

The Within-Family Labor Market Effects of Incarceration: Evidence from Ecuador[†]

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Abstract

There are over 11 million people in prison worldwide, 65% of whom are incarcerated in low- and middle-income countries. Yet, little is known about the labor market effects of prison systems in developing countries, and even less about potential within-family spillovers. This paper leverages variation in case-level outcomes and sentencing timing to examine the direct and indirect effects of incarceration in Ecuador. Using a unique database combining web-scraped criminal justice records, high-frequency labor market data, and constructed family networks for over 254k people, I estimate the impact of incarceration on defendants, their partners, and siblings. I find that, post-sentencing, earnings and the probability of employment fall by around 38% and 44%, respectively, with no recovery even after seven years. These long-lasting effects (i) stand in contrast with findings from recent studies and (ii) cannot be fully explained by incapacitation. Additionally, I find evidence of negative spillover effects onto partners and siblings. Together, these results highlight the role of incarceration in reshaping labor market outcomes at both the individual and household levels.

JEL Classification: K14, J48, D04

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1. Introduction

Incarceration is one of the most pervasive and consequential features of criminal justice systems around the globe. With over eleven million individuals currently imprisoned and millions more cycling through the system each year (Fair and Walmsley, 2024), the social cost of incarceration extends far beyond the incarcerated person. There is a growing body of research showing that incarceration can disrupt labor market trajectories, erode human capital, and exacerbate socioeconomic inequality (Western, 2002; Pager, 2003; Mueller-Smith, 2015; Landersø, 2015; Bhuller et al., 2020; Garin et al., 2025). However, evidence on potential within-family spillover effects on *labor outcomes* is non-existent. Understanding if, and how, families adjust to the loss of an income earner – often in contexts where economic resources are already constrained – is vital for better criminal justice policy design.

These adjustments can fundamentally reshape labor market activity for remaining working-age household members. On one hand, incarceration of the household head can force other working-age members to either enter the labor market or work more to substitute for the lost income. Such adjustments have been documented in other settings such as job loss (Lundberg, 1985; Halla et al., 2020) and spousal death (Fadlon and Nielsen, 2021). Conversely, incarceration could depress household income either because of psychological factors, social stigma, or caregiving responsibilities. Such effects have been documented in settings such as negative health shocks (Coile, 2004; Jeon and Pohl, 2017; Anand et al., 2022). Therefore, to better understand these dynamics, I estimate the effect of incarceration on the household head and further extend the analysis to individuals who are indirectly impacted: spouses and siblings. Studying these effects has proven challenging due to data limitations, which restrict the ability to link inmates with their immediate families.

This paper contributes to this debate by studying the within-family labor market effects of incarceration in Ecuador. To do so, I use three unique sources of administrative data matching individual-level criminal justice records with data on family networks and high-frequency earnings and employment records. Criminal justice records are scraped from a publicly accessible website and processed using natural language processing (NLP) methods to extract individual-level outcomes. Data on family networks, linking defendants listed on court cases with their spouses, partners, and siblings, comes from the office of vital statistics. Finally, employment and labor data come from social security records. Merging these data sources at the individual level allows me to track labor earnings for over 254k people representing the *universe* of defendants – and their immediate working-age family members – who transitioned through a criminal court between 2010 and 2017.

In Ecuador, the setting of this paper, the incarceration rate – that is, the number of people in prison per 100k of the population – has nearly quadrupled since 2000. The incarceration rate rose from 64 in 2000 to 229 in 2019 (Fair and Walmsley, 2024). This trend is not unique to Ecuador but rather

reflects a common pattern within criminal justice systems in Latin America, the region with the highest incarceration rate, with a median of around 250 per 100k (Clegg et al., 2024). Additionally, during the timeframe of this study, the average prison sentence in my sample increased from 18 to 28 months. These two facts: growing prison population who on average face longer prison terms – combined with the availability of *rich* administrative data at the individual level – provides a unique context to study the direct and potential spillover effects of incarceration.

In my panel, I observe labor outcomes for the *universe* of people who transitioned through the *entire judicial process* leading to a verdict or dismissal in a criminal court. Therefore, I can estimate the different earning trajectories of those who are incarcerated against those who received a non-custodial sentence, a non-guilty verdict, or had their case dismissed. Using an event study design, I find that after sentencing, monthly earnings and the probability of employment for those who were incarcerated fall by around 38% and 44%, respectively. Unlike recent research, which estimates a short-lived negative effect, I find that earnings do not recover – even seven years after sentencing. These effects are (i) not entirely driven by incapacitation and (ii) almost *twice* as large as those documented in the literature focusing on specific U.S. states or Nordic countries. This is relevant given that most of our understanding of the labor market effects of incarceration comes from studying high-income countries, and over 65% of the global incarcerated population is located within low- or middle-income countries.

Second, I extend the analysis to other working-age family members. First, I link defendants to their partners – through marriage or shared parenthood – and find that four years after sentencing, both monthly earnings and the probability of employment fall by 7%. Second, I link defendants with their siblings and find that four years after sentencing monthly earnings and the probability of employment fall by 10% and 5%, respectively. Rather than an immediate drop in earnings, I observe a progressive and compounding effect, as family members experience increasing economic strain and reduced attachment to the labor market over time. Taken together, these results imply that incarceration affects not only the defendant but also their immediate family members, effectively reshaping within-family labor dynamics in the long run.

This paper connects to two related strands of literature. The first focuses on the direct labor market effects of incarceration. This literature has found that former inmates face lower earnings, higher unemployment, and worse job stability than the general population (Western, 2002; BJS, 2021; Bergman and Fondevila, 2021; Russo et al., 2023). However, recent quasi-experimental work studying specific counties or states in the U.S. or Nordic countries has produced mixed evidence. Some studies found positive effects on earnings or employment, associated with work-release programs, longer incarceration spells, or impacts concentrated among previously unemployed inmates (Kling, 2006; Loeffler, 2013; Bhuller et al., 2020). Conversely, other studies found negative effects on lifetime income, earnings, or employment, associated

with transitions to lower-paying jobs or reoffending (Mueller-Smith, 2015; Bhuller et al., 2018; Harding et al., 2018; Garin et al., 2025). The second strand focuses on the intergenerational effects of incarceration. This literature finds that parental incarceration leads to positive effects on children’s education (Arteaga, 2023; Billings, 2018) and decreases in their probability of incarceration (Norris et al., 2021). This paper contributes to this space by studying labor market effects within a context representative of developing countries – with *large* differences in social safety net programs and informal labor markets—and extending the analysis to other working-age household members.

Additionally, it connects with the literature on the *added worker* effect. This literature, starting with the seminal work of Heckman and Macurdy (1980), studies the effect of unemployment spells of the head of household – with extensions to other negative income shocks at the household level – on the labor supply of married women. Within this space, studies have found modest increases in the labor supply of women in response to their partners’ job loss, partially offsetting household income declines (Lundberg, 1985; Cullen and Gruber, 2000; Hardoy and Schøne, 2014; Halla et al., 2020; Cammeraat et al., 2023). However, the magnitude and persistence of this response vary across settings, depending on labor market opportunities, social norms, and the availability of social insurance. Although there are clear differences in terms of the social and financial implications of incarceration – compared to job loss – this paper stands in contrast with these findings by estimating *long-term* decreases in earnings and employment for the partners of those who are incarcerated. Therefore, by examining incarceration, this paper extends this literature to a novel and previously unexplored context.

The rest of the paper is organized as follows. Section 2 introduces a simple conceptual framework. Section 3 provides an overview of the criminal justice system in Ecuador. Section 4 explains the data collection process. Sections 6 and 7 discuss the empirical strategy to estimate direct and spillover effects and show the main results. Finally, Section 8 concludes.

2. Conceptual Framework

The labor market effects of incarceration can be understood through several complementary channels. Incarceration may directly reduce the imprisoned individual’s future earnings through human-capital depreciation, employer stigma, recidivism, or the overall psychological costs of prison. Additionally, it mechanically removes the individual from the labor market during the incarceration spell through incapacitation. At the household level, the removal or incapacitation of a primary earner represents a large negative income shock that can induce a reallocation of market and non-market time within the household. Two broad household responses are commonly emphasized in the literature: (i) a substitution into market work by the partner (the added worker response), and (ii) a reduction in partner market work because of increased caregiving, housework, or childcare needs (the caregiving response).

I illustrate the added worker versus caregiving responses with a simple static two-person household model in the spirit of [Lundberg \(1985\)](#). In this framework, incarceration is modeled as a reduced-form incapacitation shock, and caregiving needs are introduced as an explicit parameter. Time is normalized to one, and the household has two adults: the (potentially incarcerated) primary earner m , representing the male household member, and the partner f , representing the female household member. I use male for the potentially incarcerated individual given that over 91% of incarcerated individuals in my sample are male. Each household member supplies market hours $h_i \in [0, 1]$ and leisure $L_i = 1 - h_i$ for $i \in \{m, f\}$. The incarceration shock is represented by $\delta \in [0, 1]$: when $\delta = 0$ there is no incapacitation, and when $\delta = 1$ the male is fully incapacitated. The male's pre-shock hours are taken as fixed at $h_m > 0$ (isolating the household adjustment through the partner); incarceration then reduces the male's earnings mechanically to $(1 - \delta)w_m h_m$.

Caregiving requirements are captured by an exogenous minimum unpaid care-time parameter:

$$g(\delta), \quad g'(\delta) \geq 0,$$

This parameter represents any non-market work required by the partner, such as household work or additional childcare, which directly reduces the partner's feasible market hours. Household consumption is then given by:

$$C = (1 - \delta)w_m h_m + w_f h_f + N,$$

where $w_m, w_f > 0$ and N is non-labor income. For simplicity, preferences are separable and sufficiently regular:

$$U(C, L_f) = u(C) + \alpha v(L_f),$$

with $u' > 0, u'' < 0, v' > 0, v'' < 0$, and $\alpha > 0$. The male's leisure term is omitted since male decisions are fixed for the local comparative static. Therefore, if the caregiving constraint is not binding, the female chooses h_f such that the interior FOC is:

$$u'(C) w_f = \alpha v'(1 - h_f - g(\delta)). \quad (1)$$

However, if the caregiving constraint binds, then $h_f = 1 - g(\delta)$ and the comparative static is determined directly by $g'(\delta)$. Totally differentiating equation 1 with respect to δ , denoting partial derivatives by primes, and replacing L_f with $1 - h_f - g(\delta)$, gives:

$$dC = -w_m h_m d\delta + w_f dh_f, \quad dv'(L_f) = v''(L_f)(-dh_f - g'(\delta) d\delta).$$

This yields:

$$w_f u''(C) (-w_m h_m d\delta + w_f dh_f) = \alpha v''(L_f) (-dh_f - g'(\delta) d\delta).$$

Collecting terms gives the partner's comparative static response:

$$\frac{dh_f}{d\delta} = \frac{w_f u''(C) w_m h_m - \alpha v''(L_f) g'(\delta)}{w_f^2 u''(C) + \alpha v''(L_f)}. \quad (2)$$

Since $u''(C) < 0$ and $v''(L_f) < 0$, the denominator in equation 2 is negative. The numerator is the sum of two terms with opposing economic interpretations:

1. $w_f u''(C) w_m h_m$: (with $u'' < 0$) this term is *negative* and captures the *income-replacement* channel. Mechanically, an increase in δ reduces household labor income by $w_m h_m$, raising the marginal utility of consumption and, ceteris paribus, inducing the partner to increase market hours.
2. $-\alpha v''(L_f) g'(\delta)$: since $v'' < 0$, this term is *positive*. It captures the *caregiving constraint* channel, whereby higher δ increases required care $g(\delta)$ and directly crowds out partner market hours.

Because the denominator is negative, the sign of $dh_f/d\delta$ is the *opposite* of the numerator's sign. Concretely:

1. If the income-replacement term dominates (i.e. $|w_f u'' w_m h_m|$ is large relative to $\alpha |v''| g'$), the numerator is negative and hence $dh_f/d\delta > 0$ — the classical *added-worker* response.
2. If the caregiving crowd-out dominates (i.e. $\alpha |v''| g'$ is larger), the numerator is positive and hence $dh_f/d\delta < 0$ — the *caregiving* (or labor-supply reduction) response.
3. If the caregiving constraint binds exactly, the partner is at the corner $h_f = 1 - g(\delta)$ and $dh_f/d\delta = -g'(\delta)$.

Equation 2 thus provides a compact condition that balances the mechanical income-replacement motive against the caregiving crowd-out effect. It shows transparently how an exogenous caregiving requirement $g(\delta)$ can attenuate or even reverse the added worker response to incapacitation or incarceration.

3. Background: The Criminal Justice System in Ecuador

The criminal justice system in Ecuador is composed of district, provincial i.e. state, and a national court. Unlike countries with federal systems, such as the United States, criminal law in Ecuador is homogeneous across all jurisdictions. This implies that there is a single overarching criminal code, the Codigo Organico Integral Penal (COIP), which also details criminal procedures. Filings under the COIP represent over 50% of all judicial court filings. That number has been rising, reaching its peak in 2023 with

over 322k filings. The second legal code with the most filings is the *Codigo Organico General de Procesos* (COGEP) which refer to marital – divorces, alimony, child support, etc – and labor disputes. The third one, are constitutional disputes which refer to protection measures. For the purposes of this study, I will focus on the cases filed under the COIP which represents the legal code with potential prison terms.

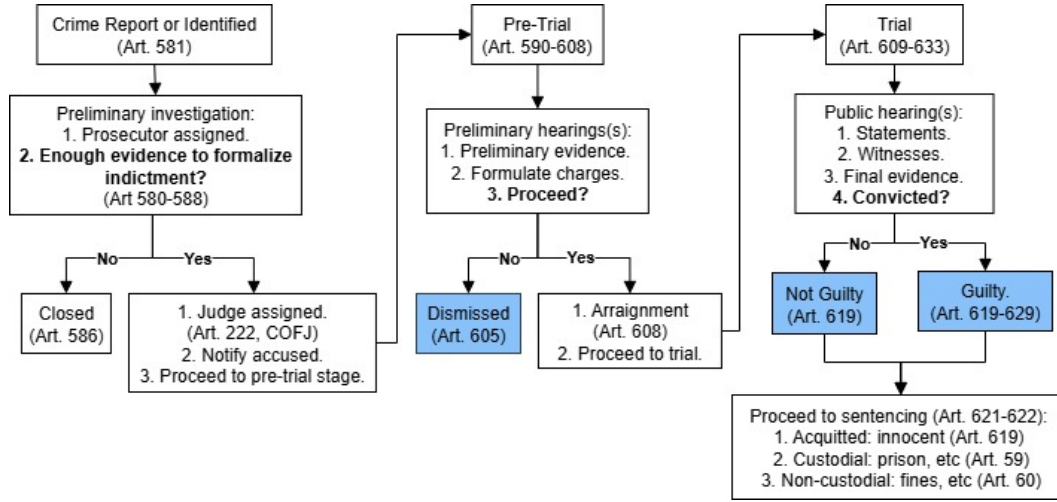
The COIP outlines the two phases – pre-trial and court procedures – in a criminal case broken down into the typical four stages: i) preliminary, ii) intermediate, iii) trial, and iv) appeals¹. The preliminary stage begins once a crime is formally reported or identified by law enforcement. During this stage the investigation is assigned to a prosecutor who has a maximum of two years to decide whether there is enough evidence to proceed with a formal examination. If there is enough evidence, the accused is notified and the prosecutor has up to 90 days to formulate charges. At this point a judge is assigned to the case. The intermediate stage starts with a preliminary hearing during which the judge decides whether to proceed with trial or order a dismissal. The trial stage is carried out through public hearings which end with a guilty or innocent verdict issued by the judge. In the case of a guilty verdict that same judge is tasked with sentencing the accused based on the relevant article in the COIP and any aggravating or mitigating factors. If the verdict is innocent, the accused is acquitted. After sentencing both parties can appeal. Figure 1 describes the ordinary procedure as its outlined in the penal code. Alternative, there is an expedited process i.e. *procedimiento abreviado* – similar to that of a plea bargain in the U.S. – however, in practice it is rarely applied compared to an ordinary procedure.

The COIP divides judicial sentencing into three non-mutually exclusive options: i) custodial, ii) non-custodial, and iii) fines. Non-custodial options are rehabilitation programs, community service, and suspensions such as driving license or passport withholding². If the judge decides on prison time then the range, conditional on the type of crime, is given within the COIP. For example, murder carries between 22 to 26 years, robbery 5 to 7 years, and theft 0.5 to 2 years. There is no lifetime or death sentence with the maximum term being up to 30 years. For the purposes of this study, I will focus on the universe of people who transitioned through the ordinary procedure in a criminal court between 2010 and 2017 and either: (i) got their case dismissed (Art. 605) at pre-trial, (ii) moved on to trial and received a non-guilty verdict (Art. 619), (iii) moved on to trial and received a guilty verdict but with a non-custodial sentence (Art. 60), or (iv) moved on to trial, received a guilty verdict and a custodial sentence (Art. 59). This allows me to compare the labor market trajectories of individuals who *transitioned* through an ordinary procedure, however, received different outcomes by a presiding judge or set of judges.

¹For further details refer to titles 1-5, 2nd book within the COIP.

²For further details refer to articles 58-71, chapter 2, title 2, 1st book within the COIP.

Figure 1: Ordinary procedure described in the penal code [COIP]



Notes: This figure details the ordinary procedure in the penal code. The boxes highlighted in light blue color represent the outcomes for the defendants in my sample. In other words, it outlines the path all individuals in my sample transition through, ending in either a: (i) dismissal at pre-trial, a (ii) non-guilty verdict, a (iii) guilty verdict with a non-custodial sentence, or a (iv) guilty verdict with a custodial sentence.

4. Data

This section describes the judicial data extraction process, as well as details on the labor market data and the way I construct family networks.

4.1. Court Case Records

Court case records in Ecuador are public and accessible through a public online repository maintained by the Judiciary Council. The Judiciary Council is the governing body of the judicial branch which oversees and manages the operations of all courts in the country. They maintain the Automatic System for Judicial Proceedings (SATJE). The SATJE works as the public digital repository for all court cases filled in the country. In practice, it digitizes, organizes, and documents at the case level all its judicial proceedings i.e. actuaciones judiciales, and their content. Each case is given a unique id consisting of: (i) a two digit number associated with Ecuador's 24 provinces i.e. states, (ii) a three digit number associated with a judicial unit, (iii) a four digit year, and (iv) a consecutive number which resets back to zero every year. For example, the court case 19281-2019-00488 represents the 488th case filed in 2019 to the Criminal Judicial Unit in the city of Zamora – code 281 – located in the province of Loja – code 19. Therefore, my data collection process takes advantage of this numbering process to sequentially search for the universe

of non-confidential court case records in the country ³.

From each successful submitted request, I obtain two main outputs. The first one is homogeneous and structured as an .html table containing: filling date, defendants, plaintiffs, judicial unit, field of law and crime code under which the case was filed. See figure 2 for an example. The second one is a heterogeneous and unstructured plain text file, separated by proceeding type, which contains all documented judicial records associated with the case. In other words, each separate proceeding: court/judge assignment, initial hearing, sentencing, etc produces a separate text file alongside its respective date. This system is unique in several dimensions, however, given the purposes of this paper I will highlight the three most relevant. First, as discussed above, the Ecuadorian judicial system is heavily centralized. This translates to the SATJE containing records for all – not just criminal – fields of law: civil, administrative, tax, transit, family, juvenile, labor, and criminal law. Second, access to such granular judicial data is unique. Large scale access to court case records is effectively denied or virtually unattainable within federal countries in Latin America such as Mexico, Brazil, Peru, Argentina, or Chile. Third, it includes information at the individual level which allows me to merge with separate administrative databases. Therefore, the SATJE allows me to create a database representing all individuals who have been listed, not just convicted, as a defendant in a lawsuit within the judicial system in Ecuador.

Figure 2: Court case details extracted from SATJE

Datos generales			
Número de proceso	CASE ID	Fecha ingreso	22/12/2017 17:11
Materia	TRÁNSITO COIP	Tipo de acción	DELITOS DE TRÁNSITO
Delito/Asunto	379 LESIONES CAUSADAS POR ACCIDENTE DE TRÁNSITO, NUM. 3	Judicatura	UNIDAD JUDICIAL DE GARANTÍAS PENALES CON SEDE EN EL CANTÓN IBARRA
Tipo de Ingreso	Ingreso directo	No. proceso vinculado	RELATED CASE ID
Actor/Ofendido:	PLAINTIFF	Demandado/Procesado:	DEFENDANT

Notes: This figure shows an example of the first data output as described in section 4.1. It contains the unique case id, field of law and crime code, judicial unit and location i.e city, filling type, filling date, plaintiffs and defendant names. Sensitive information has been blacked out.

4.2. Sentencing Assignment

Given that details regarding the case outcomes come in an unstructured format – text data – I use a series of rule-based methods to extract case level outcomes. This starts with identifying the sentencing act – last formal step in figure 1 – within each case. According to the law governing judicial proceedings (COFJ), prior to sentencing the presiding judge or set of judges must issue the following statement: *"Administering justice, in the name of the sovereign people of Ecuador, and by authority of the Constitution and the laws of the*

³Sex crimes, family violence, and crimes against the state are classified as confidential therefore not available in the SATJE. See articles 336-365 and 362 of the Código Orgánico Integral Penal (COIP 2014).

Republic". After which they outline the verdict, as well as any additional details regarding the case. Figure 3 outlines an example of a sentencing act and the information extracted from this document. Therefore, for the universe of cases in my sample I extract the relevant information from the sentencing act regarding the case verdict ⁴. Given that custodial and non-custodial terms are non-mutually exclusive a defendant who received a custodial term might also receive a non-custodial sentence, such as a monetary fine.

Figure 3: Judicial Sentencing Details (Art. 621-622 in the COIP)

Fecha de ingreso	Detalle
	consideraciones que anteceden, ADMINISTRANDO JUSTICIA EN NOMBRE DEL PUEBLO SOBERANO DEL ECUADOR Y POR AUTORIDAD DE LA CONSTITUCIÓN Y LEYES DE LA REPÚBLICA, en base al análisis efectuado y a la valoración de la prueba aportada en la etapa de juicio, en mi calidad de juez de la Unidad Judicial Penal de Ibarra, dicto sentencia de culpabilidad en contra de DEFENDANT ecuatoriano, mayor de edad, por ser el autor del delito de tránsito tipificado y sancionado en el Art. 379 inciso tercero del COIP en
	
	amparada en el artículo 66 numeral 3 de la Constitución de la República del Ecuador); a quien se le impone: 1.- La pena de UN AÑO CUATRO MESES DE PRIVACIÓN DE LIBERTAD en razón del siguiente análisis: La pena
	
	el organismo técnico así lo disponga; pena que se empezará a cumplir una vez ejecutoriada la sentencia. 2).- No se dispone la suspensión de la licencia de conducir ya que el sentenciado no tiene licencia. 3).- Al pago de la multa de CUATRO salarios básicos unificados del trabajador en general de conformidad con lo dispuesto en el artículo 70 numeral 4 del COIP. 4).- De conformidad con lo dispuesto en el artículo 64 numeral 2 de la

Notes: This figure shows an example of a sentencing act as described in section 4.2. The highlighted portion of the act is extracted in order to assign outcomes corresponding to each case. In this example, the first box details the custodial term: 1 year and 4 months of prison time, and the second box details any non-custodial details: monetary fine of 4 monthly minimum wages.

4.3. Employment and Earnings

In order to measure the effect on employment and earnings I rely on monthly earnings data for the 2010-2017 period. This data is reported by employers to the Ecuadorian social security institute (IESS) and it covers all formal sector employees in the country (Adão et al., 2022). All employers based in Ecuador, public or private, by law, need to affiliate their employees to social security ⁵. New employees must be affiliated within 15 days of starting a job. Employers report monthly earnings, which includes bonuses or extra-hours, for all their employees. In other words, they do not just report their employment contract and length but rather the amount distributed per month to each employee. Additionally, they have 3 days to report any change in the employment status – firing, quitting, leave, etc – of their employees. This detail is key since it allows me to capture immediate changes in employment status for individuals in my

⁴Unfortunately, I do not observe details on any potential pre-trial detention or date of arrest which might alter labor trajectories prior to trial.

⁵See article 369 of the Ecuadorian constitution.

sample. Therefore, in summary, I observe monthly earnings and employment status for the universe of formal employees in the country between 2010-2017.

4.4. Family Networks

In order to study potential within-family spillover effects in labor outcomes I construct family networks using data from the Office of Vital Statistics. In practice, this data source allows me to link defendants with their partners through a marriage certificate or through shared parenthood. Given that I observe the timing of the event – marriage or childbirth – I assign partners relative to the trial filing date. Additionally, I extend the family network up to their siblings which are identified by having the same mother listed in their birth certificate. Therefore, I can link the universe of defendants in my sample to their respective partners and siblings.

5. Sample Description

This section provides details regarding the sample selection process as well as summary statistics broken down by case outcome.

5.1. Sample Selection

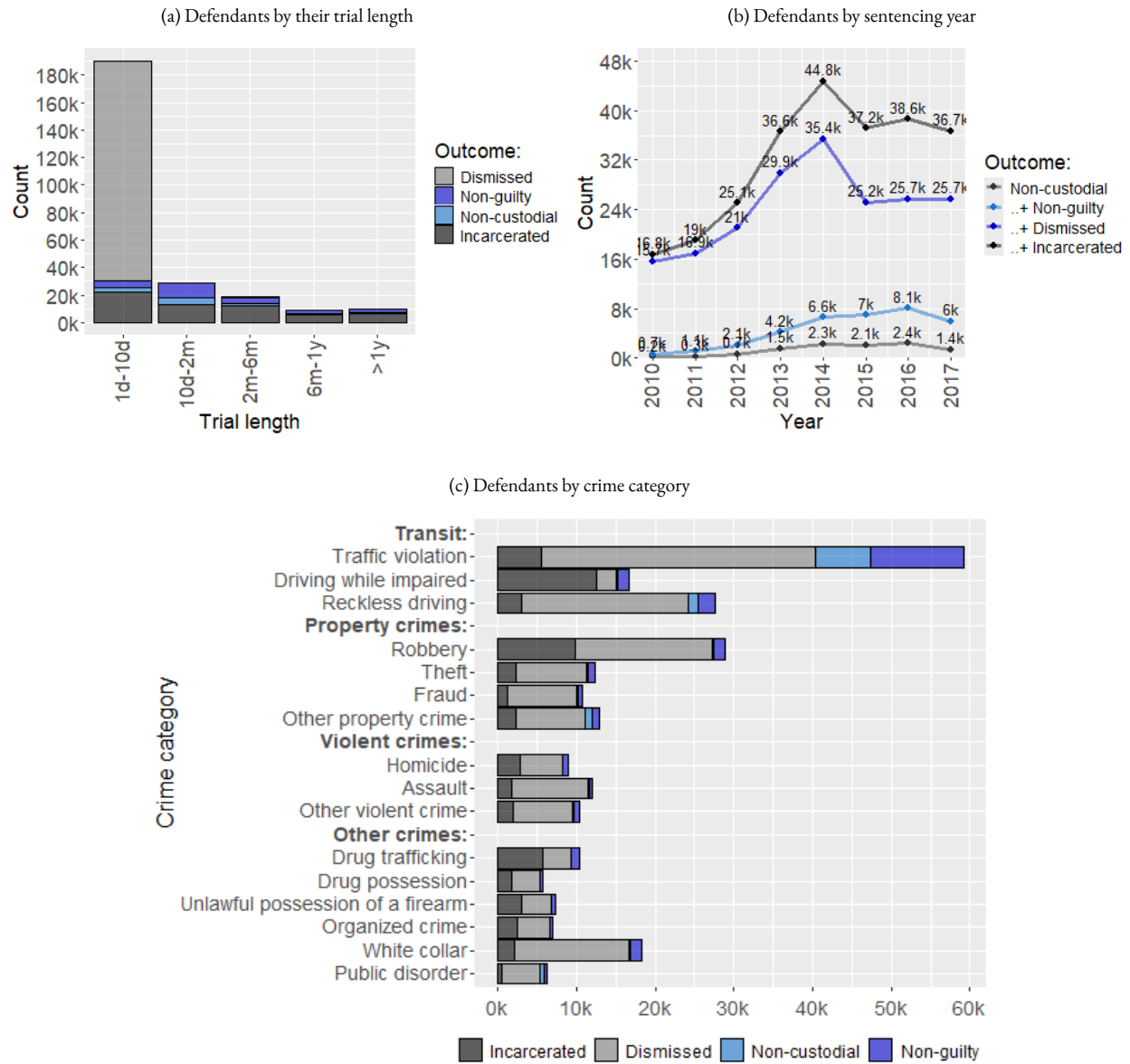
In order to improve the pre-trial comparability of my sample of defendants I impose the following restrictions. First, at the court case level I restrict to cases which reached a resolution within a criminal court between 2010 and 2017 and whose resolution was either a pre-trial dismissal or a verdict: custodial (prison term), non-custodial (monetary fine, drivers license points deduction, community service, etc) or a non-guilty verdict. Second, among defendants listed in those court cases I restrict to those who were formally employed for at least one month between 2010 and 2017, those who were between 18 and 55 years old at the time the case was filed, and those who had not been incarcerated before. In addition to improving comparability, these restrictions narrows down on individuals who were working age, active in the labor market, and *had not* served prison time before. Once that sample is selected I am left with 324k different criminal cases representing a total of 254k unique defendants. Figure 4 shows the breakdown of defendants in the final sample by sentencing year, trial length, and crime category. Mechanically, cases which are dismissed have a shorter trial length with the average trial lasting around 52 days.

Since custodial terms are usually accompanied by a non-custodial term (prison time plus a monetary fine) I assign those defendants to the sample of incarcerated. Defendants may appear more than once in my sample. For those who received more than one prison term between 2010-2017 – represents less than 2.4% of the sample – I keep their first incarceration trial. For those who do not receive a prison term I keep their first trial, irrespective of the case outcome. Figure 4b shows the breakdown of defendants by sentencing year and case outcome. Around 61% had their case dismissed during pre-trial proceedings. Out

of the defendants which reached a verdict, 23% received a prison term, 5% a non-custodial term, and 10% a non-guilty verdict. These numbers varied year over year between 2010 and 2017.

Given that the penal code covers all crimes including those who are transit-related, I further group them – for descriptive purposes – into broader categories. Figure 4c shows the breakdown of the final sample by each of these broader categories and also by case outcome. Importantly, among all categories there are defendants who received custodial terms. Around 20% of all cases were filed for a severe or third-degree traffic violation – such as disobeying transit police orders, driving without a license or an expired license, etc. The other transit-related crimes represent 14% of all filings. They relate to defendants driving while impaired – such as drunk driving – or reckless driving – such as causing a traffic accident. There are 28% of cases filed for a property crime, 13% for a violent crime, 8% for a drug-related crime, and the remaining 18% for other crimes.

Figure 4: Distribution of defendants by their trial length, sentencing year and crime category



Notes: This figure shows the distribution of defendants in the estimation sample by their trial length (figure 4a), sentencing year (figure 4b), and crime category (figure 4c). Additionally, each plot shows the breakdown by their case outcome: incarceration, non-custodial verdict, non-guilty verdict, and dismissed.

5.2. Summary Statistics

For the final sample of defendants I assign demographic characteristics. Table 1 shows summary statistics for a rich set of demographic variables by incarceration status and time relative to the case filing date. Defendants who are eventually incarcerated are more likely to be male, younger, and single by the

time of trial. They are also more likely to have been listed as a defendant in other criminal case in the past. Finally, they have lower earnings prior to trial, more employers, and are employed within larger firms. Table 2 shows details on the case and its respective verdict. On average, defendants who are incarcerated face longer trials and eventually larger fines than those who are not. Additionally, they are accused and later sentenced for more severe crimes. For instance, about 27% of non-incarcerated individuals were tried for severe traffic violations, compared to only 9% among the incarcerated. Conversely, around 16% of incarcerated defendants were tried for robbery, compared to only 9% among non-incarcerated. Finally, given the nationwide coverage of the sample over 43% of cases occurred within smaller provinces.

Table 1: Summary statistics across incarceration status: demographics

Variable	All:		Incarcerated:	Non-incarcerated:
	Range	Mean (SD)	Mean (SD)	Mean (SD)
Demographics:				
Male	[0, 1]	0.846 (0.361)	0.919 (0.273)	0.824 (0.381)
Citizen	[0, 1]	0.995 (0.071)	0.996 (0.062)	0.995 (0.074)
Absent father	[0, 1]	0.230 (0.421)	0.230 (0.421)	0.230 (0.421)
Educational attainment:				
None	[0, 1]	0.007 (0.082)	0.009 (0.095)	0.006 (0.077)
Preschool	[0, 1]	0.011 (0.105)	0.017 (0.127)	0.010 (0.097)
Primary	[0, 1]	0.324 (0.468)	0.385 (0.487)	0.305 (0.461)
Secondary	[0, 1]	0.450 (0.497)	0.444 (0.497)	0.452 (0.498)
Tertiary	[0, 1]	0.208 (0.406)	0.146 (0.353)	0.227 (0.419)
Demographics (pre-trial):				
Age	[18, 55]	33.280 (9.491)	31.046 (8.635)	33.956 (9.634)
Nbr. Children	[0, 18]	1.640 (1.584)	1.461 (1.530)	1.695 (1.596)
Married	[0, 1]	0.402 (0.490)	0.300 (0.458)	0.433 (0.495)
Nbr. Partners	[0, 10]	0.938 (0.800)	0.870 (0.799)	0.958 (0.800)
Nbr. Siblings	[0, 12]	2.586 (2.310)	2.581 (2.275)	2.588 (2.320)
Nbr. Cases	[0, 19]	0.780 (1.764)	1.252 (2.233)	0.637 (1.567)
Nbr. Cases w/Verdict	[0, 6]	0.139 (0.514)	0.169 (0.527)	0.129 (0.510)
Labor outcomes (pre-trial):				
Monthly earnings (USD)	[0, 33,719]	409 (511.084)	342 (397.761)	430 (540.169)
Monthly earnings [24m pre-trial] (USD)	[0, 29,087]	446 (551.079)	378 (428.197)	465 (579.948)
Employed	[0, 1]	0.849 (0.358)	0.875 (0.330)	0.841 (0.366)
Employed [24m pre-trial]	[0, 1]	0.674 (0.469)	0.644 (0.479)	0.683 (0.465)
Employer details (pre-trial):				
Nbr. Employers	[1, 13]	1.245 (0.561)	1.258 (0.579)	1.241 (0.555)
a. Employer size (Nbr. employees):				
Micro (1-9)	[0, 1]	0.137 (0.344)	0.118 (0.322)	0.143 (0.350)
Small (10-49)	[0, 1]	0.113 (0.317)	0.123 (0.328)	0.110 (0.313)
Medium (50-249)	[0, 1]	0.132 (0.338)	0.146 (0.353)	0.127 (0.333)
Large (>=250)	[0, 1]	0.325 (0.468)	0.358 (0.479)	0.315 (0.464)
b. Employer size (Wage bill):				
Micro (<\$50k)	[0, 1]	0.200 (0.400)	0.190 (0.392)	0.203 (0.402)
Small (\$50k-\$250k)	[0, 1]	0.120 (0.325)	0.135 (0.342)	0.116 (0.320)
Medium (\$250k-\$1m)	[0, 1]	0.109 (0.311)	0.126 (0.331)	0.104 (0.305)
Large (>=\$1m)	[0, 1]	0.277 (0.448)	0.293 (0.455)	0.273 (0.445)
Sample:		254,646	59,119	195,527

Notes: This table reports summary statistics broken down by treatment status for the estimation sample: cases (i) filed within a criminal court with a resolution between 2010-2017, where the defendant was (ii) between 18-55 years old at filing, (iii) employed for at least one month between 2010-2017 in the formal sector, and (iv) had not served jail time before. Absent father represents a missing father in the registry of the Office of Vital Statistics. Educational attainment represents the max level achieved by 2021.

The third, fourth, and fifth panel are estimated at the time the case is filed. Number of partners represents the number of people with which the defendant has had a children. Number of cases represents the total number of cases with or without a verdict where that person was listed as a defendant in a case. Number of cases with verdict represents the total number of cases with a non-prison verdict where that person was listed as a defendant in a case. The fourth panel is estimated during two different time windows: (i) all months prior to filing and (ii) 24 months prior to filing. The fifth panel represents characteristics of the defendant's employer during the year prior to filing. Number of employers represents the total number of firms who listed the defendant as an employee the year prior to filing.

Table 2: Summary statistics across incarceration status: case & verdict details

Variable	All:		Incarcerated:	Non-incarcerated:
	Range	Mean (SD)	Mean (SD)	Mean (SD)
Case & verdict details:				
Trial length (days)	[0, 2577]	52.903 (204.436)	147.395 (323.977)	24.332 (138.491)
Prison term (months)	[0, 360]	5.404 (25.849)	23.277 (49.618)	0.000 (0.000)
Non-guilty verdict	[0, 1]	0.105 (0.307)	0.020 (0.138)	0.131 (0.338)
Monetary fine (USD)	[0, 702,114]	453 (10,481)	1,763,122 (20,992)	57.114 (3,026)
Monetary fine (SBU)	[0, 1,500]	3.811 (52.840)	16.110 (108.299)	0.093 (5.514)
License points deductions	[0, 30]	0.718 (3.163)	2.109 (5.780)	0.297 (1.472)
Financial restitution (USD)	[0, 155,355]	115,242 (2545,595)	473,136 (5235,817)	7,030 (316,644)
Financial restitution (SBU)	[0, 800]	0.208 (10.542)	0.879 (21.795)	0.005 (0.973)
Miscellaneous legal fee (USD)	[0, 20,000]	7,876 (283,329)	25,745 (498,359)	2,473 (171,256)
Crime category:				
a. Transit:				
Severe traffic violation	[0, 1]	0.232 (0.422)	0.095 (0.294)	0.274 (0.446)
Driving while impaired	[0, 1]	0.066 (0.248)	0.213 (0.409)	0.021 (0.144)
Reckless driving	[0, 1]	0.109 (0.311)	0.052 (0.221)	0.126 (0.332)
b. Property crime:				
Robbery	[0, 1]	0.113 (0.317)	0.167 (0.373)	0.097 (0.296)
Theft	[0, 1]	0.048 (0.215)	0.038 (0.192)	0.051 (0.221)
Fraud	[0, 1]	0.042 (0.201)	0.022 (0.146)	0.048 (0.214)
Other property crime	[0, 1]	0.050 (0.219)	0.039 (0.194)	0.054 (0.226)
c. Violent crime:				
Homicide	[0, 1]	0.035 (0.185)	0.048 (0.214)	0.032 (0.175)
Assault	[0, 1]	0.047 (0.212)	0.030 (0.169)	0.053 (0.223)
Other violent crime	[0, 1]	0.040 (0.197)	0.033 (0.178)	0.043 (0.202)
d. Other crimes:				
Drug trafficking	[0, 1]	0.041 (0.197)	0.097 (0.296)	0.024 (0.152)
Drug possession	[0, 1]	0.022 (0.148)	0.030 (0.169)	0.020 (0.141)
Unlawful possession of a firearm	[0, 1]	0.029 (0.167)	0.051 (0.219)	0.022 (0.147)
Organized crime	[0, 1]	0.028 (0.164)	0.042 (0.200)	0.024 (0.152)
White collar	[0, 1]	0.072 (0.258)	0.036 (0.185)	0.083 (0.276)
Public disorder	[0, 1]	0.025 (0.155)	0.009 (0.096)	0.029 (0.168)
Court location:				
Guayas	[0, 1]	0.253 (0.435)	0.304 (0.460)	0.237 (0.425)
Pichincha	[0, 1]	0.150 (0.357)	0.149 (0.356)	0.150 (0.357)
Manabi	[0, 1]	0.080 (0.272)	0.062 (0.242)	0.086 (0.280)
Azuay	[0, 1]	0.079 (0.269)	0.102 (0.302)	0.072 (0.258)
Other province	[0, 1]	0.438 (0.496)	0.383 (0.486)	0.455 (0.498)
Sample:		254,646	59,119	195,527

Notes: This table reports summary statistics broken down by treatment status for the estimation sample: cases (i) filed within a criminal court with a resolution between 2010-2017, where the defendant was (ii) between 18-55 years old at filing, (iii) employed for at least one month between 2010-2017 in the formal sector, and (iv) had not served jail time before. Monetary fine (SBU) represents the monetary amount of the fine expressed in terms of the number of monthly minimum wages (in 2017 the SBU was \$375). License points deductions represents the number of points deducted from the defendants driver license (licensed drivers start with 30 points). Financial restitution represents the court order restitution amount. The four largest provinces in terms of population (where 56% of people reside) are: Guayas, Pichincha, Manabi, and Azuay.

6. Direct Effects of Incarceration

This section presents stylized facts regarding the separate sub-samples, details on the empirical strategy used to estimate the effects of incarceration on the defendant, and the main results from that estimation on earnings and probability of employment.

6.1. Stylized Facts

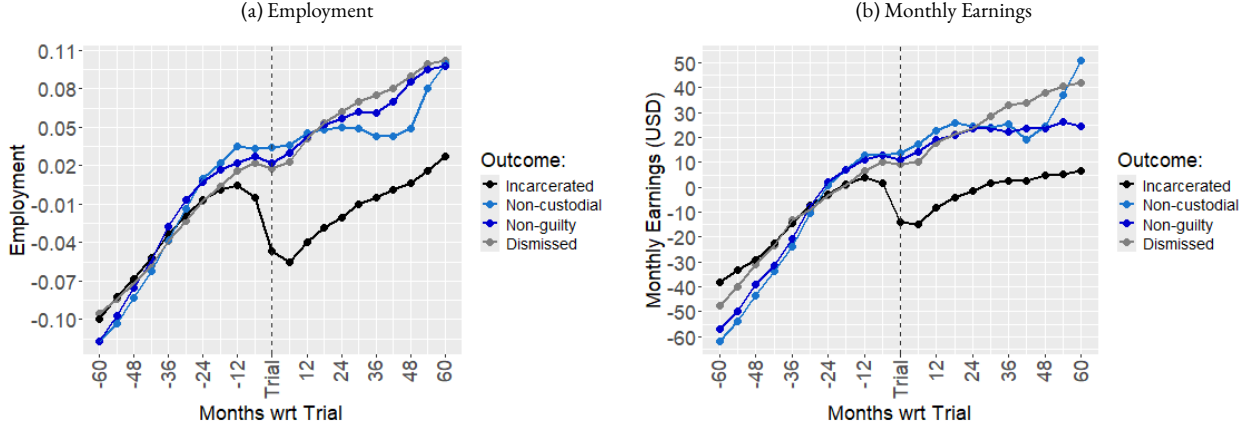
Before discussing the empirical strategy and estimation results, I provide visual evidence to show the impact of incarceration on labor outcomes. I use a straightforward approach to illustrate the dynamics of the main outcomes of interest: monthly earnings and employment status, around the trial dates. First, as shown in table 1, there are clear baseline differences between defendants who are incarcerated compared to those who are not. In order to visually narrow those differences I take advantage of the *very* rich set of covariates in my data. First, I construct a panel at the defendant-month level. Then, I residualize monthly earnings and employment status by regressing both variables on several demographic covariates: age at filing, number of children, partners, and siblings pre-trial, gender, citizenship, and marital status. I also include trial length and the number of cases the person was listed as a defendant as a measure for prior contact with the justice system. Finally, I add pre-trial earnings and employment status as well as the crime category and the court location for their case. I then estimate their average value relative to the trial months. Figure 5 plots these averages. In other words, the values on the vertical dotted line represent the average for all months during which the defendant was on trial. Therefore, all negative months are relative to the case filing month and all positive months are relative to the sentencing date. The average trial length was 52 days. These figures show how defendants labor outcomes evolve relative to their own expected values given their background characteristics.

These plots illustrate some interesting patterns in the data. First, during the study period the Ecuadorian government raised the monthly minimum wage (Salario Basico Unificado) at a pace which largely outpaced inflation. The monthly minimum wage increased from \$218 to \$385. Additionally, an amendment to the constitution in 2008 forced employers to affiliate their employees to social security. This meant a large share of previously informal workers became formally employed and had access to social security. Prior to that, it was voluntary and up to the employer. Combined, these two facts explain the positive trend in both outcomes observed for *all* groups. Second, it appears that up to 1 year prior to trial the trend was similar for all groups⁶. After that, there is a clear drop in earnings and employment which is *more* pronounced for those who are incarcerated. However, all other groups – on average – observe a *much* smaller but still discernible drop in earnings and employment during trial and up to a year after. For the purposes of this study this implies that any estimated effect of incarceration, relative to other outcomes,

⁶I will formally test for pre-trends in section 6.3

can be interpreted as a lower bound of the treatment effect of incarceration. Third, this drop appears to be long-lasting with the gap remaining constant ever after 5 years. This descriptive analysis supports the finding that incarceration leads to sharp and long-lasting negative labor market effects.

Figure 5: Residualized monthly earnings and employment status by case outcome



Notes: This figure plots residualized employment status (figure 5b) and monthly earnings (figure 5a) relative to the trial dates by case outcome. To estimate residualized values I regress both outcomes on several pre-trial case level controls listed in table 2, and demographic controls listed in table 1. Average monthly values by case outcome are estimated relative to the trial dates. In other words, the average values on the vertical dotted line represents the average for all months during which the defendant was on trial. The average trial length was 52 days. Therefore, all negative months are relative to the case filing month and all positive months are relative to the sentencing date.

6.2. Empirical Strategy

In order to estimate the effect of incarceration on labor market outcomes for the defendant I use an event study design. This allows me to compare labor market trajectories for defendants who were incarcerated against those who (i) had their case dismissed by a judge during pre-trial, (ii) received a non-custodial verdict (monetary fine, license points deduction, etc), or (iii) received a non-guilty verdict. For the sample described in section 5.1, I construct a panel at the defendant-month level which allows me to estimate the following event study using OLS:

$$y_{d,t} = \sum_{k=-96}^{96} \beta_k 1\{t = t_T + k\} \times Incarcerated_d + \alpha_d + \omega_t + \epsilon_{d,t} \quad (3)$$

Where $y_{d,t}$ represents the outcome of interest for defendant d during month t . Similarly, $Incarcerated_d$ is an indicator variable which equals 1 if defendant d was incarcerated, 0 otherwise, and $1\{t = t_T + k\}$ represent event time dummies relative to the trial months (t_T). Finally, I include defendant i.e. individual (α_d) and time (ω_t) fixed effects and $\epsilon_{d,t}$ is the error term. Throughout the analysis, I use a 7 year (96

months) window before and after the trial months. For simplicity, I bin months into years relative the trial dates. The trial starts once the case is filed and it ends once a verdict or dismissal is issued. Given that I do not observe potential pre-trial detention dates, which can alter labor trajectories prior to case filing, I omit the dummies representing the 7th year before case filing. Therefore, the estimated β_k represents changes in monthly labor outcomes between incarcerated and non-incarcerated defendants relative to the same difference 7 years before the case filing date ⁷. Finally, I cluster the standard errors at the individual level.

6.3. Main Results

Using the approach described in section 6.2 I estimate the dynamic effects of incarceration relative to those who received a non-custodial verdict, non-guilty verdict or had the case dismissed by a judge during pre-trial. In other words, I define as the control sample all individuals who were not incarcerated. Estimates from this specification are plotted in figure 6. First, figure 6a plots the dynamic effects of incarceration on the intensive margin: monthly earnings. Consistent with the descriptive analysis, I find a clear pre-trial decrease in monthly earnings starting the year before trial. The estimated difference of \$7 the year before trial represents a decrease of around 6% (\$7/\$107) relative to the baseline mean for the incarcerated sample. During trial, defendants who are eventually incarcerated suffer a large drop of \$37, on average, which represents a 35% decrease. Finally, after sentencing there is a negative effect of around 37% which remains constant up until 7 years. This result stands in contrast to prior evidence which has documented a rather short-lived effect on earnings. Figure 6c plots estimates on logged earnings which produce larger effects but show the same pattern.

After sentencing, defendants who are incarcerated are forcefully removed from the labor market. Therefore, mechanically I observe drops in earnings that can be explained by incapacitation. However, in my sample the average prison term is 23 months. Figure 6a shows that even after 2 years, there are still *large* differences in earnings which are not directly explained by incapacitation. I replicate the analysis now looking at the extensive margin, whether the defendant is formally employed or not. Figure 6b plots estimates on the probability of employment. Interestingly, I find the same pattern. The year prior to trial there is a large drop in the probability of employment of around 3 percentage points which represents a 13% drop relative to the baseline mean (0.2248). During trial, those who are incarcerated observe a drop of around 44% in their probability of employment. That effect is constant and observed up until 7 years after sentencing. These two results imply that incarceration has a lasting long-term negative effect on earnings

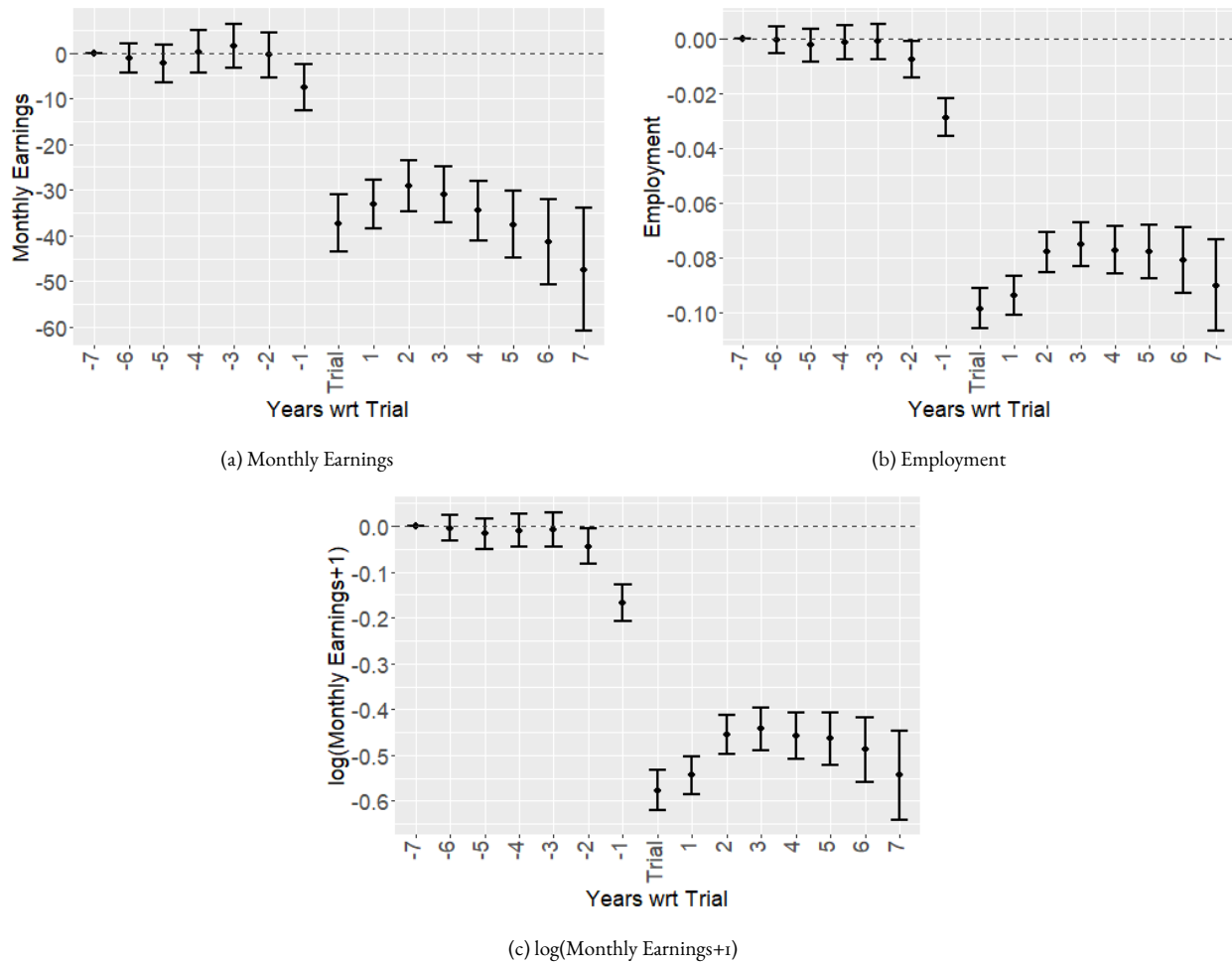
⁷As robustness, figure 9 in the appendix varies the sample of controls to each specific sub-sample: only dismissals, or only non-guilty, or only non-custodial and finds that the estimates vary *very* little. Figure 10 uses the approach proposed by Callaway and Sant'Anna (2021) to estimate coefficients robust to staggered treatment designs and finds similar coefficients and effect sizes. Figure 8 employs matching methods – NNM and CEM – to improve balance at baseline and finds that coefficients vary *very* little.

and the probability of employment. These effects appear to not be driven by incapacitation since I find consistent effects well after the expiration of their prison term.

In summary, defendants who are eventually sentenced to prison observe drops in monthly earnings and employment which start prior to the case filing date and last up to 7 years after sentencing. Importantly, I observe no significant differences in earnings up to a year before trial which suggests that both groups were on similar trends. Ideally I would want to observe pre-trial detention dates or arrest dates but such dates are not systematically reported in case files. However, to formally test for pre-trends I present a simplified version in table 3. Table 3 shows binned estimates of equation 3 on earnings – both monthly and annual earnings by collapsing the panel at the defendant-year level – and on the probability of employment. Just as in figure 6 I estimate non-significant differences up to a year prior to trial which suggests no evidence of pre-trends.

Finally, I attempt to contextualize the magnitude of the effects withing the broader literature looking at the causal effect of incarceration on employment. [Garin et al. \(2025\)](#) use data from Ohio and North Carolina and they estimate a short-term negative effect on the probability of employment of around 10%. This effect disappears after 3-4 years. Using a similar design, [Bhuller et al. \(2020\)](#) study the Norwegian context and find that those who were previously employed observe a decrease in the probability of employment of around 30%. Similarly, [Bhuller et al. \(2018\)](#) also use Norwegian data and find that incarceration reduces the probability of employment by around 20%. Finally, [Harding et al. \(2018\)](#) use data from Michigan and estimate a short-term negative effect of around 24% on the probability of employment. This effect *shrinks* to 9% by the end of the 3rd year post-release. Therefore, in comparison to the literature, the estimated effect sizes for my setting are up to *twice* as large as those which have been documented for high-income contexts. I argue that these differences are driven by the high degree of informality in labor markets within developing countries – around 42.8% of workers were informal in Ecuador in 2010 ([INEC, 2010](#)) – rather than transitions to unemployment. In other words, former inmates might be transitioning to the informal sector rather than remaining unemployed. I, unfortunately, cannot test for this given that I do not observe the informal sector in my data.

Figure 6: Estimated Effect of Incarceration on Labor Outcomes: Defendant



Notes: This figure plots estimates from equation 3 for monthly earnings (figure 6a), probability of employment (figure 6b), and logged earnings (figure 6c). The treated sample are all defendants – as defined in section 5 – who were incarcerated. The control sample are **all** defendants who: got their case dismissed during pre-trial proceeding, were handed a non-custodial sentence, or found non-guilty. We normalize to the 7th year before the case filing date and we bin – for simplicity – all months relative to the trial dates into years. Errors are clustered at the individual level. These estimates include individual and year-month fixed effects.

Table 3: Estimated Effect of Incarceration on Labor Outcomes: Defendant

	log(Monthly Earnings+1) (1)	Monthly Earnings (2)	Annual Earnings (3)	Employment (4)
Pre-trial (6-2yrs)	-0.0201 (0.0164)	-0.5780 (2.004)	-10.68 (29.64)	-0.0033 (0.0028)
Pre-trial (1yr)	-0.1695*** (0.0191)	-7.949*** (2.398)	-52.95 (33.48)	-0.0292*** (0.0033)
Trial	-0.5781*** (0.0211)	-37.78*** (3.125)	-321.8*** (35.75)	-0.0988*** (0.0036)
Post-trial (1-3yrs)	-0.4942*** (0.0197)	-31.84*** (2.622)	-376.1*** (37.83)	-0.0849*** (0.0034)
Post-trial (4-7yrs)	-0.4744*** (0.0242)	-37.49*** (3.186)	-464.7*** (45.74)	-0.0798*** (0.0041)
Observations	24,446,016	24,446,016	2,037,168	24,446,016
Sample	254,646	254,646	254,646	254,646
R ²	0.55651	0.66511	0.76015	0.51709
Mean DV		\$107	\$1,210	0.2248

Notes: This table represents estimates of separate regressions on logged monthly earnings, monthly earnings, annual earnings – collapsed panel – and monthly employment. Mean values for each dependent variable represent the average for the treated sample during their baseline period: 7 years before trial. Errors are clustered – reported in parenthesis – at the individual level.

These estimates include individual and year-month fixed effects. Significance codes: ***: 0.01, **: 0.05, *: 0.1

6.4. Robustness

As robustness I produce three separate results. First, I estimate equation 3 varying the sub-sample which serve as control. In other words, I compare earnings trajectories of those who are incarcerated against those who had their case dismissed, *or* those who received a non-guilty verdict, *or* those who received a non-custodial verdict. Estimates from this exercise are plotted in figure 9. Regardless of the choice of control group I observe the same pattern and the magnitude of the coefficient varies very little. As highlighted in section 6.1 I interpret the magnitudes of these effects as lower bounds given that all defendants observe *slight* drops in earnings around their trial dates.

Second, recent developments in the difference-in-differences literature find that settings with staggered treatment designs could produce biased estimates when there are heterogeneous treatment effects over time or across units (de Chaisemartin and D’Haultfoeuille, 2023; Roth et al., 2023). The main reason for this issue has to do with earlier treated units which serve as controls for later treated units. Under heterogeneous effects the treatment can cause units, in this case individuals, to be on different trends. This implies that earlier treated units may no longer work as valid comparisons for later treated units, potentially leading to biased estimates. Therefore, I use the estimator proposed in Callaway and Sant’Anna (2021) which directly address this common issue in staggered treatment designs. I plot these estimates – using both never-treated or not-yet treated as controls – against the standard approach in

figure 10. Estimates from both approaches produce very similar estimates for both monthly earnings and the probability of employment.

Third, I use propensity score matching methods to improve the pre-treatment comparability of my estimation sample. I use three methods: nearest-neighbor matching (NNM) with and without replacement, and coarsened exact matching (CEM). First, using NNM I pair each defendant in the incarcerated sample (59k) to a defendant in the control sample. The possible control group comprises all defendants who were not incarcerated (195k). To perform the matching, I first estimated a logit model using the cross-sectional sample of treated and potential control defendants. The dependent variable is a binary indicator for whether a defendant was incarcerated or not. The independent variables include the following controls: gender, age, citizenship, absent father, education level, marriage status, number of children, partners, siblings, number of cases, monthly wages, employment status, and trial duration. The following were estimated by the trial date: age, number of children, partners, siblings, and cases. Number of cases refers to the number of times the person was listed as a defendant in a criminal case. Wages and employment are estimated as the average values during the 24 months preceding the trial date.

Using the predicted values from this model, I matched each incarcerated defendant with its closest control defendant without replacement. I reproduce this procedure, allowing for control defendants to serve as control for more than one incarcerated defendant i.e. with replacement. Second, I also employ a CEM approach, in which covariates are first grouped into substantively meaningful intervals before performing exact matching, ensuring that treated and control defendants fall within the same coarsened strata. Figure 8c plots the standardized mean difference for these covariates across matching methods. Estimates from these three approaches are plotted in figure 8. Regardless of the matching method, I observe the same pattern and the magnitude of the coefficients vary very little. These results make me confident about the estimated main effect on earnings and employment.

7. Spillover Effects of Incarceration

This section presents details on the sample selection, the empirical strategy used to estimate potential within-family spillover effects of incarceration, and the main results from that estimation on earnings and probability of employment.

7.1. Sample Selection

First, I identify partners for those defendants in my sample. I define a partner based on marriage and shared parenthood, both occurring prior to trial. In other words, given that I observe the timing of the event – marriage or childbirth – I assign partners relative to the trial filing date. This restricts to partners who were more likely to be directly related to or financially relying on the defendant around trial dates. Additionally, I restrict to partners who were between 18 and 55 years at trial and who were

active in the labor market – employed for at least one month between 2010-2017. After these restrictions I identify 220k partners. Additionally, I extend the family network up to their siblings. Studying the labor supply response of siblings is relevant given that (i) extended family households are common on Ecuador, and (ii) post-incarceration caregiving responsibilities might not be limited to partners but rather distributed within direct family members. Siblings are identified by having the same mother listed in their birth certificate. I add the same restriction: 18 to 55 years old and being active in the labor market. After this restriction I identify 319k siblings in my sample ⁸.

7.2. Empirical Strategy

In order to estimate the effect of incarceration on labor market outcomes for the partners and siblings I use an event study design. This allows me to compare labor market trajectories for individuals whose partner or sibling was incarcerated against those whose partner or sibling (i) had their case dismissed by a judge during pre-trial, (ii) received a non-custodial verdict (monetary fine, license points deduction, etc), or (iii) received a non-guilty verdict. For the sample described in section 7.1, I construct a panel at the individual-month level which allows me to estimate the following event study separately for partners and siblings using OLS:

$$y_{s,t} = \sum_{k=-96}^{96} \beta_k 1\{t = t_T + k\} \times Incarcerated_d + \alpha_s + \omega_t + \epsilon_{s,t} \quad (4)$$

Where $y_{s,t}$ represents the outcome of interest for the partner or sibling s during month t . Similarly, $Incarcerated_d$ is an indicator variable which equals 1 if their partner or sibling was incarcerated, 0 otherwise, and $1\{t = t_T + k\}$ represent event time dummies relative to the trial months (t_T). Finally, I include individual (α_s) and time (ω_t) fixed effects and $\epsilon_{s,t}$ is the error term. As in the direct effect estimation, I use a 7 year (96 months) window before and after the trial months and bin months into years relative the trial dates. Finally, I cluster the standard errors at the defendant level which represents the treatment level. This is relevant given that a defendant might have multiple partners or siblings – in fact the average defendant has 3 working age siblings.

7.3. Main Results

Using the approach described in section 7.2 I estimate the dynamic spillover effects of incarceration on the sample of partners described in section 7.1. For this specification, I compare earning trajectories for the partners of those who were incarcerated against the partners of those who were not. Estimates

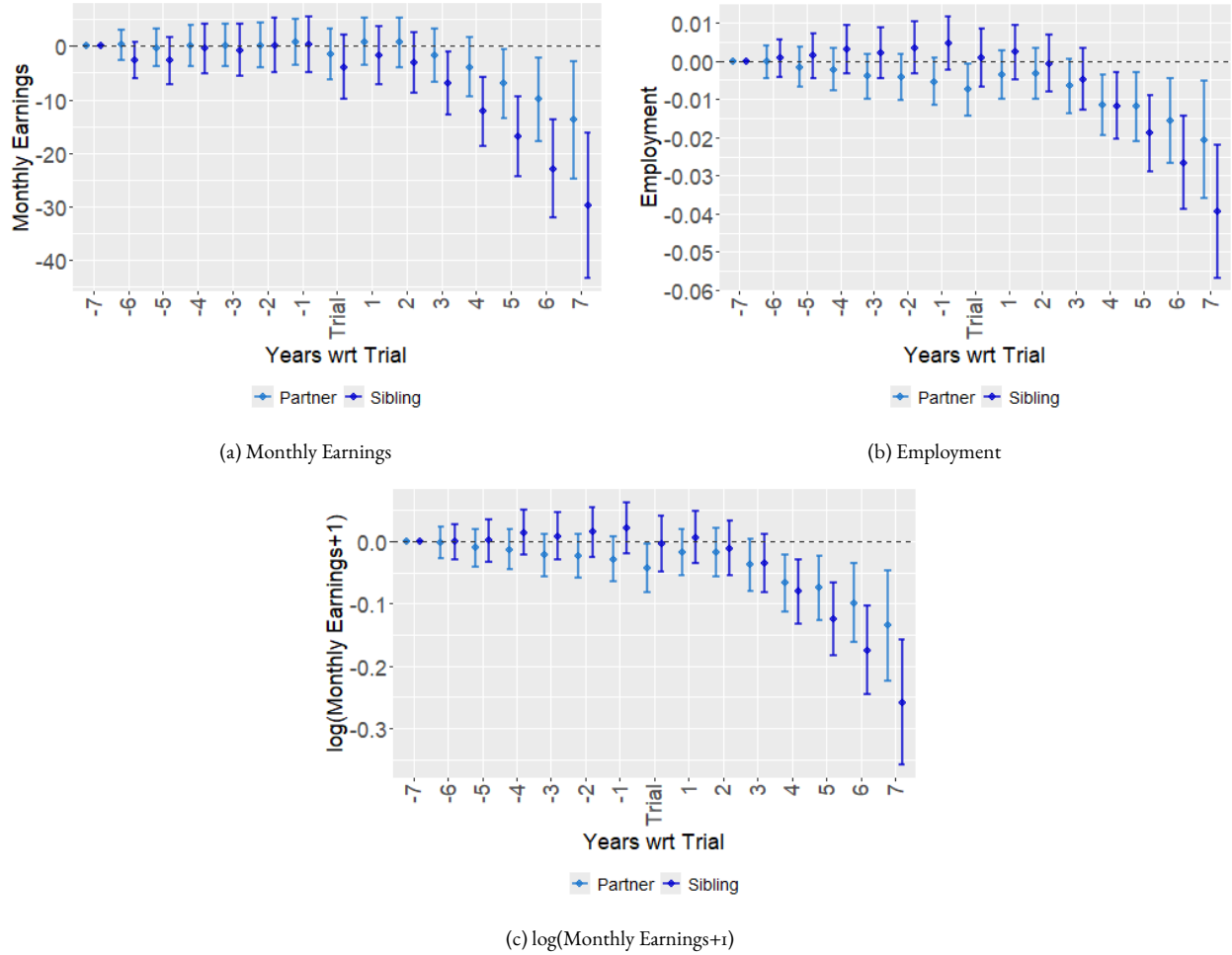
⁸I replicate the above process for parents and children, however, after restricting on age and labor market activity the sample becomes *too* small given that the average age for parents of defendants in my sample is 56 and for their children 14.

from this specification are plotted in figure 7. First, figure 7a plots the dynamic effects on the intensive margin: monthly earnings. I find that partners do not observe an immediate drop in earnings but rather a *gradual* decrease which appears to grow over time. Specifically, as described in panel A of table 4, after 4 years partners monthly earnings decrease by 7% (\$5/\$76), relative to the baseline mean. Rather than an immediate adjustment to compensate for the lost income, these drops evolve *gradually* and persist even after 7 years. This finding is consistent with progressive financial strain, reduced attachment to formal employment, and possible stigma or caregiving constraints within the family.

Second, I replicate the analysis now looking at the extensive margin, whether their partner is formally employed or not. Figure 7b plots estimates on the probability of employment. I find the same pattern. Their probability of employment drops gradually and that difference grows over time. Specifically, as shown in panel A of table 4, after 4 years their probability of employment decreases by 7% ($-0.011/0.1552$), relative to the baseline mean. This finding stands in contrast to prior evidence on the added worker effect which has documented modest increases in partners earnings or employment after large negative household level income shocks. Finally, given my research design, I observe no significant differences in earnings prior to trial which suggests that both groups were on similar trends.

I further estimate the effects on siblings. For this specification, I compare earning trajectories for the siblings of defendants who were incarcerated against the siblings of defendants who were not. Estimates from this specification are plotted in figure 7. First, figure 7a plots the dynamic effects on the intensive margin: monthly earnings. I find that siblings do not observe an immediate drop in earnings but rather a *gradual* decrease which appears to grow over time. Specifically, as described in panel B of table 4, after 4 years siblings' monthly earnings decrease by 10% (\$15/\$146), relative to the baseline mean. Second, I replicate the analysis now looking at the extensive margin, whether their siblings are formally employed or not. Figure 7b plots estimates on the probability of employment. I find the same pattern. Their probability of employment drops gradually and that difference grows over time. Specifically, as shown in panel B of table 4, after 4 years their probability of employment decreases by 5% ($-0.016/0.2922$), relative to the baseline mean. Finally, as with the analysis on partners, I observe no significant differences in earnings prior to trial.

Figure 7: Estimated Effect of Incarceration on Labor Outcomes: Spillovers



Notes: This figure plots estimates from equation 4 for monthly earnings (figure 7a), probability of employment (figure 7b), and logged earnings (figure 7c). The treated samples are all partners/siblings – as defined in section 7.1 – of those defendants who were incarcerated. The control samples are all partners/siblings of those defendants who got their case dismissed during pre-trial proceeding, or those who were handed a non-custodial sentence or found non-guilty. We normalize to the 7th year before the case filing date and we bin – for simplicity – all months relative to the trial dates into years. Errors are clustered at the defendant level. These estimates include individual and year-month fixed effects.

Table 4: Estimated Effect of Incarceration on Labor Outcomes: Spillovers

	log(Monthly Earnings+1) (1)	Monthly Earnings (2)	Annual Earnings (3)	Employment (4)
Panel A: Partners				
Pre-trial (6-2yrs)	-0.0138 (0.0147)	0.1214 (1.752)	5.955 (26.55)	-0.0023 (0.0025)
Pre-trial (1yr)	-0.0243 (0.0172)	0.9295 (2.064)	12.99 (29.76)	-0.0046 (0.0030)
Trial	-0.0376** (0.0188)	-1.298 (2.297)	9.368 (30.54)	-0.0066** (0.0032)
Post-trial (1-3yrs)	-0.0165 (0.0177)	0.4691 (2.145)	5.849 (31.39)	-0.0030 (0.0030)
Post-trial (4-7yrs)	-0.0675*** (0.0219)	-5.492** (2.704)	-74.73* (39.25)	-0.0110*** (0.0038)
Observations	21,159,648	21,159,648	1,763,304	21,159,648
Sample	220,413	220,413	220,413	220,413
R ²	0.70164	0.73915	0.81038	0.67023
Mean DV		\$76	\$854	0.1552
Panel B: Siblings				
Pre-trial (6-2yrs)	0.0088 (0.0167)	-1.174 (2.125)	-14.14 (32.28)	0.0023 (0.0029)
Pre-trial (1yr)	0.0225 (0.0194)	0.2399 (2.479)	10.37 (35.82)	0.0048 (0.0033)
Trial	-0.0032 (0.0215)	-4.020 (2.938)	-10.10 (36.87)	0.0009 (0.0037)
Post-trial (1-3yrs)	-0.0067 (0.0201)	-3.361 (2.596)	-50.31 (37.69)	0.0001 (0.0034)
Post-trial (4-7yrs)	-0.1078*** (0.0248)	-15.42*** (3.211)	-203.0*** (46.56)	-0.0160*** (0.0043)
Observations	30,671,520	30,671,520	2,555,960	30,671,520
Sample	319,495	319,495	319,495	319,495
R ²	0.57594	0.69123	0.77676	0.53361
Mean DV		\$146	\$1,660	0.2922

Notes: This table represents estimates of separate regressions on logged monthly earnings, monthly earnings, annual earnings – collapsed panel – and monthly employment. Mean values for each dependent variable represent the average for the treated sample during their baseline period: 7 years before trial. Errors are clustered – reported in parenthesis – at the defendant level.

These estimates include individual and year-month fixed effects. Significance codes: ***: 0.01, **: 0.05, *: 0.1

8. Conclusion

This paper examines how incarceration shapes the labor market trajectories of both the directly affected individuals and their family members. Using unique administrative data from Ecuador combining individual-level criminal justice records, family networks, and social security data, I study how incarceration alters labor earnings and employment over time for the universe of defendants passing through the criminal courts between 2010 and 2017. Overall, I find *large and persistent* negative effects. For those who are incarcerated, monthly earnings and employment probabilities fall by approximately 38% and 44%,

respectively, and these effects do not dissipate even seven years after sentencing. These long-term effects are not entirely driven by incapacitation and stand in contrast with recent evidence from higher-income countries

Additionally, I extend the analysis to family members and find that among working-age partners, both monthly earnings and employment probabilities decline by around 7% starting four years after sentencing. Siblings of the incarcerated experience similar reductions. Rather than an immediate adjustment, these drops evolve *gradually*. This is consistent with progressive financial strain, reduced attachment to formal employment, and possible stigma or caregiving constraints within the family. Taken together, these findings imply that incarceration is a household-level shock and criminal justice policy designs should incorporate these dynamics into account. In other words, the true labor market effects of incarceration is significantly larger than what is captured by estimates focusing solely on the incarcerated population.

The contributions of this paper are threefold. First, it expands the empirical evidence on the labor market effects of incarceration to a setting representative of developing countries. Second, the paper introduces a unique, large-scale administrative dataset linking the universe of criminal court defendants to their family members and to high-frequency labor market data. This linkage allows for a comprehensive view of how incarceration affects not only the defendants' labor market outcomes but those connected to them through family ties. Third, the paper contributes to a broader understanding of how households adjust to adverse income shocks. By documenting that incarceration leads to long-term declines in labor market participation among partners – contrary to the added worker effect often observed in response to other shocks such as unemployment or spousal death – these findings suggest that the mechanisms underlying household responses to incarceration are fundamentally different. Incarceration introduces constraints which can suppress rather than amplify labor supply responses within the household.

Several avenues for future research emerge from this analysis. First, understanding the mechanisms driving the spillover effects remains an open question. Future work could explore whether the observed declines in partner and sibling labor outcomes arise primarily through caregiving demands, reduced job search, loss of social networks, or stigma associated with having a family member incarcerated. Second, linking this dataset with additional administrative sources – such as household composition, childcare usage, or informal sector activity – could help understand how families reorganize their time allocation and income sources following incarceration. Third, examining intergenerational outcomes would help establish whether these negative effects persist into the next generation, affecting children's education, health, and future labor market attachment. Finally, policy evaluations could test whether targeted interventions – such as family support programs, prison-to-work transitions, or financial assistance during incarceration – attenuate these long-term effects. Recognizing and addressing these broader effects is critical for designing criminal justice and social policies that mitigate, rather than reinforce, the cycle of

disadvantage associated with incarceration.

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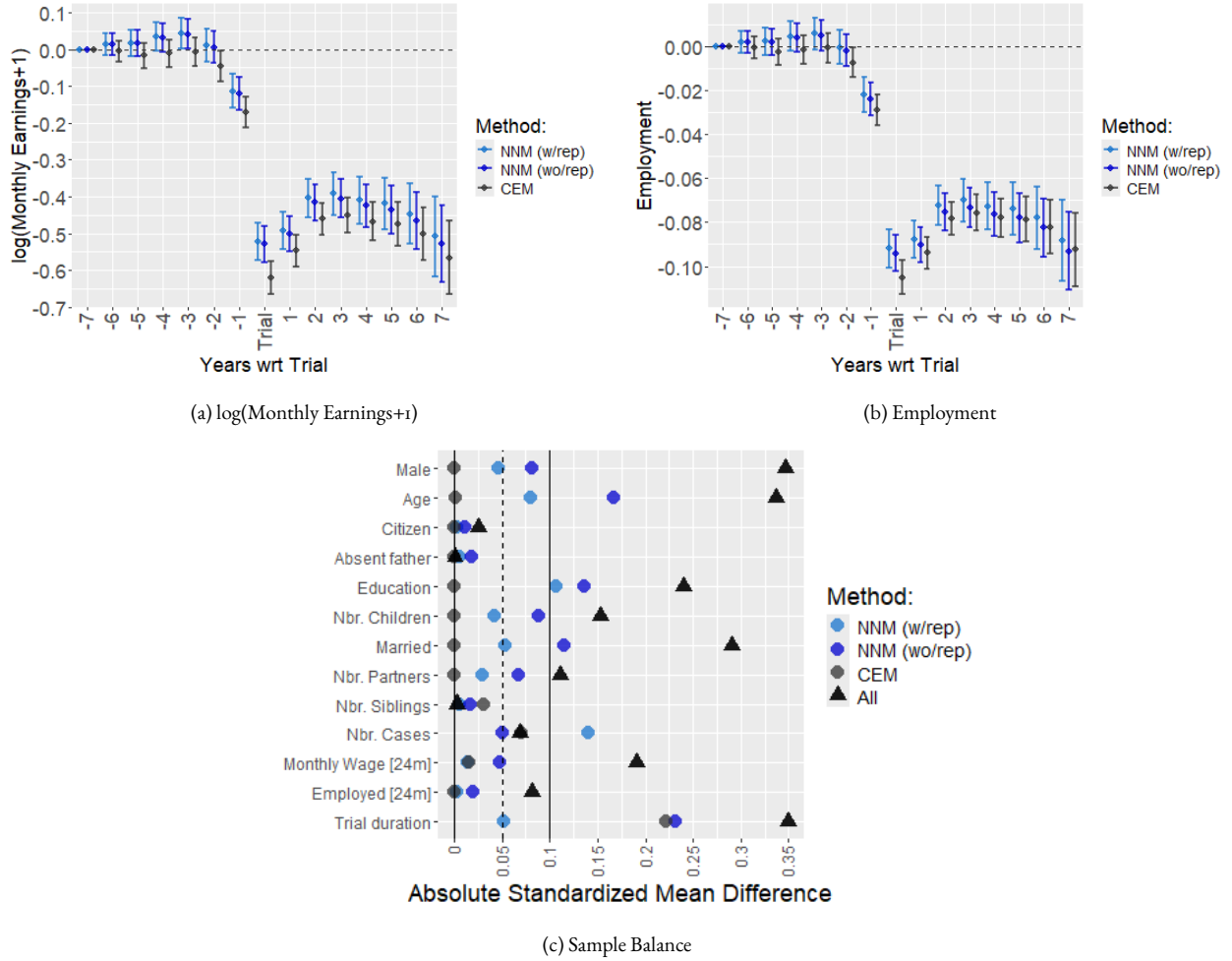
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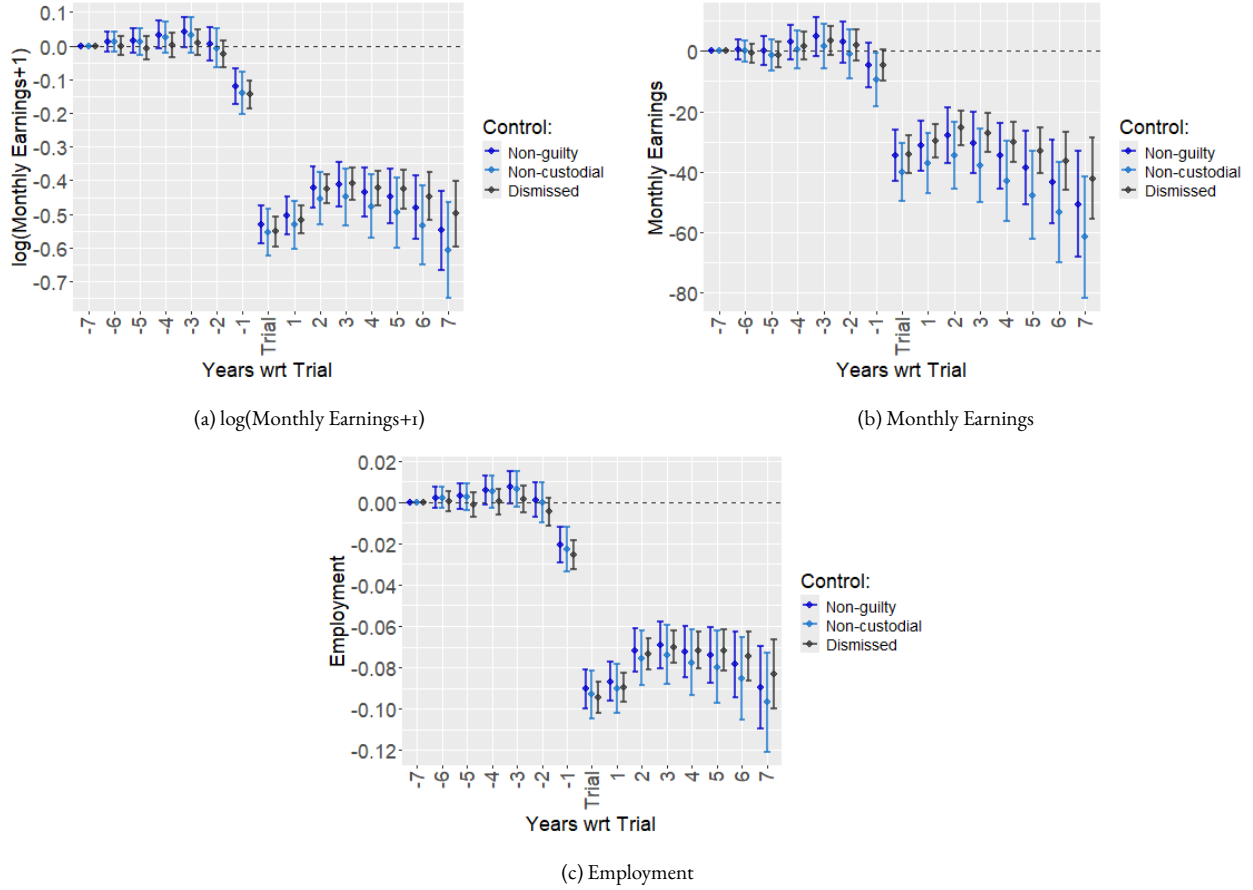
10. Appendix

Figure 8: Estimated Effect of Incarceration on Labor Outcomes by Matching Method: Defendant



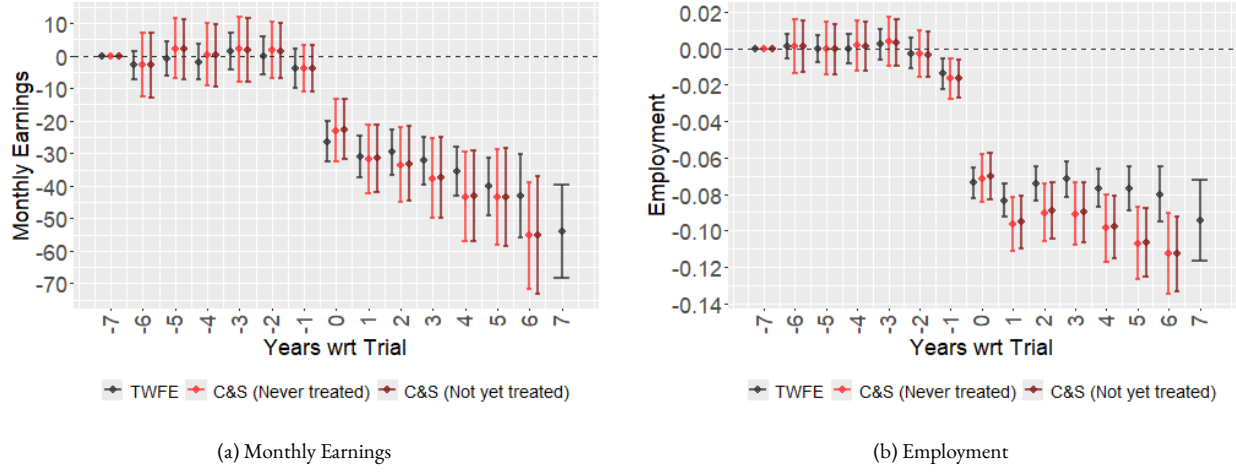
Notes: This figure plots estimates from equation 3 for logged earnings (figure 8a) and probability of employment (figure 8b) for each separate matching method: nearest neighbor matching (NNM) with and without replacement, and coarsened exact matching (CEM). Figure 8c plots the standardized mean difference for the covariates used in the matching process. The following were estimated by the trial date: age, number of children, partners, siblings, and cases. Number of cases refers to the number of times they were listed as a defendant in a criminal case. Wages and employment are estimated as the average values during the 24 months preceding the trial date. The treated sample are defendants – as defined in section 5 – who were incarcerated. The control samples are defendants who: got their case dismissed during pre-trial proceeding, were handed a non-custodial sentence, or found non-guilty. We normalize to the 7th year before the case filing date and we bin – for simplicity – all months relative to the trial dates into years. Errors are clustered at the individual level. These estimates include individual and year-month fixed effects.

Figure 9: Estimated Effect of Incarceration on Labor Outcomes by Separate Control Group: Defendant



Notes: This figure plots estimates from equation 3 for monthly earnings (figure 9b), probability of employment (figure 9c), and logged earnings (figure 9a) by each separate control sample. The treated sample are all defendants – as defined in section 5 – who were incarcerated. The control samples are (i) **Dismissed**: defendants who got their case dismissed during pre-trial proceeding, (ii) **Non-custodial**: those who were handed a non-custodial sentence, or (iii) **Non-guilty**: those found non-guilty. We normalize to the 7th year before the case filing date and we bin – for simplicity – all months relative to the trial dates into years. Errors are clustered at the individual level. These estimates include individual and year-month fixed effects.

Figure 10: Estimated Effect of Incarceration on Labor Outcomes: Alternative Estimators



Notes: This figure plots estimates following [Callaway and Sant'Anna \(2021\)](#) and compares them to our estimates from equation 3. I show results using (i) only the sample of never treated as controls, and also results (ii) incorporating those not-yet treated as controls. For computational reasons I collapse the panel at the individual-year level. Figure [10a](#) shows estimates on average monthly earnings and figure [10b](#) shows estimates on probability of employment. The treated sample are all defendants – as defined in section 5 – who were incarcerated. The control sample are **all** defendants who: got their case dismissed during pre-trial proceeding, were handed a non-custodial sentence, or found non-guilty. We normalize to the 7th year before the case filing date and we bin – for simplicity – all months relative to the trial dates into years. Errors are clustered at the individual level. These estimates include individual and year fixed effects.