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**Benjamin MONNERY**

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**Prison, reentry and recidivism :**

*micro-econometric applications /*

**Prison, réinsertion et récidive :**

*Application micro-économiques*

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**Benjamin MONNERY**

*le 29 novembre 2016*

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**PRISON, RÉINSERTION ET RÉCIDIVE :  
APPLICATIONS MICRO-ÉCONOMÉTRIQUES**

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# Table des matières

<b>Remerciements</b>	<b>v</b>
<b>Introduction générale</b>	<b>1</b>
0.1 Contexte et enjeux . . . . .	1
0.1.1 Les prisons françaises : surpeuplées, coûteuses, et inefficaces ? . . . . .	1
0.1.2 Qui est en prison ? Un rapide portrait des détenus et de leurs parcours . . . . .	4
0.1.3 Le poids de la récidive dans le système pénal et son coût social . . . . .	5
0.1.4 Les nombreuses tentatives de réformes pénales . . . . .	7
0.1.5 L'opinion publique face à la prison et à la récidive : une enquête dans la population lyonnaise . . . . .	9
0.2 Politiques publiques et choix délinquants : l'apport de la théorie économique . . . . .	13
0.2.1 Fonctions et efficacité des peines . . . . .	13
0.2.2 Le cadre théorique du choix rationnel . . . . .	14
0.2.3 Efficacité et efficacité-coût . . . . .	18
0.2.4 L'efficience sociale des politiques publiques . . . . .	19
0.3 Prison et récidive : une brève revue de la recherche empirique . . . . .	21
0.3.1 Avant la peine : le poids des anticipations et des délais . . . . .	22
0.3.2 Pendant la peine : ouvrir la boîte noire de la prison . . . . .	26
0.3.3 Au-delà de la peine : réinsérer, contrôler, et faire changer . . . . .	31
0.4 Résumé de la thèse et de ses implications . . . . .	36
0.4.1 Une thèse en économie du crime . . . . .	36
0.4.2 Contributions et principaux résultats . . . . .	37
<b>Bibliographie</b>	<b>43</b>

<b>1</b>	<b>The Determinants of Recidivism among Ex-Prisoners : a Survival Analysis on French Data</b>	<b>51</b>
1.1	Introduction . . . . .	52
1.2	The determinants of recidivism : survey of the literature . . . . .	53
1.3	Data . . . . .	56
1.3.1	Descriptive Statistics and Non-Parametric Evidence . . . . .	57
1.4	Model specification . . . . .	60
1.5	Results . . . . .	62
1.6	Conclusion . . . . .	66
	<b>Bibliographie</b>	<b>71</b>
<b>2</b>	<b>Time To Get Out ? How Sentence Reductions Affect Recidivism After Prison</b>	<b>75</b>
2.1	Introduction . . . . .	76
2.2	Theory and current evidence . . . . .	78
2.2.1	A theory of expectations and adaptation to surprise . . . . .	80
2.3	Testing the role of anticipations and adaptation to surprise . . . . .	83
2.3.1	Data . . . . .	84
2.4	Empirical strategy : exploiting the French collective pardon of 1996 . . . . .	86
2.5	Results . . . . .	89
2.5.1	Benchmark results . . . . .	89
2.5.2	IV results : overall effects . . . . .	89
2.5.3	IV results : mediating effects of anticipation and adaptation . . . . .	91
2.5.4	Mechanism : psychology or capital? . . . . .	93
2.6	Conclusion . . . . .	97
	<b>Bibliographie</b>	<b>99</b>
<b>3</b>	<b>Better at Home than in Prison ? The Effects of Electronic Monitoring on Recidivism in France</b>	<b>107</b>
3.1	Introduction . . . . .	108
3.2	Related Literature . . . . .	110
3.3	Institutional Context and Data . . . . .	113
3.3.1	An experiment (2000-2001) followed by a gradual rollout . . . . .	113

3.3.2	Selecting offenders for EM . . . . .	114
3.3.3	Data and Descriptive Statistics . . . . .	116
3.4	Empirical Strategy and Specification . . . . .	119
3.4.1	Exploring the gradual rollout of EM in France . . . . .	119
3.4.2	Econometric Specification . . . . .	120
3.4.3	Testing for the endogenous selection of EM by courts . . . . .	121
3.5	Results . . . . .	124
3.5.1	Benchmark estimates . . . . .	124
3.5.2	Causal estimates by IV . . . . .	125
3.5.3	Robustness Checks . . . . .	128
3.5.4	Why/When is EM effective? . . . . .	129
3.6	Discussion . . . . .	132
3.6.1	Incapacitation, deterrence, or genuine long-term change? . . . . .	132
3.6.2	Current validity : diminishing returns and changes in supervision? . . . . .	133
3.7	Conclusion . . . . .	135
	<b>Bibliographie</b>	<b>139</b>
<b>4</b>	<b>The Cost of Slow Justice : How Delayed Incarceration Fuels Recidivism</b>	<b>153</b>
4.1	Introduction . . . . .	154
4.2	Related research . . . . .	155
4.3	Institutional Context and Data . . . . .	157
4.3.1	The French penal procedure . . . . .	157
4.3.2	Dataset . . . . .	159
4.4	Empirical strategies . . . . .	160
4.4.1	Discontinuity at age 18 . . . . .	161
4.4.2	Court size . . . . .	165
4.5	Results . . . . .	167
4.5.1	Using the discontinuity at age 18 . . . . .	167
4.5.2	Using court size . . . . .	169
4.6	Conclusion . . . . .	171
	<b>Bibliographie</b>	<b>173</b>

<b>Conclusion générale</b>	<b>177</b>
4.6.1 Implications pour les pratiques et les politiques pénales . . . . .	177
4.6.2 Implications pour la recherche future . . . . .	182

# Table des figures

1	Evolution du nombre de détenus en France . . . . .	2
2	Taux de récidive cumulé (sur 5 ans) en France et aux Etats-Unis . . . . .	4
3	Contribution des anciens détenus au volume des condamnations futures . . . . .	6
4	Distribution des notes obtenues au test de connaissances . . . . .	10
5	L’opinion publique sur l’efficacité de la prison . . . . .	12
6	L’opinion publique sur les alternatives à l’incarcération . . . . .	12
7	L’opinion publique sur l’efficacité du bracelet électronique . . . . .	12
8	Le niveau d’efficience de la délinquance . . . . .	21
9	Une même trajectoire délinquante, trois délais d’incarcération différents . . . . .	24
1.1	Smoothed (non-parametric) hazard function . . . . .	59
1.2	Survival functions by gender, age at release and type of initial offense . . . . .	60
2.1	Distribution of Sentence Reductions Before/After Winsorizing . . . . .	102
2.2	Distribution of MCA score of capital . . . . .	102
3.1	Recidivism over time . . . . .	118
3.2	Number of EM granted in Pilot courts . . . . .	119
3.3	Crime Rates . . . . .	123
3.4	The widespread development of EM in France . . . . .	134
3.5	Observable characteristics of EM offenders . . . . .	135
3.6	Supervision and Incidents under EM . . . . .	136
3.7	Map of EM roll-in in French courts (2000-2003) . . . . .	143
3.8	Prison overcrowding rates on January 1 <sup>st</sup> . . . . .	148
3.9	Rates of Property Crime (left) and Violent Crime (right) . . . . .	148
3.10	Density of Date of Release . . . . .	149

3.11	Evolution of Aggregate Recidivism as Courts Adopt EM . . . . .	150
4.1	Cumulative Density of Delay by type of procedure . . . . .	160
4.2	Fast-Track Procedures by age at offense . . . . .	163
4.3	Recidivism and Return to Prison in the next 5 years after release . . . . .	164
4.4	Fast-Track Procedures and Court Size . . . . .	166

# Liste des tableaux

1	Notes moyennes obtenues (sur 14) par différentes catégories de répondants . . .	11
2	Nouvelles infractions avant incarcération . . . . .	25
1.1	Descriptive statistics (n = 2204) . . . . .	58
1.2	Cox Models With Time-Varying Effects . . . . .	64
1.3	Proportional-Hazards Tests based on the Schoenfeld residuals . . . . .	68
1.4	Cox Models With Time-Varying Effects . . . . .	69
1.5	Cox Models With Two Alternative Specifications Of Time Dependency . . . . .	70
2.1	Sentence length and time served . . . . .	86
2.2	Recidivism Variables . . . . .	87
2.3	Benchmark Estimates on 5-year Recidivism : Full Sample . . . . .	90
2.4	IV Estimates on 5-year Recidivism : Full Sample . . . . .	91
2.5	IV Estimates on 5-year Recidivism, by Anticipation and Adaptation . . . . .	93
2.6	IV Estimates on 5-year Recidivism, by Anticipation and Adaptation . . . . .	94
2.7	IV Estimates on 5-year Recidivism, by Adaptation and Age . . . . .	95
2.8	IV Estimates on 5-year Recidivism, by Initial Offense Type . . . . .	95
2.9	IV Estimates on 5-year Recidivism, by Score of Capital at Entry . . . . .	96
2.10	IV Estimates on 5-year Property Crime, by Anticipation and Adapation . . . . .	97
2.11	IV Estimates on 5-year Recidivism, by Adaptation and Age (Interaction) . . . . .	103
2.12	IV Estimates on 5-year Recidivism, by Anticipation and Adaptation . . . . .	103
2.13	IV Estimates on Different Crime Types after 5 years . . . . .	104
3.1	Electronic Monitoring and Recidivism . . . . .	125
3.2	Electronic Monitoring and Recidivism . . . . .	127
3.3	Heterogeneity of effects by profile and supervision . . . . .	130



3.4	Seriousness of new crime(s) over the Next 5 Years . . . . .	131
3.5	Causal effects in different time windows . . . . .	132
3.6	<b>Sociodemographic and judicial variables</b> . . . . .	145
3.7	Number of EM executed across courts by April 1 <sup>st</sup> 2003 . . . . .	146
3.8	<b>Differences in 5-Year Recidivism Before the Introduction of EM</b> . .	147
3.9	Robustness : Electronic Monitoring and Recidivism . . . . .	149
3.10	Crime-specific effects . . . . .	151
3.11	Marginal effectiveness . . . . .	151
4.1	Distribution of Delay in days . . . . .	159
4.2	Probit : Recidivism After 5 Years . . . . .	167
4.3	Bivariate Probit : Fast-Track and Recidivism After 5 Years . . . . .	168
4.4	Court size IV : Recidivism After 5 Years . . . . .	170
4.5	Robustness tests . . . . .	175
4.6	IV Estimates : Heterogeneity by Previous Criminal Experience . . . . .	175





# Introduction générale

## 0.1 Contexte et enjeux

### 0.1.1 Les prisons françaises : surpeuplées, coûteuses, et inefficaces ?

Les prisons françaises comptent 69 375 détenus au 1<sup>er</sup> juillet 2016, soit un record absolu dans l'histoire de l'Administration Pénitentiaire (le précédent record datait d'avril 2014 avec 68 859 détenus). Comme le montre le graphique 1, la population carcérale française croit fortement depuis 2001 avec près de 20 000 détenus supplémentaires en quinze ans. Dans le même temps, le parc pénitentiaire n'a augmenté que de 10 000 places, générant ainsi un problème chronique de forte surpopulation carcérale. Au 1<sup>er</sup> juillet 2016, la capacité opérationnelle des 188 établissements pénitentiaires est de 58 311 places, soit une densité moyenne de 119 détenus pour 100 places. Ce chiffre officiel minore d'ailleurs la gravité du problème, puisque quelque 3900 places de prison sont inoccupées pour cause de travaux ou par mesure de sécurité<sup>1</sup>. Ainsi, les prisons françaises comptent en réalité 69 375 détenus répartis sur 54 383 places occupées, soit 28% de détenus en surnombre (au 1<sup>er</sup> juillet 2016). En conséquence, 1 648 détenus dorment chaque nuit sur un matelas posé à même le sol (Ministère de la Justice, 2016). Cette forte surpopulation, particulièrement marquée dans les Maisons d'Arrêt et dans les établissements d'Outre-Mer, entraîne régulièrement la condamnation de l'Etat pour traitements inhumains et dégradants (107 condamnations en 2015, selon le Garde des Sceaux Jean-Jacques Urvoas) et génère de vraies difficultés au quotidien : promiscuité, tensions entre codétenus, insécurité, accès réduit aux activités en détention (travail, sport) et aux douches, aux parloirs, etc. D'après les chiffres de l'Administration Pénitentiaire, on a dénombré sur l'année 2014 près de 8 000 agressions entre détenus et 4 000 agressions physiques contre le personnel (dont 149 avec ITT), 680 mouvements collectifs, 12 prises d'otages, 94 suicides et

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1. Pour faciliter la gestion de la détention et améliorer les conditions de sécurité, de nombreux établissements pour peine comme les Maisons Centrales accueillent moins de détenus que leur capacité opérationnelle. De plus, certaines places sont inoccupées parce que l'offre est supérieure aux besoins locaux.

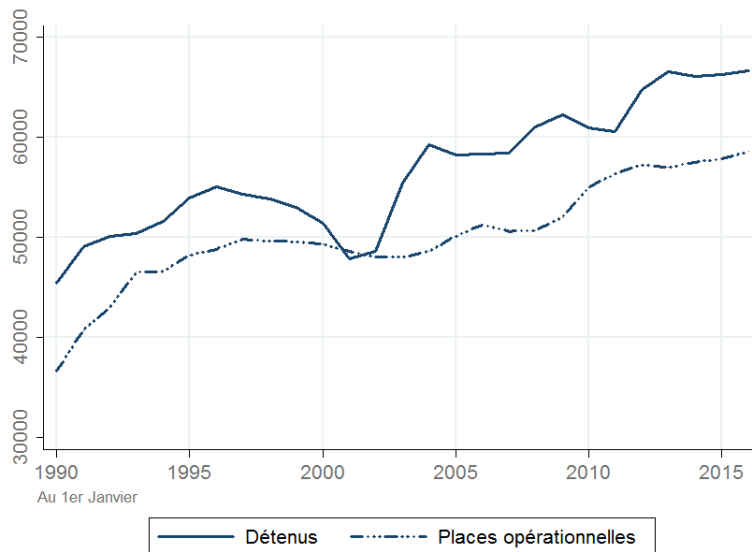


FIGURE 1: Evolution du nombre de détenus en France

1 homicide en détention.

Les prisons coûtent cher : elles mobilisent ainsi l'essentiel des ressources de l'Administration Pénitentiaire (AP) dont le budget annuel est de 2,64 milliards d'euros pour l'année 2015 (sur les 8 milliards du Ministère de la Justice). L'AP emploie environ 37 000 personnels dont 27 000 surveillants pénitentiaires. Le milieu fermé concentre à lui seul près de trois quarts des dépenses de l'AP (soit environ 2 milliards d'euros) pour héberger une moyenne de 68 000 détenus, alors que cette administration doit également assurer le contrôle de plus de 170 000 personnes en milieu ouvert. Le coût budgétaire de la détention avoisine les 30 000 euros par an et par détenu, un niveau comparable aux estimations fournies dans le rapport du sénateur Jean-René Lecerf (2015) (coût moyen journalier de 99,5 euros). Le coût de la détention varie du simple au double en fonction du type d'établissement : 90€ pour les Maisons d'Arrêt, 104€ pour les Centres Pénitentiaires, 109€ pour les Centres de Détention et 197€ pour les Maisons Centrales<sup>2</sup>. A l'inverse, les peines alternatives à l'incarcération et les aménagements de peine coûtent beaucoup moins cher : en 2013, le même rapport estime le coût moyen journalier à 12 euros pour la surveillance électronique fixe, 30 euros pour le placement à l'extérieur, et 50 euros pour la semi-liberté (Lecerf, 2015).

Enfin, l'efficacité des peines de prison à prévenir la récidive paraît toute relative, tant les

2. Ces estimations du coût de la journée de détention proviennent d'un calcul du Ministère de la Justice sur l'exercice 2014, en réponse à une question écrite du député Serge Coronado. Sont pris en compte les dépenses totales de chaque établissement ainsi que les coûts ventilables des administrations centrales (AC) et régionales (DISP). Ce montant est divisé par le nombre de journées de détention hébergées dans l'établissement sur l'année.

recondamnations sont nombreuses (Figure 2). Selon la dernière enquête disponible en France, portant sur un échantillon représentatif de 6907 détenus libérés au second semestre 2002, le taux de recondamnation pour une nouvelle affaire dans les cinq ans suivant la sortie est de 59% (Kensey et Benaouda, 2011). La grande majorité des infractions en récidive interviennent dès les deux premières années, et conduisent pour 80% à une nouvelle peine de prison ferme. Ces chiffres semblent relativement stables dans le temps<sup>3</sup> et similaires d'un pays à l'autre. Aux Etats-Unis par exemple, la dernière étude du *Bureau of Justice Statistics* (représentative de 30 Etats) évalue à 55% le taux de recondamnation dans les cinq ans suivant la sortie de prison, et à 77% le taux de nouvelle arrestation (Durose et al., 2014). En Angleterre et au Pays de Galle, le taux de recondamnation des sortants de prison de l'année 2013-2014 est de 45% dès la première année, chiffre stable depuis dix ans (Ministry of Justice, 2016). Ces taux de récidive élevés ont conduit de nombreux chercheurs à parler de la prison comme d'une *revolving door*<sup>4</sup>, puisqu'une marge de la population entre et sort souvent de prison et compte ainsi pour l'essentiel des flux (Freeman, 2003).

En outre, la récidive est un phénomène massif qui va au-delà des seuls sortants de prison. Parmi les 500 000 personnes condamnées pour un délit ou une contravention de 5<sup>e</sup> classe en 2004 en France (80% d'entre eux furent condamnés à une peine autre que la prison ferme, comme une amende ou une peine de sursis), environ 41% furent à nouveau condamnés dans les 5 années suivantes, et 45% après 8 ans (Josnin, 2014). Quel que soit le type de peine prononcée, les taux de récidive sont donc relativement élevés. Et parmi l'ensemble des condamnés, les détenus sont généralement les délinquants les plus à risque de récidive du fait de leurs antécédents judiciaires, de leurs profils socio-démographiques, et des infractions commises (voir ci-dessous). De plus, la prison est bien souvent utilisée en dernier recours par les magistrats français, après diverses tentatives de peines alternatives à l'incarcération (peines de sursis simple, de sursis avec mise à l'épreuve, de placement sous surveillance électronique, etc.). Il serait donc hasardeux de juger la prison inefficace, voire criminogène, au seul regard des statistiques de la récidive, plus élevées chez les condamnés à une peine ferme que chez les autres condamnés.

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3. Dans une précédente enquête représentative sur les sortants de prison de 1996-1997, l'Administration Pénitentiaire estime le taux de recondamnation dans les cinq ans à 52%.

4. *Porte tambour* en français.

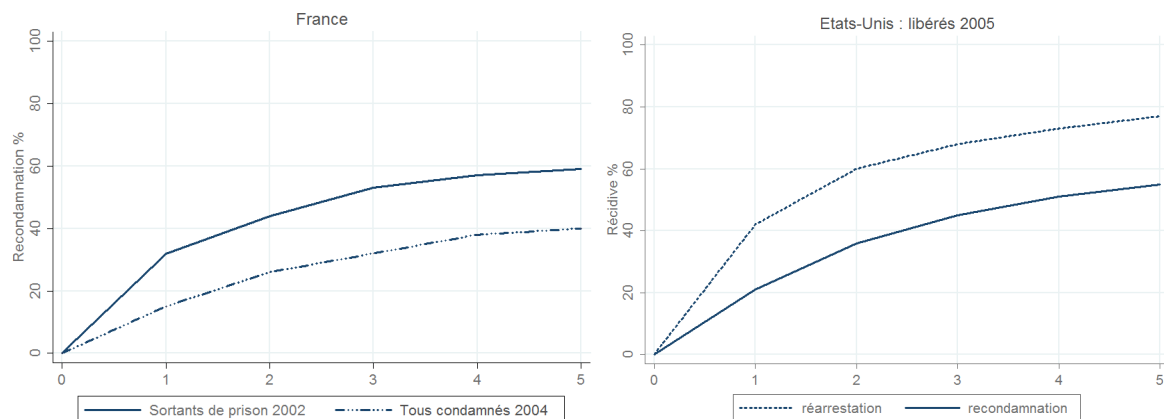


FIGURE 2: Taux de récidive cumulé (sur 5 ans) en France et aux Etats-Unis

### 0.1.2 Qui est en prison ? Un rapide portrait des détenus et de leurs parcours

Contrairement à certaines idées reçues, la grande majorité des détenus effectue des courtes peines : sur l'année 2014 par exemple, 31% des sortants avaient effectué 3 mois de détention ou moins, 78% un an ou moins, et seuls 2% étaient restés plus de 5 ans en prison. Ces durées de détention courtes soulignent la forte rotation de la population carcérale française, où le flux annuel d'entrées/sorties (environ 70 000) est très proche du stock moyen de détenus. En stock, les condamnés à des peines de 5 ans ou plus représentent cependant 23% du total des condamnés écroués.

Parmi les personnes détenues au 1<sup>er</sup> janvier 2014, la part des infractions les plus graves est relativement limitée : 9% sont incarcérés pour homicide volontaire, 13% pour viol ou agression sexuelle, 4% pour vols criminels. Les infractions les plus représentées sont les vols délictueux (19%), les infractions à la législation sur les stupéfiants (16%), les violences volontaires (13%), les recels-escroqueries (9%), puis d'autres types d'infractions (atteintes involontaires, infractions routières, police des étrangers, etc.). Seuls 17% des entrants en 2014 déclarent avoir déjà été incarcérés auparavant (les données d'enquêtes sur la cohorte des sortants 2002 suggèrent plutôt une part d'anciens détenus de l'ordre de 30%), mais la majorité ont accumulé d'autres condamnations dans leur casier judiciaire avant d'être incarcérés.

Au-delà de leur parcours judiciaire, les détenus constituent un groupe social marqué par la marginalité, les difficultés économiques, sociales et familiales, et les problèmes de santé et d'addictions : dans une récente étude menée sur 1 780 personnes entrant en détention en Picardie en 2013, Fauchille et al. (2016) montrent que 23% des nouveaux détenus faisaient l'objet d'un suivi psychiatrique avant l'incarcération, 7% ayant même été hospitalisés dans un service psychiatrique dans l'année précédente ; 24% consommaient régulièrement de la drogue

(principalement cannabis, mais aussi héroïne), 18% déclaraient une consommation excessive d'alcool, 85% étaient fumeurs de tabac ; la moitié nécessitait des soins dentaires. Seuls 5% des nouveaux détenus étaient propriétaires de leur logement, contre 12% en logement précaire ou sans domicile fixe et 40% hébergés par leur famille. Ces résultats corroborent d'autres enquêtes socio-sanitaires menées dans les prisons françaises (Falissard et al., 2006).

L'enquête américaine *National Longitudinal Survey of Youth* (NLSY 1997), qui a suivi de 1997 à 2010 une cohorte de jeunes adolescents âgés de 12 à 16 ans en 1996, permet également d'identifier le profil des jeunes américains qui furent ensuite incarcérés au cours de leur vie (jusqu'à l'âge de 30 ans environ). Raphael (2014) montre que la situation sociale, familiale, scolaire des enfants entre 12 et 16 ans est très fortement corrélée à leur risque d'être incarcérés avant 30 ans : 26% des futurs détenus vivaient en 1997 dans une famille pauvre, contre 11% chez les autres ; seulement 24% des futurs détenus vivaient avec leurs deux parents biologiques, contre 55% chez les autres. De plus, seuls 16% des futurs détenus déclaraient avoir de bonnes notes à l'école contre 49% de ceux qui ne furent jamais incarcérés par la suite. Beaucoup des futurs détenus n'ont pas fini le lycée, et la plupart d'entre eux avaient déjà fumé (65% contre 41%), bu de l'alcool (58% contre 46%), porté une arme à feu (32% contre 16%) et même vendu de la drogue (18% contre 8%) et commis diverses infractions.

### 0.1.3 Le poids de la récidive dans le système pénal et son coût social

Il est désormais bien connu que la Justice française -et en particulier la justice pénale- est engorgée, comme en témoigne le cas très médiatisé du TGI de Bobigny. Mais quelle est la contribution de la récidive des anciens détenus à cette situation ? Pour quantifier cette surcharge de travail pour les tribunaux français, on peut estimer la contribution d'une cohorte de libérés donnée à l'ensemble des condamnations prononcées en France dans l'année qui suit. Pour cela, on exploite les données de l'enquête représentative sur les 6907 détenus libérés au second semestre 2002 : dès la première année de libération, ces sortants de prison accumulent en moyenne 0,55 nouvelles condamnations toutes peines confondues, dont 0,42 nouvelles peines fermes (32% furent recondamnés, dont environ 80% à une nouvelle peine de prison ferme). Si l'on généralise ces estimations à l'ensemble des 69 382 détenus libérés en France durant l'année 2002, on en déduit que cette cohorte de sortants de prison a généré en un an un volume d'environ 38 200 nouvelles condamnations, dont 29 000 nouvelles peines fermes. En rapportant ces chiffres au volume total des condamnations prononcées par les



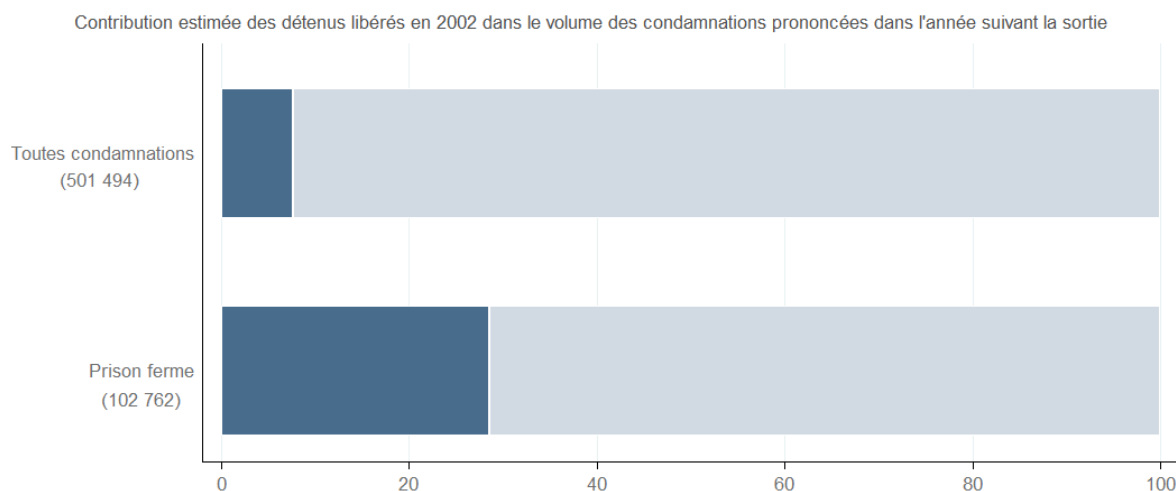


FIGURE 3: Contribution des anciens détenus au volume des condamnations futures

tribunaux français sur la période<sup>5</sup>, on estime que les détenus libérés en 2002 en France ont contribué pour 7,6% à l'ensemble des condamnations prononcées dans l'année qui a suivi. Ce chiffre est très proche des estimations de Rosenfeld et al. (2005) aux Etats-Unis pour la contribution des détenus libérés en 1994 sur le volume des arrestations dans l'année suivante (entre 4,5% et 6,5% selon l'infraction considérée). Enfin, si l'on se concentre sur les seules peines de prison ferme, la contribution des sortants de l'année 2002 dans le total des nouvelles peines fermes prononcées en France atteint 28%.

Ces estimations, synthétisées dans la Figure 3, montrent le poids de la récidive des anciens détenus dans le système pénal français : à elle seule, une cohorte de sortants d'une année donnée génère près de 30% des peines fermes prononcées dans l'année suivante. Rendre la prison et ses alternatives plus efficaces peut donc aboutir à une réduction rapide et importante de la charge des tribunaux (en particulier les tribunaux correctionnels), du nombre de détenus en France, et par là même à un fonctionnement plus satisfaisant du système judiciaire et pénitentiaire.

Un autre moyen de mesurer l'enjeu de la récidive consiste à estimer le coût social des infractions commises à la sortie. Une vaste littérature en criminologie tente ainsi d'évaluer le "coût du crime" : il peut s'agir d'estimations *ex-post* qui mesurent et additionnent tous les coûts occasionnés par une infraction, ou d'estimations *ex-ante* par évaluation contingente (mesure de la propension à payer des individus pour se prémunir du risque de victimation)

5. Sachant qu'on étudie la cohorte des détenus sortis durant l'année 2002 (du 1<sup>er</sup> janvier au 31 décembre), on utilise au dénominateur le nombre moyen de condamnations prononcées en France pour les années 2002 et 2003, soit  $(477\,935 + 525\,053)/2$ . La méthode de calcul est la même pour les seules condamnations à des peines fermes.

ou par la méthode des prix hédoniques (estimation de la valeur sociale de la délinquance à l'aide de l'observation des comportements de marché des agents économiques, comme les différences de prix de logements situés dans des quartiers plus ou moins sûrs). Le coût de la récidive peut donc être approché en affectant à chaque nouvelle infraction son coût social estimé dans la littérature. En se basant sur les récentes estimations de coûts de Cohen et al. (2010), Ostermann et Caplan (2016) évaluent ainsi qu'en moyenne, chaque détenu du New Jersey libéré entre 2005 et 2007 coûte entre \$41,000 et \$95,000 à la société dans les 3 ans qui suivent sa libération, du fait des nouvelles infractions commises (le taux de réarrestation étant de 56% dans cet échantillon). Ce type d'exercice permet de mesurer combien il peut être socialement bénéfique d'investir dans des programmes, même coûteux, de prévention de la récidive.

#### 0.1.4 Les nombreuses tentatives de réformes pénales

La lutte contre la récidive a fait l'objet de nombreuses réformes pénales en France. En moyenne sur les vingt dernières années, une nouvelle loi sur la récidive est votée tous les deux ans environ. Ces réformes pénales ont rarement été guidées par la science ou par les expériences étrangères, à l'exception notable de la loi d'août 2014 qui fut précédée d'une Conférence du Consensus réunissant de nombreux chercheurs. Parmi les principales réformes des vingt dernières années, on peut citer :

- Décembre 1997 : loi consacrant le placement sous surveillance électronique comme modalité d'exécution des peines privatives de liberté, qui instaure le bracelet électronique
- Juin 1998 : loi relative à la prévention et à la répression des infractions sexuelles ainsi qu'à la protection des mineurs, qui consacre le suivi socio-judiciaire comme moyen de prévention de la récidive
- Septembre 2002 : loi d'orientation et de programmation de la justice (dite loi Perben I), instaurant les Centres Educatifs Fermés et les Etablissements Pénitentiaires pour Mineurs, et élargissant l'usage du bracelet électronique
- Juin 2003 : loi renforçant la lutte contre les violences routières, qui durcit les sanctions en cas de récidive
- Mars 2004 : loi portant adaptation de la justice aux évolutions de la criminalité (dite loi Perben II)
- Décembre 2005 : loi relative au traitement de la récidive des infractions pénales, qui

instaure le placement sous surveillance électronique mobile, allonge les peines de sûreté et élargit le champ de la récidive légale

- Août 2007 : loi relative à la récidive des majeurs et des mineurs (dite loi Dati), instaurant des peines plancher en récidive, durcissant les sanctions contre les mineurs récidivistes et créant l'injonction de soins
- Février 2008 : loi relative à la rétention de sûreté et à la déclaration d'irresponsabilité pénale pour cause de trouble mental, qui instaure la rétention de sûreté au-delà de la peine ferme pour les criminels jugés dangereux
- Novembre 2009 : loi pénitentiaire, qui promeut les aménagements de peine avant et pendant l'incarcération, notamment sous bracelet électronique
- Mars 2010 : loi tendant à amoindrir le risque de récidive criminelle, précisant les règles relatives à la rétention de sûreté
- Août 2014 : loi relative à l'individualisation des peines et renforçant l'efficacité des sanctions pénales (dite loi Taubira), qui instaure la contrainte pénale, promeut la libération sous contrainte, et abroge les peines plancher

Depuis 2009 et la loi pénitentiaire, une certaine cohérence se dégage dans l'orientation des réformes françaises : dans l'espoir de désengorger les prisons et de mieux prévenir la récidive, le législateur tente de promouvoir les aménagements de peines pour éviter le plus souvent possible la prison (même lorsque la juridiction de jugement a prononcé une peine ferme) ou raccourcir la durée d'incarcération. Au-delà du placement à l'extérieur, de la semi-liberté et de la libération conditionnelle (les trois aménagements historiques en France), la surveillance électronique est rapidement devenue la principale alternative à l'incarcération dans la loi comme dans la pratique (en particulier de la part des Juges d'Application des Peines).

Malgré les réformes et cette évolution des pratiques, les résultats en matière de surpopulation carcérale ne sont clairement pas au rendez-vous, comme on l'a vu sur la Figure 1 : bien que le nombre annuel d'entrées en prison ait baissé ces dix dernières années (environ 70 000 entrées en 2014 contre 83 000 en 2007) au profit des placements sous surveillance électronique (22 000 en 2014 contre 8 000 en 2007), la durée moyenne de détention s'est fortement allongée (passant de 8,6 mois en 2007 à environ 12 mois aujourd'hui), générant une augmentation nette du stock des détenus. Et en termes de prévention de la récidive, nul n'est capable d'évaluer l'effet de la plupart des réformes récentes compte tenu du manque de données accessibles aux chercheurs<sup>6</sup>.

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6. En matière d'évaluation ex-post, les chercheurs français doivent se résigner à exploiter des données

### 0.1.5 L'opinion publique face à la prison et à la récidive : une enquête dans la population lyonnaise

Les changements politiques opèrent rarement sans un certain soutien de l'opinion publique. Or, il existe peu d'enquêtes d'opinions récentes en France sur la justice pénale. Les rares enquêtes existantes, menées par la Direction de l'Administration Pénitentiaire<sup>7</sup>, interrogent les Français sur leurs connaissances et leurs opinions relatives à la prison. La dernière enquête de ce type date cependant de l'année 2009.

Afin d'actualiser ce travail et de répondre à de nouvelles questions en lien avec cette thèse, une enquête a été menée avec l'aide d'une équipe d'étudiants de l'Université Lumière Lyon 2<sup>8</sup>. Cette enquête visait à tester le niveau de connaissances des répondants, et à les interroger sur leurs opinions quant à la prison mais aussi quant au placement sous surveillance électronique. L'enquête en face-à-face, menée en Avril 2016 dans différents arrondissements de Lyon et dans quelques communes proches (Bron, Villeurbanne, Vaulx-en-Velin), a permis de constituer un échantillon de 507 répondants âgés de 15 et plus. Après redressement sur les variables de sexe, de classe d'âge et de catégorie socio-professionnelle, cet échantillon est représentatif de la population du Grand Lyon<sup>9</sup>.

#### Une opinion publique qui connaît mal la prison

La première partie de l'enquête consistait en un rapide test de connaissances de 7 questions. Celles-ci portaient sur la population carcérale (nombre de détenus actuel et tendance depuis 10 ans, durée de détention moyenne, part des femmes, part des mineurs), le taux de récidive parmi les sortants de prison, les aménagements de peine existants, et le nombre actuel de personnes sous bracelet électronique. Ces 7 questions ont permis de construire une note individuelle allant de 0 à 14, en allouant 0, 1 ou 2 points à chaque question (en fonction du niveau de précision de la réponse<sup>10</sup>).

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relativement anciennes, telles que celles sur les premiers placés sous surveillance électronique en France, entre 2000 et 2003 (Chapitre 3). L'évaluation française la plus récente à ma connaissance concerne l'effet dissuasif des peines plancher en 2007 par Philippe (2015).

7. Les enquêtes de la DAP sur "Les Français et la prison" en 2007 et 2009 font figure d'exception : [http://www.justice.gouv.fr/art\\_pix/Infostat\\_122.pdf](http://www.justice.gouv.fr/art_pix/Infostat_122.pdf)

8. L'équipe était composée de 5 étudiants de la Licence d'Econométrie, 2 étudiantes du Master 1 Economie Quantitative, et d'une quinzaine d'étudiants de L1 Economie-Gestion. Qu'ils en soient tous remerciés ici, ainsi que Martin Fournier (GATE - Lyon 2) en charge du cours de Méthodes d'enquêtes.

9. Les informations relatives à la population-cible proviennent du dernier recensement (2012) de l'INSEE pour la communauté urbaine du Grand Lyon.

10. Par exemple, à la question "Selon vous, sur 100 détenus, combien sont à nouveau condamnés dans les 5 ans suivant leur libération ?" (réponse = 60%), les personnes ayant répondu entre 55 et 65 reçoivent 2 points, les personnes ayant répondu entre [50 ;55] et [65 ;70] reçoivent 1 point, et les autres (y compris les non-réponses)

La distribution des notes obtenues par les 507 répondants apparaît dans la Figure 4. La note moyenne est de 3,9/14, et seuls 12,8% des répondants obtiennent une note supérieure ou égale à 7/14. A titre d'exemple, seuls 23% connaissent le nombre actuel de détenus en France à  $\pm 40000$  près. De même, seuls 27% donnent une bonne estimation, à  $\pm 5000$  près, du nombre actuel de bracelets électroniques (bonne réponse : 10000). Cependant, en ce qui concerne la récidive après la prison, 40% des répondants ont une bonne idée du taux de recondamnation dans les 5 ans, en donnant une réponse entre 50% et 70% (bonne réponse : 60%). Globalement, l'enquête révèle un niveau de connaissances très limité de la part du grand public lyonnais sur la prison. Ces mauvais résultats confirment ceux obtenus par les enquêtes nationales existantes, portant sur de plus gros échantillons et sur plus de questions (Belmokhtar et Benzakri, 2013).

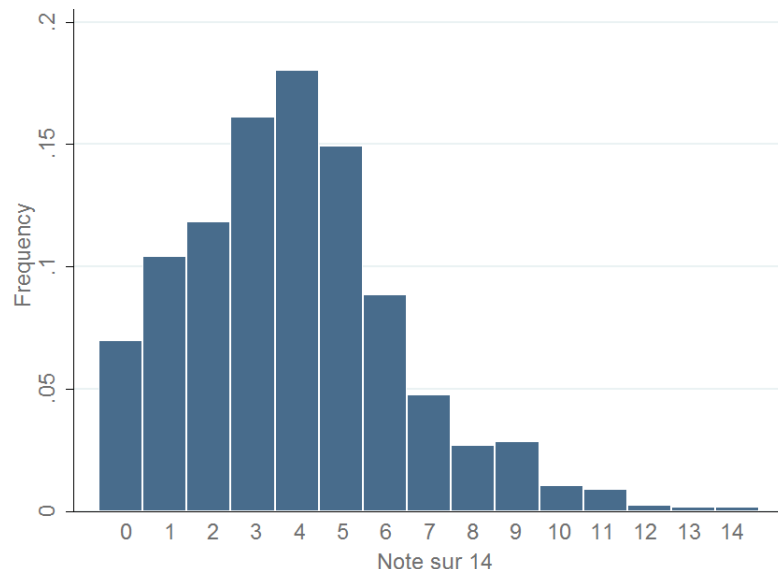


FIGURE 4: Distribution des notes obtenues au test de connaissances

Sans surprise, le niveau de connaissances est sensiblement plus élevé chez les répondants ayant déjà été confronté à la prison ou au bracelet électronique, soit personnellement soit par le biais d'un proche (Table 1 en Annexe). A l'inverse, les notes obtenues ne diffèrent pas significativement selon l'orientation politique des répondants, ou selon leur niveau de diplôme. On constate cependant que le niveau de connaissances est maximal dans la tranche d'âge 35-45 ans, et au contraire plus faible chez les jeunes (15-25 ans) et chez les plus de 55 ans.

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reçoivent 0 point.

TABLE 1: Notes moyennes obtenues (sur 14) par différentes catégories de répondants

A. Déjà confontré à la prison ou au bracelet ?	Moyenne	B. Orientation politique	Moyenne
Non (N=361)	3.7	Droite (N=167)	4.0
Oui par un proche (N=125)	4.3	Gauche (N=230)	3.9
Oui personnellement (N=11)	6.1	Ne souhaite pas répondre (N=110)	3.7
Ne souhaite pas répondre (N=10)	5.6		

C. Niveau de diplôme	Moyenne	D. Tranche d'âge	Moyenne
Inférieur au Bac (N=74)	3.8	< 25 ans (N=84)	3.8
Niveau Bac (N=127)	3.9	[25 ;35[ (N=118)	4.0
Niveau Bac+2 (N=63)	4.4	[35 ;45[ (N=75)	4.5
Niveau Bac+3 (N=111)	3.9	[45 ;55[ (N=87)	4.1
Niveau Bac+5 (N=72)	4.2	[55 ;65[ (N=66)	3.7
Supérieur à Bac+5 (N=8)	3.7	≥ 65 (N=77)	3.5

Une opinion publique dubitative quant à l'efficacité de la prison, et plus optimiste vis-à-vis des alternatives

La seconde partie de l'enquête concernait l'opinion des répondants sur la prison et les peines alternatives. Les répondants se montrent très réservés quant à l'efficacité de l'incarcération à réduire le risque de récidive des délinquants (Figure 5) : plus de 75% des répondants considèrent ainsi que la prison ne réduit pas la récidive, et 40% pensent même qu'elle augmente les risques de récidive. Cette opinion critique est partagée aussi bien à droite qu'à gauche.

A l'inverse, les répondants ont une opinion favorable des alternatives à l'incarcération, et en particulier du placement sous surveillance électronique (Figures 6 et 7). Ainsi, environ deux tiers des répondants déclarent avoir un avis globalement favorable sur les peines alternatives (80% chez les personnes de gauche), et 60% pensent que le bracelet électronique est un moyen efficace pour lutter contre la récidive. Cependant, un gros quart des répondants se montre tout de même critique, considérant que le bracelet électronique est une marque de faiblesse de la justice (28%) et que les délinquants placés sous surveillance électronique ne respectent pas leurs obligations (27%). Malgré ces réserves, l'opinion publique lyonnaise reste en moyenne favorable aux alternatives à l'incarcération telles que le bracelet électronique, et y voit un meilleur moyen de prévenir la récidive que la prison.

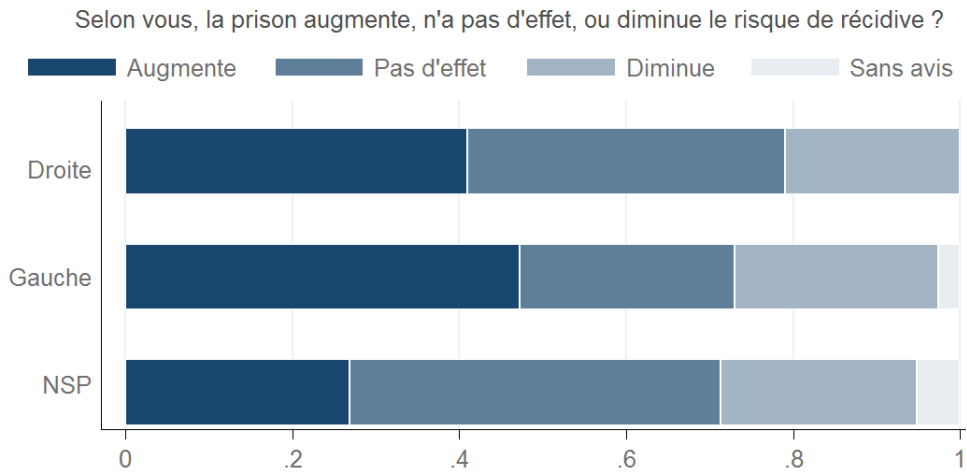


FIGURE 5: L'opinion publique sur l'efficacité de la prison

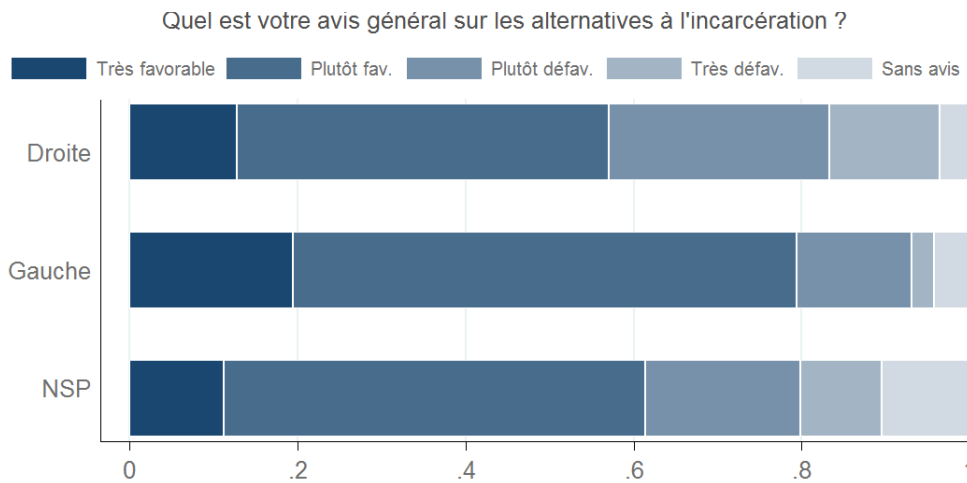


FIGURE 6: L'opinion publique sur les alternatives à l'incarcération

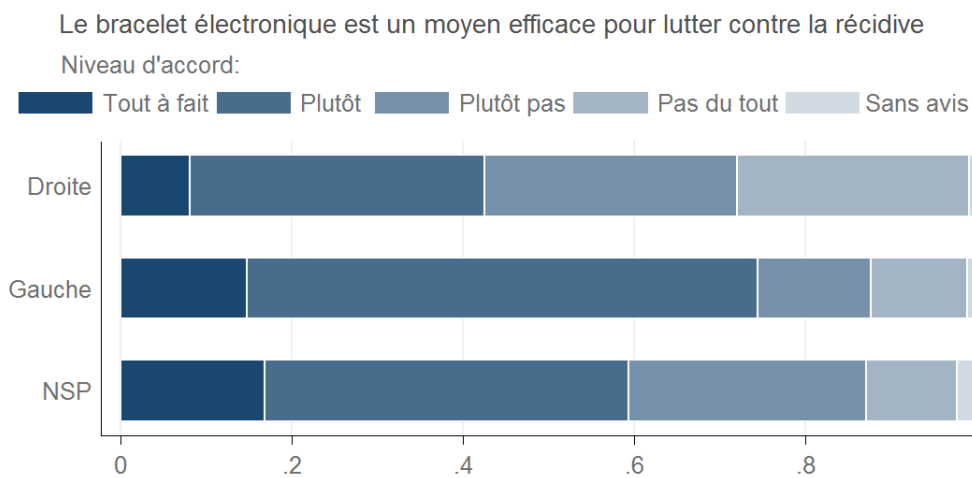


FIGURE 7: L'opinion publique sur l'efficacité du bracelet électronique

Ces résultats font écho aux enquêtes d'opinions menées dans d'autres pays, comme aux Etats-Unis. Selon une enquête nationale représentative du Pew Research Center en 2012, 84% des électeurs américains souhaitent réorienter les moyens alloués à l'incarcération des délinquants non-violents vers des peines de probation et des sorties anticipées. 87% des interrogés considèrent que le plus important n'est pas de savoir si les détenus subissent une peine de 18, 24 ou 30 mois, mais de faire en sorte que leur risque de récidive à la sortie diminue. Enfin 68% accordent plus d'importance à la part de la peine ferme réellement passée en prison, plutôt qu'à la durée absolue d'incarcération. Tous ces éléments suggèrent que, par souci d'efficacité dans la prévention de la récidive, une large majorité de l'opinion publique américaine est prête à accepter une réduction du poids de la prison dans le système pénal, en faveur d'autres types de sanctions. Cette évolution de l'opinion publique américaine intervient d'ailleurs au moment où le gouvernement fédéral, sous l'impulsion de Barack Obama, entreprend une vaste réforme de la justice pénale allant précisément dans ce sens.

## 0.2 Politiques publiques et choix délinquants : l'apport de la théorie économique

### 0.2.1 Fonctions et efficacité des peines

Les sanctions pénales ont traditionnellement plusieurs fonctions : les peines encourues doivent d'abord dissuader les délinquants potentiels de passer à l'acte (on parle de dissuasion générale) ou les orienter vers les infractions les moins graves (dissuasion marginale). Les peines doivent également être proportionnelles à la gravité des faits pour assurer une juste rétribution des victimes et de la société toute entière (fonction de rétribution). Les sanctions pénales doivent aussi permettre d'éviter de nouvelles infractions de la part des condamnés en les privant temporairement de certaines de leurs libertés (fonction de neutralisation)<sup>11</sup>. Enfin, l'expérience personnelle d'une sanction doit permettre de prévenir la récidive des condamnés au-delà de l'exécution de leur peine (notamment après leur sortie de prison), soit en leur montrant que le crime ne paie pas (fonction de dissuasion spécifique), soit en favorisant leur réinsertion sociale et professionnelle (fonction de réhabilitation).

On le voit d'emblée, il est compliqué d'envisager une peine "idéale" qui remplirait efficacement toutes ces fonctions à la fois. En effet, les peines s'adressent à des acteurs très différents (non seulement les coupables, mais aussi les délinquants potentiels, les victimes,

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11. Par exemple par l'emprisonnement, la surveillance électronique, l'interdiction de paraître dans certains lieux ou de fréquenter certaines personnes, etc.



et la société toute entière) et ont des fonctions assez contradictoires. Comment imaginer par exemple une peine suffisamment dissuasive pour éviter l'essentiel des passages à l'acte, mais qui favoriserait en même temps la réinsertion des condamnés qui la subissent ?

La modélisation économique est un outil adapté pour traiter formellement ce type de questions. Les modèles théoriques permettent d'explicitier le rôle de chaque mécanisme dans la production d'un phénomène (comme la délinquance ou la récidive), et de fournir des prédictions testables sur l'effet net attendu de divers changements (par exemple une réforme pénale, ou l'embauche de policiers supplémentaires). L'économie comportementale, par la conduite d'expériences contrôlées en laboratoire ou sur le terrain, permet de raffiner les hypothèses théoriques en explorant les processus réels de décision des acteurs et leurs biais éventuels. Enfin, l'économétrie permet quant à elle d'estimer quantitativement les effets à l'aide de données réelles, provenant généralement de bases de données de la police, de la justice, ou du système pénitentiaire.

Pour mener à bien ce programme de recherche, les économistes partent du cadre microéconomique de la théorie du choix rationnel et évaluent les politiques publiques à l'aune de plusieurs critères (efficacité, efficacité-coût, efficience).

### 0.2.2 Le cadre théorique du choix rationnel

Parmi les nombreux cadres d'analyse du crime, les économistes privilégient, depuis les travaux précurseurs de Becker (1968), la théorie du choix rationnel. Cette théorie considère le passage à l'acte délinquant comme le fruit d'un arbitrage entre les coûts et les bénéfices des activités légales et illégales. Les délinquants sont des agents rationnels qui cherchent à maximiser leur bien-être : ils choisissent ainsi de commettre une infraction dès lors que l'utilité qu'ils espèrent en retirer (qu'il s'agisse de gains monétaires, psychologiques, ou autres), notée  $U_c$ , est supérieure à l'utilité de réserve qui serait obtenue sans passage à l'acte,  $\underline{U}$ . Dans ce cadre, la règle de décision conduisant à la commission d'une infraction s'écrit :

$$U_c = pU(B - C) + (1 - p)U(B) > \underline{U} \quad (1)$$

où  $B$  représente le bénéfice retiré de l'infraction,  $p$  le risque perçu d'être sanctionné,  $C$  le coût de cette sanction (qu'elle soit légale ou sociale), et  $\underline{U}$  l'utilité de réserve de l'agent s'il s'abstient de commettre l'infraction (liberté, vie sociale, salaires, etc.).

Tous ces éléments sont évalués subjectivement par chaque individu : les individus diffèrent

par exemple dans leur crainte des sanctions (telles que la prison par exemple) ou dans leur “productivité criminelle” (leur capacité à voler et revendre des biens de valeur par exemple). Les individus diffèrent aussi dans leur attitude vis-à-vis du risque (il existe en particulier un risque de sanction) et vis-à-vis de l’incertitude (le gain qui pourra être retiré d’un cambriolage est incertain *a priori*, par exemple).

Le modèle du choix rationnel prédit donc que les criminels, à l’instar des autres agents, réagissent aux incitations, qu’elles soient positives (augmentation des salaires légaux par exemple) ou négatives (aggravation des peines). Il met ainsi en avant le rôle clé des politiques publiques de dissuasion (probabilité et coût de la sanction) sur les comportements criminels. Pour bien voir ces implications, on peut définir un point d’indifférence  $B^*$  tel qu’un individu choisit de commettre une infraction si et seulement si elle rapporte  $B > B^*$ . On déduit alors de l’équation 1 la règle suivante :

$$\frac{U(B^*) - \underline{U}}{|U(B^* - C) - \underline{U}|} = \frac{p}{1 - p} \quad (2)$$

Cette règle montre tout d’abord que toute augmentation de  $p$  ou de  $C$  réduit la probabilité d’une infraction (tant que  $U'(\cdot) > 0$ ). De plus, pour qu’un individu maximisateur décide de passer à l’acte, le rapport gains (en cas de succès) sur pertes (en cas de condamnation) doit être supérieur au risque relatif de condamnation. Autrement dit, si un individu a deux fois plus de chances d’être arrêté que de s’en sortir, son gain d’utilité en commettant l’infraction devra être au moins deux fois supérieur aux pertes liées à une condamnation pour qu’il décide de passer à l’acte.

Ce cadre théorique très simple, fondé sur un objectif de maximisation d’utilité, peut sembler éloigné de la réalité des prises de décision de nombreux délinquants, réputés impulsifs et peu rationnels : aux Etats-Unis par exemple, 65% des délinquants sont testés positifs à l’usage d’au moins un stupéfiant au moment de leur arrestation par la police (parmi la marijuana, l’héroïne, la cocaïne, les méthamphétamines et le PCP)<sup>12</sup>. Cependant, le modèle du choix rationnel permet de rendre compte de nombreuses régularités observées empiriquement, au niveau micro comme au niveau macro : Draca et al. (2015) trouvent par exemple que les vols de métaux à Londres sont très sensibles à l’évolution de leurs prix sur les marchés internationaux, tandis que Campaniello et al. (*forthcoming*) montrent que les suicides de détenus italiens en prison suivent l’évolution des débats sur les grâces collectives au Parlement italien.

12. Estimations de McCrary (2010) basées sur les données du programme *Arrestee Drug Abuse Monitoring*.

Au total, il est clairement établi par la recherche empirique que de nombreux comportements *a priori* irrationnels réagissent en réalité aux incitations extérieures, comme le suggère le modèle du choix rationnel de Becker (1968). De plus, ce modèle peut facilement être amendé pour tenir compte des biais cognitifs mis en évidence en économie comportementale (van Winden et Elliott, 2012) : l'équation 1 peut par exemple intégrer les perceptions distordues du risque par certains agents (le *gambler's fallacy* par exemple), l'aversion aux pertes (*loss aversion*) ou le rôle des émotions et du *self-control*.

Le modèle standard de Becker (1968) peut également intégrer les spécificités du système judiciaire, comme l'existence de délais souvent longs en pratique entre l'infraction et la sanction pénale. D'un point de vue théorique, ces délais amoindrissent l'effet dissuasif des sanctions auprès des délinquants impatients, puisque ceux-ci pondèrent le coût espéré des sanctions futures  $p.C$  d'un facteur d'escompte intertemporel  $\delta < 1$ . Une autre spécificité des peines de prison est que celles-ci "neutralisent" l'individu pour une certaine période, pendant laquelle il subit les coûts de l'incarcération sans pouvoir bénéficier de son utilité de réserve (par exemple son salaire légal). Le passage à l'acte n'est donc plus une décision statique comme dans le modèle de Becker (1968), mais un problème dynamique fait d'arbitrages entre des flux d'utilité future dont la valeur est actualisée. La question de la préférence pour le présent devient d'autant plus centrale dans ces modèles à horizon infini (Lee et McCrary, *forthcoming*).

Empiriquement, plusieurs études montrent justement que les délinquants sont marqués par une forte préférence pour le présent : Mastrobuoni et Rivers (2016) estiment à  $\delta = 0.74$  le facteur d'escompte annuel moyen parmi les détenus italiens libérés en 2006. De même, Åkerlund et al. (2016) montrent que le niveau d'impatience d'écoliers suédois mesuré à l'âge de 13 ans (par leur choix entre deux lotteries dont les gains sont plus ou moins différés) est un fort déterminant de leurs comportements de délinquance enregistrés 18 ans plus tard.

### Les éléments d'une théorie spécifique de la récidive

Bien que des modèles théoriques récents tiennent compte de la dimension dynamique des choix délinquants, ils ne disent généralement rien des effets de l'expérience d'une sanction sur les décisions futures des délinquants. En particulier, ces modèles considèrent que la prison n'a que des effets mécaniques (dissuader les délinquants potentiels et neutraliser temporairement les délinquants incarcérés), et pas d'effets sur les préférences, les croyances ou les contraintes des individus après la sortie de prison.

Or, le passage en prison transmet de l'information au délinquant et peut entraîner une

révision de ses croyances. Plusieurs études empiriques récentes illustrent cette capacité des délinquants à apprendre de leurs expériences (voire de celles de leurs pairs) par un processus bayésien (Anwar et Loughran, 2011; Lochner, 2007). A l'aide de données longitudinales sur la délinquance auto-déclarée et la perception des risques de jeunes américains, Anwar et Loughran (2011) montrent ainsi que les jeunes délinquants révisent dans le temps leurs croyances quant à la probabilité d'être arrêté par la police, en fonction de leurs expériences passées : ainsi, ces auteurs estiment que chaque infraction faisant l'objet d'une arrestation entraîne une réévaluation moyenne de +6% de la probabilité perçue d'être arrêté par la suite (comparativement à la situation où le jeune n'est pas arrêté après son infraction). Cette réévaluation est d'autant plus forte que les jeunes ont peu d'expérience délinquante, c'est-à-dire des croyances *ex-ante* incertaines. La réévaluation des croyances peut également porter sur la perception de la sévérité des peines : à l'aide d'une enquête menée en prison auprès de 1300 détenus au Danemark, Raaijmakers et al. (2016) montrent qu'en moyenne, leur évaluation subjective de la dureté de leur peine décroît avec le nombre de condamnations antérieures, ce qui suggère que les condamnés s'habituent à la dureté des sanctions pénales<sup>13</sup>.

Le passage en prison peut également affecter les espérances de gains des individus, à la fois sur le marché légal (sous la forme de salaires notamment) et dans les activités illégales (gains criminels). En exploitant une récente réforme pénale en Suède, Landersø (2015) montre ainsi que l'allongement de peines de prison très courtes peut permettre une amélioration de l'insertion professionnelle des sortants de prison (en termes d'accès à l'emploi et de salaires), grâce à une meilleure réhabilitation en détention et une meilleure préparation de la sortie. Mais l'incarcération peut aussi augmenter les gains illégaux. De nombreux travaux empiriques montrent ainsi l'existence d'effets d'apprentissage criminel entre codétenus d'une même prison : le fait pour un détenu d'être incarcéré avec des délinquants ayant commis le même type d'infraction augmente son risque de récidive à la sortie dans cette infraction (Bayer et al., 2009; Damm et Gorinas, 2013; Ouss, 2011).

Ces effets de pairs entre codétenus peuvent également s'expliquer par des effets de conformisme : partager son quotidien avec d'autres prisonniers peut favoriser une criminalisation des normes et des préférences, plutôt qu'un changement vertueux de son mode de vie. Or, une expérience menée dans une prison suisse par Cohn et al. (2015) montre bien l'importance de l'identité criminelle sur les comportements anti-sociaux : le simple fait de rappeler leur passé délinquant à des détenus tirés aléatoirement augmente significativement leur comportement

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13. Une autre explication serait un effet de sélection : les individus faisant carrière dans la délinquance sont ceux qui craignent le moins la prison.

de triche dans un jeu de pile ou face, par rapport aux autres détenus.

### 0.2.3 Efficacité et efficacité-coût

L'approche économique permet, par les méthodes de l'évaluation des politiques publiques, de mettre en comparaison les coûts et les bénéfices d'une politique. En matière de justice pénale, cet effort d'évaluation est décisif pour au moins trois raisons : tout d'abord, les enjeux financiers sont très importants (la délinquance comme l'incarcération coûtent très cher); deuxièmement, la mesure de l'efficacité d'une politique pénale est techniquement difficile (manque de données, biais de simultanéité, biais de sélection); enfin, les politiques publiques peuvent avoir des effets non-anticipés sur le comportement des délinquants (voire sur celui des juges ou des policiers<sup>14</sup>).

Un bon exemple de l'apport des économistes dans le champ pénal concerne l'évaluation des réformes répressives américaines dites *Three Strikes and You're Out*. Ces lois, adoptées par de nombreux Etats au cours des années 1990 (et souvent amendées depuis), prévoient un durcissement très fort des peines encourues en cas de récidive : en Californie, la loi prévoit ainsi un doublement de la peine encourue en récidive après une première infraction jugée sérieuse (*first strike*), et une peine minimale de 25 ans de prison en récidive après deux *strikes*. En exploitant les différentes peines encourues par des sortants de prison aux parcours comparables (tous avaient déjà deux condamnations éligibles aux *three strikes* mais certains n'avaient reçu qu'un avertissement et pas deux), Helland et Tabarrok (2007) montrent que le risque instantané (ou "hasard") de récidive dans les trois ans est inférieur de 17% chez les délinquants sous le coup d'une peine minimale de 25 ans (*third strike*) que chez ceux menacés d'une aggravation plus faible de leur peine (*second strike*). Cette différence de récidive est significative statistiquement et a permis d'éviter chaque année en Californie entre 20 000 et 30 000 infractions de la part des condamnés éligibles. Cependant, Helland et Tabarrok (2007) rapportent les bénéfices de cette politique à son coût financier et estiment que chaque infraction évitée coûte \$148 000 (du fait du coût de l'incarcération très longue de ceux qui récidivent tout de même), soit bien plus cher que le coût moyen d'une infraction (de l'ordre de \$34 000). Bien que le fort durcissement des peines ait un effet dissuasif chez les condamnés éligibles, cette politique semble donc loin de remplir le critère coût-bénéfice.

En plus d'estimer quelles politiques sont socialement rentables (au sens où les bénéfices

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14. Dans un article évaluant l'effet dissuasif d'une réforme simplifiant et accélérant les procédures pénales, Dušek (2015) montre que cette réforme a conduit à une augmentation de la délinquance enregistrée pour les délits routiers, par une réallocation des efforts de la police vers ces infractions faciles à élucider.

sont supérieurs aux coûts), l'évaluation économique permet aussi de comparer le ratio efficacité-coût de diverses alternatives en répondant à la question suivante : pour un euro dépensé, quelle option politique offre les meilleurs résultats ? Ce raisonnement permet à Helland et Tabarrok (2007) de conclure qu'une politique bien plus efficace en Californie, plutôt que d'allonger fortement les peines encourues en cas de récidive, aurait probablement consisté à renforcer les moyens de la police : en effet, l'élasticité qu'ils estiment vis-à-vis de la sévérité des peines encourues est beaucoup plus faible que les élasticités du crime vis-à-vis du risque d'arrestation et de condamnation estimées dans la littérature.

L'exemple des lois *Three Strikes and You're Out* témoigne également de l'importance des effets collatéraux des réformes pénales, et en particulier des effets de substitution et de déplacement. Tout d'abord, Meares et al. (2004) notent que l'instauration de peines minimales automatiques (donc non-proportionnelles) en cas de récidive peut alimenter une substitution néfaste entre types d'infraction ou mode opératoire : les individus qui décident de récidiver malgré la menace de peines automatiques sont incités à opter pour des infractions plus graves qu'en l'absence d'automatisme des peines (car le gain est supérieur et la sanction identique), et à faire usage de violences et d'armes (pour augmenter les chances de succès, à sanction identique). Ces effets néfastes sont justement identifiés empiriquement par Iyengar (2008) dans le cadre des *Three Strikes* en Californie : parmi les délinquants sous la menace d'une peine minimale de 25 ans (*third strike*), la probabilité de commettre un acte violent augmente de 9 points. De plus, l'auteure montre que l'implémentation de cette politique en Californie a provoqué le déplacement des délinquants sous le coup de peines accrues vers les Etats limitrophes (Nevada, Arizona). Ces coûts non-anticipés de la réforme en font une politique encore moins judicieuse que ne le suggéraient Helland et Tabarrok (2007).

#### 0.2.4 L'efficacité sociale des politiques publiques

Au-delà de l'efficacité-coût de différentes politiques publiques, l'approche économique permet d'interroger leur efficacité (Cooter et Ulen, 2016, chapitre 12). Au sens économique, une politique socialement efficace de lutte contre la délinquance minimise la somme des coûts des infractions commises et des dépenses publiques et privées dédiées à leur prévention ou répression (dépenses de police, de justice, protection privée, etc.). Cet objectif d'efficacité revient à choisir le niveau de répression qui permet d'égaliser le coût social marginal qu'engendrerait le prochain délit commis, et le bénéfice social marginal retiré de la prévention de ce même délit.

Ce critère d'efficacité apparaît dans la Figure 8. L'axe des abscisses représente le pourcentage de délits qui sont évités grâce à une politique (embauche de policiers supplémentaires, construction de prisons, subvention à la protection privée, etc.) : 0% correspond donc au *statu quo* et 100% correspond à une élimination totale de la délinquance dans l'économie. En ordonnées apparaissent le coût et le bénéfice pour la société, évalués en euros, de la prévention d'une infraction supplémentaire. Le coût social marginal de cette politique publique croît très certainement avec le pourcentage de réduction de la délinquance, car il est de plus en plus difficile de prévenir de nouvelles infractions (au fur et à la mesure, ne restent que les délinquants les plus motivés et les plus compétents, les délits les plus difficiles à détecter...). A l'inverse, le bénéfice social de la prévention de la délinquance décroît certainement avec le niveau de réduction atteint, puisque la société accorde plus d'importance à la réduction de la délinquance quand celle-ci est très forte, que quand la délinquance est déjà faible. L'intersection de ces deux fonctions au point A correspond à la politique socialement efficiente (réduction de la délinquance de  $r^*$ ) puisque en ce point, la société dépense autant pour se protéger de la prochaine infraction que le coût social de cette même infraction. Une politique de prévention moins ambitieuse,  $r < r^*$ , ne serait à l'inverse pas efficiente socialement puisque le bénéfice social retiré de la prévention de délits supplémentaires serait supérieur à son coût. A l'opposé, une politique plus ambitieuse  $r > r^*$  serait également sous-optimale puisque la société dépenserait plus que ce qu'elle est prête à payer pour prévenir la dernière infraction. Ce raisonnement micro-économique, à la marge, permet donc de justifier l'existence d'un niveau de délinquance d'efficacité très probablement supérieur à zéro. Dans l'exemple A, compte tenu des préférences sociales et du rapport coût-efficacité des techniques de prévention-répression utilisées, il est socialement efficace de réduire la délinquance de  $r^*$  (environ 40% ici).

Cet optimum social peut varier dans le temps et dans l'espace puisqu'il dépend des préférences des individus (en particulier de leur aversion à la délinquance) et de l'efficacité-coût des techniques utilisées. On peut imaginer que certaines opinions publiques sont plus hostiles à l'idée d'une forte surveillance policière que d'autres, ou que certains systèmes judiciaires nationaux sont plus efficaces que d'autres. L'optimum B de la Figure 8 correspond à une situation où la technologie de production de sécurité utilisée est beaucoup plus efficace (rapportée à son coût social) que celle utilisée en A, si bien que, pour un même bénéfice social marginal (même aversion à la délinquance), la politique efficace de lutte contre la délinquance sera beaucoup plus ambitieuse ( $r^{**} > r^*$ ), et le taux de délinquance optimal beaucoup plus faible.

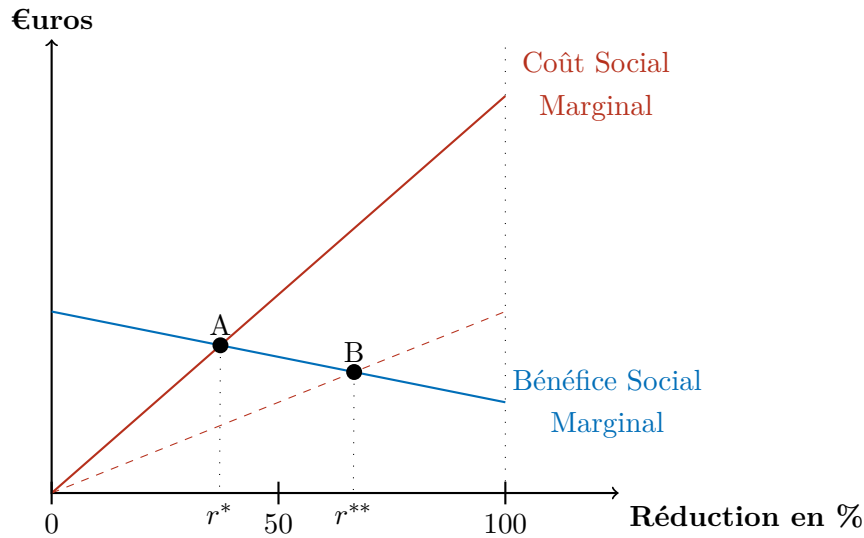


FIGURE 8: Le niveau d'efficacité de la délinquance

Dans une étude sur le pouvoir discrétionnaire des juges en matière de libération anticipée dans l'Etat de Géorgie, Kuziemko (2013) s'appuie sur ce type d'analyse *à la marge* pour étudier empiriquement l'efficacité des durées effectives d'incarcération fixées par les juges d'application des peines (*parole boards* dans le contexte américain). Ses résultats suggèrent que les juges fixent bien des durées d'incarcération efficaces, au sens où celles-ci minimisent les coûts sociaux de l'incarcération et de la récidive. Fixer des peines minimales, ou des seuils de peines à exécuter en détention avant tout aménagement<sup>15</sup>, peut donc avoir un coût en termes d'efficacité.

### 0.3 Prison et récidive : une brève revue de la recherche empirique

La recherche empirique sur les liens entre prison et récidive s'est beaucoup développée depuis le début des années 2000, du fait d'un meilleur accès aux données administratives (tels que les casiers judiciaires) et de l'investissement de ce champ par les économistes. En effet, les méthodes économétriques d'évaluation des politiques publiques ont permis d'estimer de manière beaucoup plus crédible qu'auparavant les effets causaux des peines, en contournant les biais d'endogénéité particulièrement marqués dans le domaine de la justice. Pour synthétiser les principaux résultats de cette littérature empirique, on peut suivre la chronologie de l'exécution d'une peine : avant, pendant, et après.

15. Beaucoup d'Etats américains ont par exemple adopté des lois dites *Truth-In-Sentencing*, qui prévoient qu'un détenu doit exécuter au moins 85% de sa peine ferme avant d'être libérable.



### 0.3.1 Avant la peine : le poids des anticipations et des délais

#### Peines anticipées et peines subies

L'efficacité des sanctions à prévenir la récidive peut se jouer dès la formation des croyances par les délinquants : une même peine peut être ressentie comme plus ou moins sévère par deux condamnés en fonction de leurs anticipations initiales. Dans une enquête portant sur 1300 détenus danois, Raaijmakers et al. (2016) interrogent au travers de quatre questions les condamnés sur leur perception de la dureté de leur incarcération (“La détention est plus dure que je le pensais auparavant”, “L’incarcération est une vraie punition”, etc.). Ils construisent ainsi un indice de la dureté perçue de l’incarcération allant de 1 à 5 (niveau maximal). Le score moyen est de 3,4 dans cet échantillon, et l’écart-type de 1. Les auteurs étudient les déterminants de ce score de dureté subjective de la peine : la prison est notamment vécue comme plus difficile par les condamnés qui sont les mieux insérés socialement et professionnellement, et ceux qui ont peu de condamnations antérieures. Raaijmakers et al. (2016) étudient aussi la corrélation entre ce score et la récidive dans les mois suivant la sortie. Bien que les auteurs ne trouvent pas de lien statistique robuste entre la dureté subjective des peines et la récidive (corrélation négative mais non-significative après l’ajout de variables de contrôle), cette étude est parmi les premières à documenter empiriquement les différences de ressenti entre condamnés dans le coût des sanctions.

Dans un article portant sur des données de l’Etat du Maryland, Bushway et Owens (2013) étudient également l’impact sur la récidive de recevoir une peine inférieure (ou non) à la peine anticipée. Les auteurs ne disposant pas des anticipations réelles des condamnés, ils exploitent une réforme de la grille des peines encourues pour certaines infractions (ou peines *recommandées* dans le contexte du Maryland) qui a altéré les anticipations de certains condamnés sans pour autant affecter les peines effectivement prononcées contre eux par les juges. En utilisant des estimations par doubles différences, Bushway et Owens (2013) montrent que la probabilité de récidive à la sortie de prison augmente d’autant plus que la peine effectivement subie par le condamné est inférieure à la peine qu’il pouvait craindre : en moyenne, une augmentation de 10% du ratio entre la peine encourue et la peine subie augmente de 1,2% la probabilité de réarrestation dans les trois ans. Ce résultat suggère l’existence d’un effet d’impunité criminogène lorsque les détenus sont condamnés à des peines inférieures à celles qu’ils pouvaient anticiper<sup>16</sup>. Ce résultat fait écho à une expérience en laboratoire menée par

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16. Cette intuition selon laquelle les condamnés récidivent plus lorsque la peine leur paraît moins dure qu’anticipée, pourrait expliquer les résultats de nombreux travaux sur la dissuasion spécifique, qui exploitent

DeAngelo et Charness (2012), qui montre que les participants "récidivent" d'autant plus que le régime de sanction est conforme à leur volonté.

L'une des implications politiques de l'article de Bushway et Owens (2013) est qu'une certaine cohérence entre peine encourue et peine subie est nécessaire pour maximiser l'efficacité des sanctions pénales. Or, un vaste mouvement de réformes répressives tendait, jusqu'à récemment, à l'allourdissement des peines encourues : en France par exemple, les peines plancher introduites en 2007 visaient à fixer des peines minimales plus sévères pour les condamnés en état de récidive légale. Mais selon les calculs de Leturcq (2012), ces peines plancher ne furent prononcées que dans 38% des cas applicables sur la période 2008-2010, ce qui suggère un creusement des écarts entre peines encourues et peines subies pour de très nombreux récidivistes. Les résultats de Bushway et Owens (2013) pourraient donc pointer un effet contre-productif des peines plancher françaises.

#### La question des délais d'incarcération

Outre l'écart pouvant exister entre les peines encourues et les peines effectivement prononcées, se pose le problème du délai de réponse pénale et de mise à exécution des peines. En France, le délai moyen entre la commission d'une infraction et la condamnation était de 11,3 mois en 2008 en matière correctionnelle. De plus, seules 35,1% des peines de prison ferme prononcées en 2008 avaient été mises à exécution à la fin de l'année (Blanc et Warsmann, 2011).

D'un point de vue théorique, l'existence de tels délais peut réduire l'effet dissuasif des peines encourues (puisque celles-ci sont actualisées avec un facteur d'escompte intertemporel  $\delta < 1$ ) et réduire l'efficacité des peines subies par les condamnés (perte de sens et de légitimité de la peine, révision des croyances sur  $C$ ). Enfin, ces délais ont l'effet mécanique de laisser l'opportunité aux délinquants de commettre des infractions supplémentaires avant leur incarcération. A ma connaissance, il n'existe pas de travaux empiriques sur cet effet néfaste des délais infraction-sanction sur la récidive avant incarcération. Avant de proposer quelques éléments d'évaluation, reconstituons la trajectoire fictive d'un délinquant récidiviste. La Figure 9 représente la succession d'infractions commises par ce délinquant avant d'être

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pour la plupart des situations dans lesquelles certains condamnés ont plus de chance que d'autres (ils bénéficient d'une loi d'amnistie ou de commutation de peines, ils tombent sur un juge peu sévère ou sur un avocat très compétent, etc.) et savent (ou peuvent se douter) qu'ils ont eu de la chance. D'un point de vue méthodologique, l'article de Bushway et Owens (2013) met ainsi en lumière la nécessité de mener des expériences aléatoires en aveugle (ou d'exploiter des expériences naturelles dans lesquelles les traités ne savent pas qu'ils sont traités) pour mesurer un vrai effet de dissuasion spécifique, et non la somme d'un effet de dissuasion et d'un effet de *framing*.

incarcéré : il commet initialement une série de deux infractions (1 et 2) constituant une seule et même affaire aux yeux du tribunal (par exemple, conduite sous l'empire d'un état alcoolique et délit de fuite). Cette affaire entrainera à terme la condamnation et l'incarcération du délinquant. Cependant, ce processus d'incarcération peut être plus ou moins long selon la célérité de la police (arrêter l'individu et réunir les preuves), le choix d'orientation de l'affaire par le parquet (procédure de comparution immédiate, instruction, convocation ultérieure par officier de police judiciaire, etc.), la décision éventuelle de placement en détention provisoire (par le Juge des Libertés et de la Détention), la peine finalement prononcée par le tribunal, et la décision de prononcer un mandat de dépôt ou au contraire de laisser le condamné libre pour une mise à exécution (incarcération) future. Les trois cas de figure les plus classiques en matière correctionnelle apparaissent dans la Figure 9 : ces différentes options judiciaires laissent plus ou moins de temps au délinquant pour commettre de nouvelles infractions (les infractions 3, 4 et 5 sur la Figure).

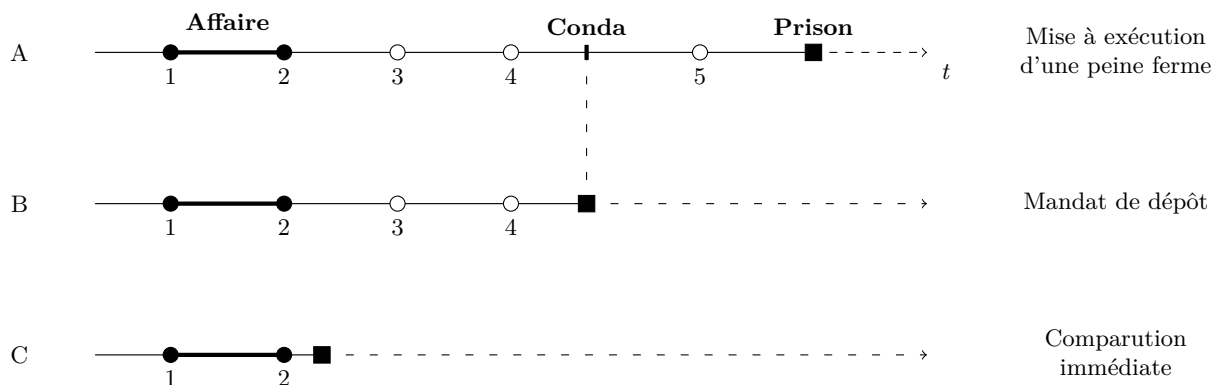


FIGURE 9: Une même trajectoire délinquante, trois délais d'incarcération différents

Le cas A correspond à une situation où le délinquant est condamné longtemps après les deux infractions initiales, ce qui lui laisse l'opportunité de commettre 2 nouvelles infractions avant condamnation. A l'audience, le tribunal ne prononce pas de mandat de dépôt (soit par décision du juge, soit car la peine ferme est inférieure à un an ce qui rend tout mandat de dépôt impossible), ce qui laisse à nouveau l'occasion au délinquant de commettre une cinquième infraction avant la mise à exécution de sa peine de prison ferme.

Le cas B correspond à la situation où le délinquant fait l'objet d'un mandat de dépôt le jour de sa condamnation (ou bien subit une période de détention provisoire), ce qui lui laisse tout de même l'opportunité de commettre deux infractions supplémentaires avant incarceration.

Le cas C correspond à la situation de la comparution immédiate, où le délinquant est arrêté

TABLE 2: Nouvelles infractions avant incarcération

Délai considéré :	Comparution immédiate	Mandat de dépôt	Mise à exécution	Total
<b>Début de l'affaire-incarcération</b>				
Part de récidivistes	24.95%	38.98%	54.09%	39.03 %
Nombre de récidives si nb>0	3.62	4.69	3.23	3.71
<b>Fin de l'affaire-incarcération</b>				
Part de récidivistes	4.70%	5.56%	32.47%	16.08%
Nombre de récidives si nb>0	2.77	3.90	2.70	2.90
<b>Condamnation-incarcération</b>				
Part de récidivistes	0.00	0.00	17.05%	7.37%
Nombre de récidives si nb>0	-	-	2.34	2.34
N	1965	2054	1540	6202

très rapidement après les deux infractions initiales, jugé en comparution immédiate dans la journée (ou les tout prochains jours), et incarcéré sur le champ. Cette dernière procédure ne laisse matériellement pas le temps au délinquant de commettre de nouvelles infractions avant son incarcération.

Pour comparer ces trajectoires fictives à la réalité des données françaises, on exploite les casiers judiciaires complets de l'échantillon de détenus libérés en 2002, qui recensent toutes les infractions ayant donné lieu à une condamnation (quelle que soit la peine prononcée) jusqu'en 2008. Les dates précises de chaque infraction permettent de reconstituer la trajectoire délinquante des détenus avant (et après) leur incarcération<sup>17</sup>. Les résultats apparaissent dans le Tableau 2 : sans surprise, la procédure rapide de comparution immédiate produit les taux de récidive avant incarcération les plus faibles (moins de 5% entre la fin de l'affaire et l'incarcération). Cependant, ces statistiques montrent le poids de la récidive avant incarcération dans les procédures plus longues, en particulier lorsque le condamné entre en prison pour mise à exécution d'une peine ferme : dans ces cas-là, un tiers d'entre eux a commis d'autres infractions depuis la fin de leur première affaire, et 17% depuis leur date de condamnation.

En plus de leur coût en matière de récidive avant incarcération, ces délais peuvent aussi nuire à l'efficacité des peines chez les détenus, soit parce que les peines tardives sont perçues comme plus injustes et moins légitimes par les condamnés, soit parce qu'elles entraînent une révision des croyances quant au coût actualisé des sanctions futures (voir Chapitre 4).

17. Il convient de souligner que l'on utilise ici uniquement les données issues de condamnations définitives inscrites au Casier Judiciaire National : sont donc exclues toutes les infractions n'ayant pas fait l'objet d'une condamnation. Les estimations données constituent donc une borne inférieure de la réalité de la délinquance dans l'échantillon.

### 0.3.2 Pendant la peine : ouvrir la boîte noire de la prison

Bien qu'il soit clairement établi que l'emprisonnement permet d'éviter de nombreuses infractions par son effet de neutralisation des détenus, l'impact de l'incarcération sur le comportement futur des condamnés est beaucoup moins clair (Durlauf et Nagin, 2011). Cette absence de consensus tient d'abord à la difficulté technique d'identifier statistiquement l'effet causal de l'incarcération, puisque les juges tentent généralement d'adapter au mieux les sanctions à chaque situation. Ces décisions discrétionnaires, basées sur des informations plus fines que celles accessibles aux chercheurs<sup>18</sup>, introduisent la menace d'un biais de sélection (ou biais de variable omise) dans les estimations : en effet, les condamnés qui sont envoyés en prison par les juges sont généralement intrinsèquement plus à risque de récidiver que les autres, quel que soit l'effet éventuel de l'incarcération. Cependant, cette difficulté technique est désormais résolue par plusieurs travaux récents. Mais une deuxième difficulté tient au fait que toutes les prisons ne se valent pas (certaines mettent plus l'accent sur la réinsertion que d'autres, par exemple) et que tous les détenus ne réagissent pas de façon uniforme à l'incarcération : en termes économétriques, il existe donc en réalité une multitude de "traitements" différents et une grande hétérogénéité des effets dans la population. Plusieurs résultats récents permettent cependant d'en prendre la mesure, et d'en tirer des enseignements sur l'efficacité de la prison par rapport aux peines alternatives.

#### Identifier un effet causal de l'incarcération

La solution idéale pour éviter tout biais de sélection et évaluer un effet causal consisterait à assigner les peines au hasard : un groupe aléatoire de condamnés serait envoyé en prison, tandis qu'un autre groupe subirait une peine alternative. Ce type d'expérimentation contrôlée a été conduit en Suisse dans les années 1990 par Killias et al. (2010). Entre 1993 et 1995, 123 condamnés de la région de Genève furent répartis aléatoirement entre deux types de peine : une courte incarcération (de 15 jours maximum) ou une peine de travail d'intérêt général. Les auteurs ont ensuite suivi le parcours de ces délinquants pendant onze ans. Contrairement à une thèse largement répandue, Killias et al. (2010) ne trouvent pas de différences significatives de récidive entre les deux groupes de condamnés. Les anciens détenus se portent même relativement mieux après onze ans en termes d'emploi et de revenus déclarés, ce qui remet

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18. Par rapport au chercheur, le juge dispose par exemple d'informations plus précises sur le contexte de l'infraction et sur le profil du prévenu (informations socio-démographiques, casier judiciaire, enquête sociale, expertise psychiatrique, avis médical sur les problèmes d'addiction, etc.).

en cause l'idée que les peines de prison -en particulier les courtes peines- seraient forcément néfastes. Dans une autre expérience contrôlée menée au Maryland entre 2000 et 2003, Bierie (2009) trouve quant à lui un impact néfaste de l'incarcération par rapport à une peine de *boot camp* (stage de type militaire) durant six mois : le groupe des détenus accumule significativement plus de recondamnations et de retours en prison que les autres dans les années suivant la sortie.

Outre les expériences contrôlées, difficiles à implémenter sur le terrain pour des raisons éthiques et pratiques, une autre méthode d'évaluation consiste à corriger le biais de sélection par des méthodes économétriques. Intuitivement, il s'agit d'exploiter statistiquement des situations dans lesquelles des délinquants *apparemment similaires* reçoivent des peines différentes pour une raison arbitraire, ou exogène. De nombreuses études récentes utilisent par exemple le fait que les affaires judiciaires sont réparties de manière aléatoire entre des juges qui diffèrent dans leur sévérité (Aizer et Doyle, 2015; Di Tella et Schargrodsky, 2013; Green et Winik, 2010; Kling, 2006; Loeffler, 2013; Manudeep et al., 2016; Mueller-Smith, 2014). Un autre exemple d'*expérience naturelle* intervient lorsque les prévenus sont défendus par des avocats commis d'office qui varient dans leur compétence, c'est-à-dire dans leur capacité à obtenir des peines plus légères pour leurs clients (Abrams, 2011). Enfin, les règles juridiques offrent souvent un troisième type d'instrument, cette fois basé sur la rigidité des peines qui peuvent être prononcées par les juges : dans de nombreux Etats américains par exemple, les juges sont souvent guidés -voire contraints- dans leurs choix par des grilles de peines recommandées qui fixent, pour chaque type d'infraction et chaque profil de délinquant, une peine minimale et maximale à respecter. Les discontinuités des peines recommandées par ces grilles permettent là-aussi d'identifier les effets causaux de peines plus ou moins sévères sur la récidive de condamnés *a priori* très similaires (Hjalmarsson, 2009; Kuziemko, 2013; Ganong, 2012).

Ces travaux économétriques rigoureux, utilisant des stratégies empiriques très similaires et bien souvent basés sur le même pays (les Etats-Unis), n'offrent pas de réponse univoque à la question de l'efficacité de l'incarcération. Parmi les études exploitant des différences de sévérité entre juges par exemple, certaines ne trouvent aucun effet significatif du passage en prison sur le parcours futur des condamnés (Green et Winik, 2010; Kling, 2006; Loeffler, 2013) tandis que d'autres identifient de larges effets criminogènes (Aizer et Doyle, 2015; Mueller-Smith, 2014). A l'inverse, Manudeep et al. (2016) estiment des effets très bénéfiques de l'incarcération en Norvège, avec une très forte réduction du risque de récidive. Ces divergences

persistent quand on restreint la discussion aux toutes meilleures études, qui tiennent compte de l'éventuelle non-monotonie des peines prononcées par les juges<sup>19</sup> et de la multidimensionalité des mesures judiciaires qui peuvent accompagner une peine comme l'incarcération<sup>20</sup> (Mueller-Smith, 2014; Manudeep et al., 2016). Pour comprendre cette absence de consensus, il convient de s'intéresser au contexte précis de chaque étude, en particulier à la qualité des conditions de détention et au rôle des liens sociaux (entre codétenus et avec l'extérieur).

### L'hétérogénéité des conditions de détention

Tout d'abord, les deux études économétriques montrant les effets les plus néfastes de l'incarcération proviennent des Etats-Unis. En étudiant un large panel de condamnés adultes au Texas, Mueller-Smith (2014) estime que chaque année d'incarcération supplémentaire provoque une augmentation de 6 points de la probabilité d'être poursuivi pour une nouvelle affaire dans les trimestres suivants, et réduit de 4 points la probabilité d'avoir un emploi. Ces effets néfastes de l'incarcération sont particulièrement marqués chez les condamnés qui avaient un emploi stable auparavant, et entraînent une série de difficultés économiques et familiales (dépendance accrue aux *food stamps*, hausse des divorces et baisse des mariages, etc.). En étudiant le parcours d'adolescents condamnés pour la première fois à Chicago dans l'Illinois, Aizer et Doyle (2015) trouvent également des effets très négatifs du passage en prison jusqu'à dix ans plus tard : selon leurs estimations, être incarcéré durant l'adolescence (entre 13 et 16 ans) augmente de 20 points de pourcentage le risque d'être ré-incarcéré à l'âge adulte (soit +70% environ) et diminue de 13 points les chances de terminer le lycée avec un diplôme.

Ces résultats contrastent très fortement avec ceux obtenus par Hjalmarsson (2009) et Manudeep et al. (2016) par exemple : en exploitant des discontinuités dans les grilles de peines recommandées dans l'Etat de Washington, Hjalmarsson (2009) identifie des effets très bénéfiques de placer les jeunes délinquants dans des prisons pour mineurs (plutôt que de prononcer des peines de probation), avec une réduction de 37% du hasard de récidive. Comme l'explique l'auteure, ce résultat pourrait s'expliquer par les forts moyens investis par l'Etat de Washington pour favoriser la réhabilitation des jeunes détenus. Les résultats obtenus par

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19. On parle de non-monotonie si un même juge est relativement sévère avec certains délinquants, mais relativement peu sévère avec d'autres. Ce type de comportement biaiserait les estimations exploitant les différences de sévérité moyenne entre juges.

20. Un juge peut par exemple assortir ses peines d'incarcération d'une obligation de soins, ou d'une période de probation plus longue que les autres, si bien que les différences attribuées à l'incarcération sont en réalité possiblement dues à l'obligation de soins.

Manudeep et al. (2016) en Norvège sont encore plus flagrants : en exploitant un panel de 23 000 délinquants ayant été condamnés par 500 juges différents (plus ou moins sévères), ces auteurs montrent que le passage en prison réduit le risque de récidive dans les cinq ans de 27 points et permet un meilleur retour à l'emploi. Ces effets bénéfiques de l'incarcération sont particulièrement forts chez les condamnés qui étaient les moins bien insérés auparavant, tandis que les effets nets sont nuls chez ceux qui avaient une situation professionnelle stable avant leur passage en prison. Ce type de résultat se retrouve d'ailleurs dans d'autres travaux scandinaves, comme par exemple Landersø (2015) en Suède : cet auteur estime que l'allongement des durées d'incarcération (très courtes en Suède, de l'ordre de un mois en moyenne) favorise fortement la réinsertion professionnelle des sortants de prison.

Pour donner sens à ces différents résultats, il faut noter que le système pénitentiaire scandinave est généralement présenté comme un modèle dans le monde : en Norvège par exemple, les prisons sont à taille humaine (392 cellules pour la plus grande, à Oslo), respectent le principe de l'encellulement individuel et offrent de nombreux services de réhabilitation et d'aide à la réinsertion. La quasi-totalité des détenus exécutent au moins une partie de leur peine ferme dans des "prisons ouvertes", qui ressemblent à des dortoirs avec une grande liberté de mouvement. L'expérience de l'incarcération par un délinquant d'un pays scandinave est donc probablement très différente de celle vécue par la plupart des détenus aux Etats-Unis et dans de nombreux autres pays.

Mastrobuoni et Terlizese (2014) apportent un indice supplémentaire du rôle clé des conditions de détention : ces auteurs évaluent les effets sur la récidive d'une nouvelle prison ouverte en Italie (la prison de *Bollate*, proche de Milan, ressemble beaucoup aux prisons ouvertes scandinaves), par rapport aux prisons italiennes traditionnelles. Pour corriger les estimations du biais de sélection dans l'attribution des places aux meilleurs condamnés, Mastrobuoni et Terlizese (2014) exploitent le caractère arbitraire des affectations à *Bollate* dues à des problèmes de surpopulation dans les établissements pénitentiaires voisins. Leurs résultats suggèrent que passer une année dans cette prison ouverte, plutôt qu'une prison traditionnelle, réduit la probabilité de récidive de 10 à 15 points dans les trois ans (par rapport à un taux moyen de 40%). Cette étude offre donc un test direct du rôle des conditions de détention dans la récidive. D'autres travaux récents vont dans le même sens : Gaes et Camp (2009) mènent une expérience contrôlée en Californie dans laquelle des détenus présentant le même niveau de risque initial (niveau 3) sont assignés de façon aléatoire à des prisons de niveau de sécurité différents (niveau 1 ou niveau 3). Les auteurs montrent que le "hasard" de récidive à la sor-



tie est supérieur de 31% chez les détenus placés dans les prisons plus sécurisées. Chen et Shapiro (2007) obtiennent des résultats très similaires en exploitant les discontinuités dans l'affectation des prisonniers fédéraux à des niveaux de sécurité différents.

Les méthodes utilisées dans ces études ne permettent cependant pas toujours d'*expliquer* pourquoi des prisons plus ouvertes, avec des régimes de détention plus souples, permettent une meilleure prévention de la récidive. Il semble que l'accès accru à des programmes de réinsertion par le travail soit bénéfique, d'autant que les effets positifs de l'incarcération se concentrent souvent sur les détenus les plus désavantagés socialement (Manudeep et al., 2016; Landersø, 2015; Mastrobuoni et Terlizzese, 2014). Un autre aspect bénéfique est l'accès à des enseignements (fondamentaux et professionnalisants) en détention : Tyler et Kling (2007) montrent que le fait de préparer (et obtenir) un diplôme en détention, même de très faible niveau (le *GED*, comparable au brevet des collèges), entraîne une augmentation des salaires perçus à la sortie, principalement chez les détenus noirs.

#### Le rôle des interactions sociales à l'intérieur et avec l'extérieur

Cependant, l'effet bénéfique des prisons plus centrées sur la réhabilitation des détenus pourrait aussi venir de la composition différente du groupe de "pairs" : alors qu'en prison classique, les détenus peuvent cohabiter avec des criminels aguerris, condamnés pour des infractions très lourdes et ayant un comportement violent en détention, les régimes de détention plus souples sont généralement sélectifs et n'intègrent donc que les "meilleurs" détenus (les mieux insérés, les moins violents, etc.). Un même condamné est donc exposé à des codétenus très différents selon qu'il est placé dans une prison ouverte ou dans une prison traditionnelle. Or, l'influence des codétenus dans les comportements de récidive est bien documentée empiriquement : en étudiant les prisons pour adolescents en Floride, Bayer et al. (2009) montrent que le fait, pour un détenu, d'être incarcéré en même temps que d'autres délinquants condamnés pour la même infraction, augmente significativement son propre risque de récidive dans cette infraction. Damm et Gorinas (2013) au Danemark et Ouss (2011) en France trouvent des effets de pairs très similaires, et montrent que ces influences sont d'autant plus fortes que les codétenus appartiennent à la même tranche d'âge. En Italie, une libération massive de détenus en 2006 permet même à Drago et Galbiati (2012) de calculer le multiplicateur social de la récidive : ces auteurs estiment qu'une réduction exogène du risque individuel de récidive de 1% permettrait une baisse de 2% du taux de récidive agrégé du fait des effets de pairs

entre anciens co-détenus<sup>21</sup>.

Bien que l'influence des codétenus soit généralement négative, les prisons peuvent aussi être le lieu d'interactions positives par le biais des visites de personnes extérieures. Duwe et Clark (2013) montrent ainsi que le fait, pour les détenus du Minnesota, de recevoir des visites en prison a un rôle bénéfique sur leur réinsertion (bien que les effets soient hétérogènes selon les types de relations avec le visiteur). De plus, Drago et al. (2011) trouvent que l'isolement géographique des prisons italiennes (mesuré par la distance entre la prison et la préfecture) est générateur de récidive, probablement du fait d'interactions sociales plus limitées avec l'extérieur (visites de proches, interventions de bénévoles associatifs). A partir d'une enquête portant sur l'ensemble des Etats-Unis, le think-tank Prison Policy Initiative (2015) établit statistiquement le lien entre distance géographique et visites : alors que 50% des détenus étant incarcérés à moins de 50 miles de chez eux ont reçu une visite le mois précédant l'enquête, ce taux baisse avec la distance, jusqu'à 15% pour ceux incarcérés à plus de 1 000 miles de chez eux.

### 0.3.3 Au-delà de la peine : réinsérer, contrôler, et faire changer

La prévention de la récidive peut également se jouer lors de la transition entre le milieu fermé et le milieu ouvert, grâce notamment au travail des agents de probation en charge du contrôle et de l'aide à la réinsertion des sortants de prison. Ce travail paraît d'autant plus nécessaire que le hasard de récidive est maximal dans les premières semaines qui suivent la sortie de prison, et diminue ensuite fortement pour se rapprocher de zéro après quelques années.

#### La transition et les programmes de réinsertion

De nombreux programmes tentent de faciliter le passage du milieu fermé au milieu ouvert, en fournissant une aide financière (*cash transfers*), un travail ou une formation professionnelle aux sortants de prison. Ces programmes sont particulièrement nombreux aux Etats-Unis, où les difficultés rencontrées par les anciens détenus (barrières légales à l'emploi, discriminations) sont très bien documentées empiriquement (Bushway et al., 2007).

Les programmes américains d'aide à la réinsertion (dont les plus connus sont *LIFE*, *TARP* ou *CEO*) font l'objet d'évaluations rigoureuses, par expérimentation aléatoire, depuis les

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21. A noter que plusieurs autres études empiriques montrent aussi que les effets de pairs criminogènes n'opèrent pas seulement en prison, mais aussi à l'extérieur (Corno (forthcoming) ou Kirk (2009)).

années 1970. Une recension de ces résultats par Raphael (2014) montre cependant les limites de tels programmes : le programme *TARP* par exemple, implémenté en Géorgie et au Texas, prévoyait une aide financière pendant 3 à 6 mois suivant la libération, ainsi que des séances de conseil à l'insertion professionnelle : ce programme s'est révélé inefficace pour lutter contre la récidive (pas d'effet, même sur les atteintes aux biens) et a eu des effets néfastes sur l'emploi des participants. L'aide financière était en effet un substitut, et non un complément, aux revenus salariaux obtenus par les sortants de prison, si bien que l'incitation à travailler était très faible pour les participants de ce programme.

Les programmes proposant un emploi de transition se révèlent en général un peu plus efficaces, en particulier quand les emplois offerts sont de longue durée : le programme *NSW*, qui proposait des emplois pour 12 à 18 mois à des anciens détenus, a ainsi permis une réduction du risque de récidive de 10 points dans les trois ans pour les participants de plus de 26 ans (pas d'effet chez les plus jeunes).

D'après Raphael (2014), les programmes de réinsertion les plus efficaces associent un emploi de transition à une série d'enseignements fondamentaux et de formations aux *soft skills*. Le programme *CEO* à New York en 2004-2005 fait figure d'exemple : ce programme intégré, offrant un travail en équipe entre anciens détenus en plus de nombreuses formations, a permis une réduction de 11% du risque de recondamnation dans les trois ans, bien que les effets bénéfiques sur l'emploi n'aient pas perduré après la fin du programme. D'autres programmes mêlant enseignements fondamentaux, formations professionnelles et *soft skills* ont aussi enregistré des résultats positifs : les *Job Corps*, qui prévoyaient l'hébergement obligatoire de jeunes dans un centre pendant 8 mois, ont ainsi eu des effets très bénéfiques sur la scolarité et l'emploi des participants, et permis une réduction de 5 points du risque de récidive.

L'une des leçons de ces nombreuses évaluations est que ces programmes sont d'autant plus efficaces qu'ils recrutent les participants juste après leur sortie de prison (voire avant), et non plusieurs mois après. De plus, les programmes intégrés, fournissant un soutien cohérent sur plusieurs dimensions (scolaire, professionnelle, psychologique, sociale), sont beaucoup plus efficaces que les formules strictement basées sur l'offre d'un emploi temporaire, voire sur de simples *cash transfers*.

En France, les rares programmes existants sont le fruit d'initiatives associatives locales, et ne font pas l'objet d'évaluations rigoureuses. La marge de progrès paraît donc très grande en matière d'aide à la transition des sortants de prison. La difficulté est d'autant plus grande que 80% des libérations correspondent à des "sorties sèches", c'est-à-dire sans le moindre suivi :

le condamné passe directement de la prison à la liberté totale, sans aucun contrôle ni soutien à la réinsertion au-delà des tout premiers jours. En plus du manque de moyens alloués à cet objectif, ce sont donc les conditions légales des sorties de prison (libération en fin de peine sans aucune obligation supplémentaire) qui compliquent l'aide à la réinsertion des anciens détenus en France.

Pourtant, plusieurs travaux montrent que des solutions simples et peu coûteuses peuvent se révéler efficaces. Dans une étude récente de la récidive de tous les détenus libérés en 2009 et 2010 en France, Galbiati et al. (2015) montrent que le risque individuel de récidive est fortement corrélé à la situation locale de l'emploi au moment de la libération : plus précisément, les auteurs estiment qu'une augmentation d'un écart-type du nombre d'ouvertures de postes dans le département durant le mois de libération réduit de 10% le risque d'être réincarcéré dans les 6 mois. Mais en utilisant des données sur la couverture médiatique du marché de l'emploi local, cette étude montre surtout que le lien entre emploi local et récidive s'explique avant tout par les informations relayées dans les médias sur les créations et destructions de postes : à situation de l'emploi constante, les bonnes nouvelles économiques locales réduisent la récidive des sortants de prison locaux, tandis que les mauvaises nouvelles favorisent leur récidive. Cette étude souligne donc qu'au-delà des effets directs de la situation objective de l'emploi sur la récidive (également démontrés dans d'autres contextes, comme Schnepel (2016) en Californie), l'accès à l'information économique est un levier d'action supplémentaire. On pourrait ainsi imaginer que dans les semaines précédant leur libération, les agents de probation informent systématiquement les détenus des offres d'emploi disponibles localement.

Les travaux de Munyo et Rossi (2015) en Uruguay suggèrent une deuxième piste pour contrer la récidive de court-terme. En croisant les variations quotidiennes du nombre de détenus libérés à Montevideo et de la délinquance enregistrée par la police de la ville, ces auteurs estiment que, jusqu'en septembre 2010, environ 25% des sortants de prison récidivaient dès le jour même de leur libération (principalement pour des atteintes aux biens, commises non loin de la prison). Alors, les autorités ont décidé de tripler le montant donné à chaque détenu le jour de sa sortie (de UR\$ 30 à UR\$ 100) : Munyo et Rossi (2015) montrent que ce changement a permis de faire disparaître la récidive du premier jour, sans déplacer la récidive aux jours suivants. Les auteurs en déduisent que les contraintes de liquidité sont au coeur de la récidive de court terme, et qu'un simple coup de pouce financier peut générer d'importants bénéfices pour un coût très faible.

En France, il est à noter que les détenus ne bénéficient pas d'aide financière spécifique

le jour de leur sortie<sup>22</sup> : les détenus les mieux lotis, ceux qui ont un solde créditeur sur leur compte personnel (grâce à leur travail en détention ou aux virements de leur entourage), récupèrent la totalité de leur pécule le jour de la sortie lorsqu'il s'agit d'une libération définitive (fin de peine). Mais pour les détenus qui sont libérés dans le cadre d'un aménagement de peine (libération conditionnelle, bracelet électronique, etc.), c'est au chef d'établissement de fixer le montant qui est rendu au détenu au moment de sa sortie. Certains détenus en aménagement se retrouvent donc avec seulement 30€ en poche pour affronter leurs premiers jours en liberté<sup>23</sup>.

### La supervision et le contrôle

Le modèle dominant pour prévenir la récidive en milieu ouvert consiste à soumettre les condamnés à une période de probation, pendant laquelle ils doivent respecter une série d'obligations (travailler, suivre un traitement, etc.) et rendre des comptes réguliers à un agent de probation. L'intensité de ce contrôle peut bien sûr varier d'un service à l'autre, mais aussi d'un probationnaire à l'autre selon les risques qu'ils présentent (évalués à l'aide d'échelles actuarielles) : aux Etats-Unis, les condamnés les plus à risques sont ainsi généralement soumis à un régime de probation strict, appelé ISP (*Intensive Supervision Probation*), qui prévoient des rendez-vous plus fréquents avec l'agent de probation, plus de contrôles, plus de tests sanguins, etc. Pourtant, la recherche empirique peine à démontrer l'efficacité de ces pratiques.

Dans l'Etat de Washington par exemple, Georgiou (2014) exploite les discontinuités dans l'intensité du contrôle subi par les probationnaires (basée sur un score de risque calculé en amont) pour évaluer l'efficacité de la supervision. Cet auteur ne trouve aucun effet significatif d'une surveillance accrue sur la récidive des condamnés. Dans une expérimentation aléatoire menée à Philadelphie, Hyatt et Barnes (2014) obtiennent des résultats très similaires : un contrôle plus strict ne prévient pas la récidive, et tend au contraire à augmenter le non-respect des obligations sous probation. Etudiant quant à elle la durée de la période de probation dans l'Etat de Géorgie (et non son intensité), Zapryanova (2014) ne trouve pas non plus d'effet sur la récidive.

En réalité, l'expérience la plus concluante en matière de probation provient du service de probation de Hawaï, qui a mis en place au début des années 2000 le programme *HOPE*. Ce

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22. Les sortants de prison peuvent cependant obtenir l'Allocation Temporaire d'Attente (525€ par mois pour une personne seule) s'ils effectuent les démarches auprès de Pôle Emploi après leur sortie.

23. Voir par exemple cet article de *Rue89* sur le sujet : <http://rue89.nouvelobs.com/2012/07/03/sortir-de-prison-avec-30-euros-en-poches-vous-avez-dit-reinsertion-233549>

programme de probation “*swift and certain*” instaure des sanctions rapides et automatiques (incarcération d’un à deux jours le week-end suivant) pour les probationnaires ne respectant pas leurs obligations (ne pas consommer de drogue, assister aux rendez-vous fixés, etc.), ainsi que des tests sanguins aléatoires. Cette combinaison de sanctions rapides et automatiques, et de contrôles imprévisibles, a permis une très forte augmentation du respect des obligations. Dans une expérimentation aléatoire, Hawken et Kleiman (2009) montrent ainsi que la part des probationnaires testés positifs aux drogues est divisée par trois, de même que la part de ceux qui manquent un rendez-vous. Au total, bien que les participants au programme *HOPE* soient automatiquement envoyés en prison en cas de non-respect, ceux-ci accumulent bien moins de journées de détention que les autres probationnaires. A la suite de ce succès, des programmes similaires furent adoptés dans d’autres Etats et pour d’autres publics : exploitant la mise en place graduelle du programme dans le Dakota du Sud, Heaton et al. (2015) trouvent aussi des effets très bénéfiques des sanctions rapides et automatiques sur la récidive de condamnés alcooliques.

### Changer l’individu, changer la justice

Au-delà de la peine, l’objectif de prévention de la récidive passe enfin par la transformation de l’individu, et peut-être même par la manière de rendre la justice.

Le changement de l’individu, dans ses attitudes, dans ses croyances, et donc dans ses actions, est l’objectif affiché des thérapies cognitivo-comportementales, dites *TCC* (ou *Cognitive Behavioral Therapy* en anglais). Ces programmes visent à renforcer le contrôle de soi, à améliorer l’image de soi, et à rendre la prise de décision plus réfléchie. Il est en particulier clairement établi que les délinquants peinent à tenir compte des conséquences négatives de leurs actes à moyen ou long terme, et focalisent leur prise de décision sur des rendements de court terme. De nombreux travaux, en laboratoire ou sur données de terrain, montrent aussi que les émotions positives et négatives (comme les défaites sportives inattendues) provoquent des passages à l’acte délinquant. Les thérapies cognitivo-comportementales visent donc à corriger les biais ou incohérences de tels choix en apprenant aux individus à réfléchir avant d’agir et à changer d’attitudes.

Plusieurs expérimentations de terrain récentes montrent la grande efficacité de tels programmes. A Chicago, Heller et al. (forthcoming) montrent que la participation de collégiens à des séances de *TCC* en classe et en dehors (13 séances en moyenne) réduit significativement la proportion des jeunes arrêtés par la police (-44%) et réduit la part de ceux qui abandonnent

l'école. Des résultats très bénéfiques sont également obtenus au Libéria auprès de délinquants adultes, où Blattman et al. (2015) montrent que les thérapies cognitivo-comportementales associées à un soutien financier permettent une réduction forte et durable de la délinquance chez les participants. Ces résultats suggèrent que les *TCC* sont parmi les moyens les plus efficaces de prévenir le passage à l'acte délinquant.

D'autres approches récentes se concentrent sur les interactions entre le condamné et l'institution judiciaire. La justice restaurative, par exemple, réunit des victimes et des condamnés dans des séances d'échange pour faire changer les attitudes, et donc les comportements, des délinquants. De la même manière, les travaux sur la légitimité de la justice suggèrent que des procédures judiciaires perçues comme justes et légitimes permettent d'améliorer le comportement des condamnés et l'efficacité des sanctions (Beijersbergen et al., 2016). Les évaluations causales restent cependant peu nombreuses dans ce domaine.

## 0.4 Résumé de la thèse et de ses implications

### 0.4.1 Une thèse en économie du crime

Cette thèse propose quatre contributions sur les liens entre prison, réinsertion et récidive. Ces thèmes de recherche s'inscrivent dans un champ, l'économie du crime, qui connaît un renouveau depuis vingt ans et les travaux de Steven Levitt<sup>24</sup>. Aujourd'hui, l'économie du crime est un champ bien établi dans le paysage de la recherche en économie, avec des publications très fréquentes dans les meilleures revues internationales, des workshops de haut niveau (comme le *Transatlantic Workshop on the Economics of Crime* organisé chaque année en Europe), des structures et réseaux institutionnalisés (le *Crime Lab* de l'université de Chicago, un groupe de travail et une série de working papers *NBER* dédiés, le réseau de chercheurs sud-américains *AL CAPONE*, etc.).

A l'image du champ de l'économie du crime, cette thèse s'appuie essentiellement sur des fondements théoriques micro-économiques (Becker, 1968) et adopte une approche économétrique imprégnée des méthodes de l'évaluation des politiques publiques. Les données utilisées dans les quatre chapitres sont françaises et proviennent des deux dernières bases nationales construites par la Direction de l'Administration Pénitentiaire (enquête sortants 1996-1997 ; enquête sor-

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24. Steven Levitt, professeur à l'université de Chicago et lauréat de la John Bates Clark Medal en 2003, a publié plusieurs articles précurseurs en économie du crime dès la fin des années 1990. On peut par exemple citer son article sur l'élasticité police-délinquance (Levitt, 1997), sur l'effet dissuasif et neutralisant de l'allongement des peines (Kessler et Levitt, 1999) ou sur les déterminants de la délinquance des jeunes (Levitt, 1998).

tants 2002).

L'objectif de la thèse consiste, d'une part, à identifier les déterminants individuels de la récidive des sortants de prison, et d'autre part, à évaluer les effets du traitement pénal des condamnés sur ce risque de récidive. La thèse fournit donc à la fois un outil d'évaluation des risques individuels (Chapitre 1), et une série de réponses à trois questions pratiques et politiques : qui incarcérer (Chapitre 3), quand incarcérer (Chapitre 4), et pour combien de temps (Chapitre 2).

#### 0.4.2 Contributions et principaux résultats

**Le premier chapitre** étudie les déterminants individuels de la récidive des sortants de prison dans le cadre d'un modèle de durée (ou modèle de survie). Jusqu'alors, aucune recherche en France n'explorait la dimension temporelle du risque de récidive des sortants de prison. Il était pourtant clairement établi dans la littérature internationale que le risque instantané de récidive est très fort dans les semaines suivant la sortie de prison, et diminue fortement ensuite. Ce chapitre est la première étude à documenter empiriquement en France la distribution du risque de récidive dans le temps : les sortants de prison présentent ainsi un hasard de récidive maximal à leur libération, lequel diminue très fortement dès la première année pour se rapprocher de zéro après quatre ou cinq ans. Ce constat d'un hasard fortement décroissant a depuis été reproduit par Josnin (2014), en suivant pendant 8 ans les casiers judiciaires des 500 000 personnes condamnées en France en 2004 (pour délits ou contraventions de 5<sup>e</sup> classe). Ces résultats non-paramétriques montrent donc que les anciens détenus qui n'ont pas commis de nouvelles infractions dans leur(s) première(s) année(s) de liberté (on parle d'état de *survie* dans la non-récidive) présentent un très faible risque de récidiver plus tard. En termes de politique publique, ce résultat suggère que l'essentiel des moyens de prévention de la récidive (en particulier l'aide à la réinsertion et le travail de contrôle des agents de probation) doivent être concentrés sur les premières semaines et les premiers mois suivant la libération.

Dans un deuxième temps, ce chapitre recourt à des estimations économétriques pour isoler les déterminants individuels du hasard de récidive. Les modèles de durée généralement utilisés, comme le modèle de Cox (1972), reposent sur l'hypothèse rarement discutée de proportionalité des hasards : chaque variable explicative est supposée affecter le risque instantané de récidive de manière multiplicative et constante dans le temps. Or, on peut penser que certaines caractéristiques ont un effet sur la récidive qui varie dans le temps : par exemple,



on peut imaginer que le fait d'être de nationalité étrangère peut augmenter le risque de condamnation à court terme (du fait d'un moindre accès à des programmes sociaux, d'un risque accru d'être contrôlé et arrêté par la police, etc.), mais qu'à moyen terme, les personnes étrangères ont plus de chances que les autres d'émigrer hors de France (par exemple pour retourner dans leur pays d'origine) si bien que leur risque de récidive en France est plus faible. Les résultats économétriques du Chapitre 1 vont exactement dans ce sens : la variable "nationalité étrangère" est associée à une forte augmentation du hasard de récidive à court terme mais à une rapide diminution de ce risque dans le temps. Ce type de relation dynamique est obtenu pour d'autres caractéristiques individuelles, alors que la plupart des travaux antérieurs négligent cette possibilité. Pour cela, la méthode utilisée consiste simplement à adapter la régression de Cox (1972) en ajoutant parmi les régresseurs une suite de termes d'interactions avec le temps. Le Chapitre 1 entend montrer que cette modélisation peut fournir des résultats utiles sur l'évolution dynamique des facteurs de risque de récidive, même s'il n'est pas toujours permis de distinguer le rôle de la dépendance d'état des effets de composition dans le temps. Cependant, ces modèles ont aussi leurs défauts (difficulté d'interprétation des résultats, hypothèses discutables sur la forme fonctionnelle de l'interaction temporelle, difficulté à traiter de l'endogénéité des variables), et les chapitres suivants emploient des modèles plus classiques en micro-économétrie (probit simple ou bivarié, modèle linéaire, etc.).

**Le deuxième chapitre** porte sur les effets des réductions de peine sur la récidive des sortants de prison, c'est-à-dire la *marge intensive* de l'incarcération. Ce travail part du constat que la littérature empirique internationale n'offre aucun consensus sur l'effet criminogène ou non de la durée d'incarcération : parmi les meilleurs articles empiriques, certains identifient un effet bénéfique du temps passé en prison, d'autres un effet néfaste, et d'autres encore un effet nul. Ce chapitre de thèse tente de réconcilier ces résultats divergents en intégrant à la discussion le design des réductions de peine, c'est-à-dire le contexte précis qui amènent certains détenus à être libérés plus tôt que d'autres, pour des raisons exogènes. Le chapitre s'inspire pour cela du modèle de *reference-dependent preferences* de Köszegi et Rabin (2006) pour montrer que, d'un point de vue théorique, l'effet d'une réduction de peine peut fortement dépendre des anticipations des détenus et de leur capacité à s'adapter à une éventuelle surprise avant leur libération. Intuitivement, une réduction de peine peut être positive pour un détenu qui l'anticipe et qui peut se préparer à sa libération anticipée (à la fois psychologiquement et

matériellement, en termes d'hébergement, d'emploi, etc.), mais la même réduction de peine peut à l'inverse être néfaste et criminogène pour un détenu pris au dépourvu et libéré "par surprise" peu après avoir appris la nouvelle.

Les résultats économétriques obtenus dans le contexte français soutiennent cette intuition. Afin d'identifier un effet causal des réductions de peine sur la récidive et non pas une relation biaisée par des variables omises (les détenus qui obtiennent le plus de réductions de peine sont ceux qui présentent intrinsèquement les moindres risques de récidive), le Chapitre 2 exploite la grâce présidentielle de Juillet 1996. Cette grâce octroyait le même jour à (presque) tous les détenus une réduction de peine proportionnelle à la durée qu'il leur restait à subir au 9 Juillet. Pour deux détenus similaires condamnés à une même peine initiale, la grâce introduit donc une variation exogène dans le montant de leurs réductions de peine (qui dépend de leur date d'entrée en prison), ce qui fournit une variable instrumentale crédible. En exploitant cet instrument et le fait que la grâce n'intervient pas au même stade de la peine pour tous les détenus (certains au début de leur peine, d'autres au milieu, d'autres à la fin), les estimations d'un modèle probit à variable instrumentale par Maximum de Vraisemblance montrent que ces réductions de peine furent criminogènes chez les détenus qui devaient être libérés peu de temps après la date de la grâce, et au contraire bénéfiques chez les détenus qui avaient plus de temps pour s'adapter à la nouvelle et préparer leur libération anticipée. De manière cohérente avec le cadre de Kőszegi et Rabin (2006) sur les anticipations, les estimations montrent que ces effets bénéfiques et néfastes de la grâce présidentielle se concentrent sur les détenus ayant *a priori* peu d'expérience personnelle de cette mesure (ils n'étaient pas en prison au moment de la précédente grâce de 1995 et avaient été rarement condamnés auparavant). Les détenus plus expérimentés ne furent à l'inverse pas significativement affectés par ces réductions de peine, probablement du fait de leur meilleure capacité à anticiper la grâce présidentielle. Enfin, les résultats montrent que l'effet criminogène de ces réductions de peine se concentre sur les détenus les plus démunis (faible capital économique et social) et concerne surtout des atteintes aux biens : il semble donc que les réductions de peine non-anticipées entraînent une libération mal préparée, dans de mauvaises conditions matérielles, qui mène finalement à la récidive. A l'inverse, lorsque les réductions de peine laissent le temps au détenu de s'adapter avant sa libération, les effets sur la récidive deviennent bénéfiques ou nuls. Ce chapitre montre donc la grande hétérogénéité des effets des réductions de peine et le rôle déterminant du *timing* et des anticipations.

**Le troisième chapitre** interroge quant à lui la *marge extensive* de l’incarcération et évalue l’efficacité de la prison par rapport au bracelet électronique. Le placement sous surveillance électronique (PSE) s’est considérablement développé en France depuis son introduction en octobre 2000 : aujourd’hui, près de 10 000 personnes portent chaque jour un bracelet électronique à leur cheville, pour un flux annuel supérieur à 20 000 placés (depuis 2011). L’immense majorité des bracelets électroniques concernent des personnes condamnées à des courtes peines de prison ferme (inférieures à 2 ans aujourd’hui, contre 1 an auparavant) : plutôt que d’incarcérer ces condamnés, le Juge d’Application des Peines peut décider d’aménager leurs peines fermes, avant toute incarcération, sous la forme d’un placement sous surveillance électronique. Les placés sont alors soumis pendant plusieurs mois à un “couvre-feu” (rester à leur domicile de 18H à 8H du matin par exemple) et à diverses obligations (travailler, suivre une formation ou des soins, rendre des comptes réguliers à son agent de probation, etc.). Cet aménagement est réputé moins criminogène que la prison, et se révèle bien moins coûteux (environ 12€ par jour pour un bracelet). Cependant, il n’existait jusqu’alors aucune évaluation rigoureuse des effets sur la récidive du bracelet électronique, par rapport à l’incarcération, en France. Seul un article publié, exploitant des données de Buenos Aires, montrait des effets causaux très bénéfiques du bracelet par rapport aux prisons argentines en termes de récidive (Di Tella et Schargrotsky, 2013).

Pour mesurer de manière convaincante l’effet causal du bracelet électronique sur la récidive, il convient de tenir compte des critères de décisions d’aménagement utilisés par les Juges d’Application des Peines. Au-delà des critères d’éligibilité (courte peine sans mandat de dépôt, domicile stable), les juges sélectionnent les “meilleurs” condamnés, c’est-à-dire ceux qui présentent intrinsèquement les meilleures chances de réinsertion et les plus faibles risques de récidive. Cette prise de décision s’effectue donc à partir de caractéristiques objectives observables dans nos bases de données (situation familiale, emploi, âge, etc.) mais aussi sur des dimensions plus qualitatives que seul le juge observe (motivation du condamné, problèmes d’addictions éventuels, contexte précis de l’affaire, etc.). Ces variables inobservables introduisent un risque de biais de variables omises dans les estimations. Le chapitre corrige ces différents biais en exploitant la mise en place graduelle du PSE en France : cet aménagement fut en effet d’abord testé pendant un an dans 4 tribunaux (à Agen, Aix-en-Provence, Grenoble, Lille, entre octobre 2000 et octobre 2001) avant d’être élargi à l’ensemble du territoire et adopté progressivement par d’autres tribunaux. Ces discontinuités spatiales et temporelles dans l’accès au bracelet électronique permettent de comparer la récidive de condamnés *a priori*

semblables en tous points, excepté que certains avaient une chance de pouvoir bénéficier d'un bracelet tandis que c'était impossible pour d'autres (car leur tribunal n'utilisait pas encore le bracelet au moment de leur incarcération). Les résultats économétriques du modèle probit bivarié récursif montrent un effet bénéfique du PSE sur la récidive, avec une réduction de 6 à 7 points de la probabilité de récidive dans les 5 ans (soit une réduction de 9 à 11%), par rapport à l'incarcération. Cet effet moyen bénéfique est assez hétérogène parmi les condamnés : il est notamment plus fort chez les placés qui avaient déjà connu la prison avant d'obtenir un bracelet, chez ceux qui ont reçu des visites de contrôle à domicile durant leur période de surveillance électronique, et chez les placés qui avaient l'obligation de travailler. Ces résultats montrent donc que des alternatives moins coûteuses que la prison peuvent être également plus efficaces pour prévenir la récidive. Cependant, les profils choisis et le contrôle effectif des placés semblent cruciaux pour assurer l'efficacité du bracelet électronique. Cette conclusion met donc en question la situation actuelle en France, où la surveillance électronique s'est tellement développée que le contrôle réel est beaucoup plus limité. Malheureusement, aucune base de données française ne permet d'actualiser notre évaluation sur des données plus récentes.

**Le quatrième chapitre** s'intéresse enfin à l'influence des délais infraction-incarcération. En France, le délai médian entre une infraction et sa condamnation est de 5 mois en matière correctionnelle (le délai moyen est de 10 mois), et pour les condamnés à une peine ferme sans mandat de dépôt, la mise à exécution de la peine médiane dure encore 8 mois. D'un point de vue théorique, ces longs délais entre la date de commission d'une infraction et le moment où le délinquant commence à subir sa peine, paraissent néfastes pour deux raisons principales : ces délais laissent aux délinquants le loisir de commettre d'autres infractions entre temps, et ils montrent aux délinquants que la justice est lente et les sanctions très tardives. Un délinquant qui subirait une peine de prison longtemps après avoir commis l'infraction sera donc moins dissuadé de récidiver, dès lors qu'il a une préférence pour le présent, puisqu'il pourra s'attendre à des délais de sanction en récidive tout aussi longs (en tout cas plus longs que ses croyances initiales).

Ce chapitre explicite théoriquement ce mécanisme et montre que les comportements de récidive des détenus en France réagissent bien aux délais infraction-incarcération. Empiriquement, pour tenir compte de l'endogénéité de ces délais (les policiers peuvent essayer d'arrêter plus rapidement les délinquants à fort risque de récidive, les procureurs et les juges plus prompts à les incarcérer), cette étude exploite des variations *a priori* arbitraires, ou exogènes,

dans les choix de procédures effectués par les procureurs pendant une garde à vue : les procureurs doivent en effet décider d'utiliser une procédure de comparution immédiate (CI), qui entraîne le jugement et bien souvent l'incarcération du prévenu le jour même ou dans les jours suivants ; ou bien une procédure classique, plus lente, qui voit généralement le prévenu être jugé et incarcéré bien plus tard. Cependant, les délinquants mineurs ne sont pas éligibles à la CI, si bien qu'il existe une discontinuité nette à l'âge de 18 ans (au moment de l'infraction) dans les chances d'être incarcérés plus ou moins vite. Une deuxième source d'exogénéité possible provient du fait bien connu que les grands tribunaux (généralement les plus engorgés) utilisent beaucoup plus les procédures rapides de comparution immédiate, que les autres tribunaux français. En exploitant tour à tour ces deux caractéristiques de la CI dans le cadre de Régressions par Discontinuité ou de modèles probits bivariés, nous obtenons des résultats économétriques très proches : le fait d'être incarcéré rapidement dans le cadre d'une comparution immédiate, plutôt que dans une procédure plus longue, entraîne une très forte diminution du risque de récidive dans les 5 ans (environ -50%). Cette étude est la première au niveau international à quantifier sur données réelles le coût de sanctions plus lentes en termes d'accroissement de la récidive. Ce résultat met donc l'accent sur l'importance d'avoir un système judiciaire rapide et efficace, capable de sanctionner au plus vite les délinquants.

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## Chapitre 1

# The Determinants of Recidivism among Ex-Prisoners : a Survival Analysis on French Data

### **Abstract**

This article explores the main determinants of the hazard of recidivism among ex-prisoners. We use a nationally-representative sample of prisoners released in 1996-1997 in France, drawn from a 5-year follow-up survey run by the French correctional administration. We estimate semi-parametric duration models which deal with violations of the proportional hazards hypothesis. Our results confirm the importance of gender, age, nationality, access to employment and prior convictions on recidivism within five years after release from prison. We also find significant differences in hazards of recidivism by type of initial offense, penal status at entry, and type of release (early release under parole, etc.), while controlling for prison fixed effects. Finally, our study casts doubt on the influence of certain variables (marital status at entry, education, homelessness) and on the effectiveness of *semi-liberté* as a way to prevent recidivism.

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## 1.1 Introduction

According to the last figures published by the French correctional administration<sup>1</sup>, the number of people incarcerated in French prisons hit a historic high in December 2012 with 67,674 prisoners (+ 3,7% over the last 12 months) : this figure represents an incarceration rate of 103 for 100,000 inhabitants. Since French prisons have a total capacity of only 56,953, most facilities face overcrowding. Moreover, each year, more than 80,000 prisoners are released back into society. One of the missions of the french judicial and correctional system is to rehabilitate prisoners, facilitate their reentry, and prevent recidivism<sup>2</sup> and return to prison. Thus it is important to measure recidivism among ex-prisoners, better understand its determinants, and eventually offer possible solutions.

The most recent survey on recidivism in France, which is about a cohort of detainees released between June and December 2002, suggests that 59% of ex-prisoners are re-convicted in the next five years following release, 80% of them returning back to prison (Kensey and Benaouda, 2011). Besides its frequency, recidivism occurs rapidly, as 75% of recidivists reoffend in the first two years after release. International surveys show that France is no exception : in England and Wales for example, 57,6% of the prisoners released in 1998 were convicted again in the two years following release (Bowles and Florackis, 2007). In the United States, the most recent federal survey suggests that about 70% of those released in 1994 were rearrested in the next three years (Langan and Levin, 2002). These high recidivism rates generate substantial additional costs to society, whether direct or indirect (costs to the victims, additional police and justice expenditures, cost of incarceration, loss of human and social capital, loss of production while incarcerated, etc.).

Economists pay increasing attention to the study of crime and recidivism, notably since the seminal work of Gary Becker. His rational-choice crime model (Becker, 1968) led to the development of the field of economics of crime, which applies microeconomic concepts and methods to the study of illegal behaviors and repressive institutions. Following Becker, many researchers used econometrics and experimental economics to test the empirical validity of this model; however, to this day, most research rely on a small set of countries (US, UK, Italy and a few others). Conversely, there are only a few analyses on French data, which

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1. Official monthly statistics can be obtained at [www.justice.gouv.fr/prison-et-reinsertion-10036/les-chiffres-clefs-10041/statistiques-mensuelles-de-la-population-detenu-e-et-ecrouee-23435.html](http://www.justice.gouv.fr/prison-et-reinsertion-10036/les-chiffres-clefs-10041/statistiques-mensuelles-de-la-population-detenu-e-et-ecrouee-23435.html)

2. In this study, the term "recidivism" only stands for "re-conviction", i.e. the fact that an ex-prisoner commits a new offense and is re-convicted after release

do not take into account the time-dependent aspect of recidivism processes (Kensey and Benaouda (2011); Maurin and Ouss (2009)). This article attempts to contribute to this topic by applying duration models to French data.

The article is structured as follows : the next section briefly reviews the main determinants of recidivism identified by prior research. The third section presents the dataset, while the fourth section discusses the model specification we chose. The fifth section presents the results obtained in terms of instantaneous probability of recidivism. Finally, the last section concludes and discusses the limitations of this study.

## 1.2 The determinants of recidivism : survey of the literature

The analysis of recidivism among ex-prisoners relates to a much broader research field studying delinquency and crime. In this field, some scholars focus on understanding what leads to criminal behavior, whereas others attempt to better capture the long-term pathways to desistance (Sampson and Laub, 2003). Overall, the existing literature relies on four main types of factors in the study of individual criminal behavior :

- Sociodemographic factors : sex, age, ethnic origin, marital status, education level, standard of living (access to employment, occupation, income), etc.
- Cognitive and psychological factors : risk aversion, time preferences, motivation, self-control, moral values and emotions<sup>3</sup>, coping skills<sup>4</sup>, mental health, drug addiction, etc.
- Factors related to prior criminal behavior : number of prior convictions, type of offense(s), time passed since last offense, type and duration of sentence(s), etc.
- Environmental and institutional factors : socioeconomic environment<sup>5</sup> (unemployment, poverty, inequality, anomie, etc.), repressive institutions (police, justice), family ties (capacity to provide financial and psychological support, to exert positive social control), neighborhood effects, peer effects, etc.

Among the many existing theories of crime (Barlow and Kauzlarich, 2010), economists dominantly rely on rational choice theory. More precisely, Becker's theoretical model (1968) suggests that agents face a trade-off between legitimate and illegitimate activities and choose the strategy that maximizes their expected utility. Naturally, further theoretical developments

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3. See Svensson et al. (2013) for an empirical study of the role of moral values, anticipated shame and guilt on juvenile delinquency in the Netherlands

4. See Zamble and Quinsey (1997)

5. See Kubrin and Stewart (2006) for the first empirical investigation on the subject



introduced additional factors and greater complexity (see for example Van Winden and Ash (2012) for a behavioral crime model taking cognitive and emotional factors into account) but utility-maximization remains central. The most straightforward prediction of the rational choice crime model is that criminals, like all other agents, react to incentives, either positive or negative : it notably puts emphasis on the role of deterrence on criminal behavior (through probability and cost of apprehension). It also offers a theoretical explanation for many stylized facts such as the beneficial role of education and aging for example.

Many experiments have been run over the years to test the validity of Becker's rational criminal model. Farrington (2003) reviews all the randomized experiments completed in United Kingdom : most of them do not specifically address recidivism but study illegal or antisocial behaviors such as lying or stealing money. However, several experiments focus on evaluating the effect of different programs on ex-prisoners reentry : Folkard et al. (1976) for example evaluates how changes in supervision intensity during probation affect recidivism. More recently, Bierie (2009) compares recidivism rates between convicts who were randomly assigned to a traditional prison or to a boot camp : his results suggest that tougher conditions of detention lead to greater probability of recidivism. In a lab experiment, DeAngelo and Charness (2012) test the deterrent effect of penalties on recidivism (measured as repeated speeding by participants) : among other things, they find that individuals choose to commit more offenses and recidivate more often when penalties decrease.

Econometric studies of the effect of deterrence on recidivism have long been unsatisfactory due to the presence of endogeneity and selection bias. However, recent empirical work strongly confirms a specific deterrent effect of prison sentences : Maurin and Ouss (2009) for example study the effect of collective sentence reductions on ex-inmates' recidivism in France : their results suggest that reducing prison sentences for all prisoners leads to increased probability of recidivism. Taking advantage of a large reform in Italy, Drago et al. (2009) exploit random variations in expected sentences in case of recidivism : they show that the probability of recidivism diminishes when expected sentence increases.

Beyond deterrence, a growing part of recidivism studies attempt to identify the causal effects of imprisonment. Prison is often considered as a school of crime where inmates acquire "criminal capital" (which can be a mix of social and human capital), lose attachment to their social network (*social bond theory* - Hirschi, 1969) and from where they leave as labelled criminals (*labeling theory* - Bernburg et al. (2006)). But imprisonment can conversely be viewed as a way to rehabilitate convicts and deter future offending. Even though a complete

survey of these topics is far beyond the scope of this article, it is noteworthy that this research field is growing and combines different approaches to document the effect of imprisonment on recidivism. For example, Di Tella and Schargrotsky (2013) study the impact of the type of sentence in Argentina : more precisely, they show that defendants who serve pre-trial time under electronic monitoring instead of pre-trial detention are significantly less likely to recidivate (controlling for selection bias through IV). Using US data, Chen and Shapiro (2007), followed by Drago et al. (2011) in Italy, focus on the effect of prison conditions on recidivism : they find that tougher prison conditions (in terms of security levels or geographical isolation) lead to greater risk of recidivism after release. Finally, recent work investigates the role played by social interactions in prison : for example, Bayer et al. (2009) identify peer effects among a sample of 8,000 young inmates in Florida. Their results specifically suggest that "exposure to peers with a history of committing a particular crime increases the probability that an individual who has already committed the same type of crime recidivates with that crime". Such exogenous interactions<sup>6</sup> may be driven by networks formation and exchange of specific human capital while in prison. Using individual data from Italy, Drago and Galbiati (2012) are able to identify endogenous social interactions among inmates of the same nationality, such that peers' recidivism behavior has a positive causal effect on one's behavior.

Since the early work of Schmidt and Witte (1989), a large amount of recidivism studies use duration analysis (instead of binary models) in order to investigate the dynamics of recidivism and to account for state dependency. Indeed, it is well known that the instantaneous risk of recidivism decreases over time, starting very high at date of release (or soon after) and then slowly decreasing close to zero after several years. Using semi-parametric Cox models, Duwe and Clark (2013) attempt to evaluate the effect of visits in prison (number, frequency, timing, types of visitors) on recidivism among Minnesota's ex-prisoners. Amirault and Lussier (2011) investigate the decrease of the predictive power of prior criminal charges on sex offenders' recidivism over time. Skardhamar and Telle (2009) estimate Cox models with shared frailty and time-varying variables to investigate the effect of getting a job on the hazard of recidivism. Similarly, Uggen (2000) evaluates the effectiveness of a US program which randomly grants jobs to ex-convicts. An alternative to Cox models is the use of parametric models, where the shape of the baseline hazard is explicitly specified : Kim et al. (1993) estimate a Weibull model (including proxies for opportunity cost and probability of arrest)

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6. See Manski (2000)'s classification of social interactions

to study the validity of Becker's model among drug offenders. Using UK data, Bowles and Florackis (2007) use exponential and Weibull parametrizations (with shared frailty and stratification) to study the relationship between age and recidivism. Bierens and Carvalho (2002) provide an important contribution by distinguishing two competing risks : violent recidivism and non-violent recidivism. Competing-risks models allow for a better understanding of the complexities of recidivism since the effect of each explanatory variable is estimated for both violent and non-violent recidivism, these two risks being correlated. Another major contribution is the use of split-population models, which split samples into two groups - one capturing those who present a real, non-null risk of recidivism, the other representing those who would never recidivate (no matter how long they were followed). Schmidt and Witte (1989) are one of the first scholars to apply this method to the study of recidivism among ex-inmates in the US. More recently, Escarela et al. (2000) mix a split-population model to a competing-risks model on British data.<sup>7</sup>

Overall, most -if not all- research confirms the dramatic influence of gender, age and criminal background on recidivism patterns among ex-prisoners : young males with a criminal record present the highest instantaneous risk of recidivism. These results initially came from US data and have been confirmed in other countries ; this excludes France where no survival analysis of ex-inmates' recidivism has been conducted to our knowledge.

### 1.3 Data

Our dataset comes from a survey run by Annie Kensey and Pierre-Victor Tournier (2005) for the French correctional administration. It is a nationally representative cohort of 2,858 convicted ex-prisoners released between May 1, 1996 and April 30, 1997 in France. Many socio-demographic, judiciary and carceral information is gathered for all sampled releasees. Recidivism is measured as any new conviction registered in one's criminal record in June 2002, hence a follow-up period of at least 5 years after release. Criminal records could not be retrieved or used for 654 ex-prisoners<sup>8</sup>, therefore we are left with a study sample of 2,204 individuals.

This survey seems to come close to the real recidivism patterns of ex-prisoners for several

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7. While attractive, these methods necessitate very long follow-up periods (a decade or more) to distinguish those who are at risk (called *persisters*) from those who would never recidivate (*desisters*). Therefore most follow-up surveys do not allow for the use of such methods.

8. 653 criminal records could not be retrieved or used for one of the following reasons : un-matched identity, deceased, incomplete record, recording error. One record was not used because it reflected a very special case of incarceration ("contrainte par corps").

reasons. First, the follow-up period (more than 5 years) is greater than those used in standard surveys of ex-inmates, and the few existing long-term surveys suggest that a large majority of recidivists reoffend before 5 years after release. Second, recidivism is tracked through a very reliable measure : recidivism is established if at least one new conviction (for an offense committed after release) appears in the official, nationwide criminal records in June 2002, whatever the new sentence. Most surveys generally use rearrest (or reconviction in the same region, e.g. one state of the US) as a measure of recidivism. Finally, we have access to very precise temporal data which gives us the exact number of days between one's release from prison and the date of first reoffending, if any. The reliability of duration models estimates clearly benefits from such precision.

However, using official criminal records has the obvious limitation of capturing only reoffenders who get arrested and convicted. Plus, in France, a convict can obtain -in very rare cases- the withdrawal of his conviction from his criminal record.

### 1.3.1 Descriptive Statistics and Non-Parametric Evidence

Socio-demographic data are extracted from each detainee's personal prison file : prisoners fill these informations at date of entry. Therefore, these variables do not necessarily capture one's situation at or after release, and their reliability mainly relies on honesty. Finally, certain variables are of limited precision, such as the education variable, which is dichotomous and only distinguishes between those who went to middle school and those who did not (thus many human capital differences remain unobserved). Table 1.1 reports a list of all the variables used, as well as their mean, standard deviation and a brief definition.

96% of the sample are men, as in the current prison population (96,6% on December 1st, 2012). Only 63% went to middle school before entry. One half declared they had a job and 17% declared they were married. The share of foreign prisoners equals 29%, while 16% described themselves as homeless. The average releasee was 32 years old at exit, but some were under 18 (1%). 70% of the sample served pre-trial detention, either for a short or a longer time (*procédure rapide* or not), and the 30% remaining were incarcerated after conviction to prison. On average, prisoners had already been convicted three times previously, though 27% had no criminal record. More than three quarters of the sample were convicted to prison for a single case. Time served ranges from one day to almost 20 years : it equals 14 months on average, the median time served being 5 months<sup>9</sup>. 12% benefited from an early

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9. See Kensey and Tournier (2002) for a detailed summary of time served among this sample

TABLE 1.1: Descriptive statistics (n = 2204)

Variable	Mean	s.d.	Definition
Gender	0.96	0.20	Male (1) or female (0)
Education level	0.63	0.48	At least Middle school (1) ou lower (0)
Employment	0.50	0.50	Declared having a job (1) or not (0)
Marital status	0.17	0.38	Married (1) or other (0)
Nationality	0.29	0.46	Foreigner (1) or French (0)
Homeless	0.16	0.36	Declared being homeless (1) or not (0)
Age at release	31.89	10.11	Age at release, in years
Age < 23	0.18	0.39	
$23 \leq \text{Age} < 28$	0.24	0.43	
$28 \leq \text{Age} < 33$	0.20	0.40	
$33 \leq \text{Age} < 38$	0.14	0.34	
Age $\geq 38$	0.23	0.42	
Penal status at entry			
Short pre-trial detention	0.40	0.49	Fast procedure*
Pre-trial detention	0.30	0.46	Classic procedure
Execution of prison sentence	0.30	0.46	No pre-trial detention
Prior convictions	2.96	3.41	Number of prior convictions
None	0.27	0.44	
One	0.18	0.38	
Two or three	0.23	0.42	
Four and more	0.32	0.47	
Number of cases	1.32	0.76	Number of cases which led to conviction
Time served	13.64	23.51	Duration of effective incarceration, in months
<i>Libération conditionnelle</i>	0.12	0.32	Released under parole (1) or not (0)
<i>Placement à l'extérieur</i>	0.02	0.14	Placement à l'extérieur (1) or not (0)
<i>Semi-liberté</i>	0.08	0.28	Semi-liberté (1) or not (0)
Type of initial offense			Of the principal offense if multiple
Against persons	0.29	0.45	Violent crimes, rape, homicide
Against property	0.36	0.48	Stealing, swindle, etc.
Drug-related	0.16	0.36	Trafficking, selling, use
Other	0.19	0.40	Administrative, driving and other offenses
Year of release	0.37	0.48	Released in 1997 (1) or in 1996 (0)
Recidivism	0.58	0.49	At least one new conviction
Return to prison	0.44	0.50	New sentence to prison

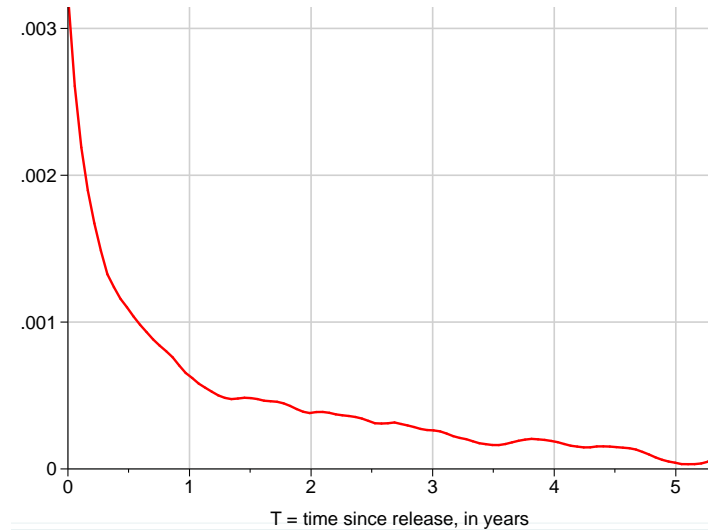
\* Fast procedure : immediate trial, in the act, etc.

release under parole (*libération conditionnelle*), 2% obtained a *placement à l'extérieur* and 8% a *semi-liberté*<sup>10</sup>. Regarding offense types, 36% were convicted for property crime, 29% for violent crime, and 16% for drug-related offenses<sup>11</sup>. Most prisoners were released in year 1996 (between May 1st and December 31). In terms of recidivism, 58% of the 2,204 releasees

10. These two French legal measures allow a prisoner to go out of prison routinely for several hours or days to do a particular activity, such as working

11. When one is convicted for multiple offenses, the category of the most serious crime is used

FIGURE 1.1: Smoothed (non-parametric) hazard function



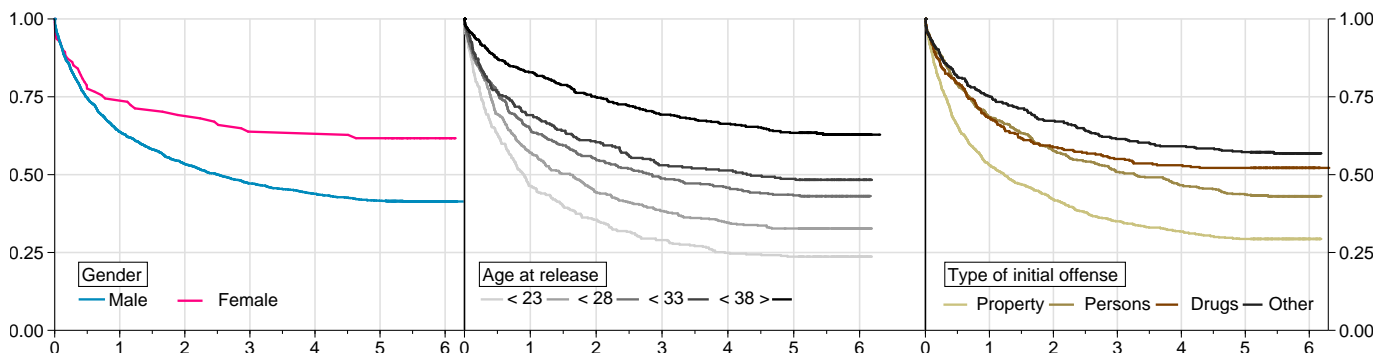
were re-convicted before June 2002 and 44% went back to prison for a new offense.

Figure 1.1 gives a graphical representation of recidivism dynamics. More precisely, it shows the evolution of hazard over time<sup>12</sup>, that is the change in the probability of recidivism at time  $t$  among those who are still at risk at that time (those who have not reoffended yet, or those who are right-censored). As shown by Figure 1.1, the hazard is maximal at the time of release, rapidly diminishes until  $T = 1$  year, and then gets closer and closer to zero after five years. The shape of this hazard function (monotonically decreasing, convex) is a bit different for what is often observed in other countries (first increasing for a few months, then decreasing and convex) : this is probably because in most foreign studies the duration that is used is the time to rearrest or to reconviction, not the time to reoffense. Therefore, such durations include a lag accounting for the fact that arresting and convicting a criminal takes time.

The non-parametric statistics described above give a very aggregated description of recidivism patterns. However, huge disparities exist between prisoners in terms of recidivism. Before turning to our econometric analysis, it can be interesting to illustrate survival rates for different groups of prisoners. Figure 1.2 plots survival functions by gender, age and type of initial offense, three variables that are traditionally considered as strong predictors of recidivism : we can notice that the patterns are quite different between men and women and between age brackets (three fourth of those under 23 at release reoffend during the follow-up period, compared to 39% of the 38+ group). Moreover, survival rates vary by type of initial

12. The hazard function is smoothed with Epanechnikov's alternative kernel function, available in Stata

FIGURE 1.2: Survival functions by gender, age at release and type of initial offense



offense, those convicted for property crime being the most likely to reoffend. These differences are a first indication of the complexity of recidivism behaviors among ex-prisoners. We now turn to our econometric duration analysis which, by reasoning "everything else equal", allows us to isolate the influence of each covariate on the dynamic of recidivism.

### 1.4 Model specification

Our model specification attempts to offer a better understanding of recidivism among ex-prisoners, by identifying its main determinants. It also attempts to capture most inter-individual heterogeneity to limit omitted-variable bias.

To do so, all the available socio-demographic variables are included in the model : some of them are considered as major determinants of recidivism in the existing research (gender, age, labor-market status, education) while others have less documented effects but appear relevant (marital status, nationality, homelessness).

Our specification also includes the number of prior convictions, as empirical evidence strongly suggests that prior offending behavior -which precisely indicates whether one has already reoffended- is one of the best predictors of future recidivism. This stylized fact is equivocal : it can suggest that recidivism is a state dependent process (prior convictions have a causal effect on future behavior), but it can also support the selection hypothesis such that individuals are intrinsically heterogenous in terms of crime propensity (spurious correlation due to uncontrolled unobserved heterogeneity). Amirault and Lussier (2011) also show that past convictions lose predictive power over time : we can not study this point here, as our dataset only includes the total number of prior convictions, not their dates.

Additional variables are used in our specification to better capture individual recidivism

propensity : first, we include the number of cases which led to imprisonment and the type of offense committed. We also use pre-trial detention (fast procedure, classic procedure, or no pre-trial detention), time served<sup>13</sup>, and individual sentence reductions (*libération conditionnelle*, *placement à l'étranger* and *semi-liberté*). Most prior econometric studies fail to control for pre-trial detention, but this variable may capture a great deal of information concerning one's criminal propensity, as perceived by the judge : indeed, french judges can decide to put high-risk suspects on pre-trial detention and choose a "fast procedure" to incapacitate them. Thus, these variables capture both objective and subjective risk assessments and can therefore serve as proxies for criminal propensity.

Finally, a dummy variable representing the year of release (1996 or 1997) controls for potential cohort effects : this variable ensures that, if there is systematic unobserved heterogeneity between 1996 releasees and 1997 releasees (for example, they may not face the same institutional or economic environment at release), such cohort effects will not bias our estimates. Similarly, we include prison fixed-effects (last prison of incarceration) so that potential inter-prison differences are controlled for (e.g. prison conditions, in-prison rehabilitative programs). These prison dummies may also capture unobserved heterogeneity between the local economic and social environments prisoners face at release (imperfectly, since we do not have data regarding prison location or residential mobility after release).

Even though our goal is to minimize the risk of omitted variable bias, we can not reject it *a priori*, as several potentially important covariates remain unobserved, such as self-control, motivation, social capital, etc. Duration models applied to recidivism also rely on the too-rarely-discussed assumption that no systematic change in police and justice practices happened during the follow-up period. If, for example, some types of crime became a priority among police forces during the study period, not controlling for this change through time-varying covariates would lead to mis-specification. Even though several legal reforms were enacted in France during the 1996-2002 period, we regard the assumption above as credible.

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13. The causal effect of time served on recidivism is a very debated theoretical and empirical topic. This article does not adress this difficult question, but it is important to control for time served in recidivism studies



## 1.5 Results

Most survival analyses applied to recidivism rely on Cox's semi-parametric model, where instantaneous probability is characterized as :

$$h_i(t) = h_0(t) \cdot e^{X_i\beta} \tag{1.1}$$

where  $h_0(t)$  represents the baseline hazard (left unparametrized). This flexible model assumes that each explanatory variable affects hazard in a multiplicative and constant-over-time way. Thus, the shape of the baseline hazard is assumed to be the same for all ex-prisoners, whatever their profile. However, we may think that recidivism dynamics differ between a prisoner convicted for murder and another convicted for drug trafficking, for example (Bowles and Florackis (2007); Kim et al. (1993)). Statistical tests (based on the Schoenfeld residuals) tend to confirm this intuition and suggest that several variables violate the proportional-hazards (PH) hypothesis : gender, nationality, pre-detention, sentence reductions and type of initial offense (we denote  $Z$  this vector of variables that are supposed to violate the PH hypothesis)<sup>14</sup>. Such results are not surprising : foreign prisoners, for example, may differ from french detainees not only in terms of their effective criminal dynamics but also in terms of emigration behavior, two possibilities that would violate the PH assumption. Plus, prisoners released under parole are supervised temporarily at release by probation officers : this supervision may alter parolees' recidivism dynamics compared to unsupervised releasees.

Since non-proportional hazards lead to biased coefficients (toward zero when hazards converge over time) and biased standard errors, it is important to account for the violation of the PH assumption. Our approach consists in estimating a semi-parametric Cox model with interaction terms between the variables  $Z$  and (the log of) time<sup>15</sup>, such that :

$$h_i(t) = h_0(t) \cdot e^{X_i\beta + Z_i\theta \ln(t)} \tag{1.2}$$

This specification allows each variable  $Z \in X$  to have a time-varying effect, i.e. two effects : one on the level of hazard (at all time) and another on the evolution of hazard over time<sup>16</sup>. Not only is this distinction necessary from a methodological standpoint (to relax

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14. The results of these tests are available in Table 1.3 in the appendix

15. The log function implies that the effect of an explanatory variable changes over time in a non-linear way, which seems more realistic than a linear function. However, Table 1.5 shows the results we obtained with  $f(t) = t$  and  $f(t) = \sqrt{t}$

16. See for example Box-Steffensmeier (2004) for more details on this method. Noteworthy is the fact that

the PH assumption), it is also useful to better understand recidivism processes among ex-prisoners : indeed, this method enables us to distinguish between prisoners who present a high but fastly-decreasing hazard of recidivism (thereby suggesting the need for an intensive short-term supervision at release) and releasees whose risk is more modest but stable over time. Acknowledging these two dimensions seems critical to better prepare prison releases and improve recidivism prevention.

The results from the time-varying-effects Cox model are shown in Table 1.2 (expressed as hazard ratios<sup>17</sup>). In this model, all the explanatory variables discussed in Section 3 are included, except for type of initial offense which only appears in the vector  $Z$  of the variables violating the PH assumption. This specification minimizes the Bayesian information criterion compared to two alternatives (inclusion of type of offense in  $X$  only, or in both  $X$  and  $Z$ ), as can be seen in Table 1.4 (appendix). The estimates from Model 1 suggest that the temporal dynamic of recidivism varies significantly from one type of offense to another (Model 3 rejects the hypothesis of a "double effect", on both the hazard at release and its evolution over time). However, noteworthy is the fact that these three specifications lead to very similar results regarding the role played by the other explanatory variables. We now focus on the estimates derived from Model 1.

First, our results show that the effect of gender is two-fold : everything else equal, male releasees experience a 69% lower hazard of recidivism at release compared to females, but this ranking reverses very rapidly (after 57 days<sup>18</sup>) because hazard decreases at a much slower pace among male than among female ex-prisoners. These results suggest that women experience a relatively high risk of recidivism just after release from prison, but that this risk decreases relatively fast : in terms of public policy, it seems that short-term intensive supervision may be particularly appropriate among women. An alternative interpretation (in terms of compositional effects) is that, except for a minority who reoffends quickly, most female releasees desist more easily from crime than males (maybe thanks to a more positive reentry into their families and communities), hence the need for better-targeted supervision and reentry programs.

The next estimates show that neither marital status, nor education, nor homelessness, are associated with a significant change in the instantaneous probability of recidivism. These

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this approach leads to very similar results to those obtained with a Generalized Weibull model, when the time dependency parameter  $p$  is a function of the variables  $Z$

17. Hazard ratios are obtained by computing  $exp(\hat{\beta})$

18. because  $exp(\frac{\hat{\beta}_{gender}}{\hat{\theta}_{gender}}) = exp(\frac{\ln 0.309}{\ln 1.333}) = 57$  days

TABLE 1.2: Cox Models With Time-Varying Effects

<b>Explanatory variables X</b>	Model 1	
	<b>Haz. ratio</b>	<b>p-val</b>
Gender (1 = Male)	0.309*	0.010
Marital status (1 = Married)	0.925	0.414
Education level (1 = Middle-school)	1.037	0.590
Employment (1 = Yes)	0.875*	0.048
Nationality (1 = Foreign)	2.307***	0.000
Homelessness	1.107	0.262
Age at release	0.895***	0.000
Age squared	1.001***	0.000
Nb of prior convictions	1.243***	0.000
Nb of prior convictions squared	0.994***	0.000
Penal status		
No pre-trial detention (ref)	1	-
Short pre-trial detention	3.163***	0.000
Classic pre-trial detention	1.709+	0.094
Nb of cases	1.105*	0.013
Time served (x100 days)	0.988	0.203
Libération conditionnelle	0.264**	0.006
Placement à l'extérieur	0.104*	0.040
Semi-liberté	0.513	0.199
Year of release (1 = 1997)	0.918	0.173
<b>Duration dependency : <math>Z \cdot \ln(t)</math></b>		
Gender	1.333**	0.001
Nationality	0.823***	0.000
Libération conditionnelle	1.221*	0.016
Placement à l'extérieur	1.433+	0.050
Semi-liberté	1.115	0.215
Penal status		
No pre-trial detention (ref)	1	-
Short pre-trial detention	0.843***	0.000
Classic pre-trial detention	0.893*	0.044
Type of initial offense		
Against property (ref)	1	-
Against persons	0.989	0.431
Drug-related	0.951**	0.006
Other	0.929***	0.000
<b>Prison fixed-effects</b>	yes	
LR-test	227.85**	0.002
<b>Log-likelihood</b>	-8829.7	
<b>Observations</b>	2204	

**Notes :** Significance levels are noted + (10%), \* (5%), \*\* (1%) et \*\*\* (0.1%). We use Efron's method for ties. Prison fixed-effects are captured by dummies for the 171 prisons in the sample. A Likelihood-Ratio Test is performed to test their significance.

results seem counterintuitive at first, but recall that these informations are declared by each prisoner at time of entry (not at release), and that their precision is limited (dummy variables). Releasees who declared they had a job at entry face a significantly lower hazard after release (12% lower), which is consistent with rational choice theory (tradeoff between legal and illegal activities). At release, foreign prisoners display a hazard that is much higher than among French releasees (*hazard ratio* = 2,31) but it diminishes much faster over time (18% faster), probably because foreigners are more likely to leave the country in the months following release, notably. The estimates for age and age squared show that young releasees are far more likely to recidivate than their older peers. However, this relation lessens (slowly) with age : for example, the hazard for a 30-year old releasee is half that experienced by his 20-year old peer<sup>19</sup>) whereas this ratio equals 36% for a prisoner aged 40. Conversely, the instantaneous probability of recidivism is an increasing, concave function of the number of prior convictions : having one prior conviction (compared to zero) increases one's hazard by 24%. This positive marginal effect lessens slowly but remains greater than 16% up to five prior convictions : thus, the hazard is 150% greater when one has five prior convictions versus none. Our estimates also suggest that both the hazard and its dynamic vary by penal status at entry : those who faced pre-trial detention display a much higher risk at release, compared to those who did not, but this risk decreases faster. More precisely, those who were incarcerated under fast procedure have a three-fold higher hazard at release ; however, this risk decreases 16% faster, which seems to suggest that part of the fast-procedure convicts display a very high criminal propensity (i.e. are embedded in a criminal lifestyle where illegal opportunities show up regularly). Our results also show that hazard increases with the number of cases which led to conviction : each additionnal case is associated with a 11% greater instantaneous probability of recidivism. Conversely, hazard seems not to depend on time served, everything else equal<sup>20</sup>.

The estimates associated with the variable *libération conditionnelle* show that the influence of this sentence reduction is two-fold : at release, parolees experience a four time lower hazard of recidivism compared to those who did not benefit from this sentence reduction ; however, this gap lessens slowly over time (it remains significant for up to two years). The fact that parolees are much less prone to reoffend is equivocal : it may be interpreted as a causal effect of shortened time served, a causal effect of increased supervision at release<sup>21</sup>, or

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19. This figure is obtained as follows :  $\frac{\hat{h}_{age=30}}{\hat{h}_{age=20}} = \frac{\exp(30 \cdot \hat{\beta}_{age} + 30^2 \cdot \hat{\beta}_{age^2})}{\exp(20 \cdot \hat{\beta}_{age} + 20^2 \cdot \hat{\beta}_{age^2})} = 0.54$

20. For a discussion of the causal effect of time served, see Section 1

21. In France, parolees are supervised by probation officers after release, for a period equal (at least) to the remainder of their prison sentence

simply a sign of selection bias<sup>22</sup>. Our results show that the second legal measure considered, *placement à l'extérieur*, seems to play a very similar role on recidivism. Surprisingly, we do not find the same effect for *semi-liberté* (those who benefited from *semi-liberté* do not display significantly different recidivism patterns compared to those who did not) even though both measures are very similar<sup>23</sup> : this result suggests that *semi-liberté* is not an effective way to reduce recidivism. Our estimates also show that there are significant differences between types of initial offense regarding the dynamic of recidivism : hazard decreases faster among those convicted for drug-related and other offenses, compared to property and violent criminals. Finally, it is interesting to note that prison fixed-effects appear very significant : the type of prison (which we can not control for) is probably an important factor as detainees are allocated to different types of prison based on their profile. But we could also imagine that prison conditions differ (in terms of access to rehabilitative programs, health services, security, material conditions, visitation policy, etc.) and may alter one's path after release.

## 1.6 Conclusion

This article attempts to better understand the main determinants of the recidivism dynamics. Our main contribution is to apply for the first time duration-data models to the study of this phenomenon in France.

Our results confirm the dramatic influence of gender, age, nationality, employment status and prior convictions on future offending among ex-prisoners. Our estimates notably show that females and foreigners face a particularly high risk of recidivism at release, but that these trends reverse rapidly. In terms of public policy, these results suggest that more intensive supervision and better release planning might be beneficial for these groups of prisoners. However, we do not find evidence of potential effects of marital status, education, homelessness, or time served. Our estimates highlight differences between those who served pre-trial detention and the others : we find that part of those who were incarcerated under fast procedure reoffend very quickly, even though they faced rapid convictions. It seems that imprisonment did not have the expected deterrent and rehabilitative effect, but simply in-

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22. There is selection bias if parolees (treatment group) differ significantly from the others (control group) in terms of unobservable characteristics that affect the probability of being released under parole (such as motivation, reentry prospects, etc.)

23. *Semi-liberté* and *placement à l'extérieur* both allow prisoners to leave prison during day hours or several consecutive days to achieve a particular task (such as work or training). The different results between these two measures may be due to the fact that *placement à l'extérieur* is much more selective (2% vs 8% of the sample) and that it generally allows longer out-of-prison spells

capacitated them for some time. We also find that property and violent criminals tend to reoffend faster than others. Finally, our results show that prisoners under parole and *placement à l'extérieur* display durably lower risk of recidivism. At this stage, however, *semi-liberté* does not seem to be effective in reducing recidivism.

From a methodological standpoint, many improvements can be achieved : competing-risks models, split-population models, inclusion of an unobservable heterogeneity parameter, etc. Moreover, it might be of great interest to study more precisely the role of prison conditions and social interactions (between inmates and with the outside) on reoffending. Finally, considering the current debate about recidivism in France, an important contribution would be the identification of the causal effects of parole and time served.

## Appendix

TABLE 1.3: Proportional-Hazards Tests based on the Shoenfeld residuals

<b>Explanatory variables X</b>	Model A	Model B
Gender (1 = Male)	0.086	0.038
Marital status (1 = Married)	0.669	0.694
Education level (1 = Middle-school)	0.618	0.773
Employment (1 = Yes)	0.409	0.350
Nationality (1 = Foreign)	0.059	0.002
Homelessness	0.939	0.655
Age at release	0.095	0.087
Age squared	0.253	0.235
Nb of prior convictions	0.459	0.422
Nb or prior convictions squared	0.516	0.430
Penal status		
No pre-trial detention	-	-
Short pre-trial detention	0.115	0.054
Classic pre-trial detention	0.474	0.402
Nb of cases	0.064	0.169
Type of initial offense		
Against property	-	-
Against persons	0.044	0.066
Drug-related	0.038	0.026
Other	0.050	0.159
Time served (x100 days)	0.500	0.354
Libération conditionnelle	0.220	0.043
Placement à l'extérieur	0.030	0.069
Semi-liberté	0.309	0.022
Year of release (1 = 1997)	0.609	0.709
<b>Prison fixed-effects</b>	yes	no
<b>Observations</b>	2204	

**Notes :** The values reported correspond to the p-values associated with the PH test for each explanatory variables, after estimating the two Cox models and regressing Shoenfeld residuals on duration

TABLE 1.4: Cox Models With Time-Varying Effects

Explanatory variables X	Model 1		Model 2		Model 3	
	Haz. ratio	p-val	Haz. ratio	p-val	Haz. ratio	p-val
Gender (1 = Male)	0.309*	0.010	0.317*	0.012	0.312*	0.012
Marital status (1 = Married)	0.925	0.414	0.924	0.409	0.924	0.411
Education level (1 = Middle-school)	1.037	0.590	1.040	0.556	1.036	0.601
Employment (1 = Yes)	0.875*	0.048	0.875*	0.045	0.874*	0.046
Nationality (1 = Foreign)	2.307***	0.000	2.458***	0.000	2.149***	0.000
Homelessness	1.107	0.262	1.102	0.286	1.097	0.308
Age at release	0.895***	0.000	0.894***	0.000	0.895***	0.000
Age squared	1.001***	0.000	1.001***	0.000	1.001***	0.000
Nb of prior convictions	1.243***	0.000	1.243***	0.000	1.244***	0.000
Nb of prior convictions squared	0.994***	0.000	0.994***	0.000	0.994***	0.000
Penal status						
No pre-trial detention (ref)	1	-	1	-	1	-
Short pre-trial detention	3.163***	0.000	3.068***	0.000	3.200***	0.000
Classic pre-trial detention	1.709+	0.094	1.660	0.113	1.748+	0.083
Nb of cases	1.105*	0.013	1.105*	0.013	1.106*	0.012
Time served (x100 days)	0.988	0.203	0.988	0.224	0.988	0.207
Libération conditionnelle	0.264**	0.006	0.262**	0.006	0.264**	0.006
Placement à l'extérieur	0.104*	0.040	0.104*	0.041	0.104*	0.041
Semi-liberté	0.513	0.199	0.520	0.209	0.508	0.194
Type of initial offense						
Against property (ref)	-	-	1	-	1	-
Against persons			0.933	0.357	0.813	0.404
Drug-related			0.792*	0.018	1.214	0.498
Other			0.700***	0.000	1.228	0.417
Year of release (1 = 1997)	0.918	0.173	0.918	0.172	0.918	0.169
<b>Duration dependency : <math>Z \cdot \ln(t)</math></b>						
Gender	1.333**	0.001	1.327**	0.002	1.331**	0.001
Nationality	0.823***	0.000	0.812***	0.000	0.834***	0.000
Libération conditionnelle	1.221*	0.016	1.223*	0.015	1.221*	0.016
Placement à l'extérieur	1.433+	0.050	1.432+	0.051	1.429+	0.052
Semi-liberté	1.115	0.215	1.111	0.231	1.117	0.209
Penal status						
No pre-trial detention (ref)	1	-	1	-	1	-
Short pre-trial detention	0.843***	0.000	0.848**	0.001	0.841***	0.000
Classic pre-trial detention	0.893*	0.044	0.898+	0.055	0.889*	0.039
Type of initial offense						
Against property (ref)	1	-	-	-	1	-
Against persons	0.989	0.431			1.025	0.581
Drug-related	0.951**	0.006			0.920	0.118
Other	0.929***	0.000			0.894*	0.031
<b>Prison fixed-effects</b>	yes		yes		yes	
LR-test	227.9**	0.002	226.8**	0.002	228.4**	0.002
<b>Log-likelihood</b>	-8829.7		-8832.4		-8828.5	
<b>BIC</b>	17875		17880		17896	
<b>Observations</b>	2204		2204		2204	

**Notes :** Significance levels are noted + (10%), \* (5%), \*\* (1%) et \*\*\* (0.1%). We use Efron's method for ties. Prison fixed-effects are captured by dummies for the 171 prisons in the sample. A Likelihood-Ratio Test is performed to test their significance.



TABLE 1.5: Cox Models With Two Alternative Specifications Of Time Dependency

Explanatory Variables X	$f(t) = \sqrt{t}$		$f(t) = t$	
	Haz. ratio	p-val	Haz. ratio	p-val
Gender (1 = Male)	0.617	0.122	0.886	0.612
Marital Status (1 = Married)	0.925	0.415	0.928	0.434
Education level (1 = Middle-school)	1.033	0.631	1.032	0.641
Employment (1 = Yes)	0.870*	0.040	0.869*	0.038
Nationality (1 = Foreign)	1.343*	0.023	1.049	0.627
Homelessness	1.112	0.243	1.106	0.267
Age at release	0.895***	0.000	0.894***	0.000
Age squared	1.001***	0.000	1.001***	0.000
Nb of prior convictions	1.246***	0.000	1.250***	0.000
Nb of prior convictions squared	0.994***	0.000	0.993***	0.000
Penal status at entry				
No pre-trial detention (réf)	1	-	1	-
Short pre-trial detention	2.001***	0.000	1.622***	0.000
Classic pre-trial detention	1.216	0.265	1.078	0.563
Nb of cases	1.105*	0.013	1.103*	0.015
Time served (x100 days)	0.987	0.192	0.988	0.208
Libération conditionnelle	0.503**	0.003	0.648**	0.007
Placement à l'extérieur	0.301*	0.015	0.470*	0.024
Semi-liberté	0.513	0.672	0.741	0.150
Year of release (1 = 1997)	0.915	0.153	0.909	0.125
<b>Duration dependency : <math>Z \cdot f(t)</math></b>				
Gender	1.045**	0.008	1.001*	0.035
Nationality	0.974***	0.000	0.999**	0.002
Libération conditionnelle	1.024*	0.015	1.000*	0.035
Placement à l'extérieur	1.048*	0.016	1.001*	0.015
Semi-liberté	1.017	0.116	1.000*	0.049
Penal status				
No pre-trial detention (réf)	1	-	1	-
Short pre-trial detention	0.975***	0.000	0.999**	0.001
Classic pre-trial detention	0.986+	0.061	1.000+	0.076
Type of initial offense				
Against property (réf)	1	-	1	-
Against persons	1.001	0.877	1.000	0.310
Drug-related	0.985**	0.003	0.999**	0.002
Other	0.982***	0.000	0.999**	0.002
<b>Prison fixed-effects</b>	yes		yes	
LR-test	231.6**	0.002	232.5**	0.002
<b>Log-likelihood</b>	-8831.3		-8837.3	
<b>Observations</b>	2204		2204	

**Notes :** Significance levels are noted + (10%), \* (5%), \*\* (1%) et \*\*\* (0.1%). We use Efron's method for ties. Prison fixed-effects are captured by dummies for the 171 prisons in the sample. A Likelihood-Ratio Test is performed to test their significance.

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## Chapitre 2

### *Time To Get Out ?*

# How Sentence Reductions Affect Recidivism After Prison

#### **Abstract**

Does shorter incarceration promote successful reentry or lead to recidivism? This paper proposes a new theory to reconcile these two competing views, which both have solid empirical and theoretical grounding. Building on a reference-dependent utility model, I incorporate the concrete design of sentence reductions, in terms of timing, anticipation and adaptation by prisoners, into a model of criminal decision-making. The model has several testable implications, which are generally supported by the data. Exploiting a collective pardon in France in July 1996, I show that sentence reductions have very heterogeneous effects on prisoners' propensity to reoffend, depending on their ability to anticipate the pardon and their capacity to adapt to the news before release, either psychologically or materially. Overall, these results suggest that sentence reductions may neither be good or bad *per se* in terms of recidivism, and that prisoners' expectations and adaptation are key.

## 2.1 Introduction

On November 1<sup>st</sup> 2015, more than 6,000 drug offenders were released early from U.S. federal prisons thanks to a large, retroactive reduction of sentence guidelines by the U.S. Sentencing Commission<sup>1</sup>. Overall, more than 20,000 prisoners have already benefited from this revision, obtaining an average sentence reduction of 21 months, or 17% of their original sentence (USSC, 2015). This historic early release plan illustrates a turn toward new strategies to fight crime and recidivism in the United States and in other countries, after several decades of "tough on crime" policies<sup>2</sup>.

As argued by the U.S. Sentencing Commission and many scholars, sentence reductions have many attractive features : they save public money, reward good conduct inside prison, incentivize rehabilitation efforts, and allow judges and parole boards to target the right time to release inmates<sup>3</sup> (Kuziemko, 2013; Polinsky, 2015). In addition, shorter incarceration reduces prisoners' exposure to the potentially criminogenic effects of prison, such as criminal hardening, labor-market detachment and stigma, and can therefore prevent recidivism (Durlauf and Nagin, 2011).

However, sentence reductions can also backfire by reducing the deterrent effects of incarceration among actual prisoners *and* potential offenders : first, inmates who obtain early release from prison may revise downward their perception of the cost of punishment, as they expect future incarceration to be shorter too. This reduction in so-called specific deterrence would induce more recidivism. Second, the threat of future punishment may lose deterrent power even among non-treated, potential offenders, who are aware that sentence reductions will be available in case of conviction (lower general deterrence). Moreover, sentence reductions have the mechanical effect of releasing prisoners at younger ages, i.e. with intrinsically greater propensities to reoffend<sup>4</sup>.

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1. The Sentencing Commission reduced the (very long) recommended prison sentences for a series of drug crimes, making thousands of federal prisoners eligible for early release. This news received a lot of media coverage in the United States as it is one of the largest early release in American history. See for example the New York Times : <http://www.nytimes.com/2015/10/07/us/us-to-release-6000-inmates-under-new-sentencing-guidelines.html>

2. The "tough on crime" approach, exemplified by the laws on "Three Strikes" (mandatory sentence enhancements for repeat offenders) and "Truth-in-Sentencing" (lower access to early release), led to large increases in the average length of time served behind bars, both in the U.S. (2.9 years in 2009, up by +36% since 1990, Pew (2011)) and in many European countries. In France for example, the average prison stay went up from 8.6 months to 11.5 months between 2006 and 2013, yielding a 15% increase in total prison population (+9,000 prisoners) while the number of admissions into prison was actually declining (from 81,000 to 70,000 annual entries)

3. The "right time" being when the marginal cost and benefit of an additional day in prison equalize

4. There is widespread evidence that criminal propensity declines with age (after a peak around 18-20 years old), the so called "age-crime curve". Ganong (2012) shows how aging partly explains the effect of incarceration

Interestingly, these two competing views about sentence reductions (criminogenic effect *vs* crime-preventing effect) both receive empirical support from high-quality studies which deal properly with omitted-variable bias<sup>5</sup> : for example, Berecochea and Jaman (1981), Maurin and Ouss (2009) and Kuziemko (2013) show that the probability of recidivism significantly increases among prisoners who are released early ; conversely, Mueller-Smith (2014) and Hunt and Peterson (2014) find that shorter incarceration prevents recidivism, whereas Kling (2006) and Abrams (2011) do not find any significant effect on future crime or labor market outcomes.

A major shortcoming of this research, besides its large focus on U.S. data, is that it pays no attention to the framing of punishment, i.e. the concrete design which leads similar prisoners to serve different amounts of time behind bars<sup>6</sup>. In this paper, I argue that the design of sentence reductions (in terms of timing, anticipation and adaptation by prisoners) is a key mediating factor, and can help explain the apparent inconsistency in prior research : sentence reductions tend to be criminogenic when they are unexpected and lead to the rapid release of prisoners shortly after the news (Berecochea and Jaman, 1981; Maurin and Ouss, 2009; Kuziemko, 2013) ; conversely, when inmates can easily anticipate their eventual date of release well in advance (i.e. no shock between trial and final release), shorter incarceration can have null (Kling, 2006; Abrams, 2011) or even beneficial effects on recidivism (Mueller-Smith, 2014; Hunt and Peterson, 2014).

My main contribution in this paper is therefore to explicitly consider the concrete design of sentence reductions in their interplay with recidivism. To do so, I first build on a reference-dependent utility model (Kőszegi and Rabin, 2006) to incorporate expectations and adaptation to surprise in a typical Beckerian model of criminal decision-making (Becker, 1968). Second, I test the main predictions of the model on a dataset of prisoners released in France in 1996-1997. Most of those inmates benefited from the presidential collective pardon of July 1996, which granted additional sentence reductions to inmates based on their residual sentence on the day of the pardon. Using the pardon as an exogenous source of variation, I show that such sentence reductions had very differential effects on French inmates : prisoners who were released soon after the pardon and were the least likely to anticipate it performed significantly worse during the next 5 years (greater probability of recidivism) than prisoners

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length on reoffending.

5. Omitted-variable bias stems from the fact that sentence reductions are granted endogenously to the best prisoners (in terms of current behavior and future reentry prospects). Therefore, a negative correlation between sentence reductions and future crime could arise even in the absence of causal effects, if judges effectively grant sentence reductions to the good prisoners.

6. A notable exception is Bushway and Owens (2013) who show how discrepancies between expected and actual sentences generate recidivism



who were likely to expect the pardon (because of prior experience) and who had a lot of time remaining to adapt to the news (both materially and psychologically) before actual release.

## 2.2 Theory and current evidence

In order to illustrate the potential effects of sentence reductions on recidivism, let's consider a Beckerian model of crime participation. An ex-prisoner contemplating a new criminal opportunity in time  $t$  after release from prison decides to reoffend if the expected utility derived from crime exceeds its opportunity cost, or when :

$$B(r) - p.C(r) > \underline{U}(r) \tag{2.1}$$

where  $B$  is the expected benefit from crime (monetary or psychological),  $C$  is the expected cost of future punishment,  $p$  is the probability of punishment, and  $\underline{U}$  is the expected utility from not reoffending (such as the value of free life, social ties, legitimate work).

Conditional on original sentence length  $s_i$ , the amount of sentence reductions one obtained during previous incarceration, noted  $r$  for simplicity<sup>7</sup>, may affect all three expected utilities. Regarding the cost function  $C(\cdot)$ , receiving more sentence reductions is likely to reduce one's expectation of the length of future incarceration, and therefore the perceived cost of future punishment. This hypothesis, known as specific deterrence, therefore suggests that  $\frac{\partial C}{\partial r} < 0$ . However, the signs of  $\frac{\partial U}{\partial r}$  and  $\frac{\partial B}{\partial r}$  are far less certain : there is no direct evidence on the effect of prison time on future criminal benefits<sup>8</sup>, and the current research on incarceration length and future labor market outcomes is not conclusive (Kling, 2006; Landerso, 2015)<sup>9</sup>. We therefore have little theoretical guidance as to the direction of the overall effect of sentence reductions on recidivism after release.

Empirically, a growing number of papers use convincing designs to overcome omitted-variable bias (the fact that sentence reductions are endogenously targeted to the prisoners with the best prospects and lowest risk of recidivism) and estimate this overall causal effect. As early as 1970, California ran a large-scale experiment on 1,138 prisoners who were about

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7. Note that  $r$  refers to the amount of sentence reductions obtained during the previous prison spell conditional on original sentence  $s_i$ , so a more precise notation could be  $r_{t-1}|s_i$ . Also note that the amount of sentence reductions is simply the difference between initial sentence  $s_i$  and actual length of incarceration  $s_f$  :  $r = s_i - s_f$

8. Related literatures investigate peer effects inside prison (Bayer) and the returns to criminal experience (Loughran), both finding evidence of criminal capital accumulation and specialization

9. In Sweden, a country with famously short prison sentences, Landerso (2015) finds that increasing incarceration length can promote labor-market outcomes after release. However, Kling (2006) do not find evidence of any significant effect of time served on future employment and wages in Florida and California

to be released under parole after spending three years behind bars on average : a random group of 564 prisoners was granted early release six months in advance of their scheduled date of parole, while the other prisoners (control group) were released on their original date of parole. As reported by Berecochea and Jaman (1981), the one-year rate of return to prison was 14% for the early-release group and 10.9% for the control group, which suggests that recidivism was significantly larger among prisoners released early (p-value = 5.6%)<sup>10</sup>. This result is supported by recent quasi-experimental evidence from Georgia, where Kuziemko (2013) exploits discontinuities in parole guidelines. She finds that the probability of 3-year recidivism decreases by 1.3 percentage points for each additional month of incarceration<sup>11</sup>. The common feature of these two papers is that they exploit changes in date of parole, i.e. release decisions made by a parole board *during* incarceration. Those discretionary decisions are difficult to predict and anticipate by prisoners, and typically lead to rapid release from prison (in the next weeks or months) : therefore, prisoners obtaining early release in Berecochea and Jaman (1981) and Kuziemko (2013) may have failed to anticipate and prepare their upcoming release, yielding more recidivism.

Interestingly, when researchers exploit designs where prisoners can better anticipate their eventual release date, the estimated effects of incarceration length on recidivism are no longer significant (Kling, 2006; Abrams, 2011) or even criminogenic (Mueller-Smith, 2014; Hunt and Peterson, 2014). Most papers use sentencing disparities between randomly-assigned judges, but a notable exception is Hunt and Peterson (2014) who exploit a retroactive reduction of sentence guidelines on crack-cocaine offenses decided by the U.S. Sentencing Commission in 2007 (very similar to the recent 2015 revision discussed above). This amendment took effect on March 1, 2008, and led thousands of prisoners to seek and obtain early release under parole. Hunt and Peterson (2014) compare rearrests and returns to prison between two groups of observably similar prisoners : those who obtained sentence reductions thanks to the crack-cocaine amendment of March 2008 and were released later that year, between July and November 2008 ; and prisoners who would have been eligible to sentence reductions but were released in the year before the amendment took effect (March 2007 - March 2008). As expected, the treated group served much less time in prison than the control group (85 months *versus* 97 months on average). After 5 years, the rate of recidivism was lower among the 836 early-release prisoners (43.3%) than in the control group (47.7%, N=486), with a

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10. We use a two-proportion z-test, with test statistic equal to 1.58

11. Also see Ganong (2012) and Zapryanova (2014) who extend the study of Kuziemko (2013) in Georgia to investigate the role of aging and supervision under probation

*p-value* of 5.7%<sup>12</sup>. As opposed to Berecochea and Jaman (1981), this finding of a beneficial effect of early release may be due to the fact that, in their study, Hunt and Peterson (2014) focus on early-release prisoners who had a lot of time to prepare and adapt to the news : their actual release (July-Nov 2008) occurred long after the announcement of the new sentence guidelines (2007).

### 2.2.1 A theory of expectations and adaptation to surprise

Many reasons may explain why sound empirical research on the effects of sentence reductions on future crime produces such divergent results : heterogeneity of effects by criminal profile or prison conditions, use of different measures of recidivism or follow-up periods, etc. However, a main shortcoming of this literature is that it pays no attention to the allocation process of sentence reductions : as illustrated in Equation 2.1, sentence reductions are assumed to affect recidivism independently of their design and timing. Yet, I argue in this paper that a similar amount of sentence reductions can have dramatically different effects on recidivism depending on whether prisoners were able to anticipate them and, for those who were surprised by the news, whether they had the time and capacity to adapt before actual release.

In practice, there are several types of sentence reductions with very different concrete designs : “earned good time credit”, for example, is easy to track over time by prisoners since it is based on their conduct inside prison (plus, they usually receive frequent feedback) : thus, prisoners can easily update their priors and predict eventual date of release. Conversely, mass release, pardons and parole are highly discretionary decisions, and are therefore less predictable (in both probability and amount). Plus, such decisions can occur late during incarceration, leaving prisoners with little time to adapt.

Thus, conditional on initial sentence length, the decision to commit a new crime in time  $t$  after prison release may depend not only on the actual amount of sentence reductions obtained during incarceration,  $r$ , but also on anticipations,  $\tilde{r}$ , and adaptation to surprise,  $\alpha$  :

$$B(r) - p.C(r, \Delta^{\tilde{r}}, \alpha) > U(r, \Delta^{\tilde{r}}, \alpha) \quad (2.2)$$

where  $\Delta^{\tilde{r}} = r - \tilde{r}$  captures the anticipation error (how much of actual sentence reductions were not expected by the prisoner), and the coefficient  $\alpha$  measures prisoner’s capacity to

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12. Using the same two-proportion z-test as for Berecochea and Jaman (1981)

adapt to the news by the time he is released, both materially (in terms of securing housing, finding a job, etc.) and psychologically (processing the information into updated beliefs).

To formally illustrate the intuition, let's consider a model of reference-dependent preferences. Kőszegi and Rabin (2006) propose a simple approach to plug expectations into a model of utility-maximizing decision-making. Early applications of such models in economics of crime are found in Bushway and Owens (2013), who estimate how the discrepancy between expected and actual sentences affects recidivism (finding evidence of criminogenic framing effects)<sup>13</sup>, Card and Dahl (2011) and Munyo and Rossi (2013) (on how unexpected wins and losses in soccer and football games generate sudden changes in violent crime). In the case of sentence reductions, I can decompose utility functions  $C(\cdot)$  and  $U(\cdot)$  in two additive components : the pure effects of *actual* sentence reductions,  $c_1$  and  $u_1$  (respectively the specific deterrent effect of sentence reductions, and their effect on future legitimate prospects); and the reference-dependent effects of sentence reductions deriving from poor anticipation ( $c_2$  and  $u_2$ ).

$$C(r, \Delta^{\tilde{r}}, \alpha) = c_1(r) + (1 - \alpha).c_2(\Delta^{\tilde{r}}) \quad (2.3)$$

$$\underline{U}(r, \Delta^{\tilde{r}}, \alpha) = u_1(r) + (1 - \alpha).u_2(\Delta^{\tilde{r}}) \quad (2.4)$$

The cost function  $c_2(\cdot)$  represents prisoners' way of processing the surprise (in this case, an unanticipated shock in sentence reductions) into new beliefs regarding the cost of future punishment : intuitively, some prisoners may well interpret unexpected sentence reductions as a signal of impunity (further suggesting that the justice system is "all bark and no bite") so that  $\frac{\partial c_2}{\partial \Delta^{\tilde{r}}} < 0$ , consistent with the framing effects found by Bushway and Owens (2013). I expect this criminogenic feeling of impunity to be particularly strong on the most short-sighted prisoners, i.e. young and impulsive offenders, and to affect all types of offenses similarly.

The function  $u_2(\cdot)$  captures prisoners' change in reservation utility (the prospects of a law abiding life) due to unanticipated sentence reductions. Considering the difficult social background of most prisoners, early release can adversely affect their material conditions at release : surprise release can for example reduce their capacity to secure housing, seek social assistance, find a job, and more generally reenter society successfully. In support of this hypothesis, Wolff et al. (2012) provide qualitative evidence that time in prison allows prisoners

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13. Exploiting a policy change in Maryland which altered recommended sentences but not actual sentences, Bushway and Owens (2013) show that, conditional on actual sentence length, a larger gap between expected and actual punishment generates significantly more crime after release.

to better plan/prepare their release. Moreover, Munyo and Rossi (Munyo and Rossi) show how short-term recidivism in Uruguay is driven by the liquidity constraints faced by prisoners at release : they estimate that each prison releasee generates 0.25 additional property crime locally on the day of release. Interestingly, they find that this large correlation completely disappeared in 2010 after an increase in the stipend given to each prisoner at release. Therefore, I expect that unanticipated early release reduces material preparedness for release, and thus reduces the utility of a law-abiding life outside prison, i.e.  $\frac{\partial u_2}{\partial \Delta^{\tilde{r}}} < 0$ . This "unprepared release" effect is likely to be stronger on prisoners with low resources (in the form of wealth, housing, human and social capital) and should mainly trigger income-generating crime (e.g. property crime).

Finally, expectation-dependent utilities  $c_2$  and  $u_2$  are weighted by a parameter of adaption  $\alpha \in [0, 1]$ .  $\alpha$  captures prisoners' capacity to adapt to and cope with unexpected sentence reductions (both materially and psychologically) *before they actually exit prison*. Operationally, I compute it as the amount of time (in absolute terms or relative to initial sentence) that remains to be served by the date of the news (date of the pardon). The intuition is that a shock in sentence reductions should have negligible effects on prisoners who are at the beginning of their sentence ( $\alpha \approx 1$ ), since they have a lot of time to adapt (materially) and "get used to it" (psychologically) : thus, by the time they are released, the shock is no longer salient. Conversely, the model predicts that the detrimental effects of unanticipated sentence reductions should be maximal on prisoners who are near the end of their sentence when the news comes ( $\alpha \approx 0$ ).

Overall, this reference-dependent model of recidivism yields the following formula for the propensity to reoffend :

$$Recid^* = B(r) - p[c_1(r) + (1 - \alpha).c_2(\Delta^{\tilde{r}})] - [u_1(r) + (1 - \alpha).u_2(\Delta^{\tilde{r}})] \quad (2.5)$$

The model unambiguously predicts that unexpected shocks in sentence reductions are criminogenic, through increased feeling of impunity (psychological channel) and lowered preparedness for release (material channel) :  $\frac{\partial Recid^*}{\partial \Delta^{\tilde{r}}} = -(1 - \alpha)(p \frac{\partial c_2}{\partial \Delta^{\tilde{r}}} + \frac{\partial u_2}{\partial \Delta^{\tilde{r}}}) > 0$ . Therefore, prisoners who correctly anticipate sentence reductions ( $\Delta^{\tilde{r}} \approx 0$ ) should benefit the most (or suffer the least) from sentence reductions. Second, the adverse effect of unanticipated sentence reductions should be maximal among prisoners who had little time or capacity to adapt ( $\alpha \approx 0$ ) between the news and eventual release.

### 2.3 Testing the role of anticipations and adaptation to surprise

In order to test the main predictions of the model, I build on prior work by Maurin and Ouss (2009) and exploit the collective pardon of July 1996 in France. From the 1980's to 2006, French Presidents Mitterrand and Chirac regularly issued collective pardons near the National Holiday (the 14<sup>th</sup> of July), granting sentence reductions to almost all incarcerated prisoners. This practice, which was at the discretion of the President, suddenly stopped after Sarkozy's election in 2007.

French collective pardons have several attractive features to test my theory. First, all prisoners who are in prison on the day of the pardon are eligible, except for a short list of specific offence types (terrorism, crime against children or police, and serious drug trafficking for the July 1996 pardon). Second, the amount of sentence reductions granted follows a universal rule which applies to all eligible inmates, based on their residual sentence (in 1996, it was one week of sentence reduction for each month that remains to be served in prison on the day of the pardon). Third, collective pardons do not have any consequence other than granting sentence reductions : importantly, they do not affect the minimum or maximum penalty in case of recidivism (conversely to the famous Italian clemency bill of 2006 studied by Drago et al. (2009)), or the period of post-release supervision (conversely to parole-based experiments such as Kuziemko (2013)). Therefore, French collective pardons provide individual-level variation in the amount of sentence reductions that is unrelated to prisoners' personal traits or behavior : after constructing a purely exogenous instrumental variable, this design allows me to get rid of omitted-variable bias and identify the true causal effect of sentence reductions on future crime. Finally, a nice feature of French collective pardons is that they affect all prisoners on the same day, with some inmates being at the beginning of their sentence and others at the end of their term : this feature allows me to test the role of adaptation to surprise, using time before release (in absolute or relative terms) as a proxy for capacity to adapt.

However, the fact that collective pardons were a yearly tradition every month of July in France (until 2007) may seem problematic : it suggests that prisoners were able to perfectly anticipate pardons. I argue that it is highly unlikely and that prisoners differed in their capacity to anticipate the pardon : first, note that collective pardons were at the full discretion of the President, who could decide whether to issue a pardon or not, without any justification. Thus, French pardons were not a matter of long parliamentary debate, conversely to Italian

pardons for example (Campaniello et al., forthcoming). Second, the precise design of the pardon (which offense types are eligible or not, how to compute time remaining, how much sentence reductions to grant, etc.) often varied from one year to the next<sup>14</sup>. Therefore, though prisoners could expect the occurrence of a pardon in July, it was impossible for them to perfectly anticipate how much sentence reductions they would obtain, and therefore when they would be eventually released. As noted by Evans (2006), during the 1990's, many prisoners were suddenly released on the day of the pardon with no prior notice. The Ministry of Justice became aware of this problem in the early 2000's, and explicitly instructed parole officers to anticipate upcoming pardons to better prepare the exit of those *soon-to-be-released* prisoners. Overall, both anecdotal evidence and the concrete design of French pardons tend to reject the idea that prisoners easily anticipate those sentence reductions. Therefore, the pardon of July 1996 presumably induced at least *some* surprise among French prisoners.

In order to empirically test the adverse effect of such surprise on recidivism, I assume that prisoners who had large prior criminal experience (because they were already incarcerated during the previous pardon of July 1995, or because they were repeat offenders) were more likely to expect the collective pardon of 1996, more able to correctly anticipate its effect on eventual date of release, than prisoners with little to no criminal experience.

### 2.3.1 Data

The dataset is drawn from a nationally representative survey by Kensey and Tournier (2005) for the French Prison Administration. It contains individual information on 2408 convicted prisoners who were released from French prisons between May 1st, 1996 and April 30th, 1997. Data include sociodemographic and judicial characteristics recorded at entry (gender, age, educational level, employment status, marital status, homelessness, French citizenship), detailed information regarding the initial conviction (type of offense committed, prison sentence length) and the number of prior convictions. Importantly, the dataset precisely records the dates of incarceration and release, the amount of pre-trial detention, as well as the total amount of sentence reductions by type (earned good time credit, sentence reductions from collective pardons, individual early-release programs, etc.). Recidivism is measured as any new conviction in criminal records by June 2002, allowing a follow-up period of 5 years after prison release<sup>15</sup>. Finally, I retrieve rudimentary prison-level characteristics

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14. For a complete historical and legal analysis of pardons in France, see Evans (2006)

15. Criminal records cover all offenses that led to any new conviction by June 2002, and include date and type of new offense(s) in a quite detailed format, as well as type and length of new sentence(s). This measure

for each prisoner (region, type of prison, capacity, overcrowding rate as of Jan 1, 1996)<sup>16</sup>.

By design, most of the offenders in the sample were in prison when the collective pardon of 1996 was enacted on July 9; a fraction of them, serving long sentences, also benefited from previous pardons (July 1995, July 1994, etc.), but none benefited from the next pardon of July 1997. In this paper, I focus on the effect of the 1996 pardon on sentence reductions and future recidivism among prisoners who were in prison on July 9, 1996 : I exclude sampled prisoners whose expected release date was before July 9 because they were not eligible to the 1996 pardon<sup>17</sup>, and prisoners who entered prison after July 9 because pardon eligibility for this group can not be recovered from the data (eligibility was based on (missing) date of conviction)<sup>18</sup>. These two sample restrictions are necessary to obtain robust identification of the causal effect of sentence reductions on future crime.

Additionally, in order to improve the precision of my instrument (thus avoiding a problem of weak IV) and study a more homogenous group of prisoners, I exclude the fraction of prisoners who were convicted to prison sentences longer than 3 years (N=303) and the handful of prisoners who benefited from amnesties (N=36). The study sample reduces to 952 observations.

In this new sample, sentence reductions remain highly right-skewed, as depicted in Figure 2.1 : 95% of prisoners obtain less than one year of sentence reductions, and 5% of prisoners obtain between 360 and 720 days. A similar pattern is observed for the instrumental variable (*TimeRem*, see below). Such large skewness, i.e. extreme values, could well bias standard estimators. Therefore, I winsorize these two variables at the 95th percentile (capping extreme values at the 95th percentile), i.e. at 360 days for sentence reductions and 212 days for time remaining. Plus, I treat the handful of negative values for sentence reductions as zeros, since

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of recidivism is particularly reliable for three reasons : (a) it requires final conviction for a new crime (and not simply rearrest or parole violation) committed after release ; (b) it covers a 5-to-6 years follow-up period, much longer than most empirical studies in the field ; (c) it captures new offenses no matter whether the new sentence is custodial or not.

16. For the minority of inmates who served time in several different prisons for the current sentence, I can only retrieve information for the last prison facility before release, since previous facilities are not recorded. Among the 183 different prisons represented in the original dataset, it was impossible to clearly identify three facilities except for their type and region (probably because they shut down since 1996) so I replace the few missing values for capacity and overcrowding by their *region \* type* averages.

17. Note that this group of prisoners released just before July 9, 1996, could serve as a control group in a Regression Discontinuity design with the day of pardon as the cutoff, following Maurin and Ouss (2009). However, this group is rather small in the dataset, and there is evidence that trends in recidivism are not parallel on both sides of the July 9 cutoff, yielding biased RD estimates. Therefore, I choose to avoid this problem and use an alternative empirical strategy on a smaller sample.

18. Some offenders who entered prison after July 9 could benefit from the 1996 pardon (and obtain a flat sentence reduction of 2 months) depending on their date of *conviction* to prison. However, date of conviction is not recorded in the dataset, so this cutoff can not be exploited as a source of identification.



such cases only occur when prisoners serve too much time in pre-trial detention compared to their eventual prison sentence. As expected, these manipulations have a limited impact on the overall distribution of these two variables, but treat the problem of outliers (see Figure 2.1 in Appendix).

Descriptive statistics about initial sentence and sentence reductions appear in Table 2.1. On average, sampled prisoners were initially convicted to 14 months of prison (424 days) but actually served about 10 months, thanks to 130 days of sentence reductions (31% of the initial sentence). “Good behavior” explains more than half of total sentence reductions (72 days obtained on average), but pardons also contribute to a large extent (about one third of sentence reductions, or 40 days). Early-release programs do not play a major role on average, since they only concern about 20% of prisoners.

TABLE 2.1: Sentence length and time served

Variables (in days)	Mean	Std. Dev.	Share of initial sentence
Initial sentence length	424.2	281.9	100%
Sentence reductions	129.9	98.5	30.6%
Good behavior	71.7	57.9	16.9%
Pardon	39.5	46.2	9.3%
Early-release programs	23.4	74.8	5.5%
Sample Size		952	

Table 2.2 provides summary statistics for the outcome variables. My main dependent variable for recidivism is reconviction in the 5 years following prison release : in the sample, 60% of prisoners are reconvicted in this time window. Reconviction is already 38% after one year. About two thirds of reoffenders are reconvicted to a new prison sentence for their first new offense. Finally, recidivism is largest in property crime (24% commit a property crime in the 5 years after release) but violent, traffic and other crimes also exhibit large rates of recidivism, over 10%. Note that these crime-specific frequencies do not add up to 60% since offenders may be reconvicted for several offenses in a single criminal case.

## 2.4 Empirical strategy : exploiting the French collective pardon of 1996

In order to exploit the collective pardon of July 1996, I need to construct a credibly exogenous instrumental variable. As explained earlier, all prisoners who were in prison on July 9 were eligible to the pardon, except for those convicted for terrorism, serious drug trafficking, and crimes against children under 15, police or correctional officers. Descriptive

TABLE 2.2: Recidivism Variables

Outcome	Share
Any Reconviction after 5 years	59.9%
... 1 year	37.7%
Reconviction to ...	
Prison sentence	40.0%
Alternative sentence	19.9%
Reconviction for ...	
Property crime	23.8%
Violent crime	10.7%
Drug crime	5.4%
Traffic crime	11.0%
Other	19.4%
Sample Size	952

statistics from the study sample suggest that about 80% of incarcerated prisoners obtained sentence reductions from the pardon.

More problematic is the computation of sentence reductions : according to the pardon decree, eligible prisoners are granted one week of sentence reduction for each month that remained to be served in prison on July 9, 1996, in the limit of 4 months of sentence reductions. In their evaluation of the pardon, Maurin and Ouss (2009) consider this rule as exogenous : they calculate the *actual* amount of prison time remaining on July 9 and use it to construct their IV. However, this approach presumably introduces bias in their IV estimates, since the rule of allocation of sentence reductions is actually endogenous : conditional on original sentence length, the amount of reductions granted by the pardon depends (negatively) on how much sentence reductions were already obtained by prisoners prior to July 9 (in the form of “earned good time credit” for example)<sup>19</sup>. Therefore, all else equal, the collective pardon tends to reward the worst prisoners -who accumulated little sentence reductions prior to July 9- more than the good ones.

This design of French pardons is likely to bias upward IV estimates where *actual* time remaining serves as an instrument. To obtain a more credible, exogenous instrument for the

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19. In France as in many countries, prisoners can obtain “earned good time credit” periodically throughout incarceration, as long as they do not misbehave in prison. Most prisoners are granted the maximum amount of good time credit (4 months for the first year of incarceration, then 3 months per year), but these sentence reductions are often withdrawn in case of misconduct (such as violence or use of drugs). Conversely, some prisoners can obtain extra “good time credit” if they behave particularly well inside prison, demonstrate exceptional rehabilitation efforts, or help to solve a criminal case for example. Because they are based on merit and rehabilitation efforts, these sentence reductions are likely endogenous, i.e. correlated with intrinsic propensity to reoffend.

total amount of sentence reductions obtained by prisoners, I construct the variable *TimeRem* capturing the *theoretical* time remaining by July 9, 1996 : this simple measure is computed as the difference between the *ex-ante* date of exit from prison *had the prisoner served the whole of his original sentence* (i.e. date of incarceration  $t_i$  + length of original prison sentence  $s_i$ ), and the exact date of the pardon (July 9, 1996).

$$TimeRem = t_i + s_i - t_{July9} \quad (2.6)$$

This computation of time remaining is agnostic about what happened inside prison before July 9, i.e. whether prisoners earned good-time credit or not. It only depends on the date of incarceration (which is supposedly exogenous) and the length of the original prison sentence (which can be easily controlled for in regressions). Therefore, I view this variable as a credibly valid instrument for the total amount of sentence reductions, in contrast to the measure of “actual” time remaining used by Maurin and Ouss (2009).

**Model specification** The main goal of my econometric model is to estimate the effect of sentence reductions on the probability of recidivism among ex-prisoners, treating (potential) omitted-variable bias by exploiting the exogenous variation in sentence reductions induced by the collective pardon of July 1996. Therefore, the baseline econometric model writes as follows :

$$Recid_i^* = \beta_0 + \beta_1 r_i + \mathbf{\Gamma}' \mathbf{s}_i + \mathbf{\Pi}' Pretrial_i + \mathbf{\Psi}' \mathbf{X}_i + \epsilon_i \quad (2.7)$$

$$r_i = \alpha_0 + \alpha_1 TimeRem_i + \mathbf{\Omega}' \mathbf{s}_i + \mathbf{\Theta}' Pretrial_i + \mathbf{\Lambda}' \mathbf{X}_i + e_i \quad (2.8)$$

where  $Recid^*$  is the latent propensity to reoffend after release expressed in Equation 2.5. The coefficient of interest,  $\beta_1$ , is identified by the instrument *TimeRem* included in Eq. 2.8. However, for this variable to be a credible instrument, I need to precisely control for original sentence length  $s_i$  and pre-trial detention : to do so, I include a third-degree polynomial for  $s_i$ , and two dummy variables measuring whether prisoners served some pre-trial detention before July 9, and after July 9.

Less important for identification, but possibly useful to improve the precision of the estimates, I occasionally include an additional vector of control variables  $\mathbf{X}$  measured at the individual- and prison-levels. The individual-specific variables include : type of initial offense (five dummy variables), prior criminal convictions (one dummy for any conviction, and one

continuous variable for the total number), gender, age and age squared on July 9, and type of release (three dummies capturing the three main types of early-release programs)<sup>20</sup>. Regarding prison-specific variables, I retrieve data on the type of prison<sup>21</sup> and the overcrowding rate in the facility as of January 1, 1996.

Since the dependent variable is dichotomous, I mostly rely on Maximum Likelihood estimation of IV Probits to estimate my two-equation model (though I also use the more straightforward 2SLS estimator on occasions). However, for benchmark purposes, I also estimate naïve regressions of Equation 2.7 alone by Probit or OLS, treating sentence reductions  $r$  as exogenous.

## 2.5 Results

### 2.5.1 Benchmark results

Table 2.3 reports naïve Probit and OLS estimates : they consider the total amount of sentence reductions obtained,  $r$ , as exogenous. They only serve as benchmarks before turning to the more credible IV estimates.

Columns 1 to 3 report Average Marginal Effects after Probit estimation. On average, when only the most basic control variables are included (original sentence and pre-trial detention), there is a significant negative relationship between sentence reductions and probability of recidivism in the 5 years after release : a 10-day increase in sentence reductions is associated with a 0.9 percentage point decrease in probability of recidivism. However, as we include more and more control variables, the estimates diminish in absolute terms and lose statistical significance. The results are essentially unchanged when OLS is used.

### 2.5.2 IV results : overall effects

In Table 2.4, I now treat sentence reductions as endogenous (using *TimeRem* as my instrument), paying no particular attention to anticipation and adaptation -following prior research. Both Maximum Likelihood (IV Probit) and 2SLS (Linear Probability Model) results are shown.

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20. *Libération conditionnelle* (parole), *Semi-liberté* (halfway houses) et *Placement à l'extérieur*

21. There are three main types of prisons in France : most prisons are devoted to pre-trial detainees and short sentences (Maisons d'Arrêt); others are dedicated to host medium sentences (Centres de Détention); and a few prisons only hosts very serious criminals and gang members (Maisons Centrales)

TABLE 2.3: Benchmark Estimates on 5-year Recidivism : Full Sample

VARIABLES	(1) Probit	(2) Probit	(3) Probit	(4) OLS	(5) OLS	(6) OLS
Sentence Reductions (/10)	-0.0090** (0.0040)	-0.0039 (0.0045)	-0.0038 (0.0045)	-0.0092** (0.0041)	-0.0042 (0.0045)	-0.0039 (0.0046)
Initial Sentence	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)	-0.0001 (0.0002)
— squared	-0.0338 (0.0657)	-0.0149 (0.0579)	-0.0120 (0.0585)	-0.0361 (0.0674)	-0.0107 (0.0616)	-0.0096 (0.0624)
— cubed	0.0913 (0.0632)	0.0507 (0.0559)	0.0473 (0.0568)	0.0956 (0.0651)	0.0529 (0.0586)	0.0492 (0.0595)
Any Pre-trial Detention	0.0139 (0.0368)	0.0229 (0.0329)	0.0173 (0.0330)	0.0139 (0.0365)	0.0194 (0.0329)	0.0123 (0.0331)
Any Pre-trial After July 9	-0.1095** (0.0522)	-0.0316 (0.0484)	-0.0251 (0.0493)	-0.1117** (0.0538)	-0.0329 (0.0516)	-0.0237 (0.0529)
Constant				0.6929*** (0.2129)	0.4123 (0.3219)	0.6234* (0.3469)
Observations	952	952	932	952	952	932
R-squared				0.0504	0.2437	0.2487
Indiv. Controls	-	X	X	-	X	X
Prison Controls	-	-	X	-	-	X

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Regarding the First Stage, the instrument *TimeRem* behaves as expected : *theoretical* time remaining on July 9 is a (very) significant predictor of overall amount of sentence reductions. The estimates suggest that, conditional on original sentence and pre-trial detention, each additional day that remains to be served on July 9 generates about 0.2 additional days of sentence reductions. This figure is very consistent with the rules of the July 1996 pardon, as it suggests that a 30-day (one month) increase in time remaining leads to an overall reduction of time served by about 6 days<sup>22</sup>. This is confirmation that the collective pardon of 1996 introduced large, arbitrary differences in incarceration length between similar prisoners, depending on how much of their original sentence remained to be served by July 9 (i.e. depending on their date of incarceration).

Regarding the Second Stage estimates of the effect of sentence reductions on future crime,

22. Following the rules of the pardon, readers may have expected a relationship of 7-day per month remaining, not 6 days or so. However, this result should not come as a surprise since the dependent variable *r* sums all sentence reductions obtained during incarceration, both before and after the pardon. Prisoners who benefited a lot from the pardon had lower opportunities to benefit from additional sentence reductions after the pardon, compared to prisoners who benefited less from the pardon. This is the reason why the overall effect of the July 1996 pardon on sentence reductions is a bit smaller than 7 days per month. Another reason may also have to do with the 4-month limit for the pardon.

all point estimates are slightly negative but insignificant. Thus, there is no detectable evidence that sentence reductions have any overall causal effect on recidivism after 5 years, similarly to Abrams (2011) for example. Moreover, compared to the Probit and OLS results, all these IV estimates are of smaller magnitude (in absolute terms). Though these differences are thin and non-significant, this pattern is consistent with the presence of omitted variable bias, i.e. a selective allocation of sentence reductions, with good prisoners (low intrinsic propensity to reoffend) receiving more sentence reductions than bad prisoners.

TABLE 2.4: IV Estimates on 5-year Recidivism : Full Sample

VARIABLES	(1) IV Probit	(2) IV Probit	(3) IV Probit	(4) 2SLS	(5) 2SLS	(6) 2SLS
<b>Second Stage</b>						
Sentence Reductions (/10)	-0.0025 (0.0073)	-0.0015 (0.0079)	-0.0024 (0.0083)	-0.0025 (0.0073)	-0.0012 (0.0079)	-0.0018 (0.0083)
<b>First Stage</b>						
TimeRem (/10)	0.2305*** (0.0110)	0.1937*** (0.0094)	0.1876*** (0.0095)	0.2305*** (0.0111)	0.1937*** (0.0095)	0.1876*** (0.0096)
Observations	952	952	932	952	952	932
Indiv. Controls	-	X	X	-	X	X
Prison Controls	-	-	X	-	-	X
R-squared				0.0477	0.2434	0.2485
First Stage R-squared				0.8980	0.9301	0.9314
F-stat				433	419	380

Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

### 2.5.3 IV results : mediating effects of anticipation and adaptation

In order to test the main implications of the model on anticipations and adaptation, I successively estimate Equations 2.7 and 2.8 on different subgroups. First, to test the mediating effect of adaptation, I use the length of original sentence remaining by July 9 as a proxy for prisoners' capacity to adapt to the news before release, both materially and psychologically. The sample is divided in two same-sized groups of time remaining (more or less than 6 months remaining). Since absolute time remaining is also used as the instrumental variable, I later consider interaction terms between sentence reductions and *relative* time remaining (constructed as the share of the original sentence that remains to be served on July 9).

Second, to test the mediating effect of surprise (i.e. poor anticipations), I split the sample in two same-sized groups of prisoners based on their prior exposure to pardons and the criminal justice system : prisoners who were already in prison during the previous pardon of July 1995 and inmates who had at least 4 previous criminal convictions are coded as “large prior experience” and are expected to better anticipate the pardon of July 1996 and eventual date of release. By contrast, I hypothesize that less experienced prisoners, those who were not in prison for the pardon of July 1995 and who had less than 4 prior convictions, are more likely to be surprised by the pardon of 1996.

Table 2.5 reports average marginal effects from the Second Stage estimates after running IV Probits on each subgroup. First, in Columns 1 and 2, I split the sample by absolute time remaining (more than 6 months versus less than 6 months). The results are in stark contrast with Table 2.4 : they suggest that sentence reductions have a large, significant criminogenic effect among prisoners who had less than 6 months between the news of the pardon (July 9) and original date of release. The average marginal effect is large : a 10-day rise in sentence reductions increases probability of recidivism within five years by 5 percentage points. Conversely, the point estimate is negative and insignificant among prisoners who had more than 6 months remaining to adapt and prepare for release. These results are consistent with the predictions of the model : the effect of sentence reductions on future crime is mediated by prisoners’ time to adapt between news and release.

The model also predicts that anticipations matter : the criminogenic effects of sentence reductions should concentrate on prisoners who are unable to anticipate them, whereas inmates who are not surprised should not react. To test this prediction, I further split the sample between prisoners who had “large prior experience” of pardons or the justice system (presumably high capacity to anticipate) and those with low or null prior experience (low capacity to anticipate). The results appear in Columns 3 to 6. The estimates are again consistent with the implications of the model : specifically, sentence reductions have a large criminogenic effect on inmates who combine low experience and little time left to adapt (recidivism is up by 8 pp for each 10-day increase in sentence reductions), a large crime-preventing effect on prisoners with low experience but a lot of time to adapt to the news (-4 pp), and no detectable reaction from experienced inmates, whatever the amount of time remaining. As expected from my design, these results are robust to the exclusion of individual-level and prison-level control variables (Table 2.12 in Appendix).

As an additional test of the role of anticipation and adaptation, I use a second approach.

TABLE 2.5: IV Estimates on 5-year Recidivism, by Anticipation and Adaptation

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	All		Little Experience		Large Experience	
	< 180 days	≥ 180 days	< 180 days	≥ 180	< 180 days	≥ 180
<b>Second Stage</b>						
Sentence Reductions (/10)	0.0465** (0.0186)	-0.0152 (0.0168)	0.0966*** (0.0262)	-0.0362* (0.0217)	0.0021 (0.0208)	0.0084 (0.0252)
<b>First Stage</b>						
TimeRem (/10)	0.2149*** (0.0183)	0.1325*** (0.0171)	0.1840*** (0.0197)	0.1975*** (0.0311)	0.2663*** (0.0331)	0.1226*** (0.0253)
Observations	463	469	268	224	195	245
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Instead of splitting the sample in two by absolute time remaining (which is also my instrument), I construct the share of original sentence remaining by July 9, *ShareRem*, and interact it with sentence reductions. In order to estimate causal effects by IV, I also interact *ShareRem* with the instrument in the first stage. The results after 2SLS estimation appear in Table 2.6.

The pattern is similar to the previous results : sentence reductions tend to have large criminogenic effects when prisoners only have a small share of their original sentence left to serve on July 9. Conversely, when prisoners have more and more of their original sentence remaining to adapt to the news and prepare eventual release, the criminogenic effect of sentence reductions diminishes significantly. As expected, this pattern of a diminishing effect of sentence reductions as *ShareRem* increases is much clearer on prisoners with low experience of pardons or the justice system (the reaction of experienced prisoners to sentence reductions seems much smaller and flatter with regards to time remaining).

#### 2.5.4 Mechanism : psychology or capital ?

These results are consistent with the main implications of the model, i.e. the fact that the impact of sentence reductions on recidivism crucially depends on prisoners' capacity to anticipate them, adapt, and prepare for early release. However, the results do not allow to disentangle between the two potential mechanisms,  $c_2(\cdot)$  and  $u_2(\cdot)$ , discussed in Section 2.

According to the *psychological* channel, unexpected sentence reductions leading to rapid



TABLE 2.6: IV Estimates on 5-year Recidivism, by Anticipation and Adaptation

VARIABLES	(1) All	(2) Little Experience	(3) Large Experience
Sentence Reductions (/10)	0.0693 (0.0438)	0.0890 (0.0675)	0.0333 (0.0398)
r * ShareRem	-0.0381** (0.0188)	-0.0677* (0.0346)	-0.0112 (0.0211)
ShareRem	0.0638 (0.1248)	0.3182** (0.1546)	-0.1337 (0.1820)
Observations	932	492	440
R-squared	0.1663	0.1372	0.2736
Indiv. Controls	X	X	X
Prison Controls	X	X	X

Robust standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

release are particularly salient in prisoners' mind and foster feelings of impunity. I expect this kind of reasoning to lead to recidivism among impulsive prisoners, such as young offenders convicted for expressive crime (e.g. violence).

According to the *capital* channel, unexpected sentence reductions leading to rapid release leave prisoners worse off, with less time to plan their release and make material arrangements (in terms of housing, employment, etc.). This adverse effect of sentence reductions is likely stronger on offenders with low social, human and economic capital at their disposal. Moreover, this criminogenic effect should mostly lead to income-generating crime (e.g. property crime).

In order to disentangle these two mechanisms, I investigate the presence of heterogenous effects based on prisoners' age and initial offense type (two possible proxies for impulsivity), and based on their socio-economic capital. I also estimate crime-specific regressions to study the differential effect of sentence reductions on five types of new crime : property, violent, drug, traffic, and other offenses.

**Impunity/Impulsivity** In Table 2.8, I split the sample by age on July 9 (above/below median) to test whether the criminogenic effect of sentence reductions diminishes with age. The estimates do not reveal large differences : younger and older offenders seem to react similarly to sentence reductions (more recidivism when they have little time left to adapt). The results are essentially unchanged when I interact age with sentence reductions, instead of running separate regressions on two age groups : the coefficient for the interaction term is virtually

zero for all groups (Table 2.11 in Appendix).

TABLE 2.7: IV Estimates on 5-year Recidivism, by Adaptation and Age

VARIABLES	(1)	(2)	(3)	(4)
	Age < 29		Age ≥ 29	
	< 180 days	≥ 180 days	< 180 days	≥ 180
Sentence Reductions (/10)	0.0430*	-0.0400	0.0520*	-0.0075
	(0.0239)	(0.0260)	(0.0288)	(0.0203)
Observations	243	224	220	245
Indiv. Controls	X	X	X	X
Prison Controls	X	X	X	X

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

To further test the impunity/impulsivity mechanism, I also split the sample between 4 types of original offense : property, violent, drug, and traffic or other offenses<sup>23</sup>. Initial offense can possibly serve as a proxy for impulsivity, with violent offenders more impulsive than property offenders for example. The separate regressions yield similar results for the different types of initial offenses, and notably for property offenders (0.004) and violent offenders (0.003).

Overall, there is little evidence that the effect of sentence reductions is mediated by these two proxy measures of impulsivity.

TABLE 2.8: IV Estimates on 5-year Recidivism, by Initial Offense Type

VARIABLES	(1)	(2)	(3)	(4)
	Property	Violent	Drug	Traffic+Others
Sentence Reductions (/10)	0.0043	0.0030	0.0039	-0.0128
	(0.0124)	(0.0152)	(0.0183)	(0.0190)
Observations	284	203	174	271
Indiv. Controls	X	X	X	X
Prison Controls	X	X	X	X

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Score of Capital** To test the implication that prisoners with low capital were more adversely affected by the pardon, I build an individual score of socio-economic resources for each

23. There are only 53 prisoners convicted for traffic offenses initially, so I add them to the "other" category

prisoner. This score is obtained from a Multiple Correspondance Analysis aggregating 5 socio-economic characteristics measured at entry : marital status, employment status, education level, homelessness, property offender. These dummy variables are rather crude but they likely proxy the social, economic, human capital available to prisoners to reenter society successfully. The results of the MCA are reported in Figure 2.2 (Appendix).

After constructing the score of capital at entry, I split the sample in two same-size groups : prisoners with low capital and prisoners with high capital (MCA score below/above median). The results are reported in Table 2.9. Sentence reductions have a very large criminogenic effect on prisoners with little time remaining and a low score of capital : it is estimated that a 10-day increase in sentence reductions increases probability of 5-year recidivism by 7 percentage points in this group. Conversely, sentence reductions have no large or significant effect on low capital prisoners with more time to adapt, or on high capital prisoners. This pattern is consistent with a criminogenic "impreparedness for release" mechanism.

TABLE 2.9: IV Estimates on 5-year Recidivism, by Score of Capital at Entry

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	All < 180 days	≥ 180	Low Capital < 180 days	≥ 180	High Capital < 180 days	≥ 180 days
Sentence Reductions (/10)	0.0464** (0.0186)	-0.0152 (0.0168)	0.0707*** (0.0220)	-0.0168 (0.0197)	0.0150 (0.0342)	-0.0308 (0.0254)
Observations	463	469	245	258	218	211
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Crime-specific Effects** Another approach to test this mechanism is to estimate the effect of sentence reductions on different types of new crimes. According to the *capital* channel, I expect the criminogenic effect to concentrate on new income-generating crime (property crime). Therefore, I successively consider 5 crime-specific dependent variables<sup>24</sup> : property crime, violent crime, drug crime, traffic crime, and other.

Significant estimates are obtained only for property crime. As shown in Table 2.10, sentence reductions generate property crime recidivism among prisoners with little experience

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24. These crime-specific variables are not mutually exclusive, since some reoffenders may commit several types of crimes for which they will be reconvicted in a single criminal case

and little time to adapt. Conversely, when these offenders have more time to adapt, sentence reductions tend to be beneficial and reduce property crime. Again, prisoners with a large prior experience of pardons and the justice system do not react. The same output is reported for the other types of crimes in Table 2.13 (Appendix) : estimates are much smaller and never significant.

TABLE 2.10: IV Estimates on 5-year Property Crime, by Anticipation and Adaption

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	All		Little Experience		Large Experience	
	< 180 days	≥ 180 days	< 180 days	≥ 180	< 180 days	≥ 180
Sentence Reductions (/10)	0.0147 (0.0191)	-0.0241 (0.0150)	0.0564** (0.0275)	-0.0206 (0.0189)	-0.0068 (0.0248)	-0.0198 (0.0251)
Observations	463	469	268	224	195	245
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Standard errors in parentheses  
 \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

Taken together, these findings that the criminogenic effect of sentence reductions concentrates on low-capital prisoners and in property crime provide suggestive evidence that capital matters : sentence reductions tend to generate crime among prisoners with little capacity to adapt materially to early release. Conversely, using age and type of initial offense as proxies, there is no evidence that impulsivity is a major explanation of prisoners’ reactions to sentence reductions.

## 2.6 Conclusion

This paper aims to introduce a new, more realistic framework to study how sentence reductions may affect future criminal behavior. Prior research makes the implicit assumption that sentence reductions only affect the "treatment intensity" of incarceration, or that they only change the amount of time served behind bars. In this paper, I argue that the concrete design of these changes, i.e. the framing of sentence reductions, may matter crucially : using a reference-dependent framework, I show how similar levels of sentence reductions can have different effects on prisoners’ propensity to reoffend after release, depending on their expectations and their capacity to adapt to these changes. The theoretical section distinguishes

between two potential mechanisms, one psychological (based on the saliency of impunity feelings) and one material (based on prisoners' capital available to adapt to early release).

The empirical part of the paper attempts to test the main implications of the model. While hypothesis testing is rather indirect (*real* expectations and adaptation are not observed), I find consistent evidence supporting the key role of framing : sentence reductions have very different effects on recidivism depending on prisoners' capacity to anticipate and adapt before release. As expected, my estimates show that the most criminogenic effects of sentence reductions are observed among prisoners who were unlikely to correctly anticipate the pardon of July 1996 and who had little time to adapt to this change, materially and psychologically. Conversely, most of my estimates for sentence reductions and recidivism are negative and insignificant among prisoners who had more chances of correctly predicting the pardon or more time to adapt to the news. Finally, I find suggestive evidence in favor of a *material* channel, i.e. the idea that unexpected sentence reductions leave prisoners worse off in terms of their preparedness for release. In a word, it seems that, for many prisoners released just after the pardon of July 1996, *it wasn't the right time to get out*.

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Appendix

FIGURE 2.1: Distribution of Sentence Reductions Before/After Winsorizing

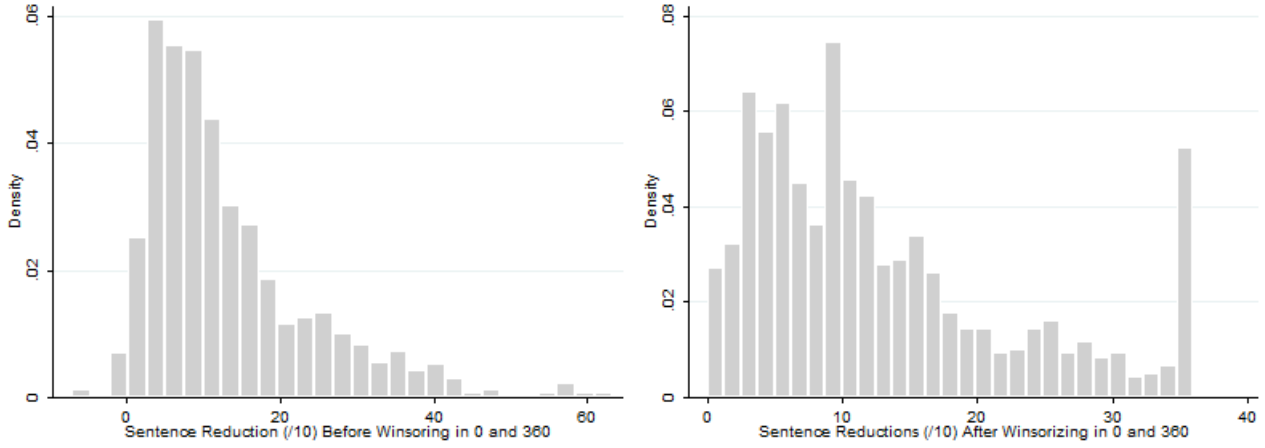


FIGURE 2.2: Distribution of MCA score of capital

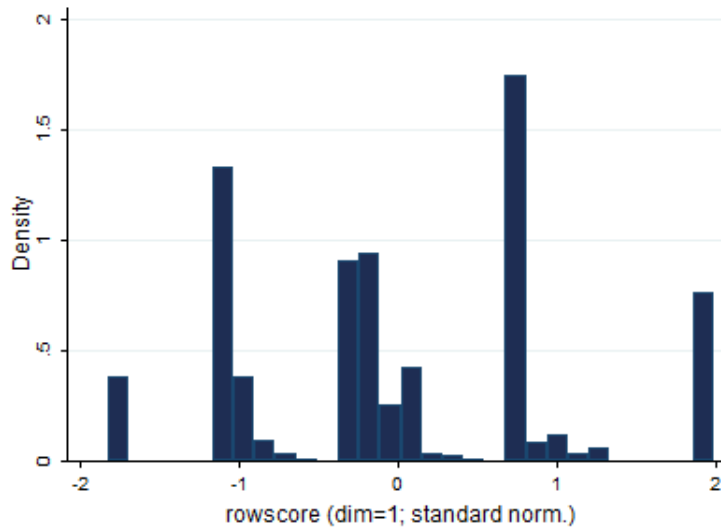


TABLE 2.11: IV Estimates on 5-year Recidivism, by Adaptation and Age (Interaction)

VARIABLES	(1)	(2)	(3)
	All recid_y5	Little Experience recid_y5	Large Experience recid_y5
Sentence Reductions (/10)	0.0032 (0.0095)	0.0527* (0.0309)	-0.0170 (0.0182)
sr10_wage	-0.0002 (0.0001)	-0.0002 (0.0008)	0.0001 (0.0002)
age	-0.0105 (0.0550)	0.0465 (0.0788)	-0.0482 (0.0800)
Observations	932	463	469
R-squared	0.2491	0.1903	0.3167
Indiv. Controls	X	X	X
Prison Controls	X	X	X

Robust standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 2.12: IV Estimates on 5-year Recidivism, by Anticipation and Adaptation

VARIABLES	IV Probit estimates by Absolute Time Remaining					
	(1)	(2)	(3)	(4)	(5)	(6)
	< 180 days	≥ 180 days	< 180 days	≥ 180	< 180 days	≥ 180
Sentence Reductions (/10)	0.0498*** (0.0172)	-0.0148 (0.0123)	0.0771*** (0.0225)	-0.0446*** (0.0155)	0.0235 (0.0229)	0.0194 (0.0160)
Observations	475	477	274	229	201	248
Indiv. Controls	-	-	-	-	-	-
Prison Controls	-	-	-	-	-	-

Standard errors in parentheses

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 2.13: IV Estimates on Different Crime Types after 5 years

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	All		Little Experience		Large Experience	
	< 180 days	≥ 180 days	< 180 days	≥ 180	< 180 days	≥ 180
Property Recid.	0.0147 (0.0191)	-0.0241 (0.0150)	0.0564** (0.0275)	-0.0206 (0.0189)	-0.0068 (0.0248)	-0.0198 (0.0251)
Violent Recid.	-0.0058 (0.0181)	0.0096 (0.0135)	-0.0151 (0.0267)	0.0001 (0.0154)	0.0237 (0.0299)	0.0229 (0.0221)
Drug Recid.	0.0092 (0.0132)	-0.0123 (0.0132)	-0.0029 (0.0218)	0.0003 (0.0152)	0.0204 (0.0219)	-0.0425 (0.0263)
Traffic Recid.	0.0145 (0.0154)	0.0047 (0.0135)	0.0292 (0.0262)	-0.0113 (0.0175)	-0.0055 (0.0194)	0.0059 (0.0218)
Other Recid.	0.0123 (0.0189)	-0.0053 (0.0155)	0.0153 (0.0295)	0.0058 (0.0212)	0.0040 (0.0264)	-0.0094 (0.0251)
Observations	463	469	268	224	195	245
Indiv. Controls	X	X	X	X	X	X
Prison Controls	X	X	X	X	X	X

Separate IV probit regressions for each dependent variable and each group.

Standard errors in parentheses \*\*\* p<0.01, \*\* p<0.05, \* p<0.1





## Chapitre 3

### *Better at Home than in Prison ?*

# The Effects of Electronic Monitoring on Recidivism in France

#### **Abstract**

Many countries have recently adopted electronic monitoring (EM) as an alternative sentence in order to reduce incarceration while maintaining public safety. However, the empirical evidence on the effects of EM on recidivism (relative to prison) is very scarce worldwide. Our empirical strategy exploits the incremental rollout of electronic monitoring in France, which started as a local experiment in four courts in 2000-2001 and was later adopted by an increasing number of courts (2002-2003). Our IV estimates show that fully converting prison sentences into electronic monitoring has long-lasting beneficial effects on recidivism, with estimated reductions in the probability of reconviction of 6-7 percentage points (9-11%) after five years. The presence of heterogeneous effects suggests that both rehabilitation and deterrence are important factors in reducing long-term recidivism, and that EM can be a very cost-effective alternative to short prison sentences.

*This chapter corresponds to an article co-authored with Anaïs Henneguëlle (ENS - CNRS IDHES) and Annie Kensey (DAP - CNRS CESDIP), accepted for publication in the Journal of Law and Economics.*

### 3.1 Introduction

Many countries are slowly turning away from mass incarceration in favor of new forms of punishment. In the United States for example, after three decades of steady growth, the total prison population has been declining for five consecutive years since the 2008 peak (Glaze and Kaeble, 2014). While the U.S. prison system remains by far the largest with 2.2 million inmates, a similar downward trend is currently observed among members of the Council of Europe, with their total prison population now under 1.7 million (Aebi and Chopin, 2014).

This slow decline in prison population is partly explained by budgetary and capacity constraints, but also by rising concerns about the effectiveness of incarceration. In the five years following release, 77% of ex-prisoners are re-arrested in the U.S. (Durose et al., 2014), while 59% are re-convicted in France (Kensey and Benaouda, 2011). This context has led to the development of alternative penal sanctions which either avoid incarceration (front-door strategies) or hasten release from prison (back-door strategies). Among them, electronic monitoring (EM) is often considered as the most promising : this technology provides live surveillance of offenders<sup>1</sup>, and therefore *some* incapacitation and deterrence, for a tenfold decrease in operating costs (about \$10 per day under EM compared to \$100 in prison<sup>2</sup>). Electronic monitoring is now available in many countries, and its use is growing fast. For instance, among the five million offenders who are supervised in the community in the U.S. (Glaze and Kaeble, 2014), it is estimated that roughly 20 percent involve electronic surveillance (Gable and Gable, 2005). In England and Wales, 90,000 cases involved EM in 2012 (National Audit Office, 2013). In France, electronic surveillance concerns more than 20,000 offenders every year, compared to an annual inflow of 70,000 prisoners (DAP, 2015).

However, in comparison to the increasing use of EM worldwide, there is surprisingly little causal evidence on the effects of electronic monitoring in terms of recidivism. Most existing studies use observational data to estimate how recidivism rates differ between groups of ex-prisoners and EM offenders, controlling for a small set of observable characteristics<sup>3</sup>. Unfortunately, these estimates are likely plagued by selection bias because judges typically try to allocate electronic monitoring to the "best" offenders (those with good reentry prospects

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1. EM offenders are located in real time through the electronic device attached to their ankle. Depending on the technology used, one can either track the exact location of tagged offenders (GPS tracking) or simply make sure they are present in a designated place, usually their home (Radio Frequency tracking)

2. These U.S. estimates are from Roman et al. (2012) and Kyckelhahn (2011). Similar figures apply in France, with a daily cost of 10€ under EM and about 100€ in prison (DAP, 2013)

3. In France for example, such methods yield a gap of about 20 percentage points in favor of EM offenders after five years (Benaouda et al., 2010)

and a low intrinsic risk of recidivism). In France for example, the decision to incarcerate or to grant EM to convicted offenders is highly discretionary and involves a preliminary social investigation and a hearing of eligible offenders : it is therefore very likely that judges' decisions rely not only on measured characteristics (like age or offense type), but also on a wide range of unobservable factors such as motivation, reentry prospects, or family support, which may themselves explain recidivism.

Only two existing papers deal with this selection bias convincingly, using quasi-experimental designs where similar offenders face dissimilar punishments (Di Tella and Schargrodsky (2013) in Argentina, and Marie (2015) in England and Wales). They both find evidence of substantial beneficial effects of EM compared to prison, but these findings may be specific to the settings studied. In particular, in both papers, all EM offenders spent time in jail, prison, or both (either a short time as in the case of pre-trial detention in Argentina, or a longer period in the case of the main sentence in England and Wales) and later were placed under electronic monitoring. Therefore these examples actually combine incarceration *and* EM, while in our study, EM offenders are all sentenced to prison sentence but never enter prison (front-door EM). Moreover, Argentina differs a great deal from the U.S. and Europe in terms of prison conditions (much harsher in Argentina), type of offenders considered for EM (all types in Argentina) and duration of supervision under EM (more than one year on average in Di Tella and Schargrodsky (2013), compared to less than six months in Marie (2015) and in France).

The main contribution of this paper is therefore to estimate the effect of fully serving a prison sentence under EM *instead of* incarceration on future criminal activity in a large European country, namely France. A second contribution is that we are able to investigate the mechanisms explaining our main result, using detailed data from the monitoring period (requirements to be met, breaches, control visits at home, etc.).

France provides a good case-study for the analysis of EM as it was among the early-adopters of electronic surveillance in Europe, and is now a massive user. Moreover, the sequential introduction of EM in France represents a natural experiment : electronic surveillance was introduced on an experimental basis in four pilot courts in the early 2000s, and later became available in an increasing number of courts. We discuss this gradual rollout in detail and provide evidence that the endogenous selection of EM by courts is unlikely. We argue that the incremental implementation of EM generated sharp, exogenous discrepancies in eligibility for electronic monitoring among similar offenders, based on time and location. Finally, another reason for studying France is that EM is considered a proper alternative to incarceration in



the Penal Code as judges can fully convert any short prison sentence to electronic monitoring *before incarceration*. Therefore, all EM recipients under study in this paper were sentenced to prison, but ended up serving their whole sentence at home under electronic monitoring. All these features of EM in France allow us to obtain quasi-experimental estimates of the effect of serving time at home under EM instead of in prison.

Our results show that simple comparisons highly overestimate the crime-prevention effect of electronic monitoring. The inclusion of a rich set of covariates reduces the gap in five-year recidivism from 14-15 pp to 8-9 percentage points. When we also control for selection based on unobservable characteristics using variation in access to EM as IV, the estimated beneficial effect of serving time at home under EM, instead of incarceration, falls to 6-7 percentage points. This effect remains significant statistically and economically, as it suggests a long-term reduction in recidivism by 9-11% thanks to the use of EM. We also find that this beneficial effect is greater on EM offenders who had prior prison convictions, received control visits at home from parole officers while under EM, and were required to work. We show that the estimated reduction in reoffending is not merely a function of short-term incapacitation at home but reveals more profound change (desistance from crime, with less recidivism and less serious new offenses), where both rehabilitation and deterrence play an active role.

The remainder of the paper is organized as follows. Section 1 reviews the existing literature on EM and recidivism. Section 2 explains how EM was introduced into the French institutional context, as well as the data we use in our estimates. Section 3 presents our empirical strategy and provides support for the main identification hypothesis. Section 4 presents the main results and investigates the presence of qualitative effects (on offense type, severity) and heterogeneity in treatment effects (by type of offender and intensity of supervision). Section 5 discusses the mechanisms driving our results and their current validity in France. Section 6 contains our conclusions.

## 3.2 Related Literature

In line with the seminal model by Becker (1968), there is now compelling evidence that prison sentences prevent crime not only by incapacitating criminals behind bars, but also by deterring potential offenders (Abrams, 2013). However, it is less clear whether the experience of incarceration also prevents recidivism among prisoners, or whether alternative punishments might be more effective.

The main argument against non-custodial sanctions is that such sentences are more lenient than prison. According to the theory of specific deterrence, the personal experience of lighter punishment (in the form of EM instead of incarceration, for example) makes the threat of future sentences less salient and less costly, thus promoting recidivism. Recent evidence of such effects is found in Hjalmarsson (2009) (who compares recidivism of young offenders sentenced to custodial or non-custodial sanctions) or Kuziemko (2013) (with length of incarceration as a measure of sentence severity)<sup>4</sup>.

However, this beneficial deterrent effect of prison may be weakened by the negative side-effects of incarceration. First of all, prisoners are exposed to many criminal peers behind bars; prison facilities allow criminals to learn from each other, build new networks and find new opportunities for crime. This view of prison as a “school of crime” is strongly supported by recent empirical research on peer-effects in Florida (Bayer et al., 2009), Denmark (Damm and Gorinas, 2013) and France (Ouss, 2011). Second, traditional prisons fail to promote social rehabilitation and labor-market integration of inmates, due to low access to education, human capital depletion, social exclusion and stigma after release (Aizer and Doyle, 2015; Mueller-Smith, 2014; Western et al., 2001). Current empirical evidence suggests that more open prisons, with less restrictive detention regimes and better access to work and rehabilitation programs, are more effective at preventing recidivism (Mastrobuoni and Terlizzese, 2014; Chen and Shapiro, 2007; Drago et al., 2009).

Overall, existing research suggests that while incarceration can induce more specific deterrence than non-custodial sanctions, it also disrupts social and labor-market ties and can eventually lead to hardened criminality. Conversely, electronic monitoring has many desirable features (e.g. it allows offenders to maintain family relationships and keep their jobs) but it may also foster feelings of impunity, producing a criminogenic effect overall.

Until recently, empirical research did not offer convincing estimates of the net effect of EM (compared to incarceration) on recidivism, due to the potential for contamination by selection bias (see Aos et al. (2006) and Villetta et al. (2006) for meta-analyses). Only two recent papers explore quasi-experimental designs whereby arbitrary rules or random events lead similar offenders to receive different punishments (EM or incarceration).

In Buenos Aires, Argentina, criminal cases are assigned randomly among judges depending on their work schedules. Di Tella and Schargrodsky (2013) report that local judges differ

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4. In Sweden, a country with very short average prison stays (1-2 months), Landersø (2015) also finds that exogenous increases in prison time actually promote employment after release, an effect which appears to be driven by increased rehabilitation and better preparation for release.

greatly in their use of EM; only one third of them (100/293) ever use EM in Buenos Aires during the period under study (for a total of 386 EM granted). Di Tella and Schargrodsky (2013) use these ideological differences as exogenous variations in the probability of EM treatment, and find a significant 50% drop in the probability of re-arrest after EM compared to prison. While this paper is the first to provide compelling evidence of the crime-prevention effect of EM, the authors note that such a striking effect may well be specific to Argentina; in that country, prison conditions are particularly inhumane, with extensive overcrowding and little hope for rehabilitation. In addition, all EM beneficiaries were pre-trial defendants who had already served time in jail, and even in prison for most of them, before their release under EM. Finally, many EM offenders in Argentina were serious violent criminals and the average monitoring period was very long (more than one year). These features are specific to the Argentinian case and most countries have different approaches towards EM (selection of low-medium risk offenders, shorter supervision).

Marie (2015) is the first to provide quasi-experimental evidence of the beneficial effects of electronic monitoring in Europe. Using a Regression Discontinuity approach, the paper provides evidence of a substantial beneficial effect of EM in England and Wales, with 20% to 40% reductions in the probability of rearrest within two years. However, the program under study grants EM as an early-release device, not as a front-door substitute for incarceration, so it remains uncertain whether fully converting custodial sentences to electronic surveillance before incarceration prevents recidivism in the same way.

The implementation of EM in Denmark also offers quasi-experimental evidence from Europe, but existing studies focus on the effect of EM on social welfare dependence (Andersen and Andersen, 2014) or educational outcomes (Larsen, 2016), not future criminal activity.

In France, prior work on EM and recidivism is mainly exploratory. Benaouda et al. (2010) compare conviction rates between EM offenders and ex-prisoners and find a lower recidivism rate for the former, but they do not go further and therefore do not avoid the selection bias issue. Another approach employed by Ouss (2013) in France is to explicitly acknowledge the presence of selection bias and test how much selection is actually needed in the data to make the estimated effect disappear (in practice, a fictitious covariate is added in the model). Using this simulation-based method, Ouss (2013) concludes that an unreasonable amount of selection bias is needed, and thus that EM and parole actually reduce recidivism in France. However, it seems unclear which level of selection bias is reasonable or not, especially in the context of highly discretionary decisions made by professional judges after individual

interviews with eligible offenders. Moreover, the conclusions from such sensitivity analyses highly depend on the richness of available data and on the quality of the benchmark regression, in terms of precisely controlling the main differences between the offenders placed under EM and those who are not.

In the current article, we take a more direct approach to estimating the causal effect on recidivism of serving time at home under electronic monitoring, instead of incarceration. Specifically, our empirical strategy exploits the gradual introduction of EM across French courts in the early 2000s as a natural experiment.

### 3.3 Institutional Context and Data

We propose to estimate the following equation :

$$Recid_i = \alpha + \beta EM_i + u_i \quad (3.1)$$

In order to obtain an unbiased estimate of  $\beta$ , we need to take into account the eligibility criteria for EM and control for observable differences between treated and non-treated individuals. However, we also have to correct for the potential for selection by French judges based on unobservables, as we explain below.

#### 3.3.1 An experiment (2000-2001) followed by a gradual rollout

On December 19 1997, after years of parliamentary debates<sup>5</sup>, Law no. 97-1159 introduced electronic monitoring in France as a substitute for incarceration. Legally, EM is not a criminal sentence but a way of serving a prison sentence either before or after incarceration. It entails a home curfew (generally during nights and weekends) but also specific obligations assigned by the judge, such as working or family care. Though the law was passed in 1997, it took several years to prepare for the introduction of EM in France<sup>6</sup>, and the practical implementation of electronic surveillance only began in year 2000 as a pilot experiment (Kensey et al., 2003; Lévy and Pitoun, 2004).

The EM experiment took place in four courts, or *Tribunaux de Grande Instance*, between October 1 2000 and October 1 2001; these courts were located in Agen, Aix-en-Provence, Grenoble and Lille. As explained by Lévy and Pitoun (2004), the choice of these four pilot

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5. Two Members of Parliament, Bonnemaïson and Cabanel, wrote seminal reports on EM in 1983 and 1996 respectively. See Bonnemaïson (1983) and Cabanel (1996).

6. Between 1997 and 2000, some preliminary studies were conducted in order to prepare a review on existing knowledge. These studies concluded that an experimental phase was necessary, in order to test the equipment and the software provided by the different suppliers.

locations was mainly motivated by whether the local judge(s) and prison wardens were sympathetic with the project, whether prison staff and judicial authorities worked well together locally, or whether the EM experiment would face resistance from local unions<sup>7</sup>. However, as we show in detail in Section 4.3, these four pilot courts did not seem to differ from the other French courts on important observable characteristics such as post-prison recidivism, prison overcrowding or local crime trends.

On January 1 2002, just after the end of the experimental phase, 143 EM had been granted to offenders from the four pilot courts, of which 120 had expired. Then, the French department of corrections decided to expand electronic monitoring to the whole country. Starting in January 2002, all French courts were allowed to grant EM to offenders meeting the legal criteria. Local judges had first to request EM devices from the central administration and could then grant tags to offenders.

The process was extended and in the end it proved to be geographically heterogeneous. Only one new court, located in Béziers (in the south of France), granted EM as a substitute for incarceration in January 2002. The first wave of adoption actually occurred between December 2002 and May 2003, with a dozen new courts participating (see the map in Figure 3.7 Appendix A). The EM rollout then intensified in the second half of 2003 (78 courts had granted at least one EM by December) and in early 2004 (112 courts by May) (Lévy and Pitoun, 2004). This process continued over the next few months, and all French courts eventually adopted electronic monitoring<sup>8</sup>. Today, EM is widely used in France, with more than 20,000 tags granted every year, and about 10,000 offenders under EM on any given day (DAP, 2015). As we detail in Section 4, we take the staggered introduction of EM across courts over time as an opportunity to estimate the causal effect of EM on recidivism.

### 3.3.2 Selecting offenders for EM

Depending on EM availability at the court level, the path towards electronic monitoring is highly selective; offenders first have to meet several eligibility criteria, and must also receive the approval of a judge after an individual hearing. This judge, known as the *Juge*

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7. Lévy and Pitoun (2004) also note that Agen hosts the national school of prison administration (ENAP), and that MP Cabanel comes from Grenoble : this may also explain why those two locations were chosen.

8. According to discussions with practitioners, part of the explanation for the delay in the introduction of EM across courts has to do with ideological resistance, since EM was quite a revolution in France in the early 2000s : it involved both a new technology, and a new philosophy on prison sentences (offenders initially sentenced to prison could now avoid incarceration altogether thanks to EM)

*d'Application des Peines*<sup>9</sup>, has ample room for discretion. The eligibility criteria are explicitly listed in the *Code de procédure pénale* :

- Offenders are to be sentenced to a short prison stay of no more than one year, or the remainder of their current sentence should not exceed one year<sup>10</sup>. In practice, about 90% of EM devices in France are granted before incarceration to short sentence offenders (Kensey et al., 2003)<sup>11</sup>.
- Offenders are to have a place to stay equipped with a fixed-line telephone (to install the electronic device)<sup>12</sup>.

Every offender who meets these two criteria and who is left free at trial (no bench warrant) is eligible for front-door EM (in the empirical section below, we discard all individuals who do not meet these criteria). The offender's case is automatically examined in the following months by a second judge, the *Juge d'Application des Peines*, who decides whether EM should be granted or not as an alternative to incarceration. In order to make his decision, this judge requests that an investigation be made by parole officers (to make sure that the landlord, family and offender give their consent, and that EM is practically possible). He later meets the offender and his lawyer (if any) for a hearing, which lasts about 20 or 30 minutes. The judge usually asks offenders questions about the current offense, the victims, past convictions, and which activities the offender would or could pursue under EM (work, training, medical treatment), etc. These interviews can therefore reveal qualitative aspects of the offender's case, that are not reported in official criminal files. A few weeks after this interview, the offender is informed of the judge's decision.

As expected from this process, Kensey et al. (2003) show that many factors are taken into account : type of offense (driving and drug offenses are particularly frequent among EM offenders), length of criminal record (recidivists are less likely to receive EM), attitude towards the sentence, "maturity" or "psychological stability", etc. In fact, Kensey and Narcy (2008) show that offenders placed under EM between 2000 and 2006 are more similar to those convicted for non-custodial sentences (suspended prison sentence, probation) than to incarcerated offenders : for instance, 92% of EM offenders were French compared to 77%

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9. The *Juge d'Application des Peines* is never the same person as the sentencing judge (who rules on convictions and sentencing). *Juges d'Application des Peines* are only in charge of issuing the prison sentence, with EM as a possible front-door alternative to incarceration.

10. This length was extended in 2009 to two years, but remained equal to one year for recidivists. Our data, which are focused on the years 2000 to 2003, are not affected by this legal change.

11. In this study, we focus on offenders who benefited from EM before incarceration to estimate the effect of electronic monitoring as a complete substitute for prison.

12. Having a fixed-line telephone is no longer necessary for current devices, but this condition was important during the time-period of our data.

among inmates; 42% had a partner, compared to 23% for prisoners. Only 18% of those under EM were illiterate or had a very low educational level, and 72% were employed before conviction (as opposed to 50% and 34% among inmates).

Overall, French judges seem to use a great amount of discretion in selecting offenders under electronic monitoring, among the large pool of eligible offenders. EM offenders tend to have better reentry prospects (in terms of family support, work history, criminal background, etc.) and are therefore less likely to reoffend in the first place. These observable differences stress the need to include a rich set of individual characteristics as control variables in Equation 3.1 above. Furthermore, we rely on a quasi-experimental design to correct for the plausibility of a selection based on unobservable characteristics as well, since *EM* is likely to be endogenous. We argue that the incremental rollout of EM in France between 2000 and 2003 provides such a setting.

### 3.3.3 Data and Descriptive Statistics

**Datasets** To investigate this issue, we merge two nationwide surveys conducted by the French Prison Administration. The first survey consists of a cohort sample of prisoners released in the year 2002, and the second is the population of the 580 first EM recipients in France (between 2000 and 2003).

The first database contains a sample of 8499 offenders released between June 1 2002 and December 31 2002. It was constructed using two sources of data, penal files and criminal records. Penal files are filled out by the prison facilities themselves, while offenders are serving their sentence. They contain basic sociodemographic data about convicts (gender, date of birth, self-declared employment and marital status, education, home city) but also some information about offenses (date, specific offense, sentence) and incarcerations (location, dates of entry and release, sentence reductions). Criminal records register offenders' sentences, both before and after the incarceration that led to the 2002 release, up to the year 2008.

This sample is not drawn at random from the general French prison population. Indeed, some categories were fully sampled, such as women, parolees or juveniles. To obtain estimates that are representative of the population of released prisoners, we follow Solon et al. (2015) and always include as regressors the variables used to stratify the sample.

The second database is the only existing study of recidivism among EM recipients in France. It contains individual data on the population of the first 580 EM offenders, from

the inception of electronic surveillance in France until March 2003<sup>13</sup>. This dataset collects socio-demographic data and criminal records up to 2008, allowing a follow-up period of more than five years.

Criminal records allow us to construct our main dependent variable, recidivism, from the two datasets. We define recidivism as any reconviction, regardless of the type of new offenses and sentences<sup>14</sup>. On occasion, we also focus on new prison sentences (reincarceration) to pinpoint cases of serious reoffending. Recidivism is measured after five years, which is typical for studies in France but much longer than most foreign research. In order to pinpoint new offenses in the at-risk period, the clock starts on day of release for prisoners, and on first day under electronic monitoring for EM recipients<sup>15</sup>. In addition to reconvicted offenders, we consider as recidivists the 26 EM recipients who were sent to prison during their supervisory period due to repeated incidents or a new offense. Neglecting those "failures" would skew the comparison in favor of EM.

**Sample restrictions** After merging the two samples, we obtain 9079 individuals. However, we make several sample restrictions to make the treated and control groups more comparable (see Appendix B for more details). First, out of the 580 EM offenders, 515 (88%) were placed under EM as a complete alternative to incarceration (front-door EM) while the others obtained early release under EM during their prison stay. As we intend to study the effect on recidivism of being placed under EM *instead of* serving time in prison, we consider the former group as the *treated* (EM offenders), while dismissing the latter from the sample. Some observations are dropped in both groups since they are not usable (due to death or the absence of criminal record for example) or have missing or inconsistent values for important variables (such as sentence length). We also drop all prisoners who were not eligible for EM, either because their original sentence was over 12 months, because they didn't have a home address, or because they were incarcerated before or on the day of conviction (pre-trial detention or bench warrant)<sup>16</sup>. Finally, we exclude from most regressions individuals whose

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13. For additional details, see Kensey et al. (2003).

14. We acknowledge that the use of reconviction data is an imperfect measure of recidivism (some offenses are not detected and prosecuted), and does not provide complete data on rehabilitation. However, there is no data on ex-prisoners' rehabilitation or self-reported crime in France. Moreover, even though the absence of recidivism does not guarantee rehabilitation, we argue that rampant recidivism clearly suggests a failed reentry.

15. An alternative is to start the clock for EM recipients on the end date of surveillance; this would account for potential short-term incapacitation during home curfew under EM, but, conversely, it would overlook new offenses during surveillance. In robustness checks, we show that our estimates are not affected by the choice of starting time.

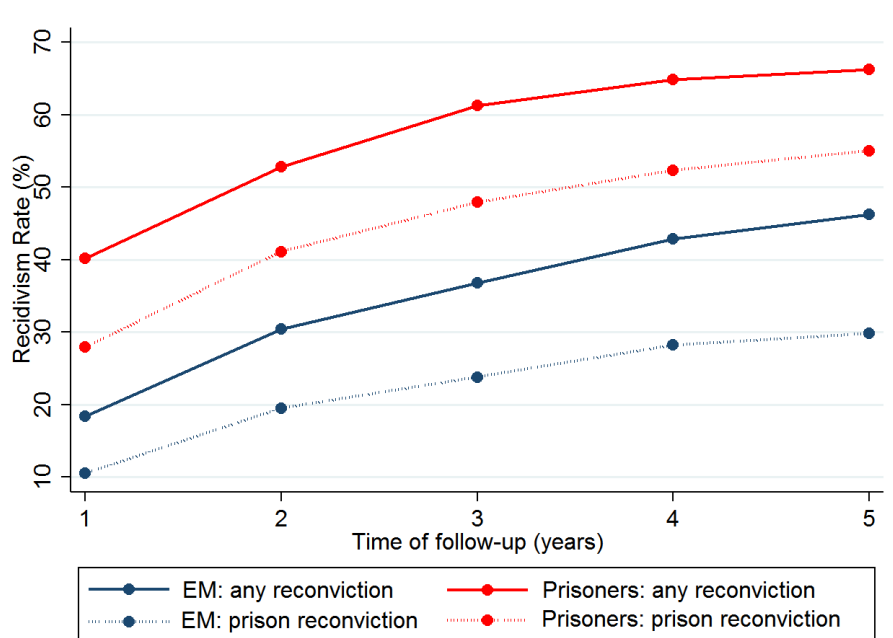
16. Our view is that these prisoners are inherently different from EM offenders, as judges considered that their case required rapid incarceration. Conversely, the very fact that treated offenders were placed under



follow-up period for new convictions is shorter than five years. All these restrictions reduce our study sample to 2827 offenders, of which 457 were sentenced to front-door EM while 2370 served time in prison.

**Descriptive statistics** Table 3.6 in Appendix B reports descriptive statistics with regard to sociodemographic data, judicial variables and recidivism, for the full study sample (Column 1) but also both for EM offenders (Col. 2) and non-EM offenders (Col. 3). Even after our sample restrictions, the two groups differ significantly in a wide range of areas (such as prior convictions and type of offense), as noted previously by Kensey et al. (2003). In terms of recidivism within five years, the two groups also differ dramatically ; 66% of ex-prisoners are reconvicted compared to 46% of ex-EM offenders, yielding a gap of 20 percentage points (after correcting for the stratification of the prisoner sample). Figure 3.1 plots the evolution of recidivism rates over time : the recidivism gap between ex-prisoners (red) and EM recipients (blue) is substantial and quite stable over the entire follow-up period, and even more substantial when we focus on new prison sentences (25 pp after five years).

FIGURE 3.1: Recidivism over time



EM at home demonstrates that judges didn't view them as too dangerous. This major difference leads us to consider as controls only prisoners who were incarcerated strictly after their final prison conviction.

### 3.4 Empirical Strategy and Specification

#### 3.4.1 Exploring the gradual rollout of EM in France

Our empirical strategy exploits the early stages of the gradual introduction of EM in France (2000-2003). As described in Section 3.1, EM was first tested as an experiment in only four courts, called *Pilot* courts, starting in October 2000. EM later became legally available to all French courts in January 2002, but most of them did not use EM until much later. We can track very precisely the staggered implementation of EM in all French courts between 2000 and April 2003 thanks to thorough data collected by Kensey and Benaouda (2011) on the population of the first 580 offenders who were granted EM in France (as a front-door or back-door strategy). As an illustration, Figure 3.2 plots the cumulative number of EM granted in *Pilot* courts over the period. EM was adopted at a fairly constant rate in all four courts between 2000 and 2003.

FIGURE 3.2: Number of EM granted in Pilot courts

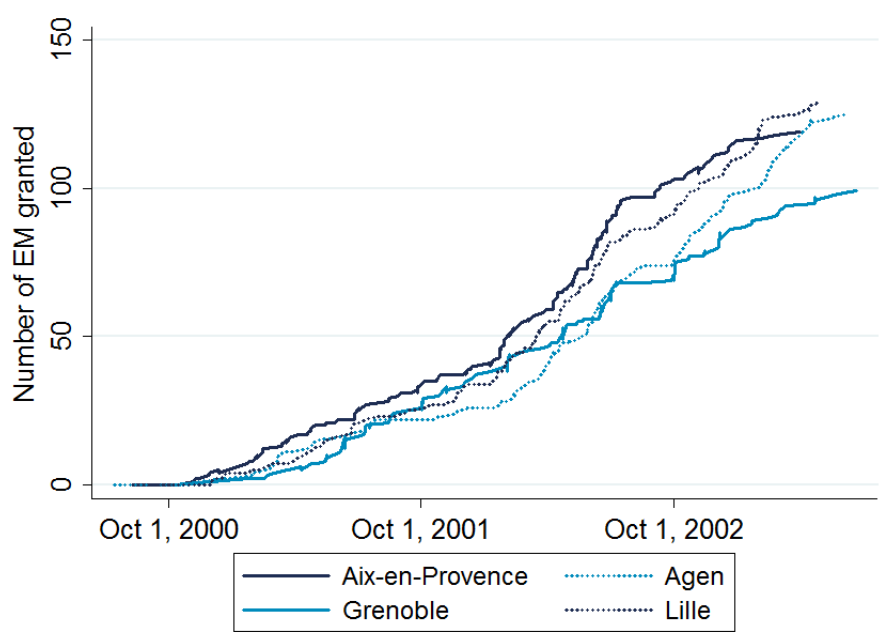


Table 3.7 in Appendix C reports the total number of EM granted by March 2003 in the four *Pilot courts* and in the 13 next courts to implement EM (at least once) by this date. Those 13 courts are called *early-adopters*, whereas all the other French courts are labelled as *late-adopters* (they adopted EM later on and thus do not appear in our EM dataset).

Intuitively, this staggered implementation of electronic surveillance in France resembles

a natural experiment under which similar offenders face differential access to EM depending on time and location : with all other factors remaining constant, offenders located in courts that were currently using EM had more chances of receiving EM than offenders located in courts that had not yet adopted EM. In addition to EM availability at the court level, we also measure the past intensity of EM use by the court (i.e. court  $c$  often or rarely used EM before  $i$ ) to utilize the fact that the magnitude of EM use changes over time and differs between *pilot* and *early-adopter* courts. These two instrumental variables are similar to those used by Di Tella and Schargrotsky (2013) in Argentina.

### 3.4.2 Econometric Specification

Formally, we consider a two-equation model where the probability of reoffending and the probability of receiving EM are estimated jointly :

$$Recid_{i,c}^* = \beta_0 + \beta_1 EM_i + \mathbf{X}'_i \boldsymbol{\beta} + \epsilon_{i,c} \quad (3.2)$$

$$EM_{i,c}^* = \alpha_0 + \alpha_1 CourtAlreadyUsedEM_{i,c} + \alpha_2 ShareEM_{i,c} + \mathbf{X}'_i \boldsymbol{\gamma} + e_{i,c} \quad (3.3)$$

Equation 3.2 resembles our naïve Equation 3.1 and simply estimates the effect of  $EM$  on  $Recid$ , controlling for a vector of observable characteristics  $\mathbf{X}$ . Therefore, Equation 3.2 alone does not correct for the selection of EM recipients from unobservable characteristics.

To correct for the potential endogeneity of  $\beta_1$ , we estimate a recursive Bivariate Probit model of Equations 3.2 and 3.3 by Maximum Likelihood : such a model explicitly controls for the correlation between the two error terms (the unobservables), denoted  $\rho$ . As an alternative method, we also use Two Stage Least Squares (2SLS) with Eq. 3.3 as the first-stage.

The introduction of instrumental variables in Equation 3.3 provides the identification of  $\beta_1$ . The main instrument,  $CourtAlreadyUsedEM$ , is a dummy taking 1 if a court  $c$  has previously granted EM before offender  $i$  starts his own sentence in that court (and 0 otherwise). We also include a second instrument in places, called  $ShareEM$  : it computes the percentage of previous offenders in court  $c$  who received EM before offender  $i$  starts his own sentence in that court (only using in the denominator previous offenders who started their sentence while EM was legally available in their court). Note that this second instrument might be confusing as the denominator is calculated from our prison dataset, a sample that is representative at the national level<sup>17</sup> but not necessarily at the court level. However, we expect  $ShareEM$  to

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17. When stratification is taken into account.

provide a good proxy for the intensity of EM use by courts over time (accounting for court size). Importantly, the structure of the error term allows for intra-court correlation in all regressions.

The vector  $\mathbf{X}$  is the same in both equations and includes a rich set of individual characteristics. First, we include in all regressions the variables used to stratify the prisoner sample to obtain a representative estimate of the effect of EM use on recidivism<sup>18</sup>; this includes type of initial offense, gender, juvenile (age<18), and early-release under parole. Thus we can measure how benefiting from EM affects the probability of reoffending in a representative sample of prisoners.

We also include sociodemographic variables that are presumably correlated with both EM treatment and future crime : age and age squared, employment status, relationship status and parenthood.

We control precisely for criminal background using four variables : two dummies for prior convictions to prison or alternative sentences, and two continuous variables for the total number in each category.

We also add a dummy for other early release programs<sup>19</sup> to account for the potential effects of early-release among prisoners. Finally, we include a linear trend for date of release to get rid of potential time trends in the risk of recidivism over time (due to changes in policing or economic conditions for example) between our two datasets<sup>20</sup>.

### 3.4.3 Testing for the endogenous selection of EM by courts

Our empirical strategy assumes that access to EM varied exogenously across courts and over time, i.e. courts' decisions to use EM early (and the extent of such use) was not related to court-level characteristics that may themselves affect recidivism. As we extensively control for a set of individual variables, our assumption of exogenous IV is conditional on  $X$  so that our strategy should not be sensitive to compositional changes across courts over time (in terms of demographics or criminal background). However, if adopting EM was a reaction to high recidivism, rising crime rates or massive overcrowding in local prisons for example, this assumption would not hold true. To investigate this scenario of the endogenous selection

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18. As recently suggested by Solon et al. (2015). This is particularly important since the sample of prisoners overrepresents parolees, a positively selected group of prisoners with good reentry prospects.

19. In addition to parole, prisoners can obtain early-release under *semi-liberté* and *placement à l'extérieur* (halfway-houses)

20. Remember that prisoners are released in the second half of 2002, whereas EM offenders are released in the 2000-2003 period. In addition to the inclusion of a linear time variable, we later consider the potential for non-linear time trends in robustness checks (results are robust).

of EM by courts, we collected additional data to check whether EM courts differed from late-adopters.

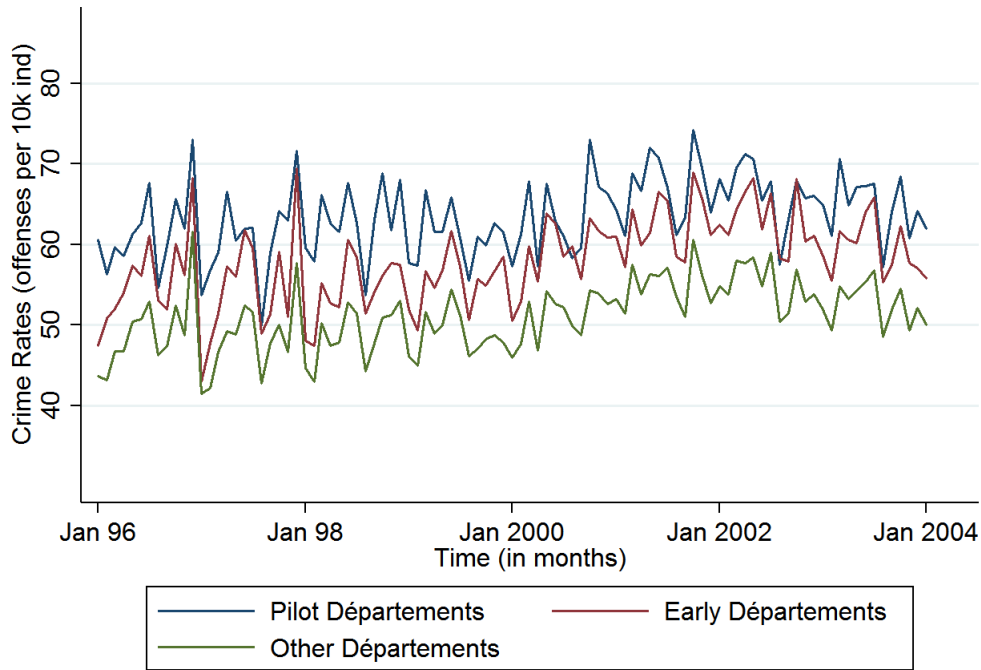
First, we consider whether recidivism rates were different between EM and non-EM courts before the introduction of electronic monitoring. To do so, we restrict the sample to prisoners who were incarcerated *before* EM became available in their court (Oct. 1 2000 in pilot courts; Jan. 1 2002 in all other courts). We account for the resulting difference in average sentence length between pilot and non-pilot courts by including a second-degree polynomial of initial sentence length. Probit regressions of recidivism on the type of court (pilot, early-adopter, or late-adopter) are reported in Table 3.8 (Appendix D). There is no evidence that early adoption of EM was related to differences in the probability of recidivism, either in the raw data or after controlling for a rich set of individual characteristics (to make sure that comparisons are not skewed by differences in offenders' composition across courts); the estimated differences are both small in magnitude (+2/-3 percentage points) and insignificant. Similarly, we look at recidivism in the late 1990s, using a previous cohort sample from the department of corrections (Kensey and Tournier, 2005). This dataset originates from a nation-wide representative study of 2207 ex-prisoners released in 1996-1997, whose reconvictions were followed until 2002. Again, raw and covariate-adjusted differences in recidivism are small and insignificant between pilot, early-adopter and late-adopter courts. Overall, these results do not support the scenario that courts which rapidly adopted EM (between 2000 and 2003) were different from other French courts in terms of recidivism. Therefore, our estimates are unlikely to be driven by local differences in recidivism.

Second, we consider local crime rates. We could expect that courts decided to adopt EM to manage a surge in local crime, threatening our empirical strategy (since local crime may correlate with our instruments and recidivism). To test this hypothesis, we gather monthly *département*-level data<sup>21</sup> (the French Interior Ministry doesn't provide crime statistics at the court level). We compare crime levels and crime trends in *départements* with a pilot or early-adopter court, and other *départements* where no court adopted EM rapidly (late-adopters). As we see in Figure 3.3, in levels, pilot and early-adopters tend to display substantially higher crime rates during the 1996-2001 period (levels are expressed in number of reported offenses per 10,000 inhabitants). This is probably due to the fact that pilot and early-adopter *départements* include densely populated areas (Marseille, Lille, Paris, etc.) which have higher levels of crime. However, crime trends are remarkably parallel in all three types of

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21. *Départements* are small administrative regions in France, like counties. France contains a total of 101 *départements* and the average number of High Courts per *département* is two.

FIGURE 3.3: Crime Rates



*departements* throughout the period. The same parallel trends are also observed when we split crime rates between property crime and violent crime (Figure 3.9 in Appendix D). Hence, the gradual adoption of EM across courts didn't seem to be a reaction to different trends in local crime rates.

Third, we consider prison overcrowding. Rising population/capacity ratios may have pushed some courts into electronic monitoring, which may independently affect recidivism. To test this hypothesis, we collect overcrowding rates in all prison facilities on January 1st of each year (1996, 1999, 2000, 2001, 2002) and construct weighted average rates for each type of court, based on the flow of offenders convicted in court  $c$  and incarcerated in prison  $p$ . Figure 3.8 (Appendix D) shows how these overcrowding rates vary over time for the three types of courts : overcrowding tends to be somewhat greater in pilot and early-adopter courts, but differences are small (less than 10 percentage points) and insignificant. Similarly, when we focus on short-term prisons (*Maisons d'Arrêt*), differences remain modest. Based on the small existing elasticities of recidivism with respect to prison overcrowding (Drago et al., 2011), we do not expect such small differences to affect our effects of EM use. In addition, note that overcrowding in early-adopter courts was very close to late-adopter courts on January 2002 (when EM was legalized in all courts), suggesting that courts which adopted EM rapidly did not face any particular problem of overcrowding compared to other French courts.

Finally, a minor concern is that some offenders may have strategically moved to EM courts after the introduction of the device to avoid incarceration in case of arrest. Hence unobservable traits could correlate with both our instruments for access to EM and recidivism. This scenario seems highly unlikely in practice because EM was a small program everywhere during the study period (so chances of being placed under EM were low anyway); second, 98% of those who received EM lived in the *departement* where they were tried. Therefore, we can safely ignore this concern of strategic choice of crime location.

## 3.5 Results

### 3.5.1 Benchmark estimates

We first report in Table 3.1 naïve estimates of Equation 2 using probit and linear probability models. They serve as a benchmark since they treat the use of EM as exogenous, and therefore do not correct for potential selection bias. We focus on the more conservative Probit estimates, but the OLS results are very similar.

The specification in Column 1 doesn't control for any individual characteristic but simply accounts for the stratification of the prisoner sample. The estimate suggests that EM offenders are 15 percentage points less likely to be reconvicted in the next five years than those released from prison. This difference is both great in magnitude and highly significant. To account for the fact that prisoners differ from EM recipients in many observable areas (see Table 3.6 in Appendix), we control in Col. 2 for the set of demographic variables : the difference in recidivism drops to 13 pp. Finally, controlling precisely for past convictions further reduces the estimated gap in recidivism to 8 pp.

Overall, the inclusion of a large set of covariates decreases the estimated gap in recidivism by more than 45%, from about 15 pp to 8 pp. This pattern supports the idea of a positive selection based on observables by French judges in assigning EM : i.e. offenders who were granted EM displayed relatively good prospects (in terms of employment, family support, criminal background, etc.) and low risk of recidivism. However, even after controlling for this large set of characteristics, the estimated difference in recidivism remains largely and significantly in favor of EM recipients compared to incarcerated offenders.

TABLE 3.1: Electronic Monitoring and Recidivism

	Y = Any Reconviction In The Next 5 Years		
	(1)	(2)	(3)
<b>Probit Model</b>			
Electronic Monitoring	-0.1523*** (0.0409)	-0.1276*** (0.0320)	-0.0832** (0.0286)
<i>Pseudo R</i> <sup>2</sup>	0.17	0.22	0.27
<b>Linear Prob. Model</b>			
Electronic Monitoring	-0.1614*** (0.0440)	-0.1344*** (0.0351)	-0.0954** (0.0318)
<i>Adj. R</i> <sup>2</sup>	0.21	0.26	0.31
Demographics		x	x
Past convictions			x
N	2827	2827	2827

Robust standard errors in parentheses, clustered at court level. <sup>+</sup>  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . All regressions control for the variables used for stratified sampling. Probits report Average Marginal Effects.

### 3.5.2 Causal estimates by IV

In order to account for potential selection based on unobservables as well, we turn to our two-equation model with instrumental variables. Column 1 reports our benchmark probit estimate (-8.3 pp) when the EM treatment is considered exogenous. In Col. 2 and 3, we run our recursive bivariate probit model by maximum likelihood, with one instrument or both instruments together.

First, our instrumental variables behave as expected : the estimates suggest that previous use of EM in a court significantly predicts one’s chances of obtaining electronic monitoring. As expected with selection based on unobservables, the correlation coefficient  $\rho$  is negative and the estimated average marginal effect of EM decreases. However, this estimate of  $\beta_1$  is still great and significant, and suggests that serving time at home instead of in prison reduces recidivism after five years by seven percentage points on average.

In Col. 5 and 6, we estimate the same model by 2SLS ; the results show similar evidence of selection bias in benchmark regressions. The estimated causal effect of EM is somewhat slighter and less precise than after bivariate probit estimation, close to six percentage points. Hansen’s test of overidentification shows no evidence of violation of the exogeneity condition, which further supports the credibility of our results.

Overall, these causal estimates tend to be somewhat lower than the benchmark, naïve



estimates (though differences are not statistically significant). This pattern, and the negative sign of  $\rho$ , support the idea that judges wisely selected EM offenders based on both observable and unobservable characteristics. However, even after controlling for this selection process, a large and significant gap of six to seven percentage points in the risk of recidivism remains. We attribute it to the average treatment effect on the treated of serving time at home under electronic monitoring instead of in prison. Given the 65% recidivism rate in the representative sample, our results suggest that the use of EM reduces the probability of recidivism by nine to eleven percent after five years.

TABLE 3.2: Electronic Monitoring and Recidivism

	Probit (1)	Bivariate Probit with 1 IV (2)	Bivariate Probit with 2 IV (3)	OLS (4)	2SLS with 1 IV (5)	2SLS with 2 IV (6)
<b><math>Y_1 = \text{Recidivism}</math></b>						
<i>ElectronicMonitoring</i>	-0.0832** (0.0286)	-0.0705* (0.0323)	-0.0711* (0.0341)	-0.0954** (0.0318)	-0.0553+ (0.0316)	-0.0571+ (0.0343)
<b><math>Y_2 = \text{Electronic Monitoring}</math></b>						
<i>CourtAlreadyUsedEM</i>		0.1294*** (0.0078)	0.0956*** (0.0145)		0.6365*** (0.0481)	0.3813*** (0.1042)
<i>ShareEM</i>			0.0582* (0.0254)			0.4986** (0.1731)
Demographics	x	x	x	x	x	x
Past convictions	x	x	x	x	x	x
$\rho$		-0.121 (0.103)	-0.126 (0.113)			
Hansen J test of exogeneity : p-value						0.706
N	2827	2827	2754	2827	2827	2754

Robust standard errors in parentheses, clustered at court level. +  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . All regressions control for the variables used to stratify the prisoner sample. Probits and Biprobits report Average Marginal Effects obtained by Maximum Likelihood.

### 3.5.3 Robustness Checks

We run a series of robustness checks in Table 3.9 (Appendix E). First, we limit the potential for unobserved heterogeneity among courts and socioeconomic contexts. Thus, we focus on neighboring courts within a radius of 100 kilometers from each EM court. This restriction reduces the sample by one third, but still yields significant negative estimates for *EM*, suggesting that our results are not driven by unobserved contextual differences.

A second concern is that local crime rates may both explain recidivism and the decision by the courts to implement EM. Though Figure 3.3 tends to reject it graphically, we further consider this hypothesis by including in our regressions two additional covariates : local crime rates in one *departement* in the starting month of the sentence, and the percentage change in local crime in the past two years. The estimates remain significantly in favor of EM (-7.5 percentage points).

Another concern is that our dataset merges two samples from different time periods : ex-inmates are all drawn from a cohort of prisoners released between June 1 and December 31 2002, whereas the EM group contains all those who were discharged from electronic surveillance from the beginning of the program (end of 2000) to March 2003. Therefore, our results could be sensitive to non-linear time trends in the risk of recidivism, for example due to temporal breaks in police forces or economic conditions. To deal with such temporal shifts, we drop all EM offenders who were discharged long before or long after the second half of 2002. To retain a sufficient number of EM beneficiaries, we keep those who were discharged between March 1 2002 and March 31 2003. Again, the estimate remains similar so temporal breaks in the risk of recidivism are not a concern.

Fourth, we use an alternative starting date for the measure of recidivism : up to now, we have computed recidivism as any new conviction in the five years following date of release (for prisoners) or the starting date of electronic surveillance (for EM offenders) since they are at risk of reoffending even during their monitoring period. However, one may argue that EM offenders are somewhat incapacitated at home while under electronic surveillance, so that the at-risk period actually starts at the end of the electronic monitoring. We use this alternative starting date to compute recidivism, and find similar results again.

Fifth, we rely only on the “largest” courts representing a total number of convicted offenders above the median (median number of offenders is 20 in our original dataset), as court size is important in the construction of our second instrument. We again obtain similar results.

Sixth, we turn to a different set of instruments, using only cross-court differences in EM use. Specifically, we use two dummies as IV, *Pilot* and *Early-adopter*, which pinpoint whether offender  $i$  was located in a court that ever used EM during the full period. This approach doesn't use time differences in access to EM but leads to similar conclusions.

Finally, another approach is to follow the evolution of aggregate recidivism rates over time as courts adopt EM. If electronic monitoring is indeed effective, court-level average recidivism rates should decline as courts grant EM to offenders who would otherwise be incarcerated. To explore such changes in recidivism between cohorts of offenders convicted before and after the adoption of EM in their court, we first need to control for the fact that these successive cohorts of convicts differ systematically in initial prison sentence length (a strong predictor of recidivism); thus we regress recidivism in pilot and early-adopter courts on a polynomial of sentence length, and use the residuals as a measure of the propensity to reoffend which is clean of (i.e. unexplained by) systematic differences in sentence length. Figure 3.11 (Appendix E) plots these residuals for different cohorts of convicts who start their sentence in the twelve quarters before or after the introduction of EM in their court (0 corresponds to the quarter of introduction of EM in one's court). As expected, the graphical evidence shows a drop in recidivism after courts adopt EM, which is consistent with a crime-prevention effect of electronic monitoring.

### 3.5.4 Why/When is EM effective ?

We now explore the heterogeneity of treatment effects. To do so, we run separate regressions of the same bivariate probit model (as in Table 2) on subsamples of EM offenders, based on possibly relevant mediating factors. The EM effect estimates for each subgroup are displayed in Table 3.3.

Regarding demographic characteristics, we do not find much evidence of heterogeneity among EM offenders. Estimated treatment effects are quite independent of the age of EM offenders (relative to the median, 30 years old) and prior employment status. However, the beneficial effect of EM seems greater among parents and, more importantly, among offenders who had already been sentenced to prison before (-12 percentage points). We view this last result as evidence that EM is mostly effective on offenders who know what is at stake should they reoffend (most probably incarceration) and do not view EM as a lenient, non-deterrent sanction but as a second chance. Conversely, granting EM to offenders who have never experienced prison might create deleterious feelings of impunity, thus reducing the effectiveness

TABLE 3.3: Heterogeneity of effects by profile and supervision

<b>Profile</b>	Yes	No	<b>Supervision</b>	Yes	No
Younger than 30	-0.0811* (0.0342)	-0.0897* (0.0440)	Control visit at home	-0.0926*** (0.0229)	-0.0218 (0.0371)
Has children	-0.1082* (0.0471)	-0.0526+ (0.0277)	EM length > median	-0.0824+ (0.0440)	-0.0543 (0.0378)
Employed	-0.0768+ (0.0393)	-0.0944+ (0.0521)	Obligation : work	-0.0816* (0.0332)	0.0043 (0.0591)
Prior incarceration	-0.1181* (0.0511)	-0.0559+ (0.0330)	Violation during EM	-0.0259 (0.0403)	-0.1077** (0.0376)

Robust standard errors in parentheses, clustered at court level. +  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . The table reports Average Marginal Effects of bivariate probit models (ML). All regressions include the full set of control variables.

of electronic monitoring.

The type and intensity of supervision under EM also seem to matter. First, electronic monitoring is very effective at reducing recidivism among those who have received at least one visit from parole officers during curfew hours, to check that they have truly followed the rules (-9 pp), while it is not among the others. One may suspect endogeneity in the choice of visiting certain offenders and not others, but note that if the most crime-prone offenders are targeted for control visits, this bias would presumably attenuate, not exacerbate, the estimated effect of visits on recidivism<sup>22</sup>. The striking difference in outcomes suggests that control visits act as a strong deterrent, which is in line with qualitative interviews with EM offenders in England (Hucklesby, 2009).

Our results also suggest that EM is more effective with offenders who were supervised longer (above the median length of two months), and who were required to work while under surveillance (-8 pp), controlling for prior employment status. As expected, we also find that EM fails to prevent recidivism among offenders who violated the rules while under surveillance (for example coming home late or leaving home during curfew hours).

We also investigate whether EM affects the seriousness of new offenses, using as proxies the type of new sentence (custodial or not) and the total length of new prison sentences

22. Moreover, it appears that the probability of a control visit mainly varies among courts, not among offenders within courts : for example, 97% of EM offenders were visited in Aix-en-Provence, compared to only 10% in Agen. We take this as evidence that control visits were not endogenous case-by-case decisions, but reflected different local practices (as confirmed by Kensey et al. (2003)).

TABLE 3.4: Seriousness of new crime(s) over the Next 5 Years

	Any New Prison Sentence		Total Length of New Prison Sent.	
<i>ElectronicMonitoring</i>	-0.0823** (0.0320)	-0.0973*** (0.0291)	-8.7884** (3.0985)	-8.9771*** (2.2792)
Conditional on Recid	No	Yes	No	Yes
N	2768	1583	2827	1635
Sample Average	45.7 %	73.5 %	19.46 mos	19.88 mos
Estimated % EM effect	-18	-13	-45	-45

Robust standard errors in parentheses, clustered at court level. <sup>+</sup>  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . Prison conviction estimates are obtained by bivariate probit. Prison sentence length estimates are obtained by jointly estimating a Tobit and a Probit on the sample of reoffenders. All regressions include the full set of control variables and correct for endogeneity of EM using the same instruments.

accumulated over the entire five-year follow-up period<sup>23</sup>. The estimated marginal effects are reported in Table 3.4.

Estimates are substantial and suggest that EM reduces the probability of receiving a new prison conviction after five years by eight percentage points (18%). When focusing on reoffenders only, the substantial beneficial effect of EM remains, with an estimated 13% reduction in prison sentences for recidivism. The same pattern emerges for total length of future imprisonment (modelled as a Tobit censored in zero for those who are never resented to prison) : serving time at home under EM reduces by half the expected prison time that reoffenders accumulate over the follow-up period (nine months over an average of 20 months).

Our severity estimates thus suggest that EM offenders commit less serious crimes than ex-prisoners in the next five years, conditional on recidivism. This interpretation is in line with the literature on hardening criminality inside prison (Abrams, 2011)<sup>24</sup>.

23. Alternative sentences are coded as zero (censored) while the few life sentences are coded as 30 years (the maximum in the sample)

24. We also consider whether EM affects certain types of new offenses more than others. The estimates, presented in Table 3.10 in Appendix E, are imprecise and inconclusive.

TABLE 3.5: Causal effects in different time windows

	Q1	Q2	Y1	Y2	Y3	Y4	Y5
<i>ElectronicMonit.</i>	-0.0290 (0.0269)	-0.0578 <sup>+</sup> (0.0307)	-0.0933 <sup>***</sup> (0.0263)	-0.0848 <sup>**</sup> (0.0308)	-0.1148 <sup>***</sup> (0.0346)	-0.0906 <sup>**</sup> (0.0313)	-0.0711 <sup>*</sup> (0.0341)
N	3001	3001	3001	3001	3001	3001	2827
Recidivism Rate	16.6%	27.5%	39.3%	51.9%	60.3%	63.9%	65.4%
EM effect in %	(-17.5)	-21.0	-23.7	-16.3	-19.0	-14.2	-10.9

Robust standard errors in parentheses, clustered at court level. <sup>+</sup>  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . The table reports Average Marginal Effects of bivariate probit models (ML). All regressions use the same two instruments and the full set of control variables.

## 3.6 Discussion

### 3.6.1 Incapacitation, deterrence, or genuine long-term change ?

While under electronic monitoring, offenders are compelled to respect strict home curfews for long hours, with permission to leave only for work or other rehabilitative activities. One may therefore suspect that the estimated crime-prevention effect of EM is explained primarily by incapacitation at home. However, we dismiss this view for several reasons : first, the average period under electronic monitoring is only 73 days (about half of EM offenders spend no more than two months, and 95% less than six months) so it seems unlikely that such short-term incapacitation may explain the substantial effects observed five years later. Second, we obtain very similar estimates when we start the five year clock after the end of the monitoring period (Table 3.9), which eliminates the short-term incapacitation-at-home effect. Third, the percent reduction in recidivism thanks to EM is not particularly great in the beginning (while under home curfew), but actually remains quite stable in the medium term (one to three years). To see this time pattern, we estimate the effect of EM on recidivism at different time periods : after only one quarter, one half year, and then each year up to the fifth year after release. The results in Table 3.5 show that the percentage reduction in recidivism is similar in the first quarter (-17.5%) and after three years (-19%). This pattern provides further evidence that the crime-prevention effect of EM is not driven by short-term incapacitation under home curfew : its impact remains similarly strong several years after the end of supervision.

Overall, our interpretation is that the beneficial effect of electronic monitoring on long-term recidivism derives from a mix of rehabilitation and deterrence. Indeed, our estimates

in Table 3.3 suggest that the obligation to work while under EM helps prevent recidivism, probably through better attachment to the labor market and increased opportunity cost of crime. Also note that home curfew implies spending more time with family, and conversely less time outside exposed to criminal peers and opportunities. In addition to opportunity cost, such changes in social ties may foster change in preferences and attitudes, promoting desistance from crime (Hucklesby, 2009). Second, our results show that EM is most effective on offenders who have already been to prison before; this may suggest that receiving EM is not perceived as a lenient punishment by former prisoners, but more as a second chance and an opportunity to be seized : therefore, reduced deterrence (namely the impunity effect) may not be a concern for ex-prisoners, but only for inexperienced offenders who have never been in prison. Finally, we find that EM has much more impact when it is accompanied by control visits at the home of the offender. This last finding suggests that visits make the probability of detection more salient in the minds of offenders (upward updating of  $p$  in the beckerian tradeoff), and that this effect lasts surprisingly much longer than simply during the monitoring period. All in all, we take this as evidence that short-term punishments in the community such as EM can have substantial, long-term effects on criminal behavior, provided that they include ingredients of both rehabilitation and deterrence.

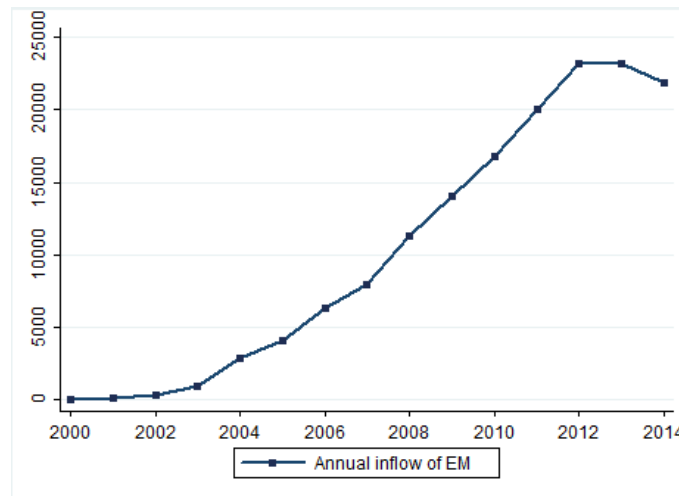
### 3.6.2 Current validity : diminishing returns and changes in supervision ?

Our results are obtained from the early stages of implementation of EM in France (from October 2000 to March 2003), a period when EM was a nascent alternative to incarceration, used in only a few hundred cases annually. As Figure ?? shows, the flow of offenders into EM rapidly increased after 2003, with about 6,000 EM delivered in 2006 for example. Today, it is estimated that about 20,000 offenders sentenced to prison avoid incarceration each year thanks to electronic monitoring. This substantial change may have widespread implications for the current validity of our findings.

First, the marginal effectiveness of EM on recidivism may be decreasing if judges first target the best candidates for EM, i.e. offenders who would benefit the most from being placed under EM instead of prison (in terms of crime reduction). Using our limited data on the first 580 EM recipients, we can offer only suggestive evidence on this hypothesis of diminishing returns. To do so, we separate EM offenders who were among the first to obtain EM in their court, and offenders who were placed under EM later on. We fix the cutoff value at 50 EM devices in a court to obtain two groups of the same size (with about 230



FIGURE 3.4: The widespread development of EM in France



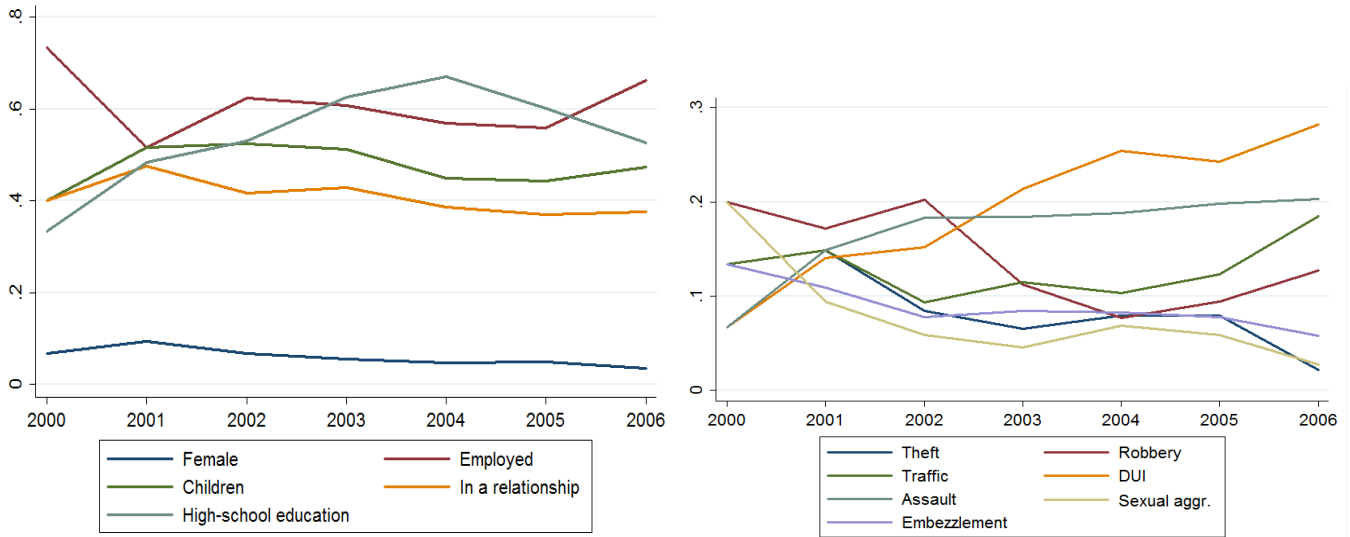
EM offenders each). We then run our bivariate probit model for both groups separately. The estimates in Table 3.11 (Appendix E) do not provide any evidence of marginally decreasing returns to EM : the estimated effect of EM on recidivism is very similar among offenders who were among the first to obtain EM (-6.1 pp) and later recipients (-6.7 pp). Note that the correlation coefficients  $\rho$  (zero in the first group, and significantly negative in the second group) suggest that judges were better at selecting EM recipients based on unobservable characteristics after a while (here, after 50 EM were delivered). However, this better selection of EM offenders over time did not lead to any sizable change in treatment effects in our data, suggesting that EM can be effective on a wide range of offenders.

To further explore changes in the selection of EM offenders over time (and the potential implications for EM effects), we utilize more recent data collected by the French Prison Administration (Kensey and Narcy, 2008) on a large representative sample of about 2,000 EM offenders, from 2003 to 2006 (covering about 20% of all EM over the period). Figure 3.5 reports the changes in the main observable characteristics of EM offenders between 2000 and 2006<sup>25</sup>. Overall, there is little change in the profile of EM offenders, except for the increase in the portion of DUI offenses. This relative stability suggests that the process of selecting EM offenders remained similar between 2000 and 2006.

In terms of supervision however, the widespread development of EM in France had major consequences. Figure 3.6 shows the evolution of control visits at home (blue bars) : from 2000 to 2002, about two thirds of EM offenders received at least one control visit from parole

<sup>25</sup>. Unfortunately the data doesn't include recidivism or past incarceration (which we find an important mediator in the effectiveness of EM).

FIGURE 3.5: Observable characteristics of EM offenders



officers during their monitoring period. After 2003, these controls declined dramatically and almost disappeared in 2006<sup>26</sup>. Similarly, the median duration of EM sharply decreased over time, from about 80 days in 2000 to less than 50 days in 2006. This pattern suggests that the widespread development of EM in France coincided with a sharp decrease in the supervision of EM offenders by parole officers. On the other hand, there is no major change in the occurrence of incidents (about 40% of cases throughout the period), control visits after an incident (less than 10%), or obligation to work (around 80%).

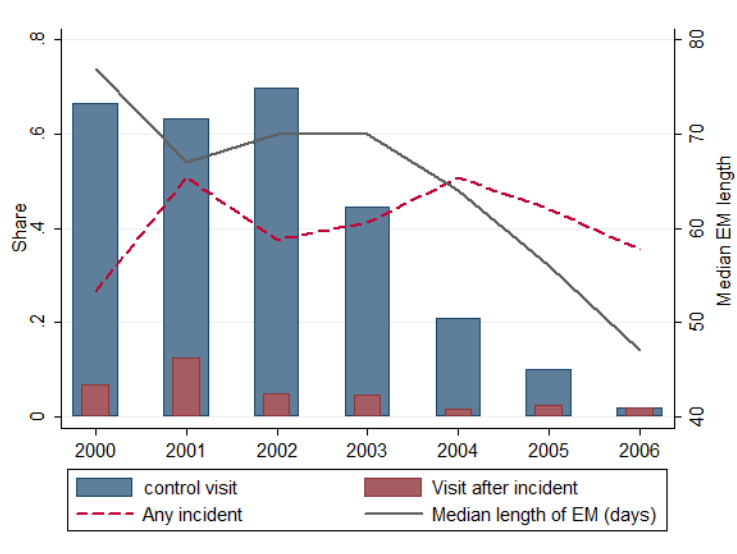
Overall, we view the dramatic fall in control visits and in the length of supervision as a threat to the current validity of our findings : indeed, our estimates in Table 3.3 suggest that EM was mainly effective in reducing five year recidivism among offenders who received control visits, and who were monitored for longer periods, while it had a lesser impact on the others.

### 3.7 Conclusion

This article provides new evidence on the effects on future crime of serving a prison sentence at home under electronic monitoring *instead of* incarceration. To identify causal effects from the use of EM, we exploit the gradual introduction of EM in France, which started as a pilot experiment in four courts (in 2000-2002) before being adopted gradually by

26. Correspondance with several practitioners suggests that such control visits are very rare today as well. But unfortunately, we do not have more recent data to support this trend.

FIGURE 3.6: Supervision and Incidents under EM



an increasing number of courts.

Our results show that simple comparisons between EM offenders and ex-prisoners highly overestimate the beneficial impact of electronic monitoring on recidivism ; the estimated gap in reconviction falls from about 14-15 percentage points to 8-9 pp after including a rich set of control variables. When we also correct for selection based on unobservables, using instruments capturing the staggered introduction of EM across courts over time, the estimated treatment effect falls further to 6-7 percentage points. However, this effect is statistically and economically significant : it implies a reduction in the probability of reconviction of 9% to 11%. We show that this effect, measured after five years, is not a result of short-term incapacitation at home, and that it applies to a wide range of offenders (note that none of our estimates suggest any criminogenic effect of EM on any subgroup). Actually, EM seems to foster long-term change and desistance from crime, with combined reductions in the probability and seriousness of criminal activity, at a much lower cost than prison.

We also investigate the mechanisms driving our results. First, the beneficial effect of EM is greater on offenders who had already experienced prison, who were required to work under EM, and to a lesser extent on offenders who had children. While these findings confirm that labor-market and social ties are important factors of desistance, they also suggest that EM can maintain specific deterrence among offenders who know what is at stake (most probably, reincarceration) from their own experience. Conversely, the beneficial effect of EM is less prevalent among offenders who serve their first prison sentence at home under EM (instead of incarceration) and is virtually zero when surveillance is not salient (through control visits at

home). Overall, we view these results as evidence that EM and incarceration can complement each other and are part of an effective "policy mix" : more precisely, EM can achieve significant crime reduction whenever the threat of reincarceration is sufficiently credible and salient.

In terms of public policy, this result has two implications. First, EM may well be more effective as a back-door strategy, or as a second sentence after prior incarceration : initial incarceration leads to some level of specific deterrence, while early release under EM offers a second chance for rehabilitation. This interpretation is consistent with the significant crime-reducing effects of EM found in England and Wales (with EM as an early release device) or in Argentina (with EM as an alternative to longer pre-trial detention in very harsh prisons). Second, supervision under EM is key : mild surveillance with no control visits and shorter monitoring periods, which followed the rapid development of EM in France after 2004-2005, are very likely to reduce the benefits of EM. This change in supervision practices inevitably calls into question the effectiveness of EM in France today.



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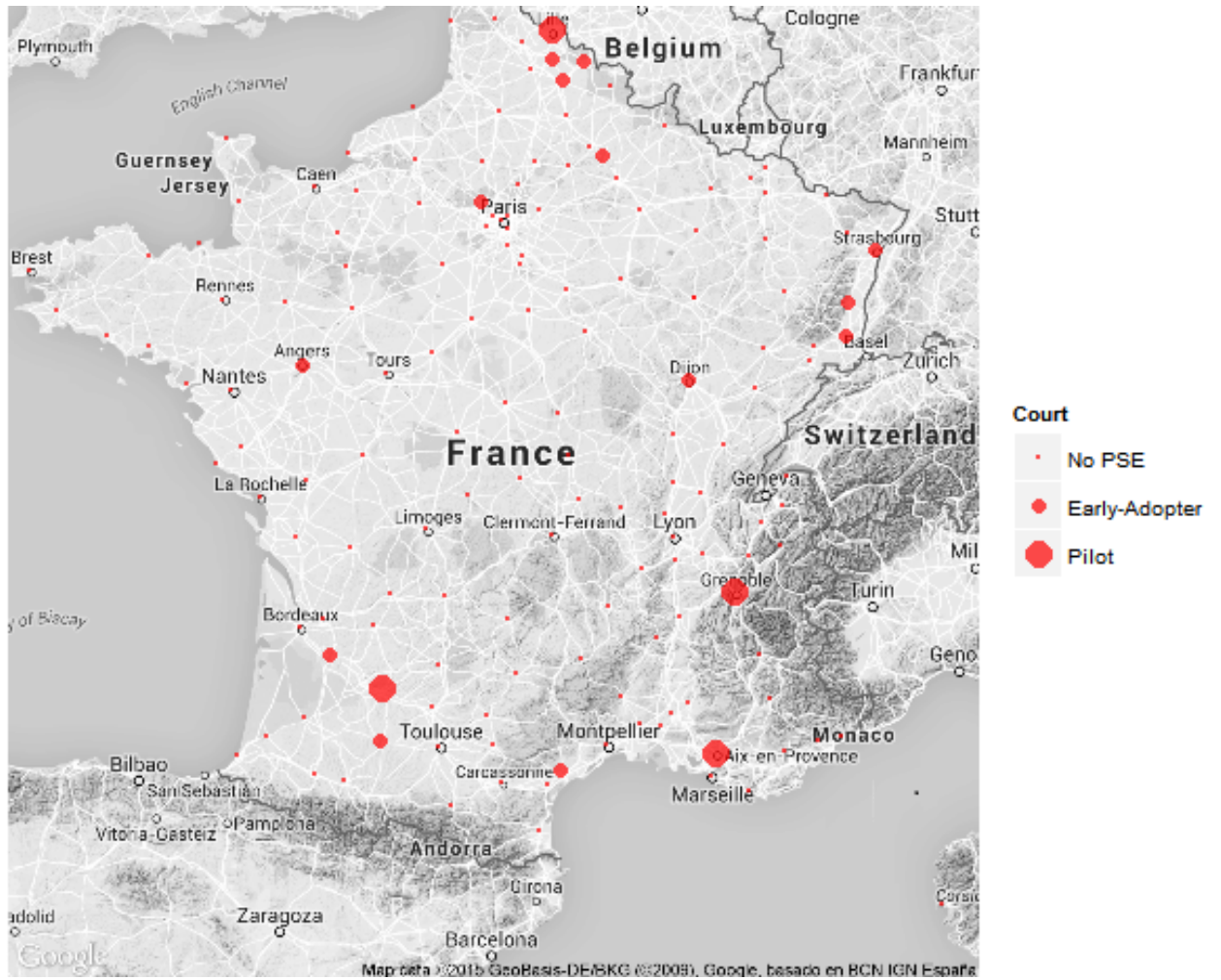


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Appendix

Appendix A : Institutional Context

FIGURE 3.7: Map of EM roll-in in French courts (2000-2003)



## Appendix B : Data and Descriptive Statistics

**Sample restrictions** Our initial sample contains 9014 individuals, 515 EM offenders (treated group) and 8499 prisoners (control group). However, we make several sample restrictions to drop those offenders who had clearly no chance of obtaining EM, regardless of its availability in their court.

First of all, many observations are not usable due to death or absence of criminal record for example. Moreover, some individuals exhibit missing values for important variables, such as sentence length. We decided to drop those offenders (43 EM and 1592 prisoners, representing respectively 8.3% and 18.7% of the initial samples), leaving us with a sample of 7379 individuals (6907 ex-inmates and 472 ex-EM).

Because having a home was a necessary condition for being placed under EM, we also dropped all ex-inmates who did not have a home when they arrived in prison (981 individuals, 11.5% of the initial sample). Our sample then contains 6398 individuals, split into 5926 ex-inmates and 472 ex-EM offenders.

A second criterion is required to get EM : offenders should be convicted to a prison sentence of no more than one year. Thus, we discard from our sample the 1973 prisoners (23.2% of the initial sample) and 11 EM offenders (2.1%) who were sentenced to longer periods. The fact that a handful of offenders received EM while convicted for sentences longer than one year (but no more than two years for all of them) is puzzling. One explanation may be a coding error in sentence length, or in the variable for front-door EM (i.e. these 11 offenders actually obtained back-door EM after serving time in prison). This leaves us with a total sample of 4414 individuals, including 3953 ex-prisoners and 461 ex-EM.

We also focus on convicts whose incarceration, if any, took place after final conviction. We conversely drop inmates who were held in pretrial detention, as well as those whose prison sentence started exactly on the day of conviction (bench warrants). Our view is that these prisoners are inherently different from EM offenders, as judges considered that their case required rapid incarceration. Conversely, the very fact that treated offenders obtained EM at home demonstrates that judges did not view them as too dangerous. This major difference leads us to consider as controls only prisoners who were incarcerated strictly after their final prison conviction. 1335 prisoners are dropped (15.7% of the initial sample), leaving a sample of 3079 individuals, with 2618 controls and 461 treated offenders.

Finally, we exclude from most regressions individuals whose follow-up period for new convictions is shorter than five years. This last restriction concerns four EM recipients and 248 inmates (2.9% of the initial sample). Our final study sample contains 2827 offenders sentenced to prison, of which 457 received EM directly and 2370 spent time incarcerated.

TABLE 3.6: Sociodemographic and judicial variables

Variables	Mean (1)	Mean (EM) (2)	Mean (Prison) (3)	Diff. <sup>a</sup> (4)	Range (5)
<b>Socio-demographic characteristics</b>					
Male	88.3%	93.2%	87.3%	***	[0;1]
Age <sup>b</sup>	30.6	33.2	30.1	***	[13.6;100.6]
<i>Standard deviation</i>	<i>(10.9)</i>	<i>(11.3)</i>	<i>(10.7)</i>		
Employment	41.9%	63.9%	37.7%	***	[0;1]
Couple	32.0%	42.9%	29.9%	***	[0;1]
Children	42.6%	50.3%	41.1%	***	[0;1]
<b>Prior convictions to prison</b>					
Frequency	61.5%	69.4%	60.0%	***	[0;1]
Average number	1.4	0.8	1.5	***	[0;27]
<i>Standard deviation</i>	<i>(2.8)</i>	<i>(1.8)</i>	<i>(2.9)</i>		
<b>Prior convictions to alternative sentences</b>					
Frequency	52.1%	50.8%	52.4%	<i>ns.</i>	[0;1]
Average number	1.0	1.9	0.8	***	[0;20]
<i>Standard deviation</i>	<i>(1.6)</i>	<i>(3.1)</i>	<i>(1.1)</i>		
<b>Type of initial offense<sup>c</sup></b>					
Acts of Violence	17.2%	18.4%	16.9%	<i>ns.</i>	[0;1]
Sexual assaults	4.6%	4.8%	4.6%	<i>ns.</i>	[0;1]
Traffic	20.5%	27.1%	19.2%	***	[0;1]
Property	39.0%	32.4%	40.2%	**	[0;1]
Drugs	10.7%	8.5%	11.2%	†	[0;1]
Immigration	1.9%	0.0%	2.3%	***	[0;1]
Weapons	2.0%	2.2%	2.0%	<i>ns.</i>	[0;1]
<b>Prison sentence</b>					
Initial sentence (months)	4.8	5.4	4.6	***	[0;12]
<i>Standard deviation</i>	<i>(3.2)</i>	<i>(3.0)</i>	<i>(3.2)</i>		
Early-release	20.0%	0.0%	23.8%	***	[0;1]
<b>Prison characteristics</b>					
Prison type <sup>d</sup>					
<i>Maison d'arrêt</i>	78.3%	80.1%	78.0%	<i>ns.</i>	[0;1]
<i>Centre de détention</i>	21.7%	19.9%	22.0%	<i>ns.</i>	[0;1]
Overcrowding rate	111.7%	113.5%	111.3%	<i>ns.</i>	[26.6%;250%]
<i>Standard deviation</i>	<i>(33.9%)</i>	<i>(30.3%)</i>	<i>(34.6%)</i>		
<b>Recidivism after 5 years (weighted for oversampling)</b>					
Any reconviction	65.4%	46.2%	66.2%	***	[0;1]
Reconviction to prison	54.0%	29.8%	55.0%	***	[0;1]
Sample Size	2827	457	2370		

The sample is composed of offenders who had a home and who started serving their sentence (in prison or under EM) strictly after their date of conviction. † p<sub>i</sub>10%, \* p<sub>i</sub>5%, \*\* p<sub>i</sub>1%, \*\*\* p<sub>i</sub>0.1%.

<sup>a</sup> Column (4) shows two-sample t-tests between EM group and prison group.

<sup>b</sup> Age at the beginning of the sentence (age at incarceration for ex-inmates or age at EM).

<sup>c</sup> "Acts of violence" include homicides, assault and battery, contempt of court and disrespect for police. "Sexual assaults" include rapes, sexual aggressions, offenses against honor and indecencies. "Traffic" includes drink-driving and other common offenses (speeding, driving license-related violations, etc.). "Property" includes all kinds of thefts and robberies, destructions and frauds.

<sup>d</sup> "Maisons d'arrêt" are prison facilities for pretrial detainees, or for those sentenced to less than 2 years. "Centres de détentions" are prison facilities for inmates sentenced to more than 2 years.

Appendix C : Exploiting the gradual rollout of EM in France

TABLE 3.7: Number of EM executed across courts by April 1<sup>st</sup> 2003

Court Name	EM executed
Agen	93
Aix-en-Provence	105
Grenoble	108
Lille	90
<b>Total Pilot courts</b>	<b>396</b>
Béziers	25
Dunkerque	13
Pontoise	13
Cambrai	5
Auch	4
Valenciennes	4
Douai	3
Colmar	2
Dijon	2
Strasbourg	2
Hazebrouck	1
Marmande	1
Reims	1
<b>Total Early-adopter courts</b>	<b>76</b>
<b>Total</b>	<b>472</b>

Note that all French courts that are not listed here did not deliver any EM device during the 2000-2003 period, and are labelled as *late adopters*.

Appendix D : Testing for endogenous selection of courts into EM

TABLE 3.8: Differences in 5-Year Recidivism Before the Introduction of EM

	2002 cohort		1996-97 cohort	
Pilot court	0.0203 (0.0392)	-0.0296 (0.0384)	0.0350 (0.1104)	0.0031 (0.0587)
Early-adopter court	0.0037 (0.0401)	-0.0002 (0.0219)	0.0015 (0.0529)	-0.0027 (0.0404)
<i>Ref : late-adopters</i>	0	0	0	0
Initial sentence length	x	x		
Stratification vars.		x		
Demographics		x		x
Past convictions		x		x
N	2270	2270	2207	2207

The table reports Average Marginal Effects (robust s.e. clustered at court level) from Probit regressions of 5-year recidivism on two dummies for type of court. Columns 2 and 4 include the same large set of additional control variables as in Table 2. The 2002 sample only includes prisoners who were incarcerated before EM was legally available in their court (Oct 1, 2000 in pilot courts; Jan 1, 2002 in other courts) : to account for this difference in sample restrictions, initial sentence length is controlled for as a second-degree polynomial. The 1996-1997 sample includes all prisoners, who are matched to the future type of their corresponding court, based on the location of their prison. † p|10%, \* p|5%, \*\* p|1%, \*\*\* p|0.1%

FIGURE 3.8: Prison overcrowding rates on January 1<sup>st</sup>

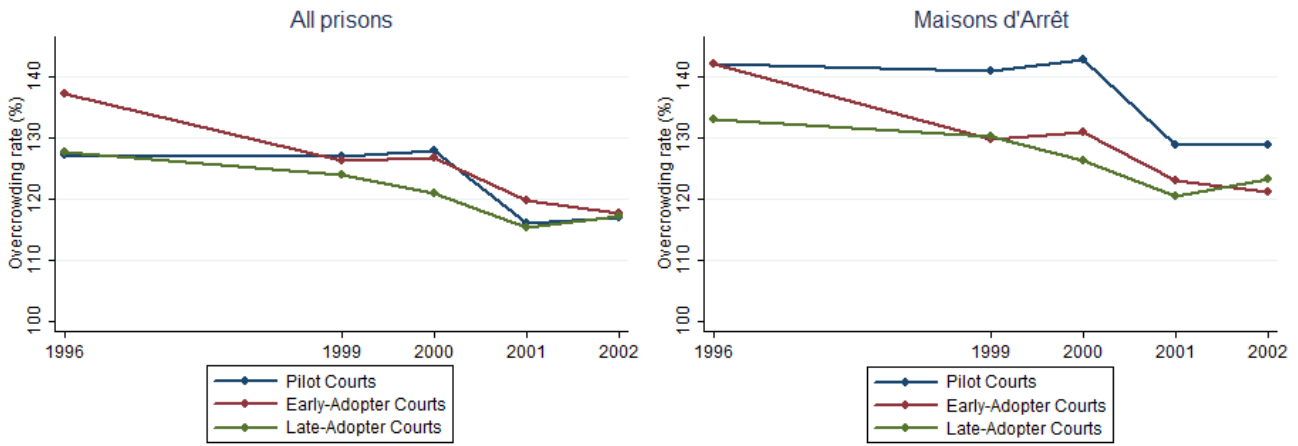
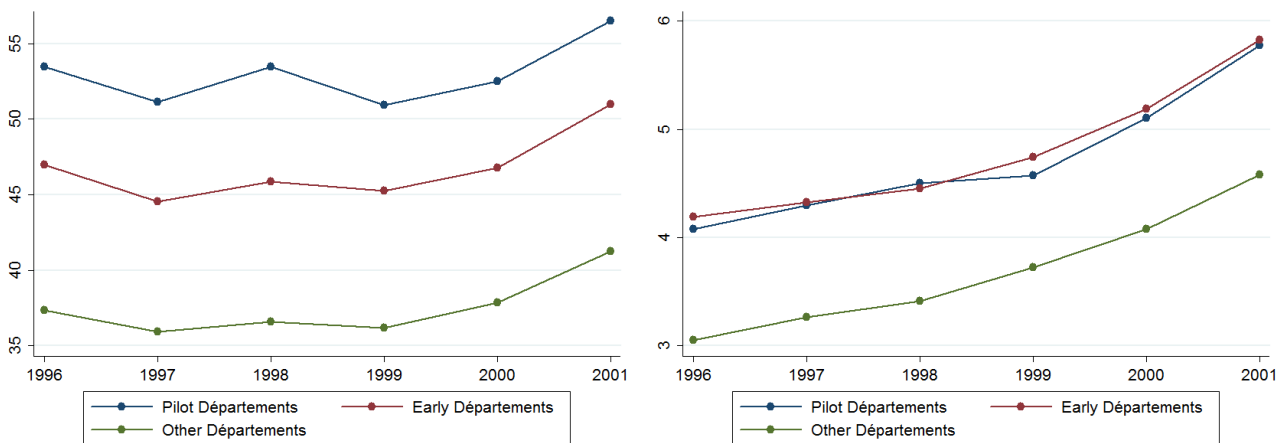


FIGURE 3.9: Rates of Property Crime (left) and Violent Crime (right)



Appendix E : Robustness Checks and Discussion

TABLE 3.9: Robustness : Electronic Monitoring and Recidivism

	Probit	Bi-Probit
100km neighboring courts	-0.0937** (0.0309)	-0.0768* (0.0390)
N = 1666		
Local crime rate (level + 2 year change)	-0.0857** (0.0314)	-0.0749* (0.0372)
N = 2716		
Released 3/1/2002 - 3/31/2003	-0.0783** (0.0301)	-0.0658+ (0.0345)
N = 2578		
Alt. Starting Date	-0.0868** (0.0281)	-0.0753* (0.0337)
N = 2754		
Instr : only courts with $\geq 20$ convicts	-0.0896** (0.0289)	-0.0789* (0.0345)
N = 2408		
Instr : Pilot, early-adopter	-0.0832** (0.0286)	-0.0696* (0.0330)
N = 2827		

Robust standard errors in parentheses, clustered at court level. +  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . All regressions include the full set of control variables. Average Marginal Effects reported.

FIGURE 3.10: Density of Date of Release

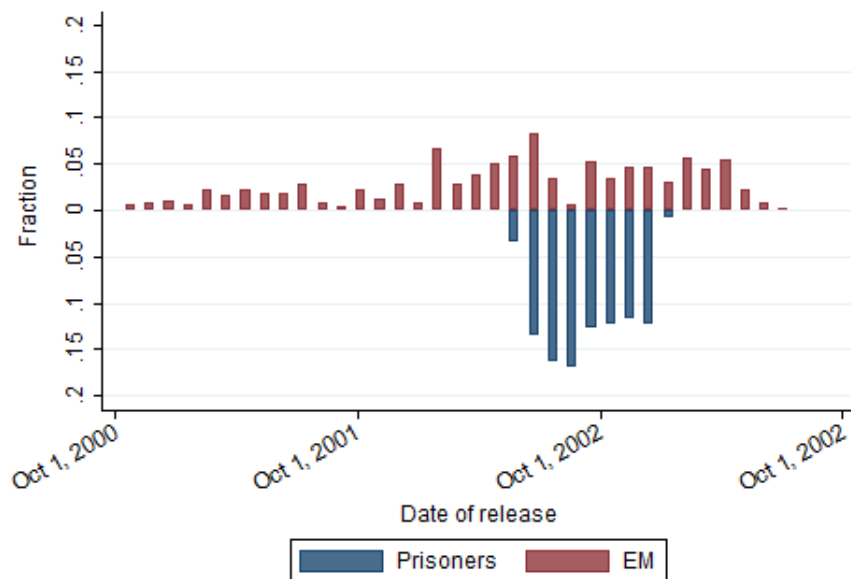




FIGURE 3.11: Evolution of Aggregate Recidivism as Courts Adopt EM

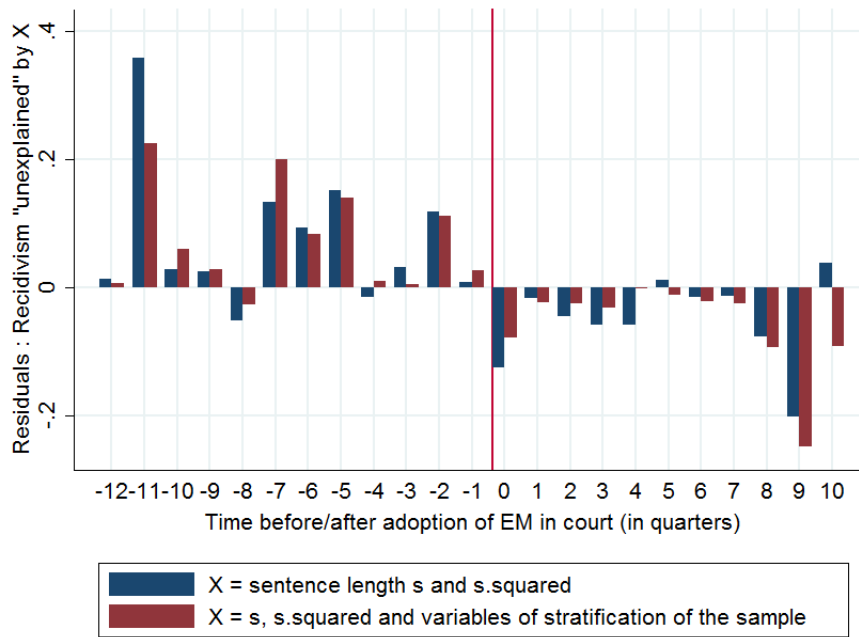


TABLE 3.10: Crime-specific effects

	Type of New Crime		
	Property	Traffic	Other
<i>ElectronicMonitoring</i>	-0.0028 (0.0214)	-0.0322 <sup>+</sup> (0.0176)	-0.0226 (0.0170)
N	2827	2827	2827
Recidivism Rate	17%	16%	20%
Estimated % EM effect	(-2)	-21	(-11)

Robust standard errors in parentheses, clustered at court level. <sup>+</sup>  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . Crime-specific estimates are obtained simultaneously by 3SLS to achieve convergence. All regressions include the full set of control variables and correct for endogeneity of EM using the same instruments.

To investigate the effect of EM on crime types, we classify new offenses in three categories with high recidivism rates : property crime (17% recidivism), traffic offenses (16%) and others such as assaults, drug-related offenses, etc. (20%). We estimate these three (non-mutually-excluding) crime-specific probabilities on EM treatment simultaneously by Three Stage Least Squares.

TABLE 3.11: Marginal effectiveness

	Number of previous EM in court	
	< 50 EM	≥ 50 EM
<i>ElectronicMonitoring</i>	-0.0610 (0.0392)	-0.0666 <sup>+</sup> (0.0394)
$\rho$	0.0094 (0.1149)	-0.3125** (0.1022)
N	2601	2596

Robust standard errors in parentheses, clustered at court level. <sup>+</sup>  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$ . The table reports Average Marginal Effects after bivariate probit, using *CourtEverUsedEM* as IV (second instrument not significant).



## Chapitre 4

# The Cost of Slow Justice : How Delayed Incarceration Fuels Recidivism

### **Abstract**

Criminal punishment often occurs late after the offense. This paper investigates how such delay affects reoffending behavior in a sample of prisoners in France. To overcome omitted variable bias (due to selective effort and diligence to arrest, prosecute and incarcerate the most crime-prone offenders), the paper exploits two sources of exogenous variation in prosecutors' use of Fast-Track Procedures (FTP) in France : the first variation is a discontinuity at age 18 in eligibility to FTP, while the second is the discrepancy in FTP use between large and smaller courts. Both empirical strategies yield similar results : fast-track incarceration has large significant deterrent effects on recidivism, suggesting that slow justice greatly reduces the potential benefits of punishment.

*The swifter and closer to the crime a punishment is,  
the juster and more useful it will be.*

---

Cesare Beccaria, *On Crimes and Punishments* (1764)

## 4.1 Introduction

It is widely assumed since Cesare Beccaria's seminal contribution that punishment is most effective when sanctions are swift, certain and severe. While there is extensive research on how probability and severity of punishment affect criminal behavior, there is surprisingly little work on the celerity of sanctions, i.e. the time between crime and punishment. This is particularly puzzling as judicial delays, while malleable by public authorities<sup>1</sup>, can be very long in practice : in France for example, the median time between crime and conviction (or dismissal) is around 5 months for all 600,000 criminal cases judged in 2013, with an average duration of 10 months (Carrasco, 2015). Regarding offenders convicted to prison (without bench warrant<sup>2</sup>), the median duration from trial to incarceration is around 8 months (Creusat, 2013). These substantial delays are a source of great concern in public opinions : in France, a recent poll shows that 95% of people think the judiciary is too slow and 66% argue that shortening judicial delays should be a top priority in justice reform (Cretin, 2014).

Theoretically, slow justice is likely to generate extra-crime both *before* and *after* punishment. First, a simple model of criminal decision-making suggests that expecting late punishment will push more people into crime because future sanctions are more discounted (less deterrent) than immediate sanctions, and because they allow criminals to benefit longer from criminal returns before paying the cost of crime (e.g. incapacitation in prison). Second, experiencing punishment late after offending can reduce the perceived fairness or legitimacy of sanctions and yield defiance among convicts (Sherman, 1993), making sanctions ineffective or even criminogenic for those who experience them with delay. Note that criminals, who are often characterized by present-orientation (Akerlund et al., 2014; Mastrobuoni and Rivers, 2016), should be particularly sensitive to delays : therefore, late punishment is unlikely to deter criminal involvement (and criminal intensity) among crime-prone individuals.

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1. For example, public authorities can increase resources on law enforcement (police) and promote fast-track procedures to hasten the arrest, prosecution, conviction and sentencing of offenders. Many countries introduced such fast-track procedures in the last years.

2. Around 30% of offenders convicted to prison receive a bench warrant, i.e. start to serve their prison sentence immediately after conviction. The other 70% are left free after trial and are later incarcerated.

The contribution of this paper is to estimate the effect of delayed incarceration on recidivism after release. The main empirical challenge is that delay of punishment is presumably related to unobserved traits that may themselves affect recidivism : for example, police and the judiciary presumably exert more effort to arrest, prosecute and incarcerate the most dangerous or crime-prone offenders, introducing omitted-variable bias in a regression of recidivism. To overcome this issue, I exploit two plausibly exogenous sources of variation in access to Fast-Track Procedure (FTP) in France : first, I take advantage of a (fuzzy) discontinuity in eligibility to FTP at age 18 ; second, I exploit the differential use of FTP across French courts, depending on court size and caseload. These two features are used as instrumental variables to estimate the reduced-form causal effect of rapid *versus* slower incarceration on future crime in a sample of prisoners released in year 2002. Both strategies show sizable deterrent effects of fast-track incarceration on recidivism, suggesting that “slow justice” reduces greatly the potentially beneficial effects of criminal punishment.

## 4.2 Related research

The seminal model of crime and punishment of Becker (1968) assumes that sanctions, as well as criminal benefits, are contemporaneous. In practice however, while many criminal benefits (either monetary or psychological) occur during or immediately after crime, punishment takes time : it involves police arrest, prosecution, conviction, and effective execution of the sentence (e.g. incarceration), a process which often lasts several months, or years. Intuitively, such delay may well weaken the deterrent effect of punishment as individuals tend to weight present outcomes (criminal benefits) more than future outcomes (cost of punishment). Therefore, a slight refinement of Becker’s static model of criminal decision-making is to discount the cost of punishment to account for the expected delay of such punishment<sup>3</sup>. Then the decision rule faced by an individual contemplating a criminal opportunity writes :

$$B - \delta^t pC > \underline{U} \tag{4.1}$$

where  $B$  captures the criminal benefit,  $C$  the cost of punishment and  $\underline{U}$  the reservation utility of not offending. As long as individuals are present-oriented (discounting factor  $\delta < 1$ ), delays between crime and punishment (measured as the number of periods  $t$ ) increase individuals’

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3. Several papers propose more formal extensions of the Beckerian model, such as Davis (1988) (two-period model) and McCrary (2010) (dynamic stochastic model).

propensity to commit crime.

In practice, individuals do not know when punishment will occur so expected delay  $t$  is likely to be a function of past experience. In the context of prisoners, experiencing fast incarceration (instead of slow incarceration) in one criminal case is likely to induce offenders to revise downward  $t$ , the delay of incarceration they expect in case of recidivism. Anwar and Loughran (2011) find evidence of such Bayesian updating for  $p$ , the probability of arrest : they estimate that getting arrested for one offense increases by 6% the perceived risk of arrest among a sample of young criminals. Such updating in expected delay by inmates would lead similar offenders to make different decisions after release (recidivism or not) depending on their past delay of incarceration.

According to Equation 4.1, this criminogenic effect of delay will be largest among the most present-oriented individuals. Akerlund et al. (2014) offer empirical evidence suggesting that criminals display such time preferences : in a sample of Swedish youth, they find that present-orientation measured at age 13 significantly predicts criminal behavior 18 years later. Similar results are obtained by Nagin and Pogarsky (2004) from the *AddHealth* data. Using Italian prisoners' recidivism in reaction to the 2006 mass release experiment, Mastrobuoni and Rivers (2016) estimate the average annual discount factor of prisoners at  $\delta = 0.74$ . Overall, there is compelling evidence that criminals are very present-oriented, making celerity of punishment much more deterrent than severity of punishment.

Surprisingly, existing research on the effect of punishment celerity on future crime is rather scarce worldwide (much more limited than research on severity or certainty). One exception is a paper on Czech Republic by Dušek (2015) who exploits the introduction of a fast-track (simpler, shorter) procedure : the author finds little evidence of a deterrent effect of fast procedures on crime ; instead, the reform led to a large increase in driving offenses, probably due to reallocation of police efforts toward those crimes. However, this absence of a deterrent effect of fast-track procedure on crime rates may be explained by a lack of public information regarding the reform, hence no updating of priors on expected delay among offenders. This scenario would echo Hjalmarsson (2009) who fails to find evidence of large updating in expected sentence among young U.S. offenders when they turn 18 (even though adult law is much harsher than juvenile law in practice). Conversely, exploiting variation in the length of criminal trials across provinces in Italy, Dalla Pellegrina (2008) finds a deterrent effect of (shorter) trial delay on property crime, as expected from Equation 1. This deterrent effect is consistent with Italian offenders being aware of province-level delays (which seems

realistic) and responding to these incentives. Therefore, a key ingredient to obtain deterrence is information and updating of offenders' perceptions.

Other papers focusing on probationers also provide related evidence. In an influential randomized experiment, Hawken and Kleiman (2009) evaluate Hawaii's HOPE probation program, which entails "swift and certain" punishment for convicted offenders who do not respect their obligations under probation, such as drug abstinence : sanctions consist in automatic and quasi-immediate incarceration (violators are usually sent to prison for the weekend). Hawken and Kleiman (2009) show that this strict policy leads to large increases in compliance, with 80% lower positive drug testing notably. Similarly positive results are obtained in other settings, for example among alcohol-involved offenders in South Dakota (Heaton et al., 2015).

Finally, research on cognitive behavioral therapies (CBT) strongly suggests that present-orientation and impulsivity are central to criminal involvement. In a series of large-scale randomized experiments in Chicago, Heller et al. (2015) consistently find that programs teaching youth to think ahead and act less automatically generate large benefits, with considerable reductions in school dropout, violent crime and custody. In another randomized experiment in Liberia, Blattman et al. (2015) also show that CBT programs promoting self-control and non-criminal self-image reduce criminal involvement among offenders during at least one year (more effectively than cash).

Overall, there is compelling evidence that present-orientation is a key determinant of crime, but the evidence on punishment celerity and crime is more mixed. This paper focuses on a group of offenders who have personally experienced punishment (ex-prisoners) to estimate their behavioral reaction to faster *versus* slower incarceration.

## 4.3 Institutional Context and Data

### 4.3.1 The French penal procedure

In France, prosecutors have a major role in the penal process spanning from crime to punishment : they decide the type of "track" that a case will follow after arrest. When a suspect is arrested for a crime and placed under police custody, the prosecutor has to choose whether to charge the suspect, or to drop charges. The decision to prosecute is the first step that will eventually lead to conviction in a trial. However, in the most common procedure, the trial date is usually fixed several months later due to the large caseload faced by most courts.



During this period from prosecution to trial and conviction, strict rules apply with regards to pre-trial detention : the most serious offenders can be sent to prison for pre-trial detention but only for a limited duration (in most cases, for a maximum duration of 4 to 12 months), and in practice most non-felony offenders are left free from prosecution to trial. Even after conviction by the court, all offenders convicted to prison sentences shorter than 1 year are automatically left free at trial (no bench warrant possible), and will only get incarcerated if no alternative sanction is offered by a second judge (such as home curfew under electronic monitoring). In this case, the prosecutor is in charge of the concrete execution of the sentence (e.g. fixing the date of incarceration of the convict). However, if the sentence is longer than 1 year, the court has full discretion to incarcerate the offender immediately after trial or to let him free. Finally, another source of delay is that about 20% of offenders are not present at their trial, so convicts have to be properly notified of their sentence before eventual incarceration (Creusat, 2013). This is particularly problematic with homeless people and illegal migrants. All in all, the French classic procedure generates very long delays between crime and incarceration among low-to-medium severity offenders (see Table 4.1 below).

Since 1981 however, in order to hasten this process for simple cases, prosecutors can opt for a faster procedure at the end of police custody, called *Comparution Immédiate* (“immediate hearing”). This fast-track procedure drastically hastens the whole process from crime to punishment thanks to simpler procedural steps and far shorter deadlines. The first critical feature of this fast track procedure is that the offender has to be judged *on the same day*, or in the next three days if no judge is on duty. In most cases, offenders are incarcerated for pre-trial detention for the few days before trial, but for the minority who are left free, the trial has to occur in the next 2 months. At trial, a vast majority of offenders are convicted to prison sentences (usually shorter than 3 years or so). The second critical feature of fast-track prosecution is that, when convicted to prison, offenders are directly sent to prison to serve their sentence (bench warrant), no matter the length of the sentence. These two features (very rapid trial and incarceration) clearly depart from the French classic procedure and lead to very short delays between crime and punishment.

Many cases are eligible to Fast-Track prosecution : the main condition, which applies frequently, is that criminal charges and guilt are well established and do not require further investigations. The legal criteria for eligibility are loose too : the offender has to be over 18 years old<sup>4</sup>, the offense should be a misdemeanor (*délit*), and the maximum sentence

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4. Analog fast-track procedures were introduced in the 1990s to extend such FTP to juveniles offenders.

should range from 2 to 7 years<sup>5</sup> (low-to-medium severity). The prosecutor usually makes his decision to use fast-track prosecution (or the classic procedure) based on brief discussions with policemen during custody, regarding the details of the offense (is it serious enough to warrant an immediate sanction?) and the profile of the offender (is he a repeat offender? is he likely not to show up at trial?). This decision process is described in more details by Viennot (2007).

Overall, the delay from offense to incarceration (either as pre-trial detention or as a prison sentence) greatly depends on prosecutors' initial track choice (Creusat, 2013). Figure 4.1 shows the cumulative density of delay for the two main types of procedure (fast-track or classic track) in a sample of prisoners released in 2002 (more details on the sample below). When FTP is chosen, about 80% of soon-to-be prisoners are incarcerated within 40 days after the (start) date of offense, compared to only 20% in the classic procedure. Table 4.1 provides further evidence that the distribution of delay differs considerably between the two procedures. Half of FTP prisoners are incarcerated within 2 days after offending, while the median delay is almost 2 years in standard procedures. Of course these are raw differences, which may be contaminated by differences in types of cases. In the next sections, I restrict the sample to improve comparability between the two groups, but differences in delay remain very large.

TABLE 4.1: Distribution of Delay in days

Procedure	Q1	Median	Q3	Range	Average
Fast-Track (N=1924)	1	2	35	[0;4546]	161
No Fast-Track (N=4011)	186	672	1499	[0;9672]	1034

Sample of prisoners released in 2002, excluding unexploitable observations and negative delays (20 obs).

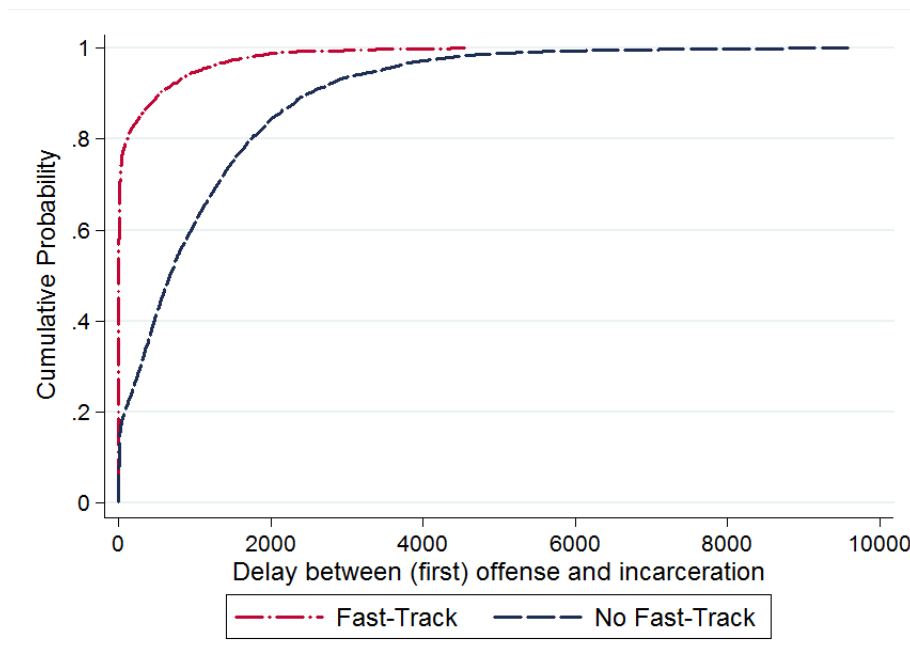
### 4.3.2 Dataset

The data sample is based on a survey from the French Department of Prisons on a cohort of prisoners released in year 2002. The original data base contains 8499 observations of prisoners released between June 1st 2002 and December 31th 2002. It relies on two sources of data, penal files and criminal records. Penal files are filled by prison staff during incarceration and contain basic sociodemographic data (gender, date of birth, self-declared employment and marital status, education, home city), as well as offense-related information (date, precise

More on this in Section 4.1.

5. The top limit of 7 years was later removed in a 2007 reform.

FIGURE 4.1: Cumulative Density of Delay by type of procedure



infraction, sentence) and incarceration-related data (location, dates of entry and release, sentence reductions). Criminal records register offenders' convictions, both before and after the incarceration that led to the 2002 release, up to year 2008. They are used to measure recidivism in the (exact) five years after day of release.

#### 4.4 Empirical strategies

Ideally, in order to test how fast *versus* slow incarceration affects reoffending after release, one would like to estimate a regression of recidivism on the celerity of punishment :

$$Recid_i = \beta_0 + \beta_1 Fast-Track_i + \epsilon_i \quad (4.2)$$

However, in such a regression, the variable of interest *Fast-Track* is presumably correlated with the error term  $\epsilon$ , leading to endogeneity bias on  $\beta_1$ . The reason is that a likely objective of the police and the judiciary is to minimize delay (i.e. hasten incarceration) for the most crime-prone offenders, by devoting more time and effort to arrest, prosecute, convict and incarcerate them. Therefore, offenders' intrinsic propensity to commit crimes, captured by  $\epsilon$ , should be positively correlated with fast incarceration. Another source of bias could also arise from using official criminal records data if the best criminals, in terms of avoiding rapid

arrest and incarceration for the first offense, are also good at avoiding future arrest and conviction in case of recidivism. *Fast-Track* would be spuriously correlated (again positively) with recidivism<sup>6</sup>. The presence of such omitted variables (intrinsic criminal propensity, ability to avoid or delay punishment) will thus bias the coefficient of interest  $\beta_1$  upward. The inclusion of control variables into the regression (such as criminal background, type of offense, etc.) may help reduce this bias, but it may not suffice to obtain a proper causal effect of *Fast-Track* on recidivism.

In order to achieve identification of the causal effect of fast *versus* slow incarceration on future crime, one needs to exploit exogenous variations in *Fast-Track*, i.e. differences in the probability of Fast-Track prosecution that are unrelated to individual-specific propensity to reoffend. In this paper, I exploit two different features of the French fast-track procedure. The first feature is a fuzzy discontinuity in eligibility to fast-track prosecution around age 18 : there is massive use of fast-track sentencing for adults, but eligibility to the analog procedure for minors is stricter and fast-track sentencing is seldom used for offenders under 18.

The second feature is a well-known discrepancy in courts' propensity to use fast-track procedures. In France, larger courts (located in large cities) face particularly large caseloads, with long queues before trial. To manage this problem, prosecutors tend to rely on shorter, simpler procedures such as FTP (but also alternatives to formal prosecution) to gain efficiency. This difference in caseload therefore leads to plausibly exogenous differences in probability of fast-track prosecution between similar offenders, depending on the size of their court.

#### 4.4.1 Discontinuity at age 18

In France, the main Fast-Track procedure, *Comparution Immédiate*, is restricted to adult offenders. However, since 1995, a similar procedure is available for juvenile offenders under 18 at date of offense<sup>7</sup>. The adult and juvenile fast-track procedures are similar in spirit, except that youth are judged by a juvenile-specific court (*Tribunal pour mineurs*), legal delays from prosecution to trial are a bit longer (between 10 days and 2 months) and eligibility to the juvenile FTP is stricter : offenders have to be known from the court for other recent offenses, and the maximum prison sentence for the offense should be over 3 years (5 years for those under 16). In practice, in addition to these slightly stricter legal criteria, prosecutors and

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6. Note that one could also think of a negative correlation between  $\epsilon$  and *Fast-Track* if prosecutors use this procedure to rapidly incapacitate offenders who display high fly risk if left free before trial (those are likely good at avoiding rearrest and reconviction too).

7. This procedure was initially called *Jugement à délai rapproché* then *Présentation immédiate*

juvenile courts are much more reluctant to use FTP than adult courts.

Therefore, a straightforward strategy is to exploit this age discontinuity in probability of Fast-Track prosecution to obtain a quasi-experimental estimate of  $\beta_1$ . Such age discontinuities are often used in economics of crime, mostly to estimate the deterrent effect of tougher sentences (when turning 18) on crime and recidivism (Hjalmarsson, 2009; Lee and McCrary, 2005; Pichler and Römer, 2011).

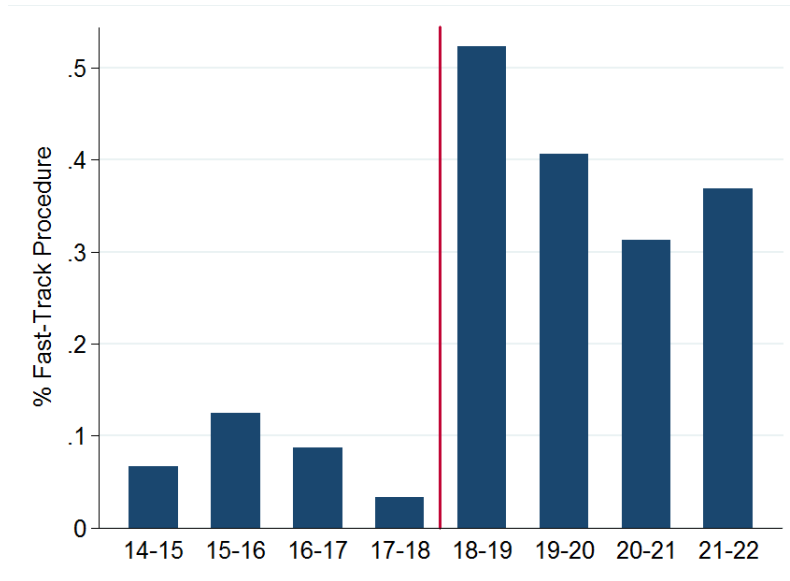
To employ this regression discontinuity design, I restrict my sample to offenders who are around the age threshold of 18 years old. Since the sample size is limited among very young prisoners, I use a bandwidth of up to  $\pm 4$  years around 18 (i.e. offenders of age [14;22] at offense). I also restrict the sample to offenders who were convicted for misdemeanors (low-to-medium severity crimes). The RD strategy crucially depends on the precise measure of age at offense : in simple cases, an offender is convicted for only one offense which occurred on a single day. Then, I compute age at offense as the difference between date of offense and date of birth, divided by 365.25. In more complicated cases where several offenses lead to one conviction and incarceration, I use the date of the last offense. Finally, if a single crime spans over several days or months (e.g. drug distribution), I use the end date of offense to compute age, as prosecutors use end date to assess offenders' eligibility to adult *versus* juvenile law.

### Graphical evidence

Before turning to the econometric specification and estimation, let's consider some graphical evidence. Figure 4.2 plots the fraction of offenders prosecuted via Fast-Track procedure, by age at offense. As expected from French law, fast-track prosecution is much more common among prisoners who were above 18 at offense (40-50%) than among juvenile offenders (10% or less).

Does this discontinuity in probability of Fast-Track prosecution convert into changes in recidivism after release? Figure 4.3 plots prisoners' probability of recidivism (left) and reconviction to prison (right) in the next 5 years. Both graphs show a large discontinuity in reoffending around the 18-years-old threshold : about 90% of prisoners who were under 18 at offense are reconvicted in the next 5 years, compared to 60% among prisoners aged [18;22] at offense. The recidivism gap is even larger for new prison convictions. Graphically, these differences do not seem to be driven by the tendency of older criminals to desist from crime (the well-known "age-crime curve"), since recidivism rates are quite flat on both sides of the 18-years-old cutoff.

FIGURE 4.2: Fast-Track Procedures by age at offense



These simple figures are consistent with the existence of a large beneficial effect of fast-track prosecution on recidivism after release<sup>8</sup>. However, many other differences between younger and older offenders may confound the true causal effect of fast-track prosecution on future crime. This possibility justifies the use of a proper econometric model (and also of a second, complementary identification strategy).

#### Econometric specification

Let's consider the following two-equation model of recidivism and *Fast-Track* prosecution :

$$Recid_i^* = \beta_0 + \beta_1 Fast-Track_i + \beta_2 Age_i + \beta_3 Age_i^2 + \mathbf{X}'_i \boldsymbol{\beta} + \epsilon_i \quad (4.3)$$

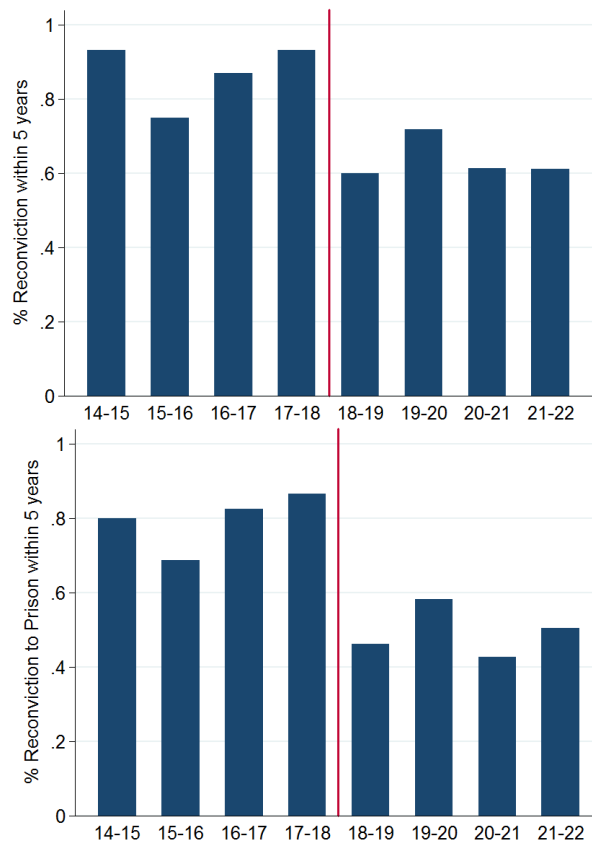
$$Fast-Track_i^* = \alpha_0 + \alpha_1 1(Age > 18)_i + \alpha_2 Age_i + \alpha_3 Age_i^2 + \mathbf{X}'_i \boldsymbol{\gamma} + e_i \quad (4.4)$$

This recursive bivariate probit model takes advantage of the discontinuity in FTP use by prosecutors at age 18, measured by  $\alpha_1$ , to obtain a causal estimate of  $\beta_1$ , the effect of *Fast-Track* on probability to reoffend after release. The dummy  $1(Age > 18)$  serves as an instrumental variable.

Both equations include the same set of control variables. Most importantly, I include a

8. From these graphs, one can compute a very rough estimate of the effect of *Fast-Track* prosecution on recidivism : let's consider that the discontinuity in probability of *Fast-Track* prosecution equals 40% points (10% on the left of the cutoff, 50% on the right), while the change in recidivism around age 18 is 30% points (90% on the left, 60% on the right). A simple calculation suggests that *Fast-Track* prosecution may reduce the risk of recidivism after release by an astonishing 75% points.

FIGURE 4.3: Recidivism and Return to Prison in the next 5 years after release



polynomial of age at offense to account for the smooth, plausibly non-linear, relationship between age and criminal propensity (a stylized fact known as the “age-crime curve”). As is common in the Regression Discontinuity framework (e.g. Hjalmarrsson (2009)), I use more flexible specifications for age with higher-degree polynomials in robustness tests.

The inclusion of other regressors should not be essential for identification, but helps to obtain more precise estimates. Moreover, including more regressors gives more credibility to the exclusion restriction that  $1(Age > 18)$  does not affect recidivism other than through the type of track chosen by prosecutors. For example, one may argue that juvenile prisoners under 18 commit different types of crime than older criminals, yielding different propensities to reoffend independently of prosecutors’ decision. To control for this possible difference on both sides of the age discontinuity, I therefore include a set of 10 dummy variables capturing the type of offense committed, and the maximum applicable sentence for the precise offense in the *Penal Code* (in months of prison). In a similar spirit, I include several sociodemographic characteristics measured at prison entry (gender, homelessness, employment status, parenthood, and French citizenship) and detailed information on previous criminal records (two dummies

capturing whether the prisoner had already been convicted to prison and to non-custodial sentences, and two continuous variables capturing the number of past convictions of each type) : these variables are likely to influence prosecutors' choice, as well as recidivism.

**Discussion of the RD design** Though intuitive, this regression discontinuity design has several drawbacks. First, focusing on young prisoners around 18 years old greatly reduces the sample size ( $N = 537$  with a 5-year bandwidth) and statistical power. Second, as in any RDD, this strategy is likely to yield a *local* average treatment effect (estimating how young offenders respond to a change in the celerity of incarceration) that may not coincide with the effect in the overall prison population (mostly adults)<sup>9</sup>. Third, the age discontinuity in fast-track prosecution may coincide with other discontinuities at age 18, so that the estimates capture the effect of other differences/treatments. For example, offenders under 18 are not judged by the same type of court (juvenile court *versus* adult court) and may consistently receive shorter sentences than those above 18 (which suggests the need to control for initial sentence length, as I do in robustness checks). Another possibility is that offenders under 18 do not face the same prison conditions as older offenders : this concern would be very relevant today, as all offenders under 18 are now incarcerated in juvenile-specific facilities with more rehabilitation services (*Etablissements Pénitentiaires pour Mineurs*). However, during the study period (2002), such facilities didn't exist in France so juvenile prisoners were incarcerated in the same prisons as adults (but in specific parts of the facility) so they essentially faced similar prison conditions. Finally, young offenders may receive more support from social services after release, compared to adults. Such differences in treatment may contaminate the RD estimates.

These drawbacks warrant the use of a second empirical strategy, to make sure that the results are not explained by the peculiarities of the French juvenile justice system.

#### 4.4.2 Court size

The second empirical strategy builds on the varying propensity of prosecutors to use FTP, depending on court size. The relationship between court size and FTP use is well-known in the French context : prosecutors in large courts tend to use fast procedures more frequently to manage their large caseload and gain efficiency. To visualize this pattern, Figure 4.4 plots the share of fast-track procedures (over all prosecuted cases) in French courts in year 2004

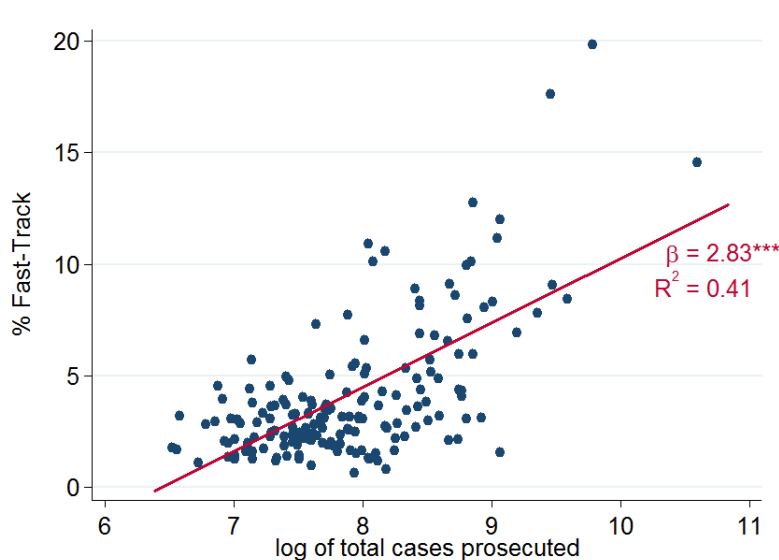
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9. Lee and McCrary (2005) show that juvenile offenders tend to be myopic, while adults are more deterrable.



depending on court size<sup>10</sup> : there is a clear correlation between prosecutors' propensity to use FTP and the (log) amount of cases they handle annually. 41% of the variance in fast-track use between French courts is explained by court size.

FIGURE 4.4: Fast-Track Procedures and Court Size



Econometric specification 2

This second, complementary strategy leads to a similar econometric specification :

$$Recid_i^* = \beta_0 + \beta_1 Fast-Track_i + \mathbf{X}'_i \boldsymbol{\beta} + \epsilon_i \tag{4.5}$$

$$Fast-Track_i^* = \alpha_0 + \alpha_1 LogCourtSize_i + \mathbf{X}'_i \boldsymbol{\gamma} + e_i \tag{4.6}$$

The recursive bivariate probit model now takes advantage of the cross-court differences in FTP use based on court size to identify  $\beta_1$ . Here the exclusion restriction assumes that court size can only correlate with recidivism through its effect on prosecutors' propensity to use fast-track procedures. However, it is well-known among practitioners that large courts operate quite differently from small courts : for example, large courts tend to pronounce more lenient sentences and to grant early release under parole<sup>11</sup> more frequently (everything else constant). Thus, for my exclusion restriction to be credible, I need to include additional control variables in vector  $X$ . I therefore add in all regressions a second-degree polynomial for initial sentence length, a dummy variable for early-release under parole, the overcrowding

10. Unfortunately, I do not have access to court-level data on prosecution before 2004.

11. *Libération conditionnelle* in French.

rate in local prisons as of January 1, 2002, and a dummy for Paris court (which gathers a large number of prisoners in the sample, N=755).

Since the sample available is much larger than for the RD design (no age restriction), I now restrict the sample to offenders with maximum sentences between 2 and 7 years (legal criteria for FTP) and who were over the age of 18 at offense.

## 4.5 Results

### 4.5.1 Using the discontinuity at age 18

As a benchmark, Table 4.2 simply reports average marginal effects of a probit model of recidivism on *Fast-Track* and control variables using different bandwidths around 18 years old.

In Column 1, a  $\pm 3$  years bandwidth with no control variables yields a very small and insignificant relationship between fast-track prosecution and recidivism : the difference in recidivism is less than 1% point. Including all control variables and using a larger sample (bandwidth up to  $\pm 5$  years around 18) has little effect on the estimates. On average, among young offenders, fast-track prosecution is not related to recidivism.

TABLE 4.2: Probit : Recidivism After 5 Years

	(1)	(2)	(3)	(4)
Bandwidth selection around 18	$\pm 3$ years	$\pm 3$ years	$\pm 4$ years	$\pm 5$ years
Fast-Track	0.0080 (0.0530)	0.0045 (0.0541)	0.0120 (0.0467)	0.0169 (0.0434)
Age at offense		-0.2519 (0.3659)	-0.3196 (0.2116)	-0.2055 (0.1583)
Age at offense squared		0.0063 (0.0099)	0.0077 (0.0056)	0.0045 (0.0041)
Controls		x	x	x
Obs.	358	356	461	537

Average Marginal Effects are reported. Standard errors in parentheses.

Significance : \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

However, as explained in Section 3, these estimates are likely to be biased upward if prosecutors selectively use fast-track procedures in order to punish and incapacitate rapidly the most crime-prone offenders (or if police exert more diligence to arrest them). Therefore, I now turn to the bivariate probit estimates which attempt to correct for this selection bias.

TABLE 4.3: Bivariate Probit : Fast-Track and Recidivism After 5 Years

	(1)	(2)	(3)
Bandwith restriction around 18	$\pm 3$ years	$\pm 4$ years	$\pm 5$ years
<b>Eq 1 : Recidivism</b>			
Fast-Track	-0.3435*** (0.1027)	-0.2670** (0.1361)	-0.1635 (0.1610)
Age at offense	0.0569 (0.3542)	-0.1369 (0.2185)	-0.1237 (0.1676)
Age at offense squared	-0.0015 (0.0095)	0.0033 (0.0057)	0.0026 (0.0042)
<b>Eq 2 : Fast-Track</b>			
Age at offense > 18	0.5104*** (0.1097)	0.4669*** (0.1040)	0.4349*** (0.1019)
Age at offense	-0.3715 (0.4444)	-0.1398 (0.2378)	-0.0919 (0.1833)
Age at offense squared	0.0089 (0.0116)	0.0033 (0.0061)	0.0023 (0.0046)
Correlation : $\rho$	0.77*	0.59*	0.37
Controls	x	x	x
Obs.	358	461	537

Average Marginal Effects are reported. Standard errors in parentheses.

Significance : \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

Table 4.3 reports average marginal effects after estimating Equations 3 and 4 as a recursive bivariate probit model. First, the instrumental variable  $1(Age > 18)$  behaves as expected. While the probability of fast-track prosecution is flat on both sides of the age cutoff, turning 18 generates a large increase in this probability of 50 percentage points on average, after controlling for sociodemographic and criminal background variables. This result is consistent with Figure 4.2. Whatever the bandwidth that is used, it is estimated that turning 18 increases the probability of fast-track prosecution by more than 40% points. Also note that the correlation coefficient  $\rho$  is positive, which is consistent with prosecutors targeting for FTP offenders with highest intrinsic criminal propensities.

Regarding the recidivism equation, the estimates for *Fast-Track* are large, negative and significant (except for the  $\pm 5$  years bandwidth) : focusing on the  $\pm 3$  years bandwidth, the results suggest that experiencing incarceration through a fast-track procedure reduces probability of recidivism in the 5 years after release by 34% points on average.

**Robustness checks** Table 4.5 in Appendix reports several robustness tests. First, instead of controlling for a second-degree polynomial of age at offense, I include a third-degree polynomial. The results are essentially unchanged. Second, I include as a control variable the length of the prison sentence : if judges convict offenders above 18 to longer prison sentences, then a discontinuity in average sentence length at the age cutoff may contaminate the IV results. The estimates in Table 4.5 are unchanged, so they do not support such a mechanism. Finally, I estimate the two-equation model by Two Stage Least Squares to make sure that identification of the causal effect of *Fast-Track* does not rely on the use of a bivariate probit model (which assumes joint normality of the errors). The 2SLS are larger (more negative) suggesting that the crime-reducing effect of fast-track prosecution is not a statistical artefact.

#### 4.5.2 Using court size

The benchmark estimates for the second empirical strategy are reported in Columns 1 and 3 of Table 4.4. Using a large sample of about 3000 adult prisoners convicted to sentences between 2 and 7 years, and including an extensive set of control variables, there is clear evidence that offenders prosecuted through fast-track procedure are significantly more likely to reoffend after release. The marginal effects after probit and OLS are similar and suggest that on average, offenders under FTP have a 7% point higher probability of recidivism than those prosecuted through a classic procedure.

Again, these naïve estimates are likely to be biased upward due to selection of the most crime-prone offenders by prosecutors. In Columns 2 and 4, the instrumental variable *LogCourtSize* is included in the second equation. Bivariate probit and 2SLS yield similar estimates for the instrument : they suggest that, on average, a doubling of court size is associated with a significant 4.5 to 5% point increase in the probability of fast-track prosecution. The causal estimates for  $\beta_1$  become large and negative : on average, fast-track prosecution reduces the probability of recidivism by 34-44% points, depending on the estimation method. These results are broadly consistent with those obtained from the regression discontinuity design (Section 4.1). Moreover, the correlation coefficient  $\rho$  is again significantly positive, which further provides evidence of selection by prosecutors in the choice of procedure.

**Heterogeneity in updating and deterrence** As explained in Section 2, the most straightforward mechanism linking delay of incarceration to recidivism is an updating by prisoners of expected delay of punishment in case of recidivism. But differences are likely to exist between

TABLE 4.4: Court size IV : Recidivism After 5 Years

	(1) Probit	(2) Bi-Probit	(3) OLS	(4) 2SLS
<b>Eq 1 : Recidivism</b>				
Fast-Track	0.0687*** (0.0203)	-0.3402*** (0.0912)	0.0682*** (0.0196)	-0.4379** (0.1849)
<b>Eq 2 : Fast-Track</b>				
LogCourtSize		0.0654** (0.0205)		0.0744*** (0.0202)
Correlation : $\rho$		0.79**		
Controls	x	x	x	x
Obs.	2929	2614	2930	2614

Marginal Effects are reported. Standard errors clustered at court level (robust in OLS)

Significance : \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$

prisoners in how they process information to reevaluate (future) delay : specifically, experienced offenders probably have good knowledge of the justice system (due to own and peers' previous experiences of incarceration) so that experiencing faster *versus* slower incarceration in the current case is not a very informative signal about future delay. Conversely, offenders who had little to no prior criminal convictions may learn a lot from the same signal and update their perception of expected delay of punishment in case of recidivism. Therefore, we can hypothesize that unexperienced offenders will react more to the experience of fast incarceration (large reduction in recidivism) than more experienced criminals. To test it, I run the same bivariate probit model (with court size as an instrument) separately on two same-sized groups : a group of prisoners with zero or only one previous conviction, and a group with at least two previous convictions. The results in Table 4.6 strongly support the idea that low-experienced offenders react much more to fast-track prosecution (with a 44% points decrease in recidivism) while more experienced criminals do not respond at all to this signal (most probably because they do not view it as informative<sup>12</sup>).

12. An alternative explanation is that experienced criminals are so immersed in criminal life-styles that they are not deterrable by delays of punishment.

## 4.6 Conclusion

This paper exploits two features of Fast-Track Procedures in France to investigate how the experience of faster *versus* slower incarceration affects recidivism after release. The topic is highly policy relevant, since conviction and punishment often occur with long delay in practice. If offenders are present-oriented, as suggested by recent empirical evidence, delays of punishment may push more individuals into crime, and may also generate recidivism among prisoners. To investigate this issue, the main empirical challenge is omitted variable bias : policemen and prosecutors presumably exert more effort and diligence to arrest, prosecute and incarcerate the most crime-prone offenders. To correct for this selection bias, the paper relies on two different empirical strategies : the first one is a large discontinuity in fast-track prosecution at age 18, and the second is a significant difference in FTP use across French courts, depending on their caseload. The two strategies yield similar estimates : experiencing fast-track prosecution reduces probability of recidivism after 5 years by 30 to 40% points, i.e. a reduction of recidivism by half. While the two designs may have weaknesses, the fact that they provide similar estimates using different estimation methods suggest that key assumptions in each setting are not driving the results : overall, the paper provides compelling evidence of the large deterrent effect of fast incarceration on future crime.

This finding is consistent with recent studies on offenders' present-orientation and the deterrent effect of swift and certain punishment. While Mastrobuoni and Rivers (2016) show that, with a discounting factor of 0.74, increasing the length of already-long sentences may not substantially deter crime among Italian prisoners, the current paper further suggests that the celerity of punishment is a key deterring factor (instead of severity of punishment). This result echoes the impressive effectiveness of "swift and certain" punishment in probation programs (Hawken and Kleiman, 2009; Heaton et al., 2015) to reduce recidivism among myopic and short-sighted individuals (Lee and McCrary, 2005).

This paper is one of the very first empirical investigations of the effect of slow justice on recidivism. Several extensions could refine our understanding of the linkages between incarceration celerity and crime : first, delayed incarceration could allow researchers to measure the incapacitation effect of prison, a highly discussed topic in the field of economics of crime. Measuring the criminogenic effects of celerity before and after punishment would provide a more comprehensive evaluation of the costs of slow justice. Second, a theoretical framework incorporating prosecutors' choice (and constraints) as well as offenders' behavior both before

and after punishment may help in the design of an optimal prosecution policy. In practice, prosecution choices vary widely from one court to the next, and intuitively, it is not clear which offenders should be primarily targeted for fast prosecution : low-frequency, inexperienced offenders with little risk of recidivism but high deterrability, or career criminals with very high risk of recidivism but low deterrability. Estimating a structural model of crime and prosecution would help answer this question.

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Appendix

TABLE 4.5: Robustness tests

	(1)	(2)	(3)
Bandwith restriction around 18	± 3 years	± 4 years	± 5 years
<b>Bivariate probit</b>			
+ age at offense cubed	-0.3673*** (0.0921)	-0.3054*** (0.1112)	-0.1781 (0.1486)
+ sentence length	-0.3819*** (0.0840)	-0.3603*** (0.1387)	-0.0946 (0.2020)
<b>Two Stage Least Squares</b>			
Second-Stage : <i>Fast-Track</i>	-0.6034** (0.2334)	-0.5326** (0.2349)	-0.4855* (0.2476)
First-Stage : <i>Age &gt; 18</i>	0.5223*** (0.1002)	0.4666*** (0.0942)	0.4180*** (0.0898)
Controls	x	x	x

Average Marginal Effects are reported. Standard errors in parentheses.  
Significance : \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

TABLE 4.6: IV Estimates : Heterogeneity by Previous Criminal Experience

	(1)	(2)
	≤1 Previous convictions	≥ 2 Previous convictions
Fast-Track	-0.4375*** (0.0194)	0.0046 (0.1422)
$\rho$	0.94***	0.24
Controls	x	x
Obs.	1319	1295

Marginal Effects are reported. Standard errors clustered at court level in parentheses.  
Significance : \*\*\* p<0.01, \*\* p<0.05, \* p<0.1



# Conclusion générale

Cette thèse contribue sur plusieurs dimensions au champ de l'économie du crime et à l'analyse des liens entre prison, réinsertion et récidive. Tout d'abord, la thèse porte sur l'étude de la France, alors que la recherche dans ce domaine était historiquement très concentrée sur le cas atypique des Etats-Unis. Bien que des travaux provenant d'autres pays européens soient apparus ces dernières années, la recherche sur la récidive était clairement limitée en France et offrait peu de réponses sur l'efficacité des pratiques et des politiques pénales propres à ce pays.

Deuxièmement, cette thèse innove par rapport à la littérature existante en explorant des questions encore peu étudiées au niveau international. C'est en particulier le cas de l'étude des effets du bracelet électronique (Chapitre 3) et des délais infraction-incarcération (Chapitre 4) dans la prévention de la récidive. Les autres chapitres se distinguent quant à eux par les nouvelles approches utilisées pour répondre à des questions déjà bien documentées (le nouveau cadre théorique du Chapitre 2, le modèle de survie avec effets dynamiques pour le Chapitre 1).

Enfin, bien que cette thèse à forte dimension économétrique emploie des modélisations et des méthodes d'estimation assez classiques, elle exploite plusieurs stratégies d'identification originales : en particulier, la mise en place graduelle dans le temps et dans l'espace du bracelet électronique en France, et les contraintes et préférences des procureurs français dans le choix des procédures de comparution immédiate.

## 4.6.1 Implications pour les pratiques et les politiques pénales

### La remise en cause de certaines évidences

Pris dans leur ensemble, les principaux résultats de cette thèse dressent un tableau contrasté des liens entre prison, réinsertion et récidive. Loin des positions simplistes, la thèse montre combien certaines évidences (pour certains) se révèlent fragiles après examen. On

peut prendre pour exemple l'idée souvent entendue que les courtes peines de prison sont néfastes et poussent nécessairement à la récidive. Sur ce point, la thèse montre que les effets d'une courte incarcération sont relativement limités par rapport à sa principale alternative en France, le placement sous surveillance électronique. On estime ainsi qu'en moyenne, le fait d'exécuter l'intégralité de sa peine de prison chez soi, sous bracelet électronique, plutôt qu'en détention, réduit de 6 à 7 points le risque de recondamnation dans les cinq ans. Quoique significative, cette diminution de la récidive paraît finalement modeste par rapport au taux moyen de récidive chez les sortants de prison, et semble donc éloignée des discours alarmistes sur le rôle fortement criminogène des prisons françaises. Ces résultats illustrent aussi le fort effet d'optique des statistiques de la récidive, qui amènent trop d'observateurs à conclure à un large effet causal devant de simples différences de moyennes : l'écart dans les taux de recondamnation entre les sortants de prison (59% dans les cinq ans) et les autres condamnés en France (41%) cache en réalité un fort biais de sélection, très défavorable à la prison. De la même manière, la forte récidive observée chez les délinquants incarcérés dans le cadre d'une comparution immédiate ne résulte pas d'un effet causal néfaste de cette procédure rapide (bien au contraire), mais des spécificités intrinsèques des délinquants visés par ces procédures de la part des procureurs.

Une autre évidence qui est remise en question dans la thèse concerne l'aggravation des peines encourues en récidive. Un large consensus s'est établi, en France et ailleurs, pour considérer que les récidivistes doivent encourir et subir des peines de plus en plus lourdes (les premières condamnations n'ayant pas suffi à les dissuader). Il s'agit donc de prononcer des sanctions relativement modérées pour les primo-délinquants (si possible peine en milieu ouvert, ou courte incarcération), et d'augmenter ensuite la gravité de la sanction avec la longueur des casiers judiciaires. Cette pratique, soutenue par les résultats de nombreux modèles théoriques, semble avoir reçue une certaine validation empirique en Italie en 2006 : le Parlement italien avait alors octroyé une grâce collective de trois ans de prison à la grande majorité des détenus (20 000 prisonniers furent libérés quasi-instantanément). En cas de récidive cependant, la grâce collective prévoyait que toute recondamnation entraînerait une aggravation automatique de la peine prononcée égale à la remise de peine obtenue en 2006. Comme attendu, Drago et al. (2009) montrent que les détenus italiens furent bien partiellement dissuadés par cet allongement des peines encourues en cas de récidive : ils estiment ainsi qu'une aggravation de 10% de la peine encourue réduit en moyenne de 7.4% la probabilité de récidive dans les sept mois suivant la sortie. Cependant, cet effet dissuasif d'une aggravation

des peines en récidive semble devoir être relativisé. Dans l'étude de l'efficacité du bracelet électronique en France, cette thèse trouve en effet que la surveillance électronique est particulièrement efficace chez ceux qui avaient déjà été condamnés à une peine de prison ferme auparavant ; à l'inverse, chez les délinquants qui voient leur première peine ferme directement aménagée en PSE, le bracelet électronique paraît sensiblement moins efficace. A l'opposé de la logique d'escalade des sanctions en récidive, ce résultat suggère qu'une peine modérée en milieu ouvert (comme le bracelet électronique) peut se révéler plus bénéfique après une première incarcération qu'en lieu et place de celle-ci. La séquence optimale de sanction d'une trajectoire délinquante ne serait donc pas croissante (aggravation successive des peines) mais possiblement dégressive (incarcération précoce suivie de mesures alternatives). Ce nouveau schéma de sanction dégressif permettrait aussi des gains d'efficacité conséquents au regard de la dynamique des risques délinquants au cours de la vie<sup>13</sup> : l'incarcération des jeunes délinquants, au moment où leur propension à commettre des délits est la plus forte, permettrait d'éviter de nombreuses infractions (comme le montrent les nombreux travaux sur l'effet neutralisant de l'emprisonnement), pourrait également permettre de travailler sur les causes du passage à l'acte (par l'éducation, le travail, les thérapies cognitivo-comportementales, etc.), et pourrait ensuite aboutir à des aménagements de peine axés sur la réinsertion, perçus comme une opportunité à saisir plutôt qu'une aubaine. Cette logique s'oppose à la pratique actuelle, qui consiste bien souvent à attendre qu'un jeune délinquant commette des infractions graves pour prononcer une peine de prison ferme relativement longue, laquelle intervient très tard dans la trajectoire délinquante, justement au moment de la vie où les risques de récidive diminuent chez la plupart des délinquants.

#### Le rôle déterminant d'aspects concrets souvent oubliés

La thèse montre à plusieurs reprises combien les effets d'une politique ou d'une pratique pénale peuvent être hétérogènes d'un individu à l'autre (en fonction de son âge, de ses antécédents, de sa situation économique et sociale, etc.) et dépendent des conditions concrètes d'application de la mesure. Dans le cas du bracelet électronique par exemple, la thèse souligne le rôle essentiel de l'effectivité et de la visibilité du contrôle (avec des visites à domicile pendant la période de surveillance électronique) ainsi que des obligations à respecter. Ainsi, lorsque le contrôle est peu visible et peu contraignant aux yeux des condamnés, le bracelet

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13. La *age-crime curve* montre un risque fort de délinquance durant l'adolescence qui croît jusqu'à l'âge de 20 ans environ, puis qui décroît pour devenir faible au-delà de 30 ans.

électronique semble ne pas se distinguer significativement de la prison dans son efficacité à prévenir la récidive. Ce résultat met donc au centre du débat la question des conditions pratiques de contrôle des condamnés et de l'effectivité du suivi, et relègue au second rang les débats binaires sur le type de peine (avec l'opposition entre prison et peine de probation). De façon similaire, l'étude de la grâce collective de Juillet 1996 montre que des réductions exogènes de peine peuvent avoir des effets très différents d'un détenu à l'autre, selon leur capacité à anticiper ces réductions de peine et à s'adapter à cette nouvelle d'une libération anticipée (rôle déterminant du temps restant avant la sortie, du capital économique et social à disposition) : les libérations soudaines et inattendues semblent ainsi particulièrement criminogènes auprès des détenus les moins bien insérés, tandis que les libérations anticipées et permettant une sortie bien préparée semblent au contraire vertueuses. Ces résultats suggèrent donc que les peines de prison peuvent être raccourcies sans effet néfaste sur la récidive, dès lors que ces réductions sont bien anticipées et préparées par tous. Pour améliorer leur efficacité, ces réductions de peine pourraient également être systématiquement accompagnée d'un véritable suivi en milieu ouvert, avec des règles strictes à respecter et une réelle menace de réincarcération rapide en cas de non-respect, comme dans le programme *HOPE*. De plus, on pourrait imaginer, à l'image de la grâce collective italienne de 2006, que les réductions de peine obtenues lors d'une incarcération soient automatiquement ajoutées à la peine encourue en cas de récidive pour garantir un fort effet de dissuasion. De façon plus générale, les résultats de la thèse sur ce sujet montrent l'importance de penser les politiques et les pratiques pénales dans leur contexte précis, en considérant les aspects concrets des mesures, leurs conséquences pratiques, et la situation personnelle de chaque condamné.

Un autre élément clé concerne la mise à exécution des peines. L'essentiel des débats, en audience comme dans l'hémicycle, porte souvent sur le type et la sévérité des peines qui seront prononcées. Pourtant, plutôt que la peine prononcée, c'est la peine réellement subie par le condamné qui importe, pour lui comme pour la victime éventuelle. Cette thèse montre que la célérité de l'incarcération est cruciale pour prévenir la récidive : d'après les estimations du Chapitre 4, le risque de récidive peut être divisé par deux lorsqu'une procédure rapide de comparution immédiate est utilisée par le procureur. Ce résultat témoigne des gains d'efficacité qui peuvent être atteints quand l'incarcération survient peu de temps après les faits. De ce point de vue, les contraintes légales du système pénal français semblent particulièrement coûteuses : hors comparution immédiate, un délinquant condamné à une courte peine ferme ne peut pas faire l'objet d'un mandat de dépôt à l'audience et être incarcéré à l'issue de sa

condamnation. Sa situation doit d'abord être étudiée par un Juge d'Application des Peines dans les semaines suivantes, qui peut décider d'aménager la peine avant toute incarcération, ou bien faire procéder à la mise à exécution plus ou moins rapide de la peine (incarcération). Ce système d'aménagement *ab-initio* et de mise à exécution retardée des peines, particulièrement complexe et déroutant, introduit une grande incertitude sur le devenir de nombreuses peines prononcées par les tribunaux (seront-elles exécutées ? sous quelle forme ? à quel horizon ?), et semblent engendrer d'importants coûts en matière de prévention de la récidive. A l'instar de cette thèse, le programme de probation *HOPE* a déjà montré la grande efficacité de sanctions relativement modestes mais certaines et rapides.

#### Des prisons, mais quelles prisons ?

Enfin, cette thèse interroge inévitablement sur le type de prisons qui permettrait la meilleure prévention de la récidive. De ce point de vue, la thèse n'apporte pas de réponse empirique nouvelle, mais les travaux existant au niveau international pointent très souvent dans la même direction en montrant que les conditions de détention impactent fortement les risques de récidive des détenus. Que les chercheurs étudient les prisons en Norvège, en Italie, ou aux Etats-Unis, leurs résultats montrent que des prisons plus ouvertes, moins sécurisées, plus proches des centre-villes (et donc de soutiens familiaux et associatifs), plus axées sur la réinsertion et offrant plus de services de réhabilitation aux détenus (comme le travail et la formation professionnelle), ont des effets causaux très bénéfiques sur la prévention de la récidive. Dans le cas de la prison ouverte de *Bollate* en Italie, celle-ci se révèle même moins coûteuse que les prisons traditionnelles du fait du nombre plus réduit de surveillants.

De plus, les travaux sur les effets de pairs en détention, menés aux Etats-Unis, en France ou au Danemark, démontrent l'effet criminogène d'affecter dans les mêmes établissements, voire les mêmes cellules, des délinquants ayant des profils similaires (même historique d'infractions, même âge) : ceux-ci s'entraînent mutuellement dans la récidive à leur sortie. A priori, une bonne façon d'éviter ces effets de pairs criminogènes en détention consisterait à respecter au maximum le principe de l'encellulement individuel, inscrit dans le droit français depuis 1875 mais jamais réellement appliqué dans la pratique. Une autre amélioration consisterait certainement à imaginer une nouvelle architecture pour les prisons françaises, avec des établissements à taille plus humaine (loin des 4000 détenus de Fleury-Mérogis), plus proches des centre-villes, et plus axés sur la réinsertion des détenus. En termes d'organisation interne, ces établissements pourraient s'inspirer du programme "Respeto" (déjà appliqué dans



quelques prisons françaises) de responsabilisation des détenus, avec des cellules ouvertes la journée et une plus grande liberté de déplacement dans l'enceinte de la prison, et une perte de ces privilèges en cas de mauvaise conduite.

Ces recommandations sont clairement visibles dans le projet récemment défendu par le Garde des Sceaux, Jean-Jacques Urvoas. Ce plan prévoit la construction de 10000 à 16000 places de prison supplémentaires à l'horizon 2025, qui pourrait permettre d'atteindre 80% d'encellulement individuel (contre moins de 40% aujourd'hui). Surtout, ce projet immobilier entend "humaniser les établissements et renouer avec la dimension symbolique de la prison républicaine", en optant pour une nouvelle architecture et des prisons moins isolées des villes. Mais pour remplir son objectif de prévention de la récidive, ce plan forcément très coûteux devra aussi s'accompagner de moyens supplémentaires pour le suivi en milieu ouvert et les Services Pénitentiaires d'Insertion et de Probation (SPIP), pour faire des alternatives à l'incarcération et des aménagements de peine des mesures vraiment crédibles et efficaces.

#### 4.6.2 Implications pour la recherche future

La thèse ouvre la voie à de nombreuses pistes de recherches futures. Concernant les effets de l'incarcération, les résultats de cette thèse montrent l'importance d'étudier les aspects concrets des peines (exécution, effectivité du suivi, préparation de la sortie) et la grande hétérogénéité des effets. Il convient donc de continuer à ouvrir la boîte noire de l'incarcération, pour en tirer des enseignements de plus en plus précis et robustes sur ce qui marche, et pourquoi.

Deuxièmement, il convient d'accorder une plus grande place dans les travaux en économie du crime au milieu ouvert, c'est-à-dire à l'ensemble des peines et mesures pénales qui se déroulent en dehors des prisons. L'emprisonnement concerne en effet une part limitée des condamnations en France (sur 500 000 condamnations par an, on compte environ 120 000 peines de prison ferme) comme dans les autres pays. Pourtant, il existe très peu d'évaluations des effets de ces autres types de peine (amende, sursis simple, sursis avec mise à l'épreuve, TIG, etc.) sur la récidive. La thèse montre pourtant que l'efficacité de mesures en milieu ouvert, comme le bracelet électronique, peut fortement dépendre des conditions pratiques dans lesquelles elles s'effectuent, en termes de suivi, de contrôles, d'obligations à respecter. L'étude des politiques pénales gagnerait donc beaucoup à ce que des travaux de recherche soient menés sur le milieu ouvert en France. Il n'existe cependant pas à ce jour de base de données individuelles nationale qui suivrait dans le temps un même délinquant condamné à

une ou plusieurs peines, de sa période de suivi-mise à l'épreuve jusqu'à ses éventuelles récidives et incarcérations (le logiciel *APPI* utilisé quotidiennement par les Juges d'Application des Peines et les SPIP renseigne cependant ce type d'informations).

Troisièmement, il conviendrait d'étudier d'autres dimensions du parcours des condamnés, au-delà de la seule récidive. On peut notamment penser à des indicateurs de réinsertion professionnelle et sociale, qui sont renseignés dans certaines bases de données d'autres pays, mais pour l'instant pas en France. Plusieurs travaux étrangers montrent pourtant que certains programmes peuvent être bénéfiques en matière de retour à l'emploi sans pour autant prévenir la récidive, par exemple. L'étude de ces indicateurs de réinsertion pourrait donc permettre d'affiner l'évaluation des politiques pénales.

Enfin, cette thèse se concentre exclusivement sur les effets de l'incarcération ou de peines alternatives sur le devenir des délinquants ayant subi ces peines. Or, l'emprisonnement a aussi un effet direct de neutralisation (temporaire) des délinquants ; et les peines encourues et prononcées agissent en parallèle sur tous les autres individus, par les signaux qu'elles envoient sur la sévérité des sanctions. Pour mener à bien une évaluation complète des politiques pénales, il convient donc d'estimer les effets de celles-ci sur la population dans son ensemble, par exemple en étudiant les taux agrégés de délinquance, et non les seules probabilités individuelles de récidive. Ce type de travaux pourrait s'appuyer sur les données fines du Ministère de l'Intérieur sur les infractions constatées en France par les services de police et de gendarmerie.





### **Abstract**

This PhD dissertation investigates the linkages between prison, reentry and recidivism. Each of the four chapters contributes to the growing field of the economics of crime, applying a series of econometric methods on French data to answer highly policy-relevant questions.

Chapter 1 explores the dynamics of the risk of recidivism after prison release. This article is the first to document the shape of the hazard of recidivism over time in France (a fastly-diminishing function) and to explore the key individual characteristics explaining the level and dynamic of this risk over time.

Chapter 2 investigates the causal effect of sentence reductions on recidivism. This article is the first to explicitly account for the key mediating role of anticipations and adaptation by prisoners. By exploiting the French collective pardon of July 1996 as a natural experiment, the article finds strong evidence supporting the importance of the concrete design of sentence reductions.

Chapter 3 offers the first evaluation of the causal effects of a major penal policy in France, the introduction of Electronic Monitoring (EM) as an alternative to short prison sentences. Using the staged rollout of EM across courts and over time, the paper finds evidence of significant beneficial effects of EM on recidivism, compared to incarceration.

Finally, Chapter 4 measures the deterrent effect of fast versus slower incarceration on future crime. This article is the first to estimate the large beneficial impact of using fast-track procedures, instead of longer procedures, on recidivism after release.

**Keywords** : economics of crime, recidivism, prison, public policy evaluation

### **Résumé**

Cette thèse de doctorat étudie les liens entre prison, réinsertion et récidive. Chacun des quatre chapitres contribue au champ croissant de l'économie du crime, en appliquant diverses méthodes économétriques sur des données françaises pour répondre à des questions essentielles pour les politiques publiques.

Le Chapitre 1 explore la dynamique du risque de récidive après la sortie de prison. Cet article est le premier à documenter la forme du hasard de récidive dans le temps en France (une fonction rapidement décroissante) et à explorer les déterminants individuels majeurs qui expliquent le niveau et la dynamique des risques dans le temps.

Le Chapitre 2 étudie l'effet causal des réductions de peine sur la récidive. Cet article est le premier à tenir explicitement compte du rôle essentiel des anticipations et de l'adaptation des détenus. En exploitant la grâce collective française de Juillet 1996 comme une expérience naturelle, les résultats soutiennent l'importance du design concret des réductions de peine.

Le Chapitre 3 propose la première évaluation des effets causaux d'une politique pénale majeure en France, l'introduction du Placement sous Surveillance Electronique (PSE) comme alternative aux courtes peines de prison fermes. En utilisant la mise en place progressive du PSE dans les tribunaux français, l'article trouve des effets significatifs bénéfiques du PSE sur la récidive, par rapport à l'incarcération.

Enfin, le Chapitre 4 mesure l'effet dissuasif d'une incarcération plus ou moins rapide sur la délinquance future. Cet article est le premier à estimer les effets très bénéfiques de recourir à des procédures rapides pour prévenir la récidive après la libération.

**Mots-clés** : économie du crime, récidive, prison, évaluation des politiques publiques