Jobs, News and Re-offending after Incarceration[*]

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ABSTRACT

While theoretically important, the relationship between crime and employment is difficult to measure empirically. This paper addresses major identification challenges by exploiting high frequency data of daily online postings on job openings and closings at the county level, merged with individual-level administrative data about *all inmates* released from French prisons. We find that people who are released when jobs are being created are less likely to recidivate; conversely, people who are released when jobs are being cut are more likely to recidivate. We further show that *news* on job creation matters, over and beyond actual employment opportunities, suggesting implications for crime-control policies. From a methodological standpoint, this paper demonstrates how using media and online information on jobs can generate higher-frequency variation than administrative employment data, and help to overcome identification challenges to capture effects of variations in job market opportunities, especially when combined with other administrative sources.

1. Introduction

In the United States, incarceration rates today are seven times higher than in the seventies (Levitt, 2004) and twice as high in European countries (Buonanno et al. 2011). A less discussed side effect has been the increase in the number of people released from prison. While the growth in prison population is associated with some reduction in crime rates (Levitt, 1996; Raphael and Stoll, 2013), many people who have been incarcerated reoffend: 67.5% of ex-prisoners released in 1994 in the United States had been re-arrested for a new offense three years later (Langan and Levin, 2002). Recidivism in itself poses important problems: many people cycle through the criminal justice system, and understanding how to reduce re-offending is in itself an important policy challenge.

One important theoretical determinant of crime is labor markets. Former inmates may be cycling in and out of prison because they have a hard time finding jobs or because they do not respond to incentives created by legitimate earnings opportunities: they might be screened out by employers in legal labor markets, or lack information on suitable jobs. In the second case, improving former inmates' chances to access legitimate jobs would be ineffective in reducing recidivism, while in the first case an increase in the probability of accessing legitimate jobs should decrease former inmates' propensity to reoffend. Understanding how former inmates respond to factors that might affect their probability of finding jobs or getting better legitimate earnings could be crucial to design effective crime control policies. From both a theoretical and a policy perspective, it is important to understand the marginal effectiveness of increasing the severity of sanctions, relative to increasing former inmates' probability of finding jobs – the two main determinants of crime in Becker's seminal model. To answer this question, we need clean evidence about former inmates' responses to variation in the relevant labor markets conditions.

This paper focuses on an important and quite unexplored determinant of likelihood of finding a job: how information about local labor market conditions affects re-offending upon release from prison. The interaction between labor markets and crime has been modeled theoretically, but relatively rarely investigated empirically. The empirical literature on recidivism after prison has mainly focused on the effect of

penal policies, such as sentence length (Kuziemko, 2013; Drago, Galbiati and Vertova 2009), alternative to incarceration (Di Tella and Schargrodsky, 2013), detention conditions (Chen and Shapiro, 2007 ; Drago, et al., 2011), or encounters in prison (Bayer et al., 2009 ; Drago and Galbiati, 2012; Ouss, 2011). Overall, these results indicate that former inmates respond to changes in the environment, but it is far from obvious that former inmates would also respond to variation in local labor market conditions, real or perceived. The standard economic model of criminal behavior (Becker, 1968; Ehrlich, 1973) implies that after release from prison, former inmates should decrease criminal activities when they face an increase in job availability: when more jobs are available, all else equal, the opportunity cost of time spent both in criminal activity and in prison if apprehended and convicted rises.[2] However, for this prediction to hold, former inmates would have to respond to variations in incentives created by changing labor market conditions. Although intuitive, this is not obvious since people entering prison tend to not have been employed in the formal sector (Western and Pettit, 2005; Loeffler, 2013), and thus they may not be responsive to this margin. This could also be the case if they lack relevant human capital or information about job availability, if they are somehow barred from the formal job market, or if experiences in prison have otherwise overwhelmingly increased returns to crime.

Understanding whether and how former inmates respond to variations in labor market conditions is empirically very challenging. Identification is hampered by a number of confounding factors correlated with both labor markets and offending. People with better jobs might elect to move out of higher crime areas, leading to a non-causal correlation between crime and lower job availability. In this paper we address this major identification challenge by using very granular data on releases from prison and on job creations and cuts. We build a novel dataset by combining administrative data on all inmates released from France in 2009-2010 and high-frequency information on media coverage of job flows, which we obtain at the *daily-county* level. For each former French inmate, we construct an index reporting the number of news stories

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[2] Job-search models of labor markets and crime also predict, from another angle, that more job opportunities for individuals just released from prison would reduce recidivism (Engelhardt, 2010).

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and classified advertisements on job openings and cuts in their county of residence in the thirty days following their release from prison. Our identification strategy exploits within county, daily variations in the flow of information about job openings and destructions. The high frequency of our data coupled with spatial variation allows us to control both for fixed and time-dependent unobserved heterogeneity. We exploit random variations in daily announcements and in exact timing of release from prison to identify the effects of news about job flows on recidivism.

Our results are consistent with predictions from the standard economic model of crime. We find that an increase in the number of announcements on job openings in their county of residence in the month following an inmate's release from prison decreases the probability of reoffending (a one standard deviation increase in stories on job openings induces a 10% reduction in the probability of a new incarceration within six months), while an increase in the number of announcements on job cuts increases the probability of re-offending. Such an effect holds across types of crime and socio-economic backgrounds of former inmates. The effect of an increase in announcements on job openings may be due to a variety of reasons. Digging into mechanisms, we look at the effect of *media coverage* of newly created jobs (beyond new vacancies) on recidivism. Exploiting the timing of announcements and job start dates, we show that the main driver of our results is the information channel. This suggests that providing information about job availability to former inmates could be an effective and low-cost way to help reduce recidivism.

Our exercise is the first documenting the impact of *news* on jobs on reoffending. We introduce two main innovations with respect of the existing literature. First we use a novel identification strategy based on the fine granularity of our data, exploiting daily, individual-level variations. Second, we are able to provide policy recommendations that could be effective in reducing crime at a relatively low cost. Moreover, our study shows how alternative sources of information about job flows (i.e., media coverage and classified advertisements) can be useful to grasp some of the mechanisms underlying individual responses to job market conditions by generating more granular job market indicators. In the same vein, recent work has used data from Careerbuilder.com to look at worker mobility (Marinescu and Rathelot, 2014) and general equilibrium effects of increased unemployment benefits (Marinescu, 2014).

Using online job postings as a finer-grained proxy for unemployment could have applications to many topics in labor studies, both to improve identification, and to capture the exact timing of events. Moreover, this paper demonstrates the usefulness of complementing administrative data with either publicly available newspaper content data, or more generally, data exhaust generated online. In that way, our paper also suggests ways to expand the literature on the effects of media content and diffusion on offending (Dahl and Della Vigna, 2009; Bhuller et al., 2013).

The paper is organized as follows. Section 2 offers a conceptual framework and discusses related literature; Section 3 presents institutions and data; Section 4 exposes our empirical strategy and main results, which are discussed in Section 5. Section 6 concludes.

2. Jobs and Recidivism: Conceptual Framework

Consistent with the standard economic approach to crime (Becker, 1968), we assume that after release from prison an individual decides on offending by comparing the relative utility of time spent working or in other activities, relative to the utility derived from committing crimes, net of the costs of incarceration weighted by probability of apprehension. We focus on the job search activity, from which standard models usually abstracts.

We assume that people have beliefs about their likelihood to find a job upon release from prison. In the absence of additional information, they would make the decision to commit a crime based on their beliefs of their likelihood of finding a job: the higher (lower) the prospects of finding a job, the lower (higher) the likelihood that they would reoffend, due to the increase (decrease) in opportunity costs of crime. But beliefs could be shifted by additional information about the labor market, which could be obtained from media coverage of local employment and classified advertisements.

For ease of exposition, we assume that individuals are unemployed when released, and that they immediately start to look for a job. At any moment, an individual might receive a job offer, the probability of which increases with search effort, which in turn

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depends on the costs of searching. An individual can work, offend, or search for a job, which for simplicity's sake are assumed to be mutually exclusive. An individual would decide to commit a crime if the expected utility from criminal activity (net of the disutility of incarceration if apprehended) is higher than the expected utility of continuing the job search, which in turn depends on the probability of finding a job. After receiving a job offer, a person then could decide whether to accept it; refuse and continue search activity; or refuse and offend. If they accept the job offer, they no longer offend; if they refuse, while in an environment where the probability of receiving a better offer is high enough, they will keep on searching; and if the probability of receiving a better offer is too low, they will stop searching and may engage in crime. In any case, the offending decision at a given point in time depends on the expected probability of finding a job.

There could be two channels through which news on jobs could affect recidivism of people release from prison. All else equal, if news about newly available jobs is a good indicator of new job vacancies, an increase in news should reflect tighter job market conditions and thus a higher probability of finding jobs.[3] More jobs might result in greater likelihood to find a job, and therefore directly change the opportunity cost of crime. When former inmates find jobs more easily, their opportunity cost of reoffending should increase. This is what we label as the direct effect of job openings. News could also provide information about jobs, *conditional* on their availability. Information could help former inmates update their prior assumptions about the probability of finding a job. We label this the information channel.

If news about jobs positively impacts individual beliefs about the probability of finding a job, an increase in news about job openings should be associated with a decrease in reoffending. Information about the existence of particular jobs also lowers search costs: former inmates can target their search effort to firms with vacancies. Note that this second channel works by reducing offending even keeping constant the number of actual jobs available: by decreasing search costs, news would affect the propensity to reoffend either if searching for a job prevents individuals from crime (through incapacitation), or if higher search efforts increase likelihood of actually

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[3] Holzer, Raphael and Stoll (2004) also document that employers are more likely to hire people with felony records – and thus, plausibly, former inmates – in better economic conditions.

finding a job, keeping all else constant.

In our empirical analysis, we assume that the relevant job market for a newly released individual is the job market in his county of residence upon entering prison.[4] We first document that news about job openings correlates with official measures of new job vacancies, and that this correlation is stronger for sectors where former inmates are more likely to potentially be employed, such as construction (Schnepel, 2013). We then document that keeping all else equal, an increase in news about job openings in the county of residence is associated with less recidivism (the opposite holds for news about job cuts). This is consistent with the idea that an increase in job market tightness should reduce reoffending by increasing former inmates' employment (the opposite holds for job cuts, which map into an increase in unemployment). We then explore whether this is mostly driven by the direct effect of new available jobs or if information itself plays a role in reducing offending, and find the latter to hold.

More specifically, we study whether news still affects recidivism after controlling for measures of job market tightness, computed from official labor market statistics. We find that publicity about jobs matters, beyond job availability. We then exploit the timing of the job announcements and their content to disentangle the effect of available jobs (the first channel), from the effect of information about existing jobs (the second channel). If conditional on available jobs information plays a role in reducing offending, this suggests that, in order to reduce offending, effective policies should aim not only to encourage the creation of new jobs but also, all else equal, to provide information about job availability to former inmates. Finally, we provide evidence about the differential effect of news about jobs according to broadband Internet availability. The idea is that conditional on existing newly available jobs, broadband Internet facilitates the collection of information on these available jobs.

Prior studies have looked at the aggregate relation between labor market conditions and crime. This literature has tried to document the robustness of the theoretical prediction of a positive relation between unemployment and crime. The evidence

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[4] Among people who recidivated, 89% listed the same county as their place of residence for both incarceration spells. In additional analyses, we also look at jobs in the county where the prison of release is located, when it differs from the county of residence.

provided by these studies is mixed: most studies find little effect for property crimes and mixed evidence for violent crime rates when using OLS regressions (Raphael and Winter-Ebmer, 2001; Machin and Meghir, 2004; Oster and Agell, 2007; Lin, 2008; Buonanno et al. 2011). Instrumental variables estimates find an increase in property crimes with higher unemployment (Gould et al., 2002; Raphael and Winter-Ebmer, 2001; Oster and Agell, 2007; Fougere, et al., 2009), confirming the relevance of confounding factors when looking at aggregate data. Even if these results are consistent with the standard model of crime, it is hard to conclude that policies aimed at reducing unemployment should have a large impact on reducing reoffending. Indeed, it is difficult to infer the behavior of former inmates by observing aggregate responses of crime rates to unemployment since such an exercise would require a number of restrictive behavioral and statistical hypotheses (Durlauf et al., 2010).

Only a few papers have focused on the effect of labor market conditions on offending behavior. Summer jobs for at-risk youth have been shown to reduce violence and victimization (Heller, 2014; Gelber, Isen, and Kessler, 2015), and targeted job opportunities for former inmates reduce recidivism (Redcross et. al., 2011). However, we know very little about whether and how these findings carry over to a broader population of adults, moreover to those who are more involved in the criminal justice system. Only a couple of papers have tried to determine the relation between job market opportunities and recidivism for adults. Schnepel (2013) uses data on parolees released from prison in California and examines the effects of variations in local unemployment rates among unskilled individuals, finding that an increase in *relevant* industries' unskilled unemployment is associated with higher recidivism. Also looking at parolees from California, Raphael and Weiman (2007) find moderate effects of county unemployment rates on the likelihood that paroled offenders return to custody.

As opposed to these papers, we study the entire universe of all former French inmates and not only parolees, who are in general selected for good behavior or other positive qualities and may be among the sub-categories of inmates most responsive to incentives. Moreover, our identification strategy includes variations in job flows: while studies on crime and the labor market use unemployment levels, we can look at the effect of both job openings and cuts. Our identification strategy exploits within

county variations in job openings at the *daily* level, thus overcoming the major identification challenges without needing an instrumental variable design. Finally, we are able to provide evidence on the effect of information about job availability conditional on the existing jobs. This last piece of evidence allows us to document the potential power of low-cost interventions that would provide information on job opportunities just before a person is released from prison.

2. Institutions and Data

2.1. Incarceration in France

As of January 2013, there were 66,572 inmates in France, which has an incarceration rate of about 110/100,000.[5] This incarceration rates is orders of magnitude smaller than that of the US (910/100,000 in 2014, Glaze and Kazble, 2014), but around the median of European incarceration rates. Sentences in France tend to be very short: the average time spent in prison is 8.9 months. Thirty-six percent of sentences are shorter than one year and 66% are shorter than three years. A corollary of that is that there is a high turnover rate in French prisons, and there were 87,958 releases in 2012. Fifty-one percent of inmates released had spent less than six months in prison and 91% had spent less than one year in custody. Most people released from prison in France are therefore overall similar, in terms of length of incarceration, to people released from jails in the US.

Relevant to our study of jobs and recidivism, criminal background checks are generally illegal in France for employment purposes. Convicted people are barred from fewer professions than in the US, but are barred from nearly all public sector jobs, which represent roughly 20% of France's labor force.[6] Criminal records can also be checked for jobs where they might pose particular risks (e.g., law enforcement, working with children or the elderly, etc.). There are also no general rules barring

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[5] Statistics on French and European incarceration can be found at
http://www.justice.gouv.fr/art_pix/Chiffres_cles_2013_opt.pdf
[6] The list of jobs for which one must have a clean criminal background can be found here:
http://www.cidj.com/sites/default/files/liste_des_metiers_pouvant_donner_lieu_a_la_consultation_directe_du_b2.pdf

people from living in certain places (such as public housing), and most former inmates can return to live where they were prior to incarceration.

France has a centralized prison system. The French Department of Prisons Administration (DAP) runs all 190 facilities, both jails (for pretrial and short-term sentences) and prisons (for inmates with longer sentences). We obtained an administrative dataset on all inmates in French prisons in 2008 – 2010, and merged that with a dataset of online job announcements, both of which we now describe.

2.2. Individual Incarceration Records

Information on incarceration comes from administrative data. A penal file is created upon each inmate's incarceration in France, and updated throughout the incarceration period. The file contains penal and socio-demographic data, and is filled over time with information on transfers within and across prisons, disciplinary incidents, and sentence reductions. All of this data populates the National Inmate File and the Numeric File of Management of Inmates under Custody File,[7] which are centralized under the direction of the Department of Prisons Administration. These files are mainly meant for internal accountability and security purposes, and they are used to count inmates in France. The French Department of Prisons Administration generously provided administrative data on all inmates incarcerated in France between 2009 and 2010.

The data contains information on gender, date of birth, nationality, place of birth, place of residency, marital status, number of children, educational attainment, job status (all of which is reported by the inmates themselves, and reflects their situation upon incarceration), offenses leading to incarceration, length of sentence for each offense, date of trial, type of prison, date of release, and sentence reductions. Each individual can be tracked over time with a unique encrypted identifier.[8] Our principal outcome of interest is recidivism. Recidivism is defined by the fact that a person reappears in the prison dataset after being released from prison.

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[7] Fichier National des Détenus, FND, and Gestion Informatisée des Détenus en Etablissement, GIDE
[8] These are unique identifiers, based on first name, last name, and date of birth. For confidentiality purposes, the encrypting was done at the Ministry of Justice.

Table 1 presents descriptive statistics on people released from prison in 2009 and 2010, which is the time period for which we have both prison and labor market data. During this time, 127,810 people were released from prison, and the average stay in prison was 10 months. The most frequent offenses were theft and battery. Relevant for our study, 39% of people did not have a job when entering prison.

2.3. Labor Market Data

Our main labor market data comes from a compilation of job openings and cuts posted online. The data was collected for commercial purposes by a private firm, the *Observatoire de l'Investissement*. The dataset was compiled from about 4,000 Internet sources, in particular local newspapers (43%), national newspapers, and job announcement websites. While we used a pre-complied version of this online data, this information could also be collected directly by scraping job announcement websites and searching local and national newspapers.

This data contains daily listings of coverage of openings of new production plants, increases in the number of perspective employees in existing ones, as well as classified job offers (simply, "positive news" or "positive announcement" hereafter) and coverage of plant closures or downsizing (hereafter, "negative news/announcement").[9] We use this dataset to create our main measures of local labor market conditions. For each county (*départment*) in France and each day, we build measures of job creations and cuts that appeared on any source listed in the dataset in the 30 days following that date. Our main labor market indicators, as reflected by the media, are the number of positive and negative announcements; and the number of jobs created and cut. For each inmate, we can match date of release to content of news on that date. We thus obtain a measure of job openings and cuts that occur in the county each former inmate lived in, for the first 30 days after their release from prison. We exclude news on public sector jobs, since as mentioned earlier former offenders are not allowed to hold civil servant positions.[10] As detailed further, we also

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[9] Also for simplicity's sake, we refer jointly to positive and negative stories and announcements as "news stories about jobs."

[10] Public sector only represents 2.2% of the 22,545 announcements.

vary the 30-day window in robustness checks.

Table 2 reports descriptive statistics on the distribution of information about job creations and cuts. On average, people released from prison faced 3.3 positive announcements and 2.1 negative announcements in the first 30 days after release. It is worth noting that our main measure of employment captures large operations, such as plant openings or closures, and big hiring plans or firms downsizing. The average estimated number of job openings in the first 30 days, conditional on having at least one positive announcement, is 149; and the average number of jobs cut if there is at least one negative announcement is 152.

How much do our measures of job opening / closing capture French labor market conditions as reflected in official statistics? Figure 1 shows the correlation between our measure of job openings and the official statistics reporting new vacancies appearing in a calendar month, collected by the French employment agency (DARES-Pole Emploi). The horizontal axis captures the number of official vacancies communicated to the French employment agency in a calendar month in a given county (*département*) in 2010 by sector of activity, and the vertical axis shows our measure of jobs openings aggregated by county and calendar month.[11] This figure shows that our measure of vacancies is positively correlated with vacancies recorded in official statistics; even though it does not catch all jobs opened or cut, it is a reliable proxy for relevant job market conditions. Interestingly, the correlation between our measure and official vacancies in the construction and industrial sectors are strongest, and these sectors have higher concentrations of low-skills jobs.[12]

4. Empirical Strategy

Our identification strategy exploits the daily variation in news on job creation and cuts that we can capture with our high-frequency online job database. We look at the effect of labor market information in the month after release on recidivism, defining

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[11] Official statistics are available only for 2010.

[12] The regression of the number of announcements about the official vacancies presents a very high R-square for both the construction sector (0.62) and the industrial sector (0.58), a high one for the service sector (0.53), and a small one for agricultural sector (0.07).

"month of release" as the 30 days following the date of release. We exploit daily variation in the number of news stories. It makes sense to focus on the period immediately following incarceration for two reasons. First, the first few weeks have been shown to be crucial in terms of successful transition out of prison.[13] Second, access to news is limited in French penal facilities. There is no Internet connection and very limited access to newspapers.[14] The main source of information is national TV channels, which likely do not have a lot of information about local labor market conditions.

Our empirical strategy relies on the hypothesis that daily variations in local labor market conditions are orthogonal to individual and context characteristics upon release from prison. The exact day of release is somewhat random, since it depends on the precise timing of the trial, date of incarceration, and sentence reductions, all of which might vary for reasons plausibly orthogonal to local job market conditions and individual characteristics. After controlling for month and county fixed effects, we look at recidivism as a function of the amount of positive and negative news in the precise 30 days following release. Keeping all else constant, people will be released in times of varying amounts of good or bad news about jobs, for idiosyncratic reasons. We exploit this randomness in *exact* timing of release from prison and in news on jobs. This identification strategy avoids problems of aggregations that plague studies of crime that use unemployment rates.

4.1. Balancing Tests and Graphical Evidence

Our main identifying assumption is that timing and content of news are orthogonal to individual characteristics, both observable and unobservable, of people released from prison. To explore this hypothesis, we run balancing tests on observables. For each individual, we split the sample at the median of job openings (cuts) in the county of residence that appeared in the thirty days after their release. We look for differences in observable characteristics above and below the median. In table 3, we report the results of t-tests for differences in observables, over and below the median of job

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[13] In France, 34% of ex-offenders have been re-convicted within three years after trial. Among them, 8% are re-convicted during the first month.

[14] Newspapers are mainly available at the prison's library. Internet is forbidden except in some pilot jails and smartphones were not common in France in 2009/2010.

information. This provides a test of observables being balanced for individuals with respect to the number of news stories on job openings or cuts. Observables are remarkably similar, and differences that are statistically significantly different from zero have very small point estimates.

Before presenting the formal regression model and our main results, we present in figure 2 some suggestive graphical evidence that captures the idea of the main treatment effects. We plot recidivism rates up to six months after release from prison for two groups of people: those who were released with more good news (lighter line) or more bad news (darker line). "Good" and "bad" news are defined as a greater number of stories on job creation (or cuts). Figure 2 shows that recidivism rates are lower for people released when there is more good news about jobs, and the gap grows over time. This preliminary evidence suggests that our measures capture relevant information with respect to offending decisions. People released in moments when there is more good news about jobs tend to reoffend less frequently. We now turn to regression analysis.

4.2. Main Regression Analysis

To estimate the effect of local labor market conditions on recidivism of French former inmates, we first estimate the following linear regression model:

$$Y_{ist} = A_s + B_t + cX_{ist} + \beta_1 Job_{st}^+ + \beta_2 Job_{st}^- + \varepsilon_{ist}$$

Where Y_{ist} is an indicator of recidivism within six months after release (for an individual i in a county s at time t), Job_{stt}^+ (Job_{st}^-) is a forward-looking variable: it captures volume of news on job creations (or cuts) in the month after release. X_{ist} controls for individual characteristics; and A_s and B_t are county and month fixed effects. As we stressed above, since we use *daily* variation in job creations and cuts, our identification hypothesis is that daily variation *within county and month* for these measures are not correlated with individual-level heterogeneity and other county-level confounding factors that may be correlated with labor market conditions. Since the exact date of release is as good as random given trial-specific timing, our

identification hypothesis concerning individual-level confounding factors is plausible and it is supported by the balancing tests presented in the previous section. Moreover, in other specifications we also introduce a set of week time effects and county-specific time effects as well as county-specific linear time trends to take into account potential heterogeneity at the county level that is not absorbed by month fixed effects.

Table 4 reports our main results. Column 1 presents the results omitting our main variable of interest and gives an idea of the correlation between observables and recidivism six months after release from prion. Gender, education, marriage and age appear to be protective factors. Likewise, people released on parole recidivate less, as do people who had jobs before going to prison. In terms of offenses, people incarcerated for theft are most likely to recidivate. These results are in line with correlations found in the literature. Columns 2 and 3 include our main variable of interest: number of articles on job openings and closings in one's county of residence, in the 30 days after release from prison. Announcements about job openings in the first thirty days after release have a negative and significant impact on the probability to reoffend within six months after release. News on job cuts have the opposite effect, increasing the probability of being re-incarcerated. Adding the full set of individual-level observables does not change the magnitude of coefficients, confirming indirectly that our variables of interest are orthogonal with respect to individual observables. We find that a one standard deviation increase in the number of new stories on job openings is associated with a 10% reduction in the probability of re-arrest within six months. A one standard deviation increase in number of stories on job cuts implies a 5% increase in the probability of re-arrest within six months. In other words, the effect is quite large: being exposed to 10 additional positive job postings is correlated with a one percentage point decrease in the likelihood of recidivism, which is similar to the effect of having had a job prior to incarceration.

Results hold across crime categories and by nationality and age of former inmates. Columns 4 and 5 of table 4 report results by employment status before the most recent incarceration. Results remain similar regardless of what the prior employment status was.

4.3. Robustness checks

We now provide some robustness checks. In particular, we investigate whether our results are sensitive to variations in the definition of our main variables of interest, to the way we control for time and space, and to various sample restrictions.

As a first step, we vary our definition of job announcements. We vary time windows for job creations, and we check if our results are affected by the inclusion of job creation in the public sector. Results are presented in the first three columns of table 5. In column 1, we measure the effect of job announcements including the public sector. In column 2, we measure the effect of job announcements within 15 days after release, while in column 3, we account for the news within two months of release. Results are similar to those obtained in the previous section: signs and orders of magnitude are close and coefficients are statistically significant.

A further concern about the robustness of our results is that, although we use daily variations in the number of announcements of job openings and cuts, some factors varying within counties that could be correlated with the volume of the announcements may be omitted. To check the robustness of our results with respect to this possible concern, we focus on different model specifications, including various time controls. In column 4 of table 5, we add a week fixed effect to the main regression. Those fixed effects account for events at the national level that could be correlated with the variation in the volume of announcements at the moment of release. Column 5 adds county by month fixed effects. In these specifications we are left with within month and county variation in the volume of announcements, thus we exploit within county and month daily variation in our independent variables. Column 6 allows for different time trends in each county, while column 7 adds county time trends plus county fixed effects. Those specifications account for time trends within counties, which could be related to an increase in the volume of news over time. Results are robust across these alternative specifications; the sign and magnitudes of effects are similar, and all are significant.

Our results can be interpreted as the causal effect of variations in local labor market conditions at the moment of release from incarceration under the hypothesis that the

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day of release is orthogonal with respect to variations in job market conditions, which we have demonstrated to be plausible. There is, however, a concern that this might be driven by an increase in early releases under more favorable economic conditions. Judges may prefer to grant early release to people who are more likely to find a job, which they might find to be more often the case when more jobs are available. Thus local labor market conditions may correlate with some characteristics of those released from incarceration because of the selection made by judges to grant parole. While the balancing tests on the individuals' observables presented above tend to rule this hypothesis, we provide evidence that our results are not driven by people on parole. The first column of table 6 shows that probability of receiving parole is not correlated with job announcements. Column 2 includes controls for parole, and columns 3 and 4 report results for parolees and simple releases. Results are of similar magnitude for people released on parole or not: differences in re-offending are not driven by differences in releasing policies.

4.4. Heterogeneity

In our main analyses, recidivism is defined simply as being sent back to prison. Yet the offenses for which one can be incarcerated – mainly property crimes, drugs, violence and driving under influence (DUI) – theoretically diverge in their relation to legal employment opportunities. Property crimes are directly revenue generating and could be viewed as a substitute for employment, or in other words have an economic motivation. On the other hand, if drug use is driven by addiction problems, legal job opportunities may have smaller effects on this criminal activity related to dependency.

We document this distinction by measuring the effect of job announcements on different type of post-release crime, namely property crimes, drugs, violence and DUIs. The outcome variables in these regressions are equal to one if a former inmate reoffends for that specific type of crime. These categories are not mutually exclusive: a person could for example be convicted of theft after drug consumption, which would count as both property crime and drugs. Results are presented in table 7. The effect of job announcements on property crime is presented in column 1, on drugs in column 2, on DUIs in column 3 and on violence in column 4. Positive job

announcements are always significant. However, the effect of news of job creation is significantly bigger for property crimes and significantly lower for drug-related offenses. Results for negative job announcements are less clear, although drug offenses also appeared to be less affected. Taken together, these results are consistent with our previous hypothesis that information about job market opportunities has a bigger effect on crimes that have an economic motivation (such as property crime) than on crime related to addictions.

5. Discussion: *Jobs, matching and optimism*

In this section we investigate whether, beyond actual labor market conditions, *information* about job availability affects the probability of recommitting a crime. The intuition, developed in detail in section 3, is the following. Conditional on the available jobs, news could reduce search costs, or change beliefs on job opportunities. In other words, our main results may reflect both the direct effect of an increase in available jobs and an effect of an increase in information about available jobs conditional on their existence. Distinguishing between these two channels is relevant since they may have different policy implications. While the first channel would be difficult to manipulate cost-effectively, the second channel suggests that simple policies providing information about relevant job market opportunities to people released from prison might be effective crime reduction tools. In this section, we dig into these channels by providing different pieces of evidence.

5.1. Timing of the news

We explore whether media coverage of available jobs has an effect on individual propensity to re-offend, beyond the direct effect of job availability. We extend our previous analysis to parse out effects of the timing of announcement versus job start date. More specifically we focus on three kinds of different job announcements:

1. News published *before* release about jobs that will be available *after* release. These types of announcements capture jobs opened after one's release that can affect people's behavior through better job market conditions, but not through information since people could not access this online information while in

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prison.[15]

2. News published *after* release about jobs opened *before* release. This type of news should not affect behavior through better job market conditions nor through information about available jobs that could ease the matching process. However, this type of news could affect people's expectations and optimism.

3. News published *after* release about jobs opened *after* release. This type of information could affect recidivism through all the mechanisms described in the conceptual framework (job opportunities, matching, optimism).

Table 8 presents results with this distinction. In column 1, we report the effect of news published after release on jobs created after release and news published before release on jobs created after release. Both coefficients are negative but the former is the only one significant with point estimates more than four times bigger than the latter. If our main effect were driven by the direct effect of increased job availability, both variables should have had similar coefficients. Indeed both capture the creation of new jobs after release from prison. However the former variable captures the effect of providing information about existing jobs, beyond simply the availability of jobs. The difference in coefficients suggests that job announcements do not only affect recidivism through job market conditions; job creations are far more effective in reducing recidivism when there is *information* about newly created jobs.

One potential concern may arise if announcements about jobs available after one's release which are published before the release happened to be a bad proxy of actual jobs available after release.[16] In order to address this concern more precisely, we regress official vacancies during month *t* on job announcements published in month *t-1* relating to job created in month *t*. The coefficient is similar in magnitude to the one

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[15] Note that while cell phones may be smuggled into jails, our data is from 2010 when smartphones were not widely used in France. In 2008, only 12% of people used their smartphones to go on the Internet, compared to 40% of people in 2012. In the past few months, there have been several stories about Facebook usage in French prisons, mainly discussing the novelty of the presence of smartphones. So while some people might have had Internet access while in prison in 2010, this was plausibly a rare occurrence. (Source: http://www.insee.fr/fr/themes/document.asp?reg_id=0&ref_id=ip1452)

[16] This is unlikely as more than 62% of the announcement are published before the effective job creation or cuts. Then, those announcements are counted as news about job available after one's release but published before release for certain offenders and after release for others.

obtained by regressing announcements in month *t* concerning job open in month *t*.[17]

Column 2 of table 8 presents the effect of news published after release for jobs created before release, and news published after release for jobs created after release. Only the latter is sizable and significant. Positive announcements for jobs created before release have no effect on recidivism, even though former inmates had access to this news. This result is more coherent with a matching story than an optimism story, since job announcements – even about past openings – are still good news. If job announcements induced an update on assumptions about the probability of finding a job, we would expect positive announcements published after release to affect behaviors even if the vacancies had already been filled. Results presented in columns 1 and 2 are confirmed in column 3, where the three measures are used together.

All in all, these results indicate that information about jobs plays a role beyond actual job creations, and this effect is plausibly driven more by increased knowledge about job opportunities than by increased optimism about job market conditions.

5.2 Effects conditional on official labor market statistics

Our second piece of evidence relies on documenting the effect of announcements about jobs creations controlling for underlying local job market conditions. The idea is the following: if the effects we document in section 4 simply come from better job market opportunities, controlling for official statistics on employment should drive the effect of job announcements to zero. If the effect is, at least partially, driven by matching or updating people's assumptions, the effect of job announcements should remain significant, even after controlling for number of jobs actually available.

Two different measures are publicly available at the month and county level: unemployment rates and the number of new vacancies collected by the national employment agency (ANPE). The latter captures a flow, and is closer to our job announcement variable. Results are presented in table 9.

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[17] When we regress announcements published in month t-1 about jobs created in month t on official vacancies at t, we obtain a coefficient of 0.74 (0.038 with month and county fixed effects). The same regression, but using announcements published at t about job created in month t, gives a coefficient of 0.54 (0.056 with month and county fixed effects).

Column 1 presents the effect of job announcements when we control for the number of vacancies recorded in official statistics by calendar month and region. Column 2 presents the effect of job announcements when we control for job market tightness measures, aggregated at the calendar month and regional level. The effect of job announcements remains significant and sizable, and the magnitude is comparable to our main effect.

One reason why the effect might still hold is because of differences in the timing of these measures. The official statistics are computed at the monthly level. For example, in our main specifications, people released on the 1st of May are assumed to face the same unemployment rates than those released the 31th of May, but a different one than people released the 30th of April. Our job announcement variable has a finer granularity, and it might be capturing these finer within-month variations of the job market conditions. We overcome the timing problem by focusing on people released the first week (column 4) of the month. For these people, official statistics and job announcement variables have roughly the same temporality. The timing of news about job openings overlaps more closely with the timing of official statistics. The effect remains significant and point estimates are similar to those presented in section 3. Taken together, these results suggest that announcements do not only affect recidivism through job market conditions: better coverage of job openings and closing in itself affects recidivism.

5.3. Internet and the access to information

We have shown that information diffusion seems to increase the effect of job market conditions. We further test this result by looking at variations in access to news, exploiting differences in access to high-speed Internet.[18] The exact introduction of high-speed Internet in France is not well documented, but county density does not appear to be correlated with access to high-speed Internet (map linked in footnote).

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[18] Data on ADSL coverage are taken from Tactis (2013): "Internet-less zones (Zones blanches) have and ADSL coverage less or equal than 2 mbps." http://www.tactis.fr/wp-content/uploads/2013/11/ZonesBlanches2M_par_dpt_nb_communes.jpg

Results are presented in table 10. Column 1 presents the main regression in the area where Internet access is the worst. Columns 2 and 3 present the same regression in places with moderate and high access to high-speed Internet. Results indicate that coefficients are smaller for the areas where high-speed Internet coverage is lower.

Taken together, these results suggest that conditional on the underlying job market conditions, the diffusion of news about job openings and cuts, which reduces search costs, may play a role in affecting recidivism. This hypothesis is consistent with recent evidence showing that, all else equal, unemployed persons who look for jobs online are re-employed faster than people who do not search for jobs online (Kuhn and Mansur, 2014): higher internet availability and more online job announcements for given job market conditions should increase job finding rates in the short run and in turn reduce short-term recidivism.

5.4. Policy implications

There are many potential public policy levers that could be used to reduce recidivism. How do job announcements contrast to other potential policies? To get an easily interpretable estimate, we look at the effect of getting *any* positive announcement: it corresponds to a 7% decrease in recidivism.[19] We can contrast this to other estimates in the literature: one extra month in prison is associated with a 4% reduction in recidivism (Kuziemko, 2013); two additional weeks in prison plus one extra month on probation is associated with 5% reduction in recidivism (Philippe, 2015); one extra month in expected future sentences is associated with a 1.3% reduction in recidivism (Drago et. al., 2009). Depending on estimates, it appears that providing information to people on jobs is about equivalent to spending two extra months in prison, or expecting five more months in prison if re-convicted.

The effects are smaller than those of alternatives to incarceration such as electronic

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[19] To calculate this easily interpretable estimate, we create a dummy = 1 if a person received any positive announcements in the 30 days flowing their release from prison, and regress recidivism on this dummy, including the same controls as in our main specifications. Receiving positive announcements is significantly correlated with recidivism outcomes.

monitoring, which are associated with a 25% (Ouss, 2013) to 50% (Di Tella and Schargrodsky, 2013) reduction in recidivism. Thus, avoiding incarceration altogether might be the most cost-effective way to reduce recidivism in some cases, but providing inmates information about available job opportunities at the time of their release from incarceration still appears to be very cost-effective policy.

6. Conclusion

This paper uses fine-grained data on job announcements matched with administrative data on releases from prison to identify the effect of local labor market conditions on recidivism. This data allows us to overcome the main identification challenges that have held back analyses on this important policy question by exploiting high-frequency variations in local labor market conditions. We find that recidivism is less likely when there is more coverage of job openings, and more likely when there is more coverage job cuts: recidivism responds to local labor market conditions. Digging into mechanisms, we find that *news* on job openings seems to be driving these effects, over and beyond the direct effect of job availability. Using estimates from previous studies, we find that this effect is comparable in size to those of increasing incarceration spells by about two months. This suggests that providing inmates with information about job market opportunities at their release from incarceration can be a cost-effective policy to reduce recidivism.

Our study also innovates by studying the effects of local labor market conditions on recidivism by examining the entire universe of former inmates in a given year in a single country. The picture emerging from our analysis suggests that, consistent with the standard economic approach to the study of crime, former inmates respond to the incentives provided by variation in formal labor market opportunities. This confirms the role of incentives in the formal labor market, even though other research has shown the importance of informal employment for people when they are released from prison. Our contribution has both theoretical and policy implications. Our empirical findings broadly fit predictions of standard economic models linking crime to labor market opportunities. From a policy perspective, the analysis suggests that policies targeted to reduce unemployment may have positive spillovers by reducing

recidivism and highlights the role of information about job availability, over and beyond the effect of unemployment reduction – which is a harder policy lever to manipulate.

Increasing job opportunities is costly, and focusing efforts on people released from prison or otherwise involved with criminal justice might be perceived as unfair, or potentially create some moral hazard problems. Our finding that *information* about jobs matters over and beyond actual job availability is much more tractable from a policy and implementation perspective: diffusing relevant job information is much less costly than increasing employment. The importance of information has been shown to play an important role in other contexts, such as investments in schooling (Jensen, 2010 and Hoxby and Turner, 2015), risky sexual behaviors (Dupas, 2011), or retirement investments (Duflo and Saez, 2003). It is not a new finding that information would play an important role in labor markets (Stigler, 1962); some research places particular emphasis on its diffusion via social networks (Ioannides and Datcher Loury, 2004). Our findings show that improving matching through information could also have impacts on important outcomes like offending, which might matter in particular when assessing the costs and benefits of social policies.

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(a) All jobs

(b) Construction

(c) Industry

(d) Agriculture
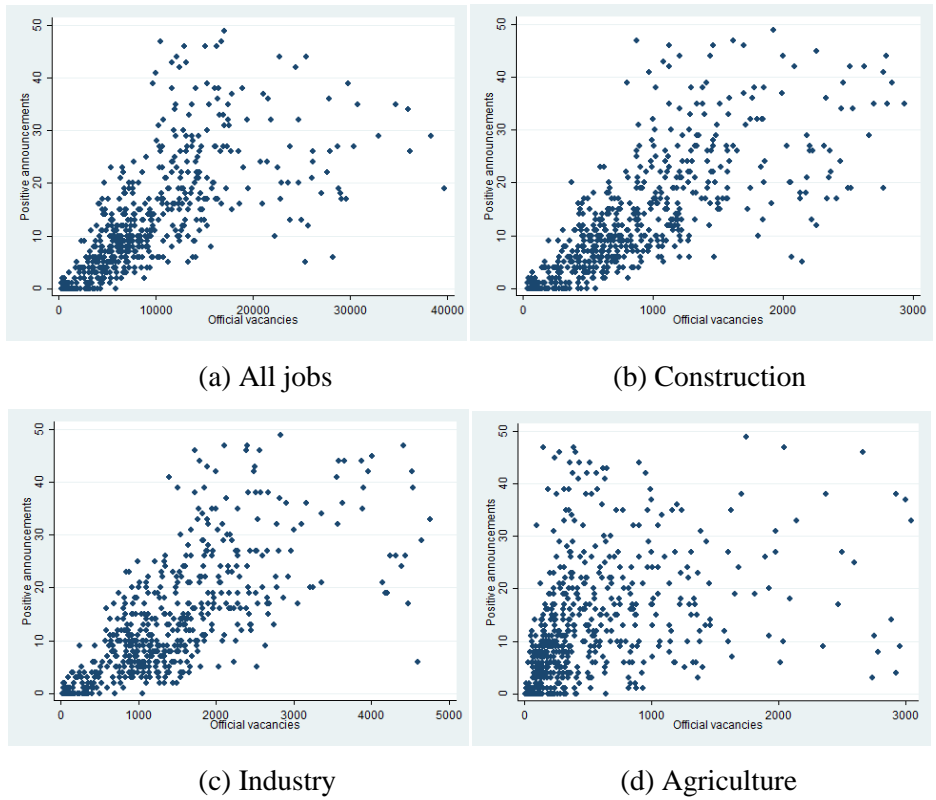
Figure 1 Monthly correlation between positive announcements and official vacancies.
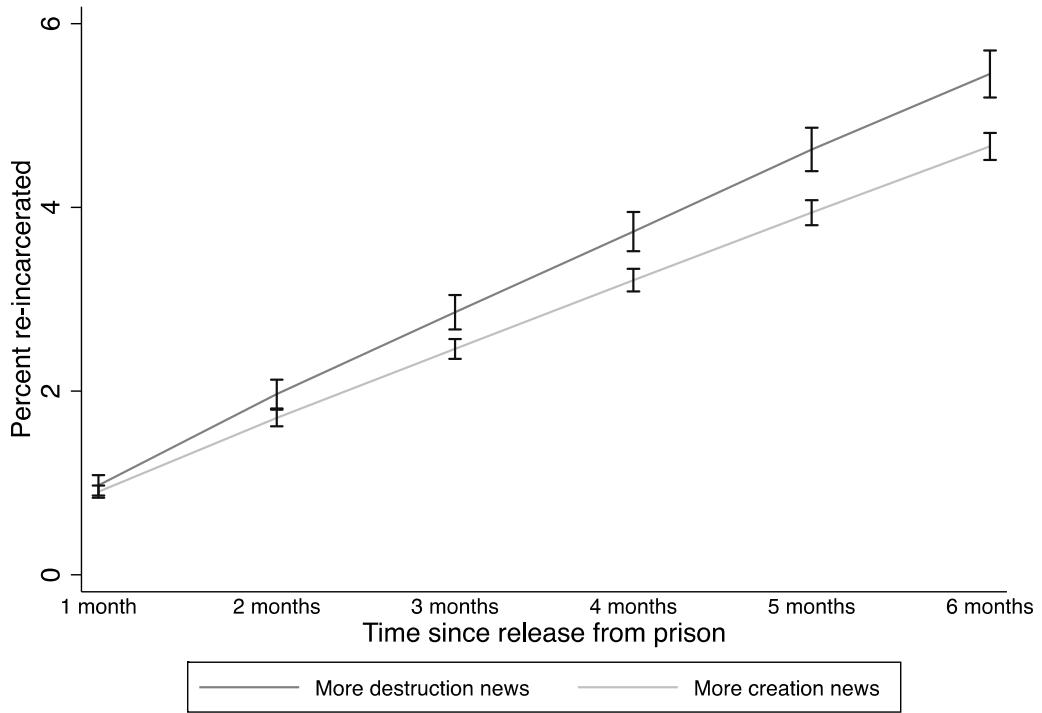
Figure 2: Percent returned to prison, by month since release from prison and content of articles: more news about job creations (lighter line) vs. more stories about job destructions (darker line).

Table 1: Descriptive statistics on releases from French prison: 2009 and 2010

	Count	Percent / Mean
Socio-demographics		
Women	5551	4%
Born in France	101814	80%
French	109803	86%
Married	39407	31%
Has children	53083	42%
Has a job when incarcerated	78061	61%
High school	12922	10%
Middle school	48465	38%
Technical education	40413	32%
No school	11523	9%
Age upon release		32,3
Offending		
Theft	44412	36%
Drugs	26917	22%
DUI	34612	28%
Assault	43312	35%
Parole	9312	7%
Short-term prison	86064	67%
Recidivated within 6 months	6307	5%
Incarceration length		10,6
Total	127,810	

Table 2: Descriptive statistics on job creations and destructions as presented in the *Observatoire de l'Investissement* data.

	Mean	Sd	Min	Max
Positive announcements 30 days	3,32	3,98	0	48
Negative announcements, 30 days	2,07	2,38	0	22
Jobs created if at least one positive announcement, 30 days	149	337	2	8565
Jobs destroyed if at least one negative announcement, 30 days	152	330	1	6736

Table 3: Balancing tests.

	Positive announcements			Negative announcements		
	(1)	(2)	(3)	(4)	(5)	(6)
	Below median	Above median	Difference	Below median	Above median	Difference
Women	0.043	0.043	0.000047	0.043	0.044	-0.0013
Born in France	0.80	0.80	-0.0037	0.79	0.80	-0.0088***
French	0.86	0.86	-0.0028	0.85	0.86	-0.0091***
Married	0.30	0.31	-0.0084**	0.31	0.31	0.0028
Has children	0.41	0.42	-0.0053	0.42	0.42	0.0018
Job when incarcerated	0.61	0.60	0.0050	0.60	0.61	-0.0078*
High school	0.10	0.10	0.000025	0.10	0.10	0.00039
Middle school	0.38	0.38	0.0031	0.37	0.38	-0.0031
Technical education	0.32	0.32	0.0015	0.32	0.32	-0.0041
No school	0.090	0.092	-0.0016	0.094	0.090	0.0033
Age upon release	32.3	32.3	-0.041	32.2	32.4	-0.20**
Theft	0.36	0.36	-0.0058	0.36	0.36	0.0049
Drugs	0.21	0.22	-0.0034	0.21	0.22	-0.0037
DUI	0.28	0.28	0.00047	0.27	0.28	-0.0043
Assault	0.35	0.35	-0.0086**	0.35	0.35	-0.0024
Parole	0.072	0.073	-0.0013	0.071	0.072	-0.0010
Short-term prison	0.67	0.66	0.014***	0.68	0.67	0.017***
Incarceration length (days)	215.9	223.8	-7.85***	218.0	221.8	-3.73

* $p<0.05$, ** $p<0.01$, *** $p<0.001$

Note: Columns 1 and 2 (4 and 5) report summary statistics for the sample divided in evenly sized groups as follows. Within each county, we calculate the median number of positive (negative) announcements. Column 1 (4) reports summary statistics for those inmates who are released when the number of positive (negative) announcements is below the median for their county, and col. 2 (5) reports summary statistics for inmates who are released when the number of positive (negative) announcements is above the median for their county. Column 3 (6) reports the point estimates of the differences between the means in cols. 2 and 3 (4 and 5).

Table 4: Job announcements and recidivism within 6 months: regression analysis.

VARIABLES	(1) social and penal indicators	(2) Number of announcements	(3) Number of announcements	(4) formerly unemployed	(5) formerly employed
Positive announcements		-0.00133***	-0.00134***	-0.00153***	-0.00115***
		(0.000165)	(0.000203)	(0.000279)	(0.000223)
Negative announcements		0.000809***	0.000912***	0.000729*	0.00101***
		(0.000304)	(0.000297)	(0.000413)	(0.000325)
Short-term prison	-0.000557		-0.000843	-0.00210	-0.000445
	(0.00228)		(0.00225)	(0.00393)	(0.00212)
Parole	-0.0162***		-0.0162***	-0.0136***	-0.0174***
	(0.00228)		(0.00227)	(0.00438)	(0.00271)
Woman	-0.0199***		-0.0199***	-0.0213***	-0.0163***
	(0.00289)		(0.00290)	(0.00361)	(0.00345)
Born in France	-0.00100		-0.000989	1.52e-05	-0.00169
	(0.00266)		(0.00267)	(0.00488)	(0.00305)
French	0.00613**		0.00624**	0.00977*	0.00343
	(0.00289)		(0.00289)	(0.00549)	(0.00305)
Married	-0.00563***		-0.00562***	-0.00500*	-0.00546***
	(0.00161)		(0.00162)	(0.00282)	(0.00188)
Has children	0.00251		0.00260	-0.00189	0.00485**
	(0.00176)		(0.00176)	(0.00266)	(0.00209)
Had a job when	-0.0149***		-0.0150***		
	(0.00153)		(0.00151)		
High school	-0.00806***		-0.00794***	-0.0129***	-0.00547**
	(0.00198)		(0.00197)	(0.00386)	(0.00222)
Technical education	-0.00544***		-0.00541***	-0.00820***	-0.00292*
	(0.00134)		(0.00133)	(0.00303)	(0.00171)
No school	-0.000137		4.14e-05	0.00323	-0.00233
	(0.00252)		(0.00245)	(0.00303)	(0.00360)
Other	-0.00382		-0.00365	-0.00722**	0.00152
	(0.00264)		(0.00259)	(0.00362)	(0.00260)
Age at release	-		-0.000890***	-0.00121***	-
	(5.78e-05)		(5.82e-05)	(0.000103)	(7.47e-05)
Theft	0.0213***		0.0214***	0.0234***	0.0203***
	(0.00212)		(0.00213)	(0.00305)	(0.00232)
Drugs	-0.00207		-0.00196	-0.00212	-0.000975
	(0.00141)		(0.00141)	(0.00299)	(0.00156)
DUI	0.00543***		0.00547***	0.00820**	0.00410***
	(0.00149)		(0.00148)	(0.00325)	(0.00156)
Assault	0.00746***		0.00756***	0.0118***	0.00488***
	(0.00151)		(0.00150)	(0.00245)	(0.00171)
Sentence	4.68e-06**		4.66e-06**	-1.56e-06	7.67e-06***
	(1.99e-06)		(1.99e-06)	(3.24e-06)	(2.22e-06)
Constant	0.0853***	0.0631***	0.0903***	0.104***	0.0633***
	(0.00425)	(0.00303)	(0.00447)	(0.00901)	(0.00561)
Observations	123,421	127,810	123,421	48,174	75,247
R-squared	0.014	0.005	0.014	0.017	0.010

Note: Regressions include month and county dummies. Standard errors are clustered at county level $*$ $p<0.05$, $**$ $p<0.01$, $***$ $p<0.001$

Table 5: Robustness checks. We vary measures of jobs announcements, as well as spatio-temporal controls.

	(1) all announcement	(2) announcements within 15 days	(3) announcements within 60 days	(4) Week and county FE	(5) Month*county FE	(6) Day*county trend	(7) Day*county trend with county FE
Positive announcements, 30 days				-0.00137***	-0.00178***	-0.00128***	-0.00146***
				(0.000200)	(0.000229)	(0.000168)	(0.000192)
Negative announcements, 30 days				0.000908***	0.00142***	0.00128***	0.00101***
				(0.000304)	(0.000424)	(0.000279)	(0.000304)
Positive announcements with public sector, 30 days	-0.00130***						
	(0.000212)						
Negative announcements with public sector, 30 days	0.000912***						
	(0.000310)						
Positive announcements, 15 days		-0.00162***					
		(0.000299)					
Negative announcements, 15 days		0.00137***					
		(0.000398)					
Positive announcements, 60 days			-0.000970***				
			(0.000106)				
Negative announcements, 60 days			0.000930***				
			(0.000243)				
Constant	0.0901***	0.0878***	0.0910***	0.0770***	0.0814***	0.0956***	0.0972***
	(0.00451)	(0.00436)	(0.00475)	(0.00319)	(0.00392)	(0.00387)	(0.00336)
Observations	123,421	123,421	123,421	123,421	123,421	123,421	123,421
R-squared	0.014	0.014	0.014	0.015	0.023	0.013	0.015

* p<0.05, ** p<0.01, *** p<0.001

Note: Regressions include month and county dummies, as well as controls for gender, marital status, education, offense, age at release, sentence length, type of prison, and dummies for being French, being born in France, having children, having a job when incarcerated. Standard errors are clustered at county level

Table 6: Robustness checks: recidivism by parole status

	(1) Number of suspended sentences	(2) Recidivism: no parole	(3) Recidivism: parole
Positive announcements, 30 days	0.000272	-0.00133***	-0.00135***
	(0.000223)	(0.000211)	(0.000501)
Negative announcements, 30 days	0.000372	0.000898***	0.00124
	(0.000352)	(0.000317)	(0.000820)
Constant	0.0687***	0.0916***	0.0531***
	(0.00373)	(0.00474)	(0.0167)
Observations	127,810	114,365	9,056
R-squared	0.015	0.014	0.029

Note: Regressions include month and county dummies, as well as controls for gender, marital status, education, offense, age at release, sentence length, type of prison, and dummies for being French, being born in France, having children, having a job when incarcerated. Standard errors are clustered at county level

Table 7: Heterogeneity, recidivism by type of offense.

	(1) recidivism for property offenses	(2) recidivism for drugs	(3) recidivism for DUI	(4) recidivism for violence
Positive announcements, 30 days	-0.000775***	-0.000337***	-0.000405***	-0.000431***
	(0.000155)	(6.03e-05)	(0.000124)	(9.90e-05)
Negative announcements, 30 days	0.000400	-7.66e-05	0.000108	0.000365**
	(0.000255)	(0.000101)	(0.000140)	(0.000178)
Constant	0.0496***	0.0194***	0.0177***	0.0362***
	(0.00284)	(0.00208)	(0.00220)	(0.00283)
Observations	123,421	123,421	123,421	123,421
R-squared	0.022	0.010	0.011	0.011

Note: Regressions include month and county dummies, as well as controls for gender, marital status, education, offense, age at release, sentence length, type of prison, and dummies for being French, being born in France, having children, having a job when incarcerated. Standard errors are clustered at county level

Table 8: Effect of job announcement depending on announcement and job timing

	(1)	(2)	(3)
News in the month after release about jobs opened before release		-2.55e-06	4.12e-05
		(0.000634)	(0.000629)
News in the month after release on jobs opened after release	-0.00151***	-0.00155***	-0.00151***
	(0.000272)	(0.000315)	(0.000309)
News in the month before release on jobs opened after release	-0.000525		-0.000526
	(0.000367)		(0.000368)
Constant	0.0869***	0.0864***	0.0868***
	(0.00501)	(0.00501)	(0.00501)
Observations	123,421	123,421	123,421
R-squared	0.014	0.014	0.014

Note: Regressions include month and county dummies, as well as controls for gender, marital status, education, offense, age at release, sentence length, type of prison, and dummies for being French, being born in France, having children, having a job when incarcerated. Standard errors are clustered at county level

Table 9: Job announcements and recidivism, control for official statistics

VARIABLES	(1) Number vacancies	(2) V/U ratio	(3) Long term V / U ratio	(4) Number of vacancies : released first week
Positive announcements no public sector, 30 days	-0.00128***	-0.00134***	-0.00137***	-0.00130***
	(0.000184)	(0.000203)	(0.000204)	(0.000359)
Negative announcements no public sector, 30 days	0.000891***	0.000905***	0.000905***	0.00173***
	(0.000300)	(0.000296)	(0.000292)	(0.000607)
number of job vacancies (official stats)	-0.00187**			-0.000164
	(0.000721)			(0.00143)
vacancies / number unemployed		-0.00871		
		(0.0419)		
long term vacancies / number unemployed			0.336*	
			(0.182)	
Constant	0.0987***	0.0914***	0.0820***	0.0753***
	(0.00582)	(0.00528)	(0.00645)	(0.00963)
Observations	123,421	123,421	123,421	27,725
R-squared	0.014	0.014	0.014	0.018

Note: Regressions include month and county dummies, as well as controls for gender, marital status, education, offense, age at release, sentence length, type of prison, and dummies for being French, being born in France, having children, having a job when incarcerated. Standard errors are clustered at county level

Table 10: Recidivism after 6 months, by quality of Internet coverage.

	(1) Bad: >15 Internet-less zones	(2) Medium:10 - 15 Internet-less zones	(3) Good: <10 Internet-less zones
Positive announcements, 30 days	-0.00102***	-0.00120***	-0.00171***
	(0.000143)	(0.000421)	(0.000276)
Negative announcements, 30 days	0.000222	0.00111	0.00137***
	(0.000509)	(0.000663)	(0.000452)
Constant	0.0983***	0.0843***	0.0896***
	(0.00897)	(0.00924)	(0.00655)
Observations	35,940	39,551	47,930
R-squared	0.014	0.015	0.014

Note: Regressions include month and county dummies, as well as controls for gender, marital status, education, offense, age at release, sentence length, type of prison, and dummies for being French, being born in France, having children, having a job when incarcerated. Standard errors are clustered at county level