were committed. I divide the day into working hours (8:00 a.m. – 17:00 p.m.) and non-working hours (the rest). As Montolio and Planells (2015) show, there are differentiated hourly patterns depending on the type of crime so it is important to keep analyzing each type of crime separately.[26] The results obtained regarding the time of the day when crimes are committed provide very interesting insights. First, robberies (and to a lesser extend serious thefts) seem to be reduced basically during non-working hours, when they generally seem to be more common. Interestingly, serious car thefts (involving some sort of violence) and minor car thefts (and to a lesser extend minor thefts) are reduced during working hours, which in principle would be consistent with an increase in employment of those individuals more prone to be involved in these types of "petty crimes."[27]

[26] The authors find three general time patterns: first, crimes related to leisure activities (road safety and drug related crimes), with peaks late at night, low rates during the daytime and rates that increase as the evening progresses; second, crimes against property (robberies, thefts and damages), with high rates during working hours, especially for thefts, and low rates at night. Robberies, in particular, show a clear peak around 18:00 (related to the time when people are leaving work on weekdays); and, third, crimes involving violence (such as murder, threats, injuries, sexual or gender violence), with rates that peak in the evenings.

[27] Note that the results for minor thefts seems not to show a distinctive pattern regarding the impact of unemployment on crime and time of the day; the relation is positive and significant for both working and non-working hours especially three months after the beginning of the execution of the FEIL projects, however with a point estimate during working hours larger than for non-working hours. The low statistical significance obtained for minor car thefts could be consistent with the fact that, as also pointed out in Montolio and Planells (2015), for minor car thefts it is usually difficult to know when the crime occurred. The time of occurrence usually has to be approximated by police officers (or victims) when filing the complaint (differently than for the case of serious car thefts, where some sort of violence takes place and, hence, usually there is a direct interaction between victim and criminal, making the reported hour more likely to be close to when the crime actually happened).

Table 6. IV Estimates for the Impact of Total Unemployment Rate on Property Crime Rates: Working Hours (WH) vs. Non-Working Hours (Non-WH)

Second-stage	*Total Property*	*Total property WH*	*Total property Non –WH*	*Robberies*	*Robberies WH*	*Robberies Non-WH*	*Serious thefts*	*Serious thefts WH*	*Serious thefts Non-WH*
(*Total_Unemp.* = $FEIL_t$)	0.7625	0.2321	**1.0224***	0.9691	0.3299	**1.0410***	-0.2515	-0.3887	0.0053
	(0.4828)	(0.4459)	**(0.5409)**	(0.5899)	(0.5280)	**(0.6050)**	(0.4336)	(0.4381)	(0.4475)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.8812	-0.1030	**1.5299****	**1.6744****	0.0738	**2.1622****	0.2423	0.7782	-0.4122
	(0.5529)	(0.4944)	**(0.6839)**	**(0.7567)**	(0.5855)	**(0.8575)**	(0.4532)	(0.4939)	(0.4757)
(*Total_Unemp.* = $FEIL_{t+2}$)	**0.9080***	**0.9912***	0.5468	0.6844	0.0738	0.8518	0.5444	0.5049	0.3276
	(0.5181)	**(0.5300)**	(0.5152)	(0.5893)	(0.5453)	(0.6101)	(0.4775)	(0.4704)	(0.4790)
(*Total_Unemp.* = $FEIL_{t+3)}$	**1.4381****	**1.0148***	**1.3868****	**1.0878***	0.3313	**1.1970***	**1.2541****	0.7717	**1.1493****
	(0.6013)	**(0.5377)**	**(0.6188)**	**(0.6305)**	(0.5532)	**(0.6520)**	**(0.5684)**	(0.4950)	**(0.5659)**
(*Total_Unemp.* = $FEIL_{t+4}$)	0.7452	0.8273	0.4357	-0.6524	-0.6765	-0.3676	0.6363	0.7484	0.2233
	(0.5018)	(0.5114)	(0.5102)	(0.6114)	(0.5864)	(0.5925)	(0.5319)	(0.5408)	(0.5183)
(*Total_Unemp.* = $FEIL_{t+5}$)	0.8909	0.4418	**1.0345***	-0.0305	-0.7354	0.4988	-0.0158	0.3242	-0.3511
	(0.5691)	(0.5227)	**(0.6214)**	(0.6313)	(0.6507)	(0.6460)	(0.4410)	(0.4481)	(0.4662)
	Minor thefts	*Minor thefts WH*	*Minor thefts Non-WH*	*Serious car*	*Serious car WH*	*Serious car Non-WH*	*Minor car*	*Minor car WH*	*Minor car Non-WH*
(*Total_Unemp.* = $FEIL_t$)	0.4196	0.1780	0.5410	0.6571	0.7456	0.3470	-0.3767	-0.6278	0.0204
	(0.4687)	(0.4595)	(0.5192)	(0.4316)	(0.4810)	(0.4273)	(0.5373)	(0.5537)	(0.5219)
(*Total_Unemp.* = $FEIL_{t+1}$)	-0.0317	0.1721	-0.2689	**1.0263****	**1.2176****	0.4878	0.3991	0.6128	0.0208
	(0.4689)	(0.4772)	(0.5197)	**(0.4945)**	**(0.5634)**	(0.4522)	(0.5591)	(0.5726)	(0.5422)
(*Total_Unemp.* = $FEIL_{t+2}$)	**1.1527****	0.6708	**1.2631****	**1.0360****	**0.9397***	0.7872	0.7482	**1.1063***	0.0733
	(0.5775)	(0.5172)	**(0.6295)**	**(0.5001)**	**(0.5275)**	(0.4831)	(0.5893)	**(0.6286)**	(0.5471)
(*Total_Unemp.* = $FEIL_{t+3)}$	**2.2347*****	**2.0200*****	**1.5641****	-0.2598	-0.3482	-0.0828	**1.0742***	0.8728	0.6840
	(0.7803)	**(0.7372)**	**(0.6758)**	(0.4243)	(0.4675)	(0.4406)	**(0.6224)**	(0.5985)	(0.5752)
(*Total_Unemp.* = $FEIL_{t+4}$)	0.7200	0.1030	**1.1774***	0.6856	0.6465	0.4958	-0.0837	-0.0810	-0.0427
	(0.5676)	(0.5239)	**(0.6745)**	(0.4978)	(0.5399)	(0.4993)	(0.6058)	(0.6061)	(0.5980)
(*Total_Unemp.* = $FEIL_{t+5}$)	-0.7144	-0.2731	**-0.9582***	**0.9309****	**0.9061***	0.6444	0.1114	0.4986	-0.2591
	(0.4933)	(0.4698)	**(0.5643)**	**(0.4747)**	**(0.5119)**	(0.4586)	(0.5424)	(0.5572)	(0.5393)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month & Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348	348	348

Notes: Standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Note that I do not claim that this is the case; I only provide further evidence regarding the causal impact of unemployment on crime rates that may help in understanding the possible mechanisms at work. As expected, the results for crime against persons (see Table B.2 in Appendix B) are, as before, far less significant and, when significant, the positive impact of unemployment on crime rates is found in non-working hours, another logical result for this type of crime.

A final piece of evidence is taken from a different dataset also provided by the Catalan Police Force, *Mossos d'Esquadra*. More precisely, I make use of an offenders' dataset for the same period of analysis. In this dataset I have monthly and municipal information regarding known offenders and, importantly, if they have committed previous offenses. Hence, I can estimate in my IV framework the impact of unemployment on the probability of recidivism in a given municipality and in a given month as a result of FEIL fund implementation. Note that this dataset is completely different than the one used so far in the sense that now I turn to recorded offenders rather than the recorded number of crimes. In this sense, not all types of crime have the same properties regarding the percentage of known offenders; for some crimes it is relatively easy to know the offender (crimes against persons) in comparison to other types of crimes (property crimes). Moreover, although the number of offenders should in principle be correlated with the number of crimes, it is in fact more volatile across municipalities, and even more so on a monthly basis. Bearing these specifics in mind, Table 7 presents the IV results of the impact of unemployment on known offenders and the probability of recidivism for property crimes (see Table B.3 in Appendix B for the results regarding crimes against persons and other types of crimes).

Despite less robust results than for the case of the impact of unemployment on crime rates, the results for the probability of repeat offending also shows a positive relation with unemployment rates. Reading the obtained results in the present setup, the reduction in unemployment rates that the FEIL funds brought to municipalities also implied a lower probability of individuals' committing repeated offenses. There is a certain parallelism between the results in Table 5 and those in Table 7; note that after three months of the FEIL project's execution unemployment rates were reduced (especially for male workers in the construction industry), the total number of property crimes was reduced (especially during non-working hours) and the probability that those committing the crimes were repeat offenders was also

reduced, meaning that previous offenders were more unlikely to repeat. The results on crime against persons and other types of crime (drug-related crimes and crimes against road safety) show only a positive relation between unemployment rates and recidivism in sexual offenses and threats.

6.3 Placebo Tests

Finally, I perform various placebo tests to confirm the previous findings. First, Figure 6 shows the random assignation of municipalities to approval months; that is, I take the actual approval months but assign municipalities to each month randomly.[28] I present the results for the main labor outcomes and for total property crime rates and crimes against persons. The results of this exercise also hold for all types of labor outcomes and disaggregated crime types. Second, Figure 7 presents results for the same outcome variables but when I randomly assign the projects the previous year; that is between December 2007 and March 2008. As expected, in both cases it can be observed that there is no impact of the variable of interest on any of the relevant variables, confirming my identification strategy and the results obtained in the previous section.

Table 7. IV Estimates for the Impact of Total Unemployment Rate on Recidivism in Property Crimes

Second-stage	*Total property*	*Robberies*	*Serious thefts*	*Minor thefts*	*Serious car*	*Minor car*	*Damages*
(*Total_Unemp.* = $FEIL_t$)	**0.6314****	**0.4999****	0.0506	-0.0625	0.0708	-0.0093	**0.4293****
	(0.3048)	**(0.2207)**	(0.1498)	(0.1495)	(0.1283)	(0.0199)	**(0.2175)**
(*Total_Unemp.* = $FEIL_{t+1}$)	-0.1430	0.2416	**0.3554***	-0.2622	0.1737	-0.0097	-0.0336
	(0.2997)	(0.2133)	**(0.1824)**	(0.1703)	(0.1402)	(0.0207)	(0.1966)
(*Total_Unemp.* = $FEIL_{t+2}$)	0.3091	0.0649	-0.1640	-0.0823	0.0552	-0.0098	-0.0505
	(0.2836)	(0.1898)	(0.1623)	(0.1573)	(0.1338)	(0.0209)	(0.1986)
(*Total_Unemp.* = $FEIL_{t+3}$)	0.2911	**0.3544***	0.0302	0.1525	0.2057	-0.0097	-0.0307
	(0.2848)	**(0.2115)**	(0.1558)	(0.1597)	(0.1439)	(0.0208)	(0.1973)
(*Total_Unemp.* = $FEIL_{t+4}$)	**0.6360****	-0.1154	0.2404	-0.1859	-0.0703	**0.0493***	0.1846
	(0.3186)	(0.1936)	(0.1854)	(0.1794)	(0.1465)	**(0.0267)**	(0.2223)
(*Total_Unemp.* = $FEIL_{t+5}$)	-0.0016	-0.1055	0.1392	-0.0265	0.0449	**0.0562****	**0.3717***
	(0.3030)	(0.2122)	(0.1574)	(0.1524)	(0.1306)	**(0.0249)**	**(0.2158)**
Municipality FE	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348

Notes: Standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[28] In this exercise I assume the same type of project and, hence, I do not take into account varying tender times across projects and municipalities. Moreover, the results remain practically unchanged if I randomly assign municipalities between only two approval months.

Figure 6. Placebo Test (I): Random Assignation of FEIL Projects across Municipalities

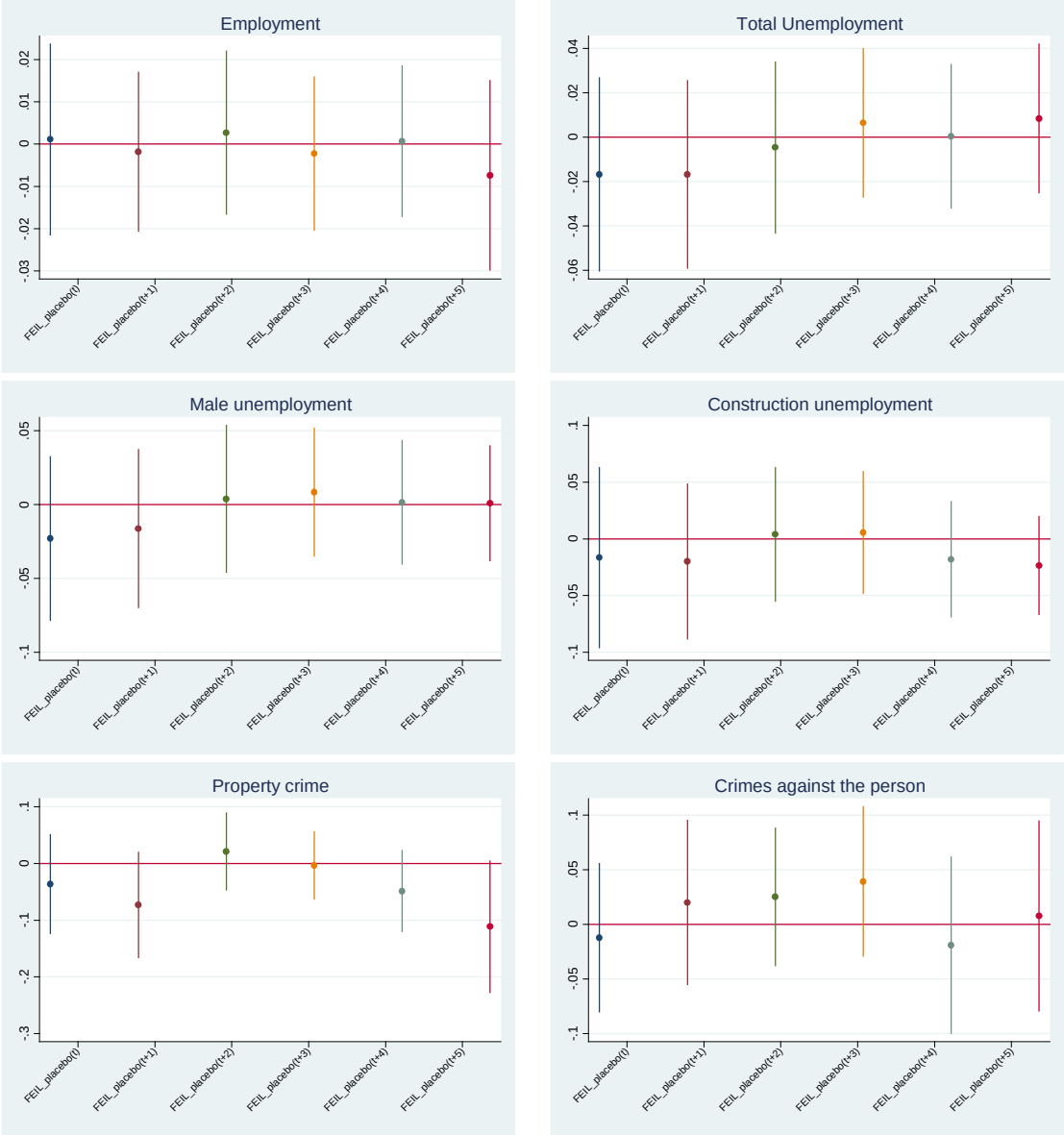
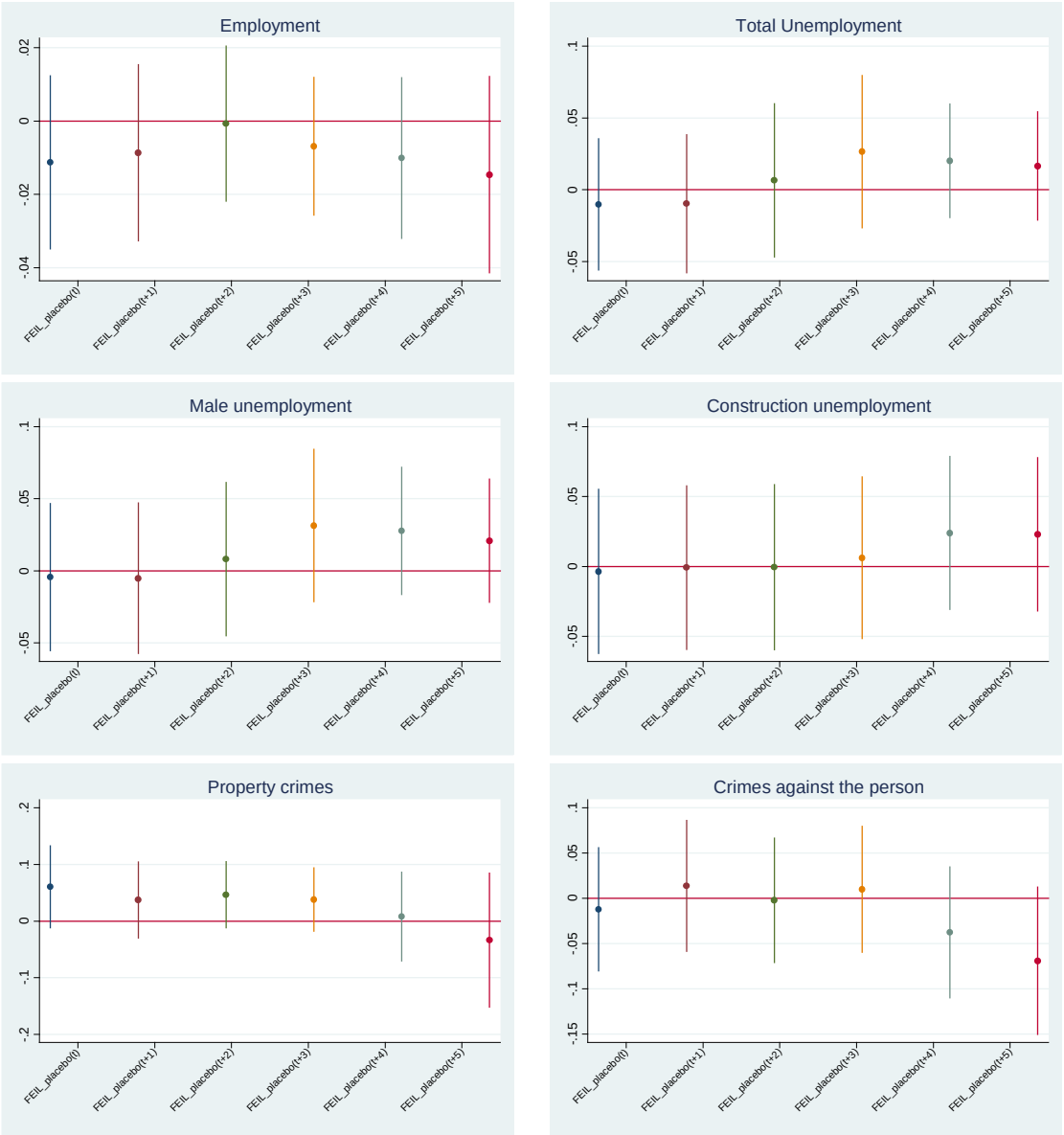


Figure 7. Placebo Test (II): Random Assignation of FEIL Projects across Municipalities and Time



7. Concluding Remarks

The relation between unemployment and crime is far from resolved in the economic literature. Many channels relate both variables in a way that makes the causality direction between them very difficult to assess together with other practical problems such as possible measurement errors in the variables of interest, timing of the events, or the level of aggregation used to approach the issue at stake. In the face of such potential problems in the estimation of the effect of unemployment on crime, a promising research strategy for obtaining unbiased estimates of labor outcomes on crime at a municipal level is to analyze the evolution of municipal crime rates in those municipalities where unemployment rates have changed due to reasons which are unrelated to their level of criminality; and this is what I have done in this paper.

The FEIL local investment fund was planned, designed and executed to increase public investment at the local level by financing the construction of new planning and immediate execution works from the beginning of 2009 and under the control and responsibility of the local authorities themselves. I show that the time of approval of FEIL funds for projects presented by municipalities was random, or at least not determined by any variable directly involved in my research strategy or that could alter the results I find. Therefore, I use the FEIL fund to properly identify the causal relation between unemployment and crime.

The obtained results seem to be in line with the "Beckerian" view of the relation between labor market opportunities and crime rates. An increase in those opportunities increases the opportunity cost of committing a crime and hence reduces the incentives to undertake illegal behaviors. As a result of the analysis undertaken I may conclude that the FEIL funds had a significant impact on registered unemployment rates, especially for unemployed people who are male, in the construction industry, low educated and with middle and mature age.

The significant results of the FEIL funds on labor outcomes have direct translation into crime rates: the reduction of unemployment rates had a positive effect on crime rates, which were also significantly reduced. The results show a clear reduction for crimes with an economic motivation and the timing of such effects resembles that expected if I take into account the nature (local public infrastructure) of the projects being executed. Moreover, I find empirical evidence that the impact on unemployment rates reduced car offenses (serious and minor) and minor thefts, especially during working hours, and it also reduced the probability that those committing property crimes were repeated offenders. Both explanations are compatible with a possible

incapacitation effect; that is, if those individuals who *ex ante* faced worse economic conditions (facing unemployment) and, hence, being more likely to be engaged in criminal activities were those who benefited from the FEIL funds, then they saw their labor opportunities improved and hence became less likely to engage in illegal activity.

I truly believe these results may help to improve the understanding of the social impact of public investment policies, especially at the local level where the benefits and costs of any public action are more easily perceived and borne by citizens. Moreover, I believe the proposed research strategy allows me to provide sound evidence on the causal relation between unemployment and crime and, hence, to improve the much-needed definition of social policy objectives into specific policies devoted to investment in infrastructure around the world.

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Appendix A. Ordered Probit for the Timing of Approval

One of the crucial points of the whole empirical setup is the assumption of random timing of approval by the Ministry of Public Administration conditional on municipal characteristics, especially labor and crime outcomes. In this appendix I provide further evidence to strengthen this point by testing an ordered Probit for the timing of approval. Let f_i denote an observable ordinal variable coded 0, 1, 2 and 3 on the basis of the month of approval of FEIL projects.[29] Let f^*_i represent an unobservable variable that captures the probability of approval for the ith municipality. The approval outcome can be expressed as a function of a vector of explanatory variables (X_i) using the following linear relationship:

$$f_i^* = \mathbf{X}_i\boldsymbol{\delta} + u_i \text{ where } u_i \sim N(0,1) \tag{A.1}$$

where $\boldsymbol{\delta}$ is a vector of unknown parameters. It is assumed that f^*_i is related to the observable ordinal variable, in general terms, f_i as follows: prob[$y_i = j$] = $\mathbf{\Phi}(\theta j - \mathbf{X}_i\boldsymbol{\delta}) - (\theta j - 1 - \mathbf{X}_i\boldsymbol{\delta})$ for j = 0, 1, 2, 3 (the approval months) where $\mathbf{\Phi}(\cdot)$ denotes the cumulative distribution function operator for the standard normal and θ_j are the so-called threshold parameters. Note that equation (A.1) is estimated for a cross-section of municipalities using six sets of potential explanatory variables. First, basic municipal characteristics such as population and the province they belong to. This last variable should be, a priori, relevant since in the approval process, as previously explained, the Provincial Central Government Delegations play an important role. Second, average project characteristics such as the average amount per project and the number of projects presented by each municipality. Third, labor market conditions in 2008. Fourth, crime rates in 2008. Fifth, census data from the 2001 Census with variables such as the municipal degradation rate (percentage of houses in bad conditions in the municipality), the percentage of young and old people, the illiteracy rate and both the employment and unemployment rate in 2001. Sixth, budgetary data for 2008 (deficit per capita, percentage of public investment and percentage of security expenses), which will try to capture possible efficiency effects coming from the municipal management.

As shown in Table A.1, it seems clear that our main variable of interest is not determined either by labor outcomes or by crime rates at the municipal level. This seems to confirm that the

[29] Note that in this robustness exercise I make use of the four months with project approvals to properly define the full set of possible approval months a given municipality could face for a given project.

FEIL fund was designed in such a way that the revision and approval stages allowed the flow of resources to reach local governments in the shortest period of time. The probability of approval does not depend on 2001 census variables (some could indicate long-lasting municipal needs) or budget variables that could be a proxy for municipal efficiency in handling budgetary issues. In fact, the only significant variables are the amount per project although with a very small impact on the probability of approval by date and being a municipality belonging to the province of Lleida. This last result is consistent with the approval procedure designed by the Ministry of Public Administration, explained in Section 3.1., which relied on Central Government Delegations at the province level to help in the verifying process of the requirements the projects should fulfill. In any case, this is not a threat to our identification strategy.

For the sake of simplicity I report in Table A.1 the estimations with the whole set of unemployment variables as regressors. The results do not change if I introduce the unemployment variables one-by-one to avoid possible problems of multicollinearity in the estimates. Moreover, results are practically unaltered if I define our dependent variable as having three categories instead of four; that is, deleting the March 2009 approval month (with very few projects and, hence, municipalities).

Table A.1. Ordered Probit Results: Determinants of Receiving Treatment in a Specific Month

VARIABLES	(1)	(2)	(3)	(4)	(5)
Barcelona provincial Gov't	-0.5178***	-0.3574**	-0.3448**	-0.2278	-0.2894
	(0.1491)	(0.1638)	(0.1681)	(0.1883)	(0.1938)
Lleida provincial Gov't	-1.2059***	-1.2552***	-1.2512***	-1.3210***	-1.4061***
	(0.1532)	(0.1620)	(0.1669)	(0.1694)	(0.1714)
Girona provincial Gov't	-0.0906	-0.0224	-0.0261	0.0503	-0.0086
	(0.1706)	(0.1718)	(0.1800)	(0.1935)	(0.1970)
Population	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Average project characteristics					
Amount per project	-0.0000***	-0.0000**	-0.0000**	-0.0000*	-0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Number of projects	-0.0092	-0.0096	-0.0084	0.0002	-0.0024
	(0.0184)	(0.0171)	(0.0187)	(0.0197)	(0.0198)
Labor market conditions					
Employment rate		-0.1550	-0.2240	-0.1206	-0.1189
		(0.1890)	(0.1857)	(0.1960)	(0.2067)
Unemployment rate		-20.9408	-21.0942	-17.9013	-25.4693
		(28.4876)	(28.7532)	(28.8283)	(29.2803)
Male unemployment rate		5.5369	2.5886	-0.2319	-1.9965
		(16.7166)	(16.9254)	(17.0121)	(17.4104)
Construction unemp. rate		33.6607	35.6688	30.7633	45.2223
		(30.5819)	(30.9418)	(31.1800)	(31.7791)
Industry unemp. rate		-0.4592	2.7979	-3.7375	6.0730
		(26.8193)	(26.9927)	(27.1050)	(27.5862)
Services unemp. rate		-0.0604	1.1209	1.4547	7.3441
		(26.0624)	(26.3158)	(26.9407)	(27.7356)
Low education unemp. rate		6.0875	5.4231	8.0572	8.4336
		(15.1005)	(15.2341)	(15.5131)	(15.5513)
Young unemp. rate		10.7357	14.9003	24.1831	26.1445
		(28.4690)	(28.6837)	(28.7027)	(29.4703)
Middle age unemp. rate		12.2232	12.6612	16.8713	16.8040
		(13.4383)	(13.4971)	(13.6482)	(14.0788)
Crime data					
Car thefts	0.0005		0.0007	-0.0000	-0.0000
	(0.0016)		(0.0016)	(0.0016)	(0.0016)
Thefts	-0.0008		-0.0010	-0.0008	-0.0009
	(0.0009)		(0.0009)	(0.0009)	(0.0009)
Robberies	0.0020		0.0024	0.0029	0.0032
	(0.0021)		(0.0021)	(0.0022)	(0.0021)
Damages	-0.0014		-0.0016	-0.0015	-0.0013
	(0.0034)		(0.0034)	(0.0034)	(0.0034)
Murders	0.0886		0.0983	0.0913	0.0683
	(0.0886)		(0.0883)	(0.0872)	(0.0882)
Injuries	0.0154*		0.0152	0.0142	0.0149
	(0.0093)		(0.0095)	(0.0095)	(0.0093)
Gender violence	-0.0139*		-0.0135*	-0.0155**	-0.0148*
	(0.0073)		(0.0074)	(0.0076)	(0.0076)
Sexual offenses	0.0626		0.0571	0.0538	0.0621
	(0.0463)		(0.0466)	(0.0469)	(0.0467)
Threats	-0.0076		-0.0077	-0.0070	-0.0088
	(0.0085)		(0.0086)	(0.0086)	(0.0085)
Other types of crime	0.0022		0.0022	0.0026	0.0025
	(0.0033)		(0.0033)	(0.0033)	(0.0034)

Table A.1. (cont.)

VARIABLES	(1)	(2)	(3)	(4)	(5)
Census data					
2001 Degradation rate				0.3179	0.3612
				(0.3480)	(0.3629)
2001 Old population rate				3.6856**	3.5373**
				(1.4360)	(1.4728)
2001 Young population rate				43.8677	36.6314
				(37.8551)	(38.8848)
2001 Illiteracy rate				0.1540	0.3278
				(0.6599)	(0.7117)
2001 Employment rate				0.1443	0.4027
				(1.4922)	(1.5674)
2001 Unemployment rate				-2.5488	-2.5154
				(3.3171)	(3.4361)
Budget data					
Deficit per capita					0.0000
					(0.0001)
Public investment (%)					-0.2039
					(0.2775)
Security expenditures (%)					-0.0007
					(0.0007)
θ_1	-3.0217***	-3.2323***	-3.2625***	-1.8187	-1.9758*
	(0.1948)	(0.2502)	(0.2550)	(1.1094)	(1.1423)
θ_2	-1.3161***	-1.5337***	-1.5402***	-0.0785	-0.2327
	(0.1418)	(0.2022)	(0.2090)	(1.1121)	(1.1455)
θ_3	2.2609***	2.0643***	2.0702***	3.5665***	3.4366***
	(0.2152)	(0.2593)	(0.2668)	(1.1385)	(1.1718)
Pseudo-R2	0.1048	0.1053	0.1128	0.1233	0.1325
Observations	872	872	872	872	852

Notes: Dependent variable is an ordered variable taking values 0 if project was approved in 2008m12, 1 if project was approved in 2009m01, 2 if project was approved in 2009m02, 3 if project was approved in 2009m03. Results remain practically unaltered if I delete the last category of the dependent variable (projects approved in 2009m3). The Tarragona provincial Delegation of the Central Government is omitted. All variables are measured in year 2008 except average project variables and census variables which are measured in 2001. "*Female unemployment rate*," "*Agriculture sector unemployment rate*," "*High education unemployment rate*," "*Mature unemployment rate*" are the omitted categories to avoid perfect collinearity. Crime variables are expressed per 1,000 inhabitants. All rates are calculated using municipal population. "*2001 Degradation rate*" indicates the percentage of houses in bad conditions in the municipality. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B. IV Results for Crimes against Persons and Other Types of Crimes

Table B.1. IV Estimates for the Impact of Total Unemployment Rate on Crime against Persons and Other Types of Crime

Second stage	*Total person*	*Murder*	*Injuries*	*Gender violence*	*Sexual*	*Threats*	*Total other crimes*	*Drugs*	*Road Safety*
(*Total_Unemp.* = $FEIL_t$)	0.0241	-0.7722	-0.0011	0.4433	-0.3227	-0.0425	-0.1252	-0.5070	-0.0376
	(0.5011)	(0.4730)	(0.4664)	(0.5927)	(0.6268)	(0.4866)	(0.4227)	(0.4969)	(0.4198)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.4892	0.1673	-0.3632	0.8762	-0.1019	0.5412	-0.1200	-0.0338	-0.1162
	(0.5340)	(0.4464)	(0.4957)	(0.6467)	(0.6471)	(0.5231)	(0.4391)	(0.4994)	(0.4371)
(*Total_Unemp.* = $FEIL_{t+2}$)	**1.6736****	0.1389	**1.1320****	0.8652	0.4401	**0.9388***	0.7024	0.8586	0.5628
	(0.6858)	(0.4495)	**(0.5735)**	(0.6511)	(0.6654)	**(0.5652)**	(0.4819)	(0.5571)	(0.4658)
(*Total_Unemp.* = $FEIL_{t+3}$)	**1.0063***	0.2048	0.4444	**1.3639***	**1.2515***	0.0351	-0.0907	-0.0618	-0.0814
	(0.5833)	(0.4492)	(0.4994)	**(0.7060)**	**(0.7377)**	(0.5071)	(0.4403)	(0.5014)	(0.4382)
(*Total_Unemp.* = $FEIL_{t+4}$)	0.1450	0.0252	0.7175	-0.8934	1.1364	-0.1237	-0.0809	0.2209	-0.1214
	(0.5749)	(0.4900)	(0.5725)	(0.7234)	(0.7947)	(0.5591)	(0.4836)	(0.5553)	(0.4821)
(*Total_Unemp.* = $FEIL_{t+5}$)	**1.0712***	0.3476	-0.0869	0.2582	-0.2774	**1.5707****	0.2781	**-0.9690***	0.4545
	(0.5812)	(0.4476)	(0.4791)	(0.6010)	(0.6414)	**(0.6438)**	(0.4389)	**(0.5534)**	(0.4472)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348	348	348

Notes: Standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B.2. IV Estimates for the Impact of Total Unemployment Rate on Crimes against Persons and Other Crime Rates: Working Hours (WH) vs Non-Working Hours (Non-WH)

Second-stage	*Total person*	*Total person WH*	*Total person Non-WH*	*Gender violence*	*Gender violence W*	*Gender violence Non-WH*	*Total other*	*Total other WH*	*Total other Non-WH*
(*Total_Unemp.* = $FEIL_t$)	0.0241	-0.0450	0.0875	0.4433	0.5142	0.1321	-0.1252	0.0642	-0.1787
	(0.5011)	(0.4693)	(0.5317)	(0.5927)	(0.5838)	(0.5927)	(0.4227)	(0.4684)	(0.4176)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.4892	0.1811	0.5535	0.8762	-0.0405	**1.2446***	-0.1200	-0.4698	0.1007
	(0.5340)	(0.4888)	(0.5693)	(0.6467)	(0.5914)	**(0.6952)**	(0.4391)	(0.5041)	(0.4329)
(*Total_Unemp.* = $FEIL_{t+2}$)	**1.6736****	**1.0099***	**1.4576****	0.8652	0.4587	0.7649	0.7024	0.4715	0.5768
	(0.6858)	**(0.5586)**	**(0.6768)**	(0.6511)	(0.6084)	(0.6508)	(0.4819)	(0.5057)	(0.4644)
(*Total_Unemp.* = $FEIL_{t+3}$)	**1.0063***	-0.0188	**1.5758****	**1.3639***	0.2304	**1.6642****	-0.0907	0.4269	-0.3243
	(0.5833)	(0.4892)	**(0.6924)**	**(0.7060)**	(0.5965)	**(0.7574)**	(0.4403)	(0.5006)	(0.4412)
(*Total_Unemp.* = $FEIL_{t+4}$)	0.1450	-0.3122	0.5729	-0.8934	-0.9553	-0.3416	-0.0809	-0.3954	0.1082
	(0.5749)	(0.5467)	(0.6292)	(0.7234)	(0.7124)	(0.6881)	(0.4836)	(0.5506)	(0.4776)
(*Total_Unemp.* = $FEIL_{t+5}$)	**1.0712***	0.4254	**1.1798***	0.2582	-0.0896	0.4390	0.2781	0.6241	0.0044
	(0.5812)	(0.4920)	**(0.6233)**	(0.6010)	(0.5841)	(0.6163)	(0.4389)	(0.5061)	(0.4261)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348	348	348

Notes: standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Similar results to those obtain for gender violence are also obtained for injuries and sexual assaults, that is, a positive impact of unemployment rates on these types of crimes against persons during non-working hours.

Table B.3. IV Estimates for the Impact of Total Unemployment Rate on Recidivism on Crimes against Persons and Other Types of Crime

Second-stage	*Total person*	*Murder*	*Injuries*	*Gender violence*	*Sexual*	*Threats*	*Total other crimes*	*Drugs*	*Road Safety*
(*Total_Unemp.* = $FEIL_t$)	**0.5339***	-0.0126	0.1980	0.2358	-0.0406	0.0522	-0.1140	0.0058	-0.1190
	(0.3092)	(0.0559)	(0.2087)	(0.2321)	(0.0995)	(0.2352)	(0.2728)	(0.1035)	(0.2564)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.2474	-0.0197	-0.1094	0.1353	0.0588	**0.4771***	0.0809	-0.0340	0.0487
	(0.2971)	(0.0582)	(0.2143)	(0.2359)	(0.1042)	**(0.2727)**	(0.2824)	(0.1079)	(0.2645)
(*Total_Unemp.* = $FEIL_{t+2}$)	0.3713	-0.0194	0.2975	0.3328	-0.0460	**0.7861****	0.2512	**0.3053****	0.4444
	(0.3080)	(0.0587)	(0.2262)	(0.2510)	(0.1044)	**(0.3212)**	(0.2918)	**(0.1363)**	(0.2916)
(*Total_Unemp.* = $FEIL_{t+3}$)	0.2209	0.0527	0.1897	0.3071	0.0283	0.3858	-0.0956	0.0619	-0.1374
	(0.2969)	(0.0599)	(0.2172)	(0.2476)	(0.1036)	(0.2638)	(0.2841)	(0.1092)	(0.2681)
(*Total_Unemp.* = $FEIL_{t+4}$)	0.2409	-0.0217	0.0756	0.2373	0.0126	0.4154	0.3445	**0.2808***	0.2447
	(0.3272)	(0.0642)	(0.2338)	(0.2661)	(0.1135)	(0.2931)	(0.3266)	**(0.1448)**	(0.2997)
(*Total_Unemp.* = $FEIL_{t+5}$)	**0.7852****	-0.0190	0.3649	-0.1807	-0.0443	**0.7042****	0.3445	0.0960	-0.0732
	(0.3505)	(0.0574)	(0.2279)	(0.2363)	(0.1021)	**(0.3008)**	(0.2922)	(0.1091)	(0.2616)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348	348	348

Notes: Standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C. IV Results Using Male Unemployment

Table C.1. IV Estimates for the Impact of Male Unemployment Rate on Crime Rates

Second stage	*Total property*	*Robberies*	*Total thefts*	*Total car*	*Damages*	*Total person*	*Murder*	*Gender Violence*	*Total other Crimes*
(*Male_Unemp.* = $FEIL_t$)	0.6471	0.8224	0.1210	0.4246	0.5577	0.0205	-0.6553	-0.0009	-0.1062
	(0.4063)	(0.5011)	(0.3694)	(0.3650)	(0.3650)	(0.4253)	(0.4037)	(0.3958)	(0.3589)
(*Male_Unemp.* = $FEIL_{t+1}$)	0.7219	**1.3717****	0.0212	**0.7937****	**0.7504****	0.3577	0.1224	-0.2656	-0.0878
	(0.4459)	**(0.6142)**	(0.3960)	**(0.3602)**	**(0.3461)**	(0.3876)	(0.3258)	(0.3605)	(0.3210)
(*Male_Unemp.* = $FEIL_{t+2}$)	**0.6639***	0.5004	0.0829	**0.8993****	**0.7751****	**1.2521****	0.1039	**0.8468****	0.5255
	(0.3652)	(0.4266)	(0.3299)	**(0.3848)**	**(0.3600)**	**(0.4899)**	(0.3359)	**(0.4175)**	(0.3543)
(*Male_Unemp.* = $FEIL_{t+3}$)	**1.0759****	**0.8138***	**0.8066****	0.0710	-0.2116	**0.8198***	0.1668	0.3621	-0.0739
	(0.4252)	**(0.4621)**	**(0.3941)**	(0.3519)	(0.3463)	**(0.4719)**	(0.3655)	(0.4066)	(0.3588)
(*Male_Unemp.* = $FEIL_{t+4}$)	0.6070	-0.5315	**1.7927*****	0.5219	0.5796	0.1226	0.0213	0.6066	-0.0684
	(0.4030)	(0.4974)	**(0.6022)**	(0.4256)	(0.4191)	(0.4859)	(0.4143)	(0.4852)	(0.4090)
(*Male_Unemp.* = $FEIL_{t+5}$)	0.7532	-0.0257	0.7085	**0.9412***	**0.9656***	**1.1111***	0.3606	-0.0902	0.2885
	(0.4765)	(0.5338)	(0.4728)	**(0.5297)**	**(0.5222)**	**(0.6380)**	(0.4687)	(0.4973)	(0.4584)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396	12,396
# Municipalities	348	348	348	348	348	348	348	348	348

Notes: standardized variables. Municipalities with more than 500 inhabitants and all approved projects of the same size. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix D. Results Using Three Periods of Approval and Municipalities with More Than 500 Inhabitants and All Types of Projects Approved

Table D1. Municipal Summary Statistics by Treatment Status (3 Approval Months)

Variable		Mean	Std. Dev.	Min.	Max.	Observations
Treatment = 2008m12						
	Population	5,659.0	5,466.3	775	16,539	9
	Number of projects	4.1	5.3	1	18	9
	Total amount	977,318.7	940,620.4	139,820.6	2,745,973	9
	Amount per project	322,029.3	222,756.4	75,796.7	758,552.5	9
	Tender period	2.3	0.6	1	3	9
Treatment = 2009m01						
	Population	6,490.1	11,534.6	510	72,987	143
	Number of projects	3.5	5.2	1	34	143
	Total amount	1,093,816	1,945,892.0	86,051.9	12,500,000	143
	Amount per project	334,478.5	490,067.1	35,256.9	4,574,085	143
	Tender period	2.3	0.5	1	3	143
Treatment = 2009m02						
	Population	4,902.6	9,075.7	501	107,770	397
	Number of projects	3.3	3.7	1	37	397
	Total amount	792,545.7	1,375,398.0	68,816.9	18,600,000	397
	Amount per project	268,273.5	339,852.5	23,207.6	3,870,910	397
	Tender period	2.2	0.5	1	3	397

Table D.2. First-Stage Estimates: Impact of FEIL Projects on Labor Outcomes

VARI-ABLES	*Employ.*	*Total Unemp.*	*Male Unemp.*	*Female Unemp.*	*Constr. Unemp.*	*Serv. Unemp.*	*Indust. Unemp.*	*Agric. Unemp*	*Low Edu. Unemp.*	*High Edu. Unemp.*	*Young Unemp.*	*Mid-age Unemp.*	*Mature Unemp.*
$FEIL_t$	**0.0227***	**-0.1268***	**-0.1529***	**-0.0742***	**-0.1550***	**-0.0861***	**-0.0725****	**-0.0768****	**-0.1397***	**-0.1047***	**-0.0958****	**-0.1322***	**-0.0889***
	(0.0123)	**(0.0289)**	**(0.0354)**	**(0.0265)**	**(0.0462)**	**(0.0299)**	**(0.0342)**	**(0.0362)**	**(0.0409)**	**(0.0287)**	**(0.0402)**	**(0.0335)**	**(0.0270)**
$FEIL_{t+1}$	-0.0056	**-0.0934***	**-0.1214***	**-0.0452***	**-0.1291***	**-0.0543***	**-0.0659****	-0.0516	**-0.1057***	**-0.0762***	**-0.0746****	**-0.0777****	**-0.0820***
	(0.0124)	**(0.0251)**	**(0.0306)**	(0.0240)	**(0.0380)**	**(0.0280)**	**(0.0308)**	(0.0332)	**(0.0361)**	**(0.0246)**	**(0.0370)**	**(0.0304)**	**(0.0252)**
$FEIL_{t+2}$	-0.0064	**-0.1000***	**-0.1398***	-0.0378	**-0.1472***	-0.0360	**-0.0723****	-0.0485	**-0.1315***	**-0.0754***	**-0.0677***	**-0.0737****	**-0.1001***
	(0.0128)	**(0.0261)**	**(0.0314)**	(0.0250)	**(0.0371)**	(0.0287)	**(0.0313)**	(0.0375)	**(0.0355)**	**(0.0258)**	**(0.0374)**	**(0.0320)**	**(0.0258)**
$FEIL_{t+3}$	-0.0021	**-0.0932***	**-0.1256***	-0.0403	**-0.1441***	-0.0457	-0.0524	-0.0003	**-0.0932****	**-0.0801***	**-0.0636***	**-0.0760****	**-0.0866***
	(0.0138)	**(0.0266)**	**(0.0309)**	(0.0265)	**(0.0358)**	(0.0291)	(0.0325)	(0.0445)	**(0.0362)**	**(0.0266)**	**(0.0361)**	**(0.0297)**	**(0.0272)**
$FEIL_{t+4}$	0.0058	**-0.1025***	**-0.1224***	**-0.0612****	**-0.1441***	**-0.0609****	-0.0424	-0.0074	**-0.1040***	**-0.0876***	-0.0455	**-0.1005***	**-0.0877***
	(0.0141)	**(0.0282)**	**(0.0320)**	**(0.0282)**	**(0.0372)**	**(0.0306)**	(0.0345)	(0.0372)	**(0.0364)**	**(0.0286)**	(0.0385)	**(0.0302)**	**(0.0284)**
$FEIL_{t+5}$	0.0200	**-0.1356***	**-0.1672***	**-0.0752****	**-0.1932***	**-0.0822****	**-0.0842****	**0.1193***	**-0.1028****	**-0.1273***	-0.0512	**-0.1218***	**-0.1289***
	(0.0167)	**(0.0324)**	**(0.0390)**	**(0.0296)**	**(0.0450)**	**(0.0322)**	**(0.0399)**	**(0.0653)**	**(0.0466)**	**(0.0337)**	(0.0424)	**(0.0350)**	**(0.0349)**
Municip. FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
F-test	1,400.5***	137.5***	68.52***	179.1***	62.0***	131.0***	30.9***	28.5***	80.9***	143.6***	35,8***	95.8***	162.3***
# Obs.	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632
# Municip.	549	549	549	549	549	549	549	549	549	549	549	549	549

Notes: Variables have been standardized. Results for municipalities with more than 500 inhabitants and all types of projects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D.3. IV Estimates for the Impact of Total Unemployment Rate on Property Crime Rates

Second-stage	*Total property*	*Robberies*	*Total thefts*	*Serious thefts*	*Minor thefts*	*Total car*	*Serious car*	*Minor car*	*Damages*
(*Total_Unemp.* = $FEIL_t$)	0.4656	**0.6234***	-0.0215	-0.1385	0.0824	0.4155	**0.6173****	-0.5311	0.2012
	(0.3518)	**(0.3753)**	(0.3791)	(0.3183)	(0.3972)	(0.2919)	**(0.2953)**	(0.3736)	(0.3221)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.5001	**0.9985****	-0.0607	0.2757	0.3665	**0.9123****	**0.8850****	0.2673	0.0958
	(0.3927)	**(0.4460)**	(0.4202)	(0.3588)	(0.4477)	**(0.3649)**	**(0.3517)**	(0.3987)	(0.3516)
(*Total_Unemp.* = $FEIL_{t+2}$)	**0.6986***	0.2897	0.3998	**0.9267****	**1.7846*****	**1.0366*****	**0.9791*****	0.3843	0.1470
	(0.4037)	(0.3980)	(0.4326)	**(0.4294)**	**(0.6241)**	**(0.3952)**	**(0.3778)**	(0.4194)	(0.3669)
(*Total_Unemp.* = $FEIL_{t+3}$)	**1.7338*****	**0.9846****	**1.7157*****	**1.5357*****	**2.4494*****	0.1197	-0.1573	**0.8679****	-0.3782
	(0.5536)	**(0.4581)**	**(0.6007)**	**(0.4573)**	**(0.6530)**	(0.2860)	(0.2751)	**(0.4035)**	(0.3328)
(*Total_Unemp.* = $FEIL_{t+4}$)	**1.2354*****	0.0167	**2.5014*****	**0.7437****	0.5510	0.2368	0.2910	-0.1175	0.1961
	(0.4278)	(0.3665)	**(0.6516)**	**(0.3293)**	(0.3845)	(0.2612)	(0.2532)	(0.3311)	(0.2961)
(*Total_Unemp.* = $FEIL_{t+5}$)	**0.5526***	-0.0036	**0.7840****	0.1637	-0.3342	0.3976	**0.4209***	0.0090	0.2889
	(0.3284)	(0.3312)	**(0.3870)**	(0.2898)	(0.3554)	(0.2614)	**(0.2532)**	(0.3228)	(0.2919)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632
# Municipalities	549	549	549	549	549	549	549	549	549

Notes: Variables have been standardized. Results for municipalities with more than 500 inhabitants and all types of projects. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table D.4. IV Estimates for the Impact of Total Unemployment Rate on Crime against Persons and Other Types of Crime

Second stage	*Total person*	*Murder*	*Injuries*	*Gender violence*	*Sexual*	*Threats*	*Total other crimes*	*Drugs*	*Road Safety*
(*Total_Unemp.* = $FEIL_t$)	0.1062	-0.2913	0.2924	-0.1562	-0.3187	0.1123	0.0848	-0.1243	0.1082
	(0.3383)	(0.2907)	(0.3228)	(0.3952)	(0.4203)	(0.3210)	(0.2823)	(0.3133)	(0.2821)
(*Total_Unemp.* = $FEIL_{t+1}$)	0.3491	0.2629	-0.3587	**0.8102***	-0.1149	0.3510	-0.1513	-0.0000	-0.1539
	(0.3779)	(0.3178)	(0.3573)	**(0.4643)**	(0.4580)	(0.3591)	(0.3113)	(0.3435)	(0.3110)
(*Total_Unemp.* = $FEIL_{t+2}$)	**0.8936****	0.0574	**0.6655***	0.4620	0.5378	0.3843	0.5382	0.6200	0.4379
	(0.4324)	(0.3261)	**(0.3920)**	(0.4609)	(0.4928)	(0.3752)	(0.3432)	(0.3845)	(0.3355)
(*Total_Unemp.* = $FEIL_{t+3}$)	**0.6334***	0.2895	**0.6970****	0.5106	0.7226	-0.1451	-0.3399	0.4044	-0.4175
	(0.3649)	(0.2955)	**(0.3513)**	(0.4125)	(0.4495)	(0.3277)	(0.2947)	(0.3291)	(0.2983)
(*Total_Unemp.* = $FEIL_{t+4}$)	0.3140	0.1702	0.4842	-0.3594	0.5099	0.1355	-0.2197	0.0595	-0.2341
	(0.3153)	(0.2638)	(0.3047)	(0.3682)	(0.3952)	(0.2955)	(0.2622)	(0.2879)	(0.2623)
(*Total_Unemp.* = $FEIL_{t+5}$)	0.4304	-0.0461	-0.1079	0.1992	-0.0810	**0.6587****	0.3051	**-0.5034***	0.3995
	(0.3121)	(0.2563)	(0.2862)	(0.3553)	(0.3745)	**(0.3088)**	(0.2586)	**(0.2932)**	(0.2619)
Municipality FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
# Observations	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632	19,632
# Municipalities	549	549	549	549	549	549	549	549	549

Notes: Variables have been standardized. Results for municipalities with more than 500 inhabitants and all types of projects. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Appendix E. Comparison Labor Data Sources

Figure E.1. Registered & LFS Quarterly Unemployment Rates Comparison at a Provincial Level