Essay on four issues in public policy evaluation

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Résumé

Cette thèse présente quatre tentatives indépendantes d'évaluations de politiques publiques, mettant en application les méthodes microéconométriques récentes.

Précédé d'un premier chapitre présentant un grand résumé en français, le deuxième chapitre s'intéresse à l'impact de la réforme dite du bonus/malus écologique sur les émissions de CO_2. Évaluer l'impact de cette mesure sur les émissions finales de CO_2 demande de modéliser non seulement les choix d'achat de véhicule, mais également l'usage de ces véhicules. La rapidité de mise en œuvre de la mesure offre une source d'identification crédible de la sensibilité des choix des consommateurs aux incitations financières. Les estimations suggèrent cependant un bilan très décevant de la mesure : du fait de forts effets volumes, le bonus/malus s'est traduit par une *augmentation* substancielle des émissions de CO_2.

Le troisième chapitre évalue la capacité de dispositifs fiscaux à redynamiser l'emploi et l'activité économique locale des zones ciblées, à travers le dispositif des Zones Franches Urbaines. L'accès à des données locales précises permet d'évaluer l'impact des exonérations fiscales accordées aux entreprises s'installant dans les ZFU de deuxième génération. Celles-ci auraient un impact positif mais faible sur la création d'entreprise et l'emploi en comparaison avec les autres zones défavorisées similaires.

Le quatrième chapitre s'intéresse aux conséquences des contrats temporaires sur les trajectoires professionnelles. En contrôlant des biais d'hétérogénéité individuelle par un modèle à effets fixes, on montre que les CDD augmentent significativement les transitions vers l'emploi stable par rapport au chômage. En revanche, le travail intérimaire n'améliore que marginalement ces transitions.

Le cinquième chapitre traite des effets des augmentations du salaire minimum créées par loi Fillon sur les inégalités salariales. Alors que la mise en place progressive des accords de réduction de temps de travail avaient créés plusieurs niveaux de rémunération minimale, leur convergence sur trois ans fournit une source d'identification. Les estimations utilisent une méthode de régression de quantiles inconditionnelles. Les augmentations de salaire minimum auraient un effet (faible) jusqu'au septième décile des distributions de salaire des hommes, mais négligeable pour ceux des femmes.

Abstract

This dissertation proposes four independent evaluations of French public policies, using recent micro-econometrics methods.

Following a first chapter that presents a French summary of the dissertation, the second chapter studies the impact of the French “ecological bonus/malus” (feebate) policy on total CO_2 emissions. The evaluation of the impact of this policy on final emissions requires to model not only the choice for new vehicles (and their sensitiveness to prices), but also the mileage done by vehicles. As the policy was implemented in a very span of time, it provides a credible source of identification for the sensitivity of consumers to financial incentives. The estimates suggest that the policy has a counterintuitive impact, as it increases total CO_2 emissions.

The third chapter examines the capacity of fiscal policies to foster employment and economic activities in targeted areas, through the French ZFU (enterprises zones). With precise local data we could evaluate the impact of tax exemptions provided to firms implemented in the second wave of enterprise zones. Enterprise zones have a significant but small impact on business creation and employment by comparison with other similar disadvantaged areas.

The fourth chapter considers consequences of temporary contracts on professional trajectories. A dynamic fixed-effect model is used to deal with unobserved heterogeneity. According to these estimates, fixed-term contracts significantly increase the transition intensity to permanent contract relatively to unemployment. By contrast, temporary agency work does not significantly improve transition to regular jobs.

The fifth chapter deals with the impact of minimum wages increases on earnings inequalities. The “Fillon law” decided an harmonization of these levels in 2002, that took place over a three-year period. This exogenous increase is used to measure the potential spread-up impact of the minimum wage over the whole earnings distributions. Estimates are based on an unconditional quantile regression method. They suggest small impact up-to the seventh decile of the distribution of earnings for male workers, but none significant impact for earnings of female workers.

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Chapitre 1

Résumé en français

La demande d'évaluation est de plus en plus présente dans le débat public français. Dans un contexte de rationalisation des dépenses, la puissance publique est redevable de la preuve de l'efficacité des actions qu'elle met en œuvre. La réforme constitutionnelle de 2008 institue ainsi l'obligation d'assortir chaque projet de loi d'une "étude d'impact", qui permet *ex ante* d'évaluer ses effets socio-économiques dans un sens très large. Le terme évaluation regroupe cependant des acceptations très variées. Même en se restreignant à celles relevant de la sphère économique, il peut s'agir simplement de fournir des indicateurs de suivi (par exemple sur le nombre de bénéficiaires d'un type de contrat aidé) ou du coût budgétaire direct de la mesure. Si ces indicateurs apportent des éléments de cadrages indispensables, ils ne permettent pas d'évaluer l'efficacité de celle-ci à répondre aux objectifs qui lui sont assignés.

La réponse de la discipline économique à cette question de l'efficacité s'est structurée à partir de deux approches. La première consiste à expliciter un modèle complet du comportement des agents économiques. Elle est particulièrement utile pour anticiper *ex ante* le résultat d'une politique inédite ou celui d'une modification des règles de politiques déjà existantes (paramètres d'un système de retraite par exemple). La critique parfois adressée à ce type de modélisation est que, souvent afin d'aboutir à des solutions interprétables, elle repose sur des hypothèses fortes sur les comportements des agents. Une deuxième approche s'est donc imposée sur les deux dernières décennies. Négligeant la modélisation explicite, elle se focalise sur la mesure empirique de l'effet direct de la mesure. L'accent porte alors essentiellement sur les conditions d'identification de l'effet propre ("causal") de la mesure, en particulier en présence d'effets de sélection. Les expérimentations contrôlées constituent l'archétype de ce type d'évaluation, mais n'en représentent qu'une partie. Les évaluations ex post non expérimentales sont en effet devenues majoritaires. Si cette deuxième approche a pris le pas sur la première, nous reviendrons cependant dans la suite de cette introduction sur les apports de l'économie structurelle à ces approches strictement empiriques.

Cette thèse se concentre sur de telles évaluations empiriques, à travers l'étude indépendante de plusieurs politiques portant sur des champs différents. La première partie propose une mesure du bilan en termes d'émissions de CO_2 de la mesure récente de bonus/malus sur les voitures de véhicules neufs (éco-pastilles). La seconde évalue la capacité à créer de l'activité économique de l'un des dispositifs de la politique de la ville en faveur des quartiers en difficultés, les Zones Franches Urbaines. Les troisièmes et quatrième s'intéressent à deux instruments importants de la régulation du marché du travail, respectivement les contrats courts et le salaire minimum. Après avoir rappelé les principes qui définissent l'évaluation de politique économique, l'introduction passera en revue la littérature sur ces questions et détaillera les résultats nouveaux obtenus ici, en reprenant le plan général de la thèse.

1.1 Introduction générale : le problème de la sélection

1.1.1 La formalisation du problème d'évaluation : le cadre de Rubin

Le cadre dit "de Rubin" (du nom du statisticien qui l'a popularisé au cours des années 70) s'est progressivement imposé pour l'analyse empirique des politiques publiques. Même si ce cadre présente des limites sur lesquelles nous reviendrons dans la suite, il offre une grille de lecture utile pour définir les problèmes sur lesquels s'est concentrée l'analyse micro-économétrique pour la mesure d'un effet causal : l'hétérogénéité dans les réactions individuelles à une politique, et les effets de sélection qui en découlent.

En pratique, considérons donc l'évaluation d'une mesure T. Dans la version la plus simple du modèle, on distingue les personnes qui bénéficient de cette mesure ($T = 1$) de celles qui n'en bénéficient pas ($T = 0$) [1]. On s'intéresse alors à l'effet de cette mesure sur une grandeur d'intérêt, qu'on désigne par commodité comme le *revenu.*

On considère alors que chacun a "virtuellement' deux revenus potentiels, selon qu'il bénéficie ou non de la mesure. On note donc Y_0 le revenu associé à $T = 0$ (pas de traitement) et Y_1 le revenu associé à $T = 1$ (traitement). Pour une personne, on peut alors définir l'effet propre (ou causal) de la mesure Δ_i, à partir de ces deux revenus potentiels : $\Delta_i = Y_{i1} - Y_{i0}$.

On suppose par ailleurs que statut de l'individu i n'a d'effet que sur son revenu, mais pas sur ceux des autres personnes. Cette hypothèse, sur laquelle nous reviendrons dans la suite, est appelée *SUTVA*, pour *Stable Unit Treatment Value Assumption.*

Deux estimateurs sont classiques dans la littérature économétrique de l'évaluation. Le premier est l'effet du traitement sur les traités (*Average Treatment effect on the Treated* ou *ATT*), c'est-à-dire l'effet moyen de la mesure sur ceux qui en ont effectivement bénéficié. Formellement, il s'écrit :

$$\Delta^{ATT} = E(Y_{i1} - Y_{i0}|T_i = 1)$$

Le second est l'effet moyen du traitement sur l'ensemble de la population (*Averaged Treatment Effect*). Il s'agit là d'estimer l'effet de cette mesure si elle était étendue à l'ensemble de la population, soit formellement :

$$\Delta^{ATE} = E(Y_{i1} - Y_{i0})$$

Le "problème fondamental de l'inférence causale" est que l'on n'observe pas simultanément Y_{i0} et Y_{i1}. On peut observer pour un bénéficiaire de la mesure ($T_i = 1$) son revenu potentiel Y_{i1} mais non le contrefactuel Y_{i0}, tandis que pour un non bénéficiaire il pourra être possible de mesurer Y_{i0} et non le contrefactuel Y_{i1}. L'enjeu des méthodes microéconométriques utilisées pour l'évaluation est justement d'estimer cette situation *contrefactuelle*, i.e. ce qui aurait été observé si la mesure n'avait pas été mise en place.

1. Le vocabulaire de cette approche emprunte beaucoup au champ de l'expérimentation médicale dont il est issu. Ainsi, on parle souvent de "traitement" pour désigner la variable dont on cherche à mesurer l'effet.

1.1.2 Sélection et identification

La difficulté de l'exercice est que les bénéficiaires ne le sont pas par hasard : le fait qu'ils ont été choisis pour en bénéficier est probablement lié aux revenus qu'ils pouvaient espérer, avec ou sans traitement [2]. Si ces effets de sélection existent, on aura donc :

$$(Y_1, Y_0) \not\perp T \tag{1.1.1}$$

et il n'est pas possible d'utiliser simplement la situation des non bénéficiaires pour estimer le contrefactuel de ce qui aurait celle des bénéficiaires, en l'absence de la mesure. Différentes méthodes empiriques ont été proposées pour apporter une réponse aux problèmes de sélection. Le paradigme de ces méthodes est celui des expériences aléatoires contrôlées (on en trouvera une présentation détaillée dans Duflo et al., 2006). Dans de telles expérimentations, groupe de traitement et groupe de contrôle sont déterminés aléatoirement. Si la répartition aléatoire est bien vérifiée, le choix d'être traité ou non est indépendant du résultat potentiel escompté, et l'estimateur :

$$\begin{aligned}\Delta &= E(Y|T=1) - E(Y|T=0) \\ &= E(Y_1) - E(Y_0)\end{aligned}$$

est un estimateur sans biais de l'effet du traitement. Si leur utilisation commence à se développer en France, elles restent encore rares dans le champ social. De fait, elles nécessitent une intervention très en amont de la mise en place de la politique qu'il s'agit d'évaluer. Souvent, il s'agit plutôt d'estimer *ex post* des politiques déjà en cours. Plusieurs méthodes ont été développées.

Un premier ensemble tente d'utiliser des variations exogènes de l'environnement économique, des réformes de législation par exemple qui créent une situation presque expérimentale. On parle d'"expériences naturelles", ou de "quasi-expériences". Selon les hypothèses d'identification nécessaires, trois méthodes se distinguent : les différences de différences, les variables instrumentales, les régressions sur discontinuités.

2. Une formalisation classique de ce problème de sélection est fournie par le modèle "de Roy", voir par exemple Heckman & Honore (1990).

L'estimateur de *différence de différences* consiste à mesurer la différence de revenu entre population traitée et non traitée avant et après la mesure et à effectuer la différence de ces deux évolutions. Sous l'hypothèse que la variable d'intérêt potentielle sans traitement des bénéficiaires aurait évolué de la même manière que dans le groupe de contrôle, comparer les évolutions des deux groupes permet théoriquement d'évaluer l'impact propre de la mesure. Formellement :

$$\Delta_t Y_0 \perp\!\!\!\perp T$$

En pratique, l'estimation peut se faire simplement par une régression linéaire simple, en ajoutant à la fois des indicatrices temporelles, des indicatrices correspondant aux différents groupes, et des indicatrices croisées (correspondant au fait d'avoir bénéficié du traitement).

L'hypothèse nécessaire pour estimer par *variables instrumentales* est différente. La définition de telles variables, classiques en économétrie, est bien connue : il s'agit de variables qui expliquent le fait d'être traité, mais qui ne sont pas corrélées aux composantes inobservées du revenu. Comme montré par Angrist et al. (1996), l'estimateur ne mesure cependant l'effet que pour une population particulière, les personnes qui adaptent leur sélection dans le programme ou pas selon la valeur de l'instrument (les "compliers"). Pour reprendre leur formalisme, dans le cadre d'un instrument binaire, noté Z et d'un traitement binaire T, on peut supposer que chaque personne a un traitement potentiel selon la valeur de l'instrument T_1 si $Z = 1$ et T_0 sinon. Bien sûr, un seul de ces traitements potentiels est observé. Au prix d'une hypothèse d'indépendance un peu plus forte qu'une simple absence de corrélation, et d'une condition d'exclusion représentée sous forme d'une hypothèse de monotonicité :

$$\begin{cases} (T_{i1}, T_{i0}, Y_{i1}, Y_{i0}) \perp\!\!\!\perp Z_i \text{ (Indépendance)} \\ T_{i1} \geq T_{i0} \text{ (Monotonicité)} \end{cases}$$

Angrist et al. (1996) montrent qu'un estimateur à variable instrumentale mesure l'effet causal moyen pour une population particulière, que la littérature désigne sous le nom de LATE (Local Average Treatment Effect) :

$$\hat{\beta}_{IV} = E(Y_{1i} - Y_{0i} | T_{1i} - T_{0i} = 1)$$

Enfin, les *régressions sur discontinuités* utilisent le fait que de nombreux dispositifs comportent des seuils. Par exemple, la carte scolaire contraint les parents sur le choix de l'école que leurs enfants peuvent fréquenter Black, 1999 ; certaines allocations sont réservées aux personnes dont les revenus se situent en dessous d'un certain plafond... L'idée est qu'autour de la discontinuité, des personnes très proches peuvent avoir des chances très différentes de bénéficier d'une mesure ou d'une aide. L'hypothèse ici consiste à dire que les personnes autour de ces seuils ont des caractéristiques inobservables identiques. On pourra alors par exemple comparer les parents habitant de chaque côté de la rue servant de frontière à la carte scolaire, ou les individus juste en dessous ou au-dessus du plafond de ressources. Dans le cas le plus simple, l'affectation au traitement est totalement déterministe en fonction d'une variable de sélection, qu'on peut noter S : au dessus d'un seuil de cette variable $\underline{S}$, toutes les personnes bénéficient du traitement, et inversement pour celles en dessous. On peut alors identifier l'effet du traitement pour les personnes dont la valeur de la variable de sélection se situe au niveau de ce seuil sous la simple hypothèse que les revenus potentiels sont continus en ce point :

$$\begin{cases} \lim_{S \searrow \underline{S}^+} E(Y_0|S) = \lim_{S \nearrow \underline{S}^-} E(Y_0|S) \\ \lim_{S \searrow \underline{S}^+} E(Y_1|S) = \lim_{S \nearrow \underline{S}^-} E(Y_1|S) \end{cases}$$

L'estimateur dans ce cas simple correspond alors à la différence de revenu autour du seuil de discontinuités. Cependant, ici aussi l'estimateur obtenu est purement local : il renseigne sur la valeur du traitement au niveau de ce seuil. Cette restriction est encore plus sévère lorsque le seuil n'est pas totalement déterministe, mais n'a d'influence que sur la probabilité d'être traité.

Quoiqu'en soit leurs limites, il n'est possible d'appliquer l'une ou l'autre des méthodes présentées ci-dessus que lorsque le dispositif que l'on souhaite évaluer présente des caractéristiques permettant de rendre crédibles l'une ou l'autre de ces hypothèses. Dans les (nombreux) cas où cette condition n'est pas vérifiée, le minimum est de contrôler des différences observables entre les bénéficiaires et les non-bénéficiaires. Les estimateurs par appariement (ou *matching*) reposent ainsi sur l'hypothèse que :

$$Y_{0i}, Y_{1i} \perp\!\!\!\perp T_i | X_i \tag{1.1.2}$$

Cette hypothèse est évidemment très forte (pour une analyse détaillée des performances de l'estimation par matching voir par exemple Heckman et al., 1997). Pour la rendre plus crédible, on peut lorsque l'on dispose de données avant et après la mise en place de la mesure estimer les effets en différence temporelle, pour éliminer d'éventuelles composantes inobservables fixes. Cette variante s'apparente à une procédure de différence de différences avec appariement, encore appelé différence de différences conditionnelles (Conditional Differences in Differences). L'hypothèse identifiante dans ce cas s'écrit :

$$\Delta_t Y_0 \perp\!\!\!\perp T|X$$

1.1.3 Quel paramètre d'intérêt ?

La mise en œuvre pratique d'une évaluation n'est pas toujours aussi simple. Elle appelle plusieurs remarques.

La première est plutôt d'ordre technique et porte sur la nature des variables. En premier lieu, le cadre de Rubin est explicité pour un traitement (une mesure) binaire. De nombreuses politiques ne sont pas aussi simples : on peut souhaiter évaluer l'efficacité relative de plusieurs dispositifs concurrents par exemple, ou d'indemnités plus ou moins généreuses. Les méthodes ci-dessus peuvent se généraliser sans trop de difficulté à ces situations de traitements multiples ou d'intensité de traitement variable. La question de la variable d'intérêt peut être plus délicate. En premier lieu, l'effet moyen de la mesure n'est pas toujours le paramètre le plus pertinent pour juger d'une politique. Une politique de lutte contre l'échec scolaire dont l'effet moyen est nul peut par exemple être jugée bénéfique si elle permet de réduire l'exclusion des élèves les plus en difficultés [3]. Alors que le débat public appelle de plus en plus à sortir de la "dictature de la moyenne", l'impact moyen du traitement n'informe pas, par exemple, sur l'évolution des inégalités. Pour cette raison de nouveaux instruments statistiques sont de plus en plus utilisés pour analyser l'impact d'une mesure sur l'ensemble de la distribution, à travers en particulier les régressions de quantile (voir par exemple Koenker & Hallock, 2001). Par ailleurs, la nature de certaines variables d'intérêt appelle également des trai-

3. Heckman & Smith (1997) : "many persons would judge programs to be successful if (...) enough of the right kinds of persons, reaped benefits from them even if the average participant did not".

tements spécifiques. Ainsi une mesure susceptible d'affecter les trajectoires sur le marché du travail se laissera mal évaluer dans le cadre statistique de l'économétrie linéaire classique, les variables d'intérêt étant ici plutôt les durées (au chômage par exemple) ou les transitions (d'un contrat temporaire à un emploi stable).

La seconde remarque renvoie justement à la définition de la variable d'intérêt. Elle n'est jamais évidente et demande de s'interroger sur les objectifs que l'on assigne à la politique qu'il s'agit d'évaluer. Dans certains cas, ceux-ci sont clairs. Une mesure comme le bonus/malus écologique par exemple vise à réduire les émissions de CO_2. Le premier attendu d'une évaluation est de vérifier que cette mesure a rempli cet objectif (même si on peut par ailleurs s'intéresser à des effets économiques sur le secteur automobile français par exemple). Dans d'autre, l'efficacité de la mesure se laisse plus difficilement circonscrire. Ainsi des réformes introduites au cours des années quatre-vingt visant à offrir plus de flexibilité au marché du travail, notion moins directement mesurable par des indicateurs directement quantifiables [4]. *A priori* introduite pour réduire le chômage endémique dont souffraient certaines économies de l'Europe continentale (Bentolila & Bertola, 1990 parlent d'"euroscleoris"), la question de leur impact sur l'offre de travail peut être différent. La flexibilité offerte aux entreprises devrait se traduire par des sorties plus fréquentes du chômage vers l'emploi, mais également des destructions d'emploi plus élevées. Le résultat sur le risque de chômage de politiques de flexibilisation "à la marge", introduisant des contrats temporaires sans modifier la protection de l'emploi, peut être ambigu (Boeri & Garibaldi, 2007). Au-delà de ces effets sur le chômage, la flexibilité peut se traduire par des stratégies d'accumulation de capital humain différentes (Wasmer, 2006), avec des conséquences sur les trajectoires d'emploi de long terme. Il est donc illusoire de produire "une" évaluation d'une telle politique par le biais d'un indicateur unique. Ce n'est que par la confrontation de résultats abordant chacun de ces aspects qu'un bilan pourrait être tiré.

Une dernière difficulté tient à la possibilité d'extrapoler les résultats obtenus dans le cadre de telles évaluations empiriques. Comme explicité plus haut, celles-ci font implicitement l'hypothèse que l'impact de la politique n'a qu'un effet direct sur les

4. Le coût des ajustements en est un, voir par exemple Goux et al. (2001) et Abowd & Kramarz (2003) pour une analyse.

personnes concernées (hypothèse dite *Stable Unit Treatment Value Assumption*). Dans de nombreux cas, cette hypothèse n'est pas plausible. Des effets d'externalités peuvent par exemple se produire. Ces effets ont été bien documentés dans le champ de l'épimédiologie médicale (les risques de contamination se réduisant avec la couverture vaccinale, l'éradication d'une maladie peut être assurée sans une vaccination totale de la population). Ces externalités peuvent également se produire pour des instruments de politique économique susceptibles d'agir sur des agents dont les intérêts sont partiellement antagonistes. C'est par exemple le cas de mesures de politique de la Ville visant à dynamiser certains quartiers, au risque de conduire à des effets d'éviction pour des zones proches. Une évaluation doit donc tenir compte de ces externalités (on en trouvera une illustration dans Miguel & Kremer, 2004). Cette hypothèse d'indépendance individuel au traitement est également remise en cause dès lors que des effets de bouclage sont susceptibles de se produire. Les effets sur les prix ou les salaires résultant de la modification de statut de bénéficiaire d'une politique peut en retour affecter les personnes qui n'en bénéficient pas directement.

Enfin, les limites de l'approche en forme réduite sont bien connues. Si elles permettent d'apporter des éléments sur une politique à un moment donnée, renoncer à modéliser les mécanismes par lequels se font ces ajustement revient à se couper d'un outil de compréhension et surtout d'interprétation de ces résultats. Utile pour établir un bilan *ex post* de ces politiques, ces évaluations ne permettent pas a priori d'évaluer l'impact potentiel d'une modification substantielle de ces outils de régulation du marché du travail. C'est sous cet angle que Deaton (2010), dans une charge violente, s'en prend à l'approche expérimentale ou quasi-expérimentale qui s'est de fait imposée comme un standard dans la littérature économique. Celle-ci conduit les praticiens, selon lui, à ne plus tenter comprendre “comment les choses fonctionnent”. Comme noté également par Heckman & Urzúa (2010), elle conduit à se concentrer sur des paramètres dont l'intérêt n'est pas évident (en particulier le “LATE”, résultat des estimations par variables instrumentales). Cependant, avec Imbens (2010), il semble nécessaire de distinguer ce qui relève de l'identification empirique d'un effet particulier, et de l'interprétation de ce résultat qu'on peut en faire. Il faut faire crédit à ces méthodes quasi-expérimentales d'avoir clarifié les premières. Cela ne doit pas conduire à ne s'intéresser qu'aux questions pour lesquelles cette approche (quasi-)expérimentale est adaptée, sans qu'on doive s'en priver lorsqu'elle l'est. Cela ne signifie pas non plus que doive être négligée la question de l'interprétation des résultats obtenus. Sur cette question, une approche structurelle est souvent requise (voir par exemple Rosenzweig & Wolpin, 2000). Des exemples récents offrent justement des tentatives stimulantes de calibrer des modèles de comportement économique à partir de l'évidence empirique offerte par des approches expérimentales (Card & Hyslop 2005, Todd & Wolpin 2006).

Cette thèse présente quatre tentatives indépendantes d'évaluations de politiques publiques, mettant en application les méthodes microéconométriques récentes. Le premier chapitre s'intéresse à l'impact de la réforme dite du bonus/malus écologique sur les émissions de CO_2. Le second évalue la capacité de dispositifs fiscaux à redynamiser l'emploi et l'activité économique locale des zones ciblées, à travers le dispositif des Zones Franches Urbaines. Le troisième s'intéresse aux conséquences des contrats temporaires sur les trajectoires professionnelles. Enfin, le quatrième chapitre traite des effets des augmentations du salaire minimum créées par loi Fillon sur les distributions de salaires.

1.2 Quelle efficacité de la taxation environnementale ? Une analyse du Bonus/Malus écologique[1]

1.2.1 Motivation

Suite au protocole de Kyoto, la plupart des pays industrialisés se sont donnés des objectifs de diminution des émissions de gaz à effet de serre. A ce titre, la réduction des émissions automobile est un enjeu crucial pour la puissance publique. Avec 138 millions de tonnes en 2007, le transport routier est en effet responsable du tiers des émissions de CO_2 de la France, et il reste en expansion. Le trafic routier a ainsi été multiplié par six depuis 1960, et a encore progressé de 27 % entre 1990 et 2007, tandis que les autres secteurs connaissaient une stabilisation

1. Ce chapitre est coécrit avec Xavier D'Haultfœuille (INSEE-CREST) et Xavier Boutin (Commission Européenne).

voire une baisse de leurs émissions. Mesure phare du grenelle de l'environnement, le "bonus/malus écologique", système original de taxation des véhicules neufs mis en place en janvier 2008, tente de répondre à cet enjeu. Depuis cette date, les acheteurs de véhicules les moins polluants bénéficient d'une réduction à l'achat, tandis que ceux choisissant des modèles plus polluants doivent s'acquitter d'une taxe. Le premier objectif de cette loi était d'orienter les choix des consommateurs vers les modèles les plus "propres". Cet objectif a été atteint rapidement puisque la part des véhicules émettant moins de 120 grammes de CO2 a doublé en quelques mois. L'impact du système semble même avoir dépassé les attentes du gouvernement. Alors que la mesure était calibrée pour être fiscalement neutre, le système a en réalité coûté 235 millions d'euros en 2008, dont 215 millions pour les seuls particuliers. Un tel coût économique appelle une évaluation des bénéfices environnementaux réels de cette politique.

Cette estimation n'est pas évidente. Tout d'abord, il est nécessaire d'estimer la déformation de la demande liée à l'introduction des (dés)incitations financières crées par le bonus/malus. Le marché automobile se caractérise par des biens très différenciés, dont la valuation diffère selon les consommateurs. Ensuite, sur ce marché concurrentiel, l'offre est susceptible de s'adapter aux modifications de prix (ou de préférences) induites par la mesure. En outre, les émissions finalement générées par la mesure dépendent non seulement du choix du véhicule par un conducteur, mais aussi (et surtout) de l'usage qu'il en fait, c'est-à-dire du nombre de kilomètres qu'il parcourera *in fine* avec ce véhicule. Ces deux choix sont liés. Un gros conducteur sera sans doute plus porté à acheter une "routière", plus lourde (et donc plus polluante) mais capable d'offrir un confort de conduite sur de long trajets, tandis qu'une petite citadine suffira pour un véhicule d'appoint. Cependant, ce lien peut aussi être inversé : une plus faible consommation au kilomètre peut inciter à une utilisation plus intensive du véhicule. Ce phénomène, bien documenté dans la littérature environnementale sous le nom d'"effet rebond", peut réduire l'efficacité de la mesure du bonus/malus. Enfin, la mesure ne s'appliquant qu'aux véhicules neufs (et donc qu'à une fraction minime du parc), les effets de court terme seront forcément modestes. Modéliser les effets de long terme repose cependant sur des hypothèses fortes, en particulier sur les modifications induites par la mesures sur les rythmes de renouvellement des véhicules. A notre connaissance, il n'existe pas aujourd'hui d'études abordant l'ensemble de ces points.

1.2.2 Revue de littérature

Modélisation de la demande pour des biens hétérogènes

La théorie du consommateur classique se limite à l'analyse d'un bien unique, l'équilibre se faisant sur le prix seul. Elle peine évidemment à rendre compte d'une réalité plus complexe : les stratégies de différentiations des entreprises conduisent à proposer des biens partiellement substituables mais hétérogènes dans de multiples dimensions, afin de servir des consommateurs dont la valuation pour ces caractéristiques sont variables. Deux modèles de voiture par exemple peuvent différer selon de nombreuses dimensions : puissance, marque et modèle, type de carburant ou nombres de portes sont autant de caractéristiques dont la valorisation peut être très variables selon les consommateurs. Sur les dernières décennies, la littérature économique en économie industrielle a vu l'essor de modèles empiriques plus réalistes tentant de rendre compte de cette réalité. L'une des contributions essentielles est celle de Berry et al. (1995), qui proposent un modèle étudiant le marché automobiles aux États-Unis. Ce type de modèle s'appuie sur une modélisation explicite des préférences des consommateurs.

Plus précisément, on se place dans le cas d'une demande pour un ensemble de produits J, et un ensemble de consommateurs I, et on postule une forme sur la préférence du consommateur i pour le produit j, $U_{ij}(\delta_j, \upsilon_i, \epsilon_{ij}; \theta)$, fonction des caractéristiques δ_j du produits j et υ_i celles du consommateurs i.

Il faut également tenir compte du fait que le consommateur peut ne choisir aucun des produits disponibles. Dans notre exemple, il peut s'agir de consommateur qui préfére acheter un véhicule d'occasion ou les transports en communs. Cela se traduit dans le modèle par l'existence d'un "bien extérieur", noté 0. Par convention, on prend comme condition de normalisation $\delta_0 = 0$. Le consommateur i choisit le bien k qui maximise son utilité :

$$U_{ik} = Max_{j \in 0..J} U_{ij}$$

A condition de faire des hypothèses sur les fonctions de préférence, on peut expliciter le bien choisi pour un consommateur i. On peut alors obtenir la part de marché de chacun des produits en agrégeant sur toute la population. En pratique, la taille du marché est définie par l'ensemble des consommateurs potentiels, et la part du bien extérieur correspond à la différence entre les ventes et le reste.

La manière la plus directe est de supposer que les fonctions de préférence ont une forme de logit simple. Cette hypothèse à l'avantage de conduire à des modèles particulièrement simples en termes d'estimation. L'inconvénient est qu'elle suppose implicitement que les élasticités de substitution entre produits ne dépendent pas de leur proximité éventuelle en termes de caractéristiques. La prise en compte de cette limite peut conduire à plutôt spécifier des logits emboités. Surtout, les modèles récents, à la suite de l'article séminal de Berry et al. (1995), mettent l'accent sur l'hétérogénéité des préférences des consommateurs. En pratique, on part de modèle à coefficients aléatoires pour la valorisation des caractéristiques des biens par les consommateurs. L'utilité du consommateur i pour le bien j s'écrit par exemple :

$$U_{ij} = \alpha - \beta_i p_j + X_j \gamma_i + \xi_j + \epsilon_{ij}$$

Par rapport au modèle initial, on autorise donc les préférences individuelles β_i et γ_i à varier selon les consommateurs. On peut par exemple décomposer le coefficient β_i en fonction des caractéristiques observables D_i et non observables ς_i du consommateur i :

$$\gamma_i = \gamma + \pi^o D_i + \pi^u \varsigma_i$$

On peut alors réécrire l'utilité du consommateur i pour le produit j comme :

$$U_{ij} = \delta_j + \mu_{ij} + \epsilon_{ij}$$

où les δ_j représentent comme précédemment les caractéristiques (observables ou pas) du produit j, et μ_{ij} liés à l'hétérogénéité des préférences du consommateur i pour les caractéristiques observables de ce produit (en regroupant le prix dans les observables X_j par souci de simplicité) :

$$\mu_{ij} = (\pi^o D_i + \pi^u \varsigma_i) * X_j$$

En postulant une loi extrême value pour le terme d'erreur ϵ_{ij}, on peut alors dériver la probabilité que le consommateur i achète le bien j :

$$\mathtt{P}(j|\delta_j, D_i, \varsigma_i) = \frac{e^{\delta_j + \mu_{ij}}}{\sum e^{\delta_k + \mu_{ik}}}$$

La part de marché du produit j s'obtient alors en intégrant selon la distribution des caractéristiques des consommateurs.

$$s_j = \int_{D_i} \int_{\varsigma_i} \mathtt{P}(j|\delta_j, D_i, \varsigma_i) dP_{D_i} dP_{\varsigma_i}$$

L'une des raisons de la popularité de ce modèle est donc qu'elle ne nécessite pas de disposer de données au niveau des consommateurs, mais qu'il suffit de disposer des parts de marchés. Les distributions des caractéristiques des consommateurs suffisent. Le modèle complet à coefficient aléatoire nécessite évidemment de postuler une distribution sur les termes d'hétérogénéité individuelle. Cependant, du fait de leur complexité, les conditions d'identification du modèle à coefficients aléatoires ne sont pas totalement claires et il peut s'avérer difficile à implémenter en pratique. En particulier, Dubé et al. (2008) et Knittel & Metaxoglu (2008) mettent en évidence la sensibilité des résultats à des choix techniques (respectivement sur le critère de tolérance de la "boucle interne" ou de l'algorithme d'optimisation). De fait, Berry et al. (1995) estime ce modèle de demande simultanément avec un modèle d'offre, et disent obtenir des résultats beaucoup moins précis lorsqu'ils estiment le modèle de demande seul. Quoiqu'il en soit, l'un des apports de ces modèles, comme mis en avant par Pakes et al. (1993), est de pouvoir estimer l'impact d'une modification de l'environnement auquel font face les consommateurs (dans leur exemple, le prix de l'essence).

Les outils de régulation environnementale

Une autre question importante est de juger de la pertinence de la politique des bonus/malus eût égard à l'objectif de réduction des émissions de CO_2. Si l'intervention publique est pleinement justifiée par les externalités négatives liées à l'utilisation de véhicules automobiles (voir Parry et al. 2007), plusieurs instruments peuvent théoriquement être utilisé à cette fin.

Pour éviter les distorsions potentielles qu'elles peuvent créer l'imposition de normes contraignantes (type pots catalytiques par exemple pour réduire les émissions de particules fines) est considéré comme moins socialement utile que des incitations fiscales. Celles-ci permettent aux agents économiques d'adapter leur comportement de manière optimale en intégrant les externalités liées par exemple par la pollution. En théorie, l'optimum social de premier rang peut être théoriquement atteint via la seule mise en place de taxes pigouviennes, i.e., de taxes indirectes sur les produits polluants (à la consommation ou à la production). On peut en effet montrer que les choix des individus sont optimaux dès lors que le montant de la taxe est fixée correctement. La TIPP (Taxe Intérieure sur les Produits Pétroliers) en France ou la taxe sur l'essence et le gazole aux Etats-Unis sont des exemples de telles taxes pigouviennes (même si leur niveau n'est a priori pas optimal, voir Parry et al. 2007 pour une discussion sur ce point). Dans ce cadre, il est inutile de recourir à des systèmes tels que le bonus/malus. Pour implémenter des taxes pigouviennes, l'État devrait cependant observer l'utilité marginale au carburant de chaque consommateur. Cette hypothèse est bien sûr irréaliste et, en général, l'État doit mettre en place un système incitant les agent à révéler leur réelle utilité marginale. Du fait de ces asymétries d'information, l'optimum de premier rang ne peut être atteint en général, et il est alors utile en général de taxer des produits complémentaires au bien polluant.

1.2.3 Résultats

Dans cet article, nous nous proposons donc d'évaluer les bénéfices environnementaux liés à la modification de la demande produite par l'introduction du bonus/malus, première tentative de grande ampleur de ce type en France. Nous nous appuyons en particulier sur la rapidité peu commune avec laquelle cette politique a été mise en place : annoncée en novembre à la suite du Grenelle de l'environnement (alors même que l'environnement n'était jusque là pas une thématique traditionnellement portée par les partis de la droite française), elle a été appliquée dès janvier. Il est donc peu probable que les constructeurs aient eu le temps d'adapter leur offre, tant en terme de prix que de caractéristique des véhicules proposés. A ce titre, cette expérience constitue une expérience naturelle de choc sur les prix, qui nous permet d'estimer l'effet de la mesure sur la demande de véhicule pour construire une demande de véhicule "contrefactuelle'. Pour estimer l'impact in fine sur les émissions de CO_2, il faut également tenir compte l'usage qui est fait des véhicules. Outre l'évaluation inédite de la mesure de bonus/malus, l'autre apport de cet article est de proposer un modèle théorique simple mais tractable qui lie le choix du véhicule et le kilométrage, en prenant en compte l'hétérogénéité des consommateurs.

Méthode

L'impact de court terme de la demande peut s'exprimer comme :

$$\Delta^{SR} = \sum_{d=1}^{D} \Pr(D_i = d)\Delta_d^{SR}$$

où n est le nombre d'acheteurs potentiels, $D_i \in \{1, ..., D\}$ représente la caractéristiques des ces acheteurs.

$$\Delta_d^{SR} = n\left[s_{d0C}E_{d0C} - s_{d0P}E_{d0P} + \sum_{j=1}^{J}(s_{djC} - s_{djP})M_j + (s_{djC}T_{jC}\overline{N}_{djC} - s_{djP}T_{jP}\overline{N}_{djP})\right],$$

où $s_{jP} = P(Y_{iP} = j|D_i = d)$ représente la part de marché du véhicule j avec la réforme, $E_{d0P} = E(T_{i0P}N_{i0P}|Y_{iP} = 0, D_i = d)$ les émissions moyennes des personnes de caractéristiques d ayant opté pour le bien extérieur, $\overline{N}_{djP} = E(N_{ijP}|Y_{iP} = j, D_i = d)$ le nombre de kilomètre moyen parcourus par les ménages de type d avec un véhicule j ; $(s_{djC}, E_{djC}, \overline{N}_{djC})$ sont définis de manière similaire.

Tous les paramètres ne sont pas observables directement dans nos données. Plusieurs hypothèses sont donc nécessaires pour les identifier. Pour cela, nous développons un modèle théorique qui lie le choix du véhicule et le kilométrage, en prenant en compte l'hétérogénéité des consommateurs. Soit un individu i, avec des caractéristiques observables X_i. Soit $d \in \{0, 1\}$ la situation ($d = 1$ si la mesure de bonus/malus est en place, $d = 0$ sinon) et soit $t \in \{t_0, t_1\}$ la date. Nous supposons que son utilité indirecte pour choisir le véhicule j en anticipant de voyager N kilometres par an satisfait :

$$U_{it}(N, j, d) = N^{\frac{\gamma_x}{\gamma_x - 1}}\alpha_x + \left(y_{it}(d) - p_{jt}(d) - \frac{c_{ijt}(d)N}{r_x}\right)\beta_x + df_{1x}(Z_{jt}) + \xi_{xjt}(d) + e_{ijt}(d),$$

où y_{it} est le revenu, p_{jt} correspond au prix réel du véhicule j à la date t (y compris le bonus/malus si $d = 1$), r_x est la préférence pour le présent, Z_{jt} est le malus du véhicule j et $\xi_{xjt}(d)$ représentent les autres caractéristiques observables et inobservables du véhicule. $c_{ijt}(d)$ (resp. $c_{i0t}(d)$) est le coût par kilomètre du véhicule j (resp. pour un individu i qui choisit l'option extérieure). Soit p_t^g (resp. p_t^d) le prix d'émettre un gramme de CO_2 quand on utilise un véhicule à essence (resp. diesel) à t, nous avons par exemple $c_{jt} = p_t^g T_{jt}$ si j est une voiture fonctionnant à l'essence. On suppose que $0 < \gamma_x < 1$ et $\alpha_x < 0$, donc les utilités sont des fonctions croissantes et concaves de N. La dépendance en x de $(\beta_x, \gamma_x, r_x, \delta_x, \xi_{xjt}(d))$ reflète l'hétérogénéité observée dans la manière dont les gens valorisent les caractéristiques. Cette spécification autorise un effet direct du bonus sur la demande, en dehors de leur impact indirect via les variations de prix qu'elles induisent. La mesure peut en effet avoir un impact direct sur la sensibilité écologique des consommateurs au-delà des effets prix (voir D'Haultfœuille et al., 2010). On fait l'hypothèse que la mesure n'a pas eu d'impact sur les prix de l'essence et les caractéristiques des véhicules en dehors des prix. Cela implique en particulier que les constructeurs n'ont pas modifié immédiatement les émissions des véhicules suite à la réforme.

Résultats

Les résultats montrent que si le report vers des classes de véhicules bénéficiant du bonus/malus a été massif, les émissions de CO2 n'ont que très faiblement baissé à court terme, et ont même augmenté si l'on inclut les émissions liées à la production de nouveaux véhicules, très coûteuses en émissions de CO2. La mesure a en effet sensiblement augmenté le volume global des ventes : elles seraient supérieures début 2008 de 13,0% à ce qui aurait été observé en l'absence de la mesure. Par ailleurs, l'effet rebond aurait également réduit de manière importante l'efficacité de la mesure. Enfin, les effets décevants de la mesure s'expliquent par les effets de seuils qu'elle induit : les acheteurs se reportent vers des véhicules qui, s'ils bénéficient du bonus, ne sont que marginalement moins polluants que ceux préférés en son absence. Au total, si l'on retient, en concordance avec le rapport de la commission Quinet, une valorisation à 32 euros de la tonne de CO2, le coût de la mesure à court terme serait d'environ 1,5 millions d'euros par trimestre.

Même à long terme, après renouvellement complet du parc automobile, la modification de la demande induite par le bonus/malus aurait un impact négatif sur les émissions de CO_2. Les estimations sont cependant beaucoup plus délicates et reposent sur des hypothèses dont la plausibilité est plus incertaine. Nous proposons plusieurs variantes, qui conduisent toutes à une évaluation négative de la réforme à long terme, et souvent beaucoup plus négatifs qu'à court terme. Il faut insister sur le fait que ces estimations sont uniquement des projections et n'ont pas de valeur prédictive. L'un des objectifs du bonus/malus est également de stimuler l'innovation des constructeurs automobiles pour produire des véhicules moins polluants. Faute de données disponibles, les évaluations ne tiennent pas compte de cet effet, qui devrait améliorer le bilan à long terme de la mesure.

1.3 Les Zones Franches Urbaines permettent-elles de redynamiser les "banlieues" françaises ?[2]

1.3.1 Motivation

Les Zones Franches Urbaines (ZFU) ont été établies, pour les premières d'entre elles, en 1996. Définies à l'échelle d'un quartier (9000 à 30 000 habitants), elles visaient, par un ensemble d'exonérations fiscales et sociales accordées aux entreprises implantées dans ces territoires, à revitaliser des quartiers caractérisés par des concentrations élevées de difficultés sociales. Après une première vague de 44 Zones Franches Urbaines actives au 1er janvier 1997, une seconde vague de 41 zones franches a été mise en place en 2004. 15 nouvelles zones franches ont enfin été ajoutées aux précédentes en 2006.

Le coût du dispositif est élevé : on estime ainsi que les non-recettes fiscales et sociales pour l'État s'élèvent en 2006 à 570 millions d'euros. Ce coût n'a cependant de sens que rapporté à l'efficacité réelle de ces exonérations à créer de l'emploi et/ou à stimuler l'activité économique. Cette efficacité est plus difficile à évaluer. Les zones franches étant par nature moins propices au développement économique, il est difficile de séparer l'impact des exonérations de ces difficultés intrinsèques. Ici aussi, il s'agit d'estimer un "contrefactuel" crédible, c'est-à-dire une estimation de ce qui se serait passé si les entreprises n'avaient pas bénéficié des exonérations ZFU. Si il existe de nombreuses études sur des dispositifs zonés d'allégements fiscaux similaires aux Etats-Unis, le cas français a fait l'objet de bien moins d'attention. Cette évaluation est pourtant intéressante à plus d'un titre. Le dispositif français se singuliarise par en particulier par une très grande générosité en termes d'exemption fiscale, qui surtout ne se limite pas à l'installation de nouvelles entreprises mais également aux entreprises déjà présentes sur la zone. Elles est limitées aux très petites entreprises (moins de cinquantes salariés) indépendantes. L'analyse de ce

2. Ce chapitre est coécrit avec Roland Rathelot (CREST) et Patrick Sillard (INSEE).

type de dispositifs renvoie donc, au-delà même de leur efficacité à développer le tissu urbain, à déterminer l'efficacité des aides financières indirectes que constituent ces allègements fiscaux pour le développement économique des PME.

1.3.2 Revue de littérature

Les première de politiques de redynamisation économique en faveur de territoires ont été expérimentées aux Royaume-Uni et aux Etats-Unis. S'il existe une littérature importante pour évaluer l'efficacité de ces *enterprise zones* ou *empowerment zones*, le bilan qu'on peut en faire n'est pas clair. Papke (1994) et Boarnet & Bogart (1996) font partie des premiers travaux économétriques proposant une évaluation de ces *enterprise zones*. Leurs résultats, obtenues sur deux Etats différents, sont opposés. Alors que Papke (1994) conclut que les allégements zonaux stimulent l'emploi et l'investissement local dans l'Indiana, Boarnet & Bogart (1996) ne trouvent aucun effet dans le cas du New Jersey.

Quoiqu'il en soit, ces résultats sont difficilement généralisable dans la mesure où il ne s'applique qu'à certains États, alors que les programmes américains, s'ils sont tous fondés sur des allégements substantiels de taxe, diffèrent largement d'un État à un autre, par la cible sur laquelle portent les allègements (emploi ou investissement), par la générosité et la durée du dispositif, mais également par la procédure de choix des quartiers bénéficiaires. Les études récentes ont montré la grande hétérogéniété dans l'efficacité des zones suivant le contenu du dispositif Bondonio & Greenbaum, 2007 et la manière dont elles sont administrées Kolko & Neumark, 2010. Leur impact peut également différer selon les types d'établissement. Bondonio & Greenbaum (2007) obtiennent ainsi des effets opposés sur les firmes nouvelles ou déjà implantées, résultant dans un effet total nul.

Concernant les Zones Franches Urbaines françaises, les travaux d'évaluation sont nettement moins nombreux. Plusieurs rapports administratifs ont cherché à tirer un bilan du dispositif, dans la foulée de son lancement. En particulier, Barilari et al. (1998), Buguet (1998) concluent à un impact positif mais modéré des ZFU. La statistique publique a ensuite développé son système de suivi, comme le montre le rapport annuel de l'Observatoire national des zones urbaines sensibles **?**, ainsi que les études de l'Insee et la Dares Thélot (2006), Bachelet (2007). Parmi les études qui ont tenté d'évaluer économétriquement l'impact des Zones Franches Urbaines sur la création d'établissements et d'emplois salariés, Rathelot & Sillard (2009) trouvent des effets positifs significatifs sur la création d'emploi et la création d'activité, mais ces effets sont d'une amplitude faible ; dans l'une des rares études françaises qui traitent explicitement en compte les effets de sélection, Gobillon et al. (2010*a*) étudient l'effet des Zones Franches Urbaines sur la sortie du chômage des demandeurs d'emplois inscrits à l'ANPE et ne trouvent pas d'impact significatif des ZFU sur cette dimension.

Les études empiriques doivent traiter deux types de problèmes. Tout d'abord, les effets de sélection sus-mentionnés, les zones n'ayant pas été déterminées au hasard (comme cela aurait pu être le cas dans le cadre d'une expérience randomisées). De nombreuses études utilisent des données de panel (voir par exemple Parry et al. 2007pap94, bb96,be00), complétés par des méthodes d'appariement sur le score de propension O'Keefe, 2004. Formellement, rappelons qu'avec les notations et notions popularisés par Rubin, et en définissant Y_i^1 la situation potentielle de la zone i si elle fait partie des zones ciblées, et Y_i^0 cette situation potentielle en l'absence de ces dispositifs, l'impact causal des allégements fiscaux pour la zone i correspond exactement à $Y_i^1 - Y_i^0$.

Les méthodes d'appariement reposent sur l'hypothèse d'indépendance conditionnelle aux observables :

$$Y_i^0 \perp T_i | X_i \tag{1.3.1}$$

où X_i représentent les caractéristiques observables de la zone.
Comme montré par Rosenbaum & Rubin (1983), si cette hypothèse est vérifiée pour un ensemble d'observables X, elle est aussi valable pour le score de propension, c'est-à-dire la probabilité pour une zone d'être ZFU conditionnellement à ces observables. Plus généralement, cette condition est vérifiée pour toute fonction "équilibrée" des observables, ce qui signifie que même si on n'observe pas le vrai score en pratique (ce qui est en général le cas), l'hypothèse est toujours vérifiée pour une estimation de ce score qui "équilibre" les observables du groupe traité et du groupe de contrôle (formellement on attend $T \perp X | \hat{p}(X)$, où T est une

indicatrice du fait d'être ciblé dans le dispositif ou pas. On a donc :

$$E(Y_{i0}|T_i = 1, p(T_i = 1|X_i)) = E(Y_{i0}|T_i = 0, p(T_i = 1|X_i))$$

En pratique, l'estimation se fait en deux étapes : l'estimation du score de propension, puis l'appariement. Une estimation de l'effet moyen des zones sur les traités (ATT) est alors donné par :

$$\hat{\Delta} = \frac{1}{N_{EZ}} \sum_{i \in EZ} (Y_i - \sum_{j \notin EZ} w(i,j) Y_j)$$

où N_{EZ} représente le nombre de ZFU, et $w(i,j)$ une fonction de pondération qui surpondère les ZRU ayant des caractéristiques observables similaires aux avec des ZFU, qui peut par exemple être fourni par une fonction à noyau (voir Parry et al. 2007hit98).

L'hypothèse d'indépendance aux observables est cependant forte. C'est pourquoi, lorsque des données sur les périodes précédant la mise en place du dispositif évalué sont disponibles, il est préférable d'utiliser des formes différenciées pour tenir compte des effets fixes. Ce type d'estimateur est parfois appelé "différence de différences conditionnelles". Comme montré par Heckman, Ichimura & Todd (1998), ces estimateurs sont moins biaisés que ceux obtenus par matching simple. Ils reposent sur une hypothèse plus faible que (1.3.1), soit :

$$Y^0_{it} - Y^0_{it-1} \perp T_{it}|X_i \tag{1.3.2}$$

Une autre difficulté pour ces études où la dimension géographique est essentielle est de disposer de données à un niveau adaptée. Le contour des zones ne correspond pas en général au découpage administratif standard, les ZFU étant définies par exemple dans le cas français au niveau infra-communal. En l'absence de données précises, ce la peut conduuire à "diluer" l'impact de la zone dans une unité administrative plus importante. La mise à disposition de données géolocalisées peut permettre d'affiner le diagnostic sur l'effet sur les seules zones. Cependant, ces effets sont potentiellement ambigus : le ciblage peut en effet créer des externalités négatives. Le développement des ZFU se ferait donc au détriment des zones voisines, ce qui n'est pas forcément souhaitable. Ici encore, les données géolocalisées peuvent permettre d'évaluer dans quelle mesure les effets potentiels mesurés à l'intérieur des zones se sont accompagnés de mouvement inverse sur les quartiers situés à la périphérie de ces zones.

1.3.3 Résultats

Pour évaluer l'impact des ZFU, une méthode de Différence-de-Différence conditionnelle est utilisée. Le groupe de contrôle est défini par le fait que le dispositif de zone franche n'est pas le seul outil de développement économique des quartiers sensibles : en effet, en même temps qu'étaient instaurées les 44 premières ZFU en 1998, plus de 300 quartiers sensibles de France métropolitaine étaient classés zones de revitalisation urbaine (ZRU). La méthode d'appariement permet de réduire en partie les effets de sélection, liés au fait que les ZFU ont été théoriquement choisies parmi les ZRU cumulant des handicaps socio-économiques. Elle est d'autant plus crédible ici que la désignation des ZFU de deuxième génération a aussi répondu à un objectif de "saupoudrage" des zones sur l'ensemble du territoire et non seulement à celui de cibler les zones les plus défavorisés. D'après les estimations (confirmées par des discussions informelles avec des responsables administratifs), l'autre critère essentiel était la taille de la zone, seules les plus grandes ZRU étant désignées comme ZFU, ce critère étant parfois atteint grâce au regroupement de zones proches. Conformément à cette intuition, nous montrons qu'une ZRU avait en 2002 d'autant plus de chances d'être désignées comme zones franches qu'elle était loin d'une ZFU de première génération et proche d'une autre ZRU (et donc potentiellement regroupable avec elle). Nous estimons ensuite l'effet sur de nombreuses dimensions des zones franches : démographie des entreprises, emploi et activité économique des entreprises en place. Pour une meilleure interprétation des résultats, il n'est pas inutile de rappeler ce qui est mesuré, à travers le prisme des revenus potentiels du cadre de Rubin déjà mentionné. En particulier, l'hypothèse implicite est que le revenu potentiel d'une zone i à la date t en l'absence des allégements fiscaux peut se décomposer comme :

$$Y_{it}^0 = \alpha_i + \alpha_t + u_{it}$$

où α_i et α_t sont des effets fixes (individuel et temporel). Soit τ_t l'effet propre des allégements de charges à la date t, le revenu observé dans la zone i peut s'écrire :

$$Y_{it} = \tau_t 1_{T_1 <= t} + \alpha_i + \alpha_t + u_{it}$$

où $T_1 = 2004$ correspond à la première année d'introduction des allègements de charges.

L'estimateur de différences temporelles conditionnels mesure la différence entre ΔY_{it} [3] et $\Delta^0 Y_{it}$, l'évolution contrefactuelle qu'on estime à partir des zones du groupe de contrôle présentant des caractéristiques proches de la zone i. Au final, on mesure donc :

$$E(\Delta Y_{it} - \hat{\Delta}^0 Y_{it}) = (\tau_t - \tau_{t-1}) 1_{T_1<t} + \tau_{T_1} 1_{t=T_1}$$

Si l'effet des allégements fiscaux est stable au cours du temps, on doit observer un impact à la première période seulement, sinon l'effet peut être persistant (ou au contraire déclinant) au cours du temps.

On montre que la transformation d'une ZRU en ZFU sur l'activité économique mesurée par le nombre d'entreprises présentes sur la zone est positive dès la première année de mise en place du dispositif. Le stock d'entreprise est ainsi de 7% plus haut dans les ZFU que dans les SRU comparables. Cet impact est même persistant les deux années suivantes, correspondant à une augmentation du taux de croissance du stock d'entreprises de 7% en plus en 2003 et 2004. Quand on se limite aux entreprises qui étaient déjà présentes sur les zones avant le classement en ZFU (soit en 2002), les allégements fiscaux se traduisent par une augmentation de 11% des entreprises encore présentes la première année après l'introduction des ZFU. L'impact ne paraît pas persistant. De manière intéressante, cet effet est limité aux plus petites entreprises et même plus forts (20%). En terme de flux, le fait q'une zone soit ZFU se traduit par l'installation de 19 entreprises supplémentaires chaque année. Un quart de ces installations correspondent à de simples transferts d'activité déjà existantes, ce qui peut traduire des comportements opportunistes. Nous n'observons aucun impact significatif sur les défaillances d'entreprises. L'impact sur les défaillances est a priori ambigu : les allégements fiscaux peuvent aider à la solvabilisation d'entreprises déjà présentes sur la zone, qui auraient fait faillite en l'absence de ces subventions indirectes tandis qu'à l'inverse les effets d'aubaine peuvent conduire à l'installation d'entreprises reposant sur des projets peu viables. Ces deux effets vont théoriquement en sens opposé.

Les effets des allègements fiscaux observés sur le stock d'entreprises se retrouvent sur le volume d'emploi, même si les effets sont non significatifs la première année.

3. qui correspond au taux de croissance pour des variable en log.

Le taux de croissance de l'emploi serait ainsi supérieur de 10% par rapport à ce qu'on aurait du observer dans la zone en 2005, et cet effet est même le double l'année suivante. L'essentiel est lié à la présence de nouvelles entreprises. Lorsqu'on se limite aux entreprises déjà présentes sur la zone en 2002, l'impact est négligeable, et même *n*égatif la première année. Pour les entreprises plus "importantes" (plus de quatre salariés), aucun effet significatif n'apparaît avant 2007 (soit trois ans après la mise en place des allègements), et ne s'observe que pour les heures travaillées. Les résultats sont encore plus décevant quand on s'intéresse aux autres variables économiques des entreprises. On observe essentiellement un impact de court-terme sur le revenu imposable, qui reflète l'exemption de certaines taxes (comme les charges sociales). Cela se traduit par une evolution positive et croissante de la trésorerie des entreprises bénéficiaires.

1.4 L'emploi temporaire permet-il (parfois) d'accéder à l'emploi stable ?[3]

1.4.1 Motivation

Depuis le début des années quatre-vingt, la plupart des pays européens ont tenté d'assouplir les lois assurant la protection de l'emploi. Il s'agissait de répondre aux critiques récurrentes sur la "sclérose" des marchés de l'emploi européens : les protections de l'emploi, mises en place dans le contexte florissant des Trente Glorieuses, contraindraient le rythme des créations d'emplois. Par contraste, la doctrine de l'employment *at will* aux États-Unis (selon laquelle la relation d'emploi peut être rompue sans aucun préavis par l'employeur ou le salarié) aurait permis à l'économie américaine de s'adapter aux ralentissements économiques et de continuer à créer des emplois à un rythme soutenu Bentolila & Bertola, 1990.

En France, les aménagements de la protection de l'emploi sont surtout passés par des extensions des possibilités du recours au travail temporaire. Les emplois dits atypiques (CDD, intérim, contrats aidés) ont alors connu une progression impressionnante (bien que de moindre ampleur de ce qu'on a pu observer en Espagne) :

3. Ce chapitre est coécrit avec Lionel Wilner (INSEE-CREST).

ils représentaient ainsi 12% de l'emploi en 2007, alors que cette proportion était marginale 25 ans auparavant. En flux, cette évolution est encore plus sensible : plus des deux tiers des embauches en 2006 étaient des CDD.

Les conséquences du développement des emplois temporaires pour les salariés restent controversées. Au niveau macroéconomique, de nombreuses études ont mis l'accent sur les effets ambigus que peuvent avoir ce type d'emploi sur le chômage. Plus récemment, des études sur données individuelles se sont attachés à déterminer le rôle de ce type de contrat sur les trajectoires professionnelles des individus. Pour reprendre la formulation désormais classique de Booth et al. (2002), il s'agit de savoir si ces emplois constituent des "tremplins" vers des emplois stables ou au contraire des "trappes" à précarité. Parce qu'ils facilitent l'embauche, ces contrats peuvent permettre à des personnes d'acquérir du capital humain grâce à une expérience professionnelle, voire d'être embauchées dans l'entreprise si cette dernière a un poste permanent. Du fait de coût de séparation réduit, les employeurs seraient en effet moins réticents à embaucher des salariés en cas de choc positif sur leur activité, même en cas d'incertitude sur la pérennité de celui-ci. D'autre part, à cet usage classique de la main d'œuvre temporaire comme amortisseur conjoncturel ("buffer stock"), ce type de contrat offre également une période d'essai prolongée. Les employeurs hésiteraient moins à recruter des salariés même en cas d'incertitude sur leur productivité (par exemple des jeunes sans expérience professionnelle) parce que les possibilités de rupture demeure au-delà des deux ou trois mois de période d'essai autorisé dans le cadre d'un CDI. *A contrario*, certains s'inquiètent de cette évolution qui fait peser sur le salarié l'essentiel de l'incertitude sur la qualité de la relation d'emploi. L'accès à un emploi en CDI conditionne également de nombreux aspects de l'insertion sociale, comme l'accès au logement. Le risque d'exposition au chômage fait craindre également une dualisation du marché du travail : les personnes les plus fragiles pourraient se retrouver durablement confinées dans des trajectoires "instables", alternant périodes de chômage et "petits boulots" sans avenir. Ce mécanisme pourrait se produire par exemple si le fait d'avoir occuper un emploi temporaire constitue un signal négatif pour les employeurs futurs, surtout si le capital humain acquis dans le cadre d'un tel emploi est spécifique à l'entreprise.

Plusieurs raisons expliquent qu'il soit difficile voire impossible d'apporter une réponse définitive à ce débat.

La première tient à la difficulté à mesurer l'insertion. Celle-ci est constituée de multiples facettes qu'on ne peut appréhender par un indicateur unique. Le niveau de salaire, l'adéquation avec les qualifications obtenues ou à l'inverse le déclassement ressenti sont par exemple des aspects importants de la qualité de l'insertion. La "précarité" ne se mesure pas uniquement par la nature du contrat de travail, et l'accès à un emploi en contrat indéterminé n'est pas forcément une garantie suffisante d'insertion.

La deuxième raison est que même en se restreignant à un aspect simple comme le contrat de travail, il n'existe pas de "mesure" objective permettant d'établir que les emplois temporaires freinent ou accélèrent l'accès à un emploi stable. Le fait que *certains* emplois temporaires débouchent sur un emploi stable, quand d'autres non n'apporte *en soi* qu'une information limitée : la question pertinente est de déterminer ce qu'il en aurait été de l'insertion de ces personnes, si elles n'avaient pas occupé un emploi temporaire. Pour illustrer de manière caricaturale cette question, on peut penser au cas hypothétique d'une personne sans emploi qui recevrait une offre d'emploi temporaire : a-t-elle intérêt à accepter cette offre, ou au contraire à attendre une offre en CDI ? Cette reformulation de la question montre qu'il est illusoire d'attendre une mesure directe de l'impact des emplois temporaires sur l'insertion : au mieux peut-on avoir une mesure relative en prenant les chômeurs comme référence. Mais cette approche est évidemment compliquée par la présence d'effets de composition : dans notre exemple précédent, on peut supposer que les personnes qui choisiront *in fine* d'occuper un emploi temporaire ne sont pas les mêmes (en termes d'opportunités futures en particulier) que celles qui préféreront attendre une meilleure offre.

Une troisième raison tient à la complexité et la diversité des trajectoires professionnelles. Pour y répondre correctement, il est nécessaire d'observer des personnes sur une longue période, ce qui est rarement possible. Lorsque c'est le cas, l'analyse est complexe. Le simple fait d'occuper un emploi temporaire ou de passer par le chômage a sans doute un effet sur la trajectoire future, mais on peut également penser que c'est également la durée passée dans ce type d'emploi ou encore la récurrence de tels épisodes, qui a un impact. Cela signifie encore que les réponses apportées à la question de l'impact de la précarité risquent de n'être pas univoques, et qu'il est nécessaire d'examiner l'effets des emplois temporaires sous ces différentes dimensions.

1.4.2 Littérature

Booth et al. (2002), dans une étude très souvent citées, s'intéressent à la situation des salariés temporaires au Royaume-Uni, à partir d'un panel de ménages. Ils montrent que si les salariés temporaires cumulent les motifs d'insatisfaction professionnelle (moindre rémunération et accès plus difficile à la formation en particulier), ces emplois peuvent servir de tremplin vers l'emploi stable pour certains. Leur étude reste cependant uniquement descriptive, et leurs conclusions par ailleurs peu extrapolables au delà du cadre britannique. A leur suite, de nombreuses études ont tenté d'évaluer le rôle de "tremplin" des emplois temporaires : en utilisant une modélisation plus complète de l'hétérogénéité individuelle, Zijl, Van der Berg et Heyma (2004) concluent que, en Suède, les emplois temporaires réduisent la durée de chômage et accroissent substantiellement la part de chômeurs qui obtiennent un emploi régulier dans les années qui suivent l'entrée au chômage, par rapport à ceux qui ne passent pas par l'emploi temporaire.

Autor & Houseman (2010) utilisent une expérience intervenue dans l'état américain du Michigan, dans lesquels les bénéficiaires de l'aide sociale étaient attribués aléatoirement à des opérateurs privés de placement. Ces opérateurs ayant des recours différents à de l'"emploi temporaire" (qui correspond à l'intérim), les bénéficiaires ont potentiellement des probabilités différentes d'occuper un emploi d'intérimaires, une partie de cette variation étant indépendante des caractéristiques propres des personnes concernées, puisque ne dépendant que de l'opérateur auquel il a été aléatoirement affecté. Leurs résultats sont peu probants sur l'efficacité de l'emploi temporaire. Ichino et al. (2008) obtiennent cependant des résultats opposés pour l'Italie, en utilisant l'introduction récente du travail intérimaire, qui s'est traduite par une présence différenciée des agences d'intérim selon les régions.

L'utilité de ces comparaisons internationales pour évaluer l'impact des contrats temporaires dans le cas français est limitée. Ces comparaisons internationales se heurtent à deux limites classiques. La première tient au sens à donner à ce qui est mesuré : le terme *temporary work* souvent utilisé peut renvoyer selon les contextes nationaux au travail intérimaire (pour lequel le contrat de travail passe par un tiers, l'agence d'intérim), des contrats plus ou moins aidés, ou des contrats à durée déterminée (relation contractuelle directe entre le salarié et son employeur). Ces différents contrats s'adressent *a priori* à des personnes et/ou des emplois disparates, et ont donc toutes les chances d'aboutir à des devenirs différents. La deuxième limite porte sur la disparité des contextes nationaux : on peut s'interroger par exemple sur la pertinence de comparer l'usage de contrat temporaire dans le cas d'un marché du travail totalement flexible comme le marché américain et l'usage de CDD dans un cadre où les contraintes sur les licenciements sont nettement plus présentes comme le cas français. Bentolila et al. (2010) montrent ainsi que deux pays pour lesquels la protection de l'emploi est proche (la France et l'Espagne), des disparités sur les coûts de séparation peuvent se trouver amplifiées dans les réactions aux chocs économiques.

Pourtant, à notre connaissance, les études françaises sur le sujet restent rares. Elles ont essentiellement porté sur l'évaluation de dispositifs particuliers d'aide à l'emploi. Bonnal et al. (1997), Magnac (2000) et plus récemment Havet (2006) tentent d'évaluer l'impact des dispositifs de contrats aidés sur l'insertion des jeunes, dont ils tirent un bilan mitigé.

La mesure empirique de l'impact des contrats temporaires sur les trajectoires d'emploi se heurte au problème classique de sélection. Les personnes en contrat temporaire sont par exemple différentes des chômeurs par rapport à de nombreuses caractéristiques, dont certaines sont observables (éducation et expérience par exemple) mais d'autres non. Ces caractéristiques peuvent également avoir une influence sur leur chance de décrocher un emploi stable. On peut distinguer deux courants dans la littérature sur la manière de traiter ce problème en lien avec les trajectoires professionnelles.

La première s'est surtout concentrée sur l'analyse de la dynamique. A la suite par exemple de Bonnal et al. (1997), de nombreux papiers ont essayé de décrire précisément les trajectoires individuelles sur le marché de l'emploi, en utilisant en particulier des modèles à risques concurrents. Des papiers récents essaient ainsi de mettre en avant les effets dynamiques liés à l'accumulation des contrats temporaires avec le temps Doiron & Gørgens, 2008, la récurrence d'emploi et la dépendance en durée Gagliarducci, 2005 sur les transitions. L'inconvénient de ces analyses de la durée est de reposer fortement sur des hypothèses paramétriques, en particulier pour l'hétérogénéité inobservée. De plus, principalement du fait de limitations techniques liés aux contraintes pour l'optimisation, un choix doit être fait entre la complexité de la dynamique et le degré de précision sur la description du marché du travail (nombres des états en particulier). Pourtant les contrats temporaires (CDD) ou intérimaire par exemple correspondent à des besoins différents, et peuvent donc conduire à des perspectives de carrière différentes. Finalement, l'analyse des durées demande d'avoir accès à des données longitudinales qui manquent pour la France : les panels sur longue période s'appuient sur des questions rétrospectives, dont la qualité est questionable du fait d'erreur de mesure Magnac & Visser, 1999.

La deuxième approche s'intéresse plus à la modélisation des transitions, en temps discret. Au prix d'une description plus grossière de la dynamique que celle autorisée par les modèles de durée – elle est souvent résumée par une chaîne de Markov d'ordre un – la modélisation de l'hétérogénéité individuelle peut être mieux prises en compte. Magnac (2000) et Honoré & Kyriazidou (2000) proposent ainsi un modèle multinomial à effets fixes qui étend la procédure de logit conditionnel de Chamberlain (1984). Cette spécification ne fait pas d'hypothèse paramétrique sur les effets individuels, la dépendance d'état est mesurée à travers des matrices de mobilité. Un inconvénient éventuel est que l'identification se fait sur un sous-échantillon, les "movers" qui expérimentent au moins une transition sur la période d'observation Magnac, 2000. Honoré & Kyriazidou (2000) montrent que l'impact de covariables variables avec le temps peuvent être identifiées, sous certaines conditions.

1.4.3 Résultats

Cette étude évalue alors la dépendance d'état, c'est-à-dire le poids de la trajectoire passée sur la situation professionnelle actuelle. Plus précisément, nous regardons si le fait d'être passé par un état d'origine donné a une influence sur le fait d'occuper un état de destination donné le trimestre suivant. Nous présentons donc les résultats sous forme de matrices d'intensité des transitions. Bien qu'intéressante en soi, une fréquence "brute" n'apporte qu'une information limitée. Il n'existe évidemment pas de niveau théorique sur les taux de conversion en CDI qui permettrait de considérer qu'en-deçà de ce seuil, les CDD constituent une trappe et un tremplin au-delà. Un indicateur relatif a plus de pertinence. Typiquement, on va chercher à savoir si les transitions vers un CDI sont plus ou moins fréquentes quand on occupait un CDD *plutôt qu'un autre état*, par exemple le chômage. En pratique, on s'intéresse donc aux probabilités relatives de transitions d'un trimestre à l'autre.

Pour nos estimations, nous utilisons les données de l'enquête Emploi en Continu. Cette enquête est menée depuis 2002 chaque trimestre auprès de 75 000 personnes environ. Chaque personne est interrogée en principe six trimestres consécutifs. L'enquête fournit des informations détaillées sur les personnes interrogées, sur leur situation professionnelle et leur emploi éventuel. Elle comprend en particulier une description précise de la durée du travail (par exemple mesure des heures supplémentaires, rémunérées et non rémunérées, et également des absences). La taille importante de l'échantillon permet en particulier de distinguer les différents emplois temporaires (essentiellement CDD et intérim). Ils correspondent a priori à des pratiques distinctes des entreprises, et on peut donc s'interroger sur la pertinence de les regrouper comme il est souvent fait. Nous utilisons les données de 2006 à 2010, soit une période marquée par de fortes fluctuations conjoncturelles. Alors que l'usage de l'emploi temporaire est en principe d'amortir les chocs conjoncturels, il semble en effet important de contrôler de cette dimension.

En pratique, on distingue en plus du chômage, pris comme état de référence, cinq états différents : inactivité, CDI, CDD, intérim et "autre", essentiellement constitué des salariés du public (mais auquel on ajoute également par souci de simplicité les indépendants et contrats aidés).

Les coefficients de la matrice de transition $\hat{\delta}$ (Table 5) correspondent à :

$$\widehat{\delta}_{kj} = log\frac{\hat{\mathbb{P}}(y_{it} = j|y_{i,t-1} = k)/\hat{\mathbb{P}}(y_{it} = 0|y_{i,t-1} = k)}{\hat{\mathbb{P}}(y_{it} = j|y_{i,t-1} = 0)/\hat{\mathbb{P}}(y_{it} = 0|y_{i,t-1} = 0)}$$

où l'état de référence 0 est le chômage. On évalue donc l'écart entre les chances de se retrouver dans l'état j plutôt qu'au chômage, sachant qu'on vient de l'état

k plutôt que du chômage.

Les estimations montrent que les contrats courts améliorent en partie, mais en partie seulement, les perspectives professionnelles des salariés qui en bénéficient. Les matrices d'intensité des transitions entre les différents états professionnels restent marquées par une forte inertie, même en tenant compte des effets liés à l'hétérogénéité individuelle. Les CDD offrent de meilleures perspectives d'accès à l'emploi stable que le chômage : un salarié en CDD a ainsi plus de deux fois plus de chance qu'un chômeur d'avoir un emploi stable plutôt qu'être au chômage un trimestre plus tard. A l'inverse, l'intérim offre moins de perspectives. Le fait de disposer d'un emploi stable plutôt qu'être au chômage n'est pas affecté par les conditions économiques. En revanche, le taux de croissance du PIB a un fort impact sur la probabilité d'être en intérim, ou en CDD, ce qui est cohérent avec l'usage de ces contrats comme "amortisseurs" des chocs conjoncturels. Alors que cette dimension est souvent absente des analyses micro-économiques, nous testons la sensibilité des résultats obtenus sur les termes de dépendance d'état à l'inclusion de cette variable. Les résultats apparaissent sensiblement différents une fois qu'ils sont pris en compte.

Plusieurs études récentes s'intéressent en effet à l'usage des contrats temporaires comme période d'essai prolongée (voir par exemple Portugal & Varejão, 2009). Engellandt & Riphahn (2005) montrent en particulier que les salariés suisses en CDD effectuent significativement plus d'heures supplémentaires et sont moins souvent absents que les salariés en CDI, ce qu'ils interprètent comme l'effet incitatif créé par le statut précaire des salariés : dans l'espoir de voir leur contrat transformé en CDI, ces salariés tenteraient de se signaler par une productivité supérieure (ce qui les conduirait à travailler plus). Dolado & Stucchi (2008) utilisent ce résultat pour interpréter le fait que les entreprises espagnoles dans lesquelles le taux de transformation de contrats temporaires est le plus élevé ont également la productivité la plus élevée. Ce lien s'expliquerait par le fait que les salariés en CDD sont effectivement incités à se signaler par un effort supplémentaire s'ils anticipent effectivement que l'entreprise valorise ce comportement et utilise les contrats temporaires pour sélectionner ses salariés pour une position fixe. La mise en évidence empirique de cette causalité est cependant complexe.

Les données sur la période récente montrent que les salariés en CDD font plutôt plus d'heures supplémentaires dans les phases *basses* du cycle, ce qui pourrait s'expliquer par le fait que cette dimension incitative serait d'autant plus efficace que les options extérieures sont faibles. Les salariés en CDD ou en intérim qui font des heures supplémentaires sont plus souvent en CDI le trimestre suivant s'ils ont fait des heures supplémentaires, même en contrôlant des caractéristiques observables et du cycle. Néanmoins, en contrôlant des caractéristiques inobservables par un modèle à effet fixe, les différences d'accès à l'emploi stable ne sont plus significatives. Si ce résultat ne signifie pas que les contrats temporaires peuvent être utilisé comme période d'essai, il semble que le "signal" offert par les salariés passe par d'autres caractéristiques.

1.5 Quel impact du salaire minimum sur les inégalités salariales ?[4]

1.5.1 Motivation

Le salaire minimum (Salaire Minimum Interprofessionnel de Croissance) est un des instruments privilégiées de la puissance publique sur le fonctionnement du fonctionnement du travail. Son niveau a des implications à la fois sociales et économiques. Pensé comme un outil de cohésion sociale (la loi du 2 janvier 1970 stipule ainsi qu'il doit

> *assurer aux salariés dont les rémunérations sont les plus faibles, la garantie de leur pouvoir d'achat et leur participation au développement économique de la nation.*

, il est susceptible d'avoir un impact sur les inégalités salariales. Ces effets sont d'au moins trois ordres. En augmentant mécaniquement les bas salaires, le SMIC peut contribuer à comprimer la hiérarchie des salaires. Cependant, l'augmentation du salaire minimum peut "se diffuser" à des salaires plus élevés, par exemple pour conserver une hiérarchie salariale. Enfin, un salaire minimum trop élevé peut

4. Ce chapitre est coécrit avec Romain Aeberhardt (DARES-CREST) et Claire Marbot (INSEE-CREST).

contribuer à l'éviction de salariés dont la productivité serait jugée trop faible au regard du salaire auquel ils peuvent prétendre, ce qui peut déformer l'ensemble de la distribution des salaires des actifs encore occupés.

Si les effets potentiels du salaire minimum sur l'emploi ont fait l'objet d'une littérature abondante, ses effets sur la distributions des salaires ont été moins étudiés. Celle-ci se heurte à au moins deux difficultés principales. Le salaire minimum étant en général le même pour tous les salariés, il est impossible de distinguer ce qui relève de l'augmentation du salaire minimum d'une évolution tendancielle des salaires ou de tout autre effet conjoncturel. Par ailleurs, dans le cas français, le salaire minimum est lui-même revalorisé en fonction des évolutions passées des salaires moyens (il est en partie indexé sur le "Salaire Horaire de Base Ouvrier") [5]. L'impact d'une évolution du SMIC sur l'évolution des salaires inclura en partie l'effet de dynamique naturel des salaires qui aurait lieu même en l'absence de revalorisation du SMIC. L'estimation doit donc tenir compte de cette source d'endogénéité.

1.5.2 Revue de littérature

Les études économiques sur les effets d'un salaire minimum en France se sont surtout intéressées à son impact sur l'emploi. Au début de la décennie précédente, Kramarz & Philippon (2001) étudient par exemple l'impact sur le maintien dans l'emploi des salariés directement touchés par les augmentations du salaire minimum. Par ailleurs, par une méthode plus paramétrique proche de celle de Meyer & Wise (1983), Laroque & Salanie (2002) proposent une décomposition du "non-emploi" en France dont le salaire minimum est un déterminant important.

Les études françaises qui s'intéressent à l'impact du SMIC sur les salaires sont plus rares. La principale contribution est celle de Koubi & Lhommeau (2007), qui conclut que les augmentations du salaire minimum auraient des effets importants, jusqu'à des salaires aussi hauts que deux fois le SMIC. Ces résultats rejoignent ceux de quelques études sur données étrangères. Ainsi Teulings (2000, 2003) ob-

5. Plus précisément sur la hausse des prix à la consommation et la moitié de l'augmentation annuelle du SHBO, auquel s'ajoute un coup de pouce éventuel. On trouvera dans Cette & Wasmer (2010) une discussion des conséquences de ce mécanisme.

serve également une diffusion importante des augmentations salariales au dessus du salaire minimum aux Pays-Bas.

De même, Dickens et al. (1999), en utilisant la coexistence de plusieurs salaires minimums sectoriels au Royaume-Uni (jusqu'en 1993), mettent en évidence des effets du salaire minimum jusqu'au 40[e] centile. Toutefois, Dickens & Manning (2004) estiment que les effets de diffusion auraient été modestes au moment de la réintroduction d'un salaire minimum (national) au Royaume-Uni en 1999. De manière plus anecdotique, une étude célèbre de Katz & Krueger (1992), portant sur les établissements de restauration rapide du New-Jersey, met en évidence des effets positifs sur l'emploi alors même que les salaires étaient augmentés au-delà du salaire minimum. Lee (1999) s'intéresse quant à lui à la contribution de la forte diminution du salaire minimum (en termes réels) à l'explosion des inégalités salariales observée depuis les années quatre-vingt aux États-Unis. Il utilise comme mesure de cette inégalité l'écart entre le salaire minimum et le salaire médian.

Comme noté par Lee (1999), deux effets peuvent expliquer cette répercussion des variations de salaire minimum sur l'ensemble de la distribution. En premier lieu, les “effets emploi” mentionnés plus haut : l'éviction d'une partie des salariés (ceux dont le salaire initial était inférieur au nouveau salaire minimum) déforme mécaniquement la distribution observée. En second lieu, les hausses de salaire minimum peuvent entrainer une augmentation des salaires plus élevés. Si ces effets sont en contradiction avec la théorie économique classique, plusieurs arguments ont été avancés pour expliquer une divergence entre productivité et salaire. La confrontation des pouvoirs de négociation respectifs du salarié et de l'employeur (*monopsone*, voir par exemple Dickens et al. 1999), ou l'existence d'asymétries d'information (salaire d'efficience) en sont des exemples. L'existence d'une grille salariale ascendante peut ainsi être un moyen de stimuler les efforts des salariés, par exemple dans un modèle de tournoi. Citons également l'étude sur données expérimentales Falk et al. (2006) qui tente de reproduire l'impact sur les négociations salariales de l'existence d'un salaire minimum. Selon ses résultats, le salaire minimum aurait un impact très net sur le salaire de réservation des employés ; à l'équilibre, cela expliquerait la présence d'augmentations importantes, au-delà même du niveau imposé par le salaire minimum. Le salaire minimum pourrait donc jouer sur la perception des agents du niveau “juste” de la rémunération. La transposition de ces résultats obtenus en laboratoire à l'économie réelle doit évidemment être questionnée.

Depuis, sur la dernière décennie, de nombreux outils ont été proposés pour une analyse plus fine de l'ensemble de la distribution salariale. Les régressions de quantiles, dont l'usage s'est répandu ces dernières années, permettent ainsi d'étudier l'impact de déterminants sur les différents quantiles de la distribution de salaire. Ces méthodes, dont le principe est ancien (voir Koenker & Hallock (2001)), se sont beaucoup enrichies depuis. Nous nous appuyons dans cette étude sur Firpo, Fortin & Lemieux (2009). La méthode proposée présente l'avantage d'estimer directement l'impact d'une modification de la distribution des observables (par exemple, une augmentation générale de la qualification, ou une modification marginale du SMIC) sur la distribution des salaires sur l'ensemble de la population.

1.5.3 Résultats

Pour identifier les effets du salaire minimum sur la distribution de salaire, nous utilisons la configuration particulière créée par les accords successifs de RTT depuis 1998, et plus précisément la période de convergence des SMIC qui fournit une expérience naturelle intéressante de croissance exogène des salaires. Rappelons que la mise en place progressive de la loi sur la réduction du temps de travail entre 1998 et 2003 s'est traduite par la coexistence de plusieurs niveaux de rémunération minimale : ils étaient six en 2003. Des Garanties Mensuelles de Rémunérations (GMR) étaient en effet destinées à maintenir la rémunération des salariés les moins payés. La loi Fillon de 2003 a mis fin à cette situation. Pour harmoniser les différents niveaux de salaire minimum, des coups de pouce différentiels ont été appliqués entre 2003 et 2005. Durant cette période, la règle de revalorisation habituelle sur la progression du salaire ouvrier de base a été gelée. Ces augmentations différenciées fournissent une méthode d'identification des effets du SMIC intéressante, puisqu'il est peu probable que le rattrapage décidé en 2005 ait été anticipé par les entreprises.

Notre stratégie d'estimation se rapproche donc d'une méthode de différence-de-différences avec intensité de traitement différenciée. En terme pratique, il s'agit d'estimer l'impact du salaire minimum sur l'ensemble de la distribution, et non plus seulement sur les seules moyennes. Nous nous appuyons donc sur la méthode proposée par Firpo, Fortin & Lemieux (2009), qui permet d'estimer l'impact du salaire minimum sur l'ensemble de la distribution des salaires. Cette méthode présente plusieurs intérêts par rapport aux méthodes de régression de quantiles classiques. Tout d'abord, elle permet d'étudier l'impact d'une variable explicative sur la distribution marginale d'intérêt, et non sur la distribution *c*onditionnelle aux autres variables observables que l'on est tenté d'introduire dans l'estimation (âge, qualification ou sexe par exemple). L'interprétation est donc plus directe : il est possible de s'interroger directement sur l'impact d'une augmentation marginale du salaire minimum sur l'ensemble de la distribution, les autres caractéristiques de la population restant constante. Elle ne se limite par ailleurs pas à l'analyse des quantiles, et peut être étendue à n'importe quelle statistique de la distribution : en pratique, on regarde ici l'évolution sur les déciles ainsi que sur les écarts inter-déciles. Enfin, les méthodes d'estimation, qui s'appuient sur les fonctions d'influence, sont très simples à mettre en œuvre et moins coûteuses en temps.

On utilise les données provenant du panel DADS au 1/25^{e}, disponible entre 1976 et 2007, en se concentrant sur la période 2002-2005, correspondant à la convergence des SMIC suite à la mise en place de la RTT. L'échantillon est restreint aux salariés âgés de 18 à 65 ans, à temps complet, ayant travaillé toute l'année (c'est-à-dire présents dans une entreprise pour une période au moins égale à 360 jours), et présents dans la même entreprise l'année précédente. Le champ est limité aux entreprises du secteur privé (à l'exclusion des entreprises détenues à majorité par l'État. Au final, le panel comprend de plus de 100 000 2002 et 2005) et près de 260 000 salariés, pour lesquels on dispose de la rémunération annuelle totale.

Les estimations sont conduites séparément pour les hommes et pour les femmes, sur les seuils des déciles des logarithmes des salaires annuels. Plusieurs variables explicatives sont ajoutées pour tenir compte des effets de composition éventuels : outre les caractéristiques individuelles des salariés (âge, catégorie sociale de l'emploi exercé) et des entreprises (taille, secteur d'activité), des effets fixes pour les différents groupes de GMR sont censés capter des différences systématiques dans les pratiques salariales des entreprises. L'impact du SMIC est donc identifié par le fait que l'on utilise plusieurs périodes, et que le niveau des GMR a évolué à des rythmes différents. Dans le cas contraire, il serait difficile de séparer ce qui relève des effets des différences de niveau de rémunération minimale de ce qui relève des pratiques salariales différentes des groupes d'entreprises.

Le SMIC a un effet significatif sur la distribution du logarithme des salaires des hommes jusqu'au septième décile. En revanche, l'impact est négligeable pour les salaires des femmes. L'effet ne serait pas significatif dans les entreprises de moins de cinquante salariés.

Chapitre 2

The Environmental Effect of Green Taxation : the Case of the French "Bonus/Malus"[1]

2.1 Introduction

Public awareness on environmental issues have raised in the past decade and global warming is now a growing concern for rich and emerging nations. Policy initiatives are launched in many countries to reduce the human contribution to greenhouse gas emissions, especially carbon dioxide (CO_2). Cutting automobile emissions is a crucial objective, as the transportation sector accounts for a large share of the CO_2 emissions (one third in France, and 28% of the US emissions according to the U.S. Environmental Protection Agency, 2006), and this share keeps on growing.

To reduce emissions stemming from transportation, a first solution, adopted by European northern countries, is to implement Pigovian taxes.[2] Such taxes have however proved very unpopular, as the recent failure of the French "taxe carbone" shows.[3] A second solution, implemented recently in California, British Columbia

1. This chapter is co-authored with Xavier D'Haultfœuille (INSEE-CREST) and Xavier Boutin (European Commission).

2. For a comprehensive survey of environmental taxation, see for instance Fullerton & West (2002) or Fullerton et al. (2008).

3. A tax of 17 euros per ton of CO_2 was adopted by the French Parliament in December 2009. Because of its unpopularity (including in the French government party), the French government finally decided to withdraw it in March 2010.

and the European Union, is to impose low carbon fuel standards. Such norms, however, may be prove inefficient (Holland et al., 2009 show they may even lead to an increase of net carbon emissions, by stimulating the production of low carbon fuel). Finally, feebates have recently received attention as an alternative way of internalizing pollution externalities. Their principle is to provide a rebate (resp. a fee) for purchasers of low-emitting (resp. high-emitting) new cars. Up to now, feebates have been implemented in Austria, France and Wallonia (a Belgium region), and are debated in other European countries (see Adamou et al., 2010). [4] In France, this feebate, called the "Bonus/Malus écologique", emerged as one of the main propositions of an environmental roundtable which took place by the end of 2007. It was implemented quickly after, at the beginning of 2008.

The main objective of such policies is to modify consumers' preferences in favor of greener cars. In the French case, this objective seems to have been achieved in a short period of time, as the share of cars whose emissions are below 120g of CO_2 per kilometer has doubled within a few months. In the long-run, feebates may also induce manufacturers to foster innovation in favor of less emitting cars. It is worth emphasizing, however, that several adverse effects can hamper the impact of the changes in the demand induced by the feebate policy on CO_2 emissions. Feebates are indeed only an indirect taxation of CO_2 emissions. The well-known "rebound effect" states that with more more fuel efficient vehicles, drivers may be induced to travel more (and thus emit more CO_2). Another issue is that the rebates were so important in the French case that the system increased notably total sales. [5] This scale effect translates into CO_2 emissions through traveling emissions but also the manufacturing of new vehicles. The manufacturing of a new car generates indeed 1.5 tons of CO_2 per ton of new vehicle (see ADEME, 2010). [6] At the end, the effect

4. Even if feebates have not been adopted yet in other European countries, most of them (17 in April 2010) have implemented a taxation which is more or less related to the average CO_2 emissions of the vehicles (for more details, see for instance the ACEA site). California also proposed in 2007 a feebate system called the "Clean Car Discount" program on new cars, but it was suppressed in 2008.

5. A crucial parameter of a feebate system is the "pivot point" that divides vehicles charging fees from those receiving rebates, and the rate that specifies the fee or rebate as a function of distance from the pivot point (see Greene et al., 2005). In the French case, these parameters were calibrated in order to make the system neutral for the State budget. But ex post the measure turned out to cost 285 millions euros in 2008.

6. The emissions stemming from scrapping of old vehicles may also be important. We do not

of the policy on CO_2 emissions may thus be negative.[7]

In this paper, we estimate the impact of the French feebate on CO_2 emissions stemming from individuals, taking most of these effects into account.[8] In order to recover the counterfactual emissions that would have prevailed in the absence of the feebate, we propose a simple demand model that combines car and annual mileage choices. This model has the advantage of both accounting for the differentiation of the automobile market and the existence of rebound effects, while remaining very simple to estimate. Besides, it allows us to estimate separately mileage and vehicle choice parameters. Our empirical analysis is indeed based on two datasets. The first is the exhaustive monthly dataset of car registration in France, which provides detailed information on both new vehicles and cars' owners. The second is a Transportation survey conducted in 2007 which records in particular annual mileages on a large sample of French households. These two datasets allow us to recover both choices with and without the feebate system, and average emissions related to car use for a particular choice of car. As a result, we can recover the total CO_2 emissions under the feebate policy and without it. Another option, chosen e.g. by Feng et al. (2005), relies on the model of Dubin & McFadden (1984) that mixes choices between several discrete options (the vehicle model) and a continuous variable (the mileage). However it would require to observe in one single dataset cars choices and mileages, whereas the Transportation Survey only records mileages for the year 2007, not precise car choices.

Thanks to our detailed monthly dataset, we estimate the model using sales just before and after the reform took place. As the reform was announced only by the end of October 2007, manufacturers were indeed not able to modify immediately their vehicles characteristics, apart from prices. We estimate a reduced form that

include them in our analysis because of data unavailability.

7. For a discussion of the optimal design of a feebate system, see for instance Peters et al. (2008) or Greene et al. (2005).

8. We restrict ourselves to private owners, as we do not have information on the mileage of company cars. These vehicles were already subject to a specific tax in favor of the less polluting cars and have probably less reacted to the introduction of the feebate. We do not analyze either the effect of the policy on manufacturers due to a lack of accurate data. Indeed do not have data on automobile industry, and observe sales until January 2009, i.e. only 13 months after the introduction of the feebate. This appears too short for estimating supply-side effects that are more likely to arise in the long-run.

combines the demand model relating market shares with characteristics and a price model. An advantage of this approach over the traditional separate estimation of demand and supply is that we do not need to observe real transaction prices. The validity of price models hinges indeed on a correct observation of real prices. However, as usually when studying the car industry, we observe list prices rather than transaction prices. Whereas it is likely that the policy has resulted in a quick adjustment of transaction prices, we do not observe such changes in the list prices. The difference between transaction prices and list prices is thus correlated with the feebate, making this measurement error problematic. Our approach does not suffer from this endogeneity limitation.

We show that if the magnitude of the shift towards the classes benefiting from a rebate is very important, the environmental short-run impact of the feebate is actually negative. This stems from three main effects. First, the policy has significantly enhanced total sales, resulting in an increase both in manufacturing and car use emissions. This stresses the need for a careful design of the feebate policy, in order not to generate such negative effects from the demand sides. The rebound effect also reduces part of the gain due to the shift in consumers' purchase towards less emitting vehicles. Finally, this disappointing result is also due to threshold effects : buyers shift their purchase to cars benefiting from rebates but with hardly lower emissions. This overall negative assessment of the feebate policy is robust to various assumptions. In particular, it still holds even if we neglect rebound effects.

We also perform an analysis of the long-run impact of the policy (still ignoring the supply side impact). In the short run, the demand shift due to the feebate indeed corresponds to a very small part of the whole fleet of cars. It is thus important to estimate what would happen with the replacement of the whole fleet. Computing such a long-run impact is however delicate. A crucial issue is the potential impact of the policy on the replacement rate of cars : as emphasized for instance by Adda & Cooper (2000), such replacement effects may indeed be large. In the absence of accurate data on car replacement, we consider two scenarii. In the first, we neglect the impact of the policy on the replacement rate. In the second, we consider a simple dynamic model with competitive prices in the second-market, following Engers et al. (2009). In this case, the change in replacement rates is related to changes in initial prices. Both scenarii lead to a large negative impact of the policy. Once more, this mainly results from the fact that the feebate leads to an increase in automobile equipment, inducing more car use emissions. This effect widely overcomes the composition effect stemming from changes in car choices. However, the estimation of long-run effects appears more sensitive to some assumptions. In particular, under alternative very favorable assumptions, we cannot reject the hypothesis that the policy is neutral in the long-run.

The paper is organized as follows. The next section presents the reform and the datasets at our disposal. The third part presents the parameters of interest and our identification strategy. Finally, the fourth part displays our results.

2.2 First insights on the policy

2.2.1 The feebate system

The feebate system on new cars sales was introduced by the French government in December 2007. The purchasers of new cars emitting less than 130g of CO_2 per kilometer benefit from a direct price cut on their invoice. The amount of the rebate varied, depending on the class of the vehicle (see Table 2.1) with a maximum of 1,000 euros, and even 5,000 euros for electric cars, which represents however a negligible share of the market. Conversely, purchasers of cars emitting more than 160g of CO_2 per kilometer had to pay a tax of up to 2,600 euros. The system was neutral for cars emitting between 130 and 160 g per kilometer. [9] In practice, rebates applied to new cars ordered on or after 5 December 2007, while fees applied to vehicles first registered in France on or after 1 January 2008. At the same moment, the government introduced a scrapping subsidy of 300 euros (called the "super bonus") for more than 15 year-old automobiles, provided that the purchaser bought a new vehicle emitting less than 160g of CO_2. Given the age condition, this additional rebate was however very seldom used, as it was provided in only 3% of the purchases of vehicles benefiting from a rebate. [10]

9. The classification corresponds to the one defined by the European Union for the cars energy labels, except that the government split the A, C and E classes into two subclasses.

10. This scrapping subsidy was extended to 1,000 euros and to cars between 10 and 14 years in 2009, in order to dampen the economic consequences of the 2009 crisis on car industry. We shall not be concerned with this hereafter as we focus on 2008 only.

TABLE 2.1 – Amount of the feebate as a function of CO_2 emissions

Class	CO_2 Emissions (g/km)	Rebate	Average Price (2007)	Market shares (2007)
A+	$\leq$60	5,000	-	-
A-	61-100	1,000	12.500	0.0%
B	101-120	700	15.500	18.4%
C+	121-130	200	19.000	10.2%
C-	131-140	0	19.000	18.8%
D	141-160	0	23.000	26.6%
E+	161-165	-200	23.500	3.2%
E-	166-200	-750	29.000	15.9%
F	201-250	-1,600	40.000	5.0%
G	$\geq$251	-2,600	60.500	1.9%

Note : we observe no sales for class A+ in 2007.

It is worth emphasizing that the feebate policy was decided and then implemented with unusual speed. It resulted indeed from a national environmental roundtable organized in Autumn 2007 by the newly elected president, whose aim was to define the key points of government policy on ecological and sustainable development issues for the coming five years.[11] The concrete measures, including the feebate system, were presented on 25 October 2007, for an almost immediate application. This roundtable came as quite a surprise as the past right-wing government used not to be preoccupied with environmental issues.

This green taxation for the purchase of new cars by private owners has no precedent in France, at least in magnitude and scope. Some measures already intended to increase the population's awareness of the environmental costs of motor vehicles. But for private users, such measures focused only on very specific and marginal segments of the market, by providing income tax reduction to the purchasers of hybrid vehicles for instance, or were larger in scope but marginal in magnitude, by imposing only very slightly the most polluting vehicles (around 100 euros for cars costing on average 35,000 euros). In contrast, the feebate introduced at the end of 2007 applied to all cars, the rebate representing up to 8.8% of the list price of the

11. This roundtable was called "Grenelle de l'Environnement" as an evocation of the "Accord de Grenelle" concluded in May 68, see http ://www.legrenelle-environnement.fr/spip.php ?rubrique112.

corresponding cars, while the penalty could be as large as 14.1% of this price.

The objective of the feebate system was twofold. First, it intended to shift consumers' demand towards low CO_2-emitting cars. Secondly, it aimed at encouraging manufacturers to develop greener vehicles. To better achieve this second purpose, it was mentioned from the beginning of the reform that the thresholds of eligibility for the rebates and imposition of the fees were to be lowered, at a pace allowing manufacturers to adapt their production (5g of CO_2/km every two years). As a result, the thresholds were decreased in January 2010. Less expectedly, the amount of the rebates were also moved at the same date (from 1,000, 700 and 200 euros to 700, 500 and 100 euros, respectively), in order to make the feebate system cost-neutral. Indeed, and although the initial values of the rebates and fees were already decided according to this criterion, the system turned out to cost around 285 million euros to the state in 2008 because of its overwhelming success in favor of low CO_2-emitting cars.

2.2.2 Demand reaction

We use the exhaustive dataset on the registration of new cars from January 2003 to January 2009 provided by the Association of French Automobile Manufacturers (CCFA, *Comité des Constructeurs Français d'Automobiles*). It includes all the information that is necessary for the registration of a new car, i.e. some characteristics of the car (brand, model, CO_2 emissions, list prices, type of fuel, number of doors, type of car-body, horsepower, weight and cylinder capacity) as well as a few information on the owner (professional activity, age and the city he lives in). These informations allow us to define our products at a rather detailed level (see the appendix for more details).

As the implementation of the measure was almost immediate, neither consumers nor manufacturers could anticipate the reform before November 2007. Figure 2.1 shows that anticipation was spectacular on consumer's side in December 2007, especially for the most polluting cars. Not surprisingly, this large increase for the last classes was followed by an "undershooting" in January and, to a lesser extent, in February. As we do not seek to measure anticipations or undershooting effects, we will exclude subsequently December 2007 as well as January and February 2008 from our analysis. We do not observe any noticeable change in November even though the reform was already announced then. This is probably due to the delivery time of new cars, as well as the shift between the purchase and registration of a new car. Similarly, we observe a jump in the sales of the less polluting sales in January only, even though the measure was already in force for vehicles ordered after the 5th of December. This stems from the fact that owners of cars bought in December had to register it after the 1st of January 2008 to receive the rebate.

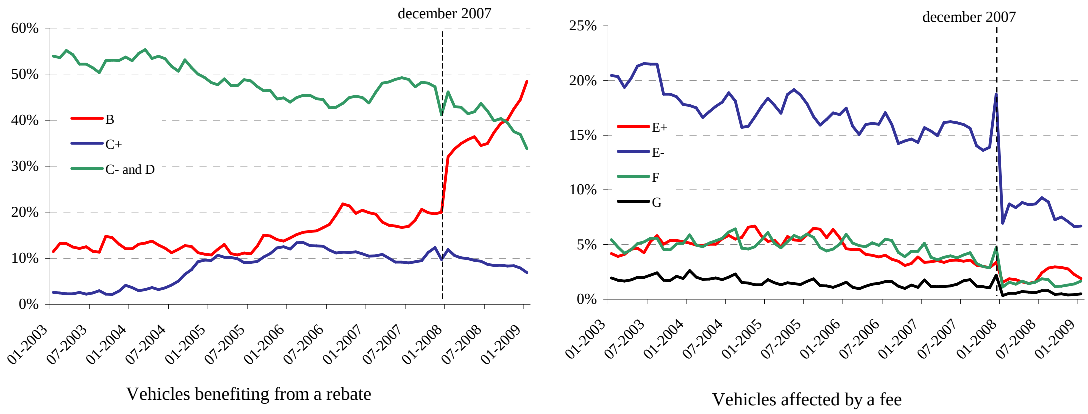


FIGURE 2.1 – Evolution of the market shares of the different classes of CO_2.

Figure 2.1 also highlights the importance of the changes in the market shares of the classes of energy after the reform took place. While class B only represented 20% of sales at the end of 2007, its market shares reached nearly 50% at the beginning of 2009. In the same time, market shares of class E- fell from nearly 15% to 5%. These variations are all the more striking that the feebate only represents a modest fraction of the list prices, around 4.7% for class B and 2.6% for class E-. It thus seems difficult to rationalize these changes by pure price effects only. [12]

These changes induce a significant impact on average emissions (see Figure 2.2). This effect is however much smaller than the one observed on market shares. Indeed, compared to the trend between November 2005 and November 2007, the average decrease between March 2008 and January 2009 only reaches 5%. [13] This mainly results from threshold effects : many buyers have probably only marginally

12. D'Haultfœuille et al. (2010) show indeed that the feebate has shifted the individual preferences towards lower CO_2-emitting vehicles.

13. We restrict ourselves to the period after November 2005 as the CO_2 emissions label became compulsory at this date. There is an acceleration in the decrease of average CO_2 emissions after this period (see D'Haultfœuille et al., 2010).

modified their purchasing decisions, choosing for instance a car emitting 120 g/km (thus belonging to class B) instead of one emitting 121 or 122g/km (belonging to class C+). This fact is confirmed by the density of average emissions of new cars bought just before and just after the reform (see Figure 2.3) : the shifts have mainly been towards the most polluting models of the lower classes. We also see that these threshold effects already existed before the introduction of the feebate. This may be due to the fact that consumers value the energy class *per se*. Since May 2006, manufacturers have to display the European Union energy labels indicating the energy class of their new cars, so that these classes were known by the consumers in 2007. It may also stem from the pre-existing taxation of company cars, already based on these classes since 2006. Car manufacturers thus already had the possibility to adapt their products to this classification.

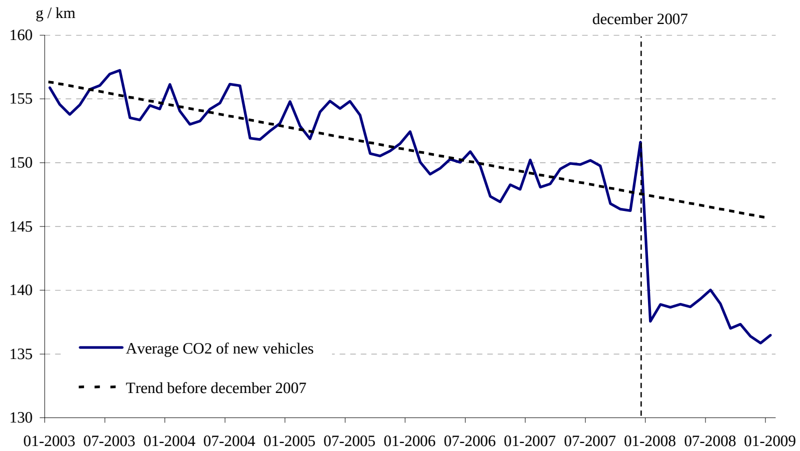


FIGURE 2.2 – Average CO_2 emissions of new cars.

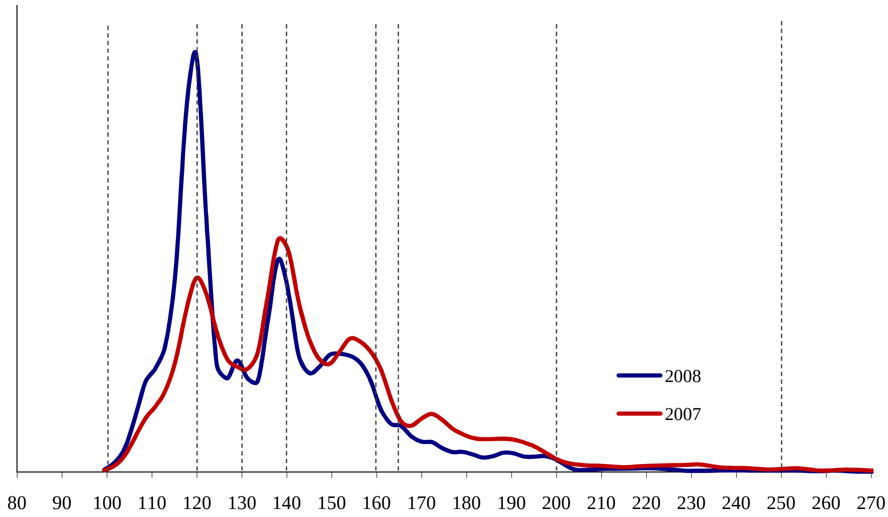


FIGURE 2.3 – Distribution of CO_2 emissions of new cars sold in 2007 and 2008.

The gains in the average emissions of new cars is an interesting insight on the effect of the policy, but does not translate directly into gains in CO_2 emissions. A first reason is that it does not incorporate the emissions due to the production of new cars. A second reason is that there is a large heterogeneity in the yearly mileage of drivers, which is also related to the characteristics of the car. This heterogeneity is likely to affect the final impact of the policy. In the following, we take this dependence into account using the Transportation Survey conducted by INSEE in 2007. This survey provides detailed information about individuals traveling (in particular the annual mileage of their car) and on the characteristics of their vehicles, such as their type of fuel, their fiscal power and age. Table 2.2 summarizes the average number of kilometers covered by the cars depending on its characteristics and those of the owners. Results confirm the importance of taking the heterogeneity in the yearly mileage into account. Drivers who have just bought a new car, or those who choose a diesel one, make much more kilometers per year than the others. People with high income, who work and live in rural areas also use theirs cars more intensively.

TABLE 2.2 – Yearly mileage in kilometers as a function of owner's or car's characteristics.

Variable	Yearly mileage (kms)
Age of the car	
Less than one year	17,456
Between 1 and 3 years	16,724
Between 3 and 5 years	14,824
Between 5 and 10 years	12,917
More than 10 years	9,475
Type of fuel	
Gasoline	10,114
Diesel	17,193
Household income	
First quintile	11,585
Second quintile	12,368
Third quintile	13,720
Fourth quintile	15,138
Fifth quintile	15,428
Type of Area	
Rural and suburban	15,108
Urban	13,024
Activity	
working	15,886
non working	10,584

The introduction of the feebate seems also to have had a positive effect on total sales. A simple comparison of the quarters that we use subsequently, namely the one from September to November 2007 and the one from March to May 2008, shows that total sales increase by around 13.4%. [14] This increase is spectacular as this period corresponded also to the beginning of the recession and an important increase of fuel price, two events which were likely to reduce the total sales of new vehicles.

14. The conclusion is similar (13.8%) if we consider the quarter from March to May 2007 instead of the one from September to November 2007.

2.2.3 Supply reactions

Apart from the effects on the demand side, an explicit goal of the reform was also to stimulate the reduction of CO_2 in a second round, by triggering innovation by manufacturers to produce lower CO_2-emitting cars. We expect that in the short-run, manufacturers were not able to modify these emissions. To check this, we looked at the evolution of average emissions of cars that are sold each month, without weighting each product by their sales to eliminate demand effects. [15] Figure 2.4 shows an acceleration of technical changes around the beginning of 2007. This may be due to the fact that European Union energy labels became compulsory in May 2006. On the other hand, we do not observe any shock in 2008. As the policy was announced very lately, manufacturers did not have time before January 2008 to adjust their production to the reform. Even if it is technically possible to modify horsepower (and thus CO_2 emissions) quickly, the vehicle with its new characteristics must be certified before being distributed. This process typically takes several months. Second, as the measure is only conducted at the French level, global manufacturers are less induced to propose specific models for this limited market. A rough quantitative analysis of the number of patents on the corresponding domains [16] does not show any particular acceleration during this period. This result is also consistent with the one of Pakes et al. (1993), who observed a two-year shift between the increase in the fuel price following the first oil crisis and the corresponding technical innovations.

15. We suppose that a new car is sold a given month if we observe at least one sale before or at the given month and one sale after or at the given month. To avoid boundary effects (at the beginning or end of the period, only vehicles with enough sales are included, and these vehicles tend to have lower CO_2 emissions), we drop the first and last six months.

16. These domains are F02B, F02D et F02M for fuel engine, B60L for electric ones.

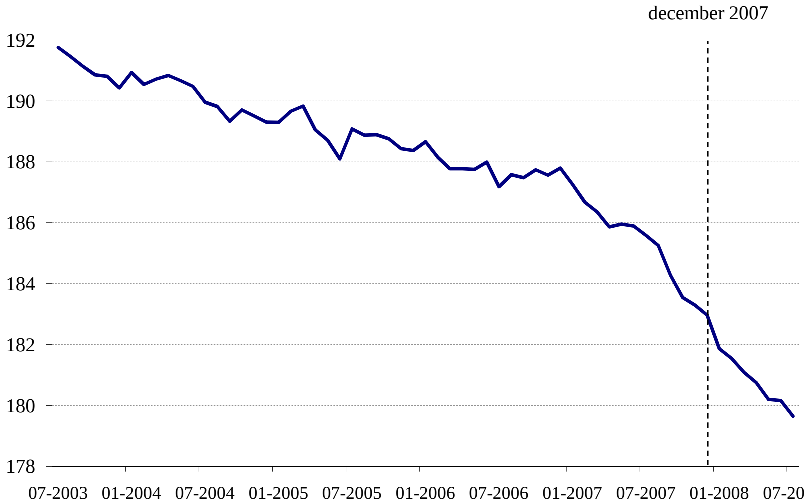


FIGURE 2.4 – Evolution of average CO_2 emissions before and after the reform.

Apart from CO_2 emissions, we should expect manufacturers to compensate the effects of the measure by increasing prices of the cars benefiting from rebates while decreasing those with fees, to soften the shifts in demand resulting from the policy. However, we do not observe systematic differences between classes of emissions in the evolution of list prices at the period of the reform (see Table 2.3). A natural explanation is that list prices are typically modified only once a year, so that between March and May 2008, many list prices were not adjusted yet to the reform. To explore further this issue, we also computed the evolution during the year 2008. We observe a rise in prices of B and C+ classes vehicles, but no sharp decrease for high-emitting vehicles. Thus, it seems that either standard predictions of the theory are wrong or list prices, as proxies of transaction prices, are unreliable (or both). This is the reason why we do not rely on list prices subsequently, and prefer to use a reduced form approach rather than modeling both the demand and supply. [17]

17. It is possible to consistently estimate a demand model with measurement error in prices because prices are instrumented anyway. Yet, consistent estimation of supply models requires to observe prices without errors.

Table 2.3 – Evolution of average prices (in %) before and after the reform.

Class of CO_2	2003	2004	2005	2006	2007	2008
B	0.32	-1.16	1.92	1.03	-0.22	1.60
C+	-1.36	2.01	2.79	-0.28	0.71	1.81
C- & D	0.76	0.88	1.78	1.39	-0.01	0.77
E+	0.55	0.16	0.37	0.44	0.74	0.54
E-	0.75	0.99	0.04	0.49	0.75	0.98
F	0.62	-0.14	0.48	-0.71	0.85	1.36
G	0.51	-0.82	0.69	-0.66	0.61	0.07
Inflation	*0.53*	*2.16*	*1.11*	*0.91*	*1.09*	*0.83*

Reading notes : For year $t = 2003$ to 2007, changes in prices are computed between September to November of year t and March to May of year $t+1$. For year 2008, changes in prices are computed between March to May 2008 and September to November 2008. Results for class A are not reported due to the few number of sales until 2007.

2.3 Methodology

2.3.1 Parameters of interest

Before presenting our identification strategy, we define here the parameters of interest. We both consider the short-run and long-run effects of the measure on CO_2 emissions. The first corresponds to emissions between March and May 2008, while the second corresponds to quarterly emissions in a long-run scenario defined below. The latter is probably the most relevant parameter, since in the short-run the policy only affects new cars, which represent less than 1% of the whole stock of cars. In the long-run, with the progressive replacement of the whole stock, the policy is expected to produce more effects. On the other hand, the short-run impact can be identified under weaker conditions.

CO_2 emissions depend on the emissions per kilometer of cars chosen by the consumers, but also on mileages (see also Feng et al., 2005). Let $d \in \{0, 1\}$ denote the policy status ($d = 1$ if the feebate is introduced, $d = 0$ otherwise) and let $t \in \{t_0, t_1\}$ denote the period of consideration. Hereafter t_0 refers to the quarter from September to November 2007, while t_1 corresponds to the quarter from March to May 2008. Let $Y_{it}(d) \in \{0, ..., J\}$ denote the new car chosen by individual i at date t with policy status d. As usual, choice 0 is the outside option, which represents either the non-replacement of an old car by a new one (or its replacement by a second-hand car), or the use of an alternative mean of transportation.

Total CO_2 emissions depend on both average emissions per kilometer of cars and annual mileages. For $j \in \{1, ...\}$, let $A_j(d)$ denote vehicle j average CO_2 emissions per kilometer for a use corresponding to 50% of high road and 50% of urban area. When $j = 0$, average emissions depend on the individual i and possibly on time t, and we denote them by $A_{i0t}(d)$. $N_{ijt}(d)$ is the mileage done by i with vehicle j during quarter t. Finally, we take into account emissions stemming from the manufacturing of new cars, and let M_j denote the emissions of producing car j (so that by definition, $M_0 = 0$). The emissions of household i during quarter t, with policy status d satisfy

$$\mathrm{CO}_{2it}(d) \quad = \quad \mathbb{1}\{Y_{it}(d) = 0\} A_{i0t}(d) N_{i0t}(d) + \sum_{j=1}^{J} \mathbb{1}\{Y_{it}(d) = j\}(M_j + A_j(d) N_{ijt}(d)).$$

Then the short-run average effect of the policy on total carbon dioxide emissions satisfies

$$\Delta^{SR} = nE\left(\mathrm{CO}_{2it_1}(1) - \mathrm{CO}_{2it_1}(0)\right),$$

where n is the number of potential buyers. To take into account the heterogeneity among households in both the purchase of cars and the annual mileage, we separate households according to some observable characteristics X_{it}, namely activity, geographical area and income.[18] Letting $X_{it} \in \{1, ..., K\}$, we then have $\Delta^{SR} = \sum_{x=1}^{K} \Pr(X_{it_1} = x)\Delta_x^{SR}$, with

$$\begin{aligned} \Delta_x^{SR} \quad = \quad & n\Bigg[s_{x0t_1}(1)\overline{E}_{x0t_1}(1) - s_{x0t_1}(0)\overline{E}_{x0t_1}(0) + \sum_{j=1}^{J}(s_{xjt_1}(1) - s_{xjt_1}(0))M_j \\ & + \sum_{j=1}^{J}(s_{xjt_1}(1)A_j(1)\overline{N}_{xjt_1}(1) - s_{xjt_1}(0)A_j(0)\overline{N}_{xjt_1}(0))\Bigg], \end{aligned} \qquad (2.3.1)$$

where, for $d \in \{0, 1\}$, we let $s_{xjt}(d) = P(Y_{it}(d) = j | X_{it} = x)$, $\overline{E}_{x0t}(d) = E(A_{i0t}(d) N_{i0t}(d) | Y_{it}(d) = 0, X_{it} = x)$ and $\overline{N}_{xjt}(d) = E(N_{ijt}(d) | Y_{it}(d) = j, X_{it} = x)$.

To understand better what are the effects at stake, let us decompose this expression. We denote by $\overline{A}$, $\overline{N}_{xt}$ and $\overline{M}$ respectively the average emission of the new

18. For more details on these variables, see Appendix A.2.

cars, the average mileage done by individuals with characteristics x using new cars at t and the average production emissions of these new cars. For any counterfactual variables $(U(0), U(1))$, we denote the impact of the policy on this variable by $\Delta U = U(1) - U(0)$. Rewriting (2.3.1), we obtain

$$\begin{aligned}
\Delta_x^{SR} \quad = \quad n\Big[& \underbrace{\sum_{j=1}^{J} \Delta s_{xjt_1}((A_j(1) - \overline{A})N_{xjt_1} + M_j - \overline{M})}_{\text{Composition effect}} + \underbrace{\overline{A}\sum_{j=1}^{J} \Delta s_{xjt_1}(\overline{N}_{xjt_1} - \overline{N}_{xt_1})}_{\text{Rebound effect}} \\
& \underbrace{+(\overline{A}\,\overline{N}_{xt_1} - \overline{E}_{x0t_1}(1))\sum_{j=1}^{J} \Delta s_{xjt_1}}_{\text{Traveling scale effect}} + \underbrace{\overline{M}\sum_{j=1}^{J} \Delta s_{xjt_1}}_{\text{Manufacturing scale effect}} \\
& \underbrace{+s_{x0t_1}(0)\Delta\overline{E}_{x0t_1} - \sum_{j=1}^{J} s_{xjt_1}(0)\Delta(A_j\overline{N}_{xjt_1})}_{\text{Second-order effect}}\Big].
\end{aligned}$$

The first component corresponds to the change in the composition of the fleet in favor of less CO_2-emitting cars. If the policy is well-design, it should be negative (thus contributing to a decrease in the overall level of CO_2 emissions). We indeed expect that the market sales of the most polluting cars, i.e. those with $A_j - \overline{A} > 0$, decreases (i.e $\Delta s_{xjt_1}<0$) and conversely for the less polluting ones. These less polluting cars are also smaller in average, so we expect that $\Delta s_{xjt_1}(M_j - \overline{M}) < 0$. However, three effects may mitigate this impact. The feebate scheme is designed on (easily observed) emissions per mileage A_j, but the result also depends on the final use of cars $(\overline{N}_{xjt_1})$. Because of the rebound effect, one could expect that for a given individual, the mileage increases as A_j decreases, as it results in a fall in the cost per kilometer. So it is likely that $N_{xjt_1} - \overline{N}_x > 0$ for the less polluting cars. Besides, the decomposition makes clear that the policy impact will depend on a scale effect : the term in factor of $\sum_{j=1}^{J} \Delta s_{xjt_1}$. If total sales increase because of the policy, the production of these new cars and the corresponding traveling emissions will lead to a rise in CO_2 emissions. This is partly offset by the fact that these new cars in excess will be used instead of older ones (the term $-\overline{E}_{x0t_1}\sum_{j=1}^{J} \Delta s_{xjt_1}$), and older cars are the higher emitting ones. Finally, the fifth component in the decomposition corresponds to second-order effects. The first term in it corresponds to the change in outside emissions due to the policy. This effect is small in the short run because the composition of the whole stock of cars is hardly affected by the reform after just a few months. The second term corresponds to changes in average emissions of an individual with car j due to the policy. Such a change may be due to a supply side effect ($\Delta A_j < 0$ if manufacturers react to the policy) and a selection effect (individuals who choose vehicle j differ with the policy status, so that $\Delta\overline{N}_{xjt_1}$ may change). We however expect the former to be negligible in the short-run, and the latter to be small once controlled for observed heterogeneity X.

Let us now turn to long-run effects. Noteworthy, due to limitations in our data, we still abstract from supply side effects here. More generally, we assume that the automobiles supplied in the long-run are those already proposed at the beginning of 2008. We also suppose that the sales of new cars and annual mileages remain constant each quarter after the beginning of 2008. [19] Hence, the only difference with the short-run is that in this long-run scenario, the whole fleet of cars has been replaced. Let $\widetilde{s}_{xj}(d)$ denote the share of households of type x equipped with model j with policy status d in this long-run scenario.

Under these assumptions, long-run average effects for group x on quarterly emissions satisfy

$$\Delta_x^{LR} = n\sum_{j=1}^{J}(s_{xj}(1) - s_{xj}(0))M_j + (\widetilde{s}_{xj}(1)A_j(1)\overline{N}_{xjt_1}(1) - \widetilde{s}_{xj}(0)A_j(0)\overline{N}_{xjt_1}(0)).$$

Implicit in this expression is the fact that we neglect emissions stemming from other means of transportation here. [20] Indeed, we do not have precise information on them in the Transporation Survey. To assess the sensitivity of our results to this assumption, we however consider alternative assumptions in Subsection 2.4.3.

For the sake of the interpretation we propose the same type of decomposition as above. First, it is useful to remark than in a steady-state equilibrium we have the following relationship between the share of the car j in the whole fleet and its

19. Thus, we abstract from potential transitory effects in sales. Sales just after the reform do not correspond to sales a few months later. If the policy affects the optimal replacement of cars (as explained below), there is a decrease in the optimal lifetime of smaller cars and an increase in the optimal lifetime of bigger ones, so that many households find it optimal to replace their (small) car at the beginning of the period, while a large part of households with bigger cars postpone their decision to replace their car. Note that as our estimation period started two months after the policy took place, most of these adjustments should have already done. This assumption is supported by the fact that the evolution of the average level of CO_2 emissions does not exhibit strong changes during the first year of the reform after these two months (see Figure 2.2).

20. Examples include buses, or individuals using vehicles they do not own.

share in the flow of new cars :

$$\widetilde{s}_{xj}(d) = T_{xj}(d)s_{xj}(d), \tag{2.3.2}$$

where $T_{xj}(d)$ is the average lifetime of vehicle j when bought by individuals of type x under policy status d. We first consider a scenario where the cars' lifetime is the same for all cars, and not affected by the reform (i.e. $T_j = \overline{T}$ constant). In this case, we obtain :

$$\begin{aligned}
\Delta_x^{LR_1} \;=\; n\Bigg[& \underbrace{\sum_{j=1}^{J} \Delta s_{xj} \left[\overline{T}(A_j(1) - \overline{A})\overline{N}_{xj}(1) + M_j - \overline{M}\right]}_{\text{Composition effect}} \\
& \underbrace{+\overline{T}\,\overline{A}\sum_{j=1}^{J} \Delta s_{xj} \left[(\overline{N}_{xj}(1) - \overline{N}_x)\right]}_{\text{Rebound effect}} + \underbrace{\overline{T}\,\overline{A}\,\overline{N}_x \sum_{j=1}^{J} \Delta s_{xj}}_{\text{Traveling scale effect}} + \underbrace{\overline{M}\sum_{j=1}^{J} \Delta s_{xjt_1}}_{\text{Manufacturing scale effect}} \\
& \underbrace{+\overline{T}\sum_{j=1}^{J} s_{x0}(0)\Delta(A_j\overline{N}_{xj})}_{\text{Second-order effect}}\Bigg].
\end{aligned}$$

The change in emissions due to the production of new cars over a quarter is the same as in the short-run, but the magnitude of the positive and rebound effects in the long-run is multiplied by $\overline{T}$ (around 80 quarters in our sample). The traveling scale effect is multiplied by an even larger factor, as it is not mitigated anymore by the fact that in the short run, new cars substitute to older (and thus more polluting) ones.

This scenario suffers however from the fact that the price differential due to the feebate is likely to modify renewal choices. On a related issue, Adda & Cooper (2000) for instance present evidence that changes in the scrapping value have significant impact on cars lifetime. We expect that vehicles with a fee are kept on a longer period than those benefiting from a rebate, so that their share in the whole fleet is larger than their shares in total sales, partially offseting the impact of the policy (as $\Delta s_{xj}\Delta T_{xj} < 0$). On the other hand, larger average lifetimes means that the increase in total sales due to the policy does not increase that much the share of individuals owning a car, mitigating the traveling scale effects. This replacement rate effect is thus potentially ambiguous. To assess its importance, we consider the following decomposition (based on $\Delta\widetilde{s}_{xj} = \Delta T_{xj}s_{xj}(1) + T_{xj}(0)\Delta s_{xj})$:

$$\Delta_x^{LR_2} = \Delta_x^{LR_1} + \underbrace{n\sum_{j=1}^{J} s_{xj}(1)\Delta T_{xj}A_j(1)\overline{N}_{xj}(1)}_{\text{Replacement rate effect}}.$$

2.3.2 The model

The short and long-run effects of the policy depend on several variables that are not observed directly in our dataset : the counterfactual variables $Y_{it_1}(0)$, $A_j(0)$ and $N_{ijt_1}(0)$, but also mileages $N_{ijt_1}(1)$ (we only observe, in the Transportation Survey, annual mileages in 2007). Several assumptions are thus needed to identify these effects. To this end, we develop a theoretical model that links car choice and mileage and takes into account consumers heterogeneity. Let us consider an individual i of type x at a date t and policy status d. We suppose that his indirect utility of choosing vehicle j and anticipating to travel N kilometers a year satisfies

$$U_{it}(N,j,d) = N^{\frac{\gamma_x}{\gamma_x-1}}\alpha_x + \left(y_{it}(d) - p_{jt}(d) - \frac{c_{ijt}(d)N}{r_x}\right)\beta_x + df_{1x}(Z_{jt}) + \xi_{xjt}(d) + e_{ijt}(d),$$

where y_{it} is the income, $p_{jt}(d)$ corresponds to the transaction price of vehicle j at date t with policy status d (including the feebate if $d=1$), r_x is the discount rate, Z_j is the fee of vehicle j under the feebate policy and $\xi_{xjt}(d)$ represents other observable and unobservable characteristics of the vehicle.[21] $c_{ijt}(d)$ (resp. $c_{i0t}(d)$) is the cost per kilometer of vehicle j (resp. for an individual i choosing the outside option).[22] We suppose that $0 < \gamma_x < 1$ and $\alpha_x < 0$, so that the utilities are increasing, concave functions of N. The dependence in x of $(\beta_x, \gamma_x, r_x, \delta_x, \xi_{xjt}(d))$ reflects the observed heterogeneity in the way people value the characteristics. Noteworthy, this specification allows for the possibility of a direct effect of feebates on demand (through $f_{1x}(Z_{jt})$), apart from their indirect effect through price variations. There is indeed evidence that the reform has had an impact on the environmental awareness of consumers beyond prices effects (see D'Haultfœuille et al., 2010). On the other hand, we suppose that $f_{1x}(0) = 0$, and impose that the policy has not affected fuel prices and automobiles characteristics other than prices.

21. We have $p_{0t}(d) = 0$, $Z_0 = 0$ and $Z_j < 0$ if the vehicle actually benefits from a rebate.
22. Note that when $j > 0$, $c_{ijt}(d)$ does not depend on i.

Assumption 1 *(Limited effect of the feebate) For all x, i, j, t, ξ_{xjt}, A_j and c_{ijt} do not depend on d.*

Assumption 1 implies in particular that manufacturers did not modify immediately average emissions of their vehicles because of the reform. As already discussed in Subsection 2.3, this condition is likely to hold here. We implicitly assumed that the fuel price is unaffected by the introduction of the feebate, as the cost per kilometer of vehicles does not change with the policy.

In each state t, the individual is supposed to maximize his utility both in N and j. For a fixed j, the optimal anticipated mileage of i, $N^*_{ijt}(d)$ satisfies

$$N^*_{ijt}(d) = \left(\frac{\beta_x(\gamma_x - 1)c_{ijt}(d)}{r_x \alpha_x \gamma_x}\right)^{\gamma_x - 1}. \tag{2.3.3}$$

This relationship is at the basis of the so-called rebound effect, since it indicates that individuals will increase their mileage following a reduction of the cost per kilometer of their car. Plugging N^*_{ijt} into the utility, we get a utility of choosing j equal to

$$U_{it}(j,d) \quad = \quad \left(y_{it}(d) - p_{jt}(d)\right)\beta_x - c^{\gamma_x}_{ijt}(d)\mu_x + f_{1x}(Z_{jt})\delta_x + \xi_{xjt} + e_{ijt}(d),$$

where $\mu_x = \frac{\alpha_x}{\gamma_x - 1}\left(\frac{\beta_x(\gamma_x-1)}{r_x\gamma_x\alpha_x}\right)^{\gamma_x}$ and $\widetilde{e}_{i0t}(d) = c^{\gamma_x}_{i0t}(d)\mu_x + e_{i0t}(d)$. We impose a structure on residuals that corresponds to the nested logit model, with two nests (corresponding to the decision of buying or not the car). More precisely, we suppose that $\widetilde{e}_{i0t}(d), e_{i1t}(d), ..., e_{ijt}(d)$ are identically distributed and follow a Gompertz distribution. $\widetilde{e}_{i0t}(d)$ is independent of $(e_{i1t}(d), ..., e_{ijt}(d))$ but those latter are correlated through a common factor $\zeta_{it}(d)$:

$$e_{ijt}(d) = \sigma_x \zeta_{it}(d) + (1 - \sigma_x)\eta_{ijt}(d),$$

where the $(\eta_{ijt}(d))_{j=1...J}$ are independently distributed according to a Gompertz distribution and independent of $\zeta_{it}(d)$.[23] Under these conditions, we get

$$\begin{aligned}\ln(s_{xjt}(d)) \quad &= \quad \frac{1}{1-\sigma_x}\left[\ln(s_{x0t}(d)) - \sigma_x \ln(1 - s_{x0t}(d)) - \xi_{x0t} - p_{jt}(d)\beta_x \right.\\ &\qquad \left. - c^{\gamma_x}_{jt}(d)\mu_x + f_{1x}(Z_{jt})d + \xi_{xjt}\right], \end{aligned} \tag{2.3.4}$$

23. Thus, the distribution of $\zeta_{it}(d)$ is implicitly defined by those of $e_{ijt}(d)$ and $\eta_{ijt}(d)$ and this independence restriction. Cardell (1997, Theorem 2.1) shows that there exists a unique distribution satisfying these conditions, for each value of $\sigma_x \in [0, 1]$.

where, as previously, $s_{xjt}(d) = P(Y_{it}(d) = j | X_i = x)$. Finally, we posit the following relationship between $p_{jt}(0)$ and $p_{jt}(1)$.

Assumption 2 *(Dependence of transaction prices on the feebate scheme)*

$$p_{jt}(1) = p_{jt}(0) + f_2(Z_{jt}) + f_3(\widetilde{Z}_{jt}),$$

where $\widetilde{Z}_{jt}$ is the sum of fees of vehicles produced by the firm that produces j and $f_2(0) = f_3(0) = 0$.

This assumption should be seen as a flexible approximation of a price model. We include $\widetilde{Z}_{jt}$ because when fixing price of j so as to maximize its profit, the firm should take into account its effect on the profit stemming from j but also from the other cars it produces.

2.3.3 Identification strategy

Short-run effect

To identify the short-run effects, we have to recover average quarterly mileages $\overline{N}_{xjt_1}(d)$, counterfactual market shares $s_{xjt_1}(0)$ and outside emissions $\overline{E}_{x0t_1}(d)$. First, let us consider average quarterly mileages. We rely on Equation (2.3.3), and on the following relationship between anticipated and actual mileages, which are denoted $N_{it}(d)$ hereafter.

Assumption 3 *(Link between anticipated and actual mileages) We have*

$$\ln N_{it}(d) = \ln N^*_{iY_{it}(d)t}(d) + \delta_x + \nu_{it}(d),$$

where ζ_x is a x-specific constant, $\nu_{it}(d)$ is independent of $(Y_{it}(d), A_{i0t}(d), c_{it}(d))$ conditional X_i, $E(\nu_{it}(d)|X_i = x) = 0$ and the distribution of $\nu_{it}(d)$ does not depend on t and policy status d.

This assumption allows for both biased and unbiased anticipations, the latter holding when $E(\exp(\zeta_x + \nu_{it}(d))|N^*_{iY_{it}(d)t}) = 1$. The important points are rather that the distribution of the error term does not depend on t and d, and is independent of car choices and of cost per kilometer. By Equation (2.3.3) and Assumption 3, we have

$$\ln N_{it_0}(d) = \widetilde{\delta}_x + \ln c_{it_0}(d)(\gamma_x - 1) + \nu_{it_0}(d). \tag{2.3.5}$$

where $\widetilde{\delta}_x$ is a x-specific constant. Under Assumption 3, $E(\nu_{it_0}(d)|\ln c_{i20007}(d), X_i) = 0$. This means that we can estimate by OLS $\widetilde{\delta}_x$, γ_x and the distribution of residuals $\nu_{it_0}(0)$ using the Transportation Survey. Then we can recover $\overline{N}_{xjt_1}(d)$ using (see Appendix A.3 for the proof)

$$\overline{N}_{xjt_1}(1) = \overline{N}_{xjt_1}(0) = E\left(\exp(\nu_{it_0}(0))|X_i = x\right)\exp(\widetilde{\delta}_x)c_{jt_1}^{(\gamma_x-1)}(1). \tag{2.3.6}$$

We now turn to the identification of emissions for individuals choosing the outside options. Recall that this outside option corresponds to individual who do not buy a new car at period t. Emissions for them depend on the share of individuals who do not have a car, and on the distribution of average emissions on the stock of existing cars. In the long-run, both are likely to be affected by the feebate policy. On the other hand, it seems very plausible that they do not depend on the policy status in the short-run. We state this formally in Assumption 4. We let hereafter $F_{i0t}(d)$ denote the type of fuel of the car owned by an individual when choosing the outside option ($F_{i0t}(d) = 2$ for a gasoline car, 1 for a diesel one and 0 if the individual does not have a car).

Assumption 4 *(No short-run effect of the policy on existing cars) For all i, the distribution of $(A_{i0t}(d), F_{i0t}(d))$ conditional on $Y_{i0t}(d) = 0$ does not depend on t and d.*

Under Assumptions 3-4, average outside emissions at period t_1 are identified by (see Appendix A.3 for the proof)

$$\overline{E}_{x0t_1}(1) = \overline{E}_{x0t_1}(0) = I_1^{\gamma_x-1}P(F_{i0t_0}(0) = 1)\overline{E}_{x0t_0,1}(0) + I_2^{\gamma_x-1}P(F_{i0t_0}(0) = 2)\overline{E}_{x0t_0,2}(0), \tag{2.3.7}$$

where I_f is the ratio between fuel price of type $f \in \{1, 2\}$ at period t_1 and at period t_0, and $\overline{E}_{x0t_0,f}(0)$ are the average outside emissions for individuals such that $F_{i0t_0}(d) = f$:

$$\overline{E}_{x0t_0,f}(0) = E(A_{i0t}(d)N_{i0t}(d)|Y_{it}(d) = 0, X_{it} = x, F_{i0t_0}(d) = f).$$

Finally, we have to recover counterfactual market shares $s_{xjt_1}(0)$. For that purpose, we differentiate Equation (2.3.4) over time (to get rid of any fixed effect) and replace prices by their expression given in Assumption 2. Using such a reduced form is not problematic, since our final aim is to obtain counterfactual market shares, not price-elasticities or parameters from the price model. Besides, an advantage of this approach over the traditional separate estimation of demand and supply is that we need not observe the real transaction prices, which are necessary for a correct estimation of the price model. As emphasized in Subsection 2.3, it is likely that the measurement error resulting from the use of list prices is correlated with the feebate. Our approach does not suffer from this endogeneity issue. We rely instead on the following exogeneity condition.

Assumption 5 *(Exogenous residuals in market shares and no systematic trend in the short-run)* $E(\varepsilon_{xj}|Z_j, \widetilde{Z}_j, c_{jt_1}, c_{jt_0}) = 0$, *where* $\varepsilon_{xj} = \xi_{x0t_1} - \xi_{x0t_0} + \xi_{xjt_1} - \xi_{xjt_0} + p_{jt_1}(0) - p_{jt_0}(0)$.

This condition is satisfied if the evolution of the price of the unobserved characteristics of vehicle j are unrelated to its feebate and to its cost per kilometer. As detailed in Subsection 2.4.3, we can partially test for this assumption by checking that variations in market shares between two years preceding the feebate introduction do not depend on classes of emissions.

Note that $\ln(s_{x0t}(d))$ is very small compared to $\ln(1 - s_{x0t}(d))$ (around -0.006, compared to -5.1), so we neglect it in (2.3.4). For the sake of simplicity we assume a linear dependency of the coefficient $\sigma_x/(1-\sigma_x) = x'\lambda$ in the individual characteristics x. Under these conditions, we obtain

$$\ln(s_{xjt_1}(1)/s_{xjt_0}(0)) = \ln\left(\frac{1 - s_{x0t_0}(0)}{1 - s_{x0t_1}(1)}\right) x'\lambda + f_{4x}(Z_j) + f_{5x}(\widetilde{Z}_j) - \left(c_{jt_1}^{\gamma_x}(1) - c_{jt_0}^{\gamma_x}(0)\right)\widetilde{\mu}_x + \varepsilon_{xj}, \tag{2.3.8}$$

where $f_{4x}(z) = (f_{1x}(z) - f_2(z)\beta_x)/(1-\sigma_x)$, $f_{5x}(z) = -f_2(z)\beta_x/(1-\sigma_x)$ and $\widetilde{\mu}_x = \mu_x/(1-\sigma_x)$. We then plug γ_x in Equation (2.3.8), and can identify λ, f_{4x}, f_{5x} and $\widetilde{\mu}_x$ by simple OLS. [24] Then, using Equation (2.3.4) and Assumption 1, we can recover the counterfactual market shares for individuals of type x by

$$s_{xjt_1}(0) = \frac{s_{xjt_1}(1)e^{-f_{4x}(Z_j) - f_{5x}(\widetilde{Z}_j)}}{\frac{s_{x0t_1}(1)}{(1-s_{x0t_1}(1))^{\sigma_x}}\left[\sum_{k=1}^J s_{xjt_1}(1)e^{-f_{4x}(Z_j) - f_{5x}(\widetilde{Z}_j)}\right]^{\sigma_x} + \sum_{k=1}^J s_{xjt_1}(1)e^{-f_{4x}(Z_j) - f_{5x}(\widetilde{Z}_j)}}.$$

Long-run effect

The identification of the long-run effect of the policy requires stronger restrictions. As explained above, it depends on the long-run shares of individuals equipped with

24. λ is indeed identified as $f_{4x}(0) = f_{5x}(0) = 0$.

model j with policy status $d \in \{0,1\}$, namely $\widetilde{s}_{xjt_1}(d)$. By Equation (2.3.2), this depends in turn on $T_{xjt_1(d)}$, the average lifetime of vehicle j when bought with individuals of type x at t_1 under policy status d.

Unfortunately, as far as we know, no French data provide recent information on cars lifetimes. In particular, the data used by Adda & Cooper (2000) no longer exist. As a result, we have to make quite restrictive assumptions. The first is that we posit a constant average lifetime across vehicles before the introduction of the feebate, $T_{xjt_0}(d) = \overline{T}_{xt_0}$. In this case $\widetilde{s}_{xjt_0} = T_{xt_0} s_{xjt_0}$ for all $j \geq 0$, so that by summing over j, we have

$$\overline{T}_{xt_0} = \frac{1 - \widetilde{s}_{0t_0}}{1 - s_{0t_0}},$$

and we can recover $\overline{T}_{xt_0}$ using the Transportation Survey. Our computation gives us an average value of around 80 quarters, consistent with the official statistics (the monthly flow of new cars represent 0.5% of the stock of cars less than 15-year old, lefting us with an estimated value of 67 quarters).[25]

To identify lifetimes at t_1, we first consider, as stated before, a scenario where the policy does not modify replacement rates.

Assumption 6 *(Constant average lifetime before the policy and in the absence of the policy) For all $j \in \{1, ..., J\}$ and $t \in \{t_0, t_1\}$, $T_{xjt}(0) = T_{xt}(0)$.*

This scenario assumes that the increase in sales observed in the short-run leads to a proportional overall increase in the equipment rate of the French population. If extreme, this assumption is not unrealistic. If the share of the French households with at least one car is rather stable in recent years (slightly above 80%), the multiequipment rates has doubled in twenty years : 35.8% of French households has at least two cars in 2008 while this rate was only 16.5% in 1980, and we can imagine that policies such as the feebate may still increase this multiequipment rate.

We also consider a scenario where lifetimes adjust to the policy. We consider in appendix a simple model inspired by Engers et al. (2009), which leads to the following approximation.

25. Official statistics are available for cars less than 15-year old only, and are not restricted to cars owned by households, both leading probably to a negative bias of the true lifetime we aim to estimate.

Assumption 7 *(Impact of the policy on cars lifetime before scrapping) for all* $j \geq 0$,

$$T_{jt_1}(1) - T_{jt_1}(0) \simeq \frac{\frac{1}{\beta^{T_{jt_0}(0)-1}} - 1}{\ln(\beta)} \frac{p_{jt_1}(0) - p_{jt_1}(1)}{p_{jt_0}(0)}.$$

This adjustment depends on the quarterly discount factor β of individuals, and sale prices $p_{jt}(d)$. In practice, we set $\beta = 0.987$, corresponding to an annual interest rate of 5%.

2.4 Results

2.4.1 Mileage and outside option emissions

Before estimating the demand and price models, we use the Insee Transportation Survey to estimate Equation (2.3.5) and the two expectations in Equation (2.3.7). We introduce the age of the car in the specification as it could change the cost per kilometer. We suppose that γ_x is constant in x (we introduced interaction terms in the regression but they were not significant at the 5% level) and that $\widetilde{\zeta}_x$ depends linearly on the dummies of activity, geographical area and income. Results are displayed on Table 2.4.

Table 2.4 – Estimates of the mileage model.

Variables	Estimate
Intercept	12.16^{***} (0.185)
Non working	-0.276^{***} (0.015)
Rural and suburban area	0.006 (0.014)
Income in 2nd quintile	0.064^{**} (0.026)
Income in 3rd quintile	0.129^{***} (0.024)
Income in 4th quintile	0.177^{***} (0.023)
Income in 5th quintile	0.217^{***} (0.023)
Age of the car (in months)	-0.0044^{***} (0.0001)
Cost per kilometer	-0.392^{***} (0.027)

Reading note : Mileages are computed on the whole 2007 year. Significance levels : *** 1%, ** 5%, * 10%.

We thus obtain $\widehat{\gamma} - 1 \simeq -0.39$. To compare this estimate with previous results, mostly based on variations in fuel price (and either with macro or micro data), recall that the actual cost per kilometer satisfies $c_{Y_{it}t} = f_{Y_{it}t} A_{Y_{it}t}$, where f_{jt} is the fuel price for vehicle j at date t. A change in the fuel price induces both a modification of $c_{Y_{it}t}$ and $A_{Y_{it}t}$, because the individual may change his car according to fuel price fluctuations. Thus, by Equation (2.3.5), and letting ε_N (resp. ε_T) denote the price elasticity of mileage (resp. of average emissions per kilometer), we get

$$\gamma - 1 = \frac{\varepsilon_N}{1 + \varepsilon_A}.$$

We expect $\gamma - 1$ to be smaller in absolute value than the the price elasticity of fuel consumption, which is equal to $\varepsilon_N + \varepsilon_A$. We indeed find that our estimate is smaller than usual estimates of the long-run elasticities, which usually lie between -0.8 and -0.6 (see, e.g., Graham & Glaister, 2002 for a survey). [26] Interestingly, it is also close to the estimates given by Johansson & Schipper (1997), who separately

26. These estimates are usually obtained on macro data. Noteworthy, our result is also smaller in absolute values than the price elasticity obtained on micro data by Clerc & Marcus (2009) in France, namely -0.70.

estimate ε_N and ε_A on 12 OECD countries and obtain for France $\varepsilon_N = -0.33$ and $\varepsilon_A = -0.38$.

2.4.2 Effect on CO_2 emissions

In order to evaluate the impact of the measure on CO_2 emissions, we first estimate Equation (2.3.8). We adopt a flexible form and use indicators for each classes. Results are presented in Table 2.5. As expected, market shares of vehicles benefiting of a bonus increase at the expense of those affected by a penalty. The penalty effect appears however strongly concave, as the negative impact of the malus is not significantly different for almost all classes penalized by a fee. Finally, as expected the estimated coefficient of the cost per kilometer is significant and negative (-2.10).

Table 2.5 – Estimates of the reduced form of the demand model.

Parameter	Estimate
Substituability terms (λ)	
Intercept	1.946^{***} (0.257)
Non working	0.017 (0.133)
Rural and suburban area	0.323^{**} (0.13)
Income in 2nd quintile	0.094 (0.215)
Income in 3rd quintile	-0.128 (0.207)
Income in 4th quintile	-0.035 (0.209)
Income in 5th quintile	0.409^{*} (0.212)
Other terms	
Cost per kilometer	-2.104^{***} (0.08)
Rebate $= 1,000$ €	0.3481^{*} (0.2087)
Rebate $= 700$ €	0.6809^{***} (0.0293)
Rebate $= 200$ €	-0.0021 (0.0288)
Fee $= 200$ €	-0.2489^{***} (0.0375)
Fee $= 750$ €	-0.2657^{***} (0.0222)
Fee $= 1,600$ €	-0.1136^{***} (0.0331)
Fee $= 2,600$ €	-0.0872^{*} (0.0495)
Sum of fees of the firm	0.0026^{***} (0.0004)

Reading notes : OLS estimation of the coefficients of Equation (2.3.8). Significance levels : *** 1%, ** 5%, * 10%.

We check informally the quality of our demand and price models by comparing the market shares observed in 2007 with those predicted by our model in absence of the feebate policy. The model reproduces quite accurately the market shares observed in 2007 (see Table 2.6). We observe slight differences for some classes, as the model indicates that the share of classes C+ and D would also have increased, absent the reform. On the other hand, the share of the most polluting cars would have decreased. Such predictions are consistent with the sharp increase in fuel price observed at the beginning of 2008 (the gasoline price was for instance 15% higher than in September-November 2007). Overall, the average gain in terms of CO_2 emissions of new vehicles is equal to 3.9%, which perfectly matches the observed gain on our subsample. Another important indicator to look at is the prediction of the model on global sales. According to our estimates, the policy has increased sales by 13.0%. This effect is substantial but consistent with the increase in sales of 13.4% observed between September-November 2007 and March-May 2008. It will prove to have large consequences on the effect of the policy on total emissions.

TABLE 2.6 – Comparison between the observed market shares and those predicted by the model.

Class	Observed in 2007	Prediction (without bonus)
A	0.02%	0.03%
B	21.56%	21.64%
C-	11.39%	11.78%
C+ et D	48.84%	50.81%
E-	2.61%	1.99%
E+	12.87%	11.75%
F	1.98%	1.51%
G	0.72%	0.50%
Total	*100.00%*	*100.00%*

Reading notes : the market shares do not include the outside option and thus sum to 100%.

The overall effects of the policy, both in the short and long-run, are displayed in Table 2.7, while the decomposition of these effects are presented in Table 2.8. Emissions stemming from the manufacturing of new cars were computed by assuming that the production of a new car generates 1.5 tons of CO_2 per ton of new vehicle, following the carbon assessment of the French agency for environment (see ADEME, 2010).

In the short-run, the composition effect of the change in the composition of the new cars' sales reaches approximately -30.3 kilotons of quarterly CO_2 emissions, well above in absolute value the rebound and traveling scale effect. Hence, the measure would be positive if we ignored manufacturing effects. However, compared to the previous effects, the latter are large in the short-run, representing around 62.3 kilotons of quarterly CO_2 emissions. At the end, we estimate a negative and significant short-run effect of around 47.2 kilotons. With a conventional cost of the ton of CO_2 of 32 euros consistent with Yohe et al. (2007) meta-analysis, we evaluate the overall environmental short-term cost of the measure at 1.5 millions of euros per quarter.

TABLE 2.7 – Short and long-run effect of the feebates policy.

	Estimates		
Parameter	Kilotons	Million of euros	% of total emissions
Short-run effect Δ^{SR}	47.2^{***} (14.7)	1.5^{***} (0.5)	$0.2\%^{***}$ (0.1%)
Long-run effect Δ_1^{LR}	$1,574.6^{***}$ (451.7)	50.4^{***} (14.5)	$11.7\%^{***}$ (3.6%)
Long-run effect Δ_2^{LR}	$1,355.8^{***}$ (443.8)	43.4^{***} (14.2)	$10.1\%^{***}$ (3.5%)

Note : we consider a price of 32 euros for a ton of CO_2. Standard errors were computed by bootstrap (with 1,000 simulations). Significance levels : *** 1%, ** 5%, * 10%.

TABLE 2.8 – Decomposition of the short and long-run effects.

	Estimates (kilotons)	
Parameter	Short-run	Long-run
Composition effect	-30.3^{***} (5.82)	-962^{***} (190.7)
Rebound effect	5.34^{***} (1.27)	434.9^{***} (103.4)
Traveling scale effect	9.90^{***} (2.43)	$2,039^{***}$ (498.1)
Manufacturing scale effect	62.3^{***} (15.1)	62.3^{***} (15.1)
Replacement rate effect		-219 (167.4)

Note : we consider a price of 32 euros for a ton of CO_2. Standard errors were computed by bootstrap (with 1,000 simulations). Significance levels : *** 1%, ** 5%, * 10%.

As expected, we obtain far higher effects in the long-run. When ignoring the potential impact of the feebate on the cars' lifetime, the impact on quarterly emissions is higher by a factor 30. The multiplicative factor is smaller in magnitude than the average lifetime (80 quarters), in particular because each quarter the same amount of new cars is produced. While in the short-run, the main component of the negative impact is manufacturing emissions, traveling emissions predominates in the long-run. As a result, we estimate that the introduction of the feebate accounts for an increase of 11.7% in total automobile emissions. It is worth emphasizing once more, however, that this ratio corrresponds to a long-run situation where the set of proposed vehicles is the same as the one at the beginning of 2008. The negative effect could in particular be partially offset by supply-side reactions to the feebate.

When taking into account changes in cars' lifetime, we obtain more positive but very similar results. We estimate a negative replacement rate, meaning that the induced traveling scale effect (less individuals own a car under this scenario than without adjustment of cars lifetime) dominates the composition effect (low CO_2-emitting cars represent a lower share of the whole fleet in this scenario) here. This estimated impact is however small (93 kilotons) and non significant. All in all, the long-run effect remains large and significant, around 1356 kilotons per quarter, still representing around 10% of total emissions.

2.4.3 Robustness checks

Several tests are performed to assess the robustness of our results. The first is related to the rebound effect. This effect is commonly evoked in cases where the sole energetic efficiency is affected (for instance in the case of a choice of a heating system). However, the car choice may also affect mileage through other channels than the cost per kilometer. One could imagine that the rebound effect may be partially offset by a comfort effect : people choosing smaller vehicles may decrease their mileage because these vehicles are less comfortable. To assess the importance of the rebound effect in our final results, we fix γ to 1 and estimate the demand model (2.3.8) and total emissions under this constraint. Results, displayed in Table 2.9, show that the short and long-run effects remain negative, though they are closer to zero, as expected.

Another important assumption is related to the average emissions for people who do not own a car. Up to now, and in the absence of detailed data on this issue, we set these emissions to zero, which may bias negatively our estimates of the true effects. These emissions include those from alternative transportation such as buses, or the use of others' cars. We reestimate the model trying to take into account the latter possibility. More specifically, we share equally car emissions between all potential "drivers" inside each household. For instance, in a household with one car and two people owning a driving license, we attribute half of the car emissions to the car owner, and half to the other driver.[27] Hence, we make the somewhat extreme assumption that the other driver actually uses the automobile as much as the owner. This assumptions does not lead to conclude to a reduction of the CO_2 emissions because of the feebates policy (the estimated effect is 43 kilotons in the short-run, around 350 kilotons in the long-run). The same diagnosis persists when combined with the no rebound effect assumption (see Table 2.9).

TABLE 2.9 – Robustness checks : Short and long-run effects on quarterly emissions under alternative assumptions.

Alternative Assumptions	Estimates (in kilotons) Δ^{SR}	Δ_1^{LR}	Δ_2^{LR}
Baseline	47.2*** (15.3)	1574.6*** (470.3)	1355.8*** (457.5)
No rebound effect	41.4*** (14.3)	1204.3*** (404.1)	1047.9*** (398.9)
$\overline{E}_0 \neq 0$	42.7*** (12.9)	354.7*** (116.8)	311.6*** (112.6)
No r.e., $\overline{E}_0 \neq 0$	39.7*** (13.3)	180.8** (87.2)	168.9* (87.8)
2006-2007	−3.5 (11.7)	−129.9 (309.6)	249.7 (1672)

Note : standard errors were computed by bootstrap with 1,000 simulations. Significance levels : *** 1%, ** 5%, * 10%.

Finally, underlying our identification strategy is the idea that feebates Z_j in Equation (2.3.4) only captures changes in prices (and possibly a direct effect of the feebates themselves) following the introduction of the policy. We rule out the possibility that Z_j captures for instance changes on unobservable characteristics of the cars. If this were the case, we would wrongly attribute the observed changes to the impact of the feebate system. Of course, we cannot directly test this assumption, but we perform hereafter a falsification test, using the 2006-2007 period instead of

27. In households without any car, we still assume that they generate no CO_2 emissions.

2007-2008. More specifically, we make as if the measure had been adopted in 2007 instead of 2008, falsely attributing the corresponding feebates to cars in 2007. If our assumption is true over the all period, the coefficients corresponding to the emissions classes should be equal to zero. Table 2.10 shows that their estimates are far smaller than those obtained for 2007-2008, even if several remain significant. [28] For instance the parameter corresponding to the class B is more than 7 times smaller than when comparing 2007 to 2008. Next, computing the short and long-run placebo estimates, we obtain estimates not significantly different from zero (the point estimates are respectively -3,5 kilotons and -130 kilotons i.e. around 12-13 times lower than our estimates on 2007-2008). Overall, this test suggests that the possible bias underlying our identification strategy is limited and does not question our final results.

TABLE 2.10 – Estimates of the demand model on 2006-2007.

Parameter	Estimate
Rebate = 1,000 €	*not identifiable*
Rebate = 700 €	-0.09^{***} (0.027)
Rebate = 200 €	-0.155^{***} (0.025)
Fee = 200 €	0.084^{***} (0.031)
Fee = 750 €	0.074^{***} (0.019)
Fee = 1,600 €	0.036 (0.024)
Fee = 2,600 €	0.09^{**} (0.041)
Sum of fees of the firm	0.00002 (0.0003)

Reading notes : OLS estimates of the coefficients on Z_j and $\widetilde{Z}_j$ in Equation (2.3.8) on 2006-2007. Significance levels : *** 1%, ** 5%, * 10%.

28. This may be due to long-run evolutions in preferences for low emitting cars among French consumers. See D'Haultfœuille et al. (2010) for a detailed analysis on this issue.

2.5 Conclusion

Overall, the impact of the policy is much disappointing. If this kind of evaluation necessarily relies on strong assumptions (especially for a long-run assessment), our global conclusion that the policy has negative effects on CO_2 emissions appears robust to several changes in the specifications. This does not invalidate, however, feebate systems as efficient tools for environmental policy. French consumers have indeed strongly reacted to financial incentives created by the policy. The problem rather comes from the design of this feebate. Our results underline that the first-order terms in the policy effect are manufacturing or traveling scale effects. The most important point, to ensure a positive environmental effect of such feebate policies, should thus to calibrate it in order to decrease or keep constant total sales.

One limitation of our study is that we do not include manufacturers reactions. Stimulating innovation in favor of less polluting cars was indeed one objective of the measure. However, it seems difficult to evaluate this effect with only data up to 13 months after the introduction of the policy and in the absence of innovation costs in cars' industry. We leave this stimulating issue for future research.

2.6 Appendix

2.6.1 Definition of products

As detailed above, we model the automobile market as a market of differentiated products, where potential buyers will have different valuation for cars given characteristics such as brand or type of fuel. In practice, we define a product by a set of characteristics. An important issue is then to choose which characteristics one should keep in this definition. On the one hand, if products are defined with few characteristics, very different items are mixed together, possibly leading to strong aggregation biases if the underlying model of demand is not linear, which is the case here. On the other hand, keeping too many characteristics leads to small market shares for each product, or even null markets shares as exactly similar cars are often not sold each month. The theoretical model presented before links the logarithm of the markets shares with the observed characteristics. Thus, null sales are not used, which leads to a selection bias.[29] As a compromise, we select the brand, the model, the type of fuel, the type of car-body (urban, station wagon, convertible, etc), the number of doors and its class of CO_2 emissions. This selection leads to define 950 different products (see Table 2.11) for the period between September and November 2007. Thus, we adopt a slightly more restrictive definition of a product than Berry et al. (1995). Even so, the dispersion of the remaining characteristics (such as price) within each product is not that small compared to the overall dispersion (see Table 2.12). A more restrictive definition of products (by including, e.g., horsepower) would reduce this dispersion but at the cost of increasing the proportion of null sales. Our definition allows us to keep this proportion of null sales relatively small on the whole population of buyers (15% of the models with positive sales between September and November 2007 have not been sold between March and May 2008).

TABLE 2.11 – Number of products and number of sales between September and November 2007

	Models	Number of sales
Overall	950	239,606
By number of doors		
3	182	42,704
5	499	168,949
Others	269	27,953
By type of car-body		
Station wagon	234	28,446
Convertible	83	6,611
Urban	626	204,538
Disabled	7	11
By type of fuel		
Gasoline	453	80,390
Diesel	497	159,216

29. The existence of null sales is a consequence of the finiteness of the French population, and does not invalidate the model. If the market share of a product is 10^{-9}, it is very unlikely that it is sold during a given quarter in France.

TABLE 2.12 – Dispersion of prices, CO_2 emissions and fiscal power of new cars registered between September and November 2007

	Overall	Within products
Price (euros)	9,107	1,169
CO_2 (g/km)	27.8	2.4
Fiscal power	2.4	0.5

2.6.2 Construction of drivers categories

As mentioned above, we observe in the registration dataset the age, activity and city of the owners. However, income, which is likely to drive an important part of the heterogeneity of preferences, is not available. In order to proxy this income, we impute to each purchaser the median income of his age class in his city, using fiscal data.[30] Using data from the French national institute of statistics (INSEE), we also included in our final dataset the type of area (urban versus rural or suburban) to which the purchaser belongs. Table 2.13 displays the average characteristics of new car purchasers in terms of age, income, rate of activity and type of location computed from the Transportation Survey. Not surprisingly, these individuals are on average older, belong to richer households and work more often than the rest of the population.

30. This information is only available for towns of at least 2,000 inhabitants. For cities of more than 50 households but less than 2,000 inhabitants, only the median income is known. In this case, or if the age of the buyer is unknown, we impute the median income of the city. Sales to individuals living in less than 500 inhabitants cities have been dropped, as the median income is missing in this case. Note that such sales only represent 5% of the data.

TABLE 2.13 – Comparative statistics between characteristics of the buyer of new cars and the overall French adult population

Variable	Buyers of new cars	Overall
Activity rate (%)	60.1	58.4
Age (years)	52.3	48.7
Rural and suburban area (%)	41.7	41.1
Median income of the household (%)		
First Quintile	10.6	41.1
Second Quintile	15.7	20.1
Third Quintile	24.1	21.7
Fourth Quintile	38.0	24.5
Fifth Quintile	52.3	48.7

To compute market shares, we also need to define potential markets. We suppose here that they correspond, for the subpopulation with characteristics x, to the number of individuals with a driving license at quarter t. We thus assume that individuals cannot purchase more than two cars during a quarter.

2.6.3 Computation of the mileage N_{t_0}

Average emissions of CO_2 vary from one vehicle to another but also according to the use of the vehicle. Emissions are indeed different in urban area and on highways. Let us denote respectively by A_j^1 and A_j^2 the corresponding average emissions for vehicle j. The total CO_2 emissions of an individual at t_0 is $N_{t_0}^1 A_{Y_{t_0}}^1 + N_{t_0}^2 A_{Y_{t_0}}^2$, where $N_{t_0}^1$ (resp $N_{t_0}^2$) corresponds to the mileage in urban area (resp. on high roads) in 2007. We only observe in the CCFA dataset the average emissions $A_j = (A_j^1 + T_j^2)/2$ corresponding to a 50% - 50% mixed use, which does not necessarily coincide with the real use of the vehicle. To obtain correct total emissions, we compute $N_{t_0}^*$, defined by

$$N_{t_0}^* \frac{A_{Y_{t_0}}^1 + A_{Y_{t_0}}^2}{2} = N_{t_0}^1 A_{Y_{t_0}}^1 + N_{t_0}^2 A_{Y_{t_0}}^2.$$

$N_{t_0}^*$ simply corresponds to a weighted average between the two mileages :

$$N_{t_0}^* = pN_{t_0}^1 + (1-p)N_{t_0}^2, \text{ where } p = \frac{2A_{Y_{t_0}}^1}{A_{Y_{t_0}}^1 + A_{Y_{t_0}}^2}.$$

Quantities A_j^1 and A_j^2 have been obtained on the ADEME website. Note that we do not observe directly $N_{t_0}^1$ and $N_{t_0}^2$ in the Transportation Survey. To compute them, we consider that 80% of "regular" travels (all travels except those made for professional purpose outside commuting, or for vacation) are made in urban areas for people living in a urban area, and on highways for people living in a rural or suburban area. We consider that other travels consist of 90 % of highways and 10 % of urban area. These assumptions allow us to compute $N_{t_0}^1$ and $N_{t_0}^2$ from the total mileage $N_{t_0}^1 + N_{t_0}^2$.

2.6.4 Proofs of Subsection 2.3.3

Equations (2.3.6) and (2.3.7)

Using notations of the model described in Section 2.3.3, let

$$g_x = E\left(\exp(\nu_{it})|X_i = x\right).$$

Note that by Assumption 3, g_x does not depend on t. Moreover, it is identified using the residuals of Equation (2.3.5). We then have

$$\begin{aligned}\overline{N}_{jt_1}(1) &= E\left[N_{it_1}(1)|Y_{ip} = j, X_i = x\right] \\ &= \exp(\widetilde{\zeta}_x)c_{jt_1}(1)^{\gamma_d - 1}E\left(\exp(\nu_{it_1}(1))|Y_{it_1}(1) = j, X_i = x\right) \\ &= g_x \exp(\widetilde{\zeta}_x)c_{jt_1}(1)^{\gamma_x - 1},\end{aligned}$$

where the third equality stems from Assumption 3. This proves (2.3.6), because under the previous conditions, $\overline{N}_{jt_1}(0) = \overline{N}_{jt_1}(1)$.

First, by the law of iterated expectations,

$$\overline{E}_{x0t_1}(0) = P(F_{i0t_0}(0) = 1)\overline{E}_{x0t_0,1}(0) + P(F_{i0t_0}(0) = 2)\overline{E}_{x0t_0,2}(0). \tag{2.6.1}$$

Second, by Equation (2.3.5) and Assumption 1, we have, for $f \in \{1, 2\}$,

$$\overline{E}_{x0t_1,f}(0) = I_f^{\gamma_x - 1}\overline{E}_{x0t_0,f}(0). \tag{2.6.2}$$

Third, by Assumptions 1 and 4, $E_{0t_1}(0) = E_{0t_1}(1)$. This, together with (2.6.1) and (2.6.2), proves Equation (2.3.6).

Effect of the feebate on cars lifetime

We rely on a similar model as Engers et al. (2009). In a dynamic setting, let us assume that at a quarter $t+k$ (the purchase of the car occuring at period t), a car can either be sold on the second market at price $\widetilde{p}_{jt+k}$ or kept, generating a current net surplus of f_{jt+k}. The value W_{jt+k} of a car j of age k then satisfies the simple relation :

$$W_{jt+k} = \max\{f_{jt+k} + \beta W_{jt+k+1}, \widetilde{p}_{jt+k}\},$$

where β denotes the quarterly discount factor. Supposing that prices perfectly adjust at equilibrium, we get

$$\widetilde{p}_{jt+k} = \max\{p_{jt+k+1}, s_j\},$$

where s_j represents the scrapping value of car j. As shown by Engers et al. (2009), the consumer keeps the car while its price remains above the scrapping value. Let us define by T_{jt} this final period. For the sake of simplicity, we assume that the current net surplus does not depend on time, so that $f_{jt+k} = f_j$. We then get the following system :

$$\begin{cases} \widetilde{p}_{jt+k} & = & f_j + \beta \widetilde{p}_{jt+k+1} & \text{if } 0 \leq k < T_{jt}, \\ \widetilde{p}_{jT_{jt}} & = & s_j & \text{if } k = T_{jt}. \end{cases}$$

After a little algreba,

$$T_{jt_1}(1) - T_{jt_1}(0) = \frac{\ln\left(\frac{f_j}{1-\beta} - p_{jt_1}(1)\right) - \ln\left(\frac{f_j}{1-\beta} - p_{jt_1}(0)\right)}{\ln(\beta)}.$$

Automobile lifetime variations thus depend on the unknown quantity $f_j/(1-\beta)$. To recover it, we use the model at $t = t_0$:

$$\frac{f_j}{1-\beta} = \frac{p_{jt_0}(0) - s_j \beta^{T_{jt_0}-1}}{1 - \beta^{T_{jt_0}-1}}.$$

As a result,

$$T_{jt_1}(1) - T_{jt_1}(0) = \frac{\ln\left(\frac{p_{jt_0}(0) - s_j \beta^{T_{jt_0}(0)-1} - (1-\beta^{T_{jt_0}(0)-1}) p_{jt_1}(1)}{p_{jt_0}(0) - s_j \beta^{T_{jt_0}(0)-1} - (1-\beta^{T_{jt_0}(0)-1}) p_{jt_1}(0)}\right)}{\ln(\beta)}.$$

A first order expansion of this expression finally yields

$$T_{jt_1}(1) - T_{jt_1}(0) \simeq \frac{\frac{1}{\beta^{T_{jt_0}(0)-1}} - 1}{\ln(\beta)} \frac{p_{jt_1}(0) - p_{jt_1}(1)}{p_{jt_0}(0)}.$$

In the right-hand side, we approximate the unobserved price $p_{jt_1}(0)$ by the observed (although probably with error) price $p_{jt_0}(0)$.

Chapitre 3

Are Entreprise Zones the remedy for the French "Banlieue" ?[1]

3.1 Introduction

A long-standing debate regarding appropriate remedy to the deteriorating conditions in some urban and/or rural areas, concentrating poverty and unemployment, is still unanswered. In France, sporadic outbursts of violence (the 2005 riots being the last example) has called for efficient policy to revitalize some of the suburban areas (the "Banlieue") characterized by a high concentration of social problems. Since the end of the nineties, France has thus experienced urban redevelopment policy, targeting specific economically distressed neighborhoods.[2] *Zones Franches Urbaines* (hereafter ZFU) are one of the main tools of this policy. They grant number of exemptions from tax and social security contributions to encourage businesses to relocate to (or to avoid leaving) these depressed areas. This system is expensive : loss of tax and social security contributions for the State is estimated at €70 million for 2006.

This paper evaluates the effects on local firms of the second wave of these ZFUs implemented in 2004. If the evaluation of local tax relief systems is the subject of relatively wide-ranging international literature, as far as we know very little empirical evidence exists for the French case, the notable (if not sole) exception

1. This chapter is co-authored with Roland Rathelot (CREST) and Patrick Sillard (INSEE).
2. A neighborhood represents here population 9,000 to 30,000.

being Gobillon et al. (2010*b*), that find very small impact of the first wave of the French Enterprise Zones on the unemployment of the local workforce. In the United States, "enterprise zones" and "empowerment zones" have been tested on a State scale, evaluated and extended to federal scale. Empirically evidence of the effectiveness of these zones as an economic development tool is rather mixed and inconclusive Fisher & Peters (2002). Most studies find no significant increase in employment, while a few do O'Keefe, Papke, Busso & Kline, 2004, 1994, 2008. Anyway, findings are hard to generalize because of the variety of EZ approaches. Recent papers have indeed showed strong heterogeneity in their effectiveness, depending on the design of the EZ Bondonio & Greenbaum (2007) and the way they are administrated Kolko & Neumark, 2010. Their impact also differ according to the kind of establishments. Bondonio & Greenbaum (2007) thus obtained countervailing effects on new firms and existing firms, resulting in no net change. In this respect, the French ZFU system is rather specific, as enterprises zones results on almost no taxation on small firms located in some narrow areas.

Our access to exhaustive administrative panel establishment-level data let us provide a comprehensive evaluation of the ZFU for the period 2002-2007. We could analyze the impact of the tax incentives on economic activities (measured by the number of businesses located in these areas), but also on businesses demography (creation and failure). Besides, we could analyze the evolution on employment, but also analyses whether alleviate financial burden on small businesses has an impact on their economic and financial situation (debt, account..) or in their investment level. Another decisive advantage of our database is that we have access to precise location of each establishment by GIS. Enterprise Zones have various outline, rarely squared with the pre-existing jurisdictions. Notwithstanding, the analyses done so far mostly use these jurisdictions as units of analysis for want of more accurate data. This compromises the precise evaluation of the zone, as one will mix establishments in the EZ with establishments located nearby but not benefiting of the tax incentives. Our data let us locate the real zoning of the tax incentives and thus provide a more accurate measure of the economic impact of the zone. Finally, we provide measures of the *indirect* impacts of the zone. The EZ could "cannibalize" neighboring communities, by causing a shift of economic activities within a city from areas outside the zones to areas within. It is obviously an important concern for public policy to know whether one development tool spurs the development of one area at the expense of another. GIS data let us measure the potential externalities on areas at the border of the EZ.

The empirical evaluation of the zone has to deal with the classic selection question. The Enterprise zones are often in blighted and economically depressed areas. Conversely, some areas designated as enterprise zones may be ripe for development. Enterprises would have located within the zone regardless of whether there were tax incentives or eased regulatory restrictions. The pervasive challenge for evaluation is to assess what would have happened but for the zone. Note however that in practice, the designation of the second wave of the French EZ does not really corresponds to the theoretic objective to target the most deprived areas. As politicians claimed for having an EZ in their electoral district, one objective of the French government seems to increase "equal" repartition of these areas in the French territories. In any cases we use a conditional difference in differences strategy, using a propensity score matching method (as e.g. O'Keefe 2004) to mitigate the selection issue. We use as a control group a pool of deprived areas that failed to become EZ.

We find that enterprise zones have generated a moderate yet significant effect in job creations *in situ*. This effect is mostly obtained at the extensive margin : we do observe a significant boost in the number of businesses within ZFUs. However, we show that a large part of the increase is the result of business transfers rather than real creations. At the intensive margin, the effects on employment of businesses that were located in the ZFU area before 2004 are much smaller on average. Interestingly, they are concentrated on the smallest businesses. We do not find any significant impact on accounting variables. If overall, the evaluation suggests that the cost-effectiveness of the zones are at best modest. One issue for the global assessment of these kind of policy is the potential negative externalities of EZ on their neighborhoods. Thanks to our data, we could compare the economic situation in EZ and their close neighborhood. Our empirical estimates do not let us conclude to the presence of this impact. Our results have policy implications for a better targeting of businesses. They also suggests that attention should be paid to the way zone are monitored. We indeed observe that a subsequent part of the eligible firms do not use tax breaks. This evidence is supported by a qualitative survey, that indicates that information of firms on the tax incentives is weak. This result has been emphasized in the US case, as Kolko & Neumark (2010) show that EZ that create jobs are those whose administrator provides marketing supports (informing the firms of the existence of tax breaks for instance).

The following section briefly presents the main characteristics of the zones. The second the data used for this study, the third the methodology, the fourth section provides the main results and the fifth a discussion.

3.2 A brief description of the French Enterprise Zones

Since the early 1980s, the deterioration of economic and social conditions in certain neighborhoods has led the government to take a series of measures, combined under the generic name Politique de la Ville (Urban Policy). The common denominators of these underprivileged neighborhoods are high crime, unemployment and poverty levels, a dilapidated property market and limited access to public services and retail outlets. The first two development stages of France's Urban Policy in the 1980s and early 1990s achieved fairly uneven results. In any case, they failed to tackle the overall problem. In 1991, the framework law on urban affairs singled out 546 priority neighborhoods where local authorities can give tax exemptions to the companies locating or extending their premises to these neighborhoods, following an ad hoc debate.

The Pacte de Relance de la Ville (Urban Revival Pact), initiated in 1996, constituted the third stage. It stresses the need for economic revival in sensitive urban areas (ZUS) as a prerequisite for their social renovation. The pact establishes three categories of underprivileged neighborhoods and grants them tax exemptions in proportion to the level of their problems. Firstly, the State defines 750 ZUS after which 416 urban rehabilitation areas (ZRU) are deployed in the most underprivileged ZUS. Finally, certain ZUSs or ZRUs (the most handicapped) are transformed into 44 economic opportunity areas (ZFU).

The objective of the ZRU (and ZFU) program is to promote the economic development of these areas. It mostly grants substantial tax relief to existing businesses and businesses locating to ZFUs and ZRUs, with a much higher exemption level in the former than in the latter (see Appendix A). Aid progression according to the different zonings takes into account the heterogeneous economic and social situations of underprivileged neighborhoods and strives to provide those most underprivileged with the most generous measures. Companies with less than 50 employees benefit, for their establishments located in ZFUs, from five-year exemptions from local business tax, corporate income tax and property tax. Employer contributions are also exempt for five years on the fraction of salary lower than 1.4 times the minimum wage (Smic). This aid is limited and the system terminates over a progressive 3 to 9 year period subsequent to the full-rate exemption period of the first five years (see the appendix).

In January 2004, 41 new ZFUs are created among existing ZRUs. These areas were selected out of the existing ZRUs. Officially, the selection criterion is specified by the law as a combination of 5 local indicators, namely the total population of the area, the unemployment rate, the proportion of young people (under 25), the proportion of over 15 year-olds with no qualification and the tax potential of the municipality.[3] The composite index is then calculated as the product of the first four elements, divided by the fifth.[4] In addition, it is specified that a ZFU should have more than 10,000 residents. However, the scope of a ZFU can be flexible in order to met this last criterion : it generally encompasses the territory of the underlying ZUS or ZRU but can often include additional territories. In some cases, the ZFU even combines several ZRUs from the same conurbation. The 41 EZs was indeed created from 51 initial ZRU.

For the classification used in 2004 to select the new ZFUs, the index was calculated based on the 1999 population census (INSEE) and fiscal sources (General Tax Office and Ministry of finance), as these sources enable the determination of the

3. This would be the product of local taxes for the municipality if the average national rate was applied to the municipality for each of the local rates, given the tax basis of the municipality. This estimate therefore characterizes the potential fiscal wealth of the municipality.

4. This index is called "PRV index" as in *Pacte de Relance pour la Ville* (Urban Revival Pact), the name of the original law – see decree 96-1159 issued on 26 December 1996.

tax potential for the latest available year. However, as confirmed by anecdotal evidence from conversations with officials of the Ministry of Urban Affairs, while policy makers had chosen really more deprived areas at their first pick in 1997, they decided which ZRUs were to be granted the EZ status in 2004 mainly according to the size of the area in terms of total population. The objective of the French government was to achieve a more "equal" repartition of ZFUs over the French territories. A closer look to the distribution of the variables composing the PRV index (plus total population and the share of foreigners in the population), in the ZFUs created in 1997, in the EZs created in 2004 and the ZRUs that failed to be upgraded into ZFUs in 2004 show indeed that this second wave does not really correspond to the most deprived areas (Figure 3.1). Indeed, the second wave of EZs has a rather smaller rate of dropouts than the areas that have not been designed as EZ (the average rate over these areas is 29% against 33%) and smaller unemployment rate (the averages are respectively 28% and 30%). The EZs of 2004 look even better off than both other groups in terms of financial capacity of municipalities. As the share of foreigners is higher in the first wave of EZs than in others areas, it is about equally distributed in EZs of 2004 and ZRUs. Finally, the variable that really makes a difference between EZs of 2004 and other areas is the total population.

We focus our analysis on this sample of the second wave of French ZFUs, that we will compare with "comparable" ZRUs (we discuss this point below). Finally, note that the issue of schedule is important. Even if the list of beneficiary areas was officially published in 2004, these areas were already known at the beginning of 2003. Data availability before and after 2003 is therefore key to our evaluation.

3.3 Data

3.3.1 Data description

The data we use come from several sources. The SIRENE directory contains exhaustive information on businesses in manufacturing, trade and services industries, in particular their location. The stock of businesses is reported yearly, at the date of December 31. New businesses are also reported, as well as their creation date and the business origin (whether a creation, a transfer or a take-over). The bankruptcy

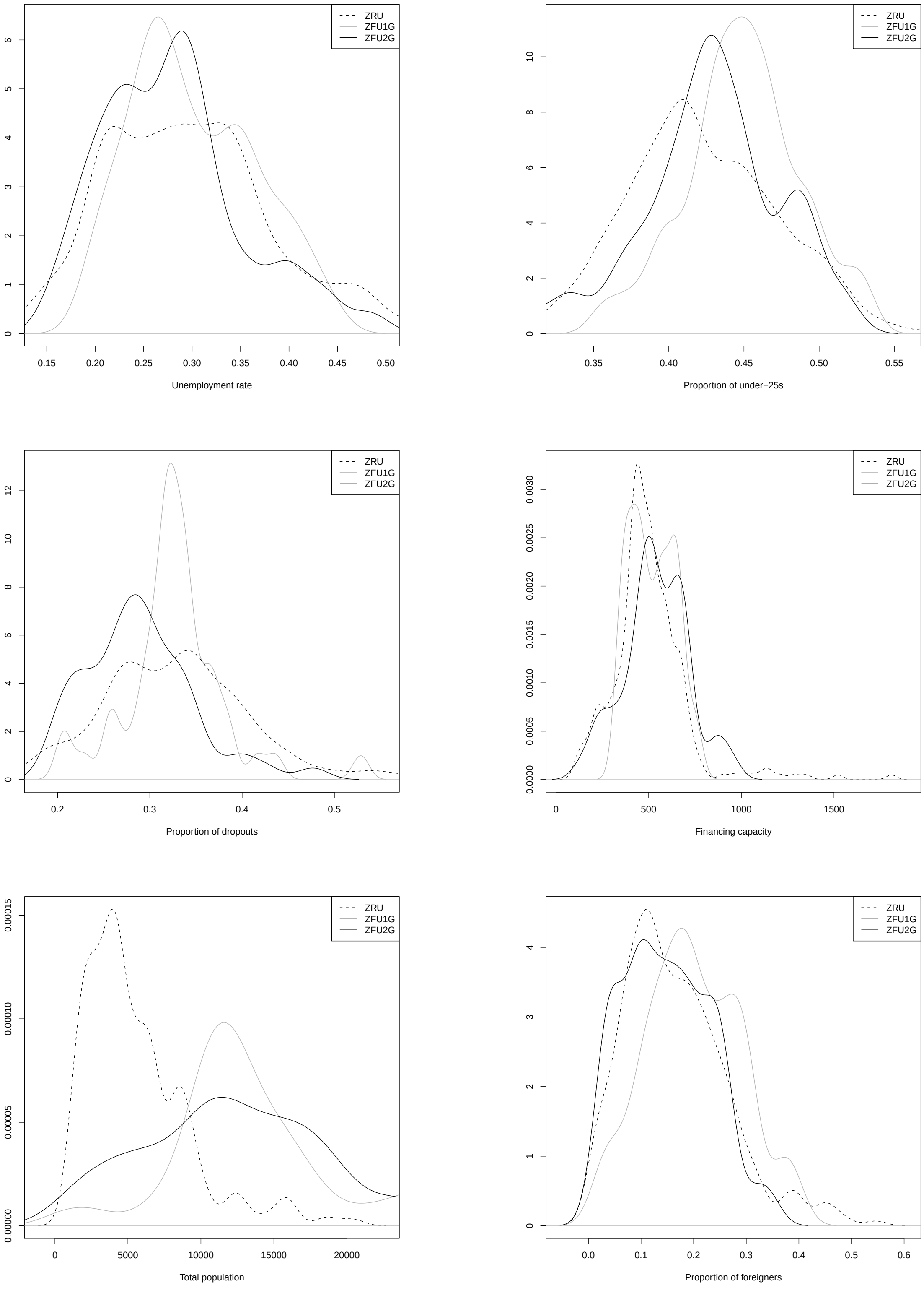


FIGURE 3.1 – Estimated densities of unemployment rate, proportion of under-25s, proportion of dropouts, municipal financing capacity, total population, share of foreigners in EZs created in 1997, EZs created in 2004 and areas in 2004

file of June 2007 contains the records of all bankruptcy proceedings brought before a trade tribunal before this date. The DADS dataset (annual declarations of social security data) contains exhaustive information on employment and salaries in these businesses. The fiscal records, called BRN and RSI, are provided by the tax administration. They provide accounting information (balance sheet and income statement) as well as the amount of taxes paid by every businesses. These data are available and matched over the 2002-2007 period.

We also use the 1999 Census to extract variables which characterize the socio-demographic situation of the areas under study. In particular, some of these characteristics correspond with the criteria considered by policy-makers to classify an area as a ZFU.

These data let us estimate the effect of tax incentives in a comprehensive way. We can check whether the transformation of a ZRU into a ZFU has an impact on economic activities measured by the stock of firms located in these areas, and on firm demography (creation and failure). We could also measure the impact on total paid employment. Besides, we could check whether this evolution is due to the new firms or to firms already present before the creation of the EZ. Access to panel data indeed let us isolate businesses with employee eligible for the system (less than fifty employees, turnover under €10 million, not held by higher corporate), located in EZ in 2002. As it is unlikely that these businesses anticipated the tax cuts two years before the EZ classification, we can rule out possible selection that could occur with firms located after. This thus let us assess whether tax exemptions can foster economic development of small firms. Micro-businesses are often confronted with financial difficulties which jeopardize their survival. Tax exemptions can therefore allow them to improve their financial situation or get out of debt. Beyond evolution of their employment, we will thus focus in several indicators of the economic and accounting situation : sales, current taxable income, debt, cash flow as well as investment.

3.3.2 Summary statistics

Table 3.1 presents the basic statistics in the different areas in 2002, before the creation of the second wave of the French EZs. We distinguish between the areas chosen as EZ in 2002 and areas that have never been classified as EZ. [5] As stated above, EZ are on average much bigger than the 284 other areas in terms of available lands and of inhabitants, which is consistent with the use of a theoretical threshold of 10,000 inhabitants. The limit was partly achieved by the junction of close smallest areas, however. As stated before, from 51 initial ZRUs only 41 EZ were created in 2004. Not surprisingly because of this size effect, more businesses are initially located in these areas (187 in average compared to 84 in others areas). A large part of these firms are not eligible to tax exemptions granted by EZ, however (meaning they have more than 50 employees or/and a turnover above €10 million). Finally, on average 37 new firms located in future EZs in 2002, while 15 firms did the same in other (smaller) areas. Even when controlling by the initial stock, the growth in the number of firms appears slightly higher in future EZ. This flow mainly corresponds to real creations (15% corresponds to transfer of preexisting industries).

The characteristics of eligible businesses in both groups are quite comparable in 2002 (Table 3.2). Note that on average, these firms are far below the threshold of 50 employees : whatever the zoning, the average labor force was around 5 employees in 2002. Eligible businesses in areas that have not became EZs are in slightly better financial situation than their counterparts in future EZs.

The ZFU classification results in substantial transfers to the companies located in these areas. Our control group is made up of businesses located in ZRUs which also benefit from tax incentives. However, we observe a marked discrepancy between the businesses located in ZFUs and those (equivalent in size) located in areas which have remained ZRUs. Thus, even if our data probably only reflects part of the exemptions, the eligible companies declare, from the year subsequent to classification as ZFU, as of 1 January 2005 (*i.e.* for the 2004 tax year, the first year following the classification as ZFU) that they benefit from exemptions on corporation tax and local business tax amounting to €11,000 on average (Figure 3.2).

5. We thus exclude the 15 third-generation EZs.

TABLE 3.1 – Economic situation in the areas in 2002

	areas not turning EZ after 2004	areas turning into EZ in 2004
Total population	5,433	12,644
Area (km^2)	772,345	1,353,931
Number of firms	84	187
Number of workers	389	685
Firm demography		
New firms	15.27	37.45
Creations	12.68	31.47
Transfers	2.59	5.98
New firms (/ Stock in 2001)	0.19	0.21
Creations (/ Stock in 2001)	0.16	0.18
Transfers (/ Stock in 2001)	0.03	0.03
Industries		
Share of manuf.	0.11	0.12
Share of retail	0.36	0.32
Share of construction	0.24	0.25
Share of services to hh.	0.15	0.15
Share of B. to B.	0.08	0.10
Share of transportation	0.04	0.04
Number of areas	284	51

Table 3.2 – Descriptive statistics for eligible firms in 2002

	area no turning into EZ after 2004	area turning into EZ in 2002
Number of workers	4.96	5.10
Hourly wage (euro)	10.54	10.64
Sales (x 1,000 euro)	250.22	232.00
Income (x 1,000 euro)	35.50	33.03
Cash flow (x 1,000 euro)	26.61	25.87
Investment (x 1,000 euro)	12.55	11.58
Debt (x 1,000 euro)	93.67	93.38
Average number of eligible firms per area	21	38

These figures compares to the almost-zero tax exemptions that prevailed from 2002 to 2004, what stresses that the specific fiscal advantages to be located in ZRUs were indeed modest, at least during this period.

The declared local business tax amount has regularly increased since 2002 for the companies outside ZFUs (Figure 3.3). From the moment the ZFU classification is effective, this trend is reversed for the companies concerned. In 2007, the companies of our sample located in second-generation ZFUs claimed they paid €900 in business tax, *i.e.* four times less than similar companies located in ZRUs which are not ZFUs. Before ZFU classification, the figures were very similar. A comparable evolution is observed for social security contributions : the ZFU classification has resulted in a stabilization although the amounts tend to increase. The amount of social security contributions declared by the companies in second-generation ZFUs is, as of 1 January 2007, €4,300 on average, *i.e.* the same figure as at the time of ZFU classification (Figure 3.4). For the companies located in ZRUs not classified as ZFUs, this amount is on average €5,000 in 2007. As of 1 January 2002, these companies declared average amounts of €3,500 whether or not they were located in future ZFUs.

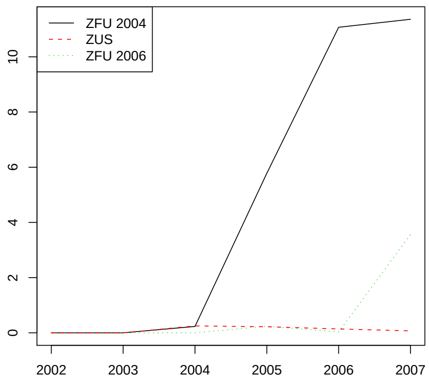


FIGURE 3.2 – Declared exemption amount per area (thousand Euros)

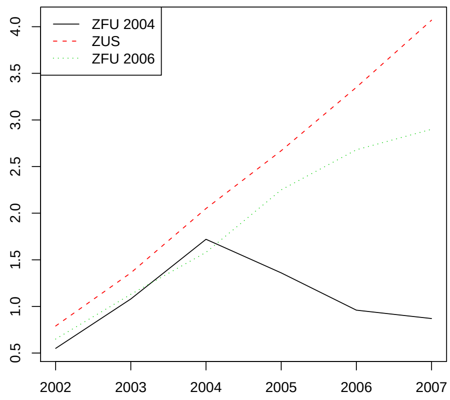


FIGURE 3.3 – Declared business tax amount (thousand Euros)

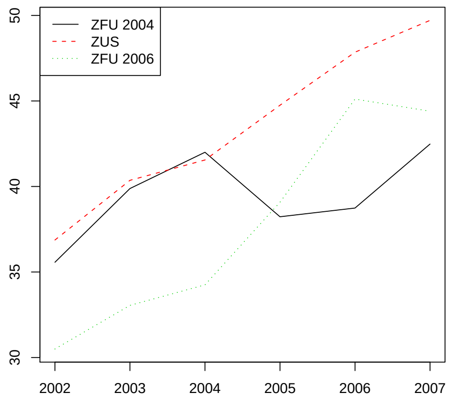


FIGURE 3.4 – Declared amount of social security contributions per area (thousand Euros)

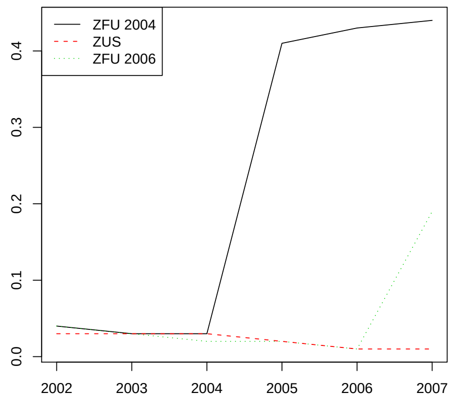


FIGURE 3.5 – Declared amount of social security contributions per area (thousand Euros)

The two above-mentioned results suggest that the ZFU classification contributed to curbing the increase in taxation rather than alleviating the tax burden. Note that this increase could be due to a general increase in the average size of these companies, as we limit ourselves to the companies present in 2002. The total amount of social security contributions, for example, automatically increases with the company's employment level.

Nevertheless, being located in a ZFU results in substantial tax relief, even compared with companies located in areas already benefiting from tax rebates. Our objective is therefore to establish the economic consequences of these financial transfers.

3.4 Methodology

Ideally, the effect we want to measure would be obtained by calculating the difference between the average of the dependent variable : on the units effectively treated (in which case we have an effective Y value observed for each beneficiary unit) on these same units but in a scenario where they have not been treated. We obviously do not observe the latter if the units in question have been treated. Direct comparison of all treated and all non treated units could lead to spurious results, as the EZs are not *a priori* randomly chosen. EZs are areas selected precisely because they suffer from an accumulation of economic and social handicaps which can cripple the economic growth in these areas. Comparing unit situations in accordance with their geographical location runs the risk of wrongly attributing these composition effects to EZ systems. However, as detailed below, we choose an empirical strategy that consists of choosing a control group of areas as close as possible to EZs. Our identification assumption states conditional on characteristics observed previously to second-generation-EZ classification, the affectation of an area to the EZ status is not related to the potential outcome.

Formally speaking, we can consider that any area i has virtually two potential outcomes, at time t : one referred to as Y_{it}^1, if it is located in a ZFU, and the other, referred to as Y_{it}^0, if it is not. The impact of the EZ classification at time t on this unit corresponds exactly with $Y_{it}^1 - Y_{it}^0$, with t posterior to the EZ classification date. It will never be possible to estimate this impact as we can never simultaneously observe both potential outcomes for the same unit at the same time. In order to obtain an evaluation of the counterfactual outcome, we use a conditional difference-in-differences strategy. The identifying assumption is that we have enough observable characteristics to assume that the remaining differences between the units are not associated with the treatment. We use time-differentiated outcomes, in order to eliminate potential systematic differences between the areas (see Heckman, Ichimura, Smith & Todd 1998).

$$\Delta Y_{it}^{0} \perp T_i | X_i \tag{3.4.1}$$

To overcome the problem of dimensionality arising when using too many observables, we perform a propensity score matching. As shown by Rosenbaum & Rubin (1983), if the conditional independence assumption holds for observables X, it also holds for the propensity score, *i.e.* the probability of being treated conditional on these observables. In other words, hypothesis (3.4.1) implies

$$\Delta Y_{it}^{0} \perp T_i | p(T_i = 1 | X_i). \tag{3.4.2}$$

In practice, the estimation is a two-stage process : the estimation of the propensity score followed by the matching itself. We estimate this propensity score using a logit model.

For conditioning variables, we use the variables officially used for the calculation of the PRV index (see Section 3.2), as well as variables reflecting the presence of housing estates, the local economic situation at the start of the period. We also introduce two variables related to the relative distance of the zones. As expected by our descriptive analysis, ZRUs that turned into ZFUs in 2004 are larger, more peopled, have more under-25s, more unemployed but less dropouts (see Table 3.3). These areas include more often a housing estate. In terms of economic activity, these areas have less employments, less businesses but seem more dynamic in 2002, as the ratio of new over existing businesses in higher in these areas. The discussion above (see section 3.2) suggests that for a ZRU in 2003 the distance to a first wave ZFU and the closeness to another ZRU affect positively the likelihood of being designed as a ZFU : the former because it improves the equal repartition of ZFU, and the latter because they could be merged in order to constitute a zone achieving the required size. This theoretically allows us to reduce the differences in composition of both groups. We find indeed that these variables has a strong significant impact of the likelihood of being designed.

In the second stage, each treated unit is matched with untreated units with a similar propensity score. We used a kernel matching method : untreated observa-

TABLE 3.3 – Probability for a area in 2003 to be part of a EZ in 2004

Variables	Probability of belonging to a EZ in 2004
Intercept	472.81 (299.24)
Distance to closest ZRU < 2 km	1.22^{**} (0.51)
Distance to closest ZRU < 5 km	1.26^{**} (0.59)
Distance to closest EZ1997 > 30 km	2.22^{***} (0.61)
Log total population	−156.22 (104.52)
Log total population ^{2}	16.93 (12.11)
Log total population ^{3}	−0.59 (0.47)
Fiscal potential	−1.12 (0.69)
Log employment	0.32 (0.29)
Proportion of social housing	0.98 (0.90)
Proportion of less than 25	3.80^{*} (2.10)
Proportion of dropouts	0.32 (1.34)
Proportion of unemployed	−0.67 (1.13)
Log number of establishments	−0.49 (0.95)
Log number of eligible ones	−0.01 (0.73)
Number of EZ 2004 observations	50
Number of non-EZ 2004 observations	250

Note : Probit estimation. The standard deviation of the estimator is in brackets. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%).

tions are weighted according to their distance from the treated observation, the weightings being determined by a (Gaussian) kernel. At the end of this matching process, the estimated effect of the treatment is obtained for each treated unit. The average effect of the treatment is then calculated by considering the average of individual differences.

As demonstrated in particular by Bertrand et al. (2004), the inference is biased by both group effects (the businesses in the same areas can experience joint shocks) and serial correlation in residuals. We thus choose to work at the area levels and use block bootstraps which respect the correlation structure of the areas throughout all observed periodsfor inference. [6]

3.5 Results

We perform yearly estimates on the (log) variable of several indicators of economic situation in the concerned areas (firm demography, employment, economic situation of preexisting firms) for 2003 to 2008. It is worth emphasizing that the first year of estimation corresponds to a period before the introduction of the tax cuts. It could thus constitutes a "falsification test" (Manning & Pischke 2006) of potential differences in the growth rate of our control group (and an informal test of the validity of our identification assumption). For most of our variables of interest, we do not observe significant differences between our treatment and (matched) control group before the introduction of the tax cuts (Tables 3.4, 3.1 for instance).

The overall impact of the transformation of a ZRU into a ZFU on the economic activity is positive from the first year of the tax breaks (Table 3.4). [7] The stocks of companies is 5% higher in the EZ than in comparable ZRU from the first year of the implementation of EZs, and surprisingly this increase accumulates the following years. When focusing on companies already present before the introduction of the label is allocated, tax cuts result in an increase in growth rate of the total

6. In the block bootstrap, blocks of individuals are drawn with replacement instead of drawing individuals. The block is defined as a group of individuals whose unobservables are potentially correlated.

7. Econometric studies have all been carried out using the R software R Development Core Team (2007).

eligible companies by 3% the first year, but this effect is not significant at usual level and not persistent. It is worth emphasizing that as stated before, we choose to work at the area levels and use block-bootstrap for inference. This choice is quite conservative, considering our sample size (a few hundreds of units only). This explains that our results are very imprecisely estimated. We also estimate separately effect according to the position in relation to the median in the distribution of employment in 2002, e.g three employees. Interestingly, this effect is higher for smaller firms, if not significant at usual level. This suggests that the number of still existing firms is persistently higher in EZ compared with the level that would have prevailed in the absence of tax break.

As regard business creation, the fact that an area belongs to a EZ increases the business creation rate by 4 to 7 percentage points per area each year subsequently the creation of EZ. A large part of this effect is due to transfer of already existing firms, while it was not the case before the EZ introduction (see Table 3.1). Besides, we do not observe any significant impact on the rate of business failure in the EZ. Note that this impact is not clear : while the tax cuts could improve the solvency of preexisting firms, windfall effect could lead to the installation of "weaker" firms. Both effects go in opposite direction concerning the failure rate.

Concerning employment, the growth in overall employment is of same magnitude in 2004 for the total amount of employment (measured in working hours or full-time equivalent jobs), and even higher the following years (Table 3.5). When focusing on companies already present before the introduction of the EZ, we still observe no significant impact. Surprisingly, the impact is negative the second year of the tax cuts (but not significant). Once again, the impact is of higher magnitude for the smallest firms, and increasing over the period.

Results are even more disappointing when looking at the accounting variables for companies already present (Table 3.6). For most of variables, results are noisy and difficult to interpret. We mainly observe a short-run impact on the current taxable income, which probably reflects the tax exemptions (for instance on employer contributions). We do not observe any significant impact in sales, nor in various indicators of accounting situation (cash flow, investment, debts), however.

Table 3.4 – Impact of the transition to EZ on stock of companies and firms demography

Variables	Years				
	2003	2004	2005	2006	2007
Stock (log)					
Number of establishments	0.02 (0.04)	0.05*** (0.02)	0.05*** (0.02)	0.04 (0.02)	0.03 (0.03)
Amongst companies eligible already present in 2002					
	−0.02 (0.03)	0.03 (0.05)	0.01 (0.06)	−0.02 (0.04)	−0.02 (0.06)
...with less than 3 employees in 2002					
	−0.06 (0.04)	0.06 (0.08)	0.05 (0.10)	0.02 (0.06)	−0.06 (0.09)
...with more than 4 employees in 2002					
	0.02 (0.05)	0.01 (0.05)	−0.04 (0.06)	−0.02 (0.06)	0.03 (0.07)
Flow (relatively to the previous stock)					
New establishments	0.00 (0.03)	0.07*** (0.02)	0.04* (0.02)	0.06** (0.03)	0.04 (0.02)
Creations	0.01 (0.02)	0.04*** (0.01)	0.03** (0.02)	0.03 (0.02)	0.00 (0.02)
Transfers	−0.01 (0.01)	0.02*** (0.01)	0.01 (0.01)	0.03*** (0.01)	0.03** (0.01)
Amongst companies eligible already present in 2002					
Failures (for 1,000 companies)	1.40 (1.67)	0.04 (2.30)	1.24 (1.64)	−1.93 (2.07)	0.08 (1.65)

Note : The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%). All results featured herewith correspond with a Gaussian kernel matching method.

TABLE 3.5 – Impact of the transition to EZ on employment (log)

Variables	Years				
	2003	2004	2005	2006	2007
All companies					
Employment	0.03	0.04	0.07*	0.05	0.21***
	(0.07)	(0.05)	(0.04)	(0.08)	(0.07)
Hours	0.02	0.05	0.08*	0.05	0.22***
	(0.06)	(0.05)	(0.04)	(0.09)	(0.07)
Amongst companies eligible already present in 2002					
Employment	0.01	0.02	−0.05	0.08	0.07*
	(0.04)	(0.07)	(0.07)	(0.11)	(0.04)
Hours	0.00	0.03	−0.07	0.07	0.04
	(0.04)	(0.07)	(0.07)	(0.09)	(0.04)
…with less than 3 employees in 2002					
Employment	0.04	−0.05	0.03	0.10	0.11
	(0.07)	(0.08)	(0.11)	(0.09)	(0.08)
Hours	0.05	−0.06	0.02	0.12*	0.02
	(0.07)	(0.07)	(0.10)	(0.07)	(0.07)
…with more than 4 employees in 2002					
Employment	−0.03	0.05	−0.05	0.07	0.05
	(0.05)	(0.07)	(0.06)	(0.12)	(0.05)
Hours	−0.04	0.07	−0.08	0.05	0.03
	(0.05)	(0.06)	(0.07)	(0.10)	(0.05)

Note : The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%). All results featured herewith correspond with a Gaussian kernel matching method.

Table 3.6 – Impact of the transition to EZ on wages and accounting indicators, for eligible companies existing in 2002

Variables	Years				
	2003	2004	2005	2006	2007
All eligible companies present in 2002					
Income	0.00 (0.01)	-0.04^{***} (0.01)	-0.00 (0.01)	-0.02 (0.02)	0.02 (0.02)
Sales	-0.00 (0.05)	0.01 (0.07)	-0.08 (0.06)	0.07 (0.07)	0.04 (0.07)
Cash flow / Sales	-0.01 (0.01)	0.01 (0.02)	-0.00 (0.01)	-0.03 (0.03)	0.02 (0.02)
Debt/ Sales	-0.03 (0.03)	-0.02 (0.03)	-0.03 (0.05)	-0.02 (0.03)	0.01 (0.04)
Hourly wage (log)	0.00 (0.03)	-0.00 (0.01)	0.01 (0.02)	0.02 (0.02)	0.01 (0.02)
Investment/ Sales	-0.02 (0.03)	0.02 (0.02)	0.01 (0.01)	0.00 (0.01)	-0.03 (0.03)

Note : The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%). All results featured herewith correspond with a Gaussian kernel matching method.

3.5.1 EZ and local externalities

Finally, one could ask whether this (slightly) positive impact on economic activity in the areas targeted by the policy is obtained at the expense of other areas. As we observe that part of this effect corresponds to activity relocations rather than real creation, one issue is that ZFUs can generate negative externalities on their neighborhood. Thanks to our dataset, we could estimate the impact of the program on ZFUs geographical vicinity. More precisely, we consider the strip of land outside the EZ less than 300 meters from this border as a new counterfactual. This limit is purely conventional but corresponds more or less to a surface of land similar to that of the ZFU. [8] We conduct the same analysis as before, using the neighborhood areas as a control group instead of ZRU areas.

Interestingly, the "falsification test" that compared the EZ with their neighborhood *before* the introduction of tax cuts does not lead to reject any null impact. Indeed, if the growth rate of the number of firms are the same in both groups, this results comes from two opposite effects : more new firms are created in EZ than in their neighborhood the year before the classification (Table 3.7), but more eligible firms disappears. Firms appears more better off in terms of wages and employment in EZ than in their neighborhood the year before (Tables 3.8 and 3.9). This is consistent with descriptive statistics of these areas, which are much more different from EZ in terms of economic activities than other ZRU (see Tables 3.10 and 3.11 in Appendix B). The following years, the estimated impact of the EZ system is more precisely estimated than with our main control group, but appears of the same magnitude than those obtained using ZRU areas as a control group. This does not provide evidence of sizable externalities of the policy on close neighborhoods.

3.6 Discussion

All in all, we observe a significant effects of the tax rebates on economic activities of the zone, in contrast with most of the previous studies on the EZ. The magnitude

8. In order to locate this neighborhood, we use the batched statistical information islets (Iris). Iris areas are infra-municipal units, a combination of islets resulting from the 1999 population census. As iris areas are also geolocalized, we could allocate small iris areas in one EZ or to its neighborhood.

Table 3.7 – Impact of the transition to EZ on wages and accounting indicators compared to their neighborhoods on stock of companies and firms demography

Variables	Years				
	2003	2004	2005	2006	2007
Stock (log)					
Number of establishments	-0.00 (0.00)	0.07^{***} (0.00)	0.06^{***} (0.01)	0.03^{***} (0.01)	0.05^{*} (0.03)
Amongst companies eligible already present in 2002					
Number of establishments	-0.07^{***} (0.00)	-0.01 (0.01)	0.00 (0.01)	-0.03 (0.02)	-0.04 (0.02)
...with less than 3 employees in 2002					
	-0.09^{***} (0.02)	0.01 (0.02)	0.04^{***} (0.01)	-0.02 (0.03)	-0.05^{***} (0.01)
...with more than 4 employees in 2002					
	-0.02^{***} (0.00)	0.01 (0.03)	-0.01^{*} (0.01)	-0.02^{***} (0.00)	-0.01 (0.04)
Flow (relatively to the previous stock)					
New establishments	0.08^{***} (0.00)	0.11^{***} (0.00)	0.13^{***} (0.01)	0.11^{***} (0.00)	0.13^{***} (0.03)
Creations	0.09^{***} (0.00)	0.10^{***} (0.02)	0.11^{***} (0.02)	0.09^{***} (0.02)	0.09^{***} (0.02)
Transfers	-0.01^{***} (0.00)	0.01 (0.01)	0.02^{**} (0.01)	0.02^{***} (0.01)	0.04^{**} (0.02)

Note : The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%). All results featured herewith correspond with a Gaussian kernel matching method.

Table 3.8 – Impact of the transition to EZ on wages and accounting indicators compared to their neighborhoods on employment (log)

Variables	Years				
	2003	2004	2005	2006	2007
All companies					
Employment	0.01 (0.05)	0.05^{***} (0.00)	0.06^{*} (0.04)	0.07 (0.07)	0.14^{*} (0.08)
Hours	-0.01^{**} (0.01)	0.05^{***} (0.01)	0.08^{***} (0.03)	0.08^{*} (0.04)	0.12 (0.08)
Amongst companies eligible already present in 2002					
Employment	0.03^{***} (0.01)	0.04^{***} (0.00)	-0.04 (0.03)	-0.01 (0.01)	-0.03^{***} (0.00)
Hours	0.06^{*} (0.03)	0.02 (0.02)	-0.05^{*} (0.03)	0.01 (0.02)	-0.04^{*} (0.02)
...with less than 3 employees in 2002					
Employment	-0.01 (0.02)	-0.00 (0.02)	0.01 (0.02)	-0.06^{***} (0.01)	-0.04^{***} (0.01)
Hours	0.05^{*} (0.03)	-0.02^{**} (0.01)	-0.02 (0.03)	-0.03^{*} (0.02)	-0.04 (0.05)
...with more than 4 employees in 2002					
Employment	-0.02^{**} (0.01)	0.03^{***} (0.00)	-0.04^{***} (0.01)	-0.00 (0.01)	-0.06 (0.05)
Hours	0.01 (0.02)	0.03^{***} (0.00)	-0.05^{***} (0.01)	0.01 (0.03)	-0.08^{*} (0.04)

Note : The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%). All results featured herewith correspond with a Gaussian kernel matching method.

Table 3.9 – Impact of the transition to EZ on wages and accounting indicators compared to their neighborhoods on wages and accounting indicators, for eligible companies existing in 2002

Variables	Years				
	2003	2004	2005	2006	2007
All eligible companies present in 2002					
Income	0.00 (0.01)	-0.02^{**} (0.01)	-0.01^{***} (0.00)	-0.01^{***} (0.01)	0.00 (0.01)
Sales	0.05 (0.03)	0.00 (0.04)	-0.08^{***} (0.03)	-0.01^{*} (0.01)	−0.02 (0.02)
Cash flow /Sales	−0.01 (0.01)	−0.00 (0.01)	0.01^{***} (0.01)	-0.02^{***} (0.00)	−0.01 (0.01)
Debt/Sales	0.01 (0.01)	-0.04^{***} (0.00)	0.01 (0.02)	-0.03^{**} (0.01)	−0.00 (0.01)
Hourly wage (log)	0.04^{***} (0.00)	−0.01 (0.04)	−0.00 (0.02)	0.01^{***} (0.00)	0.02^{**} (0.01)
Investment/Sales	-0.00^{***} (0.00)	−0.00 (0.00)	0.00 (0.01)	-0.01^{*} (0.00)	0.00 (0.00)

Note : The standard deviation of the estimator is in brackets, estimated by block bootstraps in areas. Three (respectively two, one) stars indicate a 1% significance (respectively 5%, 10%). All results featured herewith correspond with a Gaussian kernel matching method.

of this impact is small, however. Besides, it is mostly due to the implementation of new companies. The impact on preexisting companies is surprisingly almost never significant, despite substantial financial transferts.

This suggests the potential impact of this kind of program beyond simple windfall effects. Note that the few positive impact are observed on very small companies, suggesting that the program should be more carefully designed, as it could be more efficient to target specifically these businesses. Besides, one could question the efficiency to sole fiscal incentives to improve local economic situation, and in particular skill deficiency in the local labor market. According to the results of a survey conducted in 2008 targeting local authorities and some companies located in the ZFU areas, companies declare major difficulties to hire employees inside the area (and minor but not major difficulties to hire outside the area) in more than two third of these areas. This could partly explain that according to our fiscal data a significant share of companies do not use tax exemptions. The policy indeed stipulate that firms do have to hire local employees as a prerequisite for benefiting of tax exemptions. Besides, shortage supply in real-estate market could also hamper final assessment, as in almost all of these areas, companies complained about the lack of land availability and more specifically of commercial real estates. Finally, the relative failure of the policy could be due to a lack of information about the policy or uncertainty about the local application of fiscal rules, as emphasized by the local survey (some companies could waive their rights to tax exemptions in order to avoid tax inspection). As shown by Kolko & Neumark (2010), the way the areas are monitoring by zone administrators can make a difference in the final assessment of the policy.

Appendix A : Tax exemptions in ZRU and ZFU

According to the 2007 ONZUS report, companies in ZFU and ZRU benefit from the following tax exemptions :

Local business tax : only companies with less than 50 employees and a turnover of less than €10 million are concerned by ZFUs. Businesses located in ZFUs are fully exempt for five years, within the limit of a taxable amount of €337,713 (2006) and €100,000 of cumulated aid over 3 years. In ZRUs, all businesses with less than 150 employees are eligible. However, the ceiling of the taxable amount is lower : it amounted to €125,197 in 2006.

Property tax on buildings : all buildings located in ZFUs belonging to companies liable for this tax are exempt for 5 years. ZRUs are not concerned.

Corporate income tax : companies located in ZFUs with less than 50 employees and a turnover of less than €10 million are exempt for 5 years, within the limit of €100,000 per year (increased by €5,000 per new recruit). In ZRUs, this exemption is limited to newly created companies in the area, with no staff limitations. These benefit from full exemption for 2 years and decreasing exemption for the next 3. Corporate income tax exemption is limited to €225,000 per 36-month period.

Employer contributions : companies located in ZFUs with less than 50 employees, a turnover of less than €10 million and one third of whose staff live in the ZUS where the ZFU is located are concerned. Employees with open-ended contracts or fixed-term employment contracts of more than 12 months are exempt from employer contributions for 5 years, on the fraction of salary lower than 1.4 times the minimum wage (Smic). This measure concerns companies with less than 50 employees in full-time equivalent and is limited to a cumulated €100,000 over 3 years. In ZRUs, there is no staff or turnover limitation for the company, but this exemption only relates to new recruits and lasts for 12 months (maximum of 50 jobs in full-time equivalent).

Furthermore, in ZFUs, the exemption from local business tax, corporate income tax and property tax is prolonged on a decreasing basis for 3 or 9 years, depending on whether the company has more or less than 5 employees.

Appendix B : Neighborhoods of EZ

Table 3.10 – Economic situation in EZs and their neighboring areas in 2002

	neighboring areas	areas turning into EZ after 2004
Number of firms	454	275
Number of workers	5749	1094
Firm demography		
New firms	63.84	53.43
Creations	47.22	45.00
Transfers	16.62	8.43
New firms	0.14	0.19
Creations	0.10	0.17
Transfers	0.04	0.03
Industries		
Share of manuf.	0.16	0.12
Share of retail	0.32	0.32
Share of construction	0.16	0.23
Share of services to hh.	0.14	0.15
Share of B. to B.	0.15	0.10
Share of transportation	0.04	0.05
Number of areas	37	37

TABLE 3.11 – Descriptive statistics for eligible firms in 2002, comparing EZ and their neighboring areas

	neighboring areas	areas turning into EZ after 2004
Number of workers	6.64	4.80
Hourly wage (euro)	16,018.65	13,227.06
Sales (x 1,000 euro)	405.16	243.93
Income (x 1,000 euro)	38.99	33.24
Cash flow (x 1,000 euro)	41.79	27.74
Investment (x 1,000 euro)	16.68	11.55
Debt (x 1,000 euro)	106.74	93.01
Average number of eligible firms per area	100	56

Chapitre 4

Are (some) temporary jobs a path to permanent position ?[1]

4.1 Introduction

Like many other European countries, France enhanced the flexibility of the labor market in the mid-1980s by allowing employers to hire workers on a fixed-term basis. The number of short-term contracts, such as fixed-term contracts (hereafter FTC), as well as temporary agency work (*interim* missions in France, hereafter TAW), has dramatically increased since then. Short-term contracts have progressively replaced usual open-ended contracts as the traditional hiring device : more than two French hiring contracts out of three have a fixed duration.

This evolution has led to a strongly polarized debate in France. Some praise the flexibility short-term contracts offer in a labor market otherwise characterized by a high level of employment protection. Others express concerns about the poor quality of short-term jobs and about the risk of an undesirable segmentation of the labor market. Last attempts by the French government to further relax regulations imposed on temporary help work in 2005 had to be withdrawn because of strong public opposition. Note that if the primary goal of temporary employment is to provide firms with a buffer stock in face of adverse shocks, it could be used as a screening device Portugal & Varejão, 2009. Engellandt & Riphahn (2005) indeed show that Swiss employees do significantly more overtime work and are less absent with a fixed-term contract than with an open-ended contract. They interpret this effect as a signaling behavior from temporary workers in order to get a permanent position. Dolado & Stucchi (2008) show that firms with the highest past conversion

1. This chapter is co-authored with Lionel Wilner (INSEE-CREST).

rate from FTC to open-ended contracts are the most productive, as FTC workers are more prone to signal a high productivity level. This is all the more likely to be the case in France since the legal probation period of open-ended contracts does not exceed a few months. [2]

As far as we know, the French public debate on this topic relies more on subjective perceptions than on evidence. Yet since the seminal contribution of Booth et al. (2002), substantial applied microeconomic literature has tried to assess whether short-term contracts could foster access to regular jobs – the so-called "stepping-stone" effect – or coerce workers into a precarious trajectories. The former hypothesis is consistent with models where work experience, even in temporary work, results in acquisition of general human capital. It is also consistent with the idea of temporary employment being a probation period, the goal of which is to reveal the quality of the worker to the employer in presence of asymmetric information. The latter hypothesis implies on the contrary that the human capital is mostly firm-specific, and/or that being employed in a temporary position constitutes a bad signal for future employers. Most empirical studies analyze the future positions of temporary workers but few clear-cut conclusions can be drawn out. Booth et al. (2002) conclude there is a stepping-stone effect in Great Britain, while Guell & Petrongolo (2007) point out a negative effect in the Spanish case. Autor & Houseman (2010) find a weak impact of temporary-help employment for unskilled workers, considering welfare participants randomly assigned to program providers differing substantially in their placement rates into temporary-help jobs. Ichino et al. (2008) obtain opposite results in Italy where the recent introduction of temporary work results in a heterogeneous allocation of temporary work agencies among regions.

Different institutional settings can partly explain these contradictory results. As shown by Bentolila et al. (2010), the legislation about employment protection (and more specifically the relative dismissal costs of permanent and temporary work) can create sharp differences in the response of a labor market to business cycle variations. The coexistence of different types of temporary contracts in France is also relevant, because those contracts could correspond to different needs. Temporary agency work (hereafter TAW) involves a third party, an agency, which

2. The maximal duration goes from two months for blue collars to four months for executives, both being renewable once.

acts as an intermediary between the worker and the user company. It decreases hiring and bureaucracy costs, and enables firms to adjust their staff quickly. By contrast, fixed-term contracts imply a direct relationship between employees and firms but offer less dismissal compensations than permanent jobs and cost than TAW. We therefore expect that these two kinds of temporary employment react differently to business cycle fluctuations and a complete analysis of the impact of temporary employment on labor market prospects should take this distinction into account. Besides, while temporary employment was primarily introduced in the French labor market as a buffer stock for firms in case of unexpected economic volatility, it appears crucial to control for economic activity in labor force transition. Surprisingly, the impact of the business cycle has received little attention in the microeconomic analyses of temporary work.

The empirical measure of labor market transitions must overcome the classical selection problem. Temporary workers differ from the unemployed in several dimensions : some are observable such as education and experience, while others are not. These characteristics could also impact their chances of getting a permanent job. This point raises methodological issues and requires to model the state dependence of labor market trajectories. The econometric literature on this topic can be divided into two trends.

The first focuses mainly on the analysis of the dynamics of labor market outcomes. Following for instance **?**, many empirical papers have tried to carefully describe individual paths on the labor market, relying on multi-spell multi-state transition models. Recent papers try to capture the dynamics that could arise when temporary contracts accumulate over time (Doiron & Gørgens, 2008). This specification addresses questions about the impact of the duration dependence on transitions (Gagliarducci, 2005). The potential drawback of this duration analysis is to rely more heavily on parametric assumptions in the specification of unobserved heterogeneity. Furthermore, mostly because of computational issues, a trade-off between taking dynamics into account and choosing a large number of states has to be made. Yet several types of short-term contracts such as FTC or TAW correspond to distinct needs from the employer and all differ in terms of career prospects. Finally, duration analysis requires access to longitudinal data that are missing for France : long period panels rely on retrospective questions, whose quality is questionable because of measurement errors (Magnac & Visser, 1999).

The second approach cares about modeling transitions in a discrete time setting. At the cost of a less refined description of the dynamics than duration models – dynamics is typically assumed to be a first order Markov chain – it cares more about taking properly unobserved heterogeneity into account. Magnac (2000) and Honoré & Kyriazidou (2000) propose a dynamic multinomial logit with fixed effects extending the conditional logit procedure of Chamberlain (1984). This specification does not make any parametric assumption on individual effects and is very much suited to our analysis since it measures state dependence through mobility matrices. Honoré & Kyriazidou (2000) show that time-varying covariates can be introduced in such a setting and the identification is still more demanding. These covariates have nevertheless to present specific properties, as discussed below.

We propose in this paper a measure of transitions from temporary employment to permanent position in the French case, relying on the second method. More specifically, we use a dynamic multinomial logit with fixed effects in order to disentangle state dependence from unobserved heterogeneity. We use individual panel data from the French Labor Force Survey 2006-2010. It contains detailed information on quarterly transitions on the labor market over a large sample of individuals. The large sample size enables us to get a detailed segmentation of the labor force and in particular to distinguish FTC from TAW. As our observation period encompasses economic upswing (from 2006 to 2008), the 2008 crisis as well as the 2010 recovery, we can also measure the potential impact of the business cycle on temporary employment. The French LFS also provides unique information on the working time including overtime and absenteeism. Following Engellandt & Riphahn (2005), we also analyze how the intensity of the transition rate from FTC to open-ended contracts changes when a worker uses this signaling behavior.

Our main conclusions are the following : (i) a spell of FTC increases the probability of getting a permanent job and offers definitely better prospects than unemployment to this regard ; (ii) by contrast, TAW does not lead to significant transitions to permanent employment in comparison with unemployment ; (iii) ignoring economic climate does matter in the estimation of state dependence, and more specifically of the intensity of transitions from temporary employment to permanent position (iv) consistently with the use of temporary employment as a screening device, we observe a higher intensity of transition from temporary employment to permanent position when workers perform overtime work but this correlation disappears once unobserved heterogeneity has been taken into account.

The paper is organized as follows. Section 4.2 is devoted to our econometric model of transitions on the labor market. Details on our data are provided in Section 4.3. Section 4.4 displays our results and Section 4.5 concludes.

4.2 Econometric specification

4.2.1 Dynamic model of labor market transitions with fixed-effect

We use the standard latent propensity framework inspired by McFadden (1974) to describe labor market transitions. We assume the existence of latent individual propensities y^*_{ijt} for each state $j \in \{0, 1, ..., J\}$ at each period t. Individual i is actually observed at t in the state y_{it} that corresponds to :

$$y_{it} = j \Longleftrightarrow y^*_{ijt} = \max_k y^*_{ikt} \tag{4.2.1}$$

We also suppose that current positions on the labor market follow an order 1 Markov process. Formally, the latent propensity y^*_{ijt} is a function of $y_{i,t-1}$:

$$y^*_{ijt} = \sum_{k=0}^{J} \delta_{kj} \mathbb{1}_{y_{i,t-1}=k} + \alpha_{ij} + x_{it}\beta_j + \epsilon_{ijt} \tag{4.2.2}$$

where α_{ij} is a state- and individual- dependent fixed-effect that captures individual heterogeneity in the valuation of being in state j while x_{it} is a vector of time-varying covariates. For instance, α_{ij} may account for gender or education effects while x_{it} may account for business cycle effects. The error term ϵ_{ijt} is assumed to be i.i.d. extreme-value distributed. Hence the conditional probability that agent i is observed in state j at time t is given by

$$\mathbb{P}(y_{it} = j | y_{i,t-1} = k; x_i, \alpha_i, \beta, \delta) = \frac{e^{\delta_{kj} + x_{it}\beta_j + \alpha_{ij}}}{\sum_{l=0}^{J} e^{\delta_{kl} + x_{it}\beta_l + \alpha_{il}}} \tag{4.2.3}$$

Our main parameter of interest is the vector δ measuring state dependence. The coefficient δ_{kj} stands for a mobility index between states k and j, while δ_{jj} measures the persistence in state j. Only $J^2 - (2J - 1)$ components of δ are identified (Magnac, 2000) and we normalize $\delta_{j0} = \delta_{0j} = 0 \quad \forall j = 0, ..., J$. The state $j = 0$

is called the reference state and can be chosen arbitrarily among different possible states (in practice, we chose the unemployment state). Finally, δ_{kj} corresponds to

$$\delta_{kj} - \delta_{k0} = log\frac{\mathbb{P}(y_{it} = j|y_{i,t-1} = k; x_i, \alpha_i, \beta)/\mathbb{P}(y_{it} = 0|y_{i,t-1} = k; x_i, \alpha_i, \beta)}{\mathbb{P}(y_{it} = j|y_{i,t-1} = 0; x_i, \alpha_i, \beta)/\mathbb{P}(y_{it} = 0|y_{i,t-1} = 0; x_i, \alpha_i, \beta)} \quad (4.2.4)$$

i.e. the log ratio of being in state j rather than in the reference state 0, given that one comes from k rather than from 0.[3] Similarly, only $J-1$ parameters β_j are identified and we state $\beta_0 = 0$ to identify the effect of time-varying covariates.

Maximum likelihood estimation in this dynamic nonlinear model with fixed effects suffers from the incidental parameters problem. When the number of periods T is small, individual fixed effects are estimated with error ; if the likelihood of the model cannot be split into distinct parts, one of which isolating incidental parameters, the error contaminates the estimation of other parameters of the model, resulting in inconsistent estimates.

However, a consistent estimation of δ can be obtained in the fixed-effect specification using the conditional likelihood approach proposed by Chamberlain (1984). In the absence of covariates, Magnac (2000) proves that the maximization of the conditional likelihood

$$\mathcal{L}(y_{i2}, ..., y_{i,T-1}|y_{i1}, y_{iT}, n_{i1}, ..., n_{iJ}) = \frac{exp(\sum_{k,j,t=2,...,T-1} \delta_{kj} \mathbb{1}_{y_{i,t-1}=k} \mathbb{1}_{y_{it}=j})}{\sum_B exp(\sum_{k,j,t=2,...,T-1} \delta_{kj} \mathbb{1}_{b_{i,t-1}=k} \mathbb{1}_{b_{it}=j})} \quad (4.2.5)$$

with $B = \{(b_{i2}...b_{i,T-1}) \,/\, \forall k \quad \sum_{t=2}^{T-1} \mathbb{1}_{b_{it}=k} = n_{ik}\}$ being the set of labor market histories that are compatible with numbers of occurrences n_{ik}, provides a consistent estimation of δ. As a result, individuals who stay in the same state, called "stayers", do not contribute to the conditional likelihood and the model is identified on "movers" only.

Including time-varying covariates x_{it} in the model changes slightly the estimation procedure as Honoré & Kyriazidou (2000) show. To illustrate, let us consider two paths A and $\bar{A}$ sharing the same sufficient statistics in the sense that their contribution to the likelihood has the same denominator : A and $\bar{A}$ are identical except

3. These coefficients can be compared within a row (odds of being in state j rather than in state j', given that one comes from k rather than from the reference state) or within a column (odds of being in state j rather than in the reference state, given that one comes from k rather than from k').

for some “switch” between two dates :

$$\begin{cases} \forall t \notin (t_1, t_2),\ y_{it}^{A} = y_{it}^{\bar{A}} \\ y_{it_1}^{A} = y_{it_2}^{\bar{A}}, y_{it_2}^{A} = y_{it_1}^{\bar{A}} \end{cases}$$

then in equation (5), denominators of probabilities $\mathbb{P}(A|x_i, \alpha_i, \delta)$ and $\mathbb{P}(\bar{A}|x_i, \alpha_i, \delta)$ are identical provided that $x_{i,t_1+1} = x_{i,t_2+1}$. More precisely, the likelihood of the trajectory A conditional of belonging to A or $\bar{A}$ can be written as follows :

$$\mathbb{P}\left[A \,\middle|\, (y_{it_1}, y_{it_2}) \in \left((y_{it_1}^{A}, y_{it_2}^{A}); (y_{it_2}^{A}, y_{it_1}^{A})\right)\right] = \frac{exp[D_i^{t_1t_2}]}{1 + exp[D_i^{t_1t_2}]} \tag{4.2.6}$$

with

$$\begin{aligned} D_i^{t_1t_2} = &\left(\beta_{y_{it_1}^{A}} - \beta_{y_{it_2}^{A}}\right)(x_{it_1} - x_{it_2}) \\ &+ \delta_{y_{it_1-1}^{A}, y_{it_1}^{A}} + \delta_{y_{it_2-1}^{A}, y_{it_2}^{A}} + \delta_{y_{it_2}^{A}, y_{it_2+1}^{A}} + \mathbb{1}_{t_2 \neq t_1+1}\, \delta_{y_{it_1}^{A}, y_{it_1+1}^{A}} \\ &- \delta_{y_{it_1-1}^{A}, y_{it_2}^{A}} - \delta_{y_{it_1}^{A}, y_{it_2+1}^{A}} - \mathbb{1}_{t_2 = t_1+1}\, \delta_{y_{it_1+1}^{A}, y_{it_1}^{A}} - \mathbb{1}_{t_2 \neq t_1+1}(\delta_{y_{it_2-1}^{A}, y_{it_1}^{A}} + \delta_{y_{it_2-1}^{A}, y_{it_1+1}^{A}}) \end{aligned} \tag{4.2.7}$$

and this probability is independent from the vector α. In this more general setting, Honoré & Kyriazidou (2000) prove that the maximization of :

$$\sum_{i=1}^{n} \sum_{1<t_1<t_2<T} \mathbb{1}_{y_{it_1} \neq y_{it_2}}\, K\left(\frac{x_{i,t_2+1} - x_{i,t_1+1}}{\sigma_n}\right) log \frac{exp[D_i^{t_1t_2}]}{1 + exp[D_i^{t_1t_2}]} \tag{4.2.8}$$

provides a consistent and asymptotically normal estimator of the true vector of parameters (δ, β) under two sets of conditions (see Appendix for details). The first are standard conditions in nonparametric estimation like regularity conditions on the kernel and the rate of convergence of the bandwidth ($\sigma_n \to 0$ as $n \to +\infty$ and $\sqrt{n}\sigma_n^{5/2} \to 0$ as $n \to +\infty$ for instance if x_{it} is a scalar). The second set is related to the non strict monotonicity of the additional covariates. For instance, when $T = 4$, the difference $x_3 - x_4$ should be continuously distributed with a density bounded from above on its support, and strictly positive and continuous in a neighborhood of zero. There must be enough observations for whom $x_3 - x_4$ is closed to zero and indeed, the estimator puts more weight on such observations. This typically rules out strictly increasing variables like age. Finally, to identify β, data should provide enough variation of $x_2 - x_3$ conditional on $x_3 - x_4$ being closed to zero.

4.2.2 Choice of covariates

To sum up, the identification of the dynamic fixed-effect model without covariate is achieved on a subsample of "movers" who switch at least once between the second and the last but one period. Moreover, in the model with time-varying covariates, the effect of these covariates is identified provided that they meet a few requirements.

First, note that the likelihood proposed by Honoré & Kyriazidou (2000) does not solve cases with *lagged* state-dependent covariates. Indeed, if the underlying model was

$$y^*_{ijt} = \sum_{k=0}^{J} \delta_{kj} \mathbb{1}_{y_{i,t-1}=k} + \alpha_{ij} + x_{it-1}\beta_j + \epsilon_{ijt} \tag{4.2.9}$$

individual fixed-effects would disappear in the conditional likelihood given in the example above provided that : $x_{it_1}\beta_{y_{t_1}} = x_{it_2}\beta_{y_{t_2}}$, which has no reason to hold. Let us consider for instance observations such that $y_{it_1} \neq y_{it_2}$. If the impact of x_{it} is state-dependent, one should expect that $\beta_{y_{t_1}} \neq \beta_{y_{t_2}}$ and thus a non trivial condition for the covariate x (as it will depend on unobserved parameters β).

Second, though including state-dependent covariates – a list of covariates x_{ijt} that can be different according to the arrival state j – sounds appealing, it is more tricky in practice. By definition, each individual is observed at some given period in only one state, and we do not have any counterfactual values of her covariates in other states that however do appear in the likelihood. For instance, one could use observed values of individuals in other states observed at this date (as Egger et al., 2007 do in their analysis of outsourcing on sectoral employment), but it is not clear in our setting that this would be worth introducing the related measurement error : individuals are not randomly assigned in positions on the labor market, and individuals in some state are not necessarily representative of individuals in other states.

Covariates that can be used in this setting should be time-varying but also present flat episodes. Indeed, the pattern of the growth of the GDP exhibits sharp time-variations and inflection points over our estimation period.

4.3 Data

We use the Labor Force Survey provided by the INSEE, the French National Institute of Statistics and Economic Studies. Quarterly data are available from 2002 to the last quarter of 2010. This dataset is the main French source for official statistics on the labor market and the largest French panel : more than $75,000$ people are interviewed each quarter. Individuals are followed during six quarters and the sample is renewed by sixth every quarter. Detailed information is available on individual characteristics and on career paths as well. It includes usual socio-demographic characteristics (gender, age, education, experience, marital status) and a precise description of the job (seniority, working time, firm size, etc.). We dispose also of details on the type of job contract, which allows us to distinguish between different short-term contracts, in particular FTC (French "CDD") as opposed to TAW (*interim*). Last but not least, since 2006 the French Labor Force Survey has provided a quarterly measure of the amount of overtime work, and we even know whether these hours have been paid or not.

We restrict our sample to individuals aged 18 to 64, from the fourth quarter of 2006 (hereafter 2006Q4) to the fourth quarter of 2010 (2010Q4). This period considered has the advantage of providing both economic upturn and downturn. Finally, as required by the econometric model, we restrict the sample to individuals observed at least four times (but individual length of observations T_i could vary with individuals, with $T_i \in \{4, 5, 6\}$). Our working sample eventually includes $743,929$ observations corresponding to $137,351$ individuals.

We choose to distinguish six states on the labor market, namely open-ended contract, FTC, TAW, unemployment, non-participation and lastly employees working in the public sector. [4] To be precise, we aggregate this last state with self-employed and subsidized jobs : abusing notations we call them hereafter "Public". Such individuals are not very likely to move out and to go through any transition.

Summary statistics can be found in Appendix. At the beginning of 2010, the participation rate in our sample is higher than 73%. Stable employment in the private sector accounts for the largest part of the sample (38.8%), while FTC constitute a much smaller part (2.1%). Workers having a FTC are mainly women,

4. with also employees in *public* fixed-term contracts : they operate under a different legislation than in private sector ; in particular, transitions to open-ended jobs in the public sector is based on competitive examinations and not on professional behavior.

employees and work in services. On the contrary, temporary agency workers are more often unskilled men : 66.5% of temporary agency workers don't have any high school diploma (or the equivalent) and almost 90% of them are either low-qualified workers or clerks (Table 4.6).

4.4 Results

4.4.1 Intensities of transitions on the French labor market

The model presented in Section 4.2 is estimated by weighted likelihood procedure.[5] Recall that we have to normalize coefficients $\delta_{0j} = \delta_{k0} = 0$ to identify transition intensity parameters δ_{kj}. As stated before, we set unemployment as the reference state. As a result, parameters δ_{kj} measure the odds ratio of transiting to state j rather to unemployment, given that one comes from state k rather than unemployment.

TABLE 4.1 – Quarterly labor states intensities of transitions, relatively to unemployment (with GDP growth).

	Open-ended	FTC	TAW	Public	Non participation
Open-ended	3.444^{***} (0.145)	0.277^{**} (0.138)	-0.265 (0.208)	0.416^{*} (0.214)	0.820^{***} (0.123)
FTC	0.926^{***} (0.162)	1.828^{***} (0.106)	-0.339^{**} (0.152)	-0.008 (0.187)	0.335^{***} (0.107)
TAW	0.168 (0.231)	0.213 (0.186)	1.320^{***} (0.122)	-0.883^{**} (0.349)	0.100 (0.170)
Public	0.539^{**} (0.232)	0.303 (0.204)	-0.736^{**} (0.332)	3.068^{***} (0.141)	0.667^{***} (0.111)
Non participation	0.477^{***} (0.129)	0.076 (0.118)	-0.176 (0.184)	0.722^{***} (0.127)	1.480^{***} (0.068)
GDP Growth	0.198^{**} (0.084)	0.162^{**} (0.072)	0.290^{***} (0.091)	0.310^{***} (0.080)	0.068 (0.048)

Significance levels : *** 1%, ** 5%, * 10%.

When controlling for unobserved heterogeneity through fixed-effects and for the business cycle, we still observe a strong inertia with high diagonal terms and it is especially true for workers with an open-ended contract (Table 4.1). Besides,

5. Programs are available on request.

TABLE 4.2 – Quarterly labor states intensities of transitions, relatively to unemployment (without GDP growth).

	Open-ended	FTC	TAW	Public	Non participation
Open-ended	3.527^{***} (0.089)	0.157^{*} (0.092)	0.007 (0.136)	0.470^{***} (0.137)	0.820^{***} (0.081)
FTC	0.874^{***} (0.111)	1.749^{***} (0.069)	-0.289^{***} (0.109)	-0.019 (0.120)	0.321^{***} (0.070)
TAW	0.411^{**} (0.163)	0.268^{**} (0.122)	1.391^{***} (0.081)	-0.128 (0.220)	0.195^{*} (0.108)
Public	0.679^{***} (0.148)	0.364^{***} (0.135)	-0.129 (0.226)	3.127^{***} (0.092)	0.759^{***} (0.076)
Non participation	0.397^{***} (0.084)	0.114 (0.077)	-0.050 (0.112)	0.758^{***} (0.090)	1.461^{***} (0.045)

Significance levels : *** 1%, ** 5%, * 10%.

FTC offers definitively better prospects than TAW. FTC are $e^{.87} \approx 2.4$ more likely than unemployed to have an open-ended contract rather than to be unemployed a quarter later, and this difference is significantly different from 1 at the 5% level. By contrast, this ratio is only $e^{.17} \approx 1.2$ for TAW, and this difference is not significantly different from 1 at usual levels. Moreover, we reject the null hypothesis of equality of these coefficients at 5% level : the Wald statistic related to $H_0 : \delta_{FTC/open} = \delta_{TAW/open}$ is $11.00 > 3.84 \approx q_{\chi^2(1)}(0.95)$.

An empirical issue is to determine the impact of economic growth on the labor status. The position may depend on the economic activity. In accordance with the idea that temporary work is used as a buffer stock, the impact of growth on the probability of being in temporary work rather than unemployed turns out to be positive and strongly significant. By contrast, the economic activity has no significant impact on the probability of being in permanent position rather than unemployed. Note that we reject the joint hypothesis of nullity $H_0 : \beta = 0$ at usual levels : the Wald-statistic is 28.63 (it has to be compared to a $q_{\chi^2(5)}(0.95) \approx 11.07$ under the null).

Besides, one could ask whether the exclusion of economic activity as an explanatory variable has an impact on state dependence estimates. We thus compare the estimates for quarterly transitions on the labor market with and without controlling for the growth of the GDP. The estimate without covariate corresponds to the estimator proposed by Magnac (2000). Interestingly, estimates in this case lead to a different diagnosis. For instance, ignoring the impact of GDP growth overestimates the intensity of transitions from TAW to open-ended relatively to unemployment, as we reject the assumption that temporary agency worker has the same chance than unemployed to get permanent position rather to be unemployed the following quarter at 5% level (Table 4.2). One could expect that a "omitted-variable bias" occurs particularly for this specific transition, as this type of employment crucially depends on the business cycle. Note that the difference is not statistically significant at usual level, however (we perform comparison of the two sets of estimates corresponding to the state dependence by block-bootstrap).

4.4.2 Overtime work as a signaling device ?

Engellandt & Riphahn (2005) or Dolado & Stucchi (2008) emphasize that the use of temporary contracts by firms as a screening device could induce temporary workers to do more "effort" in order to signal higher productivity. Our dataset enables us to investigate this issue. Indeed, we have a measure of overtime work provided by all workers. More specifically, we know whether an employee has done overtime work during a so-called "reference week" specific to each individual. The evolution of the proportion of FTC workers doing more overtime work turns out to be negatively correlated with the business cycle over the period (Figure 4.1). This fact is consistent with the idea of temporary employment as a screening device, FTC workers being induced to signal their productivity by providing more effort, which is all the more plausible that the chance of getting another job is weak.

Hence we analyze whether the fact of doing extra work has an impact on the transition from temporary work to permanent position. FTC workers doing overtime work have 14.3% chances of getting an open-ended contract the following quarter, which is the highest rate among other departure states at the exclusion of open-ended jobs. This rate is indeed almost twice as high as the one by those not doing overtime work (8.3%) ; the difference is significant under the null at 5%. Such a difference is much weaker for temporary workers. However, these estimates control neither for observable nor for unobservable characteristics that are likely to matter in such a transition on the labor market.

TABLE 4.3 – From temporary employment to open-ended contracts (Cross-sectional Logit)

	FTC	TAW
Intercept	-4.20^{***} (.71)	-3.62^{***} (1.12)
Overtime work	$.66^{***}$ (.15)	$.46^{*}$ (.24)
Women	$-.06$ (.14)	$-.16$ (.23)
Men	*Ref.*	*Ref.*
Age	.054 (.038)	.062 (.064)
Age^2	$-.00089^{*}$ (.00052)	$-.00076$ (.00088)
Blue collars	1.02^{***} (.35)	-1.15^{*} (.61)
Clerks	1.06^{***} (.33)	$-.71$ (.63)
Intermediates	1.02^{***} (.33)	$-.97$ (.63)
Executives	*Ref.*	*Ref.*
Agriculture	$-.48$ (.44)	. (.)
Manufacturing	.22 (.20)	$-.39$ (.24)
Construction	$-.019$ (.32)	$-.89^{**}$ (.41)
Services	*Ref.*	*Ref.*
No diploma	*Ref.*	*Ref.*
High school diploma	$-.30^{*}$ (.17)	$.53^{**}$ (.24)
University diploma	.08 (.17)	.28 (.30)
Growth	.02 (.09)	$-.12$ (.13)
Number of observations	3,608	1,786

Source : LFS 2006-2010.

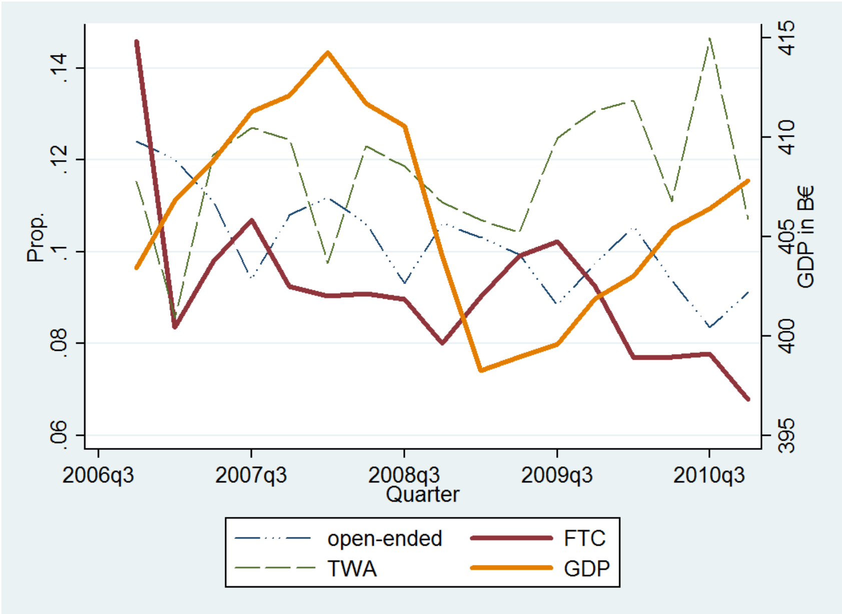


FIGURE 4.1 – Overtime work by labor market status and GDP

Controlling for observable characteristics such as gender, education, age, industry, occupation and growth, we observe a positive and significant impact of overtime work on transitions for both FTC and TAW (Table 4.3). The business climate does not appear to significantly impact these transitions. As stated before, this does not correct for potential unobserved individual heterogeneity.

In order to measure the impact of doing overtime on the transition from temporary work to permanent position, we use the same model with fixed effects as before. Depending on whether workers declare doing overtime or not, we split each state corresponding to FTC and TAW into two new states. We thus measure whether overtime helps getting a permanent transition. We compare transitions from temporary work to stable employment with or without overtime work. Note that it could appear more intuitive and appealing to use the fact of doing overtime work as an explanatory variable. However, we choose not to use directly this dimension as a time-varying covariate because of the arguments stated in Section 4.2.2 : the impact of doing effort before on current position on labor state is a state-dependent variable. We measure it for employees at work, but it is much difficult to attribute a counterfactual propensity of doing overtime to unemployed for instance. Its impact could not be identified by the previous estimator.

If previous cross-sectional evidence suggests such an impact especially for FTC workers, this result seems mostly influenced by individual heterogeneity. The intensity of the transition from temporary work to permanent position is slightly higher for FTC workers doing extra work than for FTC worker who don't, but the difference is not significant. The Wald statistic related to the assumption $H_0 : \delta_{FTC_{noeff}/open} = \delta_{FTC_{eff}/open}$ (resp. $H_0 : \delta_{TAW_{noeff}/open} = \delta_{TAW_{eff}/open}$) is only 0.31 (resp. 0.37), where $\delta_{FTC_{noeff}/open}$ (resp. $\delta_{FTC_{eff}/open}$) denotes the transition intensities (relatively to unemployment) from workers in fixed term contract having not done overtime work (resp. having done overtime work) to open-ended job, and $\delta_{TAW_{noeff}/open}$ and $\delta_{TAW_{eff}/open}$ their counterparts for temporary agency work. The difference in point estimates is more pronounced for TAW, but still not significant at usual level.

Table 4.4 – Quarterly labor states intensities of transitions (impact of doing overtime work), relatively to unemployment.

	Open-ended	FTC (no overtime work)	FTC (overtime work)	TAW (no overtime work)	TAW (overtime work)	Public	Non participation
Open-ended	3.447^{***} (0.143)	0.264^{*} (0.140)	0.442 (0.354)	−0.211 (0.220)	-0.825^{*} (0.440)	0.421^{**} (0.212)	0.819^{***} (0.123)
FTC (no overtime work)	0.915^{***} (0.165)	1.785^{***} (0.109)	2.133^{***} (0.240)	-0.292^{*} (0.171)	−0.552 (0.365)	−0.011 (0.195)	0.335^{***} (0.110)
FTC (overtime work)	1.111^{***} (0.354)	2.014^{***} (0.214)	2.319^{***} (0.371)	-0.673^{*} (0.367)	−0.749 (0.554)	−0.023 (0.413)	0.202 (0.295)
TAW (no overtime work)	0.154 (0.235)	0.255 (0.210)	−0.189 (0.510)	1.461^{***} (0.134)	0.858^{***} (0.223)	-0.883^{**} (0.353)	0.048 (0.175)
TAW (overtime work)	0.425 (0.545)	0.259 (0.458)	1.322 (1.178)	1.078^{***} (0.221)	0.949^{***} (0.359)	−0.959 (0.771)	0.356 (0.479)
Public	0.539^{**} (0.231)	0.311 (0.211)	0.246 (0.507)	-0.737^{**} (0.356)	−0.359 (0.824)	3.070^{***} (0.140)	0.667^{***} (0.111)
Non participation	0.483^{***} (0.129)	0.036 (0.119)	0.433 (0.330)	−0.098 (0.193)	−0.731 (0.528)	0.725^{***} (0.126)	1.481^{***} (0.067)
GDP Growth	0.197^{**} (0.083)	0.163^{**} (0.072)	0.076 (0.146)	0.309^{***} (0.094)	0.091 (0.185)	0.308^{***} (0.080)	0.066 (0.048)

Significance levels : *** 1%, ** 5%, * 10%.

We perform several robustness checks in relation with the measure of "effort". First, we investigate whether a more precise measure for overtime work change results. The mere fact of doing overtime is a mixed signal : it could be due to a specific boost in the activity of the firm, or to a signaling behavior from the employee. One could expect that paid overtime corresponds to the former, while unpaid overtime work is more related to the latter. The relevant measure of effort could be unpaid overtime work and we thus split FTC and TAW states accordingly. Previous results do not vary. We find that the fact of doing unpaid overtime work is small and not significant for workers with a FTC once unobserved heterogeneity has been taken into account. For TAW, the difference is higher ($e^{1.48-.34} \approx 3.1$) when considering point estimates, but not significant for unpaid overtime.

Second, one could ask whether the absence rate can be a proxy for effort, in line with results obtained by Engellandt & Riphahn (2005) showing that temporary workers in Switzerland are less absent than workers in open-ended contract. In France, this measure does not seem relevant since leave rights directly depend on seniority. As a result, temporary workers have a low probability of being entitled to take such a leave. Concerning sick leave, compensations might depend on the type of labor contract. The French public social insurance provides a daily indemnity that amounts to less than the corresponding wage. There is also a deductible for the first three days. Employees can nevertheless get additional compensation depending on collective agreements. The actual compensation rate could depend on industry, but also on experience and status. Employees with an open-ended contract are more likely to have a better coverage against health risks than temporary workers. Indeed, we observe that FTC workers being absent the last quarter are more likely to be in open-ended contracts, the difference not being significantly different from zero. The difference is higher (and with the expected negative sign) for TAW, but the difference is still not significant.

4.5 Concluding remarks

To conclude, this paper contributes to the analysis of labor market transitions. Our results suggest that fixed-term contracts lead more often to stable employment than temporary agency work, not doing significantly better than unemployment to this regard. Besides, we show that neglecting economic climate can change alter the state-dependence estimates. Finally, we do not find a significant impact of doing overtime work on transitions to open-ended positions for short-term contracts, once heterogeneity has been taken into account.

Appendix A : Sample Description

TABLE 4.5 – Labor market status (%)

Participation rate :	72.5
open-ended	39.1
unemployed	6.5
fixed-term contract (FTC)	2.8
temporary work agency (TAW)	1.2
others :	22.9
public sector	13.9
self-employed	7.9
subsidized jobs	1.1
Non-participants	27.0

Source : LFS 2006 − 2010.
$N = 137,351$ individuals (743,929 observations)

TABLE 4.6 – Summary statistics (%)

	$open - ended$	FTC	TAW
Women	45.9	58.4	30.9
Less than high school diploma	54.9	50.3	66.5
High school diploma	17.9	23.4	19.6
More than high school diploma	27.3	26.3	13.9
15-29	5.5	29.1	28.3
30-49	26.6	14.4	10.1
50-64	67.9	56.5	61.6
Blue-collars	28.8	29.0	75.3
Clerks	30.6	43.7	14.4
Intermediates	24.3	18.4	9.3
Executives	16.2	8.9	1.1
Number of individuals	63,604	3,808	1,786

Source : LFS 2006 – 2010

Chapitre 5

Does the Minimum Wage revaluation impact the earnings distribution ? An Unconditional Quantile Regression Approach[1]

5.1 Introduction

France, as many other OECD countries, has had a statutory minimum wage, the "SMIC", since 1970. Its goal is to boost the wages of lower skilled workers and improve equity.[2] Numerous contributions were written concerning the employment effects of the minimum wage on low-skilled workers. If we limit ourselves to the French case, Kramarz & Philippon (2001) study for instance the impact of minimum wage on lay-offs for employees directly affected by an increase in the minimum wage. From another perspective and with a more parametric approach, close to that of Meyer & Wise (1983), Laroque & Salanie (2002) decompose the "non-employment" in France, with the minimum wage as an important determinant. Less evidence is available in the French case on its impact on the whole distribution of earnings. Yet the existence of potential "spillover" effects of the minimum wage on higher wages is regularly used in the public debate (in particular during discussions concerning the annual revaluation of the legal minimum wage

1. This chapter is co-authored with Romain Aeberhardt (DARES-CREST) and Claire Marbot (INSEE-CREST).

2. The French law stated that the minimum wage should

> *ensure that employees with the lowest wages, have a guaranteed purchasing power, and participate in the Nation's economic development.*

level).

Several arguments have been advanced in the economic literature to explain a possible divergence between productivity and wages that could create this spillover effect, apparently inconsistent with classical economic theory. For instance, the respective bargaining powers of the employee and employer (monopsony, see *e.g.* Dickens, Machin & Manning 1999), or the existence of information asymmetries (efficiency wage). The existence of an upward compensation scheme can also be a way to stimulate the efforts of the employees. In line with the model of tournament proposed by Rosen (1986), Chen & Shum (2010) find that a large part of intra-firms wage differentials could be interpreted as an incentive tool. This intuition is also supported by evidence on experimental data. According to Falk et al. (2006), the minimum wage would have a substantial impact on the employees' reservation wage ; at the equilibrium this explains the presence of significant wage increases, even beyond the level imposed by the minimum wage. The minimum wage could therefore modify the agents' perception of the "fair" level of remuneration.

On empirical grounds, Teulings (2000, 2003) finds that increases in the minimum wage significantly spread to higher wages in the Netherlands. Similarly, Dickens et al. (1999) use the coexistence of several sectoral minimum wages in the United Kingdom (until 1993), and find effects of the minimum wage up to the 40th percentile. However, Dickens & Manning (2004) believe that *spreading* effects were probably modest in the United Kingdom in 1999 when a national minimum wage was reintroduced. More anecdotally, in a famous article, Katz & Krueger (1992) study the fast food industry in New Jersey and Pennsylvania and highlight positive effects on employment, despite the rise of the minimum wage. Lee (1999) looks at the contribution of the sharp decrease of the minimum wage (in real terms) to the explosion of wage inequality observed since the eighties in the US. He uses the difference between the minimum wage and the median wage as a measure of inequality. French studies on the impact of the minimum wage on wages are less numerous. The main contribution is that of Koubi & Lhommeau (2007), who conclude that increases in the minimum wage have a significant impact up to wages as high as twice the minimum wage.

This study aims at providing new empirical stylized facts regarding the spillover impact of the minimum wage on the wage distribution during the last decade in France. This analysis is complicated. First, as noted by Lee (1999), the already mentioned impact on employment can distort the overall wage distribution of actually employed individuals. [3] It would thus be difficult to separate what stems from this "mechanical" impact on the distribution from real spillover effect. Second, the usual French rule for the legal increase in the minimum wage made it highly endogenous, as it is yearly adjusted according to the past trend in mean wages (it is indexed to the *blue-collar worker's basic hourly pay*). Third, since the minimum wage is usually the same for all employees, it is impossible to distinguish what pertains to its specific increase or to any wage trend or other cyclical effect. However, we use the specific period 2003-2005, when the rule for increase in the level of the minimum wage was frozen in order to harmonize the different levels of minimum wages resulting from the gradual implementation of the French (left-wing) Law on working time reduction between 1998 and 2003. It has indeed resulted in the coexistence of several levels of the minimum wage (there were six of them in 2003). Monthly Guaranteed Wages (GMR) were designed to maintain the monthly wage of the lowest paid employees despite the lower number of hours worked at the time of the switch to the 35-hour week. Each year, during five years, a new GMR was created for the firms which signed the agreement that year. The new (right-wing) government elected in 2002 decided to put an end to this situation, and different discretionary increases were applied to the different GMR between 2003 and 2005. This situation provides a natural experiment for an evaluation of the impact of the minimum wage on the wage distribution, as the pace of increase in the level of the minimum wage applying in one or other firm could be considered as exogenous.

We focus on the impact of the minimum wage on the various deciles of the distribution of annual earnings of workers in the private sector, using administrative business data (the DADS) that provide exhaustive data on yearly earnings of French workers in the private sector. To disentangle as much as possible the spillover effects from changes in the composition of the labor force resulting from the

3. If we denote by F_1 (resp. F_0) the earnings distribution with a minimum wage of $\underline{w}_1$ (resp. $\underline{w}_0$), we get $F_1(w) = \frac{F_0(w)-F_0(\underline{w}_1)}{1-F_0(\underline{w}_1)}$ for $w > \underline{w}_1$ (zero otherwise). First-order development gives the expression of the τ^{th} quantile of the distribution F_1 related to its counterpart for the distribution F_0 as : $q_{1\tau} = q_{0\tau} + (1-\tau)\frac{F_0(\underline{w}_1)}{f_0(q_{0\tau})}$.

exclusion of low-productivity workers, we rely on a method that allows us to control for the impact of observable characteristics. Since then, over the last decade, many tools have been proposed for a more detailed analysis of the entire wage distribution. Quantile regressions, which have become more common in the recent years, provide means to study the impact of certain determinants on different quantiles of the wage distribution. These methods are in fact quite old (see Koenker & Hallock 2001), but they have grown a lot recently. We rely in this study on the method proposed by Firpo, Fortin & Lemieux (2009). It allows us to directly estimate the impact of a marginal change in the minimum wage level throughout the overall wage distribution, without changing the distribution of other (observable) characteristics. Besides, we restrict our sample to workers present in the labor force two years in a row. As several evidences suggest different wage settings for French male and female workers, we perform separate analysis for men and women. Our results suggest significant effects of a change in the minimum wage level up to the seventh decile for male workers in the private sector, but not for female workers beyond the first decile. In any case, the magnitude of these effects appears small.

Section 2 presents the revaluation mechanisms of the minimum wage with a specific focus on the *convergence* period of the different levels of the minimum wage. The data along with some descriptive statistics is detailed in section 3, then the statistical method and finally the results.

5.2 The revaluation rules of the minimum wage

The revaluation of the minimum wage is indexed to the increase in the consumer price index and to half the annual increase in the *purchasing power of blue-collar worker's basic hourly pay* (SHBO).[4] The introduction of laws regarding working time reduction made the revaluation of the minimum wage more complex. The principle of a monthly guaranteed wage (hereafter GMR) was to maintain the monthly wage of employees initially paid at the minimum wage at the time of the switch to the 35-hour week. Each year from 1998 to 2002, a new GMR was created according to the level of the hourly minimum wage of this year (see Appendix 5.5 for the precise creation calendar of the different GMR).

4. A discussion of the consequences of this mechanism can be found in Cette & Wasmer (2010).

However, if they guaranteed a maintained monthly remuneration at the time of the switch to the 35-hour week, the GMR did not then follow the same revaluation rules as the hourly minimum wage (SMIC). Until July 2002 they were adjusted according to changes in the blue-collar worker's basic *monthly* (SMBO) and not *hourly* (SHBO) pay. Since the monthly wage evolved slower than the hourly wage over the period (number of RTT agreements ensured the maintenance of a monthly salary, which mechanically translated into an increase in the hourly wage), the different GMR therefore benefited from lower revaluations than the minimum wage over the period.

The different levels of minimum monthly earnings therefore gradually diverged, while at least six different levels of the minimum wage coexisted (an illustration can be found for example in Koubi & Lhommeau 2007). We denote by FGMRi hereafter the set of firms that signed a working-time reduction agreement at a time when the guaranteed monthly wage was GMRi. In practice, this means that the relevant yearly minimum wage in all firms of this group is GMRi.

The nominal level of the minimum hourly wage which was to be used in a given company that had signed an agreement at date T_g can be written :

$$SMIC_{gt} = SMIC_{gt-1}\left(p_t/p_{t-1} + \frac{1}{2}\delta_{SHBO_t}1_{t\notin[T_g,2005]} + \frac{1}{2}\delta_{SMBO_t}1_{t\in[T_g,2003]} + cdp_{gt}\right)$$

where δ_{SHBO_t} corresponds to the growth rate in the purchasing power of blue-collar worker's basic hourly pay and cdp_{gt} represents the discretionary increase beyond the automatic revaluation rule (*boost*). The latter may be the same for all groups (the SMIC received a *boost* of 0.45 % on July 1, 1998, of 0.29 % on July 1, 2001 and of 0.30 % on July 1, 2006), but it can also be different, as was the case at the time of the application of the Fillon Law between 2003 and 2005. On July 1, 2002, the different revaluation mechanisms of the GMR had resulted in large differences in their levels (see Table 5.1).

From 2002 to 2005, an adjustment mechanism was put into place in order to retrieve a unique level of the minimum wage. During this period, the traditional revaluation rule of the minimum wage was frozen. While the hourly wage of GMR 5, which was the highest (it corresponded to firms that had signed an RTT agreement between July and December 2002), simply evolved as the inflation, the other GMR and the SMIC received differential *boosts* so as to arrive in 2005 at a unique hourly rate : the more they initially diverged the higher the *boosts* were during this period.

TABLE 5.1 – Level and evolution of real annual GMR (2002-2005)

	FGMR 0	FGMR 1	FGMR 2	FGMR 3	FGMR 4	FGMR 5
Level of the GMR in real terms (2006 euros)						
2002	13,611	14,463	14,643	14,894	15,078	15,167
2003	14,041	14,629	14,750	14,918	15,041	15,100
2004	14,543	14,849	14,911	14,995	15,058	15,087
2005	15,076	15,068	15,068	15,068	15,068	15,068
Evolution of the GMR in real terms (%)						
2002-2003	3.2	1.1	0.7	0.2	-0.2	-0.4
2003-2004	3.6	1.5	1.1	0.5	0.1	-0.1
2004-2005	3.7	1.5	1.1	0.5	0.1	-0.1

Source : DADS panel, 1/25th sample.
Field : employees from the private sector aged 18 to 65, without interns and apprentices, working full-time, full-year, in the same firm for two consecutive years.

5.3 The Data and descriptive statistics

5.3.1 The Data

We use the DADS panel (1/25th sample) over the period 2003-2005. This administrative business database starts in 1976 and provides the gross earnings of all workers in the private sector except for agricultural workers. [5] We restrict the sample to full-time employees aged 18 to 65, having worked all year in the same firm, and who were already in the same firm during the previous year. This last choice is explained firstly by that of the variable of interest. Indeed, the number of hours worked is available in the DADS as of 1994 but the quality of this variable

5. We limit the sample to companies and exclude private individuals

is sometimes questionable. The reconstruction of an hourly wage would thus be difficult. To reduce these measurement problems, we choose to estimate the distributions of gross annual earnings (expressed in euros of 2006). For these annual amounts to be comparable, we must limit ourselves to full-time employees who worked full year.

Besides, as we are interested in the first place by the spillover effects of an increase in the minimum wage, we restrict the sample to employees who worked in the same firm the preceding year. This restriction reduces potential employment effects. Indeed, one should in theory jointly model participation and wages. Meyer & Wise (1983) for instance propose such an approach (ignoring spillover effects), but this result appears quite sensitive to the choice on the parametric assumptions (see Dickens et al. 1998 for a discussion). This sample restriction reduces changes due to employment variation. Moreover, it has the subsidiary advantage to let us correct for a few potential errors in the declared wages through a careful analysis of the data concerning two consecutive years (see Appendix 4.3 for details). We check that the earnings distribution is not affected by these sample restrictions (see Table 5.7 in Appendix B). In the end, we have a panel of over 85,000 firms (among which about 61,000 were present between 2003 and 2005) and around 220,000 employees. [6]

5.3.2 Characteristics of the firms and wage evolution according to the GMR group

As detailed above, because of the successive RTT agreements, six levels of GMR coexisted between July 2002 and July 2005. Beside the five GMR, the hourly minimum wage (which by abuse of language will be denoted by GMR 0 in the following) applied for firms which did not sign any RTT agreement. More than half of the firms were in this case, but these were mainly small firms, so that the hourly minimum wage concerned only a third of all employees (Table 5.2). Very few firms signed RTT agreements after mid 2002, as evidenced by the low number of employees belonging to FGMR 5 in our sample.

6. The original panel comprised 120,000 firms and 330,000 employees.

TABLE 5.2 – Number of firms and employees for each GMR group in 2003

	FGMR 0	FGMR 1	FGMR 2	FGMR 3	FGMR 4	FGMR 5	Total
Firms							
Number	42,431	2,242	11,593	6,954	9,584	7,290	73,533
Share (%)	57.7	3.1	15.8	9.5	13.0	1.0	100
Employees							
Number	64,896	10,716	62,551	30,215	15,405	1,433	185,216
Share (%)	35.0	5.8	33.8	16.3	8.3	0.8	100

Source : DADS panel, 1/25th sample.
Field : employees from firms of the private sector aged 18 to 65, without interns and apprentices, working full-time, full-year, in the same firm for two consecutive years.

The signing date of an RTT agreement is obviously not exogenous : in fact, the law was more restrictive for larger firms, which explains why they are overrepresented among those who contracted an agreement soon. Thus, while three-quarters of the firms with fewer than 10 employees (and 63 % of the firms with 10 to 49 employees) had not signed an agreement in June 2002 (FGMR 0 and 5), it was the case for only 7 % of the firms with more than 5,000 employees. In addition, 68 % of the firms with more than 5,000 employees had signed an agreement before June 30, 2000 (FGMR 1 or 2) and 92 % before June 30, 2001 (FGMR 3), while for firms with 50 to 499 employees, the respective proportions are 41 % and 59 %. Ultimately, in our sample, nearly 60 % of the firms belonging to FGMR 1 and 2 employ more than 50 employees while this is the case for only 13 % of the firms from FGMR 4 and 5 (Table 5.3).

In terms of business sector, in accordance to what is observed with the size, industry is over-represented in FGMR 1, 2 and 3 while construction, agriculture and trade belong more often to GMR 0 (no agreement signed) or FGMR 4 and 5 (late signing date).
As another index of dissimilarity between firms, note that women account for 28 % of employees in firms which did not sign any RTT agreement, and 32 % in those in FGMR 4. Recall that we restrict ourselves to full-time employees, which explains

the difference with usual gender ratios.

Table 5.3 – Characteristics of firms in each GMR group (2003 to 2005)

	FGMR 0	FGMR 1	FGMR 2	FGMR 3	FGMR 4	FGMR 5	Total
Number of employees (in %)							
less than 10	38.6	6.8	3.5	9.4	36.8	37.7	29.6
10 to 49	47.6	36.0	36.3	41.9	51.0	42.4	45.5
50 to 499	13.2	49.5	51.5	42.8	11.3	18.2	22.4
500 to 4999	0.6	7.4	8.2	5.5	0.9	1.7	2.5
over 5000	0.0	0.4	0.4	0.3	0.0	-	0.1
Gender							
Share of women	27.7	31.4	30.9	30.2	29.1	31.9	29.5
Business sector							
Agriculture	1.4	1.4	1.0	0.4	1.1	1.3	1.2
Industry	21.5	37.2	34.5	37.5	25.9	23.8	26.0
Construction	17.4	10.1	8.4	8.3	14.8	12.8	14.6
Trade	25.7	21.3	21.1	25.6	24.7	27.4	24.8
Services	34.0	30.0	35.0	28.2	33.5	34.7	33.4
Wages							
Median (2005)	25,748	26,451	27,009	27,640	23,843	24,183	26,322

Source : DADS panel, 1/25th sample.
Field : employees from firms of the private sector aged 18 to 65, without interns and apprentices, working full-time, full-year, in the same firm for two consecutive years.

Firms are also very different in terms of wages. For median wages there is an opposition between FGMR 4 and 5 with a median annual gross wage of about 24,000 euros (of 2007), and FGMR 1 to 3 for which it amounts to about 27,000. For firms that never signed any RTT agreement (FGMR 0), the median annual gross wage lies in between.

A direct comparison between the wages in firms in different GMR groups also shows significant discrepancies (Figure 5.1). Wages are lower in FGMR 4 and 5 : for example the top decile in firms belonging to FGMR 4 corresponds to 85 % of that of firms from FGMR 0. Conversely, firms from FGMR 3 have the highest wages. The median wage in this group is 9 % higher than in firms that did not sign any RTT agreement.

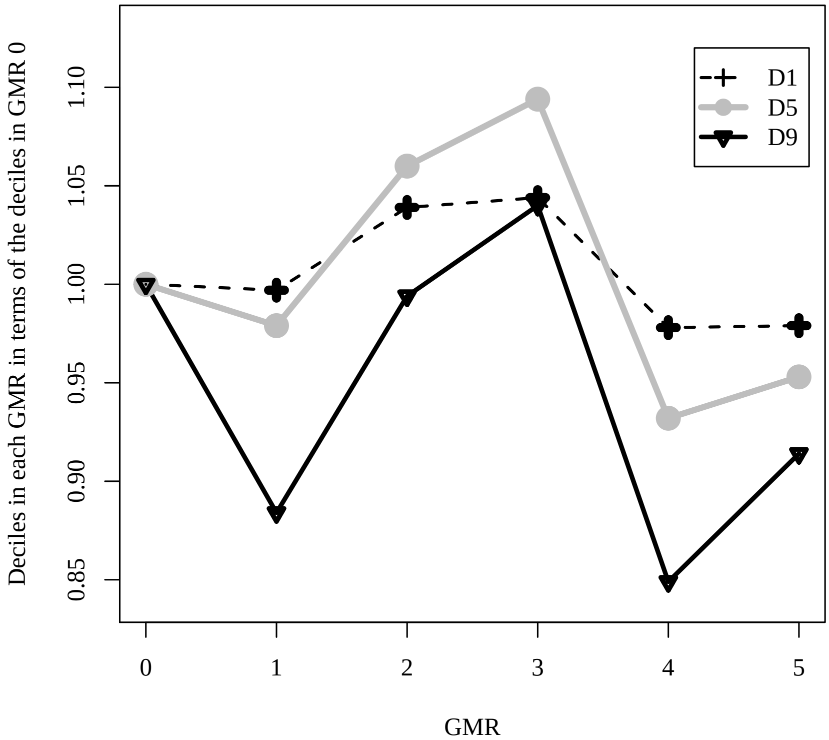


FIGURE 5.1 – Deciles in FGMR (relatively of level in FGMR 0)
Sources : DADS panel, 2003
D1 represents the first decile.

One could question whether different growth rates in the minimum wage level result in changes in employment. An informal test is performed by comparing workforce evolution between 2003 and 2005, for each FGMR. Recall that the increase in the applied minimum wage in FGMRi is all the more important that the indice i is smaller. One could thus expect that the employment level would have increased at a slower rate in firms belonging to FGMR0 than in firms belonging to FGMR5. We do not observe such pattern in our data. Restricting to firms present from 2003 to 2005, we observe a general increase between 2003 and 2004 and a decrease the following year (Figure 5.2), but no monotonic relation with the GMR level.

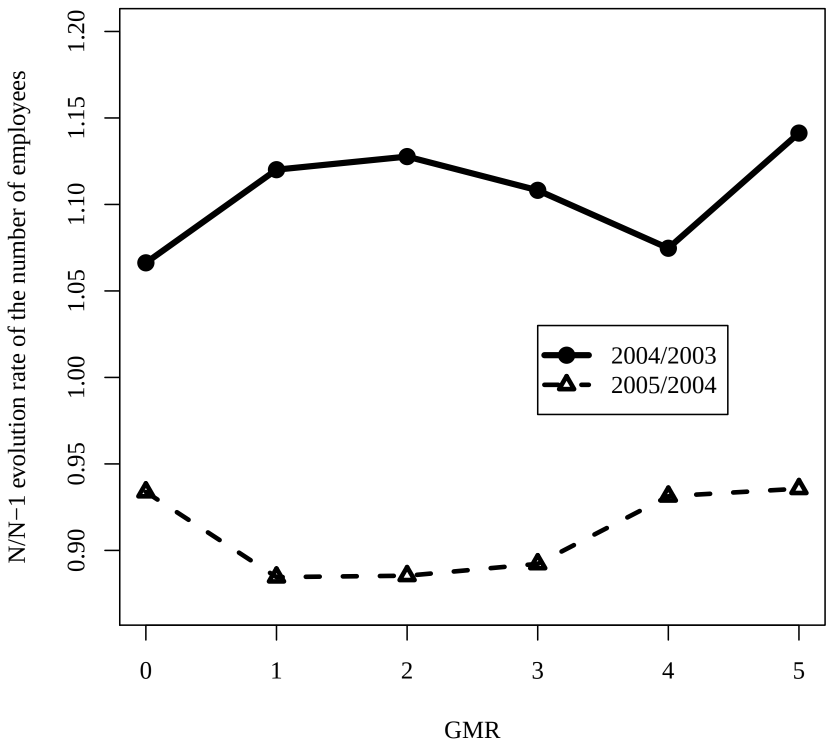


Figure 5.2 – Evolution of the number of employees in the firms according to FGMR

Sources : DADS panel, 2003-2005

5.4 Econometric Method

Our aim is to estimate the potential spillover effect of an increase in the minimum wage level over the whole distribution of earnings. Our underlying setting states that the individual wage is determined by characteristics of the employee (age, gender, qualification...) and of the firm (business sector, size...), but also partly by the minimum wage applying in this firm. This could be the case if the firm tries to maintain a certain wage hierarchy, for instance as an incentive for their employees (consistent with the wage setting in a tournament model). Very generally, we assume the following relation between wages and characteristics :

$$w_i = \phi(\underline{w}, X_i, \epsilon_i) \tag{5.4.1}$$

$\underline{w}$ stands for the minimum wage level, X_i for observed characteristics of the firms or the employees, and ϵ_i for potential unobserved characteristics (for instance productivity). Our empirical question is how the earnings distribution changes with an increase in the minimum wage level, keeping everything else equal.

As stated before, this evaluation is difficult, because the minimum wage is the same for all employees, and it is impossible to distinguish what pertains to the increase in the minimum wage from what pertains to any trend. In addition, the revaluation rule of the minimum wage according to the basic wage of a worker is a natural source of endogeneity. Indeed, we could assume that the unobserved determinants of wages, as productivity, will increase this observed basic wage. As a result the minimum wage level is not independent of this unobserved component. An estimation procedure has to take endogeneity into account.

In this context, the convergence period of the different levels of the minimum wage provides an interesting natural experiment because it is characterized by the coexistence of several levels of the minimum wage with a different pace of convergence over the period. During this period, compensation schemes were subject to large increases but with a variable intensity depending on the signing date of an RTT agreement. The revaluation rule following the evolution of the SHBO was suspended. Although it is likely that in the early years, for a given firm, the decision to sign an agreement was not independent from the anticipated evolution of the compensation scheme, the convergence process was then imposed without being anticipated. Therefore it seems plausible to consider this as a source of exogenous variation in the levels of the minimum wage. More specifically, we assume that :

$$\underline{w_{it}} \perp\!\!\!\perp \epsilon_{ijt}|X_{ijt}, e_t, e_{FGMRj} \tag{5.4.2}$$

where e_t represents temporal dummies, and e_{FGMRj} dummies for being in a firm where the minimum wage level corresponds to GMR level j.

We thus analyze how the wages of individuals are affected by the level of the minimum wage in the firm employing them, controlling for the main characteristics that may also influence the wages. Since the effects are very likely to differ depending on the position in the wage hierarchy, the goal is to go beyond the average effects to study the impact on the overall distribution. In recent years, available methods to evaluate counterfactual distributions have emerged (a detailed presentation can be found in Fortin, Lemieux & Firpo 2010).

The method, which is used here, is the so called *unconditional* quantile regression proposed by Firpo, Fortin & Lemieux (2009). The distinction with the *conditional* quantile regression has to be highlighted, and it is useful to do so by comparison with the case of a standard regression. Regression in the usual sense of the term consists in explaining a micro-economic variable –here the wage w– by a set of explanatory variables X which can be micro or macroeconomic, continuous or discrete. What is modeled is the expected value of the wage w knowing the values of these different variables. Modeling the expected value has a significant advantage in terms of interpretation : because of its linearity property, the impact of a change in the distribution of an explanatory variable on the average wage of the whole population is simply obtained as the average effect of this variable on different individuals of the population. [7]

The *conditional* quantile regression is based on the same principle, but in this case,

7. For example, if the goal is simply to measure the effect of being an executive rather than a worker, a standard regression will estimate $\beta = E(W|SC = \text{executive}) - E(W|SC = \text{worker})$. This coefficient also corresponds to the impact on the mean wage over the entire population of a slight change in the proportion of executives p relative to that of workers. Indeed : $E(W) = p_{exec}E(W|SC = \text{executive}) + (1 - p_{exec} - p_{tech} - p_{sw})E(W|SC = \text{worker}) + p_{tech}E(W|SC = \text{technicians}) + p_{sw}E(W|SC = \text{service workers})$ and therefore a marginal change of p_{exec} corresponds to $\beta = E(W|SC = \text{executive}) - E(W|SC = \text{worker})$.

the objective is to model a quantile of the distribution of W knowing the value of these variables, rather than the expected value of W. For the quantile of order τ, we write for instance the model as :

$$q_\tau(w|X) = g_\tau(X) \tag{5.4.3}$$

The result can tell, for example, the difference between what an executive or a worker have $(1-\tau)$ chances of earning. However, contrary to the previous case, this does directly provide information on how a marginal change in the level of one variable (in our case, the minimum wage), keeping the distribution of other characteristics equal, will affect the level of the τth order quantile of the distribution of wages in the population.

The estimator of Firpo et al. (2009) is a measure of what they call the "unconditional partial effect". More precisely, they are interested in the impact of a small location shift in the distribution of covariates X, from F_X to G_X, on some distributional statistic of a variable W, maintaining the conditional distribution of W given X unaffected.
Firpo et al. (2009) prove that this impact depends on the integration of the so-called recentered influence function over the difference of distributions G_X to F_X (details are provided in Appendix 5.5). [8] More precisely, it can be shown in the special case of a small location shift in the distribution of the covariates X that the vector of partial derivative α of the change in the statistic ν of the distribution of W is :

$$\alpha(\nu) = \int \frac{dE(RIF(W,\nu)|X=x)}{dx} dF(x) \tag{5.4.5}$$

In the case of a τth order quantile, the RIF is notably simple. One can indeed show that :

$$E(RIF(Y, q_\tau)|X=x) = q_\tau + \tau F_Y'^{-1}(\tau) + F_Y'^{-1}(\tau) P(Y > q_\tau | X = x). \tag{5.4.6}$$

8. By definition, the influence function of observation y is defined as

$$IF(y; \nu, F_Y) = \left.\frac{\partial \nu(F_{Y,t\Delta_y})}{\partial t}\right|_{t=0} = a(y) \tag{5.4.4}$$

It provides a measure of how ν changes when the distribution slightly changes towards the value y_i taken by the variable of interest. It can also be interpreted as the influence of an observation i on the empirical estimation of the distribution parameter $\nu(F_Y)$. The recentered influence function (RIF) is defined as $RIF(y_i; \nu, F_Y) = \nu(F_Y) + IF(y_i; \nu, F_Y)$

q_τ and $F_Y'^{-1}(\tau)$ are constant and independent of X and can be estimated.[9] We note $c_{1,\tau} = F'^{-1}(\tau)$ and $c_{2,\tau} = q_\tau + \tau F'^{-1}(\tau)$

We focus here on changes in the deciles dec_j, $j \in [1, 9]$:[10]

$$
\begin{aligned}
RIF(y_i; dec_j, F_Y) = & c_{1,dec_j} P(y_i > dec_j | X = x) && (5.4.7)\\
& + c_{2,dec_j} && (5.4.8)
\end{aligned}
$$

Once specified the dependence of $P(Y > q_\tau | X = x)$ in x, the impact on $\nu(F_Y)$ of a modification in X is obtained by differentiation of $P(Y > q_\tau | X = x)$ with respect to X and integration over the distribution of X.

In practice, we use the method designed as RIF-Logit in Firpo, Fortin & Lemieux 2009, that specifies a logit specification for $P(Y > q_\tau | X = x)$. We thus apply a two-step procedure that consists in :

1. Estimating the probabilities $\hat{T}_{idec_j}$ from the Logit specifications for $P(Y > dec_j | X = x)$;
2. Estimating the average impact of one covariate X_k on the decile j as :

$$
\frac{\partial}{\partial X_k} RIF(y_i; q_{j/10}, F_Y) = \hat{c}_{1dec_j} \hat{\beta}^k_{dec_j} \frac{1}{N} \sum_i \hat{T}_{idec_j} (1 - \hat{T}_{idec_j})
$$

Proofs of the convergence of this estimator can be found in Firpo, Fortin & Lemieux (2009). Confidence intervals are obtained by bootstrap.

The estimated coefficients of the model can then be interpreted directly in terms of effects of each variable on the overall quantile as was the case with the classical regression. More precisely, if $w_i = \phi(\underline{w}, X_i, \epsilon_i)$, it can be shown under assumption

9. The former is provided by standard software and the latter can be approximated by an infinitesimal change around the decile $\frac{F^{-1}(\tau+h) - F^{-1}(\tau-h)}{2h}$, with h small. An expression of the optimal window can be found in Koenker (2005) and verifies (under certain conditions) : $h_n = n^{-1/5} \left(\frac{4.5\phi^4(\Phi^{-1}(t))}{(2\Phi^{-1}(t)^2+1)^2} \right)^{1/5}$ where ϕ and Φ^{-1} respectively represent the pdf and the inverse of the cdf of the normal distribution and n is the sample size. An alternative solution, proposed in Firpo, Fortin & Lemieux (2009) comes from the fact that $F'^{-1}(q_{j/10}) = 1/f_Y(q_\tau)$, which is the inverse of the pdf estimated at $F^{-1}(\tau) = q_\tau$. This density can be estimated by a kernel method, but this procedure is more computer intensive.

10. Another advantage of this method, besides being much less computer-intensive than a standard quantile regression, is that it is not limited to the analysis of the quantiles of the distribution, but it can be extended to other characteristics of the distribution.

of independence (5.5) and provided that h is monotonic in ϵ that the vector of partial impact of a marginal change in minimum wave for a quantile q_τ (the $\alpha(q_\tau$ defined above), that Firpo, Fortin & Lemieux (2009) call the "unconditional quantile partial effect" (UQPE), corresponds to the integration over the distribution of covariates of a weighted partial derivative of h of w_0 (adaptation of the proof of Firpo et al. 2009 in our case is provided in appendix) :

$$\alpha_{w_0}(q_\tau) = E_{W_0,X_1(\omega_\tau(w_0,x_1)\frac{\partial h(w_0,x_1,\epsilon}{\partial w_0}} \tag{5.4.9}$$

with $\omega_\tau(w_0,x_1) = \frac{f_{W|W_0,X_1}(q_\tau,w_0,x_1)}{f_W(q_\tau)}$.

5.5 Results

The analysis is performed on men and women separately (note that we limit ourselves to full-time employees only). We study the thresholds of the deciles of log wages. Several variables are added to take into account possible composition effects : besides the characteristics of the employees (age, social category, seniority) and the firms (size, business sector), we introduce fixed effects for the different groups of GMR to capture systematic differences of wage policies in the different firms, as well as temporal fixed-effects. Note that the impact of the minimum wage is therefore identified by the fact that we use several time periods, and that the levels of the different groups of GMR evolved at different rates. Otherwise, it would be difficult to separate the effect of the different levels of the minimum wage from what comes from the different wage policies in the different groups of firms.

We use the minimum wage level delayed by one year. Indeed, until 2010 the annual revaluation of the SMIC took place in July (except if the inflation rate was above 2 %, but this did not happen during the studied period), while wage negotiations usually take place at the end of the calendar year. We can see an example of this in Koubi & Lhommeau (2007) : the analysis of quarterly effects shows a large peak of wage growth in the first quarter. With our annual data, it is thus more appropriate to use the level of the minimum wage in July of the previous year rather than the contemporary level.

We also conducted separate estimates by firm size, with a distinction between very small firms (less than fifty employees) and the other ones. For simplicity, we present detailed results for men only, on the whole sample (Tables 5.4 and 5.5).

Recall that our results measure the impact of a marginal change in the distribution of observable characteristics (for example, increasing the proportion of executives with respect to that of workers) on each decile.[11]

The minimum wage has a significant impact on the distribution of men's log-wages up to the seventh decile (see Figure 5.3). This effect is small, however : one percent increase in the minimum wage level has an effect on deciles always below 0.2%. For the sake of comparison, note that the first decile (respectively seventh decile) corresponds roughly to 1.2 times (resp. 2 times) the average minimum wages at this period (see Table 5.7 in Appendix B). The impact is much lower for female full-time employees : it has a significant impact for the first decile only. Besides, the impact is limited to the biggest firms : for both gender we do not observe any significant impact on the earnings distribution in firms with fewer than fifty employees.

11. It should be emphasized that the interpretation of the results is more complex when considering the whole distribution than when restricting ourselves to the analysis of the mean. In particular, it is not possible to have any individual interpretation without stronger hypotheses : The question is not "What is the expected wage of an employee who was paid in the first decile if she becomes an executive instead of being a worker ?" but "How will the lowest decile change if we increase the proportion of executives by x % ?", these individuals whose wage is among the lowest wages can be different or not in the two situations.

Table 5.4 – RIF-Logit estimation for deciles (Men, 2003-2005)

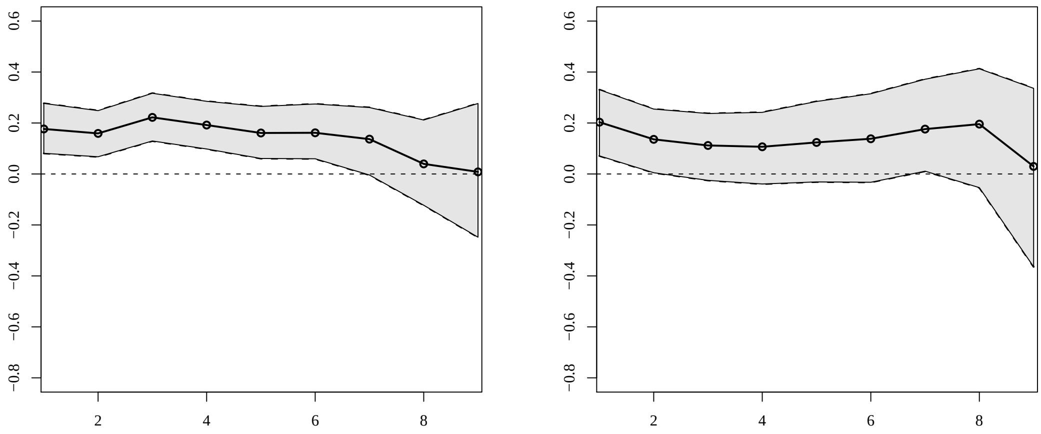
	1st decile	2nd decile	3rd decile	4th decile
Intercept	-0.13^{**} [$-0.15;-0.11$]	-0.27^{**} [$-0.29;-0.26$]	-0.44^{**} [$-0.46;-0.42$]	-0.59^{**} [$-0.62;-0.57$]
Minimum wage	0.18^{**} [0.08;0.27]	0.16^{**} [0.07;0.24]	0.22^{**} [0.13;0.31]	0.19^{**} [0.10;0.28]
Age	0.02^{**} [0.02;0.02]	0.02^{**} [0.02;0.02]	0.02^{**} [0.02;0.02]	0.03^{**} [0.02;0.03]
Age (square)	-0.00^{**} [$-0.00;-0.00$]	-0.00^{**} [$-0.00;-0.00$]	-0.00^{**} [$-0.00;-0.00$]	-0.00^{**} [$-0.00;-0.00$]
Year Dummy				
2002	*ref.*	*ref.*	*ref.*	*ref.*
2003	-0.01^{**} [$-0.02;-0.01$]	-0.01^{**} [$-0.02;-0.01$]	-0.01^{**} [$-0.01;-0.00$]	-0.00^{**} [$-0.01;-0.00$]
2004	-0.01^{**} [$-0.01;-0.01$]	-0.01^{**} [$-0.01;-0.01$]	-0.01^{**} [$-0.01;-0.00$]	-0.01^{**} [$-0.01;-0.00$]
Socio-occupational activity				
Business leaders	0.26^{**} [0.24;0.28]	0.35^{**} [0.34;0.37]	0.44^{**} [0.42;0.45]	0.49^{**} [0.48;0.51]
Executives	0.54^{**} [0.51;0.57]	0.63^{**} [0.61;0.66]	0.70^{**} [0.68;0.72]	0.72^{**} [0.71;0.73]
Technicians and associate professionals	0.22^{**} [0.22;0.23]	0.26^{**} [0.26;0.27]	0.29^{**} [0.29;0.30]	0.30^{**} [0.30;0.30]
Office clerks and service workers	-0.03^{**} [$-0.04;-0.03$]	-0.03^{**} [$-0.03;-0.03$]	-0.01^{**} [$-0.02;-0.01$]	0.01^{**} [0.01;0.02]
Skilled and unskilled workers	*ref.*	*ref.*	*ref.*	*ref.*
Business sector				
Agriculture	-0.04^{**} [$-0.05;-0.03$]	-0.05^{**} [$-0.06;-0.04$]	-0.05^{**} [$-0.07;-0.04$]	-0.04^{**} [$-0.06;-0.03$]
Industry	0.02^{**} [0.02;0.03]	0.02^{**} [0.02;0.02]	0.03^{**} [0.03;0.03]	0.04^{**} [0.04;0.04]
Construction	0.01^{**} [0.00;0.01]	-0.00 [$-0.01;0.00$]	-0.01^{**} [$-0.01;-0.00$]	-0.01^{**} [$-0.01;-0.00$]
Trade	-0.03^{**} [$-0.03;-0.03$]	-0.05^{**} [$-0.06;-0.05$]	-0.06^{**} [$-0.07;-0.06$]	-0.06^{**} [$-0.07;-0.06$]
Services	*ref.*	*ref.*	*ref.*	*ref.*
GMR group				
GMR 0	*ref.*	*ref.*	*ref.*	*ref.*
GMR 1	-0.05^{**} [$-0.05;-0.04$]	-0.05^{**} [$-0.06;-0.04$]	-0.04^{**} [$-0.05;-0.04$]	-0.04^{**} [$-0.05;-0.03$]
GMR 2	-0.03^{**} [$-0.04;-0.03$]	-0.04^{**} [$-0.04;-0.03$]	-0.04^{**} [$-0.05;-0.04$]	-0.04^{**} [$-0.05;-0.04$]
GMR 3	-0.02^{**} [$-0.03;-0.02$]	-0.03^{**} [$-0.04;-0.02$]	-0.03^{**} [$-0.04;-0.03$]	-0.04^{**} [$-0.04;-0.03$]
GMR 4	-0.03^{**} [$-0.03;-0.03$]	-0.04^{**} [$-0.05;-0.04$]	-0.05^{**} [$-0.06;-0.05$]	-0.06^{**} [$-0.06;-0.05$]
GMR 5	-0.02^{**} [$-0.03;-0.01$]	-0.02^{**} [$-0.03;-0.01$]	-0.04^{**} [$-0.05;-0.03$]	-0.04^{**} [$-0.06;-0.03$]
Size of the firm				
No employee	-0.16^{**} [$-0.21;-0.09$]	-0.13^{**} [$-0.19;-0.05$]	-0.11^{**} [$-0.18;-0.04$]	-0.10^{**} [$-0.16;-0.04$]
1 to 4 employees	-0.21^{**} [$-0.22;-0.20$]	-0.23^{**} [$-0.23;-0.22$]	-0.23^{**} [$-0.24;-0.23$]	-0.22^{**} [$-0.23;-0.22$]
5 to 9 employees	-0.16^{**} [$-0.16;-0.15$]	-0.17^{**} [$-0.18;-0.16$]	-0.18^{**} [$-0.19;-0.18$]	-0.18^{**} [$-0.18;-0.17$]
10 to 19 employees	-0.14^{**} [$-0.14;-0.13$]	-0.15^{**} [$-0.16;-0.15$]	-0.16^{**} [$-0.17;-0.16$]	-0.16^{**} [$-0.17;-0.16$]
20 to 49 employees	-0.14^{**} [$-0.14;-0.13$]	-0.15^{**} [$-0.15;-0.14$]	-0.16^{**} [$-0.16;-0.15$]	-0.15^{**} [$-0.16;-0.15$]
50 to 99 employees	-0.13^{**} [$-0.14;-0.13$]	-0.15^{**} [$-0.15;-0.14$]	-0.16^{**} [$-0.16;-0.15$]	-0.15^{**} [$-0.16;-0.15$]
100 to 249 employees	-0.11^{**} [$-0.12;-0.11$]	-0.13^{**} [$-0.14;-0.12$]	-0.14^{**} [$-0.14;-0.13$]	-0.14^{**} [$-0.14;-0.13$]
250 to 499 employees	-0.08^{**} [$-0.08;-0.07$]	-0.09^{**} [$-0.10;-0.09$]	-0.10^{**} [$-0.10;-0.09$]	-0.10^{**} [$-0.10;-0.09$]
500 to 999 employees	-0.05^{**} [$-0.05;-0.04$]	-0.06^{**} [$-0.06;-0.05$]	-0.06^{**} [$-0.07;-0.06$]	-0.06^{**} [$-0.07;-0.06$]
1000 to 1999 employees	-0.03^{**} [$-0.04;-0.02$]	-0.04^{**} [$-0.04;-0.03$]	-0.04^{**} [$-0.05;-0.04$]	-0.04^{**} [$-0.04;-0.03$]
2000 to 4999 employees	-0.01^{**} [$-0.02;-0.00$]	-0.02^{**} [$-0.03;-0.01$]	-0.02^{**} [$-0.03;-0.02$]	-0.02^{**} [$-0.02;-0.01$]
5000 employees and more	*ref.*	*ref.*	*ref.*	*ref.*
Seniority				
Less than one year	*ref.*	*ref.*	*ref.*	*ref.*
$1-5$ years	0.02^{**} [0.02;0.03]	0.02^{**} [0.02;0.03]	0.02^{**} [0.02;0.03]	0.02^{**} [0.02;0.02]
$5-10$ years	0.06^{**} [0.06;0.07]	0.06^{**} [0.06;0.07]	0.06^{**} [0.06;0.07]	0.05^{**} [0.05;0.06]
More than 10 years	0.09^{**} [0.08;0.09]	0.09^{**} [0.09;0.09]	0.09^{**} [0.09;0.09]	0.08^{**} [0.08;0.09]

Source : DADS panel, 1/25th sample.
Field : Male employees from the private sector aged 18 to 65, without interns and apprentices, working full-time, full-year, in the same firm for two consecutive years.
Reading : a 1 % variation of the minimum wage lowers by 0.18 % the difference in the first decile of the distribution of log wages.

TABLE 5.5 – RIF-Logit estimation on deciles (Men, 2003-2005)

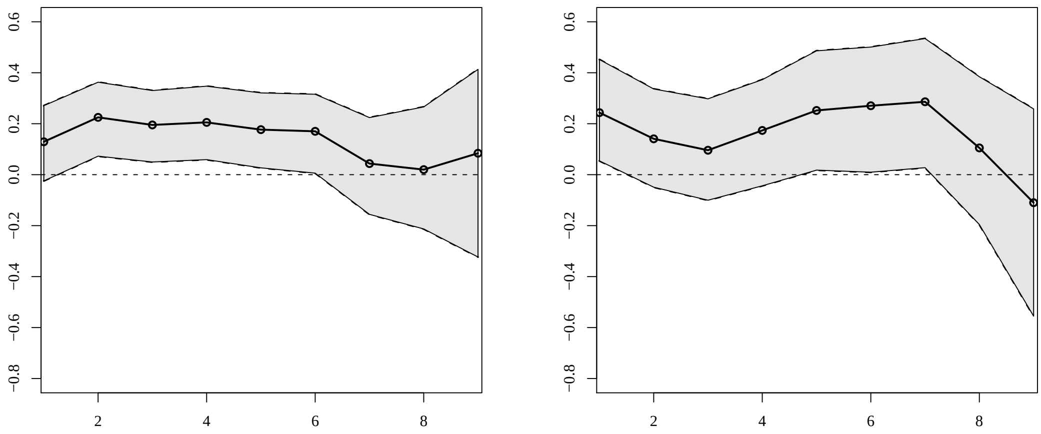
	5th decile	6th decile	7th decile	8th decile	9th decile
Intercept	−0.84** [−0.87;−0.82]	−1.08** [−1.12;−1.05]	−1.40** [−1.44;−1.36]	−1.92** [−1.97;−1.87]	−3.25** [−3.35;−3.16]
Minimum wage	0.16** [0.06;0.26]	0.16** [0.05;0.28]	0.14** [0.01;0.27]	0.04 [−0.12;0.21]	0.01 [−0.26;0.28]
Age	0.03** [0.03;0.03]	0.03** [0.03;0.03]	0.04** [0.04;0.04]	0.05** [0.05;0.05]	0.07** [0.07;0.08]
Age (square)	−0.00** [−0.00;−0.00]	−0.00** [−0.00;−0.00]	−0.00** [−0.00;−0.00]	−0.00** [−0.00;−0.00]	−0.00** [−0.00;−0.00]
Year Dummies					
2002	*ref.*	*ref.*	*ref.*	*ref.*	*ref.*
2003	−0.00** [−0.01;−0.00]	−0.00 [−0.01;0.00]	−0.01** [−0.01;−0.00]	−0.02** [−0.02;−0.01]	−0.03** [−0.04;−0.02]
2004	−0.01** [−0.01;−0.00]	−0.01** [−0.01;−0.00]	−0.01** [−0.02;−0.01]	−0.02** [−0.03;−0.02]	−0.03** [−0.04;−0.02]
Socio-occupational activity					
Business leaders	0.55** [0.54;0.57]	0.59** [0.58;0.60]	0.69** [0.68;0.70]	0.90** [0.88;0.91]	1.52** [1.47;1.55]
Executives	0.76** [0.75;0.77]	0.77** [0.77;0.78]	0.82** [0.81;0.83]	0.93** [0.92;0.95]	1.38** [1.34;1.42]
Technicians and associate professionals	0.32** [0.32;0.32]	0.33** [0.33;0.34]	0.37** [0.36;0.37]	0.44** [0.43;0.45]	0.64** [0.61;0.67]
Office clerks and service workers	0.04** [0.03;0.04]	0.06** [0.06;0.07]	0.09** [0.08;0.10]	0.12** [0.11;0.14]	0.17** [0.10;0.21]
Skilled and unskilled workers	*ref.*	*ref.*	*ref.*	*ref.*	*ref.*
Business sector					
Agriculture	−0.03** [−0.04;−0.01]	0.00 [−0.01;0.02]	0.04** [0.02;0.06]	0.01 [−0.02;0.04]	0.02 [−0.04;0.09]
Industry	0.06** [0.05;0.06]	0.06** [0.06;0.07]	0.06** [0.06;0.07]	0.04** [0.03;0.04]	−0.02** [−0.03;−0.01]
Construction	0.00** [0.00;0.01]	0.01** [0.00;0.01]	0.00 [−0.00;0.01]	−0.02** [−0.03;−0.01]	−0.07** [−0.08;−0.05]
Trade	−0.05** [−0.05;−0.05]	−0.03** [−0.04;−0.03]	−0.01** [−0.02;−0.01]	0.01** [0.00;0.01]	−0.00 [−0.01;0.01]
Services	*ref.*	*ref.*	*ref.*	*ref.*	*ref.*
GMR group					
GMR 0	*ref.*	*ref.*	*ref.*	*ref.*	*ref.*
GMR 1	−0.04** [−0.05;−0.03]	−0.03** [−0.04;−0.03]	−0.03** [−0.04;−0.02]	−0.02** [−0.04;−0.01]	−0.02 [−0.04;0.00]
GMR 2	−0.04** [−0.05;−0.03]	−0.04** [−0.04;−0.03]	−0.04** [−0.05;−0.03]	−0.04** [−0.05;−0.03]	−0.04** [−0.06;−0.02]
GMR 3	−0.04** [−0.05;−0.04]	−0.04** [−0.04;−0.03]	−0.04** [−0.04;−0.03]	−0.04** [−0.05;−0.03]	−0.03** [−0.05;−0.02]
GMR 4	−0.06** [−0.06;−0.05]	−0.05** [−0.06;−0.04]	−0.05** [−0.06;−0.04]	−0.07** [−0.07;−0.06]	−0.09** [−0.10;−0.07]
GMR 5	−0.04** [−0.06;−0.03]	−0.06** [−0.07;−0.04]	−0.06** [−0.08;−0.04]	−0.06** [−0.09;−0.04]	−0.10** [−0.14;−0.06]
Size of the firm					
No employee	−0.08** [−0.16;−0.01]	−0.06 [−0.18;0.03]	−0.04 [−0.23;0.09]	−0.08 [−0.32;0.13]	0.01 [−0.31;0.25]
1 to 4 employees	−0.21** [−0.21;−0.20]	−0.18** [−0.19;−0.17]	−0.18** [−0.19;−0.17]	−0.19** [−0.20;−0.18]	−0.24** [−0.25;−0.22]
5 to 9 employees	−0.15** [−0.16;−0.14]	−0.11** [−0.12;−0.11]	−0.10** [−0.11;−0.09]	−0.10** [−0.11;−0.09]	−0.12** [−0.13;−0.10]
10 to 19 employees	−0.14** [−0.14;−0.13]	−0.10** [−0.10;−0.09]	−0.07** [−0.08;−0.06]	−0.05** [−0.06;−0.04]	−0.05** [−0.06;−0.03]
20 to 49 employees	−0.13** [−0.14;−0.13]	−0.10** [−0.10;−0.09]	−0.06** [−0.07;−0.06]	−0.03** [−0.04;−0.03]	−0.02** [−0.03;−0.00]
50 to 99 employees	−0.13** [−0.14;−0.13]	−0.10** [−0.11;−0.09]	−0.07** [−0.08;−0.06]	−0.04** [−0.05;−0.04]	−0.02** [−0.03;−0.01]
100 to 249 employees	−0.12** [−0.13;−0.11]	−0.09** [−0.10;−0.08]	−0.07** [−0.07;−0.06]	−0.04** [−0.05;−0.04]	−0.01** [−0.02;−0.00]
250 to 499 employees	−0.08** [−0.09;−0.08]	−0.06** [−0.06;−0.05]	−0.04** [−0.05;−0.03]	−0.02** [−0.03;−0.01]	0.01 [−0.00;0.02]
500 to 999 employees	−0.05** [−0.05;−0.04]	−0.02** [−0.03;−0.02]	−0.01** [−0.02;−0.00]	0.00 [−0.00;0.01]	0.02** [0.00;0.03]
1000 to 1999 employees	−0.02** [−0.03;−0.02]	−0.00 [−0.01;0.00]	0.01** [0.01;0.02]	0.02** [0.01;0.03]	0.01 [−0.00;0.02]
2000 to 4999 employees	−0.01** [−0.01;−0.00]	0.00 [−0.00;0.01]	0.00 [−0.01;0.01]	−0.00 [−0.01;0.00]	−0.01 [−0.02;0.00]
5000 employees and more	*ref.*	*ref.*	*ref.*	*ref.*	*ref.*
Seniority					
Less than one year	*ref.*	*ref.*	*ref.*	*ref.*	*ref.*
1 − 5 years	0.02** [0.01;0.02]	0.01** [0.01;0.02]	0.01** [0.00;0.01]	−0.00 [−0.01;0.00]	−0.02** [−0.03;−0.01]
5 − 10 years	0.05** [0.04;0.05]	0.04** [0.03;0.04]	0.03** [0.02;0.03]	0.00 [−0.01;0.01]	−0.03** [−0.04;−0.02]
More than 10 years	0.08** [0.07;0.08]	0.07** [0.06;0.07]	0.05** [0.04;0.05]	0.01** [0.00;0.02]	−0.05** [−0.06;−0.04]

Source : DADS panel, 1/25th sample.
Field : Male employees from the private sector aged 18 to 65, without interns and apprentices, working full-time, full-year, in the same firm for two consecutive years.
Reading : a 1 % variation of the minimum wage lowers by 0.16 % the fifth decile of the distribution of log wages.



(Men) (Women)

Figure 5.3 – Impact of the minimum wage on the interdecile range



(Men) (Women)

Figure 5.4 – Impact of the minimum wage on the interdecile range in firms with 50 employees and more

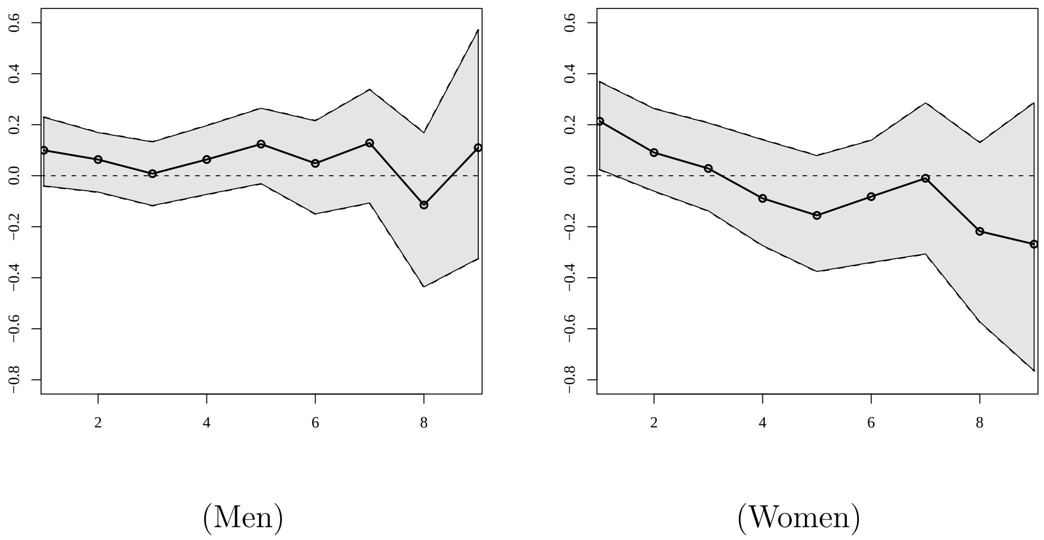


FIGURE 5.5 – Impact of the minimum wage on the interdecile range in firms with less than 50 employees

Appendix A : Schedule for the creation of different GMR

Table 5.6 – Definition of the monthly guaranteed wage (GMR)

GMR	Date of the RTT agreement
GMR1	Between June 15, 1998 and June 30, 1999
GMR2	Between July 1, 1999 and June 30, 2000
GMR3	Between July 1, 2000 and June 30, 2001
GMR4	Between July 1, 2001 and June 30, 2002
GMR5	After July 1, 2002

Appendix B : Data description

We excluded employees whose wage evolution fell in the highest or lowest 1 % of the distribution of wage evolutions between 2003 and 2004 or between 2004 and 2005 – this led to exclude 3 % of the employees. This second exclusion is justified by the fact that some of the wage evolutions were so high (in absolute terms), that they probably came from an error in the employee's number of days or hours worked in the firm during one of the two years.
Note that even when restricting ourselves to full-time employees, who worked full year, we observe that the gross annual remuneration of certain employees falls below the annual minimum wage. Several possibilities can explain that. First, the minimum wage regulations do not apply to all professions (*e.g.* in the case of employees whose working time is difficult to measure, such as some traveling salesmen). Moreover, the strict definition of the minimum wage also includes some remunerations in nature that are not always valued in the DADS. Finally, we cannot exclude reporting problems (unearned bonuses from one year to another, for example, or problems in the full-time status or the number of days worked during the year). To avoid disturbing our estimates with these cases, we choose to restrict ourselves to earnings above 0.8 times the annual SMIC. [12] Note that, conversely, because of overtime or additional bonuses that do not enter the definition of the minimum wage, some employees whose basic hourly wage is the minimum wage can *in the end* have annual earnings above the annual minimum wage. [13]

12. The chosen filter in 1999 corresponds to a 39-hour week annual gross SMIC (2028 x SMIC), then the filter remains unchanged until 2004, so long as the 35-hour annual SMIC (1820 x SMIC) remains lower. Then it is the latter which is the threshold. Note that the minimum wage is adjusted on July 1 ; the annual minimum wage is thus defined as the average of the minimum wages prevailing over the year.

13. according to Faur & Demailly (2008), a quarter of the employees directly concerned by a rise of the minimum wage earned in fact over 1.3 SMIC in 2002, after taking into account all elements of remuneration.

TABLE 5.7 – Sample variations of real earnings distribution (2003-2005)

Decile	Initial Sample	Employees in the same firm for two consecutive years (1)	(1), without (2%) extreme wages evolution (2)	(1),(2), and correction of unlikely wages evolution
0	10,970	10,978	10,978	10,978
1	17,478	17,875	17,919	17,919
2	19,370	19,811	19,837	19,835
3	21,169	21,651	21,658	21,651
4	23,069	23,575	23,562	23,549
5	**25,213**	**25,743**	**25,703**	**25,676**
6	27,850	28,425	28,349	28,300
7	31,517	32,133	31,994	31,900
8	37,176	37,840	37,601	37,415
9	48,903	49,858	49,271	48,876
10	18,158,830	18,158,830	7,628,053	2,274,019

Source : DADS panel, 1/25th sample. Field : employees from the private sector aged 18 to 65, without interns and apprentices, working full-time, full-year.

Appendix C : Definition of IF and RIF

This appendix is a summary of Firpo, Fortin & Lemieux (2009).
For a parameter or characteristic $\nu(F_Y)$ of the distribution of wages Y, the recentered influence function (RIF) is defined as :

$$RIF(y_i;\nu,F_Y) = \nu(F_Y) + IF(y_i;\nu,F_Y) \tag{5.5.0}$$

where $IF(y_i,\nu,F_Y)$ is the influence function of point y on the distribution parameter $\nu(F_Y)/$

$$IF(y;\nu,F_Y) = \left.\frac{\partial\nu(F_{Y,t\Delta_y})}{\partial t}\right|_{t=0} = a(y) \tag{5.5.0}$$

where $F_{t,G}$ is the mixing distribution $F_{Y,tG} = (1-t)F_Y + tG$.

Equation (5.4.9) can be proven following Firpo, Fortin & Lemieux (2009), under the assumptions of conditional independence () and of strict monotonicity of h in ϵ. Recall that $\alpha_{w_0}(q_\tau) = -\frac{1}{f_Y(q_\tau)}\int\frac{\partial P(Y\leq q_\tau|X_1=x_1,W_0=w_0)}{\partial w_0}dF_{X_1,W_0}(x_1,w_0)$. Assuming h monotonic in ϵ, for each value of q_τ, and couple of observables $x_1, W_0 = w_0$ an unique value of ϵ is defined. We thus derive :

$$\begin{aligned}P(Y\leq q_\tau|X_1=x_1,W_0=w_0) &= F_{\epsilon|X_1,W_0}(h^{-1}(X_1,W_0,q_\tau|X_1=x_1,W_0=w_0)\\ &= F_{\epsilon|X_1}(h^{-1}(X_1,W_0,q_\tau|X_1=x_1)\end{aligned}$$

the second line is given by conditional independence of ϵ and W_0 (Assumption 5.5).
Beside, from $h(x_1,w_0,h^{-1}(x_1,w_0,q_\tau)) = q_\tau$ we deduce that :

$$\frac{\partial h^{-1}}{\partial w_0} = -\frac{\partial h}{\partial w_0}/\frac{\partial h^{-1}}{\partial q_\tau}$$

It is also useful to notice that :

$$f\frac{\partial P(Y\leq q_\tau|X_1=x_1,W_0=w_0)}{\partial w_0} = f_{\epsilon|X_1}(\epsilon_\tau(x_1,w_0)|X_1=x_1)\frac{\partial h^{-1}(x_1,w_0,q_\tau)}{\partial q_\tau}$$

We have :

$$\begin{aligned}\frac{\partial P(Y\leq q_\tau|X_1=x_1,W_0=w_0)}{\partial w_0} &= f_{\epsilon|X_1}(\epsilon_\tau(x_1,w_0)|X_1=x_1)\frac{\partial h^{-1}(x_1,w_0,q_\tau)}{\partial w_0}\\ &= -f_{\epsilon|X_1}(\epsilon_\tau(x_1,w_0)|X_1=x_1)\frac{\partial h^{-1}(x_1,w_0,q_\tau)}{\partial q_\tau}\frac{\partial h(x_1,w_0,\epsilon_\tau(x_1,w_0))}{\partial w_0}\\ &= -\frac{dF_{\epsilon|X_1}}{dq_\tau}\frac{\partial h(x_1,w_0,\epsilon_\tau(x_1,w_0))}{\partial w_0}\\ &= -f_{Y|X_1,W_0}(q_\tau,x_1,w_0)\frac{\partial h(x_1,w_0,\epsilon_\tau(x_1,w_0))}{\partial w_0}\end{aligned}$$

and we derive equation 5.4.9.

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