The Effects of Local Infrastructure Investment on Crime[*]

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Abstract. This paper examines how a nationwide infrastructure investment policy, implemented at the local level, impacted local crime rates. The 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 more than 900 municipalities making up the Spanish region of Catalonia, the paper exploits spatial and temporal variations in the Spanish Ministry of Public Administration's random approval of local investment policies, to estimate their impact on both unemployment and crime. The combination of difference-in-differences and IV estimates makes it possible to assess both the size and timing of the policy's impact on the local labour market and on municipal-level crime rates. While the policy did little to palliate the effects of the economic recession in the long run, local public finances did experience a short-term boost, resulting in a temporary reduction in local unemployment rates (as required by the policy), as well as a significant drop in crime rates.

JEL classifications: K42, R53, H54, J40
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1. Introduction

The impact of local development on crime rates is contingent on a range of temporal and spatial factors. Indeed, different aspects of local economic activity appear to exert a series of counterbalancing forces, with mixed outcomes for crime rates. An examination of Becker's (1968) seminal model in the economics of crime literature shows that rational individuals divide their time between legal and illegal 'work' (contingent, that is, on a range of factors, including the rewards to be gained from such activities, deterrence variables, severity of punishment, and personal traits) as they seek to maximize their expected utility. Thus, *ceteris paribus*, all public interventions aimed at boosting labour market opportunities should, in principle, reduce crime.

Yet, the empirical evidence is mixed and contradictory, precisely because of the difficulties encountered in accounting for all the potential factors involved. However, by employing various mechanisms (directly or indirectly), it is relatively straightforward to demonstrate why economic activity, in general, and employment, in particular, may have a decisive impact on criminal activity. As Freedman and Owens (2016) report in a study conducted in San Antonio (Texas), while employment opportunities are unequally distributed among individuals, criminal activity is likely to increase when the earnings of those individuals benefiting from an employment program rise (i.e., there is an increase in the supply of criminal opportunities) vis-à-vis the earnings of those that do not benefit from the program. However, other forces may well be at work; for instance, better employment opportunities could result in more people working and, hence, their spending less time at home, thus increasing their chances of being burglarized. Moreover, many different forces could be operating simultaneously in the relationship between labour market opportunities and crime, with counterbalancing effects. For this reason, it is critical to identify adequate, empirical specifications that allow us to address the issue at hand, and reveal true causal relationships between employment opportunities, local development in general, and criminal activity. In this paper, I contribute to this literature by addressing the following research question: Does local investment in infrastructure affect local crime rates? To do so, I draw on a unique empirical setup that enables me to make causal estimates of the potential relationships involved.

In December 2008, as a result of the intensifying financial crisis that had been triggered three months earlier with the Lehman Brothers' bankruptcy, and which was characterized by a credit crunch, growing uncertainty about the economic outlook, and a severe contraction in private demand, the Spanish Central Government opted to implement various urgent,

extraordinary measures to boost local economic activity and local employment. The measure that captured most of the spotlight was the creation of an €8-billion nationwide public fund – the *Fondo Estatal de Inversión Local* or FEIL (State Fund for Local Investment) – to finance local public works infrastructure. The fund was more popularly referred to as *Plan E* or *Plan Zapatero*.[1]

In this paper, using the difference-in-differences technique and an instrumental variable methodology, I exploit the Spanish Ministry of Public Administration's random approval dates for these local investment programs to examine how they varied 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. Initially, I draw on monthly data from the Catalan municipalities to assess the impact of these local investment programs on unemployment rates. Once I have demonstrated that the FEIL indeed affected short-run labour outcomes, I then measure the 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 successfully reduced unemployment rates – especially among unemployed male construction workers – thus significantly reducing crime. A closer look at these results reveals some interesting features. For instance, I find that some crime types were significantly reduced during working hours and that the probability of repeat offending was lower as a result of the decrease in unemployment rates 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.

Examining the effects of local public investment programs on crime is interesting in its own right, but the outcomes reported herein should also provide insights for policy makers considering the implementation of similar programs in both developed and, more specifically, in developing countries around the world. In this regard, the International Labour Organization (ILO) issued a guide for labour-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

[1] Today, the FEIL is remembered as one of the Socialist government's worst economic decisions of those years. See https://goo.gl/66otD5.

investment. Moreover, they provide a good opportunity to each of these partners to incorporate social policy objectives into infrastructure investment policies."[2]

This paper aims, therefore, to provide tools that can better define how social policy objectives can be targeted via public investment programs.

The rest of this paper is organized as follows: Section 2 briefly reviews the extensive literature on the issue under discussion. Section 3 provides background on the FEIL state fund, which is used here as an exogenous source impacting the variation in municipal-level unemployment rates. 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. Previous literature

The relationship between labour outcomes and crime has long been of interest in both the empirical and theoretical economic literature. From the empirical perspective, unemployment became the focus of researchers' attention in the mid-1980s, when the condition was believed to be the key determinant of crime.[3] These initial studies indeed showed that high unemployment rates were associated with a rise in crime, although the relationship was less statistically significant than, for instance, that between the deterrence variables and crime. Moreover, the empirical evidence presented was far from conclusive in terms of the robustness of the relationship reported (see, for example, Cameron, 1988, and Freeman, 1996, for surveys of those initial studies).

The ambiguity of the outcomes was linked to various factors, including the level of data aggregation, the unemployment and criminality measures, and the econometric specification employed. Thus, while studies using aggregate time-series and cross-sectional data reported a

[2] Specific examples of such programs can be found in many countries, including Mexico, in 2014 (http://goo.gl/7AU3C7), and India, with the 2006-2008 National Rural Employment Guarantee Scheme (see Zimmermann, 2015). A further example is provided by the program implemented by the Australian embassy in Chile, Colombia, and Ecuador in 2015-16 (http://goo.gl/OPxXE0). Indeed, foreign aid to developing countries often takes the form of public investment programs. See, for instance, USAID (https://goo.gl/Lkf8TS) or the 2014 United Nations Development Programme (UNDP) 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 in which time is allocated between criminal activities, the labour market, and nonmarket activities (Grogger, 1998). In his model, Grogger establishes that higher wages deter crime. Job search models, such as those constructed by Burdett, Lagos, and Wright (2003, 2004), enabled crime and labour decisions to be endogenized, allowing for multiple equilibria to occur. This in turn opened the door to explanations of the high dispersion in crime rates across urban areas, for instance (see Glaeser, Sacerdote, and Scheinkman, 1996).

causal relationship between unemployment and crime, panel data studies (including, Papps and Winkelmann's (2000) article on crime in New Zealand, and Entorf and Spengler's (2000) article on crime in Germany) reported little effect. In contrast, Raphael and Winter-Ebmer (2001), drawing on state-level data for the U.S., found that the significant reduction in the (state aggregate) proportion of property crimes during the 1990s was due to a 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 the labour 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 rectifies the tendency of earlier publications to focus mainly on unemployment, while overlooking other potential job opportunities in the labour market. This strand of the literature typically finds that the beneficiaries of improved economic conditions commit fewer crimes (see, for instance, Harbaugh, Mocan, and Visser, 2013, for evidence from an economic experiment).

Another strand of the literature relevant to the current analysis seeks to disentangle the relationship – and the multiple forces that might be at work – between economic development (broadly defined) and illegal behavior. Here, the centrally planned economic measure I focus on was designed to promote a type of investment that would stimulate short-term economic activity (i.e., through job creation) while also strengthening the financing of municipalities.

In this context, improved employment opportunities would appear to be a critical issue for local development, at least as far as small and medium-sized municipalities are concerned. However, the objectives of the FEIL also included the enhancement of the overall economic standing of municipalities, by boosting local economic growth and even reducing poverty and income inequality. Indeed, Kelly (2000) showed that, for urban counties in the U.S., that while inequality has no effect on property crime, it does have a strong and robust impact on violent crime. It seems to follow, therefore, that inequality may be associated with a lack of social capital and upward mobility, and with social disorganization, all of which may lead to higher crime rates. Yet, Kelly (2000) also showed that, while poverty has a significant effect on property crime, it has little effect on violent crime.

The FEIL specifically targeted municipal infrastructure improvement projects that presented socially useful and productive goals. A detailed review of the local projects that gained approval shows that many were designed to improve the municipalities' social capital. Indeed, it is widely accepted that social capital (broadly defined) affects crime (see, for instance, Buonanno, Montolio, and Vanin, 2009), with various theories, developed by sociologists and criminologists, averring that social capital has a negative effect on crime. Rosenfeld, Messner, and Baumer (2001) claim that the social disorganization, anomie, and strain theories all predict that civic engagement and social trust (that is, 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. It is clear that the purpose of many of the local investment projects proposed under the FEIL was to improve local conditions. Thus, the empirical strategy adopted herein and the Catalan Police Department's detailed database should help in determining the factors that impacted the change in local crime rates.

3. Institutional Setting: The 2008-09 FEIL

As a result of the crisis ushered in 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, twice the average unemployment rate of the Eurozone countries. This increase in unemployment was especially marked in the construction sector, a phenomenon that would come to be known as the "bursting of the housing bubble" (in which Spain had lived since the early 2000s).

Against this backdrop, the Spanish Central Government – led by Socialist Prime Minister José Luis Rodríguez Zapatero – created a public fund to finance local investment projects, the chief purpose of which 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 that represented 0.76 percent of GDP in 2009. The objective of the FEIL was to maintain and create jobs (avoid job destruction), especially in the construction industry, and to 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). Contingent upon the project's approval, to ensure that funds were distributed equally across the municipalities, the investment per municipality was allocated in accordance with its population, with approximately €177 being made available 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 costs at the outset, and 30 percent upon its (certified) completion.

Various features of the FEIL itself, and the way it was administered, make it an ideal example of a source of exogenous variation in local unemployment rates. Analyzing the FEIL's potential impact on local crime is equally edifying. 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 had already been drawn up and many of them approved), and it clearly established that the funding would be dedicated to local public investment not included within the 2009 budget.[6] As such, this represented an unanticipated increase in local public budgets.

Second, the FEIL was pushed through urgently, meaning that, for the Central Government, timing was crucial if the policy was to have an immediate impact on the labour market. This meant that for project proposals to be funded they had to include public works that could be implemented immediately (i.e., work tenders were to begin within a month of the publication of the FEIL funding-authorization resolution on the Ministry of Public Administration's website). This aspect of the timing window is critical for the identification strategy employed herein, given that it defines when treatment started for a given municipality. As such, it is examined in more detail below.

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

In its eagerness to impact labour outcomes as quickly as possible, Spain's Central Government took a number of drastic measures. First, it fast-tracked the approval and implementation of all

[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.

public works projects proposed 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 this verification was complete, the territorial delegations sent an electronic notification to the Secretary of State for Regional Cooperation, who then issued the resolution authorizing the project's financing. Finally, the Secretary of State for Regional Cooperation published the resolution on the Ministry of Public Administration's website. In this way, the Central Government could decentralize and accelerate the process without putting the Secretary of State for Regional Cooperation under unreasonable pressure.

3.1.1 Submission and Approval of Project Applications

The period for submitting project applications ran between December 10, 2008 and January 24, 2009. As soon as an application was received, the Central Government's territorial delegations had up to 10 business days to review it. Once reviewed, and once the respective delegation had informed the Secretary of State for Regional Cooperation that the project qualified for the FEIL, 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 website.

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. However, some resolutions were 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 had to adhere strictly to public procurement rules, which in Spain vary according to the amount allocated to the project and its nature. The tendering of public works could be completed in accordance with any of the procedures under the law governing public sector contracts. That is, tenders could be open, restricted, negotiated with or without publicity, or processed as a smaller contract.

The urgent introduction of the fund meant all projects were categorized as being for immediate implementation; as such, the tender procedure had to begin within a month of the publication of the FEIL funding-authorization resolution on the Ministry of Public

Administration's website. The length of the tender process, though, varied depending on the amount allocated to the project (see Table 1).[7]

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

Type of procedure	Amount	Expected duration under normal procedure	Expected duration 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 calculations 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 amounts allocated of less than €50,000 were considered minor works contracts, and could 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.

Public procurement procedures became more complicated for project amounts above €50,000. For example, public investment projects for amounts between €50,000 and €200,000 were subject to the no-publicity procedure, and it was expected to take between one and two months for the project to be awarded. In the case of projects for amounts greater than €200,000

[7] Given how fast the program was rolled out and the fact that nearly every proposal was approved, there are possible concerns regarding corrupt practices in the management of these funds. Although some cases of corruption were later discovered and investigated (see, for instance, https://goo.gl/pkjMyH), during the period under analysis there was no (general) perception of corruption regarding these funds and, perhaps more importantly, the cases identified were associated more closely with prevarication, that is, instances of a public servant dictating an arbitrary decision in an administrative matter knowing such a resolution to be unfair. In general, cases of corruption associated with public infrastructure in Spain have not involved the non-completion of the investment, but rather the awarding of public contracts to specific firms. These firms, in return, provided illegal funding to political parties in power (those awarding the projects). However, corruption would tend to bias our results towards finding no first-stage relationship between the FEIL and labour market outcomes (since the program's funds would be lining the pockets of corrupt politicians instead of being dedicated to improving infrastructure), but this is not what the empirical results show.

and less than €5,000,000 (this upper limit being established by the Royal Decree-Law precisely to restrict tender procedures)[8], three tender options were available: a procedure negotiated with publicity, an open procedure, and a restricted procedure. These last two options applied to cases not subject to EU harmonization regulation. For all three procedures, and taking into account that all three could be issued urgently, the expected duration was between two and three months.

Timing is clearly critical for determining when these local investment projects were initiated and, consequently, when the potential impact on labour outcomes and crime rates may have occurred. Given the circumstances, it is reasonable to expect differences in the time frame dictated by the size and nature of the projects. Small projects were undoubtedly carried out within a shorter time frame than were their larger counterparts.[9]

3.2 Defining the Treatment

The identification strategy relies on the nature of the FEIL program because, as explained above, these funds emerged from nowhere and projects were undertaken and implemented as a matter of some urgency at the local level. Given the way funding was planned, organized, and managed during project submission and approval stages – and, as shown in Figure 1 (for projects submitted by the Catalan municipalities) – projects were approved on a daily basis. Moreover, resolutions listing projects granted approval throughout Spain were published almost daily.

Importantly, the timing of the approval of local investment projects varied from month to month and by municipality. Recall, projects were approved from December 2008 through March 2009.[10] However, as Figure 1 shows, only seven projects from four Catalan municipalities were approved in March 2009, whereas 211 projects from 30 Catalan municipalities (nine with a population above 500 inhabitants) were approved in December 2008. Hence, in order to simplify the empirical exercise, I opt to omit these two approval dates

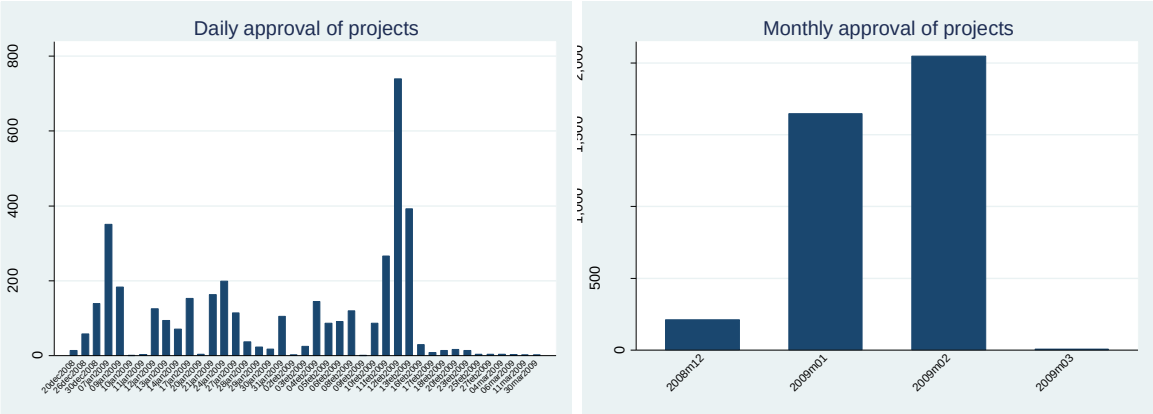
[8] In order not to exceed this amount, local governments were not permitted to split the project into two or more projects. However, it was possible for municipalities to contribute their own funds. In all circumstances, the tender procedure of FEIL-funded projects had to adhere to the rules for projects for amounts below the €5,000,000 threshold.

[9] For example, a project funded to the tune of €14,210 and entitled "Adapting Plots of Municipal Property to the Terms of Rule That Prevent Forest Fires" (which essentially involved clearing a municipal plot in Bescanó, Girona, of weeds) had a shorter implementation time and a less substantial impact on labour outcomes than the a project funded to the tune of €2,999,927 and entitled "Works of Reform and Extension of the Francesc Calvet Sports Center" in Sant Joan Despí (Barcelona).

[10] A final (extraordinary) FEIL approval was granted in December 2009. In the case of the Catalan municipalities, this only affected projects proposed by the city of Barcelona, which, as discussed in this section, is excluded from the empirical analysis.

from the rest of the analysis.[11] Consequently, I define the treatment status by the month in which a municipality's projects were approved; that is, in January 2009 or February 2009.

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



Note: Projects submitted by Catalan municipalities.

Before elaborating further on the treatment definition, I briefly present the figures for the FEIL in Catalonia. In the 948 Catalan municipalities (corresponding to 11.7 percent of the Spanish total), where the average project amount was €324,281.97,[12] 3,930 projects were approved (corresponding to 12.8 percent of the Spanish total) to the tune 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 the 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 these 76 municipalities from the analysis, since their populations are, on average, higher, and they submitted an average of 25.3 projects each (ranging from a low of two projects to a high of 302, in the case of the city of Barcelona). For these municipalities, I do not have a clearly defined temporal treatment definition.

In addition to eliminating these 76 municipalities, I further restricted the sample to municipalities with more than 500 inhabitants. There are several reasons for proceeding in this

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

[12] From a regional perspective – and consistent with the government's criterion of distributing FEIL funds in accordance with the municipalities' populations – Catalonia was the second largest recipient of total FEIL funding, behind Andalucía (which represents 9.5 percent of all the Spanish municipalities and whose share of the funding corresponded to 17.8 percent). The Madrid region occupied third place, its 179 municipalities (2.2 percent of the total) receiving 13.5 percent of the FEIL funds. It received an average of €867,292.96 per project, by far the highest amount per project among the Spanish regions.

manner. 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 means that there is often no one authorized to validate the agreements approved by the council.[13] The most common solution is for these municipalities to share an auditor who periodically rotates between the councils. This, however, affects a municipality's ability to apply and manage such funds as the FEIL. Moreover, procurement procedures for these small municipalities are not especially strict and are readily modified. In fact, in the FEIL Royal-Decree Law, explicit mention was made of this concern, to the effect 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 these municipalities, and the uncertainty with respect to their tender procedures and eventual project implementation, result in their being excluded from this study. This means the final sample is largely homogeneous in terms of the type of municipality included, thus we are able to avoid any potential distortions attributable to the inclusion of very small or very large municipalities.

The remaining 539 municipalities submitted, on average, 3.4 project proposals, with a low of 1 and a high of 37, with population figures ranging from 501 to 107,770 inhabitants.[14] However, they do not all comprise the final sample. As explained above, not only does the month in which approval was granted matter, but we also take into consideration the duration of the tender procedures and project implementation. Based on the information in Table 1, I computed a tender and award period for each project approved by the Ministry of Public Administration, according to the amount of funding granted. For projects below €50,000, I assume a tender and award period of one month following the month in which the project was approved. For projects for amounts between €50,000 and €200,000, I assume a two-month

[13] This situation is due to the fact that these municipalities cannot afford to hire a *secretario-interventor* (i.e., a high-ranking civil servant who is appointed after passing a highly competitive nationwide examination) or because not all auditors/treasurers want to live in municipalities with fewer than 500 inhabitants.

[14] As the descriptive statistics show, the municipalities submitted, on average, two projects to the Ministry, many of which were approved the same day. However, there were instances in which municipalities had various projects approved on several different days within the same month. For example, 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 February 11, 2009; and two were approved on February 12, 2009. Similarly, Sant Feliu de Llobregat, a municipality in the province of Barcelona with 43,000 inhabitants, had five projects approved on three different dates: February 3, 2009; February 10, 2009; and February 11, 2009. Vila-seca, a municipality in the province of Tarragona with 21,000 inhabitants, had projects approved on January 14, 2009; January 17, 2009; and January 20, 2009.

period, while for projects for amounts above €200,000, I assume a three-month period before project implementation.[15]

I further restrict the sample, therefore, to include municipalities submitting projects that were approved within a similar period of tender duration. 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 terms of tender duration), I use a restricted sample of 348 municipalities on which to perform the main estimates. Table 2 presents the summary statistics by approval status. Appendix D, however, also presents the full set of results when using all three approval statuses (that is, including also projects approved in December 2008) and restricting the sample to municipalities with more than 500 inhabitants.

Table 2. Municipal Summary Statistics by Approval Status

Variable	Mean	Std. Dev.	Min.	Max.	Observations
Approval = 2009m1					
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
Approval = 2009m2					
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

The identification strategy relies on two facts. First, as I demonstrate below, a different month for Ministry approval is a random factor, or at least one that is independent of the main variables of interest. Second, I take advantage of public procurement rules to assign a tender and award period for each project, depending on the amount assigned to it. This allows me to accurately match the succession of events between project approval, tender, implementation, and the project's impact on unemployment and crime.

Thus, variation in the FEIL dummy (treatment) arises from (1) the fact that applications were approved at different times for different municipalities and that (2) different sized projects took different amounts of time to go from application approval to implementation. For example, two municipalities, each with only one 100,000€ project, could have their FEIL

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

dummy activated at different times by virtue of their having their proposals approved in different months (January 2009 vs. February 2009). Similarly, two municipalities that had projects approved in the same month could have their FEIL dummies activated at different times if these projects were of different sizes (so the tender periods would be different). For example, the municipality of Arenys de Munt (in the province of Barcelona) obtained only one project approved in February 2009 for an overall amount of 1,371,739€. Given this project amount the type of public procurement procedure expected was between 2-3 months under the urgent procedure (see Table 1) so its treatment status is activated in May 2009.

Note that one positive aspect of the narrow time window used to identify FEIL's impact on unemployment and crime rates is that the timing of the approval, tender, and implementation phases of each municipality's FEIL projects is very unlikely to have coincided with another simultaneous shock that might have influenced the outcome variables and, so, invalidated the results. Moreover, the study does not suffer the potential drawback of the time distance between the announcement (approval) of a project and its implementation, as was the case in Freedman and Owens (2016) who have to rely on potential purchasing power increases of potential workers hired by the project to explain their observed results.[16]

4. Data

4.1 Labour Market Data

I use monthly employment and unemployment figures from the Spanish Ministry of Employment and Social Security.[17] At the municipal level, employment data only take into account registered workers in the 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 considerably more details; thus, I have information about the industry, gender, and age of registered unemployed individuals. These details are useful for ascertaining who, if anyone, benefited from the FEIL. They are also useful for determining the potential dynamics at play if crime rates should also be found to have been affected.[18]

[16] 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 quickly (and unpredictably) and using emergency procedures, and, second, because the projects could not have been previously budgeted for. This means that neither firms nor workers could have anticipated them by looking at the 2009 municipal budget.

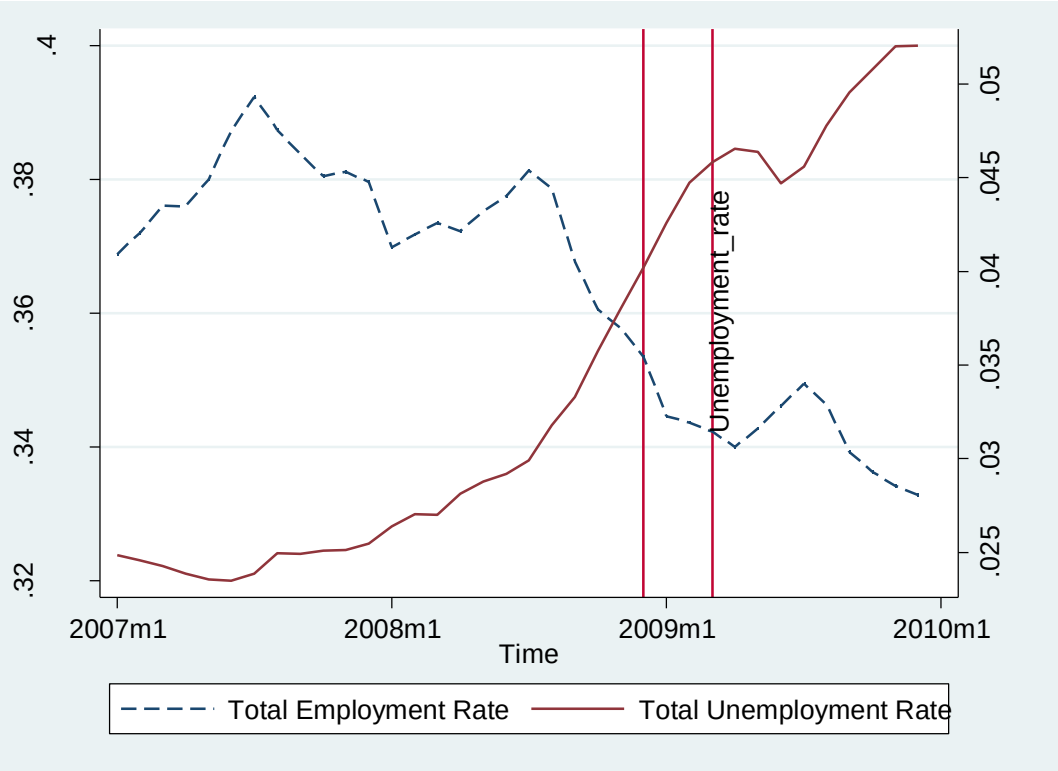
[17] I use officially registered unemployed workers at the public employment office (*Servicio Público de Empleo Estatal*) as a job seekers.

[18] It is well known that registered unemployment figures are lower than those obtained from the Spanish Labour 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, include students looking for a job, or people who have given

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

The time span for the estimations runs from January 2007 through December 2009. The main reason for this cutoff 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*, that is, the State Fund for Employment and Local Sustainability), was established on October 26, 2009 by Royal Decree-Law 13/2009. Under the FEES, potential projects seeking funding had to be submitted between November 2009 and January 2010. Funding for these 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 the estimates from capturing any possible impact from this second fund.

Figure 2. Labour Market Outcomes and the FEIL Approval Period



up looking for work through the national employment system and who seek employment on their own. Despite this, Figure F.1 in Appendix F shows that quarterly, provincial-level unemployment data from both sources present the same trends. Moreover, an individual must be registered for unemployment benefit in order to be entitled to receive it. In late 2008 and early 2009, when the economic crisis was in its early stages and unemployment benefit started being drawn, it is reasonable to assume that workers who lost their jobs applied for this benefit (by registering at the public employment offices). For this reason, it can also be assumed that registered unemployment statistics were more reliable during that period that they were during other periods when there may have been fewer incentives to register at the public employment offices.

Figure 2 presents aggregate data for both employment and unemployment in the Catalan municipalities, together with the (full) time window during which FEIL projects were approved. Starting in mid-2008, both data series show consistent change as the crisis breaks out 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 continued to decrease as unemployment rates increased. However, immediately following the program approval period (i.e., the period when the projects would have been implemented), both data series clearly underwent substantial changes. The objective here is to determine whether that change is a meaningful result of the FEIL program. If it is, the goal is then to determine whether this change in labour outcomes also affected crime rates at the local level.

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

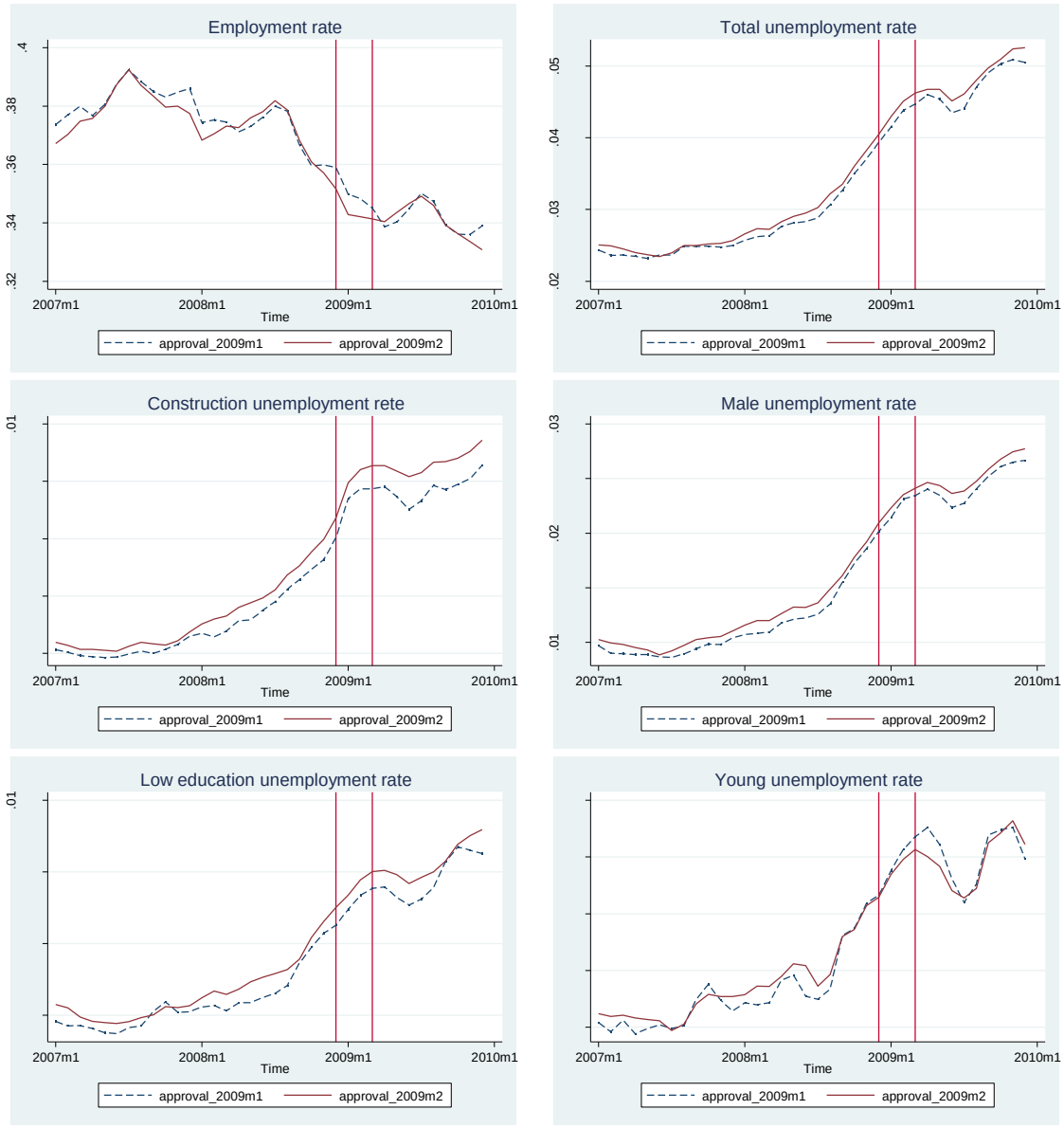
Figure 3 presents further details on the labour outcomes, this time by selected types of unemployment and approval status.[19] Note that all types of unemployment (by industry, gender, education, and age) increased after mid-2008, particularly the registered rate in the construction industry.[20] Figures 2 and 3 both show that unemployment rates increased more slowly (even decreasing temporarily). This suggests that the FEIL program may have buffered employment (and reduced registered unemployment) against the sharp economic downturn that began in mid-2008. Against this backdrop, the empirical strategy assesses whether this relationship is statistically robust and can be considered causal.[21]

[19] Figure 3 reports unemployment rates for the gender (male), sectoral (construction), education (low) and age (young) categories that I consider most relevant for the discussion below.

[20] In the case of unemployment by age, I construct three categories: young (less than 25 years old); middle-aged (25-40 years old) and mature (more than 40 years old).

[21] Figures 2 to 5 should be seen as descriptive, while the results embodied in Tables 5 to 8 (and those in the various appendices) can be considered as being more informative and accurate regarding the effect analyzed. The fact that the treatment depends on both the month in which approval was granted and the expected procurement period (which in turn depends on the project amount) does not make it especially visible when we would expect to find a differential impact on treated municipalities with respect to control municipalities.

Figure 3. Employment and Unemployment Rates, and Approval Status



4.2 Crime Data

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

The dataset records the time the crime took place (if known), the location of the crime, and the type of crime committed. The dataset extends from January 2007 through December 31,

2009.[22] Illegal activities are roughly classified in accordance with the 190 articles making up the Spanish penal code. However, to reduce the number of categories without causing an aggregation bias that might undermine the 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 the person, and other types of crime.

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

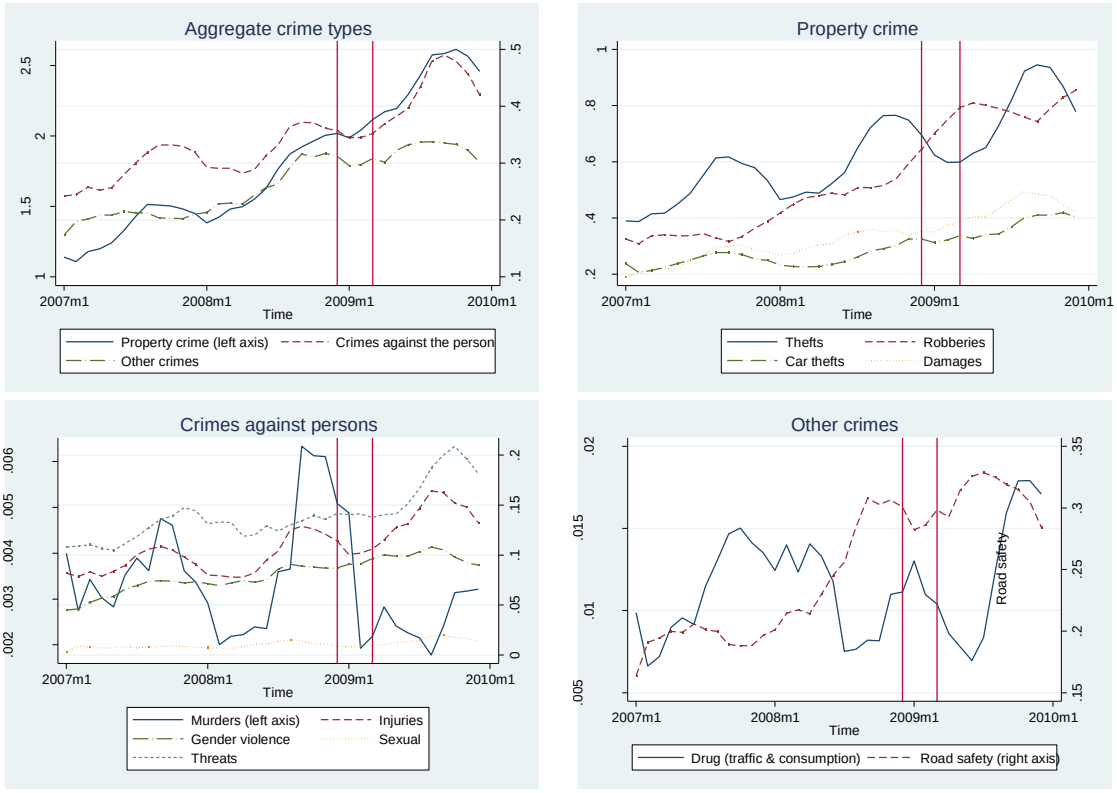
The main crimes involving interpersonal violence, here classified as crimes against the person (11 percent of all crimes recorded in Catalonia in the period 2007–09), include "Murders" (0.1 percent); "Injuries" (3.8 percent); "Gender violence" (3 percent); "Sexual assaults" (0.4 percent); and "Threats"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

Figures 4 and 5 present graphical evidence regarding the trends taken by each type of criminal behavior and how these change according to municipality type. The general upward trend in recorded crime is clear for nearly all crime types (clearly, murder figures are more random than the other crime types, given that they are rare in Catalonia). Therefore, during the crisis there was a perception – perhaps fueled by the media – that crimes with a clear economic return increased. For instance, the number of thefts of copper wire and machinery in rural areas rose sharply, as did thefts targeting businesses located outside town centers. There was also a perception that robberies and thefts increased at private properties in remote areas or in small

[22] The use of monthly data is of considerable advantage here. 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). As such, they make it possible to analyze how quickly delinquency responds to changes in the environment, changes that usually even out in yearly data.

municipalities. Consequently, the overall spike in thefts and robberies could have had an important impact on the economy.[23]

Figure 4. Trends in Local Crime Rates



Note: Crime series data have been smoothed using a uniformly weighted moving average of the crime variable using four lagged terms, and including the current observation in the filter. Crime variables are expressed per 1,000 inhabitants.

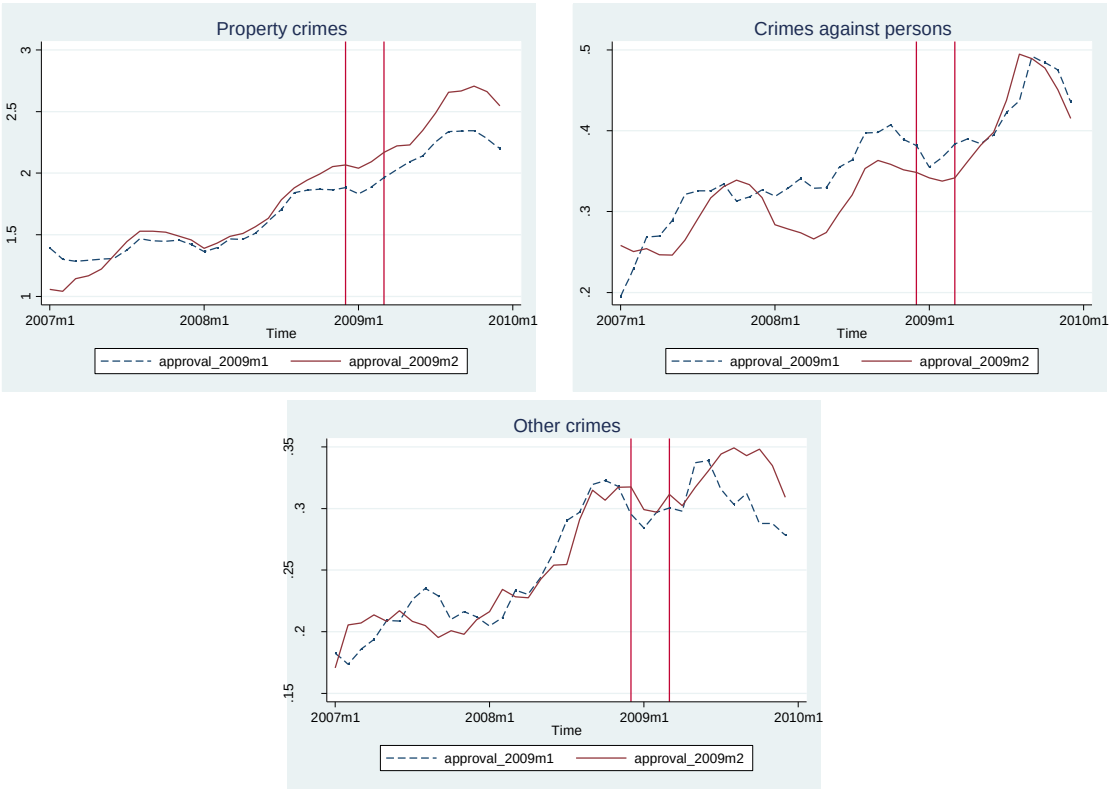
The data for the period under study seem to confirm this upward trend in crime rates in Catalonia. Note that, again, there appears to be no differential trend for the municipalities with respect to their treatment status, which is good news for our identification strategy. In line with the unemployment figures, the crime data present a downward trend some months after termination of the period of approval for FEIL projects. This trend, however, is reversed around mid-2009. In this respect, when looking at disaggregated figures for the various crime categories, each crime type presents turning points at different dates (compare, for instance, Thefts and Robberies in Figure 4). However, when the various types of crime are analyzed in aggregate terms, these differences even out.

To facilitate the presentation of the results, I only report the outcomes obtained for property crimes, that is, the crimes which, in principle, can be expected to be most affected by changes in economic outlook (in terms, that is, of increased labour opportunities) as a result of the FEIL

[23] https://goo.gl/3jfv4r.

program. The full set of results for crimes against the person and other types of crimes is presented, however, in Appendix B.

Figure 5. Crime Rates and Approval Status



Note: see Figure 4.

5. Identification Strategy and Econometric Specifications

The empirical specification outlined below seeks to reveal a causal relationship between the unemployment (as the key variable of local development) and crime rates, where the FEIL program acts as an exogenous shock to labour outcomes and, hence, to crime. The structural equation can be expressed as follows:

$$Crime_{it} = \beta(Unemployment)_{it} + \eta_t + \mu_i + \varepsilon_{it}\,, \qquad (1)$$

which, of course, is liable to suffer from all the well-documented problems that make OLS estimates inadequate: ranging from measurement errors (or omitted variables affecting both variables of interest) to reverse-causality problems. In this equation, *i* and *t* index municipalities and months, respectively. $Unemployment_{it}$ is our outcome of interest: unemployment rates (by industry, gender, education level, and age). In this specification, the first-stage regressions estimate the effects of the FEIL program on unemployment. Thus,

$$Unemployment_{it} = \delta FEIL_{it} + \eta_t + \mu_i + u_{it}\ , \qquad (2)$$

where $FEIL_{it}$ indicates a municipality receiving funds and implementing project investment at time t.[24] As discussed in Section 3, the main variable of interest takes into account not only the month of project approval, but also the tender period by project type (size). Recall, the sample is restricted to those municipalities that obtained project approvals in only one of the two-month windows for project selection, and for projects of the same size. As a result, identifying our parameter of interest, δ, only requires that the timing of approvals should be uncorrelated with time-varying, unobserved factors that themselves generate outcomes of interest. Were the sample to include municipalities that were not treated under the FEIL program, the identifying assumption would be more restrictive and require both when and if a municipality receives the public investment to be uncorrelated with trends in these unobserved factors that could also generate outcomes of interest.

A key aspect of the present specification is the timing of events. On the one hand (as discussed in Section 3.2), I deal with tender procedures that vary according to the size of the project, as defined by our main variable of interest, $FEIL_{it}$. However, there might be some uncertainty as to when the projects were actually implemented, or at least when they began to have some impact on labour outcomes and crime rates. For example, projects may present different time profiles in relation to their hiring of personnel, i.e., a project might be more labour intensive at a specific stage of its implementation. To address this issue, I use $FEIL_{it}$ variable lags (post-treatment) to account for the varying impact of a project with time. As explained, the number of lags used is conditioned by the fact that, in late 2009, a second call for projects was announced: the FEES fund.[25] Thus, a five-month forward period, coupled with a three-month tender period (for projects for amounts above €200,000), for a FEIL project approved in February 2009 provides for an estimated impact up to October 2009.

For this strategy to be successful, several conditions must be met. First, the FEIL variable cannot be correlated with the error term of the structural equation; that is, it must be uncorrelated with other (omitted) variables that might also affect crime rates. If there is little doubt that the instrument (the FEIL fund) affects the endogenous variable, given that it was one of the legal requirements of the program, what else might the FEIL program have affected in those municipalities and with such deterministic timing? Or, more generally, what other potentially invalidating factors might have changed during that period in those specific

[24] Given how the $FEIL_{it}$ indicator is constructed, equation (2) can be easily understood as a diff-in-diff setup.
[25] 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 the estimates.

municipalities (in response to the local public investment plan) and in such a way that they might also have had a significant impact on crime?

It is not easy to identify one socioeconomic factor that would change so markedly at the municipal level and on a monthly basis. Two factors represent potential threats to the identification strategy: population dynamics and the outcomes of the projects themselves. In the case of the first of these factors, it is quite unlikely for people to change residence simply because the FEIL program has been approved and implemented, especially in such a short period of time. Indeed, construction workers – those most likely to be hired under the FEIL – and the population in general did not consult on a frequent basis the Spanish government's website (or, for that matter, the website of the Ministry responsible for managing the fund) to find out when projects were approved. Nor did they know what tender procedures would be adopted so that they could estimate when a project would be implemented in the municipality. Although there was news coverage of the program's launch,[26] it is not plausible to assume that individuals would have been aware of the projects submitted by each municipality, when the projects were approved, or how tender procedures worked. Consequently, we cannot assume that these individuals would have chosen to move in order to increase their chances of being hired to work on a project. Moreover, any possible effect induced by population inflows of unofficially registered individuals ("*call-effect*") in the municipality can be ruled out, given that they could not have been registered as unemployed (i.e. individuals 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 (a legal requirement of the call).

Second, it could be argued that the projects themselves may also have reduced crime rates once completed. This may have been the case if we assume that the local development projects were designed to improve municipal infrastructure provision (given they had socially useful and productive goals), and note, as discussed above in the literature review, that there is evidence linking a better "local environment" (broadly defined) and lower crime rates (see, among others, Keizer et al., 2008, and references therein). Yet, while such a connection is possible, it would be some time before we could expect a municipality's "social capital" to react to the specific implementation of a FEIL project. Moreover, an impact on the "social capital" would be very difficult to detect in the (short-run) time profile proposed for the estimations. For example, the construction of a sports center would undoubtedly improve a

[26] News coverage of the FEIL plan included, among others, articles published on 27 November 2008 (https://goo.gl/QU6We2), 12 January 2009 (https://goo.gl/ssnmUQ) and 10 February 2009 (https://goo.gl/PYwHqL).

municipality's amenities and enhance social conditions with an expected positive (reduction) impact on crime rates once fully operative; however, some time would have to elapse before this impact could be observed. In contrast, the impact of this project on labour outcomes can be expected to be observed from the onset of the construction of the local infrastructure (see Table 3).

Table 3. FEIL Projects by Typology

Category	#	%
Rehabilitation and improvement of public spaces	1,438	36.59
Basic services facilities and infrastructure	773	19.67
Cultural, educational or sports buildings and facilities	760	19.34
Social, health and funeral buildings and facilities	263	6.69
Promoting mobility and road safety	263	6.69
Removal of architectural barriers	134	3.41
Energy savings and efficiency	113	2.88
Conservation of historic and municipal heritage	91	2.32
Environmental protection	42	1.07
Fire prevention	29	0.74
Industrial promotion	14	0.36
Promotion of tourism	10	0.25
TOTAL	3,930	100

Notes: Numbers and percentages calculated using the original full list of projects approved in Catalonia. For instance, a text search of the names of the projects reveals that only 17 projects (0.4%), corresponding to 14 municipalities, include the word "ILU", used to identify projects to install additional city lighting. Only 10 of these 14 municipalities are included in the final sample. Note that other project types were approved for these municipalities.

6. Results

6.1 First-Stage Results: The Impact of FEIL on Unemployment

I begin by examining the FEIL variable in greater detail, given that it is understood to be a source of exogenous variation in time and by municipality that should enable the identification of causal effects across the main variables of interest: namely, unemployment and crime rates. Table 4 reports the balancing t-tests of the municipal characteristics according to the month of project approval. These results are completed with the evidence provided in Appendix A, which reports further evidence on the determinants of the probability of a project being approved in a specific month. Both pieces of evidence suggest that the month of approval was random or, at least, that the Central Government did not take into account any relevant municipal

characteristics, especially related to labour outcomes and crime rates, in the approval procedure.[27]

Table 4. T-tests for Balancing Characteristics on Treatment Status

VARIABLES	**2009m1**	**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]
Labour 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]
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]
Damage to property	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

[27] Even though I am unable to observe the application process, the application time does not appear to be cause for any real concern. Apart from the very tight schedule (December 10, 2008 to January 24, 2009), Table 4 and Table A1 can also be seen as providing hints to the non-difference between municipalities with regard to the application process itself, especially if approval of projects followed a first-in-first-out rule. Although it is impossible to know how the application process was conducted exactly, it is plausible that given the huge number of projects the Spanish Ministry had to evaluate and the speed the government wanted to impose (in its efforts to confront the worst of the crisis) the approval of projects did, broadly speaking, adhere to such a scheme. Indeed, Tables 4 and A1 do not present significant differences across municipalities.

	(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 having been approved in one month and all projects being of the same size. For this subsample, 91 municipalities had projects approved in January 2009 and 257 municipalities had 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 the projects approved in one month (where 142 municipalities had projects approved in January 2009 and 397 municipalities had projects approved in February 2009).

Having shown that the FEIL variable is uncorrelated with the labour outcomes and crime rates of the affected municipalities and, hence, that it is indeed exogenous, I next present the first-stage results of the IV estimation strategy. These results are drawn from the estimation of equation (2).

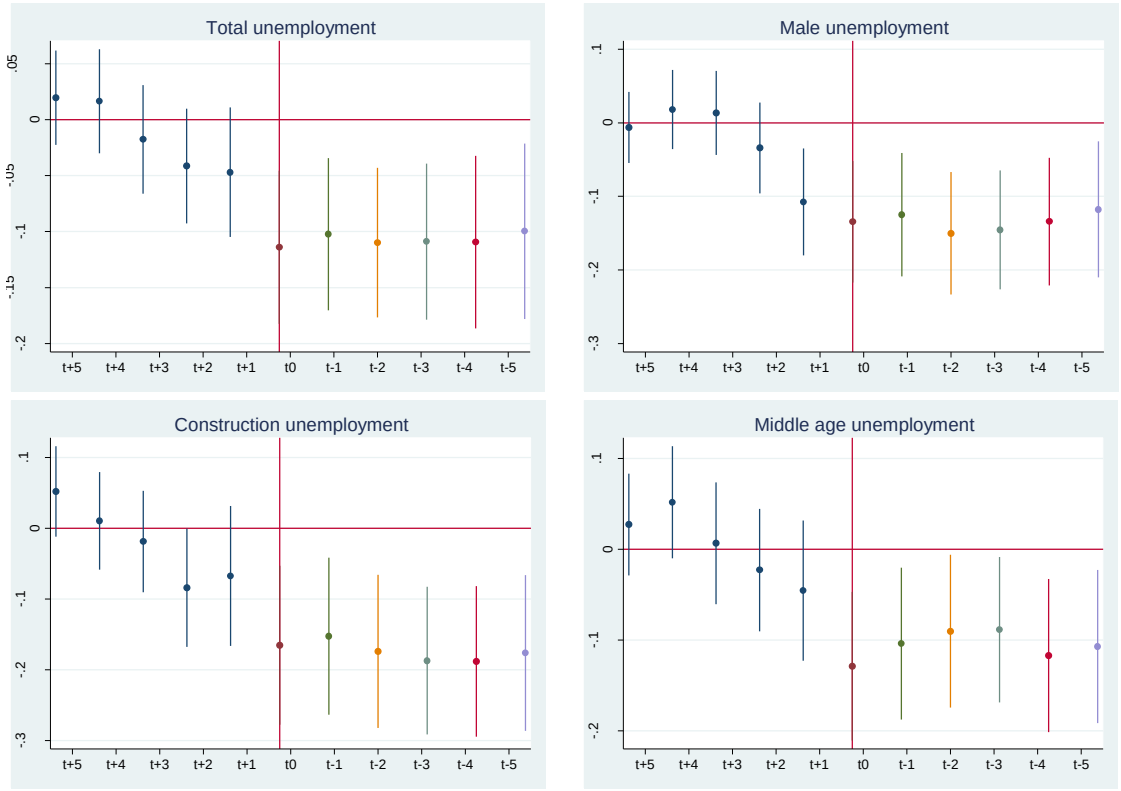
Table 5. First-Stage Estimates: Impact of FEIL Projects on Unemployment

VARIABLES	*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.*
$FEIL_{t0}$	**-0.1692*** **	**-0.1977*** **	**-0.1056*** **	**-0.2355*** **	**-0.1305*** **	-0.0693	-0.0883	**-0.1454*** **	**-0.1532*** **	**-0.1150** **	**-0.1982*** **	**-0.1081*** **
	(0.0363)	**(0.0438)**	**(0.0369)**	**(0.0599)**	**(0.0388)**	(0.0439)	(0.0538)	**(0.0505)**	**(0.0369)**	**(0.0554)**	**(0.0526)**	**(0.0377)**
$FEIL_{t-1}$	**-0.1574*** **	**-0.1881*** **	**-0.0938** **	**-0.2227*** **	**-0.1313*** **	-0.0546	-0.0714	**-0.1454*** **	**-0.1392*** **	-0.0902	**-0.1762*** **	**-0.1161*** **
	(0.0361)	**(0.0443)**	**(0.0365)**	**(0.0589)**	**(0.0405)**	(0.0455)	(0.0568)	**(0.0511)**	**(0.0365)**	(0.0567)	**(0.0543)**	**(0.0405)**
$FEIL_{t-2}$	**-0.1649*** **	**-0.2134*** **	**-0.0808** **	**-0.2440*** **	**-0.1178*** **	-0.0589	-0.0262	**-0.1698*** **	**-0.1400*** **	**-0.1035** *	**-0.1895*** **	**-0.1384*** **
	(0.0352)	**(0.0435)**	**(0.0353)**	**(0.0569)**	**(0.0391)**	(0.0445)	(0.0630)	**(0.0479)**	**(0.0365)**	**(0.0541)**	**(0.0541)**	**(0.0405)**
$FEIL_{t-3}$	**-0.1640*** **	**-0.2088*** **	**-0.0841** **	**-0.2570*** **	**-0.1207*** **	-0.0437	0.0223	**-0.1323*** **	**-0.1514*** **	**-0.1257** **	**-0.2037*** **	**-0.1311*** **
	(0.0368)	**(0.0425)**	**(0.0374)**	**(0.0550)**	**(0.0400)**	(0.0475)	(0.0702)	**(0.0488)**	**(0.0383)**	**(0.0537)**	**(0.0505)**	**(0.0427)**
$FEIL_{t-4}$	**-0.1645*** **	**-0.1975*** **	**-0.0970** **	**-0.2583*** **	**-0.1212*** **	-0.0266	0.0346	**-0.1529*** **	**-0.1452*** **	-0.0938	**-0.2268*** **	**-0.1165** **
	(0.0408)	**(0.0456)**	**(0.0413)**	**(0.0557)**	**(0.0431)**	(0.0539)	(0.0585)	**(0.0546)**	**(0.0426)**	(0.0589)	**(0.0499)**	**(0.0467)**
$FEIL_{t-5}$	**-0.1547*** **	**-0.1811*** **	**-0.0963** **	**-0.2463*** **	**-0.1120*** **	-0.0465	0.1297	**-0.1316** **	**-0.1406*** **	-0.0647	**-0.1879*** **	**-0.1162** **
	(0.0415)	**(0.0485)**	**(0.0408)**	**(0.0575)**	**(0.0423)**	(0.0549)	(0.0951)	**(0.0636)**	**(0.0449)**	(0.0607)	**(0.0508)**	**(0.0500)**
Municip. FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month_year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
F-test	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
# Municip.	348	348	348	348	348	348	348	348	348	348	348	348

Notes*:* Standardized variables. Standard errors clustered at the municipal level. Municipalities with more than 500 inhabitants and all approved projects being of the same size. *** p<0.01, ** p<0.05, * p<0.1. Given the use of month_year fixed effects by default one time period is omitted (2009m12).

The results reported in Table 5 show that the FEIL variable is a very strong and precise predictor of the evolution of the unemployment rate at the municipal level. Moreover, the results for the unemployment rates are considerably robust and provide greater insights into the type of unemployment that the FEIL program impacted and when this impact occurred. Hence, it can be seen that in the first month following project approval and the tender procedure, coinciding with the expected implementation 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.

Figure 6. Pre-trends on the Impact of the FEIL Projects on Unemployment.



Note: vertical lines on each dot represent a 95% confidence interval.

This profile of the unemployed, who in principle benefited from projects funded by FEIL at the local level, is persistent over time and appears to be coherent with the type of project approved at the municipal level, many associated with 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 appears to have met its target and reduced local unemployment rates, especially in the construction industry, by about 22-25 percent. In short, the first-stage results are robust and coherent with the aims of the FEIL program. Moreover, the results presented in Figure 6, in

which the leads (pre-treatment effects) and lags (post-treatment effects) of the treatment variable are plotted for selected unemployment rates, reinforce the empirical strategy and appear to confirm that there are no pre-trends that might call into question the results obtained.

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

In this section I discuss the IV results where the FEIL program is used to instrument total unemployment rates.[28] Table 6 presents the detailed results for property crimes, while Appendix B contains the full set of results for crimes against the person and other types of crime.

In general, the results point to the significant and positive impact of unemployment on property crimes. A closer inspection of these results shows that "Robberies" and "Serious car thefts" (i.e. offenses including some sort of violence) are the crimes that react most promptly to a decrease in unemployment. The highest estimated impact is obtained three periods after the project's expected initiation date, with a sizeable and significant reduction in nearly all types of property crime rates. "Serious thefts" and "Damage to property" are the only types of property crime not affected, at any point in time, by the decrease in unemployment rates associated with the FEIL program. Like the first-stage estimates, Figure 7 plots pre-treatment effects for selected property crime types to show that the treatment variable seems to have an impact only after its implementation (Figure 7 is, hence, the graphic representation of the coefficients presented in Table 6).

The timing of the estimated impacts of unemployment rates on crime is consistent with different size projects being implemented and with a number of periods having to elapse before a reaction is observed in crime rates. In Appendix B, the results for crimes against the person and other types of crime present a picture that is consistent with expectations and the outcomes typically reported in the literature (see Freedman and Owens, 2016) for crimes of this type, characterized by a lower degree of economic motivation: that is, a less significant impact of unemployment on crime. Quantitatively, and taking into account that the first-stage estimates suggested that the FEIL program reduced total unemployment by an average of 16 percent, a

[28] In Appendix C, Table C.1 shows the IV results for crime rates obtained when instrumenting the male unemployment rate with the FEIL projects. In Appendix E, Table E.1 reports the OLS results. The latter show an estimated coefficient that is downwardly biased, becoming negative even in some regressions (results not shown here but available upon request), which could result in misleading conclusions being drawn about the relation between unemployment and crime if endogeneity is not properly accounted for.

one-percent increase in the unemployment rate also increased total property crime by approximately 18 percentage points.[29]

The results presented up to this juncture seem to indicate that the FEIL program reduced unemployment and, as consequence, crime rates fell significantly. Moreover, the first-stage estimates (see Table 5) suggest that a particular type of unemployed worker benefited most from the program. Although the impact of the FEIL in the first month following the implementation of the project is significant for many types of unemployment, the point estimates are highest for male unemployed workers engaged in the construction industry, with a low level of education and, primarily, aged between 25 and 40 (middle-aged). This profile remains consistent when we examine the temporal impacts of the FEIL on unemployment rates. As expected, the impact on unemployment is not restricted to just one period but extends in accordance with the type of project, primarily those involving local public infrastructure. When analyzing the timing of the impact of the FEIL program on unemployment rates, this is found to remain strong for male unemployed workers in the construction industry, both middle-aged and mature, the results regarding education and age being less distinctive here.

The second-stage results show that, as a consequence of the reduction in unemployment, the crime rate also fell significantly. The impact was immediate in the case of property crimes, above all robberies and serious car thefts, but an impact was also found when analyzing the (instrumented) lags of the variable of interest. To provide a more precise interpretation of these results, it is necessary to identify the various channels that might be operational.[30]

[29] Note that this broad picture is also obtained when using the three-month approval window (December 2008 – February 2009) and only restricting the municipalities to those with more than 500 inhabitants in the IV framework (see Appendix D). In addition, Tables E.2 and E.3 and Figure E.1 in Appendix E show the reduced-form effects of the FEIL on crime. As expected, the relation is statistically significant and shows a consistent negative sign with magnitudes for total property crimes ranging between 12 and 18%, these effects being significant for many periods following the beginning of the implementation of the local infrastructures projects.

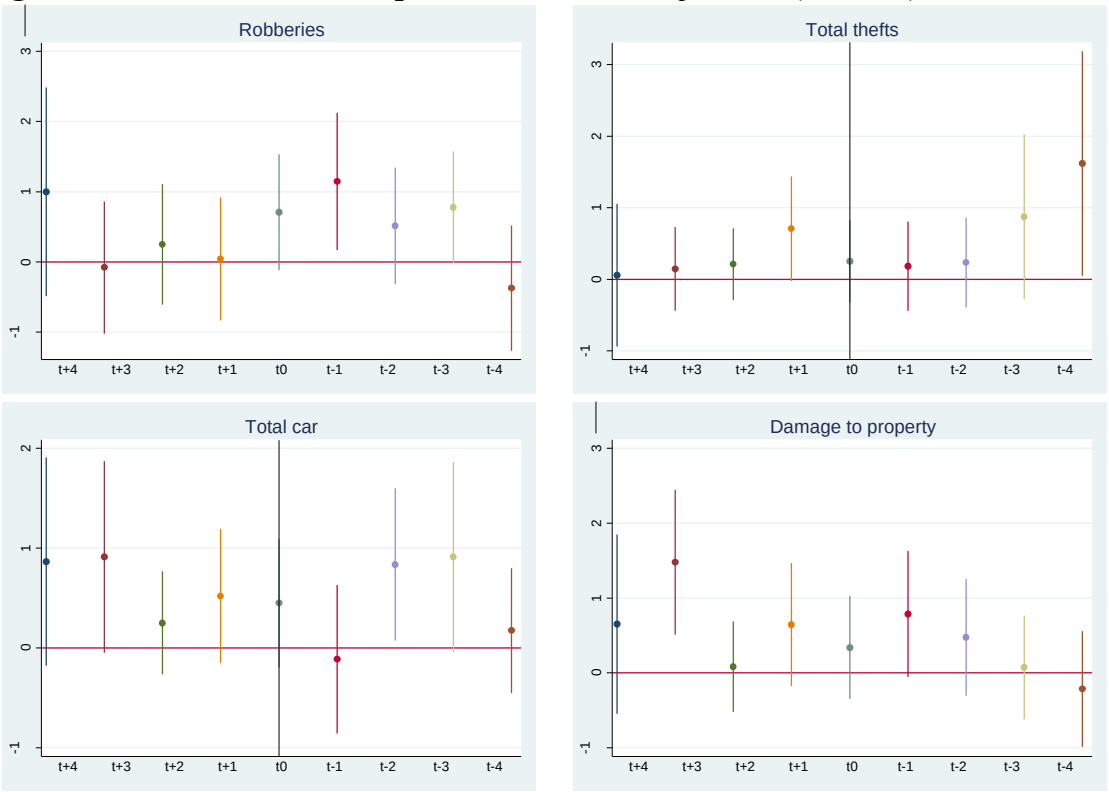
[30] I performed the main estimation by splitting the sample between municipalities with average number of projects above or below 100,000€ for the main types of crime (Table E.4 in Appendix E). Note that splitting the sample reduces the number of observations, especially for the subsample of municipalities with projects for less than 100,000€ and, consequently, this significantly reduces the power of the estimations. In any case, the results obtained for the two subsamples are robust and seem to indicate that the overall results hold. Moreover, it seems that the results are specifically due to municipalities undertaking large scale projects for which reductions in unemployment can be expected to be higher.

Table 6. 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*	*Damage to property*
(*Total_Unemp.* = $FEIL_{t0}$)	**0.6980****	**0.7094***	0.2521	-0.1166	**0.4709***	0.4493	**0.5486***	-0.2118	0.3405
	(0.3294)	**(0.4202)**	(0.2919)	(0.3271)	**(0.2761)**	(0.3275)	**(0.3260)**	(0.3369)	(0.3515)
(*Total_Unemp.* = $FEIL_{t-1}$)	**0.7700****	**1.1464****	0.1847	0.2145	0.1729	**0.8361****	**0.7907****	0.3071	0.4757
	(0.3784)	**(0.4987)**	(0.3179)	(0.3429)	(0.3419)	**(0.3886)**	**(0.3865)**	(0.3511)	(0.3977)
(*Total_Unemp.* = $FEIL_{t-2}$)	**0.7928****	0.5138	0.2355	0.4141	0.9574	**0.9118***	**0.7958***	0.5375	0.0733
	(0.3643)	(0.4244)	(0.3193)	(0.4437)	(0.5873)	**(0.4851)**	**(0.4596)**	(0.3613)	(0.3539)
(*Total_Unemp.* = $FEIL_{t-3}$)	**1.1428****	**0.7796***	0.8743	0.8847	**1.6748****	0.1741	-0.0619	**0.7541***	-0.2132
	(0.5611)	**(0.4064)**	(0.5868)	(0.6292)	**(0.7496)**	(0.3199)	(0.3132)	**(0.4181)**	(0.3935)
(*Total_Unemp.* = $FEIL_{t-4}$)	0.6847	-0.3722	**1.6185****	0.4652	**0.6683***	0.5195	0.5567	-0.0085	0.6465
	(0.5307)	(0.4558)	**(0.8003)**	(0.3740)	**(0.3878)**	(0.3430)	(0.3543)	(0.3664)	(0.4186)
(*Total_Unemp.* = $FEIL_{t-5}$)	**0.7743***	0.0442	**0.7082***	0.0427	-0.2838	0.7196	0.7291	0.1162	0.6294
	(0.3989)	(0.4477)	**(0.3742)**	(0.2801)	(0.3403)	(0.4803)	(0.5032)	(0.3278)	(0.4047)
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: See Table 5.

Figure 7. Pre-trends on the impact of the FEIL Projects on (selected) Crime Outcomes.



On the one hand, it could be that the FEIL program reduced unemployment and, hence, improved general economic conditions in the municipality, reducing the overall incentives to commit crimes in the case of both those who obtained a job and those who did not. Alternatively, and in keeping with the reasoning and evidence provided by Freedman and Owens (2016), if the fall in unemployment only benefited a specific group of individuals (and not the whole population of the municipality), this might have increased the supply of criminal opportunities for those that failed to benefit from the FEIL project and, as a result, we should observe an increase in the crime rates. Given that just the opposite is observed here would seem to suggest that, at the very least, there is a counterbalancing force that reduces the crime rate when there is a fall in unemployment (even if only a fraction of the total population receives direct benefits from the FEIL).

On the other hand, this counterbalancing force may well constitute the fact that the individuals facing the worst *ex ante* economic conditions (i.e. unemployment), and possibly more likely to be involved in illegal activities, were precisely the ones that received a positive shock in their economic expectations and, as a result, decided not to engage in illegal activities. If this were the case, the results, more in keeping with the findings of the traditional economics of crime literature, would point to some sort of incapacitation effect.

It is by no means easy to assess the precise mechanism responsible for the results observed; however, advantage can be taken of the detailed crime dataset available to explore further the relation at stake. Thus, if an incapacitation effect is operative, and the reduction in crime is due to the employment of those individuals more likely to commit a crime, at least two things should be observed. First, there should be a significant reduction in certain crime types during working hours, given that potential offenders now spend most of their time engaged in legal activities. Second, there should also be a reduction in the probability of recidivism with a fall in the unemployment rates or, in other words, those individuals who had previously committed a crime, but are now the beneficiaries of employment thanks to a FEIL project, are less likely to reoffend.

Table 7 presents the second-stage results when disaggregating crimes according to the time of the day when they were committed. The day is divided between working hours (8:00 am – 17:00 pm) and non-working hours (the rest). As shown in Montolio and Planells-Struse (2016), distinct hourly patterns can be associated with the type of crime, making it important to analyze each type separately.[31] The results obtained provide interesting insights. First, the number of robberies (and to a lesser extent minor thefts) seem to fall during non-working hours, when, however, such offences generally seem to be more common. Interestingly, the numbers of serious car thefts (involving some sort of violence) and minor car thefts (and to a lesser extent serious thefts) fall during working hours, which, in principle, would be consistent with an increase in the employment of those individuals more likely to be involved in "petty crimes" of this type.[32]

Note that I do not make any claims in this direction. All I do is provide further evidence for the causal impact of unemployment on crime rates which may help in understanding the mechanisms at work. As expected, the results for crimes against the person (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.

[31] The authors report three general patterns: first, crimes related to leisure activities (road safety and drug-related crimes) present peaks late at night, low rates during the daytime and rates that increase as the evening progresses; second, crimes against property (robberies, thefts and damage to property) present high rates during working hours, especially for thefts, and low rates at night, with robberies, in particular, presenting 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) present rates that peak in the evenings.

[32] Note that the results for minor thefts do not seem to show a distinctive pattern in terms of the impact of unemployment on the time the offence was committed. The relationship is positive and significant for both working and non-working hours, especially three/four months after a FEIL project has been implemented; however, with a higher point estimate for working hours than for non-working hours.

Table 7. 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.* = $FEIL_{t0}$)	**0.6980****	0.3077	**0.8466****	**0.7094***	0.2593	**0.7489***	-0.1166	-0.2056	0.0281
	(0.3294)	(0.3018)	**(0.3704)**	**(0.4202)**	(0.3778)	**(0.4340)**	(0.3271)	(0.3397)	(0.3288)
(*Total_Unemp.* = $FEIL_{t-1}$)	**0.7700****	0.0964	**1.1619*****	**1.1464****	0.0883	**1.4528*****	0.2145	0.1158	0.0098
	(0.3784)	(0.3595)	**(0.4391)**	**(0.4987)**	(0.4747)	**(0.5182)**	(0.3429)	(0.3080)	(0.3762)
(*Total_Unemp.* = $FEIL_{t-2}$)	**0.7928****	**0.8132****	0.5265	0.5138	0.0876	0.6159	0.4141	**0.5731***	-0.2483
	(0.3643)	**(0.3679)**	(0.3920)	(0.4244)	(0.4108)	(0.4173)	(0.4437)	**(0.3333)**	(0.3976)
(*Total_Unemp.* = $FEIL_{t-3}$)	**1.1428****	0.8278	**1.0821****	**0.7796***	0.2580	**0.8429***	0.8847	0.3912	0.2420
	(0.5611)	(0.5086)	**(0.5143)**	**(0.4064)**	(0.3559)	**(0.4455)**	(0.6292)	(0.4527)	(0.3663)
(*Total_Unemp.* = $FEIL_{t-4}$)	0.6847	0.7041	0.4529	-0.3722	-0.4094	-0.1925	0.4652	0.5683	0.7866
	(0.5307)	(0.4660)	(0.5136)	(0.4558)	(0.3805)	(0.4605)	(0.3740)	(0.5067)	(0.5677)
(*Total_Unemp.* = $FEIL_{t-5}$)	**0.7743***	0.4492	**0.8379****	0.0442	-0.4303	0.3740	0.0427	0.5405	0.1700
	(0.3989)	(0.4078)	**(0.3989)**	(0.4477)	(0.4852)	(0.4188)	(0.2801)	(0.4110)	(0.3659)
	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.* = $FEIL_{t0}$)	**0.4709***	0.2855	**0.5020***	**0.5486***	**0.5665***	0.3467	-0.2118	-0.3842	0.0368
	(0.2761)	(0.3016)	**(0.2762)**	**(0.3260)**	**(0.3409)**	(0.3016)	(0.3369)	(0.4458)	(0.2229)
(*Total_Unemp.* = $FEIL_{t-1}$)	0.1729	0.1995	0.1289	**0.7907****	0.1202	0.3108	0.3071	-0.6449	**-0.9649***
	(0.3419)	(0.2900)	(0.3640)	**(0.3865)**	(0.4043)	(0.2926)	(0.3511)	(0.7264)	**(0.5002)**
(*Total_Unemp.* = $FEIL_{t-2}$)	0.9574	0.2843	-0.0363	0.7958*	**0.8749****	0.4401	0.5375	0.4450	0.0375
	(0.5873)	(0.3719)	(0.3886)	(0.4596)	**(0.4211)**	(0.3413)	(0.3613)	(0.3270)	(0.3814)
(*Total_Unemp.* = $FEIL_{t-3}$)	**1.6748****	0.6148	0.9783	-0.0619	0.6892	0.6379	**0.7541***	**0.7705****	0.0723
	(0.7496)	(0.4738)	(0.6240)	(0.3132)	(0.5074)	(0.4021)	**(0.4181)**	**(0.3673)**	(0.3042)
(*Total_Unemp.* = $FEIL_{t-4}$)	**0.6683***	**1.5087****	1.1786	0.5567	-0.1632	0.0620	-0.0085	**0.6168****	0.4768
	(0.3878)	**(0.6378)**	(0.7503)	(0.3543)	(0.4024)	(0.2901)	(0.3664)	**(0.2978)**	(0.4347)
(*Total_Unemp.* = $FEIL_{t-5}$)	-0.2838	0.2474	**0.9063****	0.7291	0.4862	0.4421	0.1162	-0.0109	-0.0022
	(0.3403)	(0.3611)	**(0.4217)**	(0.5032)	(0.3558)		(0.3278)	(0.4228)	
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: see Table 5.

A final piece of evidence is taken from a different dataset, albeit one also provided by the *Mossos d'Esquadra*. I also make use of an offenders' dataset for the same period of analysis. This dataset provides monthly municipal information about known offenders and, importantly, whether they have previous offenses to their name. Thus, in the IV framework, I can estimate the impact of unemployment on the probability of recidivism in a given municipality and in a given month as a result of the implementation of a FEIL project. Note that this dataset is quite distinct in that I am now interested in offenders than in the recorded number of crimes. However, while for some crimes it is relatively easy to identify the offender (crimes against the person), for others it is more difficult (the case, for example, of 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 aspects in mind, Table 8 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 the person and other types of crime).

Despite obtaining less robust results than in the case of the impact of unemployment on crime rates, the results for the probability of repeat offending also show a positive relation with unemployment rates. The results provided by the present specification show that the reduction in unemployment in the municipalities attributable to the FEIL is associated with a lower probability of individuals' committing repeat offenses. There is a certain parallelism here between the results in Table 6 and those in Table 8. Note that a few months after the implementation of a FEIL project, unemployment rates fell (especially among male construction workers), the total number of property crimes fell (especially during non-working hours) and the probability that those committing the crimes were repeat offenders also fell, indicating that previous offenders were less likely to reoffend. The results for crimes against the person and for other types of crime (drug-related crimes and crimes against road safety) only show a positive relation between unemployment and recidivism in the case of gender violence (after three months of a project having been implemented) and, especially, in the cases of threats and drug-related crimes.

Table 8. IV estimates for the Impact of Total Unemployment Rate on Recidivism in Property Crimes

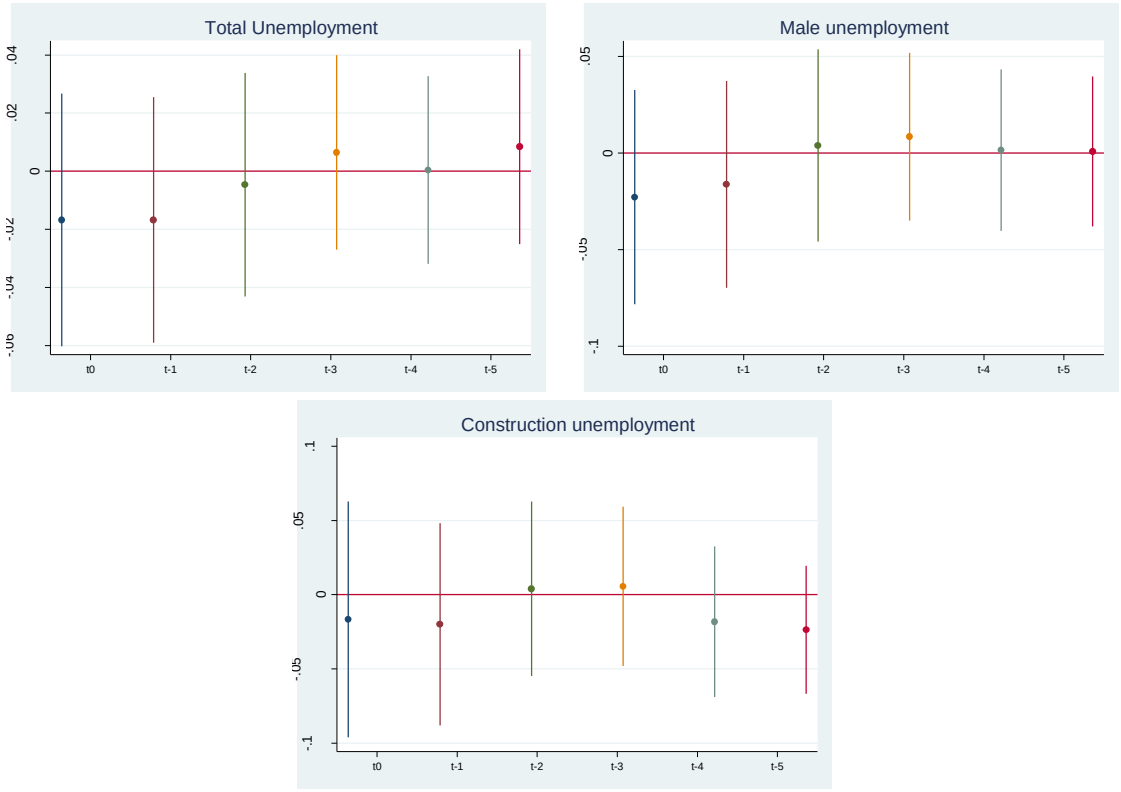
Second-stage	*Total property*	*Robberies*	*Serious thefts*	*Minor thefts*	*Serious car*	*Minor car*	*Damage to property*
(*Total_Unemp. = FEIL$_{t0}$*)	**0.4935****	**0.4307****	0.0935	0.0029	0.0912	-0.0048	**0.3489****
	(0.2104)	**(0.1756)**	(0.1101)	(0.0994)	(0.0916)	(0.0032)	**(0.1647)**
(*Total_Unemp. = FEIL$_{t-1}$*)	-0.0180	0.2583	-0.1914	-0.0388	-0.0217	-0.0051	-0.0786
	(0.2243)	(0.1591)	(0.1286)	(0.1093)	(0.0955)	(0.0035)	(0.1484)
(*Total_Unemp. = FEIL$_{t-2}$*)	0.2762	0.1404	**0.2968****	-0.1279	**0.1600***	-0.0049	0.0398
	(0.2012)	(0.1333)	**(0.1299)**	(0.1156)	**(0.0916)**	(0.0033)	(0.1752)
(*Total_Unemp. = FEIL$_{t-3}$*)	0.2641	**0.3322****	-0.0471	-0.0081	0.0815	-0.0049	0.0290
	(0.1989)	**(0.1631)**	(0.0849)	(0.1035)	(0.1105)	(0.0033)	(0.1420)
(*Total_Unemp. = FEIL$_{t-4}$*)	**0.4927****	0.0211	0.0811	0.1472	0.1811	-0.0049	0.0418
	(0.2327)	(0.1222)	(0.1184)	(0.1247)	(0.1292)	(0.0033)	(0.1313)
(*Total_Unemp. = FEIL$_{t-5}$*)	0.0748	0.0360	0.2192	-0.0700	0.0026	0.0332	0.1845
	(0.2067)	(0.1515)	(0.1408)	(0.1144)	(0.0939)	(0.0378)	(0.1546)
Municipality FE	YES	YES	YES	YES	YES	YES	YES
Month_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: See Table 5.

6.3 Placebo Tests

Finally, I perform various placebo tests to confirm the above findings. First, Figure 8 shows the estimates resulting from the random assignation of municipalities to the months in which the projects were approved; that is, I take the actual months but assign municipalities to each month randomly.[33] I present the results for the main types of unemployment and for total property crime rates and crimes against the person. The results of this exercise also hold for all types of unemployment and disaggregated crime types. Second, Figure 9 presents the results for the same outcome variables but when the projects are randomly assigned to the previous year; that is, between December 2007 and March 2008. As expected, in both cases the variable of interest has no impact on any of the relevant variables, confirming the identification strategy and the results obtained in the previous section.

Figure 8. Placebo Test (I): Random Assignation of FEIL Projects across Municipalities



[33] 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 virtually unchanged when I randomly assign municipalities between only two approval months.

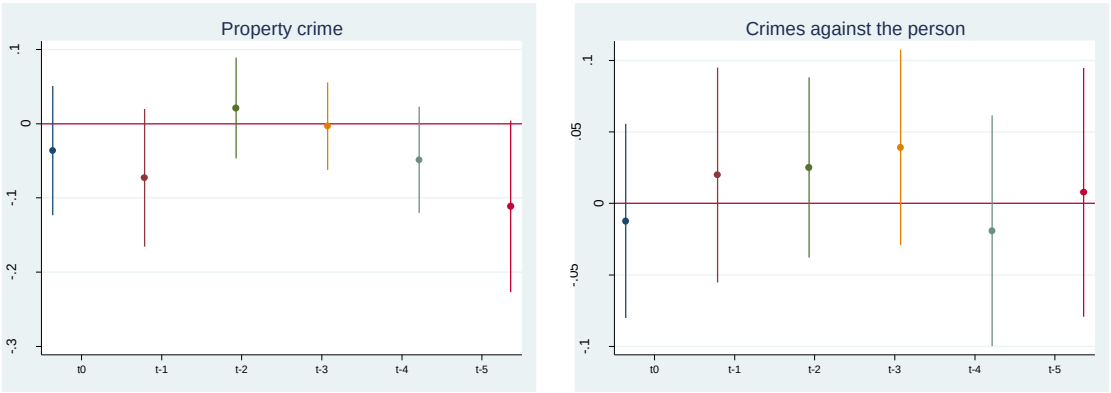
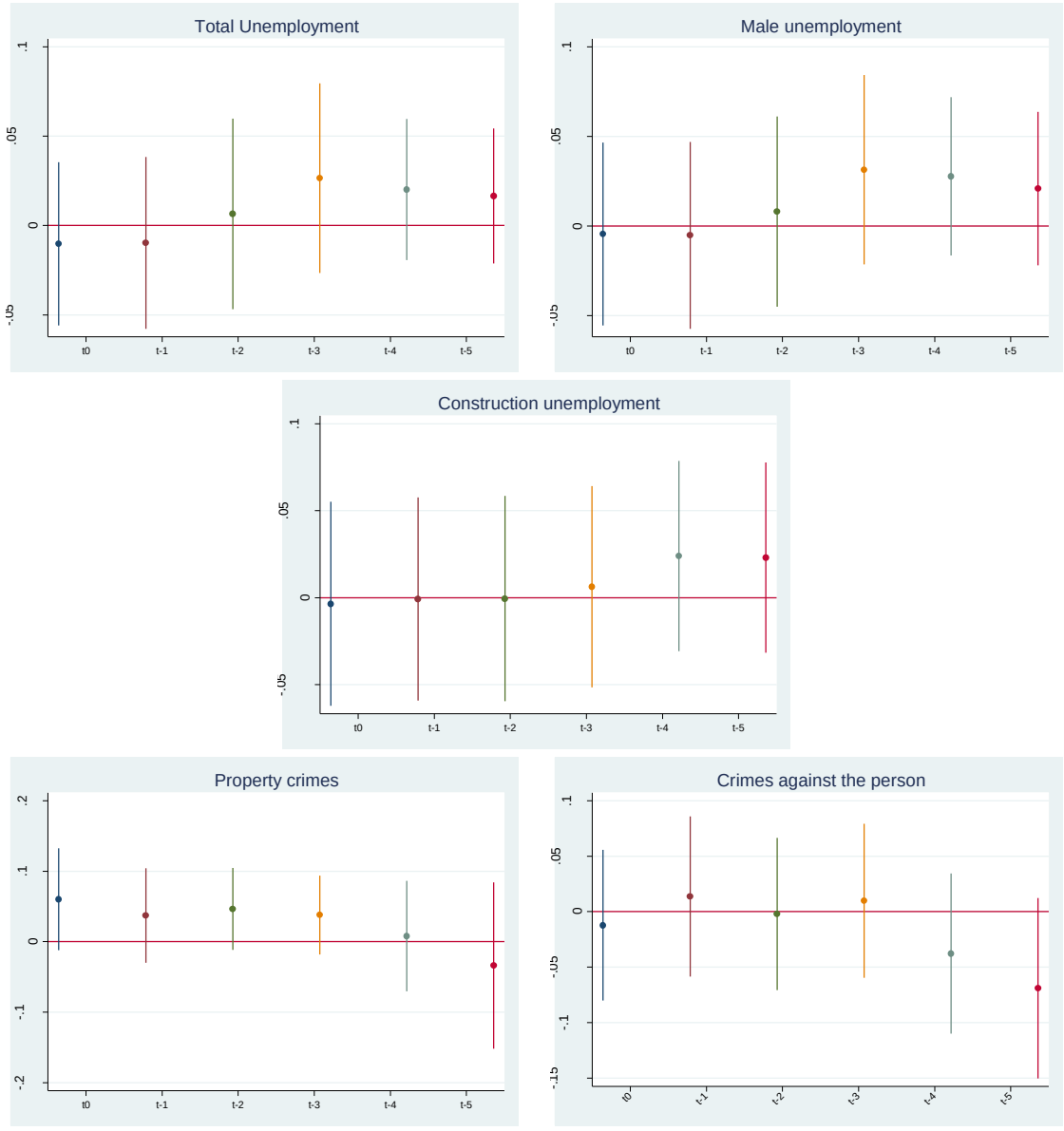


Figure 9. Placebo Test (II): Random Assignation of FEIL Projects across Municipalities and Time



7. Concluding Remarks

The relationship between unemployment and crime is one that has yet to be clearly resolved in the economic literature. The variables interact via such a variety of channels that it is difficult to assess the direction of causality between them. In addition, any discussion of the question is further complicated by a range of practical problems, including possible measurement errors in the variables of interest, the timing of the respective events, and the level of aggregation used in testing the relationship. In the face of the obstacles hindering the estimation of the effect of unemployment on crime, a promising research strategy has emerged for obtaining unbiased estimates of the impact of labour outcomes on crime at the municipal level. This involves undertaking analyses of the evolution of municipal crime rates in those municipalities where unemployment rates have changed for reasons unrelated to its level of criminality. This is the approach exploited in this paper.

The local investment fund, FEIL, was planned, designed and executed to increase public investment at the local level in Spain by financing the construction of new local public works infrastructure. The works were scheduled for immediate implementation from the beginning of 2009, under the control and responsibility of Spain's local authorities. I show that the timing of the approval granted to implement a FEIL project presented by a municipality was random or, at least, it was not determined by any variable directly involved in my research strategy or which might bias the results presented. Thus, I am able to use the FEIL program to identify the causal relation between unemployment and crime.

The results obtained appear to be in line with the Beckerian view of the relation between labour market opportunities and crime rates. An increase in job opportunities increases the opportunity cost of committing a crime and, hence, reduces the incentives to engage in illegal activities. Based on the analysis reported, I conclude that the FEIL program had a significant impact on registered unemployment rates, especially among unemployed male construction workers with a low level of education and aged over 25 (middle-aged and mature).

The significant impact of the FEIL program on labour outcomes has direct consequences for the crime rate: thus, the reduction in unemployment rates had a positive effect on crime rates, which likewise fell significantly. The results show a clear fall in the number of economically motivated crimes and the timing of these effects matched expectations when considering the nature (local public infrastructure) of the projects being implemented. Moreover, there is empirical evidence that the impact on unemployment rates reduced the number of (serious and minor) car offenses and robberies, especially those being committed

during working hours, and that it also reduced the probability that individuals committing property crimes were repeat offenders. Both trends are compatible with a possible incapacitation effect: that is, if the individuals who *ex ante* were exposed to the worst economic conditions (i.e. unemployment) and, as a result, more likely to engage in criminal activities, were the ones that actually benefited from the FEIL program, then they would have seen their labour opportunities improve and would be less likely to engage in illegal activity. Note that the results reported here coincide with a period of incipient economic crisis, which started when unemployment rates were relatively low (at least by Spanish standards) and there was some "margin" for the labour market to absorb this initial impact. Although the results control for possible fluctuations attributable to the economic cycle, by means of time fixed effects, the results may have been different (probably strengthened) if labour market conditions had differed.

In conclusion, the outcomes reported here should go some way to improving our 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 readily perceived and borne by citizens. Moreover, the research strategy adopted verifies the robustness of the causal relation between unemployment and crime and, hence, improves the much needed definition of social policy objectives that can be transferred into policies that promote investment in infrastructure around the world.

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