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Trois essais empiriques en économie de l'éducation et de la formation

UNIVERSITÉ PARIS II
PANTHÉON-ASSAS

Audrey Rain

Sous les directions de :

Marc Ferracci, Professeur des Universités, Université Paris II

Denis Fougère, Directeur de Recherches au CNRS, Sciences Po Paris

Membres du jury :

Yann Algan, Professeur des Universités, Sciences Po Paris (Rapporteur)
Claudine Desrieux, Professeure des Universités, Université Paris II
Marc Gurgand, Directeur de Recherches au CNRS, PSE (Rapporteur)
Arne Uhlendorff, Chargé de Recherches au CNRS, CREST

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Trois essais empiriques en économie de l'éducation et de la formation

Résumé :

Les travaux présentés dans cette thèse s'intéressent aux rendements individuels de l'éducation et de la formation, et cherchent à mettre en évidence des modalités d'intervention publique plus efficaces pour accroître ces rendements. Les deux premiers chapitres de ce travail étudient la manière dont les rendements des investissements individuels en éducation peuvent être optimisés en améliorant l'efficacité des systèmes éducatifs et de formation professionnelle. La dernière étude porte sur les liens entre l'investissement en capital humain et l'environnement légal ou économique qui l'encadre. Le premier article de cette thèse s'intéresse en particulier à l'effet de la scolarisation dans une école privée française en CP et CE1 sur les résultats scolaires en CE2. Le second chapitre vise à mesurer l'efficacité des formations certifiantes destinées aux demandeurs d'emploi français sur leur retour en emploi. La dernière étude montre le lien existant entre la flexibilisation du marché du travail anglais et l'accès à la formation des salariés. Les analyses réalisées reposent sur des méthodes microéconométriques qui visent à identifier l'effet causal des politiques publiques étudiées. Nous utilisons ainsi la méthode des variables instrumentales et celle des différences de différences. Cette thèse repose également sur l'estimation de modèles de durée, en utilisant la méthode du *timing-of-events* ou en estimant un modèle à risques concurrents bivarié.

Descripteurs :

Rendements de l'éducation, Evaluation des politiques publiques, Formation professionnelle, Politique du marché du travail, Modèle de durée, Législation de protection de l'emploi.

Three empirical essays on the economics of education and training

Abstract :

This thesis work focuses on individual returns to education and training, and aims at highlighting more efficient public interventions in order to increase these returns. The first two chapters of this study consider how returns to individual investments can be optimized by improving public policies efficiency in the field of education and vocational training. The last study analyzes the link between investment in human capital and the legal and economic environment in which it is realized. In details, the first article of this thesis focuses on the effect of private schooling during the first and second grade in France on test scores achievement in third grade. The second chapter aims at measuring the efficiency of certifying training programs followed by unemployed individuals on their probability to find a job. The last study shows the link between the English labor market flexibility and workers' access to training. The analyses conducted rely on microeconometric methodologies which aim at identifying the causal effect of considered public policies. We thus use the instrumental variable methodology as well as the difference-in-difference one. This thesis also relies on the estimation of duration models, using the "timing-of-events" methodology, and estimating a bivariate competing risks model.

Keywords :

Returns to education, Evaluation of public policy, Vocational training, Labor market policy, Duration model, Legislation on employment protection.

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Introduction

Motivation de la thèse

Les limites de l'investissement rationnel en éducation et l'action des pouvoirs publics

Les individus réalisent un choix rationnel d'investissement en capital humain, en comparant les coûts et bénéfices associés à chaque acquisition de savoirs et compétences (Becker 1964 ; Mincer 1974). L'objectif est d'optimiser la valeur présente des revenus futurs, nette des coûts de scolarité. On définit ainsi les "rendements de l'éducation" comme l'effet de la scolarité sur la productivité et donc sur les salaires.

Ces choix peuvent cependant être sous-optimaux lorsque l'individu manque d'information ou ne tient pas compte des externalités de ses investissements. L'asymétrie d'information se traduit par exemple par un investissement sous-optimal des individus dans leur capital humain lorsqu'ils perçoivent un rendement de l'éducation relativement bas. C'est notamment le cas dans les pays émergents (voir Jensen 2010 [1]). Dans un contexte plus proche du nôtre, Hastings et Weinstein (2008) montrent que l'augmentation de l'information disponible peut amener certaines familles à réaliser des choix de scolarisation plus pertinents [2]. D'autre part l'investissement en capital humain d'un individu a des conséquences indirectes sur son entourage proche, voire sur le reste de la société, qui ne sont pas prises en compte lors du choix d'investissement. L'externalité est considérée comme positive lorsque le rendement social de l'investissement est supérieur au rendement privé. Par exemple, les choix individuels ne tiennent pas compte de l'effet positif du niveau d'éducation global sur le développement économique d'un pays [3].

Pour pallier ces limites, les pouvoirs publics ont la possibilité d'accompagner les individus dans leurs choix d'investissement afin d'accroître la quantité et la qualité des savoirs et compétences acquis. Plutôt que d'optimiser le choix des individus, les pouvoirs publics peuvent également optimiser les rendements des investissements individuels en améliorant l'efficacité des systèmes éducatifs, ou en contribuant à créer un environnement propice à l'investissement en capital humain.

L'action des pouvoirs publics vise tout d'abord à orienter les choix des individus vers un investissement accru en capital humain lorsque ce dernier est sous-optimal. Un niveau

1. L'auteur souligne que le niveau d'instruction demeure relativement bas malgré des rendements élevés de l'éducation. Ainsi en République Dominicaine, 80 à 90 % des jeunes terminent leur scolarisation dans le premier degré en 2002 contre 25 à 30 % dans le second degré, tandis que le salaire moyen de ces derniers est 40 % plus élevé qu'un jeune ayant achevé le cycle d'études primaires.

2. Les auteurs présentent les résultats d'une expérimentation menée aux Etats-Unis où un choc d'information concernant la qualité des écoles augmente de façon significative la part de parents qui choisit les écoles les plus performantes.

3. Suite aux contributions de Mankiw et al. (1992) et Barro (1996), de nombreuses études ont souligné l'effet positif du nombre d'années de scolarité sur la croissance économique. Cet impact provient tout d'abord du fait qu'une main d'œuvre plus qualifiée accroît la productivité du travail. Ensuite, l'éducation augmente la capacité d'innovation de l'économie ; elle facilite également la transmission des connaissances nécessaires pour comprendre et utiliser les nouvelles technologies.

relativement bas d'investissement en éducation s'explique généralement par la perception individuelle de faibles rendements de l'éducation, notamment parmi les individus issus de milieux défavorisés. La décision d'investissement peut concerner la petite enfance, la formation initiale et la formation continue. Si l'enseignement obligatoire permet d'assurer une formation initiale à chaque individu de 6 à 16 ans en France, l'accès à l'éducation dès la petite enfance et à la formation continue des adultes constituent des enjeux majeurs pour continuer à accroître les compétences des individus.

L'intervention publique semble d'ailleurs très efficace lorsqu'elle vise à améliorer l'accès à l'éducation pré-primaire. Cunha et al. (2006) soulignent en effet "l'effet multiplicateur" des investissements réalisés durant la petite enfance, qui engendrent un rendement élevé ; des programmes éducatifs intensifs destinés aux très jeunes enfants issus de milieux défavorisés ont ainsi fait leurs preuves. Le *Perry Preschool Program* développé dans les années 1960 permettait par exemple à des enfants afro-américains défavorisés âgés de 3 à 5 ans de bénéficier d'un programme éducatif innovant, ce qui leur a permis d'obtenir de meilleurs résultats scolaires. À plus long terme, les enfants traités accèdent plus souvent à l'emploi que le groupe contrôle et obtiennent de meilleurs salaires [4]. À l'inverse, Cunha et al. (2006) mettent en regard le coût important des programmes destinés aux jeunes adultes et les bénéfices relatifs qui en sont retirés. Si l'investissement dans la formation des adultes a un impact moins substantiel, il n'en demeure pas moins nécessaire. Tout d'abord les compétences acquises durant la formation initiale risquent de se déprécier lorsqu'elles ne sont pas utilisées ou entretenues [5]. Ensuite, les externalités positives de la formation des adultes sont nombreuses, notamment pour l'éducation de leurs propres enfants. Les études concernant la transmission d'inégalités intergénérationnelles soulignent qu'au-delà de l'héritage génétique, le niveau d'éducation des parents détermine fortement celui de leur enfants [6].

Dans cette thèse, nous nous intéressons à deux autres types d'intervention des pouvoirs publics. Nous regardons dans un premier temps de quelle manière les rendements des

4. Les enfants participaient à des sessions de lecture individualisées ; le programme se caractérisait par une implication forte des parents et par des méthodes pédagogiques innovantes. Les évaluations montrent un effet positif sur les résultats scolaires des enfants traités jusqu'au lycée, un taux de réussite accru à l'examen de sorti. Les auteurs citent également les effets positifs du *Abecedarian Project* et du *Chicago Child-Parent Center Program* sur les résultats aux tests cognitifs notamment.

5. L'étude de l'OCDE (2016) souligne la corrélation importante entre l'âge et les compétences : les capacités cognitives atteignent en moyenne leur niveau le plus élevé entre 25 et 30 ans puis déclinent progressivement. Le niveau de compétence varie néanmoins différement avec l'âge selon les pays de l'OCDE. Cet écart entre générations peut provenir d'une différence de qualité de l'éducation entre les cohortes, mais également d'un accès différencié à la formation professionnelle selon les pays, ainsi qu'à une différence de qualité de celle-ci. L'évaluation menée par l'OCDE montre en effet une corrélation positive entre la participation à la formation et les compétences en littératie et numératie. Pourtant, ce sont les adultes disposant du niveau d'éducation le plus élevé qui accèdent le plus souvent à ces programmes de formation.

6. Björklund et al. (2006) et Sacerdote (2007) analysent par exemple les performances scolaires d'enfants adoptés en fonction de celles de leurs parents adoptifs. Les deux études montrent que le niveau d'éducation des parents adoptifs influence de façon positive celui de l'enfant, même lorsque le placement n'a pas été réalisé de façon aléatoire.

investissements individuels en éducation peuvent être optimisés en améliorant l'efficacité des systèmes éducatifs. Dans une deuxième partie, nous nous intéressons aux liens entre l'investissement en capital humain et l'environnement légal ou économique qui l'encadre.

Agir sur la qualité du système éducatif

Optimiser les rendements individuels des individus consiste également à améliorer l'efficacité des investissements qu'ils réalisent. Hanushek et Wössmann (2007) soulignent qu'accroître la *qualité* du système éducatif contribue au moins autant au développement économique que le nombre d'années d'éducation [7]. Autrement dit, identifier les mécanismes qui ont un impact significatif sur les rendements de l'éducation permet d'investir en priorité sur les politiques publiques efficaces. Les questions d'efficacité et d'équité constituent ainsi des points d'intérêt auxquels les évaluations empiriques peuvent répondre dans une certaine mesure.

Plusieurs types d'actions sont susceptibles d'accroître l'efficacité de la formation initiale. Identifier des méthodes pédagogiques efficaces ou assurer une formation de qualité aux professeurs constituent des leviers d'action relatifs aux savoirs transmis et à la façon dont ils sont enseignés. D'autre part, repenser les modes de gouvernance tels que le degré d'autonomie des établissements constitue un axe différent de réflexion [8]. Woessmann (2008) considère ainsi que les mesures visant à accroître l'autonomie et la responsabilité des établissements sont efficaces pour accroître les résultats scolaires des élèves. *Dans le premier chapitre de cette thèse, nous mesurons l'efficacité des écoles privées françaises, qui se caractérisent notamment par une autonomie des établissements plus importante par rapport au secteur public.*

La littérature concernant les programmes de formation des adultes est moins développée, cependant un consensus émerge concernant les leviers d'efficacité de la formation des demandeurs d'emploi. Ainsi, les programmes dits "intensifs en capital humain" augmenteraient de façon significative la probabilité de retrouver un emploi par rapport aux programmes d'accompagnement dans la recherche d'emploi, mais les effets de *lock-in* [9] induits sont beaucoup plus importants dans le premier cas (Weber et Hofer 2004 ; Richardson et Gerard J Berg 2013 ; Osikominu 2013 [10]). Aux Etats-Unis, une comparaison du même type a été réalisée entre le programme de "Human Capital Development" et celui du "Labor Force Attachment" (Barnow et Gubits 2003 ; Hotz et al. 2006 ; Dyke et al.

7. Plusieurs études montrent que, parmi les déterminants de la variance du PIB, l'effet du nombre d'années de scolarisation est amplement atténué par la prise en compte de la qualité du système éducatif (Hanushek et Kimko 2000 ; Belot et al. 2007).

8. Dans leur rapport sur l'internat d'excellence de Sourdun, les auteurs citent également la "mise en concurrence des écoles" ou "le contrôle des performance" parmi les choix potentiels de mode de gouvernance.

9. L'effet de lock-in d'une formation est associé à l'allongement mécanique de la durée de chômage lorsque l'individu en formation suspend sa recherche d'emploi.

10. En Allemagne, une littérature importante compare les programmes de "further training" et "retraining" (Biewen et al. 2007 ; Fitzenberger et Speckesser 2007 ; Lechner et al. 2011).

2006). En général, ces évaluations montrent que le programme intensif en capital humain est plus efficace que le second, bien que beaucoup moins économique. *Dans le second chapitre de cette thèse, nous comparons sur données françaises l'efficacité de ces contenus de formation ; nous nous intéressons en particulier aux formations préparant à l'obtention d'une certification.*

Accroître la quantité et/ou la qualité de l'enseignement, à budget donné, suppose d'identifier le public qui bénéficie le plus des politiques publiques d'éducation, afin de maximiser les rendements sociaux de l'investissement en capital humain. En France, les écarts de réussite dûs à la catégorie socio-professionnelle sont parmi les plus importants. Ainsi, l'étude de l'OCDE (2016) montre que parmi les pays participant à l'évaluation PIAAC [11], les compétences des Français sont relativement basses en littératie et numératie ; surtout, cet écart aux résultats moyens européens provient en partie des résultats relativement faibles des individus dont aucun des parents n'a suivi d'études supérieures. Plus qu'ailleurs, la corrélation entre scores et origine sociale est relativement élevée. Il s'agit également d'une caractéristique des résultats obtenus à l'évaluation PISA par les jeunes Français [12]. L'écart entre les jeunes selon leur origine sociale est l'un des plus importants dans les pays de l'OCDE : si le milieu socio-économique explique en moyenne 13 % de la performance scolaire au sein des pays de l'OCDE, cette part s'élève à 20 % en France.

Assurer un cadre légal et économique adapté

Enfin, les pouvoirs publics influencent plus indirectement les décisions des individus en assurant un cadre légal et économique adapté (Bassanini, Booth et al. 2005). Constituer un système de certification des compétences lisible constitue un premier levier d'action. Le marché du travail est caractérisé par une asymétrie d'information entre l'employeur et le potentiel travailleur concernant la productivité de ce dernier, ce qui peut mener à une mauvaise allocation des ressources (Spence 1973). Dans ce cas, un tiers parti, l'organisme certificateur, peut être requis pour révéler le niveau de productivité du travailleur. Les pouvoirs publics ont donc la possibilité de rendre le marché du travail plus transparent. Ensuite, un système de financement peut être élaboré de façon à favoriser l'accès à la formation continue et à augmenter son efficacité. En Allemagne, le système de bons ("vouchers") pour participer à un programme de formation continue a été mis en place auprès des demandeurs d'emploi par les lois Hartz. Les évaluations empiriques réalisées (Rinne et al. 2008 ; Doerr et al. 2017) montrent un relatif effet positif sur le retour à l'emploi des individus qui participent à une formation après avoir bénéficié d'un bon de financement [13].

11. Le Programme pour l'évaluation internationale des compétences des adultes (PIAAC) évalue dans plus de 40 pays les capacités cognitives et compétences des adultes qui sont nécessaires dans le monde du travail.

12. Le Programme international de l'OCDE pour le suivi des acquis des élèves (PISA) évalue les systèmes d'éducation de nombreux pays, en mesurant les capacités cognitives des élèves de 15 ans.

13. Rinne et al. (2008) soulignent l'effet positif des bons de formation sur la probabilité d'emploi et les salaires, tandis que Doerr et al. (2017) trouvent un effet positif sur la probabilité d'emploi, qui persiste

Un troisième levier d'action des pouvoirs publics consiste à élaborer un cadre légal adapté qui assure l'accès à la formation. Un marché du travail relativement flexible peut diminuer les incitations de l'employeur et de l'employé à investir dans les compétences spécifiques à l'entreprise, du fait de la diminution moyenne de la durée de la relation de travail (Suedekum et Ruehmann 2003 ; Wasmer 2006, Delacroix et Wasmer 2007 ; Belot et al. 2007). Dans un tel cadre, les pouvoirs publics peuvent mettre en place des systèmes de compensation pour permettre aux salariés de bénéficier d'un montant optimal d'investissement en formation. *In fine, si la législation du travail est susceptible d'influencer la participation des salariés à des programmes de formation, les pouvoirs publics peuvent être amenés à intervenir pour maintenir un niveau optimal d'investissement en formation. C'est l'objet du troisième chapitre de cette thèse.*

Evaluer les politiques éducatives et de retour à l'emploi

Identifier un effet causal

L'évaluation de l'efficacité des programmes mis en œuvre se heurte à l'endogénéité des choix éducatifs. Tout d'abord l'investissement individuel en éducation n'est pas aléatoire. L'environnement social affecte profondément les choix réalisés, par exemple celui d'inscrire son enfant dans une école privée, ce qui doit être pris en compte en évaluant l'effet d'une politique éducative. En prenant en compte l'endogénéité des choix, les outils microéconométriques permettent de mettre en place des stratégies empiriques qui identifient l'effet causal des programmes étudiés. On distingue d'une part les travaux mesurant l'effet d'un programme sur les performances scolaires ou d'emploi des individus une fois qu'ils ont quitté le programme, en le comparant aux performances des individus qui n'y ont pas participé. Un autre type d'évaluation consiste à mesurer l'effet net du programme en mesurant les potentielles externalités liées à la mise en place du programme évalué. Dans le cas des programmes de retour à l'emploi, Martin et Grubb (2001) distinguent ainsi les évaluations visant à mesurer l'effet de la participation à un programme de celles qui identifient son effet net sur le niveau aggrégé de chômage ou d'emploi. Les travaux présentés ici s'inscrivent dans le premier type d'évaluation. L'effet potentiel d'équilibre général n'est pas traité, ce dont nous devons tenir compte en termes de recommandations de politiques publiques au vu des résultats.

Les modèles de durée : un outil adapté à l'évaluation d'impact de politiques publiques liées à l'emploi

Mesurer l'impact d'un programme ou d'une réforme législative sur des durées d'emploi et de chômage nécessite l'utilisation d'outils spécifiques. On observe généralement des individus évoluer d'un "état" à l'autre ; au fil du temps, la situation des individus se modifie.

durant trois ans, mais aucun impact sur les salaires.

Les individus sont caractérisés par le temps écoulé entre ces différents états, ainsi que par les dates auxquelles les transitions sont réalisées. L'analyse de ces données par régression linéaire se heurte à deux principales difficultés. Tout d'abord les données dites de "survie" sont parfois censurées, i.e la durée exacte n'est pas mesurée. Par exemple, les données de chômage sur lesquelles nous travaillons renseignent sur la date d'entrée au chômage mais pas toujours sur celle de sortie, lorsque l'individu est toujours sans emploi au moment où les données sont renseignées. Une régression linéaire ne permet pas de prendre en compte la censure des données. Ensuite, les caractéristiques observées des individus peuvent varier dans le temps, ce qui nécessite une modélisation spécifique.

Dans cette thèse nous travaillons sur des données de durée continue et discrète. Dans le premier cas, les dates de transition du chômage vers l'emploi sont connues. Le temps est continu, et la durée de chômage peut être caractérisée par un nombre réel positif. Les durées peuvent également être regroupées en intervalles discrets : dans le troisième chapitre de cette thèse il s'agit de trimestres. Bien que le processus de transition sous-jacent soit continu, les données ne sont renseignées que de façon discrète.

Contribution de la thèse

Les travaux présentés dans cette thèse s'intéressent aux rendements individuels de l'éducation et cherchent à mettre en évidence des modalités d'intervention publique plus efficaces pour accroître ces rendements.

L'effet de la scolarisation en primaire dans un établissement privé sur les résultats scolaires

Dans le premier chapitre de cette thèse, issu d'une étude co-écrite avec Denis Fougère, Olivier Monso et Maxime Tô, nous mesurons l'effet de la scolarisation dans le secteur privé sur les performances scolaires des élèves à l'entrée en CE2. Les études françaises concernant la scolarisation en primaire ne tiennent pas compte de l'endogénéité du choix de scolarisation des parents (Tavan 2004a ; Valdenaire 2011). Dans le secondaire, Vandenberghe et S. Robin (2004) mesurent un effet négatif de la scolarisation dans un établissement privé sur les résultats scolaires à l'âge de 15 ans, par rapport à la scolarisation dans un établissement public. Nous nous attachons en premier lieu à comprendre le choix de scolarisation dans une école privée à l'entrée en primaire, afin de prendre en compte le biais de sélection dans les écoles privées. L'utilisation de données obtenues grâce à la géolocalisation des ménages nous permet d'identifier la distance à l'école comme un facteur déterminant du choix de secteur : plus l'école privée est proche, plus la probabilité d'être scolarisé dans cet établissement augmente. Dans un second temps nous comparons les résultats obtenus en CE2 selon le secteur de l'élève, en utilisant comme instrument la différence entre la distance à l'établissement privé le plus proche et la distance à l'établissement public le plus proche. Nos résultats montrent qu'une fois les caractéristiques observées et non-observées

prises en compte, la scolarisation dans le secteur privé n'a pas d'effet significatif sur les résultats obtenus à l'évaluation de CE2 en littératie et numératie. Elle n'influence pas non plus la probabilité de redoubler les niveaux précédents en primaire. De tels résultats s'expliquent en partie par les similarités institutionnelles et organisationnelles des écoles privées et publiques, tels que les moyens pédagogiques mis en œuvre, les volumes horaires ou les contenus des programmes. Le choix de scolariser son enfant dans le privé ne semble donc pas plus pertinent que celui du public s'il est uniquement motivé par le souhait de mieux réussir dans une école privée. En revanche, le choix du privé peut répondre à des aspirations autres qu'académiques.

L'impact des formations certifiantes sur la durée de chômage

Le second chapitre de cette thèse, fondé sur un travail co-écrit avec Marc Ferracci et Denis Fougère, s'attache à identifier l'effet causal des formations suivies par les demandeurs d'emploi sur leur probabilité de sortie du chômage, lorsque ces dernières préparent à l'obtention d'une certification. La littérature oppose traditionnellement les formations longues et spécialisées aux programmes d'accompagnement dans la recherche d'emploi, beaucoup plus courts. Les premières sont coûteuses et efficaces tandis que les secondes augmentent le taux de retour à l'emploi, dans une moindre mesure mais pour un budget plus restreint. Nous proposons de dépasser ce cadre d'analyse en identifiant un mécanisme potentiel d'efficacité transverse aux deux types de formation habituellement analysées. Les formations préparant à une certification délivrent des compétences attachées à des domaines spécifiques, et permettent également l'acquisition d'un signal sur le marché du travail. Nos résultats montrent que les formations préparant à une certification augmentent la probabilité de sortie du chômage uniquement lorsqu'il s'agit d'un diplôme délivré par un ministère. Le signal important associé à ces diplômes peut en partie expliquer l'efficacité de telles formations. Les moyens mis en œuvre dans ces programmes sont également susceptibles d'influencer l'employabilité des individus : le niveau requis des formateurs ou les moyens pédagogiques utilisés sont des leviers potentiels d'efficacité de ces formations certifiantes.

Législation sur la protection du travail et accès à la formation

Les mesures visant à flexibiliser le marché du travail augmentent en général les flux d'entrées et de sorties des entreprises, avec un effet global mitigé sur le taux de chômage. En revanche, peu d'évidence empirique concerne l'effet d'une telle législation sur l'accès à la formation des travailleurs. Pourtant, en diminuant la durée moyenne de la relation d'emploi, une législation flexible du marché du travail peut conduire l'employeur et l'employé à réduire leur investissement dans les compétences spécifiques à l'entreprise (Suedekum et Ruehmann 2003 ; Wasmer 2006 ; Delacroix et Wasmer 2007 ; Belot et al. 2007). Dans le troisième chapitre de cette thèse, issu d'un travail réalisé avec Emma Duchini, nous nous appuyons sur un cas anglais de flexibilisation de marché du travail en regardant

l'effet de l'allongement de la période d'essai de 1 à 2 ans sur la participation à la formation continue des travailleurs. En utilisant la méthode des différences de différences, nous comparons les individus embauchés avant et après la réforme, en distinguant les différents niveaux d'ancienneté dans l'entreprise. Nous mesurons ainsi l'effet causal de l'allongement de la période d'essai sur le taux de licenciement et d'accès à la formation. Nos résultats confirment que, suite à cette réforme, les travailleurs dont les coûts de licenciement diminuent participent moins souvent à des programmes de formation. Si cette étude ne permet pas de mesurer l'efficacité de la réforme sur la réduction du taux de chômage, elle permet en revanche de préconiser des subventions compensatoires pour maintenir le taux d'accès à la formation. Ces versements seraient destinés aux travailleurs des entreprises et secteurs où les flux d'emplois sont les plus élevés dans un marché du travail flexible, et de fait dans lesquels l'accès à la formation est mécaniquement réduit.

1 Private primary schools and cognitive achievement: evidence from France

This chapter is based on joint work with Denis Fougère, Olivier Monso and Maxime Tô

Abstract

This paper estimates the test score gap in numeracy and literacy between pupils of public and private primary schools in France. Self-selection of pupils in private schools is the major concern that we need to control for (Joseph G. Altonji et al. 2005; Joseph G Altonji et al. 2005). To do so, we take advantage of rich administrative data to compute the distances between pupils residential location and schools, and we instrument the school choice by the relative distances to private and public schools. The results show that the difference between distances is a strong determinant of the school choice. Although pupils from private schools have a higher cognitive achievement from a descriptive point of view, we do not find any significant impact of private schooling once we control for observed and unobserved heterogeneity. Though benefiting from a higher degree of autonomy, the organization of private schools *sous contrat* might not drastically differ from the one of public schools.

JEL codes: C36, I21.

Keywords: private schooling, school choice, distance to school.

1.1 Introduction

The French schooling system is characterized by the cohabitation of public and private schools, with a relatively high share of pupils enrolled in the private sector (one sixth). In terms of achievement, the observed differences between the two types of schools appear to be quite large: no later than the first month of grade 3, the differences in test scores in numeracy and literacy are equal to 6 and 14% of a standard deviation respectively. The simple description of schools and pupils claims for a deeper analysis when comparing scores in both sectors, as children with a higher socioeconomic background are more likely to attend private schools. We thus first aim at assessing to which extent parents should consider private schooling as a relevant alternative in order to improve their child's achievement, in the case they refuse to enroll their child in the local public school. *In this paper, we measure the test score gap in numeracy and literacy between pupils from public and private primary schools.* To control for self-selection into schools, we use an instrumental variable strategy based on relative distances to public and private schools.

Family decisions to enroll their child in a private school is motivated by the comparison of both types of schools. Public schools in France is a public service, and, by law, parents always have the possibility to freely enroll their child into the public school corresponding to their residential location, which is usually the closest school to their residence. The large majority of children are enrolled in these public schools, however a non-negligible minority of families choose to enroll their child in a private one (13% in primary schools for school year 2014-2015 [1].). In France, private schools have very strong ties with public administration. The large majority are linked to the state through a contract named *contrat d'association* (94% of pupils in 2014), which are mostly publicly funded. They commit on recruiting teachers with the same requirements as the public sector does, and are required to teach the same pedagogical content and hourly volumes of teaching as in the public sector. Private schools share some peculiarities, though [2]. First, they have some degree of autonomy in their management, and in particular headteachers play an active role in the recruitment of teachers. Second, they are often linked to religious institutions, and are allowed to propose *optional* religious instruction classes to the pupils. Third, the admission into a private school is not free nor guaranteed by the child's residential location, as it is the case in the public sector. Private schools can charge fees, and applicant children have to go through a specific admission process. Beyond institutional characteristics, an important feature of private schools is the important share of favored pupils compared to public schools [3]. Those different institutional and socio-economic features could partly

1. The descriptive figures about French schools, except mentioned otherwise, come from MENESR-DEPP, 2015 (*Ministère de l'Education Nationale de l'Enseignement Supérieur et de la Recherche - Direction de l'Evaluation, de la Prospective et de la Performance*)

2. In this analysis we only consider private schools *sous contrat*, as they welcome 97% of pupils in the first degree and because private schools *hors contrat* regroup very different organizational and pedagogical systems.

3. As an illustration, in 2011 36% of pupils who began first degree in a private school had a father

explain the test scores gap between pupils enrolled in public and private schools. If the latter outperform the public sector, organizational characteristics of private schools can be considered as potential channels of efficiency to be systematized in any schooling system. In this regard, the literature on charter schools is very complete as their efficiency was regularly assessed, and potential mechanisms of efficiency were tested in public schools (see R. G. Fryer J. 2014). *The second objective of this study is thus to assess whether the private schooling combination of institutional and organizational characteristics allows to improve scholar achievement.* If it was the case, the private sector peculiarities could pave the way for reflection on the public schooling system.

This performance issue has also been largely studied in the case of Catholic schools in the United States, mostly at the secondary level. Heterogeneous results on the effect of American Catholic schools on academic performance are found in the second degree: they can be positive or negative depending on the chosen instrument and the context they study. D. N. Figlio et Stone (1997) use community-specific variables [4] to identify selection across public and private schools. They show a negative impact of Catholic schools in mathematics and science, for 8th to 12th grade, equivalent to a 9% difference between both sectors [5]. A more recent literature emphasizes a slight but positive impact of Catholic schooling in the US: relying on county-level Catholic shares measured at the end of the 19th century, Cohen-Zada et Elder (2009) estimate a positive effect of enrollment in a private Catholic high school on access to university, not more than 8 points of percentage [6]. An alternative strategy has been developed (Joseph G Altonji et al. 2005; Joseph G. Altonji et al. 2005; Elder et Jepsen 2014), which relies on the hypothesis that the selection level on observed characteristics is the same on the level of unobserved ones. Joseph G. Altonji et al. (2005) show that the impact of Catholic schools impact lies between 5 and 8 points of standard deviations for mathematics and reading scores at the end of high school. One result still seems quite robust: private schooling would benefit to disadvantaged populations (Evans et Schwab 1995; D. N. Figlio et Stone 1997).

Few studies focus on the impact of private schooling at the primary school level: Reardon et al. (2005) show that Catholic schools pupils get lower results in mathematics in 3rd and 5th grade, but that they do not achieve differently in reading skills. Using a different method, Elder et Jepsen (2014) also highlight the negative effect of Catholic primary

whose profession was executive, teacher, self-employed or manager, against 17% in a public school.

4. The instrument set includes "the urbanicity of the school's county, three school-availability variables (...) and variables reflecting the demographic and economic characteristics of the county and characteristics of the schools in the county."

5. The authors still nuance these findings with a positive impact of non-religious schools, especially for poor performers and those who come from modest social backgrounds.

6. Considering different outcomes, D. Figlio et Ludwig (2012) exploit variations across metropolitan areas in the costs supported by parents to ensure their children's transport to private schools, as the quality of the transportation infrastructure vary locally. The authors find that religious private schooling decreases teen sexual activity, arrests and use of hard drugs but does not reduce drinking behavior or smoking for example.

schools. The latter differ importantly from private French primary schools, mostly because in the French case they are funded by the State and are more developed. However those different effects of private schooling between the first and second degree in the US should be kept in mind in the analysis of the French system.

A few studies on French data highlight the fact that most, or the whole of the score gap between pupils in both sectors can be explained by the familial background of pupils, which is generally more favorable to academic success in the private sector (higher socio-economic background, less single-parent families, etc.). Tavan (2004b) provides some insights on private primary schooling: although pupils enrolled in the private sector in preelementary and elementary school have better test scores at the beginning of sixth grade, this test score difference shrinks to values that are not significantly different from zero when accounting for observable characteristics, including familial background and age at entry in preelementary school. Pupils enrolled in the private sector throughout their secondary education have a slight advantage in terms of the highest degree they reach (J.-P. Caille 2004). Two studies provide results accounting for unobservable characteristics through instrumental variables strategies: first, Bertola (2017) shows that initially low-achieving pupils, or whose parents are lowly-educated, have a higher probability to access to higher education when they attended a private school from the 6th grade. However the instrumental variables validity puts into question the reliability of these results [7]. In the second study, Vandenberghe et S. Robin (2004) control for differences of characteristics in pupils from both sectors to account for the advantage of private schools, relying on PISA data for 15-year old pupils. In reading and mathematics, the authors show that public schools outperform private ones, and this result is solely driven by the correction for selection on unobserved variables.

Our identification strategy is based on the distance between schools and children's home. We use survey panel data following children during primary schooling that we link to administrative data; it allows us to measure the distances to the nearest public and private schools from the pupil's household. We use the difference between both distances as an instrument, which gives us variability and allows us to identify the effect of private schools. As mentioned by Joseph G Altonji et al. (2005), using distance as an instrumental variable might lead to a positive bias when estimating the impact of private schooling on the probability of graduating high school or of attending college. We argue that first our distance variable is a more refined one: we use both the distance to the nearest public and private schools and not only the distance to the private school. We show that, despite the schooling assignment system, the distance to the nearest public school varies sufficiently to ensure that the instrument does not fully depend on the distance to the nearest private school. Those distances are obtained directly from the addresses, instead of relying on zip code of residence and schools. Moreover, our identification strategy relies on the hypothesis that, conditional on the area characteristics, the choice of residence does not depend on the distance difference between the nearest private and public schools, which is a less restrictive

7. The fact of having a poor social background, or having attended a primary private school, are used as instrumental variables for example.

assumption than being independent from the distance to the nearest private school.

Our analysis focuses on the first two years of primary schooling. A naive regression shows that the raw impact of private schooling on 3rd grade test scores in literacy and numeracy is positive. However, the effect shrinks to zero as soon as we control by the student's social background. In numeracy, the impact of private schooling becomes negative when we take into account the student's characteristics and his social background. In order to control both by observed and non-observed characteristics, we implement an instrumental variable method, which drives to the conclusion that private schooling does not affect test score results in third grade, nor the probability to repeat the first or second grade.

Section 1.2 gives a full description of the institutional context faced by parents when choosing a primary school for their child. We present our data in section 1.3. In section 1.4 we discuss the empirical strategy used and present our results in the section 1.5. We focus in the section 1.6 on the specific population of repeaters. We discuss our results and conclude in section 1.7.

1.2 Institutional context

1.2.1 The French schooling system

In France, 13% of students are enrolled in a private primary school in 2014, while 21% attend the private sector in the second degree. Private schools are unequally spread on the territory, as the share of the private sector is especially more important in the North-West side of the country, as well as in the South of *Massif Central* (see figure 1.1). The private school supply depends on this historical implantation and on openings of new private schools, which occur when a "school supply need is identified" in the area of the school which makes the request [8].

Private schools for the very large majority are linked to the state through a contract. The contract system was settled by the 1959 *Debré* law. The vast majority of private schools are linked to the state through a *contrat d'association* (94% of pupils in private schools [9]).These schools are almost exclusively publicly funded: in 2014, 74% of the expenses of schools having a contract were subsidized by the state or local authorities. The school staff is paid by the state or local authorities [10], but the schools are still in charge

8. This requirement is only effective for schools linked to the state through a *contrat d'association*.

9. There exists a second type of private schools linked to the state through a lighter contract (*contrat simple*), characterized by a relative degree of autonomy in pedagogical and financial issues. Some schools are not linked to the state through any kind of contract, and do not get subsidies. They are very diverse, for example they can rely on a specific teaching method. However only a very small minority of children are enrolled in these two types of schools (3% for the *contrat simple* and 3% in schools without contract).

10. For the *contrat simple*, the contribution of local authorities for the non-teaching staff is not mandatory.

of expenses linked to buildings and equipment. Headteachers of private schools have more flexibility in terms of management. Contrary to headteachers of public schools to whom school teachers are assigned by a regional administration without consultation, they participate actively to the choice of teachers, and can refuse teachers who are proposed under certain conditions. However, these teachers have to be chosen in a specific pool of teachers that fulfill the similar qualification requirements than teachers of the public sector.

In addition to that, some degree of autonomy remains in the organization of teaching, but the pedagogical content and hourly volumes of teaching have to be the same as in the public sector. On the contrary, it has to be highlighted that the so-called *hors contrat* private schools are the most likely to differ from public ones regarding the pedagogical methods and curricula: in 2015, two thirds of those schools were characterized by the use of specific pedagogical methods or by bilingual classes.

One of the most specific features of private schools in France is their link with the Catholic religion. According to the *Secrétariat général de l'enseignement catholique* quoted by Héran (1996), 95% of the private sector is Catholic, and is subject to the supervision of a Diocese, or both to a congregation and a Diocese. The religious affiliation of private schools does not imply a deeply religious nature of these schools though. First, religious classes are often not compulsory, and correspond more often to classes related to moral issues [11]. Second, Héran (1996) highlights that most of headteachers do not belong to the clergy, as well as 97% of teachers in these schools.

One of the main peculiarities of private schools is the way they enroll pupils. Private schools having a contract have to welcome all students regardless of their origin or religion. Still, they exert an influence on the composition of the pupils. Enrollment in a private school is not free nor guaranteed by the child's residential location, as it is in the public sector. Private schools can charge fees, although they are generally quite low. According to the *Secrétariat Général de l'Enseignement Catholique*, mean fees for private Catholic schools having a contract are equal to 366 and 380 euros each year per child in pre-elementary and elementary schools in 2014.

Lunch expenses are higher in the private sector, due to the fact that they are not publicly subsidized. In the *Efforts d'Éducation des Familles* survey (year 1991-1992), mean lunch expenses paid by parents having a child in the private sector were half higher than lunch expenses in the public sector, and were also half higher than admission fees.

The way schools set these charges are thus a first way through which they can have an impact on their socio-economic composition. A second way arises from a specific admission process. Interested parents contact the school and, very often, are asked to meet the headteacher. The whole process appears to be selective and may even be discriminatory. In a research where letters from fictitious parents were sent to private schools headteachers, parents having a name suggesting a foreign origin were less often given a positive answer (see Du Parquet et al. 2013). Thus the recruitment process makes a real difference between

11. Poucet (2009) defines Catholic teaching as *un enseignement associé ouvert à tous les horizons spirituels.*

public and private schools.

Finally, differences between the public and private sectors also rely on a different organization of schools: 93% of private schools in the first degree include both pre-elementary and elementary schools, contrary to 31% in the public sector in 2014. They welcome more students in each class compared to the public sector (24.4 students by class in private schools against 23.7 in public ones on average), however class sizes are more dispersed in the private sector. The private sector is also characterized by an important enrollment of two-years-old children, which was reinforced in the 2000s as the global enrollment rate of children of that age was falling in the public sector. Indeed, in 2014 two-year-old children represented 7% of pupils enrolled in a private school at the pre-primary level, while they represented 3% in public ones [12]. Management of teachers' replacement also differs, as non-permanent staff is more frequently hired in private schools (9% of teachers in private schools under any sort of contract in 2014, against 0.5% in public ones).

1.2.2 Comparison with alternative schooling organizations in other countries

1.2.2.1 Private schools in other countries

In France, private schools enrollment rises the same selection issue but embodies a different reality from the American one. Indeed the private sector is less developed in the US: 8% of pupils are enrolled in a private elementary school against 15% in France in 2012 (OECD 2014a). Moreover, Toma (1996) highlights that American private schools are not funded by the State such as in France, which allows them to benefit from a larger financial and pedagogical autonomy. Accordingly, tuition fees are generally much higher than in French private schools. American private schools are also more diverse regarding their link with religion (whereas in France Catholic schools are a huge majority). Finally, the choice is made in a less diverse framework in France, as required teachers' qualifications are the same in the public and private sectors and programs contents do not differ.

Though most of studies have focused on the American private system, Toma (1996) highlights that it does not recover settings of the private educational system in Europe. Moreover Dronkers et Robert (2008) show that in many countries like France, private schools are publicly-funded but are thus less autonomous in terms of curriculum or payment of teachers. Vandenberghe et S. Robin (2004) also consider that France is among countries of interest to study the impact of the private schooling sector as it represents more than 10% of the total educational supply in the country [13]. However, the private sector is less important than the public one as in most European countries (except in Ireland, Belgium and in the Netherlands).

12. See Abdouni (2014).

13. Other countries studied in this paper are Dutch-speaking Belgium, French-speaking Belgium, Mexico, Ireland, Spain, Denmark, Austria and Brazil.

1.2.2.2 Charter schools in the US

In the US, charter schools have also been studied as alternative educational options to improve students' achievement. The literature on this issue is all the more relevant that charter schools efficiency was assessed through many studies, and that potential channels of efficiency were identified to be implemented in public schools.

Curto et J. R. G. Fryer (2011) define charter schools as "publicly funded schools that operate outside the direct control of local school districts". Charter schools relying on the "No Excuses" model, such as the Promise Academy in the Harlem Children's zone or the Knowledge is Power Program (KIPP) network, have been implemented in order to enhance disfavored pupils' achievement. In a nutshell, those schools include small class sizes, high quality teachers, tutoring, an increased instructional time, and student work ethic. On average, enrollment in such programs significantly increases the academic achievement of minority children (Angrist et al. 2010; Abdulkadiroğlu et al. 2011; Dobbie et al. 2011; Curto et J. R. G. Fryer 2011). The initial goal of efficiency improvement seems thus achieved. Interestingly, potential mechanisms enhancing charter schools effectiveness were tested in the public system. Indeed, R. G. Fryer J. (2014) studies the effect of charter schools "best practices" on students' achievement when implemented in public schools. He shows that such practices raise students' mathematics achievement in treated elementary and secondary schools while they have a small effect on reading achievement.

Charter schools and French private ones share a higher level of autonomy than in public schools, however they are not comparable, mainly because the former still depend on the State and specifically target disadvantaged pupils. Moreover, their characteristics differ in many important respects. Among them, the average class size in French private schools is higher than in public ones, while the number of pupils per class is reduced in charter schools to favor a better learning. Another illustration is that French private schools do not automatically include after-school tutoring while it is the case in charter schools relying on the "No Excuses" model.

1.2.3 Why do parents choose private schools?

Parents' choice of the private sector might be driven by several motivations: first, the religious affiliation is an important factor in their decision (see Héran 1996). However, Langouet et Léger (2000) show it is not the main parents' concern, as only 27% of parents "faithful [14]" to private schooling cite religion as a determinant factor.

The authors highlight that reasons for enrollment in the private sector are largely linked to educational success motives. For example, private teachers are often considered as more competent at mentoring students, and might implement innovative pedagogical practices. The overall school environment, or discipline, also determine the parents' choice towards

14. "Faithful" parents are considered by the authors as those who do not switch their children from one sector to the other from the first to last grade.

private schools.

An important motivation is the wish of parents to find a school welcoming children from the same social background. In the US, Betts et Fairlie (2003) show that a student switches to a private high school as soon as 4 new immigrant students get enrolled in a public school. Finally, switching from the public sector to the private one is often associated to scholar difficulties (see J.-P. Caille 2004), which highlights the potential educational upgrading role of some private schools. More generally, Langouet et Léger (2000) show that children who switch from one sector to another have more difficulties at school, as they repeat more frequently a year than those staying in the same sector [15]. This phenomenon differs across social groups, as children from a less disadvantaged social background go more frequently into a private school when they face difficulties at school.

School proximity does also largely influence parents' choice [16], though it depends on the social category. Relying on 1991-92 data, Héran (1996) shows that 40% of families who chose a private school made their decision because it was the nearest. This share reaches 60% in regions where the private sector is more important in the school supply, in the *Bretagne* region for example. According to Héran (1996) firm managers and independent workers are "actively looking" for a private school, i.e they rely less on proximity for their school choice. On the other hand low income families use distance as a determinant for their school choice more frequently (Langouet et Léger 2000; Hastings et Weinstein 2008).

1.3 Data

1.3.1 1997 Panel

Our primary data source is the *Panel d'élèves du premier degré 1997* (Panel 97) which is a survey collected by the French Ministry of Education (DEPP). All surveyed children have entered first grade in September 1997.

The 1997 Panel survey provides information on children's cognitive achievement when they enter 1st, 3rd and 6th grade. This information corresponds to measurements derived from formal tests run either only for panel students or at the national level, but are not used by teachers for the evaluation of children. When panel students enter first grade, they answer a multiple choice questionnaire in their own school; tests are corrected by their teachers. They cover five domains: general knowledge, verbal skills and ease in writing, logical skills and ease in numeracy, time and space concepts, and behavior. Third grade

15. In section 1.3.3.1 we show that switching from one sector to another is less common in the primary degree than in the secondary degree. However, as we restrict the sample to pupils who stayed in the same sector during two years, we still check that our final results are not affected by this restriction.

16. Our data show that distance to school does indeed influence parents' choice of schooling sector, however in section 1.5.4 we provide suggestive evidence that parents' choice of residence does not depend on the private school localization.

tests are implemented at the national level, and are corrected by the teachers. These tests aim at assessing the students' level at the beginning of the year, and to provide tools to the teacher for the rest of the year. They include both a literacy and numeracy part. The test in mathematics consists in numerical and geometry exercises, as well as questions testing time measurement and problem solving capacities. In literacy, students were assessed in regard to their knowledge of writing and reading.

All students pass 1st grade tests in September-October 1997, while 3rd grade tests are completed in 1999 by non-repeating students, and in 2000 by students who repeated either the 1st or the 2nd grade.

In addition to these assessments, the Panel 97 survey contains information about the child's social background and his family's characteristics. Some of these variables were collected in 1997 by the head of school, when the child entered first grade. An additional questionnaire was sent to parents in 1999, when children were in 3rd grade; it gives more precise information about the child's family composition and characteristics. In our specifications, we control for parents' characteristics (social category, parents' national origin, mothers' education, single motherhood, family size) and child's characteristics (gender, month of birth, initial level in first grade).

1.3.2 Census and *Enquête 19*

In addition we use the 1999 census data in order to better characterize the pupils' environment. In particular, this provides us information on the socioeconomic composition of the neighborhood where children live and on the neighborhood where schools are situated. Finally, we rely on the *Enquête 19* data: this census of schools provides us information on all schools in France from 1996 to 1998. It allows to identify the set of schools available when parents make their choice of school [17].

In total, the contextual variables we control for are whether children live in a rural area or not and the share of blue collar workers and foreigners in the census tract [18].

1.3.3 Descriptive statistics

1.3.3.1 Sample restrictions

We make the following restrictions on our initial sample of pupils located in metropolitan France. We first restrict our sample relying on geographical criteria for technical

17. In details, we identify private schools opened at the time of the parents' choice which included at least one first grade class.

18. IRIS in the French denomination. These are aggregated units for statistical information, which generally regroup between 1,800 and 5,000 inhabitants. Villages and small cities are not subdivided into IRIS and are considered as a whole.

reasons of geolocalization [19]. Then, we restrict our sample to pupils who were born in 1991 in France, and thus exclude students who repeated a year before entering first grade [20]. We only consider pupils who did not repeat their first and second grade, in order to work on a homogeneous sample [21]. Finally, among non-repeaters, we only consider pupils who spent two years in the same sector in order to capture the pure effect of private schooling. We drop 1.2% of the remaining observations, which leads to a total sample of 5,097 observations.

Though the two last restrictions do not lead us to drop many observations, we still discuss to which extent the latter might limit the representativity of our sample. Restricting the sample to non-repeaters raises two main questions. First, if a pupil showing some scholar difficulties is more likely to repeat a year in a private school, restricting the sample to non-repeaters might lead us to over-estimate the effect of private schooling on test scores results. We show in section 1.6 that it is not the case. Second, considering only non-repeaters, whose average level is higher than repeaters, might put into question the external validity of our results. There is clearly a trade-off between the representativity of our sample and treatment homogeneity. Indeed, as repeaters spend one more year in one of both sectors, the treatment differs from non-repeaters and we are not able to disentangle the private schooling effect from the one of repeating a year. We are still confident in the external validity of our results regarding the small amount of repeaters in the first and second grade.

Then, considering among non-repeaters only pupils who spend their first and second grade in the same sector might also put into question the external validity of our results, as switching from one sector to another might be correlated to unobserved heterogeneity [22]. We show that in the primary degree, at least during the two first years of schooling, it is not the case. Table 1.1 shows that among non-repeaters, the difference between test scores of non-switchers and pupils who switch sector between the first and second grade is frequently positive in first or third grade, but never significant. In the first degree, switching from one sector to another is not common, contrary to what occurs in the secondary degree [23]. Table 1.2 shows that few pupils switch from one sector to another among non-repeaters: 3% (1%) of pupils initially enrolled in the private (public) sector switch to the public (private) one in second grade. Thus, restricting the sample to pupils who stayed two years in the same sector should not bias our estimations. We still run additional regressions to ensure that it is not the case.

19. We exclude students for which we measure a distance to school superior to 30 kilometers, pupils living in Corsica, and those for which we do not have the coordinates of the chosen school. We drop 65 observations out of the initial 5,749 observations.

20. It represents 219 observations.

21. We thus drop 304 observations.

22. For example, it has been highlighted that moving from the public to the private sector is highly correlated to scholar difficulties in the secondary degree (J. Caille 2004).

23. J. Caille (2004) shows that 22% of students who entered 6th grade in 1989 switched from one sector to another at least once during the secondary degree.

Finally, it is worthwhile to highlight that pupils we observe in the same sector during the first two grades of primary schooling represent an important share of pupils who stay in the same sector until the end of 5th grade. Indeed, considering only pupils who did not repeat any grade, 93% of them spend the whole primary degree in a private school.

1.3.3.2 General characteristics

In our final sample, 14% of children spent their first and second grades in the private sector (see table 1.3). Children of farmers, managers, self-employed workers or executives are over-represented in the private sector while those whose parents are blue collar workers attend more frequently a public school: 39% of students enrolled in the public sector come from the working class background, against 27% in private schools. In the private sector, 9% of students have at least one parent who is immigrant, while this share is twice more important in the public one. Both sectors also differ regarding the educational level of pupils' parents: the share of children whose mother does not hold any diploma reaches 20% in public schools, which is twice more than in private ones.

In terms of achievement, the observed differences between the two types of schools appear to be quite large: when they begin 1st grade, the difference in global test scores is equal to 2 points out of 100, which represents 21% of a standard deviation. When those students reach 3rd grade the gap is wider in literacy than in numeracy and remains important.

Distributions of test score results in first grade [24] show that pupils who chose the private sector in first grade initially performed better in literacy compared to those who chose the public one (see figure 1.2). In numeracy, the results between both sectors do not significantly differ. As shown in figure 1.3, those patterns are identical whether considering pupils who spent either one year or two years in the same sector, as few pupils switch from one sector to another. In third grade, figure 1.4 shows that the gap narrows in literacy, while pupils who initially chose the private sector perform on average better in numeracy than those in the public one. Again, those results are robust to comparisons including only pupils who spent one year in the same sector (see figure 1.5).

1.4 Empirical strategy

1.4.1 Instrumenting the decision to attend a private school in first grade

In order to account for the potential endogeneity of parents' decision to choose a private primary school for their child, we use an IV methodology. More precisely, we select two instrumental variables, namely the difference of distances between the nearest private and

24. In this sample, 6% of test scores are missing in literacy in first grade and 4% in numeracy.

public schools [25] and the size of the pupil's city of residence [26]. Our first instrument is innovative in two ways. First, it is based on the precise home address of the pupil's parents, which is more refined than zip codes of residence and schools (see Joseph G. Altonji et al. 2005 and Joseph G Altonji et al. 2005). Second, we argue that, contrary to the distance to the nearest private school, it is very unlikely that parents settle both according to the nearest public and nearest private school [27].

In order to ensure the exogeneity of our distance instrument, we add a list of controls for neighborhood characteristics that are likely to affect residence choice, i.e., the share of blue collar workers and the share of foreigners in the neighborhood.

The distance between the nearest private and public schools is computed by geo-referencing residences and schools [28] .

In order to estimate the impact of private schooling in the two first grades on 3rd grade scores, we estimate by two-stage least squares the following model:

$$S_i = \beta_0 + \beta_1 Priv_i + \beta_2 X_i + v_i \tag{1.1}$$

$$Priv_i = \alpha_0 + \alpha_1 Z_i + \alpha_2 X_i + u_i \tag{1.2}$$

Equation 1.1 is the outcome equation, where S_i is the score obtained in 3rd grade either in literacy or in numeracy by pupil i, X_i is a vector of covariates including the pupil's gender, her quarter of birth, her baseline score when entering 1st grade, family and neighborhood characteristics, and $Priv_i$ is a dummy which takes the value 1 if the pupil does attend a private school during her first and second grades [29]. Equation 1.2 is the linear regression of the probability to choose the private sector in 1st and 2nd grades on the same set of covariates X_i and on our two instrumental variables, namely the distance difference between the nearest private and public schools denoted by $dist_{priv,i}$ and $dist_{pub,i}$ and the size of the pupil's city of residence, denoted pop_i.

A preliminary descriptive nonparametric analysis (see figure 1.6a) shows that the probability of entering a private primary school in 1st grade is a decreasing function of the distance difference [30]. On the contrary, it does not seem to be affected by the size of the

25. We only consider schools which were opened at the time of the school choice, i.e in 1996 or before. In addition, we restrict the schools' sample to the ones which were opened at least until 1998.

26. This latter instrumental variable is similar to the one selected by Vandenberghe et S. Robin (2004), except that they use a dummy variable which takes the value 1 if the pupil attends a school located in a city with more than 100,000 inhabitants, and to 0 otherwise.

27. We still need that both distances vary sufficiently to rely on an exogenous instrument: we discuss the instrument validity in details in section 1.5.4.

28. Details about the geo-reference procedure are given in appendix 1.C.

29. $Priv_i = 0$ if the pupil spent two years in a public school. We drop pupils who spent their first and second grade in different sectors. In the extension analysis, $Priv_i = 1$ if the individual spent at least one year in a private school and $Priv_i = 0$ if the individual spent two years in a public school.

30. When the distance difference variable is negative, it means that the nearest private school is closer to the pupil's residence.

pupil's city of residence[31] (see figure 1.6b). The weakness of this second instrument will be highlighted when estimating equation 1.2 (see section 1.5.1). Figure 1.7 shows that the distance difference instrument is not correlated with third grade test scores in numeracy nor in literacy.

1.4.2 Distance differences

On average, the nearest public school is 670 meters far from a pupil's residence, while she has to travel 3.85 kilometers to reach the nearest private one (see table 1.4). These distances vary as a function of the population density of the residence area: when the child lives in the least populated cities (with less than 2,000 inhabitants), the nearest private school is 7.6 kilometers far from the pupil's residence, while it is 5 times less remote for children living in large agglomerations. This gap is less important when considering the nearest public schools.

We now look at distances to the nearest public and private schools by chosen sector (table 1.5). The nearest public school is always closer from individuals' households than private ones on average as the network is more dense, however they are more remote from students enrolled in the private sector. On the other hand, the nearest private school is closer when the individual chose a private school in first grade, though it is more remote than the nearest public school. As a consequence, the effective distance traveled by students enrolled in a private school is on average 1.2 kilometers longer than those enrolled in a public school. Figure 1.8 confirms that the distance difference distribution is much more centered around zero for students who chose a private school. Indeed, among the latter the nearest private school was not very more remote than the public one on average, while it was definitely the case for those who chose a public school. Said differently, for pupils who chose the private sector in first grade, the nearest private school is almost as far as the nearest public one on average. On the contrary pupils who chose the public sector live closer from a public school.

The distance differences between pupils living in urban and rural areas are also illustrated in figure 1.9. On the one hand the distance difference is very centered toward zero for those living in urban areas: the sector choice may not rely much on distances as, on average, private and public schools are as far apart. On the contrary, in rural areas, pupils living closer to a private school are more likely to attend such a school while pupils attending a public school live further from any private school. Families face important distance differences, and their decision relies much more on this determinant.

31. Since there are few cities in France whose size is above 150,000 inhabitants, this nonparametric regression excludes pupils living in these large cities, which represent 5% of the whole sample. The huge amount of variation in the nonparametric estimation for large cities is due to the fact that in our sample 649 pupils live in cities with 80,000 inhabitants or more, against 4,448 with less.

1.5 Results

1.5.1 Sector choice determinants

We look at determinants of the private school choice when the pupils enter first grade in 1997-1998. The choice is thus made during the year 1996-1997. First we compare the statistical significance of our two potential instruments. For that purpose, using a linear probability model, we regress the choice of attending a private primary school on *i)* the difference of distances and its squared value, and *ii)* the pupil's city size, and *iii)* these three variables simultaneously. The F-statistics presented in table 1.6 show that the difference of distances and its squared value are the only strong instruments, the F-statistic for the city size being lower than 10 (which is the threshold suggested by Stock et Yogo 2005). Moreover, when the three instruments are simultaneously introduced as regressors in this first-step equation (see last column of table 1.6) the coefficient associated with the pupil's city size is not statistically significant, while those associated with the distance difference are highly significant. Consequently, in the rest of our study, we keep the distance difference and its squared value as the only instrumental variables [32]. In this regard, we deviate from the approach chosen by Vandenberghe et S. Robin (2004), who use as an instrument a dummy variable equal to 1 if a pupil attends a junior high-school located in a big city (more than 100,000 inhabitants) and to 0 otherwise. Their instrument relies on the fact that private secondary schools are overrepresented in big cities in France, like in many OECD countries. However, this instrument might be less relevant in our case because we focus on private primary schools, which are especially numerous in big cities (more than 100,000 inhabitants), but also in small cities of 2,000 to 5,000 inhabitants (see figure 1.10).

Let's comment now the sign of the effect of our main IV on the choice of a private primary school. Table 1.7 shows that the higher the distance between the nearest private and public schools, the lower the probability to attend a private school. In other terms, the distance to the nearest private school negatively affects the choice of a private school: when parents have to travel more, they are less likely to send their child to a private school. On the contrary, if the nearest public school is remote from the child's residence, she is more likely to get enrolled in a private school. We also control for the quadratic effect of the distance, which is statistically significant. The effect of the distance remains similar when controlling for other observable characteristics. Conditional on observable characteristics, an additional kilometer difference between the nearest private and public schools decreases the probability of attending a private school by 5 percentage points. This effect remains the same when we extend the sample to pupils who stay at least one year in the same sector (see table 1.8).

In addition to that, the child's social background is one of the major determinants of the school sector choice (see table 1.9): those whose father is a farmer, a craftsman, a

32. Note that, in this regression, we restrict the sample to observations for which the city size is not missing, which is not the case in the following estimations.

retail-trader, a manager or self-employed, are more likely to attend a private school. On the contrary, children whose father is a blue-collar worker are more likely to attend a public school. The mother's educational level is also statistically significant, as the probability to choose a private school decreases when the mother does not have any diploma, or is very low-educated. Finally, having one or two immigrant parents does greatly reduce the probability of choosing a private school. Results also show that a higher initial achievement, i.e., at entry in the first grade, slightly increases the choice of a private school.

The probability to get enrolled in a private school in first grade is more important when the child lives in a rural area. Lastly, parents are more likely to choose a private school when the census share of blue collar workers is important in the neighborhood. Choosing a private primary school thus appears as a tentative to avoid public schools where the social background of pupils is more often mixed.

1.5.2 Impact of private schooling on test scores

As well as in the first step, we control for the pupils' family and individual characteristics, initial level in first grade and contextual variables. We first estimate the effect of private schooling on 3rd grade test scores by OLS: the raw impact is positive, however in literacy it shrinks to zero as soon as we control by the pupil's social background (see table 1.10). In numeracy, the impact of private schooling becomes negative when we account for the student's characteristics, social background and initial level in grade 1. Private schooling would thus decrease numeracy test scores by 11% of a standard error.

Second, we use the instrumental variable method to control for both observed and unobserved students' characteristics. In both subjects, the effect of the private sector choice is not significantly different from zero. The naive estimation of private schooling in numeracy might thus be driven by a selection bias, where initially low-achieving students in numeracy choose the private rather than the public sector. As a robustness check we look at the effect of private schooling for students who spent at least one year in a private school (see table 1.11). Results confirm our first estimation as both samples are quite similar.

We do not estimate the impact of private schooling on 6th grade test scores as we observe more transitions from one sector to another: among pupils enrolled in a private school in first and second grade, 93% stay in this sector until the end of primary school without repeating a year. Restricting the sample to pupils who stayed in the same sector until 5th grade, without repeating a year, we would measure the effect of private school for a specific sample of the population. On the other hand, when measuring the impact of private schooling for pupils who chose this sector in first grade but who switched from one sector to another, we do not identify the pure impact of private schooling. Moreover, 30% of 6th grade test scores are missing in our sample, which reduces even more the sample power.

1.5.3 Heterogeneous effects of private schooling on test scores

We look at the impact of private schooling as a function of pupils' initial levels, first running an OLS regression. Table 1.12 shows that in both subjects, pupils who were initially achieving well in first grade perform worst in the private sector compared to good students who chose the public one. In numeracy, without controlling for unobserved characteristics, initially high-achieving pupils perform 15 percentage points of a standard error worst when they spent two years in a private school than those who chose the public sector. This effect cancels out when controlling for unobserved characteristics, which reflects a negative selection process for enrollment in the private sector. Among initially high-achieving pupils, those showing a small margin of improvement are more likely to choose the private sector. Said differently, pupils presenting learning difficulties get more often enrolled into a private school, which leads to an estimated negative impact of private schooling when one does not control for unobserved characteristics.

We now look at the different effects of private schooling as a function of the pupil's city size (see table 1.13). We consider that a city has a high density population when the latter is superior to 40,000 inhabitants, which is slightly below the median population density. A city has a low density population when it is inferior to 40,000 inhabitants. First, the distance difference is less correlated to the probability of attending a private primary school in high density cities, though it remains a strong instrument. Second, in numeracy, the private schooling effect is negative when we do not control for unobserved characteristics both in high and low density cities. Though this impact is not significant anymore when using instrumental variables, the amplitude of the coefficient decreases only for the sample of small cities, as our estimation method is more robust in those areas.

Considering the effect of a private school regarding the private sector density in the area is also a matter of interest, as private schooling recovers different realities. As an illustration, Tavan (2004b) highlights that the social composition of private schools differs less from public schools in areas where the share of private schools is high. Interestingly, the composition of private schools also differs in those areas regarding initial achievement in first grade. Figures 1.11 and 1.12 show that, in literacy and in numeracy respectively, the distribution of test scores in first grade varies as a function of the share of pupils attending a private school in the department. When this share is relatively high [33], the initial level of pupils differs less between the private and public sectors, compared to areas where this share is relatively low. However, our identification method does not allow us to estimate different effects according to the share of the private sector [34].

33. We compare departments where the share of private schooling in the first degree ranges within the first and last quartiles of the distribution.

34. Indeed, the instrument is less efficient if we consider areas where the share of the private sector is the highest, which leads to important standard errors. It can be due to the fact that the network of schools is more dense, thus the distance predicts less efficiently the school choice. Our instrument might also be limited in the sense that, to be highly predictive in those areas, distances should be measured more precisely than what they currently are.

We do not look at differential effects by social background as they can recover very different situations, and as we do not hold any information regarding parents' earnings.

1.5.4 Discussion on the instrument

We show that the difference of distances between the nearest private and public schools affects the probability to attend a private school, and we argue that it does not impact the pupil's test scores. Though we cannot test it, suggestive evidence shows that, conditional on other covariates, this instrument is not correlated with the error term in the second step equation and thus satisfies the exclusion restriction. Indeed, it appears unlikely that parents compare the distance between the nearest private and public school when choosing their home address. We still need to consider separately both distances variables to assess potential threats to our instrument exogeneity.

A first issue is that parents might choose their home address as a function of the surrounding schools quality. S. Black, Machin et al. (2011) show in their literature review that a large consensus exists on the causality link of schools quality on housing prices. In Boston, S. E. Black (1999) showed that on average house prices go up by 2.5% for a 5% change in test scores in elementary schools [35]. Such a link between the choice of residence and school puts into question the validity of our instrument, however it has to be reminded that, in the French case, the causal effect of schools quality on the residential choice is not only due to distance issues but also to the existence of geographical criteria to get enrolled in a public school [36]. On the contrary, it is possible to get enrolled in a private school even when parents do not live near the school as admission does not depend on parents' address. The latter might thus put less effort in living close to a private school than to a public one [37] .

Another concern is that the distance to the nearest public school does vary sufficiently. The assignment system ensures that all pupils are offered to attend a public school close enough to their home address; as a consequence, the difference of distances between both nearest schools might boil down to the only distance to the nearest private school. If it is case, we should observe, for a given value of the distance to the nearest private school, a unique value for the distance to the nearest public school. Because of the sample size we are only able to split our sample into pupils with a low or high distance to the nearest private school [38]. The figure 1.13 shows that the variance of the distance to the student's

35. Further studies in other countries show similar results : regarding English primary schools (Steve Gibbons et Machin 2003; Stephen Gibbons et Machin 2006; Stephen Gibbons, Machin et Silva 2008) or middle schools in Paris (Fack et Grenet 2010), the authors find that a one standard deviation increase in school test scores induces an increase in house prices between 3 to 4 percent.

36. The French *carte scolaire* assigns each household to a specific public school.

37. Interestingly, Fack et Grenet (2010) highlight that, in France, the presence of private schools mitigates the impact of public school performance on housing prices, as it provides an alternative option to parents.

38. In details, we split the sample according to the sample median value of the distance to the nearest

nearest public school is higher when the distance to the nearest private school is relatively high. However we still observe some variance in the group where the distance to the nearest private school is relatively low. We thus argue that the difference of distances between the nearest private and public school does not fully depend on the distance to the nearest private school.

We finally check whether our instrument is correlated to the pupil's observed ability. Figure 1.14 shows that the distance difference instrument is not correlated with first grade test scores. We thus argue that if the instrument is not correlated with the pupil's observed ability, it is not likely to be correlated with his unobserved ability either.

Despite our instrument exogeneity, our results might still be biased be the fact that we only consider a sample of non-repeaters. In the following section we thus assess to which extent private schooling affects the probability to repeat a year.

1.6 Impact on grade repetition

We look at another outcome which might be impacted by the sector choice: it is possible that the private schooling system affects the probability of students to repeat a year. We extend our sample to students who repeated their first or second grade, and thus did not reach the third grade in 1999-2000 but in 2000-2001.

In details, we consider all pupils who were enrolled in the same sector during their two first years of primary schooling, whether they repeated or not. Our outcome of interest, the probability to reach third grade in 2000 (rather than in 1999) is equal to one if the individual repeated his first or second grade. The treatment variable equals one if the pupil attended his two first years of primary schooling in the private sector, to zero if he attended a public school [39].

Table 1.14 shows that, on average, students who spent their first and second year of primary schooling in the private sector are not more likely to repeat a year. IV estimates show that a naive regression is not biased by any selection mechanism. These estimates highlight that our previous results are not negatively biased: if a low-achieving pupil is as likely to repeat a year in both sectors, test score results in both sectors are comparable.

Restricting the sample to pupils who stayed in the same sector during the two first years of primary schooling allows us to evaluate the pure effect of private schooling, however we might drop non-random observations. For example, if individuals who switch from one sector to another are also the most likely to repeat a year in a private school, we might under-estimate the effect of private schooling on the probability to repeat a year.

private school, which corresponds to 1.8 kilometers.

39. It corresponds to two potential situations: if the pupil repeated his second grade, he spent his first and at least "first" second year in the same sector. If he repeated his first grade, he spends at least two years of first grade in the same sector. In this case there can be an issue because the decision to stay in the same sector or to switch might be linked to the fact that the pupil repeats a year. However this case concerns 4 pupils in our sample.

However, as mentioned in section 1.3, switching from one sector to another is very less frequent in the primary degree compared to the second one. It is also true for pupils who repeat either their first or second grade: table 1.2 shows that among pupils repeating their first grade, 7.4% repeat a year in a different sector. In order to check that this restriction does not bias our results, we add to our sample pupils who switched during their two first years of primary schooling. Benchmark results remain similar for pupils who spent only one year in the same sector (see table 1.15).

1.7 Discussion and conclusion

Our estimates show that in France, as in many other European countries, enrollment in a private school is highly endogenous. Selection on observed characteristics is strong: 39% of students enrolled in the public sector come from the working class background, against 27% in private schools. To control for potential endogeneity, we instrument the sector choice in first grade using the difference of distances between both the nearest private and public school from the student's household.

In a first step, we show that distance is an important factor in private schooling choice, as an additional kilometer difference between the nearest private and public school decreases the probability of attending a private school by 5 percentage points. When we look at the effect of private schooling on test scores, controlling for observed characteristics leads to measure a null effect in literacy while it decreases numeracy test scores by 11% of a standard error. However, taking into account unobserved characteristics drives the effect of private schooling toward zero.

Those results might be surprising in view of the large positive selection in private schools described above, as such a selection process might translate into positive peer effects and induce a higher achievement of pupils attending a private primary school. However it has to be reminded that, though the selection on social criteria does exist, descriptive statistics show that selection on the initial educational level seems less obvious [40]. Moreover, some negative peer effects might also be at stake, as private schooling is sometimes the choice of parents who observe difficulties at school for their child [41]. The concentration of homogeneous educational levels on both sides of the distribution, that we do not observe on average, might lead to peer effects in opposite directions, which contributes to observe a null effect on average. Finally, we observe pupils for a two-years duration, which might not be enough to trigger any significant peer effect [42].

40. Figure 1.2 shows that at the entrance of first grade, pupils in private and public schools perform equally in mathematics on average.

41. The higher initial level observed in French in first grade possibly recovers different realities as some private schools target academic excellence while others mostly welcome pupils with scholar difficulties (Nauze-Fichet 2003; Felouzis et Perroton 2007).

42. Davezies (2004) highlights that between the first and third grade in a French primary school, the effect of peers' characteristics on achievement in mathematics or in French is limited, while they are

More generally, such a short period of time might not be sufficient to detect any effect of private schooling on achievement at school. It is still relevant to focus on this stage of schooling, as it is fundamental to determine through which channels students of that age could improve their test scores. Indeed, empirical evidence shows that first years of primary schooling are of the utmost importance for the acquisition of literacy skills.

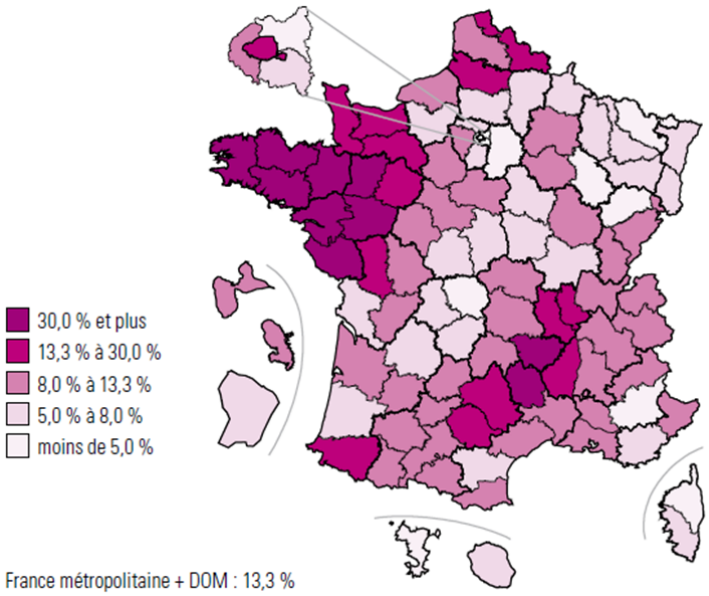
In our case, the difference between private and public schools might also be too smooth to measure any significant effect of the former, as teachers' required levels of qualifications, pedagogical content and hourly volumes are comparable. On the other hand, the absence of any effect of private schooling on achievement might not necessary imply that all potential channels of efficiency that differ from public schools would not separately improve the organization of the public schooling system. Interestingly, the literature on charter schools mentioned above showed that a significant effect on pupils' test scores was found when a range of specific and limited features was implemented in public schools (see R. G. Fryer J. 2014).

Finally, our results put into question parents' motivation for choosing a private school in first grade. Their choice may rely on the *belief* that private schooling is more efficient at improving the pupils' results, but also on the wish to provide to their child a specific environment. It is also possible that private schooling mostly affects non-cognitive skills, which we do not measure in this analysis.

correlated to the achievement in mathematics when considering the third to sixth grade.

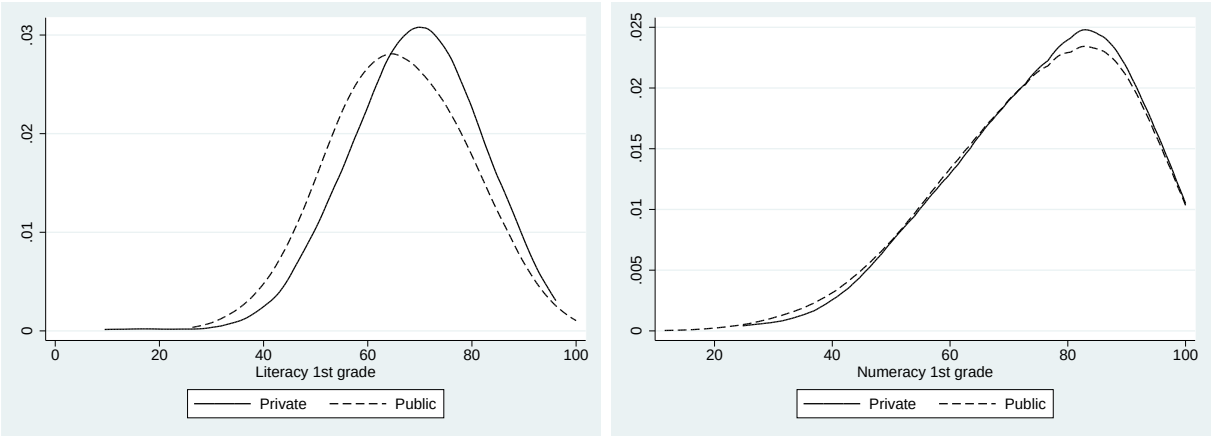
Appendix 1.A: Figures

FIGURE 1.1 – Share of pupils enrolled in a private school in primary school in 2013



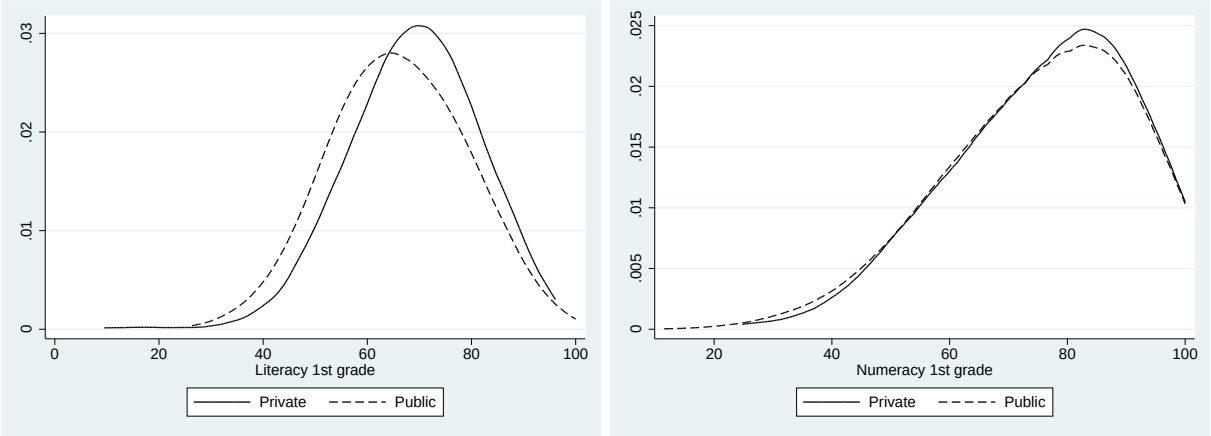
Source: MENESR DEPP.

FIGURE 1.2 – First grade test score differences in literacy and numeracy, for pupils who spent two years in the same sector (non-repeaters)



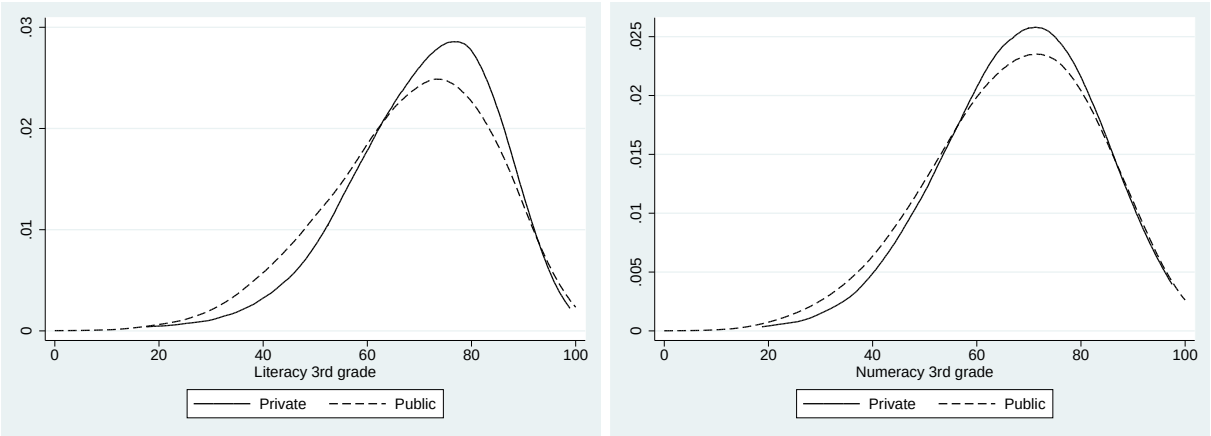
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

FIGURE 1.3 – First grade test score differences in literacy and numeracy, for pupils who spent at least one year in the same sector (non-repeaters)



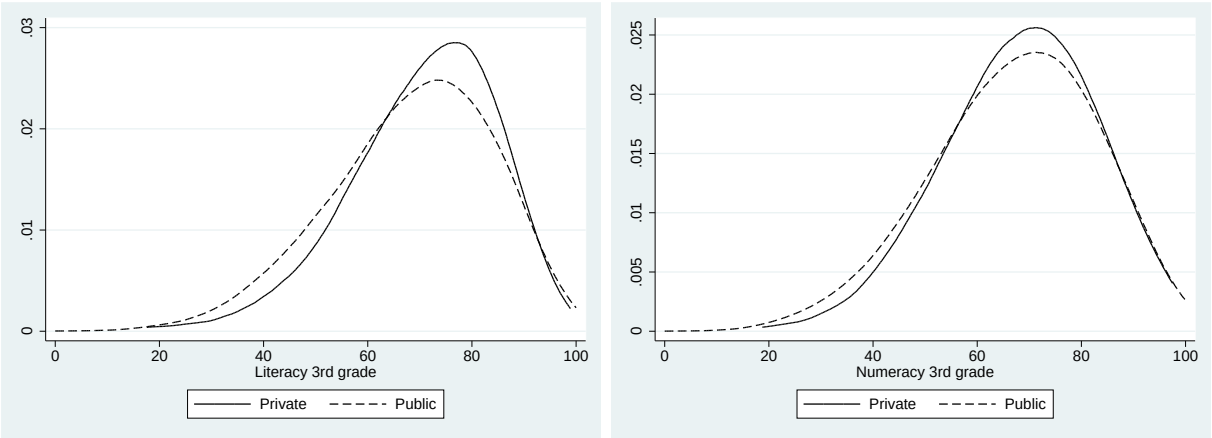
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

FIGURE 1.4 – Third grade test score differences in literacy and numeracy, for pupils who spent two years in the same sector (non-repeaters)



Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

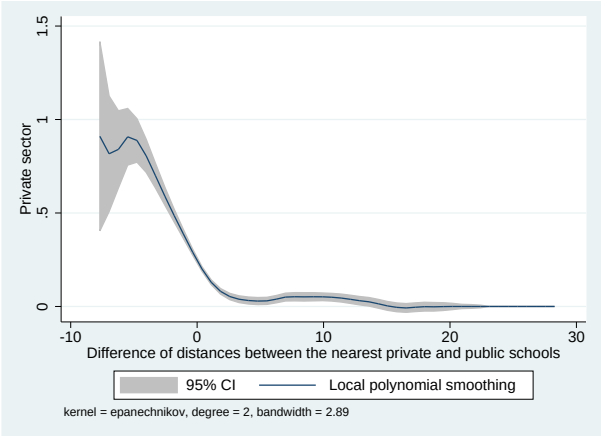
FIGURE 1.5 – Third grade test score differences in literacy and numeracy, for pupils who spent at least one year in the same sector (non-repeaters)



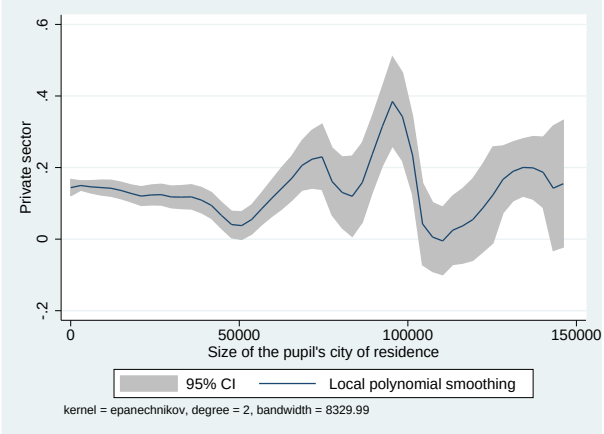
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

FIGURE 1.6 – Average probability of attending a private primary school (nonparametric regressions)

(a) As a function of the difference of distances between the nearest private and public schools



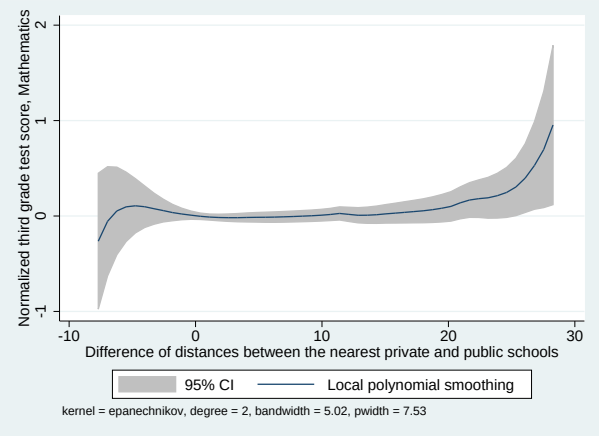
(b) As a function of the size of the pupil's city of residence



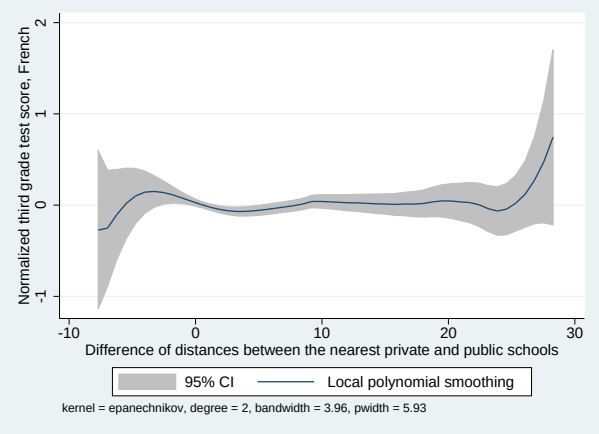
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Figures were obtained using Kernel-weighted local polynomial smoothing.

FIGURE 1.7 – Average standardized third grade test scores as a function of distance difference between the nearest private and public schools in numeracy and literacy

(a) Numeracy

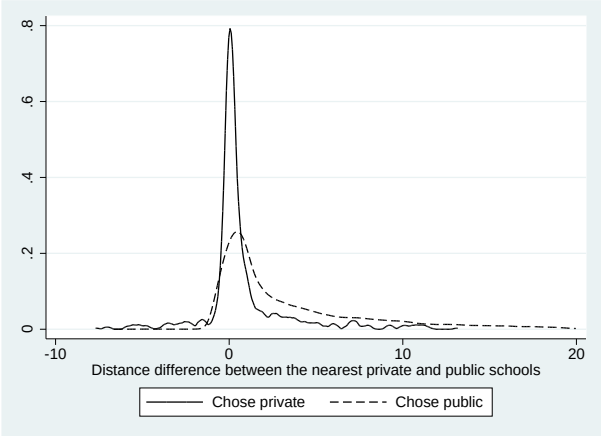


(b) Literacy



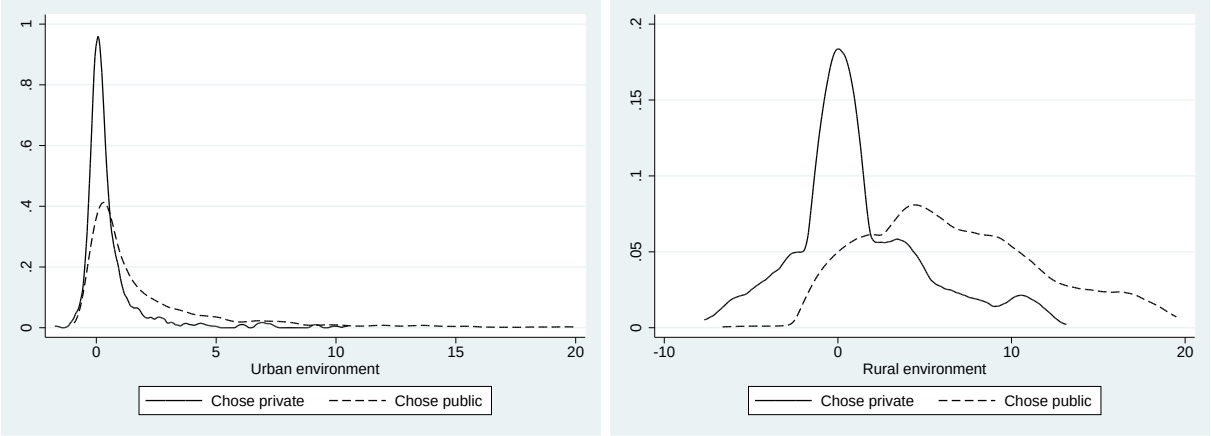
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Figures were obtained using Kernel-weighted local polynomial smoothing.

FIGURE 1.8 – Distance difference between the nearest private and public schools, by sector chosen in first grade (non-repeaters)



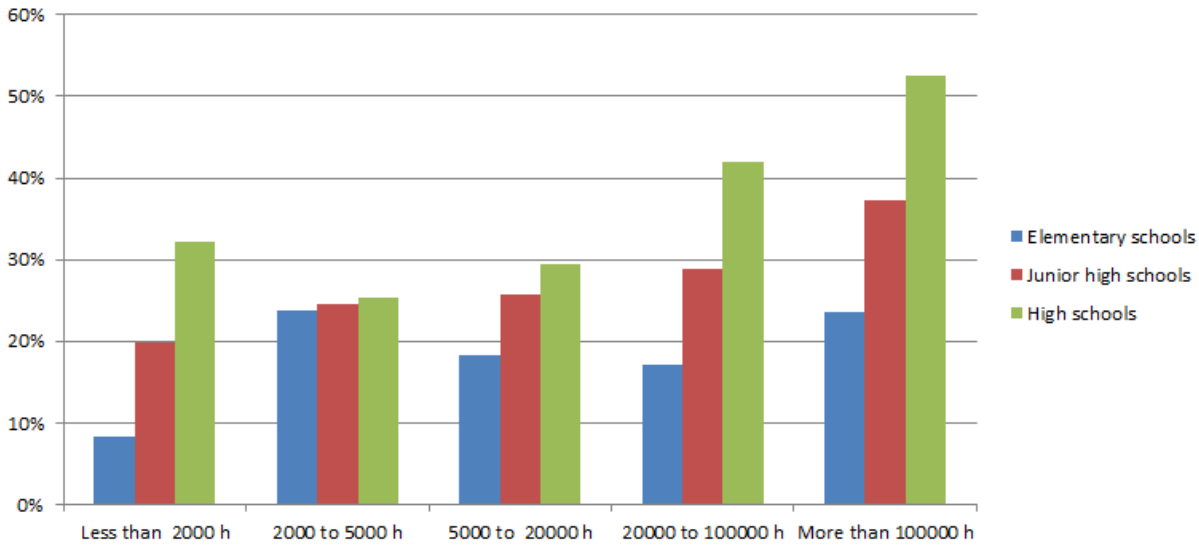
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

FIGURE 1.9 – Distance difference between the nearest private and public schools, as a function of the the pupil's environment (non-repeaters)



Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: We rely on the INSEE definition of a rural environment: a city is considered as rural when it is not included in any *unité urbaine*.

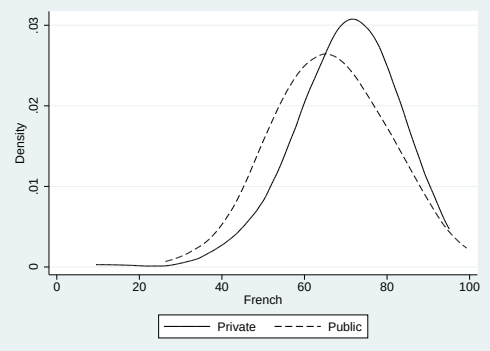
FIGURE 1.10 – Share of private schools by city size



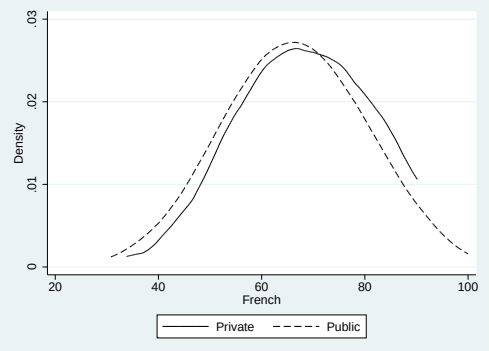
Source: MENESR-DEPP, Schools central registry; INSEE, 1999 population census.

FIGURE 1.11 – Distribution of test scores in first grade as a function of the share of the private sector in the department, in French

(a) Low share of the private sector



(b) High share of the private sector

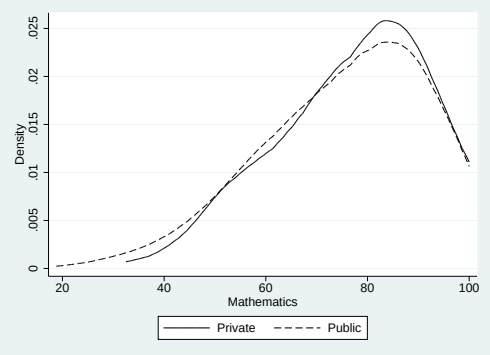


Source: Panel DEPP 1997, MEN-DPD, *Enquête dans les écoles maternelles, élémentaires et spéciales 1997-1998* (Ministry of Education).

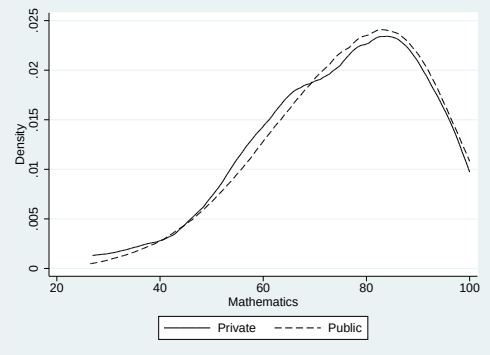
Note: The share of pupils attending a private school in the department is considered as low (high) when it ranges in the first (last) quartile of the distribution of this share.

FIGURE 1.12 – Distribution of test scores in first grade as a function of the share of the private sector in the department, in mathematics

(a) Low share of the private sector



(b) High share of the private sector

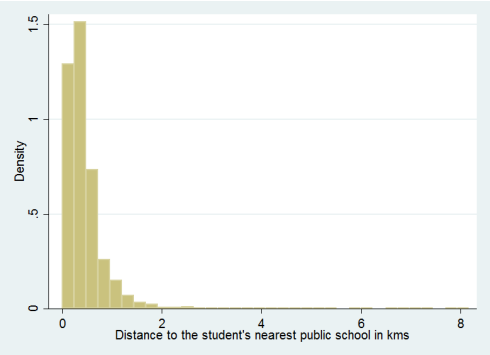


Source: Panel DEPP 1997, MEN-DPD, *Enquête dans les écoles maternelles, élémentaires et spéciales 1997-1998* (Ministry of Education).

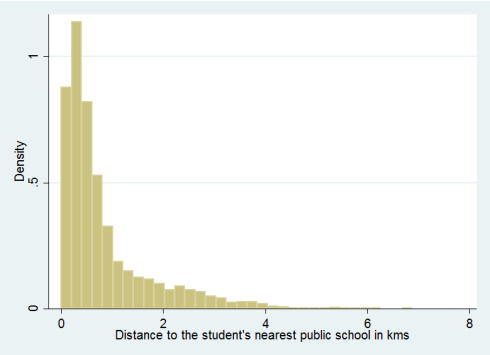
Note: The share of pupils attending a private school in the department is considered as low (high) when it ranges in the first (last) quartile of the distribution of this share.

FIGURE 1.13 – Distribution of the distance to the nearest public school

(a) When the distance to the nearest private school is relatively low



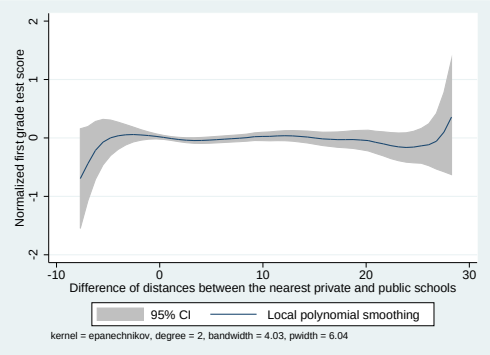
(b) When the distance to the nearest private school is relatively high



Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

Note: The sample is divided into pupils whose distance to the nearest private school is above or below the median value of this variable, which corresponds to 1.8 kilometers.

FIGURE 1.14 – Average standardized first grade test score as a function of distance difference between the nearest private and public schools



Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

Note: Figures were obtained using Kernel-weighted local polynomial smoothing.

Appendix 1.B: Tables

TABLE 1.1 – Test scores differences of switchers compared to non-switchers in first and third grade (mean test)

	First grade	Third grade, French	Third grade, Maths
Private to public	-1.42	0.66	-0.35
	(0.57)	(0.85)	(0.92)
Public to private	1.03	2.14	3.43
	(0.54)	(0.35)	(0.12)

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. For each test, notation is out of 100 points. For each mean test we make the difference between non-switchers and switchers' test scores.

TABLE 1.2 – Transition matrix in 1997-1998

	No repeaters		Repeaters		Total
	Private	Public	Private	Public	
1st gr.private	751	20	17	2	790
1st gr.public	45	4,515	8	107	4,675
Total	796	4,535	25	109	5,465

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: The table indicates the number of pupils switching from a sector to another or staying in the same sector at the end of the year 1997-1998. For example, 751 pupils enrolled in a private school in first grade stayed in the private sector to begin their second grade.

TABLE 1.3 – Pupils differences across sectors for pupils who spent two years in the same sector (non-repeaters)

	Public	Private	Difference	P-val
Father Occupation				
Farmer	0.023	0.057	-0.033	0.000
Craftman-Trader	0.073	0.092	-0.020	0.063
Manager	0.012	0.022	-0.010	0.027
Self-Employed	0.028	0.057	-0.028	0.000
Executive	0.128	0.167	-0.039	0.005
Teacher	0.036	0.037	-0.001	0.883
Interm-occ	0.158	0.175	-0.017	0.238
White-Collar	0.120	0.102	0.018	0.168
Blue-Collar	0.387	0.272	0.115	0.000
Unemployed	0.035	0.019	0.016	0.028
Mother Education				
No-diploma	0.197	0.095	0.102	0.000
BEPC	0.131	0.109	0.022	0.106
CAP-BEP	0.276	0.291	-0.015	0.398
Bac-sup	0.397	0.505	-0.108	0.000
Family Characteristics				
Immig-parent	0.194	0.091	0.103	0.000
Single-parent	0.102	0.057	0.045	0.000
Sup-3siblings	0.363	0.408	-0.046	0.018
Child Characteristics				
Trim1	0.241	0.247	-0.006	0.715
Trim2	0.263	0.287	-0.023	0.187
Trim3	0.256	0.233	0.023	0.190
Trim4	0.240	0.233	0.007	0.690
Girl	0.492	0.497	-0.005	0.820
Achievement				
Score-grade1	71.662	73.978	-2.316	0.000
Literacy-grade3	68.392	70.774	-2.382	0.000
Numeracy-grade3	67.177	68.352	-1.175	0.048
Environment				
Rural-area	0.246	0.299	-0.054	0.002
Census-share-foreigners	0.051	0.038	0.012	0.000
Census-share-blue-collar-workers	0.154	0.155	-0.000	0.880
N	4,372	725	.	.

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

TABLE 1.4 – Average distance (in kms) to the nearest private and public schools, as a function of parents' location

# City residents	Private	Public
Less than 2,000 h	7.59	1.15
2,000 to 10,000 hb	5.56	0.81
10,000 to 50,000 h	3.23	0.60
50,000 to 150,000	1.91	0.47
150,000 to 1M hbt	1.85	0.44
More than 1M hbt	1.40	0.34
Total	3.85	0.67

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

TABLE 1.5 – Distance differences across sectors for pupils who spent two years in the same sector (non-repeaters)

	Chose public	Chose private	Difference	P-val
Dist-nearest-private	4.201	1.730	2.471	0.000
Dist-nearest-public	0.617	0.981	-0.364	0.000
Dist-difference	3.584	0.749	2.835	0.000
Sq-dist-difference	41.701	5.894	35.807	0.000
Dist-effective-school	1.173	2.440	-1.267	0.000
N	4,372	725	.	.

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).

TABLE 1.6 – Impact of the instruments on the private schooling probability, only for non-missing city sizes

	Dist.diff.	City size	Both
Dist.diff.	-0.049***		-0.050***
	(0.003)		(0.003)
Sq.dist.diff.	0.002***		0.002***
	(0.000)		(0.000)
Pupil's city size		0.024***	-0.009
		(0.009)	(0.009)
Family characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes
Grade 1 level	Yes	Yes	Yes
Context	Yes	Yes	Yes
N	4,905	4,905	4,905
F-stat	248.3	7.2	165.9
R2 adj.	0.12	0.03	0.12

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. The pupil's city size is expressed in hundreds of thousands.

TABLE 1.7 – Impact of the distance for pupils who spent two years in the same sector (non-repeaters), first step (OLS)

	Model 1	Model 2	Model 3
Dist.diff.	-0.041***	-0.041***	-0.050***
	(0.003)	(0.003)	(0.003)
Sq. dist.diff.	0.001***	0.001***	0.002***
	(0.000)	(0.000)	(0.000)
Family characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes
Grade 1 level	No	Yes	Yes
Context	No	No	Yes
N	5,097	5,097	5,097
F-stat	191.6	192.3	250.0
R2 adj.	0.09	0.10	0.12

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 1.8 – Impact of the distance for pupils who spent one year in the same sector (non-repeaters), first step (OLS)

	Model 1	Model 2	Model 3
Dist.diff.	-0.041***	-0.041***	-0.049***
	(0.003)	(0.003)	(0.003)
Sq. dist.diff.	0.001***	0.001***	0.002***
	(0.000)	(0.000)	(0.000)
Family characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes
Grade 1 level	No	Yes	Yes
Context	No	No	Yes
N	5,164	5,164	5,164
F-stat	190.591	191.170	247.814

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 1.9 – Determinants of the private choice for pupils who spent two years in the same sector (non-repeaters), first step (OLS)

	Model 3
Dist.diff.	-0.050***
Sq. dist.diff.	0.002***
Farmer	0.095***
Craftsman or retail-trader	0.047**
Manager	0.099**
Independent worker	0.066**
Executive	0.019
Teacher	-0.018
Interm. prof.	-0.016
Blue collar worker	-0.031**
Unemployed	0.042
Mother : no diploma	-0.063***
Mother : Brevet	-0.037**
Mother : CAP/BEP	-0.006
Immigrant parents	-0.054***
Single parent	-0.053***
Large family	0.034***
Birth quarter 1	-0.002
Birth quarter 2	0.003
Birth quarter 3	-0.011
Girl	0.004
Global score grade 1	0.001**
Rural zone	0.094***
Census share foreigners	-0.267***
Census share blue collar W	0.449***
Const.	0.099**
N	5,097

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 1.10 – Third grade achievement, for pupils who spent two years in the same sector (non-repeaters)

	Literacy			Numeracy		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
PrivateOLS	0.025	-0.026	-0.026	-0.052	-0.106***	-0.107***
	(0.037)	(0.031)	(0.031)	(0.038)	(0.031)	(0.031)
PrivateIV	-0.083	-0.039	-0.024	-0.205	-0.159	-0.147
	(0.141)	(0.115)	(0.101)	(0.144)	(0.117)	(0.102)
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Grade 1 level	No	Yes	Yes	No	Yes	Yes
Context	No	No	Yes	No	No	Yes
N	5,097	5,097	5,097	5,097	5,097	5,097
F-stat	191.6	192.3	250.0	191.6	192.3	250.0
R2 adj.	0.18	0.43	0.43	0.18	0.43	0.43

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 1.11 – Third grade achievement, for pupils who spent at least one year in the same sector (non-repeaters)

	Literacy			Numeracy		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
PrivateOLS	0.024	-0.028	-0.029	-0.049	-0.104***	-0.105***
	(0.036)	(0.030)	(0.030)	(0.037)	(0.031)	(0.031)
PrivateIV	-0.071	-0.037	-0.018	-0.174	-0.139	-0.131
	(0.141)	(0.114)	(0.101)	(0.145)	(0.117)	(0.103)
Family characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Grade 1 level	No	Yes	Yes	No	Yes	Yes
Context	No	No	Yes	No	No	Yes
N	5,164	5,164	5,164	5,164	5,164	5,164
F-stat	190.6	191.2	247.8	190.6	191.2	247.8

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

Table 1.12 – Third grade achievement as a function of initial achievement, for pupils who spent two years in the same sector (non-repeaters)

	Literacy		Numeracy	
	1	2	1	2
PrivateOLS	0.032	-0.087**	-0.070	-0.150***
	(0.049)	(0.037)	(0.050)	(0.038)
PrivateIV	0.064	-0.051	-0.147	-0.083
	(0.161)	(0.122)	(0.165)	(0.120)
Family characteristics	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes
Grade 1 level	Yes	Yes	Yes	Yes
Context	Yes	Yes	Yes	Yes
N	2,661	2,436	2,661	2,436
F-stat	129.8	123.8	129.8	123.8
R2 adj.	0.25	0.20	0.25	0.20

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. Lowest initial level is level 1, highest initial level is level 2.

Table 1.13 – Third grade achievement, for pupils who spent two years in the same sector, as a function of the pupil's city population density (non-repeaters)

	Literacy		Numeracy	
	High	Low	High	Low
PrivateOLS	-0.061	-0.011	-0.098**	-0.124***
	(0.047)	(0.041)	(0.047)	(0.042)
PrivateIV	0.083	-0.035	-0.315	-0.081
	(0.242)	(0.110)	(0.241)	(0.112)
Family characteristics	Yes	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes	Yes
Grade 1 level	Yes	Yes	Yes	Yes
Context	Yes	Yes	Yes	Yes
N	2,629	2,473	2,629	2,473
F-stat	50.0	193.9	50.0	193.9
R2 adj.	0.44	0.42	0.44	0.42

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. The population density of the pupil's city is considered as high when it is superior to 40,000 inhabitants.

TABLE 1.14 – Impact of private schooling on the probability to repeat the first or second year, for pupils who spent two years in the same sector

	Model 1	Model 2	Model 3
PrivateOLS	-0.011	0.002	0.001
	(0.011)	(0.010)	(0.010)
PrivateIV	0.028	0.006	0.023
	(0.039)	(0.036)	(0.033)
Family characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes
Grade 1 level	No	Yes	Yes
Context	No	No	Yes
N	6,338	6,338	6,338
F-stat	245.8	247.7	314.3
R2 adj.	0.08	0.21	0.21

Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. The population density of the pupil's city is considered as high when it is superior to 40000 inhabitants.

TABLE 1.15 – Impact of private schooling on the probability to repeat the first or second year, for pupils who spent at least one year in the same sector

	Model 1	Model 2	Model 3
PrivateOLS	-0.011	0.002	0.001
	(0.011)	(0.010)	(0.010)
PrivateIV	0.032	0.011	0.016
	(0.040)	(0.037)	(0.033)
Family characteristics	Yes	Yes	Yes
Individual characteristics	Yes	Yes	Yes
Grade 1 level	No	Yes	Yes
Context	No	No	Yes
N	6,435	6,435	6,435
F-stat	240.3	242.0	314.9

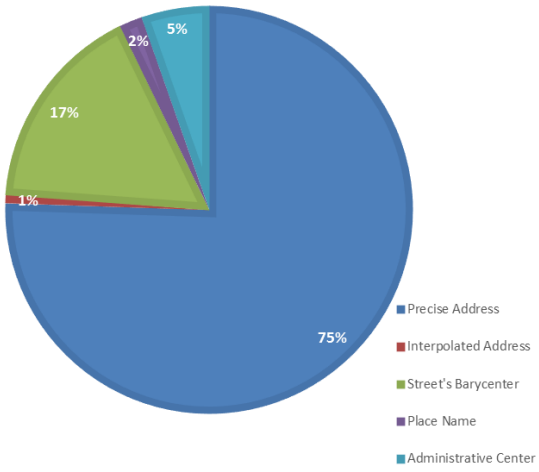
Source: Panel DEPP 1997, *Fichiers de gestion* (Ministry of Education).
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

Appendix 1.C: Computing distances from home to schools

In order to build Euclidean distances, we first consider the list of primary schools opened in 1996-1997 [43], which are geolocalized by the National Geographic Institute. They correspond to the school supply that parents faced when they made the choice of elementary school for their child. Second, we locate students' households relying on DEPP's *fichiers de gestion*, which provide Panel 1997 students' addresses. We complete by hand imprecise addresses, which are more frequently linked to socially deprived areas. Finally, data provide coordinates of the majority of students, as we are unable to localize 4% of students only. Overall, 75% of addresses are localized on a very precise level, and the rest mainly corresponds to households localized in rural areas.

Holding both students' households and primary schools' coordinates allows us to compute for each child the euclidean distance from their home to the nearest private and public schools.

FIGURE 1.15 – Geolocation quality



Source: Fichiers de gestion (Ministry of Education), author's geolocation.

43. This list is provided by the DEPP in *Base Centrale des Etablissements*.

2 The impact of certifying training programs on unemployment duration in France

This chapter is based on joint work with Marc Ferracci and Denis Fougère

Abstract

Job search-oriented and short-term training programs for unemployed individuals are traditionally compared to long-term programs providing occupational skills. We take the opportunity of rich French data to analyze potential efficiency mechanisms of training programs targeting unemployed individuals. As increasing efforts are put on the access to certification through vocational training at the European and national level, we choose to assess to which extent certifying programs reduce trainees' unemployment duration compared to other programs. Importantly, we make the distinction between programs preparing for a certification delivered by a ministry and those preparing for other types of certifications. Relying on the timing-of-events methodology to control both for observed and unobserved heterogeneity, we find that programs preparing for a certification accelerate the transition out from unemployment only when they prepare for a diploma delivered by a ministry. In line with the rest of the literature, we also find that the effect of certifying programs is higher for women, though the acquisition of ministry certifications mostly benefit to men. Our results suggest that raising the readability of the certification system and improving the quality of programs delivered would increase the probability to find a job for unemployed individuals participating to a training program.

JEL codes: H52, I26, I38, J64.

Keywords: training programs, unemployment duration, unobserved heterogeneity.

2.1 Introduction

The French labor market is characterized by a high unemployment rate: it reached 10.5% by the end of 2015. Among active labor market policies implemented to reduce it, vocational training programs are publicly funded in order to provide occupational skills to job seekers or to support them in their job search. Among those programs, unemployed individuals are increasingly prompted to participate to those preparing for a certification, for example through their individual learning account [1]. The effort put on the access to certification is also observed at the European level: to make the EU "the most competitive and dynamic knowledge-based economy in the world", the 2000 Lisbon European Council set as an objective to multiply the number of certifications and individuals holding a degree. In this study, we consider to which extent a certification acquisition could be beneficial for unemployed individuals to find a job. Importantly, we make the distinction between programs preparing for a certification delivered by a ministry and other types of certifications.

So far, the evaluation of training programs content mostly focused on the comparison between human capital intensive training programs and job search-oriented ones. The acquisition of a certification is mostly observed with the first type of program, but the causal effect of a degree acquisition has never been demonstrated. Most of studies show that long-term, human capital intensive training programs have a positive effect on the employment probability, but the mechanical extension of the trainees' unemployment duration (so called "lock-in" effects) makes these impacts insignificant compared to job search programs (Weber et Hofer 2004; Richardson et Gerard J Berg 2013; Osikominu 2013). Short-term programs seem to be the most efficient ones as they increase the transition rate of participants earlier than longer programs. For example, in Germany, a large literature aims at comparing programs extending professional skills to less content-specific programs [2] (Biewen et al. 2007; Fitzenberger et Speckesser 2007; Lechner et al. 2011). Most of studies find that the positive effect of the former type of program is mitigated or canceled by the important associated lock-in effects. Likewise, a comparison of "welfare-to-work" programs in the US [3] show that some positive effects of intensive human capital training appear with a delay but their impact is larger than job search assistance programs. However job search oriented programs are far less expensive than other training

1. In France, an individual learning account was implemented in 2015 to provide funding for training participation to programs preparing for a qualification or a certification. The list of eligible training programs is elaborated by social partners.

2. The first type of program is usually called a "further training" program, while the second type is a "retraining program".

3. Those programs were implemented since the late 1960s and aimed at getting welfare recipients back to employment. The "Human Capital Development" programs aimed at improving either basic or job-related skills of recipients while the "Labor Force Attachment" programs goal was to find a job rapidly. For example, unemployed individuals may learn in job clubs how to prepare an interview or to build resumes.

programs.

Importantly, this distinction does not fully allow to understand the mechanisms which enhance employability. An important channel of efficiency could be the acquisition of a certification. Indeed, the signaling theory developed by Spence (1973) shows that, in a context of asymmetry of information on the labor market, certification improves information regarding the worker's productivity. Empirically, the literature on "sheepskin effects" asks whether an extra year of education induces a more than proportional increase of wage when it leads to a certificate. When acquired through initial education, previous studies show evidence of important diploma signaling effects (Jaeger et Page 1996; Frazis 2002), but more recent ones do not find any impact of holding a diploma on wages (Clark et Martorell 2014). Additionally, there is no consensus on the acquisition of the General Education Diploma [4] (GED) impact on employment or earnings outcomes (Tyler et al. 2000; Tyler et al. 2003; Heckman et LaFontaine 2006; Heckman, Humphries et al. 2014; Jepsen, Mueser et al. 2016). The effect of certifications acquired through continuous training has been much less studied. However, the effect of acquiring a National Vocational Qualification of level 2 (NVQ2 [5]) through vocational training has been measured in the UK. Empirical evidence shows that some sub-groups of population seem to significantly benefit from the acquisition of a NVQ2, such as women (Coulon et al. 2008; Blanden et al. 2012) or low-educated individuals who upgrade their initial skills (Dearden et al. 2004; McIntosh et Garrett 2009; Dorsett et al. 2010). Labor market outcomes might also depend on the quality of the signal: Jepsen, Troske et al. (2014) show that associate's degrees and diplomas induce a substantial increase of income compared to certificates, especially for women [6].

In this article we ask whether the acquisition of a certification is an efficient tool for the insertion of job seekers on the labor market. For this purpose, we rely on an original dataset from the French Public Employment Service (PES) which precisely describes the content of training programs. We evaluate the impact of having followed a training program preparing for a certification on the probability of exiting from the unemployment register. Importantly, we first adopt a broad definition of what is considered as a certification, as it can be recognized by the State or not. We thus include diplomas delivered by public institutions, professional branches or private institutes. In a second part, we only consider as certifying programs those which prepare for a certification delivered by a ministry, as it is clear that they are recognized at the national level. It allows us to measure to which extent the type of certification matters.

4. Implemented in 1942 in the US, the aim was to provide a "second chance" to individuals who dropped from high school without any diploma.

5. National Vocational Qualifications are work based qualifications in the UK. A NVQ of level 2 corresponds to a *CAP* level in France.

6. In an American community college, the acquisition of a certificate does not require more than one year of course work; it is obtained through specific programs where students demonstrate a specific set of skills to potential employers.

Measuring a causal effect of training participation first requires to account for training assignment endogeneity. Indeed, participation is not random as caseworkers from the PES might assign to training the least employable individuals. On the other hand, the most motivated job seekers are more likely to apply for training participation. Second, exiting from unemployment and participating to a training program are competing risks, as such participation is not observable once the job seeker finds a job. Importantly, the latter risks are also correlated, as unobserved individual characteristics might affect both training participation and employability. We identify the effect of training participation relying on the timing-of-events methodology developed by Abbring et Van den Berg (2003), which allows to identify a causal effect accounting for the timing at which treatment occurs.

Surprisingly, we find that programs preparing for a certification do not increase the transition rate out from unemployment more rapidly than non-certifying programs. However the type of the certification which is prepared matters, as we show that programs preparing for a certification delivered by a ministry are more efficient than other programs. We also show that men mostly benefit from the acquisition of this type of certification, while the effect of programs preparing for any certification is slightly higher for women. Evidence of the higher efficiency of some training programs has important implications for the elaboration of active labor market policies. First, it should guide the public decision regarding training funding. As an example, in 2015 a national training plan targeting low-qualified unemployed individuals and long-term job-seekers has been implemented to fund 500,000 additional participations to training programs. This important investment doubles the number of training participations of unemployed individuals, mainly focusing on programs in emerging sectors. However there are few restrictions on the training program content, while our results show that attention should be paid to the certifying content of the program in order to maximize the efficiency of public investment [7]. Second, those results contribute to the debate on mechanisms of efficiency in the vocational training system. On the one hand, if the signal effect explains the largest part of the causal effect of ministry certifying programs, some effort should be made on the readability of the certifying system, in order to allow employers to better identify individuals holding the required skills. On the other hand, the positive effect of those programs on job seekers' employability could also be due to the associated curriculum, as well as to the quality of training providers. In this case, the efficiency of the investment in training could be raised by rationalizing the market of training organizations, setting some criteria on the level of trainers or on the degree to which pedagogical means are adapted to the public concerned.

The paper is organized as follows. First, we briefly describe the French training system (section 2.2), and describe our data (section 2.3). In section 2.4 we develop our empirical strategy. Results are presented in section 2.5 and we discuss them in section 2.6. We conclude in section 2.7.

7. This training plan allows funding for several types of programs, among which certifying programs but also adaptation to the workstation (*adaptation au poste de travail*) or new business development.

2.2 The French training system

The French training system for the unemployed is run by the Public Employment Service (PES), the administrative regions and the social partners. Job seekers receive a constant unemployment benefit during two years from the PES. If they participate to a training program and are still eligible to unemployment benefits, they are entitled to a specific remuneration [8] from the French PES. A trainee does not have any financial incentive to participate to a training course in order to benefit for unemployment insurance for a longer period as compensations are not cumulative; moreover the amount of both benefits is equal. When rights to unemployment benefits get exhausted before the end of the training program, individuals can benefit from a PES funding [9]. The State also provides a revenue to unemployed individuals who are not eligible to unemployment benefits, called *Régime de Stagiaire Public* (RSP). Second, training costs represent a different budget category. They are mainly funded by administrative regions (54% in 2013, see Cavan 2015) and the PES (23%). While the latter mainly aims at helping job seekers to rapidly go back to work, administrative regions traditionally fund human capital intensive training programs. All in all, training funding of job seekers and trainees remuneration represented 451 million euros in 2012 (Delort et Mesnard 2015).

Each job seeker elaborates a specific project together with a caseworker. This roadmap, called *Projet personnalisé d'accès l'emploi* (PPAE), defines the occupation, the sector and the reservation wage asked by each given job seeker. The PPAE also describes potential actions to be implemented by the job seeker, including training programs participation. In some cases, training programs include a preparation for an examination leading to certification. It can be a diploma which is recognized by the State through a national register [10]. Among them, certifications delivered by some ministries are automatically recognized, while others are registered only when specific criteria are fulfilled [11]. They can be delivered by consular organizations, private institutes or public ones, such as universities. Job seekers may also prepare a certification which is not recognized at the national level, but still delivered by these types of institutions. Finally, trainees can participate to programs preparing for the acquisition of particular certifications, corresponding to linguistic abilities or specific technologies, or for accreditations in specific fields [12]. Finally, table 2.1 provides a detailed list of the certifications that we are able to identify. Certifications de-

8. This remuneration is called *Allocation d'Aide au Retour à l'Emploi* (AREF).
9. This specific funding is called *Rémunération des formations de Pôle emploi* (RFPE).
10. This national register (*Répertoire National des Certifications Professionnelles*) includes all the certifications recognized by the State. It is regulated by a specific commission, called *Commission nationale certification professionnelle*.
11. A commission evaluates to what extent the certification is relevant regarding the labor demand, and checks the harmonization of the certification system.
12. This latter type of certifications is never recognized at the national level. For example job seekers can prepare driving licenses for specific trucks.

livered by a ministry are mainly delivered by the National Education Ministry, while 2% of certifications are elaborated through collective bargaining: *Certificats de Qualification Professionnelle* (CQP) are built by social partners in order to fill a particular demand in a sector. We are interested in the effect of the national recognition of a certification on unemployment duration, however we do not hold any information about the recognition by the State of acquired certifications. As a consequence, in a second part of our analysis we only consider as certifying programs those which prepare for a diploma delivered by a ministry, as the former are automatically registered [13].

Regarding training providers, 79% of training programs followed by unemployed individuals were delivered by a private organization in 2011 (Delort 2013). Most of them were non-profit organizations. Additionally, an important share of public structures is run by the National Ministry of Education [14] and by an agency affiliated to the National Ministry of Labor [15].

2.3 Data

2.3.1 Data sources and sample restrictions

2.3.1.1 Unemployment data

Our empirical analysis relies on a 1% sample of the *Fichier Historique* (FH) from the Public Employment Service, which contains information on French workers entering and leaving unemployment between January 2009 and September 2014 [16]. We observe individual characteristics such as gender, age, initial level of education [17] and unemployment recurrence since 2000. Entry and exit dates from the unemployment register are provided [18], as well as observed dates of training participation. We aggregate the unemployment spells which are separated by less than 30 days, which ensures that the exit from unemployment registers is stable and does not correspond to a very short employment contract.

13. Though some certifications delivered by professional branches are registered with the State, it is not automatic. Moreover those certifications are very specific to the professional field. We thus consider programs preparing for those certificates as non-certifying ones in the second part of our analysis.

14. *Groupements d'établissement* (GRETA) are local public teaching organizations which gather resources to provide vocational training for adults.

15. The *Association nationale pour la formation professionnelle des adultes* (AFPA) became in 2017 *Agence nationale pour la formation professionnelle des adultes*.

16. We exclude from the sample individuals living in overseas departments.

17. We drop from the sample observations for which the variable "education" is missing. It represents 1% of our sample.

18. The reason why the individual leaves the Public Employment Service register is badly completed in our data, which prevents us to link an exit from the administrative register to a transition toward employment. The outcome we measure is rigorously an exit from the Public Employment Service register. However, as mentioned below, restricting the sample to individuals below 55 years old reduces the share of individuals retiring after the exit from the PES register.

We consider the first four unemployment spells experienced by the job seeker since 2009, which leads to include a large majority of the sample [19] (see table 2.2).

We make further restrictions in order to work on a homogeneous sample. We exclude individuals who are trained as part of an agreement between the PES and a firm (*Action de Formation Préalable au Recrutement*, AFPR) as it is agreed before the program that the individual will be recruited upon completion. Individuals below 15 and over 55 years old are also dropped from the sample in order to study the effect of training participation of individuals who are likely to be in the labor market before and after the program attendance. Finally, we drop individuals who benefited from a *Contrat de Sécurisation Professionnelle* (CSP) or similar programs [20], which represents 2.2% of the initial sample. These programs aim at supporting individuals who are laid off for economic reasons, in firms of less than 1,000 workers. During 12 months, unemployed individuals get a specific allowance [21] and benefit from a particular assistance for reentering employment. As the compensation differs, these individuals are not comparable to other job seekers [22].

2.3.1.2 Training data

Our data also provide detailed information about training content [23]. It concerns individuals who follow a training program and are eligible to unemployment benefit (AREF) or alternative revenues [24]. Our analysis thus does not concern individuals who benefit from a State remuneration; in 2014, they represented less than 1% of the total amount of job seekers who were benefiting from any allowance while following a training program [25]. We have information on theoretical training dates, the program field and level, the training provider and whether or not it includes experience within a firm [26]. We also identify whe-

19. We drop less than 10% of observations from the initial sample. In the second part of our analysis, we consider the first 10 spells of each individual in order to identify the effect of ministry certifying programs.

20. The *Contrat de Sécurisation Professionnelle* replaced the *Convention de Reclassement Personnalisé* and *Contrat de Transition Professionnelle* in 2011.

21. This allowance, called *allocation de sécurisation*, represents 80% of the reference rate.

22. The training program duration is also badly measured, as it is automatically set to one year as soon as it is in a CSP context. This setting prevents from relying on duration data for those individuals.

23. The *Segment P2* is a specific part of the FH dataset. Data precision regarding the programs content comes from the fact that its characteristics are filled by the training provider in a specific form. The *Attestation d'Inscription en Stage* provides information regarding the timing of the course as well as its content. A second form (*Attestation d'Entrée en Stage*) is sent to the PES when the individual begins the program, which triggers the payment of the trainee's remuneration.

24. Alternative revenues correspond to *Rémunération Formation Pôle Emploi* (RFPE) or *Allocation de sécurisation professionnelle* (ASP). The former is an allocation for job seekers following a training program but whose unemployment insurance gets exhausted before the end of the program. The latter is the allowance for *Contrats de Sécurisation Professionnelle* (CSP) recipients.

25. This share is computed relying on a note from the Public Employment Service (Pôle-Emploi 2015).

26. In detail, we do not rely on theoretical training dates to measure training duration, but it provides a good idea of the completion rate. Moreover we know the training provider SIRET but we cannot match it to any database to make the distinction between several types of training centers. Finally, our data do

ther or not individuals prepare a diploma during the program. Though we do not know whether individuals obtain the diploma they were preparing during the training program, we assume that most of individuals do. Public statistics show that the completion rate among adults preparing a diploma delivered by a ministry is quite high: it reaches 80% for certifications delivered by the National Ministry of Employment [27] and stands between 89% and 79% for level V diplomas delivered by the National Ministries of Education and Agriculture [28].

Because individuals might successively participate in multiple different programs of a same training track, we aggregate the programs at the track level [29]. Then, we only consider the impact of the *first* training program followed during an unemployment spell and do not account for additional training programs, which can artificially extend the measured unemployment duration. However, table 2.3 shows that individuals following multiple training programs in the same unemployment spell represent a small share of the sample: 6% of trainees participate in two programs and less than 1% in more than two.

We compare theoretical training dates to observed training dates in order to identify dropout [30]. In our sample, 9% of trainees left the program earlier than the theoretical exit date. More precisely, 4% dropped from the program and returned to unemployment, while 5% found a job during the program [31]. During training, individuals are more likely to drop from the program toward employment when it is a certifying one (for more details, see appendix 2.C).

2.3.2 Certifications characteristics and training content

Our data show that programs in the tertiary and business support fields each concentrate one third of the total amount of training programs followed. While the former includes the longest and most certifying training programs, the latter regroups short courses including less on-the-job training than average.

Training programs are divided into four fields split into two main types of programs: general and business support programs are short programs aiming at a rapid insertion on the labor market, while training in the manufacturing/agriculture and tertiary fields pro-

not provide any information regarding the duration of the experience within a firm, nor regarding the firm involved in the program.

27. The latter are called *Titres professionnels du Ministère de l'Emploi* (see DGEFP 2015).

28. The latter correspond to *Brevet d'Etudes Professionnelles* (BEP) and *Certificat d'Aptitude Professionnelle* (CAP). See J. Robin (2016).

29. We consider that two training programs are part of the same track when they are separated by less than 65 days, which corresponds to the average duration of a break during summer holidays. Training programs also need to have the first three digits of their training program (*formacode*) in common.

30. We consider that an individual dropped from the training program if the observed ending date of the program differs by more than 15 days from the theoretical ending date, in order to account for measurement error.

31. The fact that few individuals leave training before the end of the program reflects the importance of the lock-in effect which automatically extends the job seeker unemployment duration.

vides occupational skills. General training mainly concerns career guidance and assistance with the job search. As shown in table 2.4, this type of program lasts around 3.6 months while the average program duration is 5 months. They also include less on-the-job training. They only represent 16% of training programs, however the most followed non-certifying programs fall within this category (table 2.5). Half of training programs delivered in the general field prepare for a certification, which is much less important than in other sectors [32]. Then, one third of training programs is related to business support; they are the shortest programs. They mainly concern the acquisition of skills in office automation softwares, specific driving licenses and train individuals to create their own firm [33]. On the other hand, human capital intensive training programs cover programs delivered in the manufacturing, agricultural and tertiary fields. They last between 5 and 8 months on average, include more certification preparation and on-the-job training, with almost 75% of the tertiary field programs preparing for a certification. More than half of these training programs aim at reaching a level IV or V, whether preparing or not for a certification [34]. Moreover, the three most popular certifying programs concern the tertiary field, mainly for personal assistance services [35]. Programs delivered in in the manufacturing field are less numerous. They deliver, for example, skills for sanitation facility or welding, while in the agriculture field many programs concern landscaping skills.

Overall, table 2.6 shows that training programs lead to a certification in 60% of cases. They are the longest ones, especially when preparing for a certification elaborated by a ministry: in that case programs last slightly less than 8 months on average. Ministry certifying programs represent 13% of all training programs, which means that most programs prepare for a certification which are not automatically recognized by the State. Finally, certifying programs include less experience within a firm than other programs, except those preparing for a ministry certification.

2.3.3 Individuals' characteristics and unemployment durations

Some individuals' observed characteristics are strongly associated to specific training programs types. Table 2.7 shows the results of an OLS regression on the probability to enter into *(i)* any type of training program, *(ii)* a certifying program and *(iii)* a training program preparing for a ministry certification. Note that the fist regression compares trai-

32. Among certifying programs in the general field, many certifications are being prepared in order to confirm the acquisition of the common core of knowledge and skills. For example the acquisition of the *Certificat de formation générale* (CFG) ensures the acquisition of basic knowledge in French, mathematics and "professional and social life". It can be acquired by adults who followed a training program or a social inclusion program.

33. Firm creation programs can deliver a certification such as *Passeport pour Entreprendre*, which is recognized by the French national commission of certification (CNCP).

34. This type of program aims at reaching a level of the French BAC, or CAP/BEP.

35. The main programs in the tertiary field are nurse, assistant nurse and homecare assistant.

nees' to non-trainees' characteristics, while the following estimations only include trainees. Though they access less frequently to training, the most educated individuals are, among trainees, the most likely to access to courses preparing for a certification, especially when the latter is delivered by a ministry. On the contrary, individuals initially holding a level IV diploma are the most numerous to prepare another type of certification. Regarding the effect of age, the youngest individuals get mostly assigned to certifying programs.

The unemployment spell duration increases when the individual participate to a training program. A first non-parametric estimation of the exit from unemployment hazard shows that individuals who participate to a training session experience longer unemployment durations than individuals who were not trained (see figure 2.1). It is due to a "lock-in-effect" which mechanically raises the duration during which they are unemployed. However individuals enrolled in certifying programs still experience slightly longer unemployment durations without accounting for lock-in effects [36]. It could first be due to a negative selection effect: if the PES assigns the least employable individuals to training programs, it could explain that participants have a higher probability to stay unemployed in the absence of any training participation. This pattern can also be explained by trainees' higher reservation wage or by an increase of the trainees' reservation wage once they look for a job after having benefited from a training program [37]. We then restrict certifying training programs to those who prepare for a ministry certification and consider that other programs are non-certifying ones. Figure 2.2 first shows that after an initial lock-in effect, the survival hazard function of trainees in both types of programs becomes similar again. Moreover the previously observed negative selection bias disappears for certifying programs when considering the unemployment duration excluding the training spell, as trainees in ministry certifying programs exit earlier from unemployment than other trainees. It thus appear that, without controlling for observed or unobserved characteristics, participation to ministry certifying programs raises the probability to find a job or that more employable individuals get enrolled into this type of programs.

Having those stylized facts in mind, we implement a specific estimation method to check whether the shorter duration observed after the exit from ministry certifying programs is causal or not.

2.4 Empirical strategy

We develop a competing risks duration model where an individual has the possibility either to be assigned to a certifying training program, a non-certifying program, or to find a job. The latter are competing risks in the sense that once an individual finds a job, we

36. We artificially set to zero the training duration in the second part of the graph.

37. Le Barbanchon et al. (2017) show that higher reservation wages predict a longer unemployment period.

cannot observe training participation anymore. Those risks are also correlated, as some observed and unobserved characteristics might jointly influence the individual's trajectory. We set T, T_{p1} and T_{p2} the duration until exit from unemployment, entry into the first and second type of training, respectively. T_{p1} (T_{p2}) is thus the duration of unemployment until entry into a certifying training program (non-certifying program).

Importantly, comparing observed unemployment durations of individuals who entered certifying programs to other trainees will not allow to identify the most efficient program. Following Richardson et Gerard J Berg (2013) we argue that, if individuals who participate to a certifying program experience a shorter unemployment duration than those enrolled in a non-certifying program, it can be for two reasons: either certifying programs are more efficient at reducing unemployment durations, or individuals participating to those programs would have exited unemployment at a faster rate anyway. As we observe several unemployment-employment transitions after the transitions unemployment-certifying training program, *conditional on observed and unobserved characteristics*, we can answer to this question. This is the so-called "timing-of-events" method developed by Abbring et Van den Berg (2003).

We rely on three major assumptions: first, the *no-anticipation assumption* requires that individuals do not behave differently once they know their assignment training date. In our case, it is unlikely that individuals anticipate the exact timing at which they enter training, as the French PES does not fix any statutory date beyond which training becomes mandatory. Second, according to the *conditional independence assumption*, controlling both for observed and unobserved characteristics allows to drop out any selection bias. For identification issues, we make an additional assumption regarding the way the treatment and other characteristics should affect the individual hazard rate. We assume that individuals' hazard rates shift proportionally to their own characteristics, whether observed or not, which corresponds to the *proportional hazard hypothesis*.

In our estimation, each observation corresponds to the succession of unemployment spells of job seekers [38] between 2009 and 2013. Observing several spells for each individual makes easier the estimation of the joint distribution of the unobserved heterogeneity terms. In addition to that, controlling for time-varying covariates [39] provides a more robust source of identification than time-invariant covariates (Gaure et al. 2007 [40]). We model duration in continuous time, as the unemployment spell information is provided on a daily basis in the PES registers. As shown in figure 2.3, we consider that when an individual is unemployed she can either find a job, or start a training program. The latter can prepare or not for a certification. Most of trainees complete the program they began, go back to

38. We only consider the first four spells experienced by each individual.

39. We control for the quarterly unemployment rate at the regional level.

40. The authors show that lagged time-varying variables provide an exclusion restriction, as the transition hazard rates are affected by these variables only through the selection process. Bergemann et al. (2017) highlight that "individuals with the same observed characteristics x in period t but different values of lagged time-varying variables should only have a different transition probability if the composition with respect to unobserved heterogeneity is different".

unemployment once they leave the program, and then find a job. We observe relatively few individuals finding a job right after their training program [41]. We rely on observed program exits in order to measure the true unemployment duration after training.

2.4.1 Benchmark model

We denote h_{UE}, h_{UTC} and h_{UTNC} the transition rate from unemployment to employment, certifying and non-certifying training programs respectively. These transition rates are assumed to follow a Weibull law, characterized by parameters α_{UE}, α_{UTC} and α_{UTNC} respectively. We have in mind that a piecewise constant hazard is a more flexible specification, however we have to make a trade-off between flexibility and feasibility as we deal with a non-linear model, with multiple states and unobserved heterogeneity. Estimating a piecewise constant hazard would allow more flexibility but it requires to add many new parameters (for example splitting the hazard in 4 periods would require to add 12 parameters), while a small number of periods does not allow neither to estimate the hazard correctly [42]. Additionally to this baseline hazard, we assume that individuals' hazard rates shift proportionally to their own characteristics, whether observed or not. We set $x(t)$ as a vector of observable covariates which vary over time [43]. They include individual characteristics (initial level of education, age, gender and whether she already experienced an unemployment episode) and contextual variables (year at which she entered into unemployment and the quarterly unemployment rate at that time in the department [44]).

We set ν_{UE}, ν_{UTC} and ν_{UTNC} as unobserved individual characteristics which influence the probability of transition from unemployment to exit out from unemployment, from unemployment to certifying and non-certifying training programs respectively. We denote t, t_{p1} and t_{p2} the realizations of T, T_{p1} and T_{p2}. We also denote t_{e1} (t_{e2}) the duration from entry into unemployment until certifying (non-certifying) program exit. The difference $t_{e1} - t_{p1}$ ($t_{e2} - t_{p2}$) corresponds to the certifying (non-certifying) program duration. Transition rates from unemployment toward these three risks are specified as follows:

41. Individuals leaving unemployment right after their training program completion represent 6.5% of trainees in our data. When the individual immediately leaves unemployment after training exit, we artificially add one day during which the individual is still unemployed in order to keep the model tractable.

42. Moreover, figure 2.4 shows that the hazard function of exit from unemployment relatively fits a Weibull law.

43. In the benchmark model accounting for unobserved heterogeneity, x does not include a constant : in hazard rates h_{UE}, h_{UTC} and h_{UTNC}, the constant terms are represented by the means of heterogeneity terms ν_{UE}, ν_{UTC} and ν_{UTNC}, respectively.

44. We match our data to INSEE data providing the unemployment rate measured quarterly in each department.

$$h_{UE} = \alpha_{UE} * t^{\alpha_{UE}-1} * exp\Bigg(x(t)'\beta_0 + \beta_{01}.\mathbf{1}(TC = 1)\mathbf{1}(t > t_{e1}) + \beta_{02}.\mathbf{1}(TNC = 1)\mathbf{1}(t > t_{e2}) + \nu_{UE} \Bigg) \tag{2.1}$$

$$h_{UTC} = \alpha_{UTC} * t_{p1}^{\alpha_{UTC}-1} * exp\Big(x(t)'\beta_1 + \nu_{UTC} \Big) \tag{2.2}$$

$$h_{UTNC} = \alpha_{UTNC} * t_{p2}^{\alpha_{UTNC}-1} * exp\Big(x(t)'\beta_2 + \nu_{UTNC} \Big) \tag{2.3}$$

where $(TC = 1)$ if the job seeker followed a certifying training program, and $(TNC = 1)$ if he followed a non-certifying program. β_0, β_1 and β_2 measure the effect of observed characteristics on the transition rates toward employment, certifying and non-certifying programs participation. We are interested in the β_{01} and β_{02} parameters, which correspond to the impact of getting enrolled into a certifying and non-certifying program. We first want to test the hypothesis that following a training program has a positive effect on the hazard rate out from unemployment, i.e $\beta_{01} > 0$ and $\beta_{02} > 0$. We then check the hypothesis that a training program preparing for a certification boosts the exit out from unemployment compared to another type of training, i.e $\beta_{01} > \beta_{02}$.

Following Richardson et Gerard J Berg (2013) we measure the *ex-post* effect of both types of training program, which induces that between t_{p1} and t_{e1} (t_{p2} and t_{e2}), the "clock" stops running. Said differently, a program is considered as efficient if it allows to reduce the job seeker's ex-post duration in unemployment, independently from the initial lock-in effect. We address the effect on the overall unemployment duration in section 2.5.2.

2.4.2 Unobserved heterogeneity specification

Individuals present unobserved characteristics associated to each risk: transition to employment (V_{UE}), certifying training (V_{UTC}) and non-certifying training program participation [45] (V_{UTNC}). We consider the joint distribution of the unobserved heterogeneity as bivariate, with two unrestricted mass point locations for each term. Let ν_{UE}^1, ν_{UE}^2, ν_{UTC}^3, ν_{UTC}^4, ν_{UTNC}^5 and ν_{UTNC}^6 be the points of support of V_{UE}, V_{UTC} and V_{UTNC} respectively. The associated probabilities are denoted as $p_{jkl} = Pr(V_{UE} = \nu_{UE}^j, V_{UTC} = \nu_{UTC}^k, V_{UTNC} = \nu_{UTNC}^l)$ with $j = 1, 2$, $k = 3, 4$ and $l = 5, 6$.

45. Notations adopted here follow the presentation made by Richardson et Gerard J Berg (2013).

We parameterize those probabilities as a multinomial logit in order that they each range between 0 and 1, and that they sum to one. It gives the following specification of the normalized probability π_{jkl} :

$$\pi_{jkl} = \frac{exp(p_{jkl})}{\sum_{j^*=1}^{2} \sum_{k^*=3}^{4} \sum_{l^*=5}^{6} exp(p_{jkl})}$$

We normalize $p_{246} = 0$ such that the π_{jkl} probabilities sum to one.

The final log likelihood function is available in appendix 2.D. We estimate this model by maximum likelihood.

2.4.3 Restricting the definition of a certifying training program

We restrict our definition of certifying programs to those preparing for certifications delivered by a ministry. We use the same specification, where h_{UTC} corresponds to the transition rate to ministry certifying programs and h_{UTNC} is the transition hazard to any other program [46]. Similarly to our benchmark model, unobserved heterogeneity terms V_{UE}, V_{UTC} and V_{UTNC} are associated to three transitions from unemployment, namely the transition toward exit from the PES register, entry into a ministry certifying program and entry into another type of training program.

2.5 Results

2.5.1 Main results

2.5.1.1 Effect of training participation

We first estimate the impact of participation to any type of training program (see table 2.8). Without accounting for unobserved heterogeneity, following a training program raises the transition rate from unemployment to employment by 31% (exp(0.271)-1). This impact remains stable when accounting for unobserved characteristics, however the smallest value of the Akaike Information Criterion (AIC) is reached in this second case, which suggests that the model fit is still improved when accounting for unobserved heterogeneity. The training effect we estimate is slightly more important than the one presented by Crépon, Ferracci et al. (2012), which could be due to several reasons. First, we only focus on individuals eligible to unemployment benefits, while the authors look both at eligible individuals and welfare recipients. Second, we exclude job seekers who benefit

46. As there are fewer individuals following the first type of training, we now rely on the first 10 spells to observe enough participations to programs of that type.

from *Contrat de Sécurisation Professionnelle*, as they benefit from a better compensation. Samples are thus not fully comparable [47].

2.5.1.2 Baseline estimation

Table 2.9 presents the main results of our baseline estimation. As a comparison, we first remove unobserved heterogeneity terms: we find that following a non-certifying training program raises individuals' transition rate from unemployment to employment by exp(0.315)-1 = 37% while those preparing for a certification raise it by 27%. Accounting for unobserved characteristics mainly raises the estimated effect of certifying training programs: participating to such a program raises the hazard rate from unemployment to exit from the PES register by 33% while assignment to a non-certifying program raises this hazard rate by 39%. Though both coefficients still do not significantly differ, it is worth noticing the slightly more important negative bias for enrollment in certifying programs. The inclusion of unobserved heterogeneity relatively improves the model fit, as shown by the comparison of AIC values and the correlation between unobserved heterogeneity terms. Table 2.10 shows that there is a positive correlation between unobserved heterogeneity terms associated to exit from unemployment and participation to any type of training. However the latter is stronger for the second type of program, as the correlation term between V_{UE} and V_{UTNC} equals 0.344. It reflects the fact that, *conditional on observable characteristics*, employable individuals are more likely to participate to a training program, particularly to a non-certifying one.

Table 2.11 provides the detailed results of our baseline estimation. The least educated job seekers are very less likely than highly-educated individuals to enter into a certifying program, while there is not any significant difference between individuals holding a level IV/V diploma and more educated ones. Moreover, individuals who already experienced at least one unemployment spell are less likely than others to participate to a training program. Regarding employability determinants, the transition rate from unemployment to exit from the PES registers decreases with age, which might be due to the fact that the youngest unemployed individuals are the most likely to accept short term labor contracts. It might also be the case for individuals who do not hold any diploma.

2.5.1.3 Estimation results relying on a more restrictive definition of a certification

We now restrict the definition of certifying programs to those preparing for a certification delivered by a ministry and consider that other training programs are non-certifying ones. Table 2.9 shows that the first type of program raises the transition rate from unemployment to the exit from the PES registers by 43% while other programs raise it by 21%.

47. Moreover, the training duration of job seekers benefiting from this agreement is systematically set to one year in the data, which might lead to overestimate the lock-in effect associated to training participation in the mentioned study.

Interestingly, accounting for unobserved heterogeneity widens the gap between both estimated effects. We additionally show in table 2.12 that the probability to participate to a certifying program highly decreases with age, and that individuals holding the lowest level of diploma are even less likely to participate to this type of program compared to the previous definition of a certification. Women enter more often into certifying programs than men. A further analysis allows us to estimate the specific effect of those programs by gender.

2.5.1.4 Heterogeneous effects by gender

We first consider the initial definition of certifying programs, which prepare to any type of certification. Table 2.13 shows that training programs preparing for a certification are more efficient than non-certifying programs specifically when considering *women*: it raises their transition rate out from unemployment by 36% as opposed to 17% for men, who benefit more from non-certifying programs. This is in line with previous studies [48], which explain this difference in benefits by different training impacts across occupations. For example, women work more frequently in the health and social sectors, in which certifications are required more often than in other fields. The measured gap might thus reflect a composition effect across sectors rather than a gender difference. Surprisingly, when restricting certifying programs to those preparing for a ministry certification, we find that its positive effect on exit from unemployment is mainly driven by the impact on men's unemployment duration: participating to this type of program raises the hazard rate by 55% for men against 31% for women. Those results suggest that the quality of the prepared certification makes the difference mostly for men; on the contrary, women might benefit from the acquisition of certifications highly recognized in specific sectors, without requiring to be "labeled" by any ministry.

2.5.1.5 Heterogeneous effects over time

We now provide a descriptive analysis of the effect of training participation over time. Figure 2.5 shows the transition rate out from unemployment after the exit from a certifying training, considering our two successive definitions. It is compared to trainees participating to non-certifying programs. For both types of certifying programs, the probability to exit from the PES register is the highest when the program ends, and it slightly drops right after. However this probability regularly decreases during two years for certifying programs trainees, while it increases again for ministry certifying programs participants.

This pattern has to be considered with caution as we do not account for observed and unobserved heterogeneity: it is possible that ministry certifying programs trainees stay highly motivated after the end of the program, which leads to an increase of the probability to find a job. It is also possible that skills delivered during those specific programs

48. See Lechner et al. (2011); Osikominu (2013).

depreciate at a slower rate than in other training programs. Interestingly, the probability to exit from unemployment becomes smaller than other trainees slightly after two years when individuals participated to the first type of certifying program. Again, this pattern has different possible interpretations: it might be that individuals assigned to certifying training programs are less employable than other trainees, which induces that they exit later from unemployment. It is also possible that, after a long period of unemployment consecutively to a certifying program, motivation decreases more than after non-certifying programs, or that such a long unemployment period combined to a certifying program provides a negative signal on the labor market.

2.5.2 Robustness checks

We implement several robustness estimations to check the validity of our baseline estimation results, i.e those obtained considering the least restrictive definition of a certification. We present them in table 2.14.

2.5.2.1 Excluding unemployed individuals working under a subsidized contract

We first verify that our estimations are not driven by a specific part of the unemployed population. Our sample includes individuals registered in a specific category as they benefit from a subsidized contract [49]. A sizable part of these individuals follow a training program alongside their work. Including this population in our benchmark sample might bias our results downward, as those individuals are not asked by the PES to immediately look for a job. Column (2) of table 2.14 shows that the effect of training participation slightly increases compared to previous estimations, as participating to a program preparing for a certification raises the exit hazard rate by 38%. This effect is still not significantly different from the one of non-certifying training programs.

2.5.2.2 Controlling for the researched sector of activity

We additionally control for the field in which the unemployed individuals are looking for a job. This information is filled by the job seeker at the beginning of the unemployment spell [50]. Our baseline estimation results are robust to the inclusion of those controls (see column 3).

49. They are classified in the 5th category, “demandeurs d’emploi non tenus de faire de actes positifs de recherche d’emploi, en emploi”.

50. The individual provides the “ROME code” of the job he is looking for. The first letter informs on the field of the researched job. We are thus able to identify the following categories: agriculture, retail, and banking, communication, building, tourism, arts, industry, health and business support.

2.5.2.3 Restricting the observation window to the two first years of eligibility

Then, we check whether the measured unemployment durations are censored at the 24th month of unemployment. If we observe a higher exit rate at the end of the second year of unemployment, it might reflect the termination of unemployment benefits rather than the beginning of a new job [51]. Such a censored framework would bias our estimated results as we would not measure the true ex-post unemployment duration. We run a robustness check where we restrict the sample to the two first years of unemployment of each spell, which corresponds to the period during which individuals are entitled to unemployment benefits. We treat as censored training programs which occur after the two first years of unemployment [52]. However those results have to be cautiously considered as we only provide estimation results without controlling for unobserved heterogeneity [53]. Column (4) of table 2.14 shows that this restriction slightly raises the gap between certifying and non-certifying programs effects. Figure 2.4 additionally shows the hazard rate of the exit from the PES register for all job seekers in our sample. We do not observe any spike at the 24 months threshold, which strengthens the hypothesis that we measure the true unemployment durations.

2.5.2.4 Lock-in effects

We considered so far the ex-post effect of both training programs; we now assess whether training participation decreases the overall unemployment duration. Tables 2.15 and 2.16 provide a descriptive analysis of trainees' unemployment durations. They show that most individuals participating to certifying programs enter training slightly later than those who get enrolled in a non-certifying program and experience longer training spells. Though ex-post unemployment durations are sometimes shorter after certifying programs participation, mainly in the tertiary field, they do not compensate the longer unemployment spell previously experienced.

The last column of table 2.14 provides additional suggestive evidence [54] on this issue: training participation raises the overall unemployment duration, especially when it prepares for a certification. While our benchmark estimation showed that certifying and non-certifying programs perform similarly, these results highlight that it is at the expense of different unemployment durations. It suggests that the second type of program is more efficient from a cost-benefit perspective. However it is possible that this difference de-

51. Individuals are required to register at the PES to get unemployment benefits, however there is no such obligation when those benefits are over.

52. In details, we consider the first 723 days of unemployment for each spell, which corresponds to two years minus one week of unemployment, as we do not want to include the 24 months threshold in our analysis.

53. This particular setting raises the share of censored observations, which makes more difficult the estimation of all unobserved heterogeneity parameters.

54. We do not control for unobserved heterogeneity characteristics. The main change in the specification is that we now "let the clock running" during the training participation.

creases when considering ministry certifying programs, as the latter perform better than other programs. Moreover this analysis is less relevant from a welfare point of view, as the accumulation of human capital should enhance individuals' employability in the future, and should be evaluated during a longer window than an unemployment spell.

2.6 Interpretation of the treatment effect estimates

We now analyze the potential mechanisms through which ministry certifying programs could positively affect the job seekers' exit out from unemployment. The signaling effect of the prepared diploma might substantially increase the employability of individuals who obtain the certification. However our empirical strategy does not allow us to disentangle this effect from the impact of the program quality itself, which could affect trainees' skills independently from the acquisition of a diploma.

Ministry certifying programs effect on unemployment durations might come from the higher *signaling effect* on the labor market compared to other types of certifications. Few studies focus on the variability of the signaling effect as a function of the type of certification, especially when considering awards acquired through continuous training. Jepsen, Troske et al. (2014) compare returns to different types of certifications delivered in community colleges [55]: certificates, diplomas and associate's degrees [56]. The authors show that the type of certification does matter, as the acquisition of associate's degrees or diplomas induce a substantial increase of income compared to certificates, especially for women [57]. They separately consider the returns for the number of credits earned and the additional gain from the award itself, in order to identify a potential "sheepskin effect", and show that most of the increase in earnings induced by associate's degrees and diplomas comes from the award acquisition rather than from the number of credits earned. In our case, as certifications delivered by a ministry are well-known when acquired through initial education, it is likely that they are well-identified on the labor market by employers.

The substantial effect of ministry certifying programs might also be partly due to the longer duration and the higher quality of the program itself. In our sample, this type of programs lasts 7.6 months against 5 on average. A *longer program duration* mechanically

55. Community colleges are public institutions of higher education that mostly offer two years programs participation in the USA. In their data, Jepsen, Mueser et al. (2016) consider a population between 20 and 60 years old, which confirms that individuals can acquire these diplomas as part of continuous training.

56. Certificates and diplomas are more common in technical fields, however the latter require more than one year of study, contrary to certificates. Associate's degrees include general and specific courses, and the curricula is comparable to the first 2 years of a 4-year college.

57. The authors show that associate's degrees and diplomas lead to quarterly earnings returns of nearly $2400 for women and $1500 for men, while they induce smaller returns for certificates. Associate's degrees and diplomas are also associated with large gains in employment.

allows a trainee to accumulate more human capital. However an important amount of time might not automatically allow the acquisition of relevant skills: it is likely that ministry certifying programs combine a sufficient amount of class hours to a high *training quality*. The literature on initial education provides several insights in order to better understand mechanisms of efficiency. When analyzing the determinants of educational quality, Lee et Barro (2001) mention teachers' salary and educational level, the pupil-teacher ratios and the availability of teaching materials. Unfortunately our data do not allow us to check the link between those criteria and trainees' achievement. However we are still able to identify two training providers: the training center network affiliated to the National Ministry of Education (GRETA) and the former association linked to the National Ministry of Labor (AFPA). In our data, 39% of ministry certifying training programs are delivered by the first type of provider. We expect that standards of quality which are implemented, such as the attention paid to the training staff qualifications, might positively affect the overall quality of delivered program [58]. Then, the *curricula content* of ministry certifying programs should also be considered: certifications delivered by the National Ministry of Education induce a mix between general and vocational courses [59]. Some studies highlight that workers with general education will better adapt to new technologies (Krueger et Kumar 2002; Krueger et Kumar 2004), and are more able to take any career-related training when they become older (Hanushek, Schwerdt et al. 2017). Employers might then value such a combination between specific and general skills. Moreover, ministry certifying programs are also linked to other training contents which could increase the program quality. First, those programs include slightly more training within a firm than other programs [60]. Second, ministry certifying programs are mostly delivered in specific fields (agriculture, manufacturing or tertiary field), while many studies highlight that job-search programs are less efficient to reduce post-training unemployment duration (Biewen et al. 2007; Fitzenberger et Speckesser 2007; Lechner et al. 2011).

We also argue that *the course composition* might partly explain the estimated effect of ministry certifying programs. Indeed, we showed that among trainees the most educated individuals attend this type of programs, which can induce two potential effects. First, the important literature on peer effects in initial education [61] shows that the presence of high ability students in a class raises the individual achievement. Though mechanisms might partly differ for adults' classes, we expect that the presence of initially high-educated individuals positively impacts the achievement of other trainees in the same class. Second,

58. A labeling system has been implemented by the National Ministry of Education, that providers affiliated to the Ministry can acquire by satisfying some quality criteria. Among the requirements for the provision of ongoing training, the will to provide personalized counseling and to build an individualized training track is mentioned. Pedagogical support should be adapted to each program, and the training staff qualification should be relevant.

59. See Rose (2007a).

60. Among ministry certifying programs, 69% include on-the-job training against 63% on average.

61. For a detailed literature review, see Sacerdote et al. 2011.

meeting trainees looking for a job in the same field could generate positive network effects. Jacobson et al. (2005) argue that part of the positive effect of a year of community college can be explained by an expanded network of contacts.

Finally, a potential reason why we observe such positive effects is that ministry certifying programs are followed by a relatively small share of job seekers, while it has been shown that some active labor market policies might trigger important *displacement effects* (Cahuc et al. 2008; Albrecht et al. 2009; Crépon, Duflo et al. 2013; Ferracci et al. 2014). In our data, 3% of job seekers follow a certifying program and less than 1% attend a ministry certifying one.

2.7 Conclusion

In this study we compare the effect of programs preparing for a certification to those which do not on the hazard of exit from unemployment in the French labor market. Additionally, our data allow us identifying programs preparing for a certification delivered by a ministry. Relying on the timing-of-events methodology, we account for observed and unobserved individual characteristics in order to measure the causal effect of training participation on the transition rate out from unemployment.

Our analysis shows that certifying training programs are as efficient as others to reduce the ex-post unemployment duration. However, among them, those preparing for a ministry certification do significantly accelerate the transition out from unemployment. Participating to a certifying training program is more efficient for women compared to men, which might be due to occupational preferences, but men mostly benefit from the acquisition of certifications recognized at the national level. Several mechanisms could explain the higher efficiency of ministry certifying programs: first, they prepare for a certification which is associated to an important positive signal on the labor market. Second, the curriculum delivered and the quality of training providers might contribute to raise the level of trainees' skills. Those possible channels have different implications in terms of public policy recommendations: on the one hand, the attention paid to the recognition of a diploma at the national level would require to increase the readability of the certifying system in order to raise employers' confidence in other diplomas than those delivered by a ministry. On the other hand, if ministry certifications are mostly prepared with high quality training providers, raising the quality standards of training providers would allow to increase the overall efficiency of training programs targeting unemployed individuals.

We assume that certifying programs costs are higher than the non-certifying ones, mainly because of their longer duration and higher requirements in terms of the instructors' qualifications. However, as we do not hold any detailed information regarding training programs costs depending on their content, we cannot implement a cost-benefit analysis. From a public policy perspective, it is worthwhile to highlight that certifying programs are

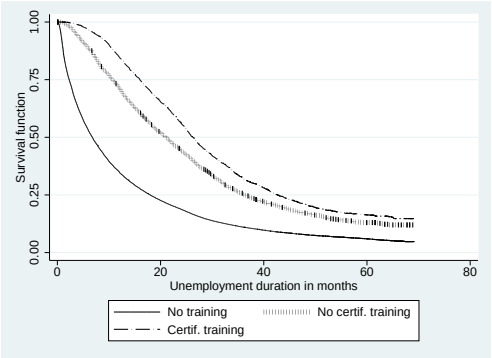
mainly funded by the administrative regions while short and job-search oriented programs are more frequently funded by the Public Employment Service. The PES funding targeting non-certifying programs could thus be substituted to the funding of programs preparing for a ministry certification. Further research should allow to identify which public mostly benefits from such programs.

Finally, those results are relevant at the European level: the French vocational training system is closer from the English one, which relies on a "market of qualifications", through which individuals acquire certifications to enhance their employability. Contrary to the German knowledge-based model, the English skills-based approach disconnects the certification acquisition from the educational path. Multiplying the ways to obtain a certification is expected to increase individuals' employability; however a quality standard regarding the training provider should be carefully elaborated to ensure that all existing possibilities are comparable and allow the same career path.

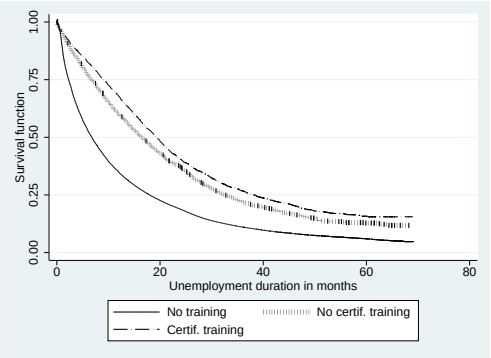
Appendix 2.A: Figures

FIGURE 2.1 – Kaplan-Meier estimates of survival functions of unemployment spell durations, by certifying training participation

(a) With training duration



(b) Without training duration

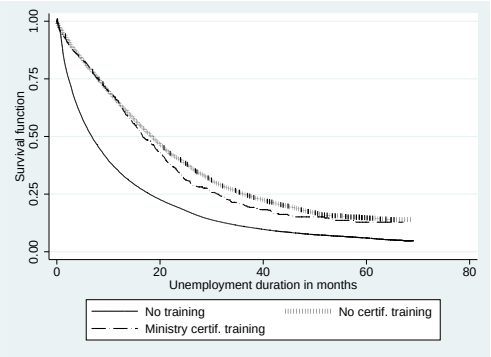


Source: FHS, 2009-2014.
Note: The sample includes 220,991 spells for a total of 132,706 individuals.

FIGURE 2.2 – Kaplan-Meier estimates of survival functions of unemployment spell durations, by ministry certifying training participation

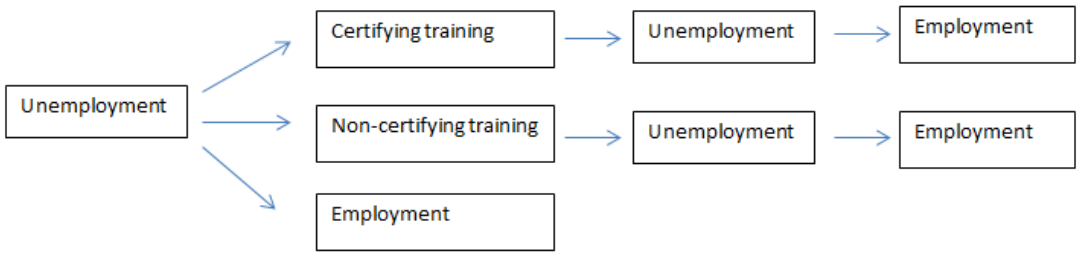
(a) With training duration

(b) Without training duration



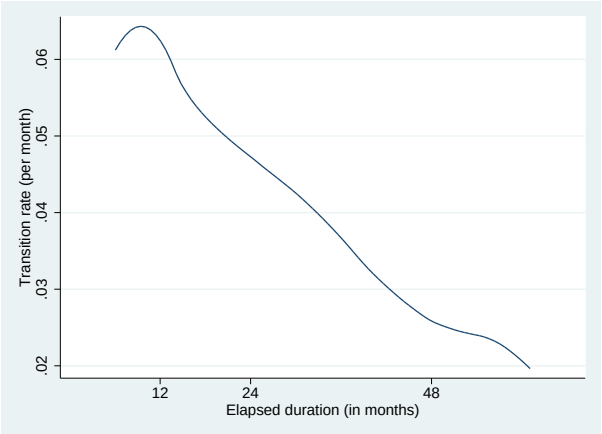
Source: FHS, 2009-2014.
Note: The sample includes 220,991 spells for a total of 132,706 individuals.

FIGURE 2.3 – Observed transitions from unemployment to training and employment



Source: FHS, 2009-2014.
Note: We do not account for training program drop out and further assume that all individuals go back to unemployment after the end of the program. We further assume that individuals find a job when they leave the PES register.

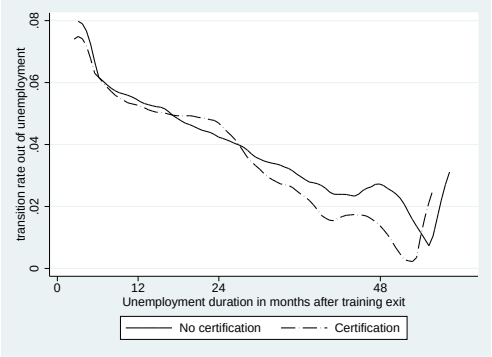
FIGURE 2.4 – Nonparametric estimate of the transition rate from unemployment to exit from unemployment



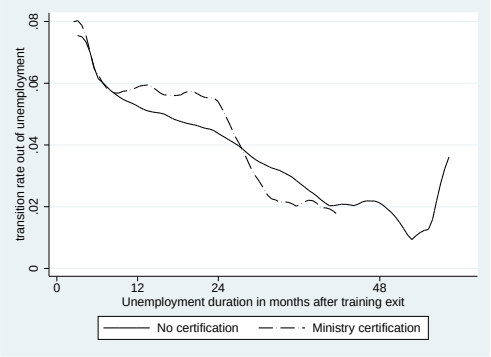
Source: FHS, 2009-2014.
Note: The sample includes 220,991 spells for a total of 132,706 individuals.

FIGURE 2.5 – Nonparametric estimate of the transition rate from unemployment to exit from unemployment, *since training exit*

(a) After a certifying program



(b) After a ministry certifying program



Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

Appendix 2.B: Tables

TABLE 2.1 – Certifications prepared during a training program by unemployed individuals

	Share (in %)
No certif.	39.41
Ministry of Education	7.14
Ministry of Labor	3.13
Ministry of Agriculture	1.08
Ministry of Youth	1.32
Collective bargaining	2.23
Others	45.69
Total	100.00

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

TABLE 2.2 – Number of unemployment spells per individual

	Share (%)	Cumulative share (%)
1	35.25	35.25
2	28.57	63.82
3	17.65	81.48
4	10.06	91.54
5	4.64	96.18
6	2.11	98.28
7	0.88	99.17
8	0.46	99.62
9	0.19	99.81
10 and more	0.19	100.00
Total	100.00	100.00

Source: FHS, 2009-2014.
Note: The sample includes 220,991 spells for a total of 132,706 individuals.

TABLE 2.3 – Number of training programs by individual during an unemployment spell

	Share (%)
1	93.79
2	5.79
3	0.41
4	0.01
Total	100.00

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

TABLE 2.4 – Training program characteristics by training field

	Duration	On-the-job	Certif.	Level IV/V	Global share
General	3.62	0.58	0.52	0.34	0.16
Manufacturing or agriculture	4.97	0.70	0.66	0.61	0.15
Business support	3.18	0.53	0.63	0.49	0.32
Tertiary domain	8.17	0.69	0.71	0.54	0.30
Unknown	2.94	0.90	0.11	0.10	0.07
Total	5.01	0.64	0.61	0.47	1
N	10,601	10,601	10,601	10,601	10,601

Source: FHS, 2009-2014.
Note: This table reports the duration in months of training programs, the share of programs including on-the-job training, preparing for a certification, and the share of programs aiming at the acquisition of a level IV or V. A level IV diploma is equivalent to the French baccalaureate, while level V diploma correspond to the French *CAP / BEP*. The sample includes 10,601 spells for a total of 10,405 individuals.

TABLE 2.5 – The most frequently followed programs, when preparing for a certification or not

	Certif.	No certif.
1	Assistant-nurse	Career guidance
2	Nurse	Job seeking support
3	Homecare assistant	Firm creation
4	Career guidance	Assistant-nurse
5	Forklift truck driving	Office automation software
	N =6,423	N =4,178

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

Table 2.6 – Training program durations (in months) by training content

	Total		On-the-job only	
	%	Duration	%	Duration
No certif.	39.4	3.6	43.6	4.2
Certif.	60.6	5.9	56.4	7.0
Ministry certif.only	12.7	7.6	13.7	8.0
Total	100.0	5.0	100.0	5.8

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

Table 2.7 – Probability to access to training by individual characteristics (OLS regression)

	Training	Certif.training	Ministry certif. training
Female	0.002*	0.021**	0.014**
Age 25-35	0.011***	0.019	0.001
Age 35-45	0.020***	-0.002	-0.019**
Age 45-55	0.014***	-0.017	-0.033***
Initial level : III	0.017***	-0.027	-0.029**
Initial level : IV	0.021***	0.008	-0.050***
Initial level : V/Vbis	0.010***	-0.042***	-0.069***
Initial level : VI	-0.005***	-0.073***	-0.087***
Unemployment recurrence	-0.029***	-0.019*	0.008
Foreigner	-0.016***	-0.019	-0.009
Const.	0.047***	0.626***	0.176***
N	220,991	10,601	10,601

Source: FHS, 2009-2014.
Note: Standard errors are clustered at the individual level. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 2.8 – Effect of training program participation on the hazard rate out from unemployment

	Without unobs.	*With unobs.*
Training	0.271*** (0.010)	0.275*** (0.015)
Log likelihood	-671,752.9	-666,031.2
Number of parameters	29	34
AIC Criteria	671,810.9	666,099.2

Source: FHS, 2009-2014.
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. We control for the initial level of education, age, gender and whether the individual already experienced an unemployment episode. We also control for the year at which the individual enters unemployment, and for the quarterly unemployment rate in the department when he enters unemployment. The sample includes 220,991 spells for a total of 132,706 individuals.

TABLE 2.9 – Effect of certifying and non-certifying training program participation on the hazard rate out from unemployment

	Certifying		Ministry certifying	
	Without unobs.	*With unobs.*	*Without unobs.*	*With unobs.*
Certif	0.239*** (0.013)	0.282*** (0.052)	0.309*** (0.027)	0.355*** (0.040)
No certif	0.315*** (0.014)	0.329** (0.153)	0.202*** (0.011)	0.188*** (0.016)
Log likelihood	-678,529.6	-672,799.7	-689,792.5	-683,604
Number of parameters	44	54	44	54
AIC Criteria	678,617.6	672,907.7	689,880.5	683,712

Source: FHS, 2009-2014.
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. We control for the initial level of education, age, gender and whether the individual already experienced an unemployment episode. We also control for the year at which the individual enters unemployment, and for the quarterly unemployment rate in the department when he enters unemployment. The sample includes 220,991 spells for a total of 132,706 individuals.

TABLE 2.10 – Unobserved heterogeneity distribution

	Certif.		Ministry certif.	
p_{111}	2.773***	(0.383)	0.178	(4.015)
p_{121}	-15.836***	(0.618)	-0.089	(0.533)
p_{112}	4.001***	(0.262)	1.031	(2.790)
p_{122}	1.502	(1.346)	-1.379	(4.987)
p_{211}	-14.738***	(0.244)	1.287	(2.633)
p_{221}	-6.728***	(0.229)	-8.562	(9328.503)
p_{212}	4.290***	(0.378)	-0.120	(4.675)
ν^1_{UE}	-1.014***	(0.098)	-1.003***	(0.019)
ν^2_{UE}	-2.405***	(0.110)	-2.365***	(0.020)
ν^3_{UTC}	-5.472***	(1.234)	-8.225	(13.423)
ν^4_{UTC}	-3.420***	(0.102)	-4.764**	(1.870)
ν^5_{UTNC}	-4.177***	(0.642)	-6.831***	(1.314)
ν^6_{UTNC}	-5.787***	(1.289)	-4.349***	(0.217)
$corr(V_{UE}, V_{UTC})$	0.123		0.055	
$corr(V_{UE}, V_{UTNC})$	0.344		0.250	
$corr(V_{UTC}, V_{UTNC})$	-0.068		0.116	

Source: FHS, 2009-2014.
Note: p_{jkl} is the probability of the joint distribution of the unobserved heterogeneity terms ν^j_{UE}, ν^k_{UTC} and ν^l_{UTNC}. ν_{UE}, ν_{UTC} and ν_{UTNC} correspond to the unobserved heterogeneity term associated to employability, participation to certifying and non-certifying training, respectively. Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 2.11 – Baseline estimation, detailed results

	Unemp.-Empl.		Unemp-Certif.training		Unemp-Non-certif.training	
Certifying training	0.282***	(0.052)				
Non-certifying training	0.329**	(0.153)				
Age : 30-44	-0.547***	(0.007)	-0.119	(0.299)	-0.168	(0.158)
Age : 45-55	-0.870***	(0.010)	-0.443	(0.536)	-0.337	(0.402)
Woman	-0.035***	(0.007)	0.069	(0.078)	-0.041	(0.133)
No diploma	0.145***	(0.022)	-0.472*	(0.266)	-0.262	(0.991)
Bac.CAP.BEP	-0.047***	(0.017)	0.090	(0.290)	0.090	(0.365)
Unemp.recurr.	-0.077***	(0.014)	-0.624***	(0.025)	-0.540***	(0.106)

Source: FHS, 2009-2014.
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. We control for the year at which the individual enters unemployment, and for the quarterly unemployment rate in the department when he enters unemployment. The sample includes 220,991 spells for a total of 132,706 individuals.

TABLE 2.12 – Baseline estimation results, adopting a restricted definition of certifying programs

	Unemp.-Empl.		Unemp-Certif.training		Unemp-Non-certif.training	
Certifying training	0.355***	(0.040)				
Non-certifying training	0.188***	(0.016)				
Age : 30-44	-0.530***	(0.006)	-0.274***	(0.063)	-0.112***	0.026
Age : 45-55	-0.840***	(0.009)	-0.710***	(0.096)	-0.338***	(0.034)
Woman	-0.037***	(0.006)	0.162***	(0.057)	0.022	(0.023)
No diploma	0.132***	(0.009)	-0.927***	(0.111)	-0.313***	(0.040)
Bac.CAP.BEP	-0.047***	(0.007)	-0.256***	(0.062)	0.136***	(0.027)
Unemp.recurr.	-0.070***	(0.006)	-0.565***	(0.057)	-0.675***	(0.023)
N	132,706					

Source: FHS, 2009-2014.
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. We control for the year at which the individual enters unemployment, and for the quarterly unemployment rate in the department when he enters unemployment. The sample includes 220,991 spells for a total of 132,706 individuals.

TABLE 2.13 – Certifying training effect, by gender

		Certifying		Ministry certifying	
Female	Certif. Training	0.310***	(0.027)	0.268***	(0.054)
	Non-certif. Training	0.260***	(0.030)	0.244***	(0.022)
Male	Certif. Training	0.156***	(0.031)	0.438***	(0.062)
	Non-certif. Training	0.395***	(0.033)	0.130***	(0.025)

Source: FHS, 2009-2014.
Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. We control for the initial level of education, age, gender and whether the individual already experienced an unemployment episode. We also control for the year at which the individual enters unemployment, and for the quarterly unemployment rate in the department when he enters unemployment. The sample includes 220,991 spells for a total of 66,874 women and 65,832 men.

TABLE 2.14 – Robustness checks

	(1) Benchmark		(2) No subsidized contract		(3) Control field		(4) First two years		(5) Lock-in effect	
Certif. Training	0.282***	(0.052)	0.321***	(0.019)	0.235***	(0.020)	0.257***	(0.017)	-0.413***	(0.018)
Non-certif. Training	0.329**	(0.153)	0.387***	(0.021)	0.339***	(0.019)	0.444***	(0.017)	-0.117***	(0.018)

Source: FHS, 2009-2014.

Note: Standard errors are in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. We control for the initial level of education, age, gender and whether the individual already experienced an unemployment episode. We also control for the year at which the individual enters unemployment, and for the quarterly unemployment rate in the department when he enters unemployment. The sample includes 220,991 spells for a total of 132,706 individuals. Columns (1) to (3) present estimated results accounting for unobserved heterogeneity, while columns (4) and (5) present estimated results when only accounting for observed characteristics.

TABLE 2.15 – Certifying program durations (in months), by training field

	Pre-training	Training	Post-training	Total
General	8.7	4.2	10.8	23.8
Manuf./agric.	9.2	5.4	8.4	23.1
Business supp.	9.5	3.5	9.2	22.2
Tertiary	8.3	8.8	6.1	23.2
Unknown	8.4	11.9	6.3	26.7
Total	8.9	5.9	8.2	23.0

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

TABLE 2.16 – Non-certifying program durations (in months), by training field

	Pre-training	Training	Post-training	Total
General	9.3	2.9	10.5	22.7
Manuf./agric.	9.2	4.1	9.3	22.6
Business supp.	8.8	2.6	9.9	21.2
Tertiary	7.6	6.6	8.3	22.5
Unknown	5.4	1.8	4.9	12.2
Total	8.2	3.6	8.8	20.6

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

Appendix 2.C: Dropout analysis

Defining dropout

We say that an individual dropped from his training program when the program theoretically ends later than the observed date of training exit, and when the difference between both dates is larger than 15 days. When the difference is smaller than 15 days we assume that it is a mismeasurement error and consider that the individual did not drop from the program.

Dropout can be followed by two situations: the individual returns back to unemployment (3.6% of the sample of trainees, see table 2.17) or he directly finds a job (5.1%). We assume that the individual finds a job right after training when the training program theoretically ends after the observed end of the unemployment spell. On the contrary, we suppose that he went back to unemployment if he is still observed unemployed right after the end of the training program.

Descriptive statistics

We look at individual characteristics of trainees dropping from their training program, as well as training programs characteristics. Individual drop more often from a certifying program, which can be explained by their longer than average duration. On the other hand, if more employable job seekers enroll into this type of programs they are also more likely to find a job before its completion.

Individuals who leave training were supposed to follow longer training programs than average (table 2.18). It is especially the case when they exit from the program because they found a job: the average training program lasts 5 months against 16.4 months in their case, theoretically. On average, a job seeker leaves the program at half of the program. Table 2.19 shows that trainees who find a job while being trained are younger and more educated than average, which corresponds to the most employable individuals in the population. It is also possible that the lock-in effect is less relevant in the case of more autonomous individuals who keep on looking for a job during the training program. Finally, the training program content also has an incidence on the drop out probability: there are 1.5% more individuals who leave a certifying program for a job than in non-certifying programs (see table 2.20). One possible explanation is that individuals enrolling in those training programs also present unobserved characteristics which increase their transition rate out of unemployment. In the case of certifying training programs, another explanation is also that even if the individual does not obtain the certification, following a certifying training program is a sufficient positive signal for the employer.

TABLE 2.17 – Share of individuals dropping from their training program

	Share (%)
To unemployment	3.55
To exit from unemp.	5.15

Source: FHS, 2009-2014.
Note: The sample includes 10,601 spells for a total of 10,405 individuals.

TABLE 2.18 – Observed and theoretical training programs durations (in months)

	Observed	Theoretical
Droppers for unemployment	4.1	9.5
Droppers for employment	7.9	16.4
Non-droppers	5.0	5.0

Source: FHS, 2009-2014.
Note: This table reports the theoretical and observed training programs durations for individuals leaving the program and coming back to unemployment, leaving the program and exiting from unemployment, and for individuals who complete the program. The sample includes 10,601 spells for a total of 10,405 individuals.

Table 2.19 – Determinants of the probability to drop from a training program in order to enter employment or to reenter unemployment

	To empl.	To unempl.
Female	0.014***	0.001
	(0.00)	(0.00)
Age 25-35	-0.027***	-0.005
	(0.01)	(0.01)
Age 35-45	-0.035***	-0.007
	(0.01)	(0.01)
Age 45-55	-0.037***	-0.008
	(0.01)	(0.01)
No diploma	-0.016**	-0.002
	(0.01)	(0.01)
BAC,BEP,CAP	-0.012***	0.004
	(0.00)	(0.01)
Unemployment recurrence	-0.006*	0.007*
	(0.00)	(0.00)
Foreigner	-0.000	-0.004
	(0.01)	(0.01)
Const.	0.063***	0.051***
	(0.01)	(0.01)
N	10,601	10,601

Source: FHS, 2009-2014.
Note: Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively. The sample includes 10,601 spells for a total of 10,405 individuals.

Table 2.20 – Dropout rate by type of training program, mean test

	Certifying	Only Ministry certif.
Droppers for unemployment	-0.006	-0.014**
Droppers for employment	-0.015***	-0.016***
N	10,601	10,601

Source: FHS, 2009-2014.
Note: The difference corresponds to the difference between the outcome mean of the trainees in non-certifying programs and the outcome mean of the trainees in certifying programs. As an example, there are 1.5% more individuals who leave a certifying program for a job than in non-certifying programs. $*$, $**$ and $***$ denote significance of the difference at the 10, 5, and 1% level, respectively. The sample includes 10,601 spells for a total of 10,405 individuals.

Appendix 2.D: Log likelihood contribution

We specify three survival functions. First, when the individual is unemployed he faces three "risks", leaving unemployment, entering a certifying training program and entering a non-certifying training program. We account for the fact that until the time considered, the individual has survived these three risks. We thus have :

$$
\begin{aligned}
&S_{UE} = exp[-exp(x'\beta_0 + \nu_{UE}) * t^{\alpha_{UE}} - exp(x'\beta_1 + \nu_{UTC}) * t^{\alpha_{UTC}} \\
&-exp(x'\beta_2 + \nu_{UTNC}) * t^{\alpha_{UTNC}}] \\
&S_{UTC} = exp[-exp(x'\beta_0 + \nu_{UE}) * t_{p1}^{\alpha_{UE}} - exp(x'\beta_1 + \nu_{UTC}) * t_{p1}^{\alpha_{UTC}} \\
&-exp(x'\beta_2 + \nu_{UTNC}) * t_{p1}^{\alpha_{UTNC}}] \\
&S_{UTNC} = exp[-exp(x'\beta_0 + \nu_{UE}) * t_{p2}^{\alpha_{UE}} - exp(x'\beta_1 + \nu_{UTC}) * t_{p2}^{\alpha_{UTC}} \\
&-exp(x'\beta_2 + \nu_{UTNC}) * t_{p2}^{\alpha_{UTNC}}]
\end{aligned}
$$

S_{UE} is the probability of having survived until time t before exiting unemployment. S_{UTC} is the probability to have survived the risk of entering a certifying program until time t_{p1}, and S_{UTC} corresponds to the same probability for the risk of entering a non-certifying program until time t_{p2}.

If an individual follows a training program, we account for this effect when he is looking for a job again. We specify a different survival function for each program participation, such that:

$$
\begin{aligned}
&S_{UTCE} = exp[-exp(x'\beta_0 + \beta_{01}.\mathbf{1}(TC = 1)\mathbf{1}(t > t_{e1}) + \nu_{UE}) * (t^{\alpha_{UE}} - t_{e1}^{\alpha_{UE}})] \\
&S_{UTNCE} = exp[-exp(x'\beta_0 + \beta_{02}.\mathbf{1}(TNC = 1)\mathbf{1}(t > t_{e2}) + \nu_{UE}) * (t^{\alpha_{UE}} - t_{e2}^{\alpha_{UE}})]
\end{aligned}
$$

S_{UTCE} and S_{UTNCE} differs from S_{UE} as once an individual has followed a training program, we make the assumption that he does not follow another program afterwards, thus he does not face a competing but single risk once exiting from a training program. Moreover, as we are looking at the ex-post effect of both types of training programs, we stop the clock during training.

The likelihood function for one individual spell is :

$$
L = (h_{UE})^{\delta=1} * (h_{UTC})^{TC=1} * (h_{UTNC})^{TNC=1} * (S_{UE})^{training=0} * (S_{UT} * S_{UTE})^{training=1}
$$

with $\delta = 1$ if the unemployment spell is not censored, i.e if we observe the individual exiting from unemployment. $TC = 1$ and $TNC = 1$ if the individual follows a certifying or a non-certifying program, respectively.

We set L_{jkl} as the likelihood function for one type jkl, as a function of the unobserved heterogeneity associated to transition to employment, certifying and non-certifying training programs. Accounting for unobserved heterogeneity, an individual contribution to the likelihood is thus :

$$L_{jkl} = (\prod_{sp}^{Sp} L_{jkl}) * \pi_{jkl}$$

Where Sp is the total number of spells for each individual, and π_{jkl} is the normalized probability to be characterized by a specific combination of unobserved characteristics V_{UE}, V_{UTC} and V_{UTNC} .

3 Does Employment Protection Legislation Affect Training Investments? Evidence from the United Kingdom

This chapter is based on joint work with Emma Duchini

Abstract

In this paper we study how employment protection legislation affects firms and workers' decisions to invest in human capital. To do so, we exploit a reform implemented in the United Kingdom that extends the probationary period duration from 12 to 24 months, and thus reduces expected firing costs for workers with more than one year of tenure. To study the impact of this intervention, we use a difference-in-difference strategy exploiting the variation in its implementation over time and across workers' tenure. Our findings show that, by enhancing both job creation and job destruction, this reform decreases training investment for workers with more than one year of tenure by 8 to 14 percent, depending on the tenure group considered. These results are further validated via the estimation of a competing-risks model where separating from a firm and being trained are treated as competing risks. In light of those results, introducing training subsidies targeted at workers who are going through their probationary period seems relevant to compensate for side effects of the decrease of the expected employment relationship duration.

JEL codes: J24, J58.

Keywords: employment protection legislation, dismissals, training.

3.1 Introduction

In the issue of January 12 2017, the Economist published a special report titled "Lifelong learning is becoming an imperative", highlighting that for an increasing number of workers it is becoming a necessity to acquire new skills, as established ones become rapidly obsolete, due to the combined effects of globalization and job automation (David et Dorn 2013; Autor, Dorn et al. 2016). Yet, what is still unclear is how workers should acquire new skills on-the-job, who should pay for their provision, and who should capture the returns of such an investment.

In this paper, we consider the role of labor market institutions, and in particular, we ask ourselves whether employment protection legislation (EPL hereafter) can affect firms' and workers' decisions to invest in human capital. More precisely, we investigate whether loosening EPL hinders or favors investment in training.

By now the literature on the economic impact of EPL agrees that higher firing costs discourage both job creation and destruction (Blanchard et Portugal 2001; A. D. Kugler, Jimeno-Serrano et al. 2003; A. D. Kugler et Saint-Paul 2004; Boeri et Jimeno 2005; Autor, Kerr et al. 2007; A. Kugler et Pica 2008; Schivardi et Torrini 2008; Marinescu 2009; Von Below et Thoursie 2010). Yet, there is still no such consensus on the effect that EPL can have on the overall productivity of the economy. On the one hand, stricter EPL can reduce labor productivity, in so far as it reduces the ability of firms and workers to fully respond to market shocks, to reallocate from declining to growing sectors and to adopt new technologies (Hopenhayn et Rogerson 1993), or because it decreases insiders' effort (Ichino et Riphahn 2005; Martins 2009). On the other hand, strict EPL can enhance labor productivity if it leads employers to improve their recruitment practices in order to raise the quality of the job match (Malcomson 1999; Marinescu 2009), or induces firms to invest more in capital (Autor, Kerr et al. 2007; Cingano et al. 2016).

Importantly, EPL can affect productivity through its impact on training investment and in this case the sign of the effect should depend on the type of training considered and on who finances it. On the one hand, stricter EPL can reduce workers' incentives to invest in general skills if it lowers their mobility prospects. On the other hand, when generating wage compression, it can induce employers to increase investment in general skills (Acemoglu et Pischke 1996; Acemoglu et Pischke 1999). In addition, by increasing the expected duration of the employment relationship, rigid EPL can make both the employer and the employee more willing to invest in firm-specific skills (Suedekum et Ruehmann 2003; Wasmer 2006; Belot et al. 2007; Delacroix et Wasmer 2007).

Despite the importance of understanding the relationship between EPL and training, so far the literature on this topic has only provided a theoretical framework, but there exists basically no rigorous evidence on its empirical predictions. There are several reasons why this is so. First, comparing workers who are subjected to different EPL regimes might confound the effect of this legislation with that of unobservable characteristics that differ

between workers in permanent and temporary contracts. Secondly, the possibility of exploiting reforms of EPL as natural experiments to identify the effect of interest is limited by the fact that training levels have been historically very low precisely in most of the countries that have recently undertaken major EPL reforms, such as Italy or Spain. Third, to properly identify the impact of EPL reforms on training investment, one has to isolate this from the first-order effect that these interventions can have on job flows.

In this paper, we try to address all these identification problems. To do so, we look at the United Kingdom, as it offers the ideal setting to study this question. EPL in the United Kingdom is characterized by the presence of the single/unified open-ended contract. The main feature of this contract is that workers' rights at termination are equalized across temporary and permanent workers, and rise with seniority, after a probationary period during which workers are not entitled either to the right to sue their employer for unfair dismissal, or to severance payments in case of dismissal. In addition, the United Kingdom is one of the few countries, at least in Europe, where workers receive high doses of on-the-job training, as shown in figure 3.1. And crucially, in April 2012 the British parliament approved a reform of EPL that extended the length of the probationary period from 12 to 24 months of tenure for workers hired after this date [1].

In a first step, to estimate the impact of this reform on training investment, we use single hazard models and a difference-in-difference strategy that compares the evolution of the firing and training hazard of workers affected by the reform, with that of employees who are not concerned by this intervention. Our results indicate that a decrease of expected firing costs lowers the hazard of participating in training both for workers with 1 to 2 years of tenure and for those with more than 2 years of seniority, in comparison to workers who had been hired prior to the introduction of this reform. In particular, the training hazard decreases by up to 8 percent for workers with 1 to 2 years of tenure and by up to 14 percent for those with 2 to 4 years of tenure.

In a second step, to take into account that separating from a firm and being trained are competing risks, we compare our main results with those obtained using a bivariate competing-risks model. The results of this model, which we detail below, are practically indistinguishable from our main findings.

In detail, our difference-in-difference strategy works as follows. We distinguish three groups that are clearly affected by this intervention. The first one comprises workers hired from April 2012 onwards and interviewed during their first year in the firm, that is employees with 0 to 11 months of tenure. We will call this the treatment group 1. On the one hand, this reform may decrease their dismissal hazard if firms postpone their firing

1. This reform follows a previous one implemented in 1999, when the British government shortened the probationary period from 24 to 12 months of tenure. Marinescu (2009) finds that the 1999 reform significantly decreased the hazard of termination for workers with 12 to 23 months of tenure. She also provides evidence that the firing hazard decreased for employees with less than 12 months of tenure, which is consistent with firms having increased the quality of new recruits after the policy change. Importantly, the author does not focus on the impact of the 1999 intervention on training investment.

decision to the second year of tenure. On the other hand, by postponing the tenure threshold to qualify for rights at termination, the reform might raise their propensity to quit their job. The resulting impact on training investment targeted at this group of workers is therefore ambiguous. The second treatment group includes workers hired from April 2012 onwards and interviewed during their second year in the firm, that is employees with 12 to 23 months of tenure. This will be the treatment group 2. While it is unclear whether this reform hinders or enhances their hazard of quitting, it could clearly increase their firing hazard. If, as a result, the expected duration of their employment relationship decreases, we should observe that both firms and workers will have less incentives to invest in firm-specific skills. And while firms would have lower incentives to provide general training, workers should be more willing to invest on general human capital. Finally, the third treatment group is constituted by workers hired from April 2012 onwards once they survive to the new probationary period, i.e. workers who have at least 24 months of tenure in a firm. Notice that we have data until December 2015. This implies that workers who are hired under the new EPL can have at most reached 44 months of tenure in the estimation sample. Firing costs will decrease for this group of workers as well, given that, as a result of the reform, severance payments start to be calculated from the second year in the firm, rather than the first one. Moreover, if the reform were to enhance job creation, together with job destruction, these workers might also become more inclined to leave the firm. This should be the more so, if quitting a job right after the end of the probationary period sends the market a strong signal on a worker's quality. As a consequence, even for this group of workers we should expect that both firms and workers will have less incentives to invest in firm-specific skills. And while firms would have lower incentives to provide general training, workers should be more willing to invest on general human capital.

Next, notice that in our sample we can observe workers hired before April 2012 when they have between 0-11, 12-23, and 24-44 months of tenure. In addition, we can observe workers who have at least 45 months of tenure, and interviewed both before and after April 2012. All these workers are hired before April 2012. The 2012 reform does not change firing costs for any of them. Hence, we choose to use workers with at least 45 months of tenure as our control group in the difference-in-difference strategy.

Given that we are mainly interested in the impact of the reform on training, we estimate a bivariate competing-risks model for the hazard of training and the hazard of separation. This model allows us to take into account that the sample of workers who gets trained is a selected sample of those who survive to the risk of separating from the firm, and enables us to measure to what extent this selection bias contaminates our main results. As stated above, both the estimation of this model, as well as an extensive battery of robustness checks validate our main findings [2].

Importantly, in the Labor Force Survey we cannot distinguish between training provi-

2. Notice that the fact that estimation results from the single and competing-risks models are comparable suggests that the probability of participating in training does not differ between workers who leave the firm and those who do not.

ding firm-specific versus general skills. Yet, the sign of our results suggests that, although this reform might have induced workers to invest more in acquiring general skills, it has a more important and negative impact on both firms' and workers' willingness to invest in firm-specific skills, and on firms' incentive to provide general training.

Although we can only provide suggestive evidence regarding the mechanisms that drive these results, our analysis indicates that the drop in training investment is most likely the result of an increase in both job creation and destruction. On the one hand, when we estimate the impact of the reform on the hazard of dismissal, point estimates, though not significant, suggest that this intervention increases the risk of dismissal for workers who have more than 1 year of tenure in the firm. On the other hand, we descriptively show that job creation seems to increase following the introduction of this reform. And we bring suggestive evidence that this, in turn, could stimulate the propensity to change job of workers who have just finished their probationary period [3].

Our findings have important policy-implications. To overcome the labor market duality between workers in permanent contracts and those in temporary ones, many countries are considering the opportunity to introduce the unified contract, as the one in force in the UK. An increasing number of papers discusses the pros and cons of this form of EPL (Bentolila et al. 2012; Cahuc 2012; OECD 2014b). Our analysis adds to this literature by showing that the design of its flexible element, the probationary period, can have important consequences on training investment. To take this into account - under the hypothesis that workers are credit-constrained - governments might want to consider, for instance, the opportunity of introducing training subsidies targeted at workers who are going through their probationary period.

The paper proceeds as follows. Section 3.2 describes the institutional setting. Section 3.3 discusses the conceptual framework and introduces the theoretical predictions that can be drawn from it. Section 3.4 illustrates the data used in the empirical analysis. Section 3.5 explains the identification strategy and provides the main results. Section 3.6 describes the robustness checks. Section 3.7 discusses alternative mechanisms that could explain the main results. Section 3.8 concludes.

3. Notice that the reform may in principle have an ambiguous impact on the quitting propensity of workers who have just finished their probationary period. On the one hand, the lengthening of the probationary period should discourage those who have just acquired some rights at termination to look for another job in which they will have to wait for two years to gain some job protection again. On the other hand, quitting a job after having survived to the probationary period might send to the market a strong positive signal on the worker's quality. As shown below, our analysis suggests that this last mechanism is prevailing on the first one.

3.2 Institutional setting

3.2.1 Employment protection legislation in the UK

The United Kingdom is a particularly interesting case to study, as it proposes an EPL model that aims at eliminating the segmentation between insiders and outsiders, while maintaining the possibility for the employer to hire workers on contracts with different lengths. In particular, workers' rights at termination are equalized across contracts, and rise with seniority. The so called "unified contract" prescribes also a probationary period, whose length has been repeatedly modified, during which workers have neither the right to claim unfair dismissal nor are entitled to receive severance payments in case of dismissal. Only after this probationary period, workers obtain the right to sue their employer for unfair dismissal, and get entitled to severance payments that smoothly increase with years of seniority [4]. The rational behind a probationary period is, on the one hand, to allow firms to have a certain period of time to evaluate the quality of the new hire, without incurring in any cost in case the employment relationship results to be a bad match. On the other hand, the unified contract allows all workers to enjoy an increasing level of job protection after a relatively short period of time.

In April 2012, the British government approves a reform that extends the length of the probationary period from 12 to 24 months for all employees hired from April onward. As a consequence, workers hired after April 2012 loose the right to sue their employer for unfair dismissal and the entitlement to severance payments when reaching 12 months of tenure, and they can only get to acquire these rights after 24 months with the same firm.

De facto, this intervention restores the legislation in place before 1999, when the Labor Government shortened the length of the probationary period from 2 to 1 year [5]. Marinescu (2009) finds that the 1999 intervention significantly decreased the hazard of termination for workers with 12 to 23 months of tenure. She also provides evidence that the firing hazard decreased for employees with less than 12 months of tenure, which is consistent with firms having increased the quality of new recruits after the policy change.

4. In details, after the first two years of tenure, severance payments grow with seniority and age. Workers younger than 22 years old receive half a week per year of service. Those aged between 22 to 40 get one week per year of service, and those older than 41 get 1.5 weeks per year. A limit upward is set to 30 weeks and to an amount of 464 pounds per week.

5. These repeated modifications of the duration of the probationary period underline the implicit tension between firms' and workers' interests in the framework of a flexible labor market such as the UK one. As mentioned by Marinescu (2009), the 1999 reform was part of a package enforced by the new Labour government, which aimed at promoting new labor practices. The main justification for reducing the probationary period was to increase the adhesion of workers to a more flexible organization of the labor market. On the other hand, the 2012 extension of the probationary period was implemented following a proposition of the report "Resolving workplace disputes: public consultation", which stated that this modification would allow to "provide more time for employers and employees to resolve difficulties, give employers greater confidence in taking on people and ease the burden on the employment tribunal process".

Importantly, the author does not focus on the impact of the 1999 intervention on training investment.

3.2.2 On-the-job training in the UK

Before discussing how the 2012 reform might have modified firms' and workers' incentives to invest in training, it is important to get more insights on the nature, quantity, and financing sources of training investment in the UK.

For about 20 years since the mid 1960s, the UK had in place a system of Industry Training Boards (ITBs) that had the legal power to operate levy-grant systems to finance sectoral training investment (Industrial Training Act, 1964). Continuous criticisms regarding the administrative costs of these systems, coupled with unclear requirements regarding firms' exemptions and government inaction, undermined the scope of these organisms over time. This situation culminated with the abolition of ITBs in 1988 by the Conservative government in power at the time. Since then, only the Construction and Engineering Construction sectors maintained statutory levy-grants systems in place. In addition, at the end of the 1990s, the film industry also established its own "Skills Investment Fund", although on a voluntary basis (Casey et Gospel 2012).

Although there is no nationwide statutory system in place for employer-financed on-the-job training, the most recent available figures released by the Department for Innovation, Business and Skills, reported in figure 3.2, indicate that in 2013 employers were by large the most important source of funding for adults' training (Gloster et Marvell 2016). Importantly, this publication reports that both employers' training expenditures, as well as those sustained by public and non-profitable entities, decreased between 2010/11 and 2013/14. In particular, employers' contributions fell by 7 percent during this period, especially in the Public Administration and Defense sectors. By contrast, other training spend rose in sectors such as Business Services and Wholesale and Retail.

Next, the Employer Skills Survey, run by the UK Commission for Employment and Skills' (UKCES), provides information regarding the nature and quantity of employer-provided training [6]. Here we report some figures taken from the 2011 wave of the survey that precedes the implementation of the 2012 reform of the probationary period. As shown in table 3.1, in all sectors firms are more likely to offer job-specific, security and induction training. In addition, table 3.2 shows that most firms offer a training spell that lasts at most one week. Larger firms are slightly more likely to sponsor training than smaller ones, as reported in table 3.3. In what follows we will provide additional figures drawn from the UK Labor Force Survey, the data set used for the regression analysis.

6. The survey, which is run every two years since 2011, has two waves, with a main stage questionnaire of around 91,000 establishments, and a follow-up survey of investment in training of roughly 13,000 establishments that have conducted training over the previous 12 months.

3.3 Conceptual framework and theoretical predictions

Theoretically, EPL may affect training investment decisions through the impact it can have on labor market frictions and, consequently, on the expected duration of employment relationships. In addition, it is worth bearing in mind that EPL might have different implications on investment in firm-specific versus general skills. Although empirically it might be difficult to distinguish between these two types of skills, with firm-specific human capital we refer to those skills that increase productivity only in the firm in which they are acquired. Both Wasmer (2006) and Delacroix et Wasmer (2007) show that the value of the investment in firm-specific human capital should decrease, both to the employer and to the employee, as the expected duration of the employment relationship decreases. General human capital includes instead all skills that are identically useful to many firms, including the training company. The relationship between EPL and investment in this type of skills is a priori more ambiguous. On the one hand, the seminal papers by Acemoglu and Pischke (Acemoglu et Pischke 1996; Acemoglu et Pischke 1999) prove that any factor that augments frictions in the labor market and generates wage compression can push firms to finance general training [7]. A reduction of labor market frictions might then weaken firms' incentives to provide general training. On the other hand, we might expect that, in a more flexible labor market where job mobility is enhanced, workers might be more willing to invest in general training, in order to be prepared for new job opportunities. The overall impact of a reduction of firing costs on general training is therefore unclear.

In this paper we want to test these theoretical predictions exploiting the 2012 reform that extended the length of the probationary period in the UK. Importantly, any impact that this reform might have on training investment should depend on its first-order effects on job flows. Let us first discuss these in details.

3.3.1 Effects on job flows

First, this reform decreases firing costs for all workers hired after April 2012 and staying at least 12 months in the same firm. In particular, firing costs decrease sharply for workers with 12 to 24 months of tenure, as these employees loose both their right to sue their employer for unfair dismissal and the right to get severance payments if dismissed [8]. Yet, firing costs also decrease for workers with more than 24 months of tenure, as severance payments are not calculated from the first year anymore, but only starting from the second year in the firm. As a consequence, concerning dismissals, we might observe: a drop in dismissals for workers with less than 12 months of tenure as employers might postpone to the second year their decision regarding who they want to fire; an increase in dismissals

7. Wage compression denotes a situation in which training boosts productivity more strongly than pay, creating a wedge between the two that increases with the level of skill.

8. On April 6 2012, the Guardian reports that, according to the government estimates, defending a claim for unfair dismissal costs a business an average of £4,000. Moreover, it says that in 2010-11, just under one quarter of cases taken to tribunal involved a claim for unfair dismissal.

for workers with 12 to 23 months of tenure. In particular we might observe a spike in dismissals for workers approaching the end of the new probationary period; a slight increase in dismissals for all workers even after the probationary period.

A longer probationary period might induce firms to change their hiring practices as well. First, in the months preceding the reform, firms might postpone some hirings to the post-reform period to exploit the new EPL. Secondly and more importantly, the decrease of firing costs might lead firms to post more vacancies. As a consequence, the quality of newly hired workers might decrease, if firms choose to exploit the first two years of the employment relationship to judge their workers, rather than investing in their recruitment practices to spot the best job candidates [9]. Yet, as firing costs sharply increase after the first two years of tenure, once the probationary period ends, worker's quality in the post-reform period should converge to that of the pre-reform era.

Next, the reform might have an impact on quits and this might vary depending on the tenure group considered. Overall, we might assume that if firms start posting more vacancies, all workers might be induced to change job more often. Yet, let us analyze workers' incentives specifically by tenure category. First, workers hired from April 2012 onwards might be especially stimulated to change job more often while still in their first year, given that the reform has postponed the tenure threshold to qualify for rights at termination [10]. Secondly, the impact of the reform on the quitting hazard of workers with 12 to 24 months of tenure is ambiguous. On the one hand, if job creation increases, they might also be induced to search for other jobs more often. On the other hand, as the end of the probationary period approaches, this incentive should weaken. In addition, the market might interpret a quit that takes place right before the end of the probationary period as a negative signal on worker's chances to survive to any probationary period. Third, the reform has also an ambiguous impact on the quitting propensity of workers who have just finished their probationary period. On the one hand, the lengthening of the probationary period should discourage those who have just acquired some rights at termination to look for another job in which they will have to wait for two years to gain some job protection again. On the other hand, the increased job creation might also push these workers to become more mobile. Importantly, notice that, in line with what said above, a worker who already wants to quit his/her job during the probationary period might prefer to wait for this to end, and then quit. Quitting a job after having survived to the probationary period might indeed send to the market a better signal on the worker's quality, than doing it

9. As reported by the Guardian, when the reform was implemented in April 2012, "groups representing employers welcomed the change, claiming it will give smaller companies, in particular, more confidence to take on staff. Neil Bentley, deputy director general of the Confederation of British Industry, or CBI, said that the reform will be a particular boon for young people and those who have been out of work, as it will give them longer to demonstrate their value to employers. John Longworth, director general of the British Chambers of Commerce, said that dismissal rules are a major barrier to growth for many businesses. The majority of small businesses have ambitions to grow, and this will boost their confidence to hire."

10. Gielen et Tatsiramos (2012) show that in a flexible labor market, workers are more likely to quit if their job satisfaction is low compared to a situation with a high level of job protection.

before.

Finally, this reform can also affect the wage-setting process. On the one hand, firms might increase entry wages to compensate workers for the prolonged risk of being dismissed. On the other hand, as we will see in a moment, conditional on not being credit-constrained, after April 2012 some workers might be more willing to accept lower wages in order to finance investment in general training. Moreover, wages of workers hired after April 2012 might grow more slowly with tenure if their bargaining power decreases as a result of this reform.

3.3.2 Effects on training investment

In light of all these considerations, this reform might affect training investment as follows. Investment in firm-specific skills should decrease for all workers whose expected duration of the employment relationship diminishes as a result of the new EPL. First, this should concern workers with 12 to 23 months of tenure as both dismissals and quits can increase for them, following the 2012 reform [11]. Secondly, investment in firm-specific skills might also decrease for workers hired from April 2012 onwards and "surviving" to the probationary period - that is workers reaching 24 months of tenure in April 2014 - as their firing hazard and propensity to leave can also increase with respect to the pre-reform period.

However, it is unclear what could happen to workers hired after the reform, before they reach the end of the first year in the firm. The ambiguity comes from the fact that, on the one hand, their firing hazard could decrease, while, on the other hand, their job mobility might increase. Finally, as firing costs do not change for workers hired before the implementation of the reform, investment in firm-specific skills targeted at these workers should not be affected by this intervention, unless their job mobility is also strongly enhanced by the reform. Yet, the lengthening of the probationary period should discourage them from changing job.

The impact of this reform on investment in general skills could be more homogeneous across tenure groups. On the one hand, if labor market frictions decrease, firms should be less willing to invest in providing general skills to all their employees, and this effect should be bigger the more the reform enhances workers' propensity to leave the firm. On the other hand, conditionally on not being credit-constrained, all workers might be more willing to finance investment in general human capital by themselves, and once again this effect should be larger, the more the reform enhances workers' propensity to leave the firm.

After briefly introducing the data in the next paragraph, in section 3.5 we will extensively discuss the empirical strategy we use to identify all these effects.

11. Although the impact on their propensity to quit is rather ambiguous as we have seen.

3.4 Data

3.4.1 Outcomes definition

To test all the theoretical predictions discussed above, we use the 2009-2015 waves of the five-quarter longitudinal version of the UK Labor Force Survey, LFS, that offers the possibility to follow each individual for five consecutive quarters. We only consider individuals who are employed in the first quarter they are interviewed, and further restrict the sample of study to employees working at least 16 hours, as the reform only applies to this group of workers [12]. Finally, we exclude workers on temporary contracts, as it is very rare to find some with more than one year of tenure.

To measure job separations, we proceed as follows. First, we create a binary variable equal to one and called "job separation" if respondents either declare that they have changed job between one quarter t and the next, or that they have been unemployed for at most three months. Next, we create a dummy variable that we call "job mobility" if respondents declare that they have left a paid job between period t and $t+1$, without experiencing a transition into unemployment.

Then, to distinguish dismissals from other forms of separations we make use of an additional question asking the reason why the job ended. Table 3.4 shows the distribution of the different possible answers as reported by individuals interviewed before April 2012. We then create a binary variable being equal to one if an individual experiences a job separation between t and period $t+1$ and declares that this is because he/she was dismissed or made redundant [13] [14].

To analyze wages we use the gross hourly pay. Each individual reports this twice over the 5 quarters he/she is interviewed, more precisely at the first and last quarter of interview.

Turning to training, the LFS asks respondents whether they have participated in education or training related to their job in the past three months from which we derive the binary variable "Training" if an individual participates in training between t and $t+1$.

12. We assess to which extent this restriction might bias our results in section 3.6.

13. Importantly, the 2012 reform intervenes only at regulating dismissals. Yet, following Marinescu (2009), we include redundancies in our definition of dismissals. We do this mainly for two reasons. First, when asked about this in a survey, some respondents may prefer to declare that they have been made redundant, rather than been dismissed, even though this is what has actually happened. As a result, we might fail to measure all dismissals if we do not include redundancies as well. Secondly, the variable reporting the reason of a job separation is often missing, especially in the year 2011. Yet, the LFS contains another question asking directly whether an individual has been made redundant between one quarter and another and this answer is better reported. Thus, including redundancies in our definition of dismissals allows us to measure this variable all along our estimation period.

14. In principle we would also like to distinguish quits from other forms of separations. However, to do so we would need to use the question regarding the reason of a separation, and once again this has too many missing values in 2011 to be used for the entire sample period. For this reason, in what follows, we use job mobility as a proxy for quits.

Tables 3.5 and 3.6 report summary statistics for this variable, broken down by tenure group, demographic characteristics, occupation and industry. In both these tables the sample is restricted to workers hired before April 2012. Overall, around 30 percent of workers declare that they have participated in training in the last three months, and this share does not seem to vary much with seniority. On the other hand, highly educated workers, women, full-time workers, and those working in the public sector participate in training more often than other groups. Across occupations, workers in professional and personal service occupations participate more frequently in training than individuals in other professions. Finally, across industries, those working in community, social and personal service activities have more than twice the probability of participating in training than workers in other sectors.

Unfortunately, the LFS does not ask respondents to clearly distinguish between training providing firm-specific skills and training supplying general skills [15]. Yet, notice that if we were to find that the reform has a negative impact on the variable "Training", we could still infer that workers' incentives to invest in firm-specific skills, and firms' willingness to provide any form of training are decreasing enough to prevail on workers' increased willingness to invest in general training.

3.4.2 Outcomes dynamics

Now that we have described our main variables of interest, let us have a look at their dynamics, before and after the reform. Figure 3.3 shows the evolution of the hazards of job separations, dismissals and training investment as a function of months of tenure with the same firm [16]. The dotted line refers to workers hired before April 2012, while the solid one represents workers hired from April 2012 onwards.

The top graph in figure 3.3 displays the hazard of separation as a function of seniority in the firm. As expected it is hump-shaped in both the pre- and post-reform period, with the risk of separation being higher in the very first months in a new job and then rapidly

15. To be precise, to those who claim that they have participated in some training, not only in the last three months, but specifically in the last four weeks, the LFS also asks to report what type of training the experience refers to and the respondent has three options to choose from. The first option is "on-the-job training only", defined as any form of learning through example and practice while actually performing the job. The second option is defined as "training away from the job" and refers to any training done while not actually working. This could take place both at the employer's premises, as well as in a training center, at home - through correspondence for instance - or in an educational institution. Finally, the respondent could declare that he/she has participated in both forms of training. Yet, this classification does not seem to be especially useful to distinguish between training providing firm-specific versus general skills.

16. In the LFS, years and months of tenure in a firm can be calculated for more than 99 percent of the sample. To deal with missing information on tenure we proceed as follows. Whenever the year the worker enters into the firm is missing, we drop the individual. When it is the month the worker starts his/her job that is missing, we impute it to January if the individual has been in the firm for more than 4 years. However if this information is missing and the worker has less than 4 years of seniority, we drop this individual as we want to measure low levels of tenure precisely.

decaying. However, the two lines do not seem to differ much between the two periods.

Yet, in line with the theoretical predictions described above, the hazard of dismissal seems to decrease in the post-reform period for workers in their first year in the firm, and then exhibits a spike right before the expiration of the probationary period. Importantly, notice that these dynamics do not stand out when looking at the hazard of separation. This might be due to the fact that dismissals represent only around 20 percent of all separations [17].

The evolution of the hazard of training also seems to reflect the theoretical predictions. In particular, the solid line exhibits a sharp decline in comparison to the dotted one, precisely for those workers who seem to have a higher firing hazard in the post-reform period, that is those who have around 20 months of tenure in the firm. Interestingly, the hazard of training also seems to decrease for workers with more than 2 years of tenure in the post-reform period in comparison to the pre-reform one. This would also be consistent with the theory predicting that, in the post-reform period, both the firing hazard and the propensity to leave the firm might increase for workers with more than 2 years of tenure. Yet, the fact that we do not clearly observe these dynamics when looking at the firing hazard might be due to the limited size of the group of workers with more than 2 years of tenure in the post-reform period. This might indeed cause the evolution of the solid line to be too fuzzy to be interpreted only through a graphical analysis.

Let us now turn to the regression analysis to understand whether the dynamics revealed by this graphical analysis could indeed be attributed to the effect of the reform.

3.5 Empirical analysis

3.5.1 Identification strategy

To identify the impact of the 2012 reform on dismissals and training decisions, we proceed as follows. In light of the theoretical predictions introduced in section 3.3, we can distinguish three groups that are clearly affected by this intervention. First, workers hired from April 2012 onwards and interviewed during their first year in the firm, that is employees with 0 to 11 months of tenure. We will call this the treatment group 1. Secondly, workers hired from April 2012 onwards and interviewed during their second year in the firm, that is employees with 12 to 23 months of tenure. This will be the treatment group 2. Third, workers hired from April 2012 onwards once they survive to the new probationary period, i.e. workers who have at least 24 months of tenure in a firm. Notice that we have data until December 2015. This implies that workers who are hired under the new EPL can have at most reached 44 months of tenure in the estimation sample. Next, in our sample we can observe workers hired before April 2012 when they have between 0-11, 12-23, and

17. Note also that the y-axis of the graph relative to the hazard of separation covers a larger range than the one of the hazard of dismissal.

24-44 months of tenure. In addition, we can observe workers who have at least 45 months of tenure, interviewed both before and after April 2012. Notice that these workers are all hired before April 2012, which implies that the 2012 reform does not change firing costs for any of them. Not surprisingly, figure 3.4 shows that the dynamics of dismissals and training investments for workers with 45 to 96 months of tenure are pretty similar when we compare workers interviewed before the implementation of the reform to those interviewed after April 2012. Hence, to identify the impact of this reform on all the outcomes of interest, we choose to implement a difference-in-difference strategy using workers with at least 45 months of tenure as control group.

Three factors need to be considered in order to ensure the validity of this identification strategy. First, we need to verify that before the introduction of the reform the evolution of the outcomes of interest are indeed comparable between the treatment and the control groups. In other words we need to verify that the parallel trend assumption is satisfied in this context.

Secondly, we need to consider that the sample of workers who gets trained in a firm is a selected sample of those who survive to the risk of separating from the firm. In other words, separating from a firm and being trained are competing risks. To assess to what extent this selection bias contaminates our main results, we will compare them to the estimates obtained using a bivariate competing risks model for the hazard of training and the hazard of separation [18] [19].

Third, we need to take into account that, if the reform reduces labor market frictions, it could in principle induce all workers to change job more frequently, included those hired before April 2012. This in turn might affect training investment, as we discussed above. Crucially, this would only bias down our estimates on the impact of the reform on training investment, given that the sign of the effect would be the same for both the treatment and the control group.

3.5.2 Main results

We first estimate a single-hazard model using a complementary log-log specification with delayed entry. The cloglog specification allows us to take into account that individuals are observed only at discrete intervals - t corresponds to a quarter in our sample. Estimating

18. Ideally, we would like to estimate a three competing-risks model for the hazard of training, dismissal and other types of separation. As this model is hardly estimable, we consider a bivariate competing-risks model for the hazard of separation and training, given that we are mainly interested in the impact of the reform on the latter outcome.

19. For the moment, we estimate only a competing risks model with observable characteristics. In principle we would also like to control for unobserved heterogeneity, that is the fact that unobservable characteristics may jointly influence both hazard rates. However, given that inference with left truncated data in a model with shared frailty is not trivial (Gerard J van den Berg et Drepper 2016) this stays out of the scope of the paper for the moment.

a model with delayed entry accounts for the fact that individuals are not observed since the beginning of their employment spell. As a result, the single-hazard regression for risk z, and individual i, interviewed in quarter j, looks as follows:

$$h_{ij}^{z} = 1 - exp\Big(-exp(\gamma_j^z + \delta^z X_{ij} + \alpha_1^z Tenure_{0-11} + \alpha_2^z Tenure_{12-23} + \alpha_3^z Tenure_{24-44} + \beta_1^z Tenure_{0-11} * After + \beta_2^z Tenure_{12-23} * After + \beta_3^z Tenure_{24-44} * After)\Big) \tag{3.1}$$

In this regression X includes individual characteristics such as age, education, and marital status, region of work, but also occupation and industry fixed effects, a dummy for whether the individual works in the private sector, and one accounting for whether the job is part-time. We also control for quarters of interview fixed effects γ_j^z. The dummy $Tenure_{0-11}$ is equal to one for workers having between 0 and 11 months of tenure, $Tenure_{12-23}$ takes value 1 for workers having between 12 and 23 months of seniority, and $Tenure_{24-44}$ is equal to one for workers having between 24 and 44 months of tenure. The main coefficients of interest in these regressions are those on the variables $Tenure_{0-11} * After$, $Tenure_{12-23} * After$, and $Tenure_{24-44} * After$. These are interaction terms between the dummy "$After$", which is equal to one if a worker is hired from April 2012 onwards, and the dummy for the specific tenure group considered. As a result β_1, β_2 and β_3 should capture any deviation from a parallel evolution of the hazard of interest between the treatment groups and the control group that is due to the implementation of the reform. Finally, in all the regressions, we use robust standard errors clustered at the individual level.

Table 3.7 shows the estimates of regression 3.1 for the hazard of dismissal, in column 1, and the hazard of training, in column 2. Although none of the coefficients in column 1 shows up significant, the point estimates suggest that the 2012 reform decreases the firing hazard of workers with less than 1 year of tenure and increases that of workers with at least 12 months of tenure in the firm [20]. Consistently with these effects and with the theoretical predictions sketched in section 3.3, in table 3.7 we can see that the reform also decreases training investment both in the case of workers with 12-23 months of tenure, and in that of workers with more than 24 months of tenure. Interestingly, while point estimates are positive, the reform does not seem to have a significant impact on training investment targeted at workers in their first year in the firm [21].

Before analyzing more in depth these findings, let us first proceed to formally test the validity of the difference-in-difference strategy. The standard way to do this is to check

20. Yet, the coefficient on treatment group 1 is not significantly different from the one on treatment group 2.

21. Notice that the coefficient on treatment group 2 is significantly different from that on treatment group 1, but not from that on treatment group 3.

whether the evolution of the outcomes of interest for the treatment group starts diverging from that of the control group already prior to the implementation of the reform. To do so, we augment regression 3.1 by introducing interactions terms between each of the dummies $Tenure_{0-11}$, $Tenure_{12-23}$, $Tenure_{24-44}$ and dummies that are equal to one if the worker has been hired, respectively, 4, 3, 2, or 1 semester before the actual introduction of the reform - these terms are usually called "the leads of a reform". If we fail to reject a test on the joint significance of these terms - performed separately for each treatment group - we could then conclude that the treatment and the control group are indeed comparable [22]. Moreover, we can also use this specification to study the dynamic response to the reform by including the "lags of the reform", that is a series of interaction terms between the three treatment groups dummies and the hiring semester starting from April 2012 onwards. These terms allow us to check how the effects evolve from those on workers hired in the first semester following the introduction of the reform to those on workers hired three years (or 6 semesters) after. As a result, we estimate the following regression:

$$h_{ij}^{z} = 1 - exp\Bigg(-exp(\gamma_j^z + \delta^z X_{ij} + \alpha_1^z Tenure_{0-11} + \alpha_2^z Tenure_{12-23} + \alpha_3^z Tenure_{24-44} + \sum_k \beta_k^z Tenure_{0-11} * Leads\_Lags_k + \sum_k \beta_k^z Tenure_{12-23} * Leads\_Lags_k + \sum_k \beta_k^z Tenure_{24-44} * Leads\_Lags_k)\Bigg) \tag{3.2}$$

with k referring to the semester when a worker has been hired, going from the one starting in April 2010 to the one beginning in April 2015. Figures 3.5 and 3.6 give a graphical representation of the estimated coefficients on the leads and lags of the reform for each of the three treatment groups, when the outcome considered is, respectively, the hazard of dismissal and the hazard of training. Two things are worth noticing. First, the coefficients on the leads of the reform are never jointly significant, with the exception of those referring to treatment group 2 in the regression for the hazard of dismissal. Yet, even in this case, they are only significant at 10 percent significance level [23].

Secondly, consistently with the results illustrated in table 3.7 the reform does not have a clear impact on dismissals, while it clearly seems to have a negative impact on training investment for workers with at least 12 months of tenure - with the effect becoming more important over time.

Next, we compare the main results presented in table 3.7 with those obtained from the estimation of a competing-risks model with two risks, z_1 and z_2, which correspond to the

22. Or that, conditional on observable characteristics, the outcomes of interest for the treatment and the control groups follow a parallel trend prior to the introduction of the reform.

23. Table 3.8 reports the corresponding p-values for these tests.

probability to get trained and to leave the firm. In this case, the individual contribution to the likelihood function looks as follows:

$$L_i = \prod_{j=1}^{J} (\frac{h_{ij}^{z_1}}{1-h_{ij}^{z_1}})^{\delta_{z_1}=1} * (\frac{h_{ij}^{z_2}}{1-h_{ij}^{z_2}})^{\delta_{z_2}=1} . \prod_{k=u_i+1}^{j} [(1-h_{ik}^{z_1}) * (1-h_{ik}^{z_2})] \tag{3.3}$$

In this model δ_{z_1} (δ_{z_2}) takes value 1 if the first (second) risk occurs during the $j^{th}+1$ spell. Notice that, by specifying this model with delayed entry at time u_i, we have to condition on survival up to time u_i. As a result, in the second part of this expression, we compute the survival function from the k^{th} spell until the j^{th} spell, with $k = u_i + 1$.

Table 3.9 shows that the results obtained from the estimation of this model support our main findings. Even when taking into account how the reform affects the individual risk of separating from a firm, we still observe that this intervention triggers a 8 percent drop in the probability of participating in training for workers with 12 to 23 months of tenure, and a 13 percent drop in this hazard for workers with 24 to 44 months of tenure. Interestingly and in accordance with the theoretical predictions, this competing-risks model also suggests that the reform decreases the hazard of separation for workers with less than 1 year of tenure. Yet, we still fail to obtain any strong evidence that the reform increases job separations for workers with more than 12 months of seniority.

3.6 Further robustness checks

So far we have seen that the reform does appear to have a negative impact on training investment for workers with more than 1 year of tenure in the firm. Yet, we do not have strong evidence that this is due to an increase of dismissals. Before discussing other channels that could explain these results, we present two additional robustness checks to ensure that what we capture is really the impact of the reform.

First, in tables 3.10 and 3.11, we restrict the control group in column 2, and enlarge it in column 3, to check to what extent our estimates are sensitive to its definition. As we can see, neither the estimates regarding the hazard of dismissal nor those concerning the hazard of training change much in comparison to the main ones, reported in column 1 of each table.

Secondly, in tables 3.12 and 3.13, we estimate the effect of two placebo reforms on, respectively, the hazard of dismissal and that of training, by pretending that the intervention took place before April 2012. In the first column of each table we report our main results. Then, in columns 2 and 3 we report the impact of two fake reforms that should have taken place, respectively, in October 2011 and January 2010. In both cases we restrict the estimation sample to workers hired before the introduction of the actual 2012 reform. Yet, we allow both reforms to display their effect during the subsequent 3.5 years, that is, respectively, until the second quarter of the year 2015, and until the third quarter of the

year 2013 [24]. As we can see, there is no evidence that any of these placebo reforms has an impact on either of the three treated groups, and this is the case both for the hazard of dismissal and that of training. The fact that none of these coefficients shows up significant strongly suggests that in the main regressions we are not capturing the effect of persistent differences in the evolution of the outcomes of interest between the treatment and the control groups.

As a last robustness check, we analyze whether the introduction of this reform affects the probability of working more than 16 hours, given that it applies only to employees working at least this amount of hours. Firms might indeed modify their recruitment or management practices to have more employees above this threshold - and hence have lower expected firing costs. The treatment groups might then get to include workers who would have presumably worked fewer hours in the absence of the reform. If these are more likely to be of lower ability, and the probability of getting trained is positively correlated with ability, our results might just capture this selection bias. Column 1 of table 3.14 shows that workers hired from April 2012 are not more likely to work at least 16 hours than workers in the control group, regardless of their tenure in the firm. In addition, column 2 and 3 also show that including employees working less than 16 hours in the estimation sample does not change our main results. It is not surprising as we enlarge the benchmark sample by only 5%.

Overall, this set of robustness checks supports the validity of our identification strategy [25].

3.7 Alternative mechanisms

In section 3.3 we theorized that the main channel through which this reform could affect firms' and workers' willingness to invest in training is by changing the expected duration of employment relationships. Yet, so far we have not found any strong evidence that dismissals increase in response to this reform. How can we explain the drop in training investment

24. Indeed, we want to observe the effect of placebo reforms during the same duration as in the 2012 reform case.

25. In table 3.15, we also check whether the way we define the treatment groups affects our results. The reason for doing so is the following. Our outcome variables take value 1 if, between t and period $t+1$, the individual experiences the event of interest. However, our data are interval-censored, that is we can only observe such an event every three months. This implies that if an individual with 10 or 11 months of seniority at t declares that he/she has left the firm at time $t+1$, we cannot know whether this happens when he/she has already 12 months of tenure or before. In our main analysis, we keep these workers who are at the margin between one treatment group and the other, that is those with 10 and 11 months of tenure, and those with 22 or 23 months of tenure, as they only represent a small fraction of the estimation sample - 7,000 observations in total. In table 3.15 we show that our results do not change when we exclude them from the estimation sample.

then? On the one hand, as the graph showing the hazard of dismissal suggests (figure 3.3), it is possible that dismissals do increase, but that this effect becomes especially evident only for workers who are approaching the new expiration of the probationary period. On the other hand, the reform might affect training through other mechanisms, notably a decrease in the quality of workers hired after April 2012, an increase in job mobility due to reasons other than dismissals, or an increase in wages of workers hired after April 2012. Importantly, all these mechanisms can also be simultaneously at play. In this paragraph, we analyze each in turn all these possibilities.

First, in table 3.16 we study the impact of the reform on more detailed tenure categories. Column 1 displays the impact on the hazard of dismissals, and column 2 reports the impact on the hazard of training. Although once again none of the coefficients in column 1 turns out significant, it is interesting to notice that the sign of the point estimates switches from negative to positive when we get to consider workers who are very close to the new expiration of the probationary period. And while the coefficients on the impact of the reform on training start becoming negative already for workers with 7 to 11 months of tenure, they become significant only for workers with at least 12 months of tenure in the firm. Although far from being conclusive, this table brings further suggestive evidence that the 2012 reform does increase the hazard of dismissal for workers with more than 1 year of tenure in the firm, precisely as predicted by the theory sketched in section 3.3.

Next, in section 3.3 we predicted that the 2012 reform could also affect firms' hiring decisions. The decrease of firing costs might indeed induce firms to post more vacancies and hire more workers. This could have two consequences. On the one hand, it could decrease the quality of newly hired workers. On the other hand, it could stimulate job mobility.

If the quality of the marginal worker hired in the post-reform period decreases in comparison to the pre-reform era, and if training is complementary to ability, this channel could explain why we observe a drop in training investment for workers with 12 to 23 months of tenure. Yet, given that firing costs sharply increase after the first two years of tenure, we should expect that, once the probationary period is over, workers' quality in the post-reform period will converge to that of the pre-reform era. In other words, this channel could hardly explain the drop in training investment that concerns workers with more than 2 years of tenure in the firm.

However, if the reform stimulates job creation, this drop in training investment might be explained by workers' increased propensity to change their job in the post-reform period.

The data mainly give some support to this second hypothesis. First of all, figure 3.7 plots the evolution of the job-finding rate from 2009 to 2015. Although we cannot identify a causal link in this case, from this graph it does seem that firms' hirings increase right after the implementation of the reform. However, this increase of the job finding rate does not seem to induce a decrease of the workers' quality. In table 3.17 we compare some observable characteristics of individuals hired before and after the April 2012 reform: it seems that

workers hired after the reform are younger than those hired before, while education and gender remains approximatively the same. Indeed, the share of individuals younger than 21 years old more than doubles compare to the pre-reform era. If anything, these findings suggest that workers' quality did not decrease, and that the reform enhanced the mobility of the youngest workers.

Does this stimulate workers' willingness to change job? Before exploring the answer to this question in the data, we have to consider three issues. First, in the data section we showed that the only way we can analyze quits is to look at overall job mobility, given that, over the period considered, the LFS does not allow us to measure quits precisely. Job mobility records any job-to-job transition, regardless of the reason that caused it. In particular, it comprises both job changes due to a dismissal and those due to a quit. Inferring the impact of the reform on quits when looking at job mobility might then be inconclusive [26]. Secondly, as mentioned in section 3.5 these estimates may be downward-biased if the reform also induces the control group to change job more frequently. Third, the formal test on the validity of the diff-in-diff strategy suggests that the parallel trend assumption is not satisfied in the case of job mobility. Hence, these results should be interpreted with caution. With these caveats in mind, we report the impact of the reform on job mobility in table 3.18. Although only the coefficients on treatment group 1 show up (barely) significant, point estimates suggest that the reform increases job mobility for workers with more than 2 years of tenure in the firm, while decreasing job mobility for workers who are still in their probationary period - the coefficient on treatment group 3 is significantly different both from the one on treatment group 2 and from the one on treatment group 1. These findings, if true, might contribute to explain the drop in training investment for workers with more than two years of tenure in the firm.

The last channel we consider to explain the drop of training that follows the implementation of the reform, is the impact of this intervention on the wage-setting process. In particular, we want to understand whether firms increase entry wages to compensate workers for the prolonged risk of being dismissed, and whether this in turn could explain the drop in training investment. In table 3.19 we test this hypothesis by estimating an OLS model on gross hourly wages and using the same difference-in-difference strategy to identify the impact of the reform. Contrary to our first hypothesis, the point estimates on the impact of the reform are negative and barely significant for those concerning the third treatment group. If anything, these findings suggest that the decrease in firing costs triggered by this reform has weaken workers' bargaining power, which in turn may have slowed down their wage growth.

In light of these additional findings, we are inclined to conclude that two factors seem to be the most plausible candidates to explain the observed drop in training investment.

26. The theory discussed in section 3.3 shows indeed that: dismissals should decrease, while quits should increase for treatment group 1; dismissals should increase, while the impact of the reform on quits is ambiguous for both treatment group 2 and 3.

First, although not significant, point estimates suggest that the hazard of dismissal does increase for workers with more than 1 year of tenure in the firm [27]. Secondly, by enhancing job creation, this reform might have stimulated job mobility, especially for what concerns workers who "survived" to the new probationary period.

3.8 Conclusion

In this paper we study how employment protection legislation affects firms and workers' decisions to invest in human capital. To do so, we look at the United Kingdom, as it offers the ideal setting to study this question. First, EPL in the United Kingdom is characterized by the presence of the single/unified open-ended contract. The main feature of this contract is that workers' rights at termination are equalized across temporary and permanent workers, and rise with seniority, after a probationary period during which workers are not entitled either to the right to sue their employer for unfair dismissal, or to severance payments in case of dismissal. Secondly, the United Kingdom is one of the few countries, at least in Europe, where workers receive high doses of on-the-job training. And crucially, in April 2012 the British parliament approved a reform of EPL that extended the length of the probationary period from 12 to 24 months of tenure for workers hired after this date.

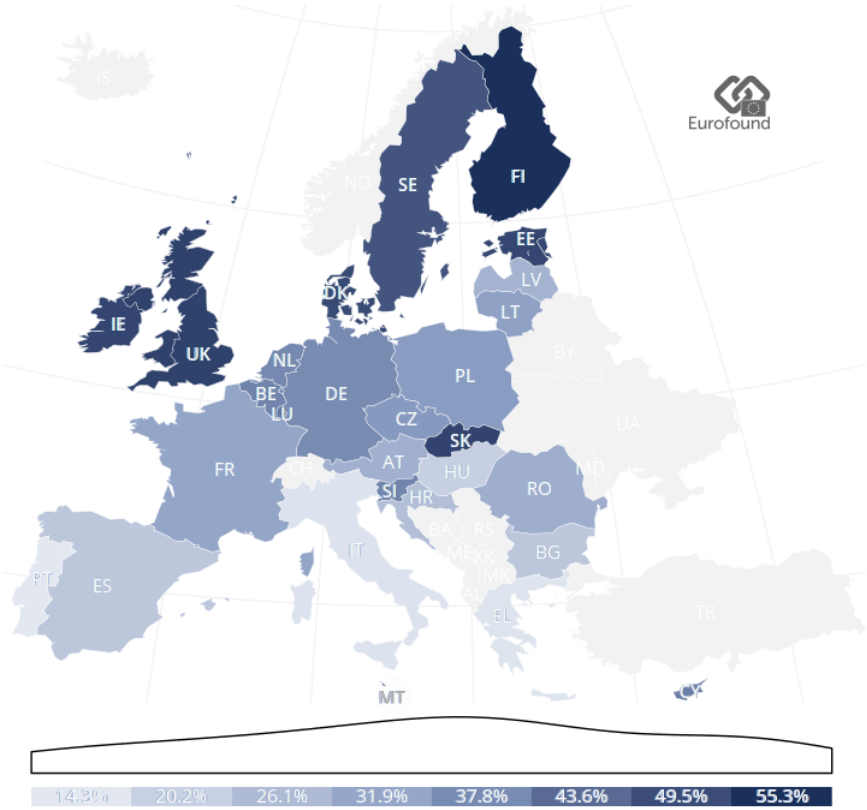
To study the impact of this intervention, we use a difference-in-difference strategy exploiting the variation in its implementation over time and across workers' tenure. Our findings show that, by enhancing both job creation and job destruction, this reform decreases training investment for workers with more than 1 year of tenure by 8 to 14 percent, depending on the tenure group considered. These results are further validated via the estimation of a competing-risks model where separating from a firm and being trained are treated as competing risks.

These findings have important policy-implications. The rational behind the existence of a probationary period in the unified contract is to allow firms to have a certain period of time to evaluate the quality of the new hire, without incurring in any cost in case the employment relationship results to be a bad match. Yet, while enhancing job creation, this tool seems to hinder training investment. To overcome this drawback, under the hypothesis that workers are credit-constrained, governments might want to consider the opportunity of introducing training subsidies targeted at workers who are going through their probationary period.

27. When studying the heterogeneous effects of this reform, we also find that the reform significantly increases the dismissal hazard of low-educated workers with 12 to 23 months of tenure. It is in line with the fact that the least educated individuals are the most vulnerable to unemployment. These results, reported in table 3.20, bring further support to the hypothesis that the reform might have lowered firms' and workers' expectations regarding the duration of an employment relationship.

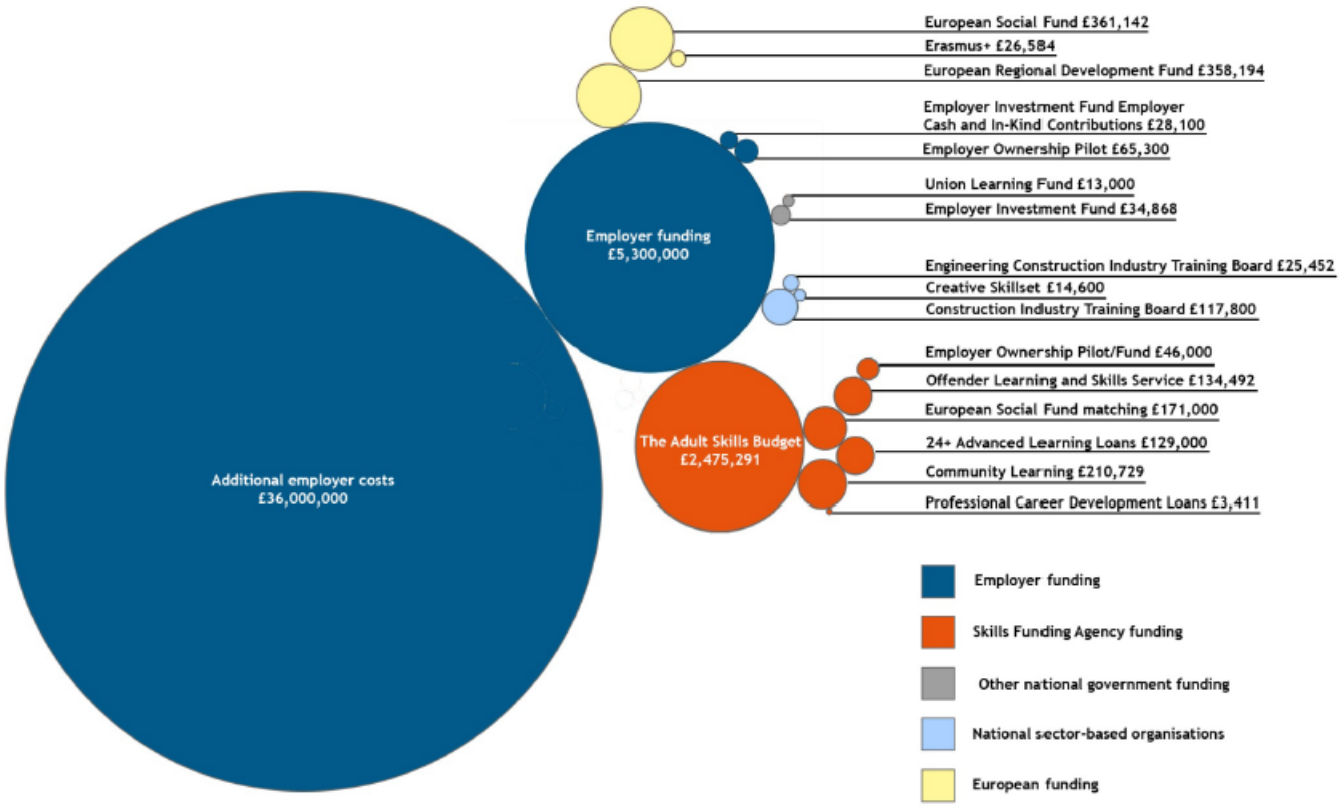
Appendix 3.A: Figures

FIGURE 3.1 – On-the-job training across countries



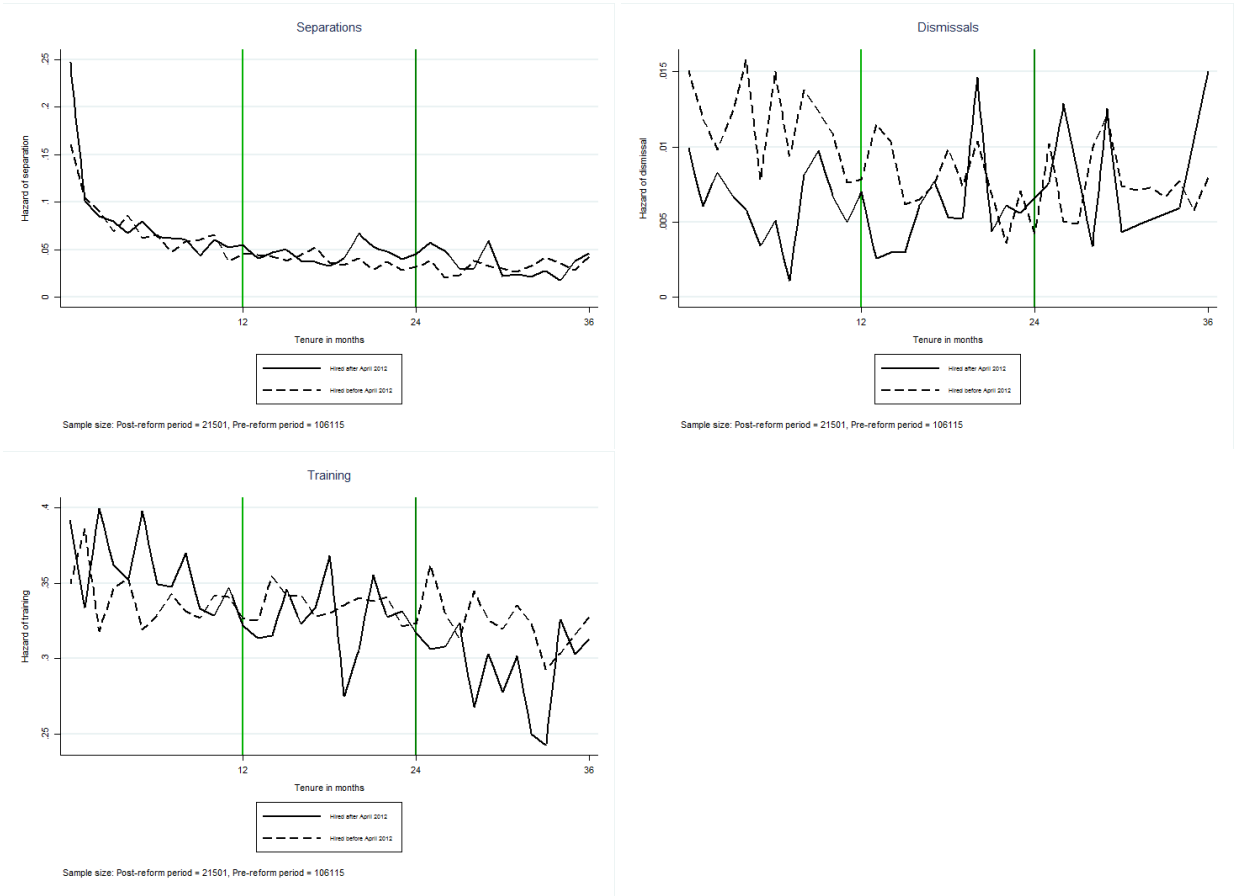
Source: Sixth European Working Conditions Survey 2015, Eurofound.
Note: The figure reports the percentages of workers per country that answered "Yes", when asked "Have you had on-the-job training in the past year ?"

FIGURE 3.2 – Main sources of investment in training in the UK



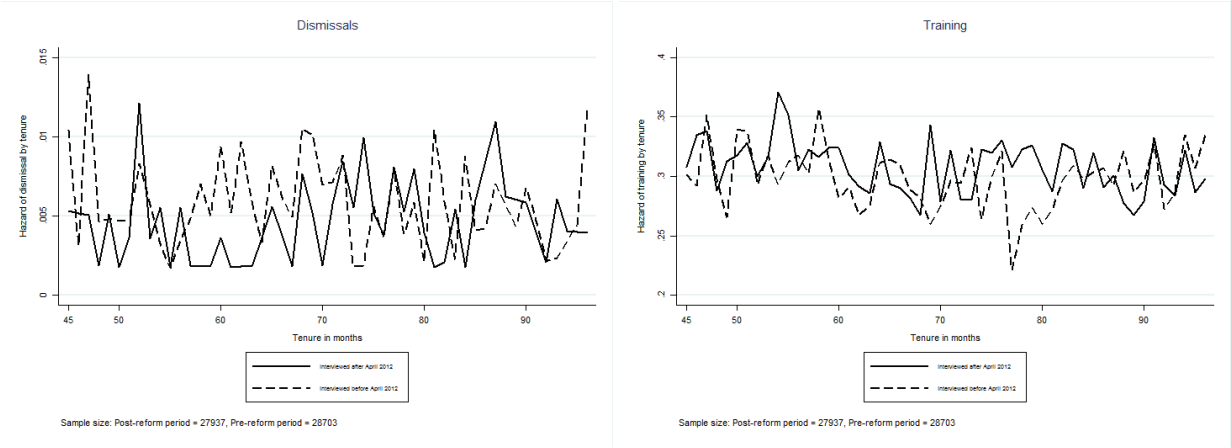
Source: "Mapping investment in adult skills – which individuals, in what learning and with what returns?" BIS Research Paper, 2016.
Note: The figure reports the main providers of training for the year 2013/2014. Training refers both to activities organized by the firm's staff and to formal training courses organized outside the firm's premises.

FIGURE 3.3 – Hazard rates by tenure in the firm



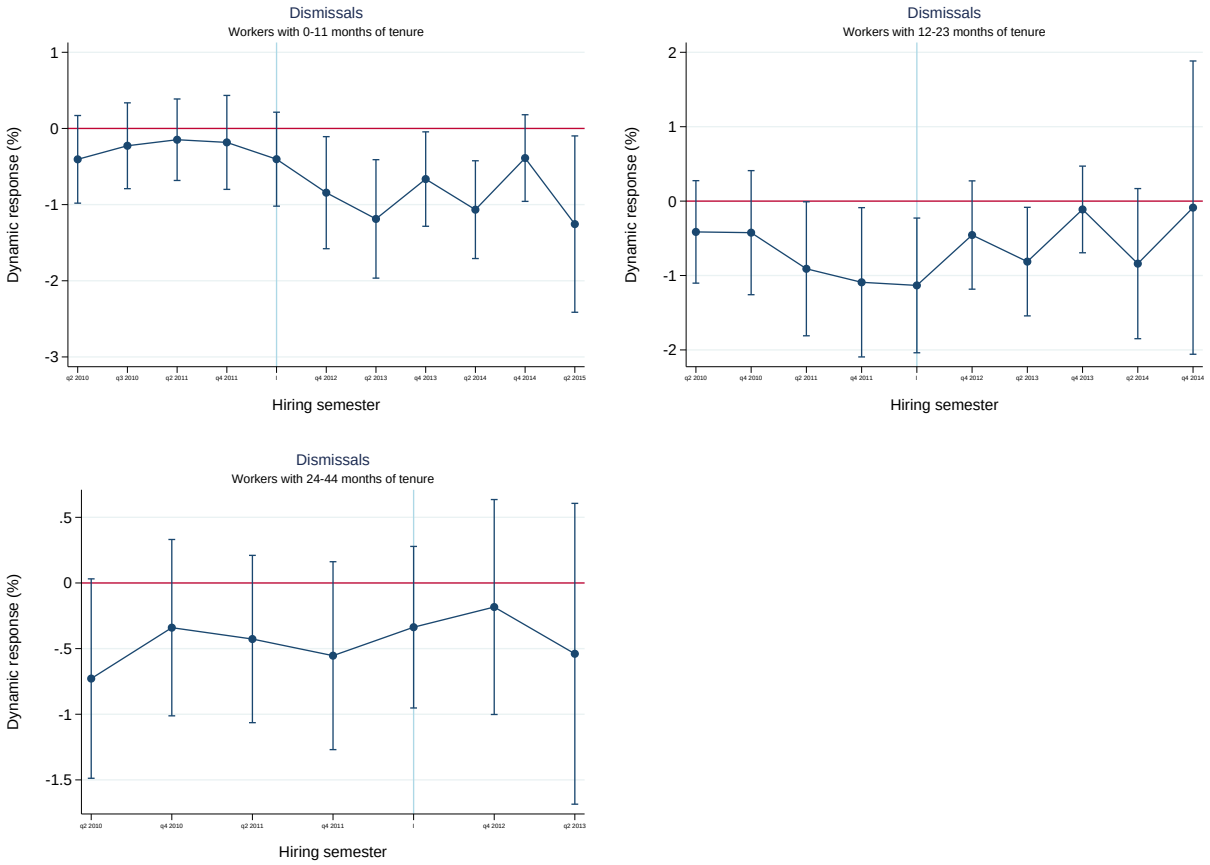
Source: UK Labour Force Survey 2009-2015.
Note: These graphs report the hazards of a job separation, dismissal and training investment as a function of tenure in the firm. The sample is restricted to individuals working more than 16 hours per week, with a permanent contract and known tenure. The dotted lines represent the hazards in the pre-reform period, while the solid lines refer to the period after April 2012 when the reform becomes binding for all workers in the two treated groups. The sample comprises 21,707 individuals with 11 months of tenure or below, 20,305 between 12 and 23 months of tenure, and 84,667 between 24 and 96 months of tenure. Among each group, the number of treated individuals amounts to 10,804, 7,044 and 3,653, respectively.

FIGURE 3.4 – Hazard rates for individuals between 45 and 96 months of tenure



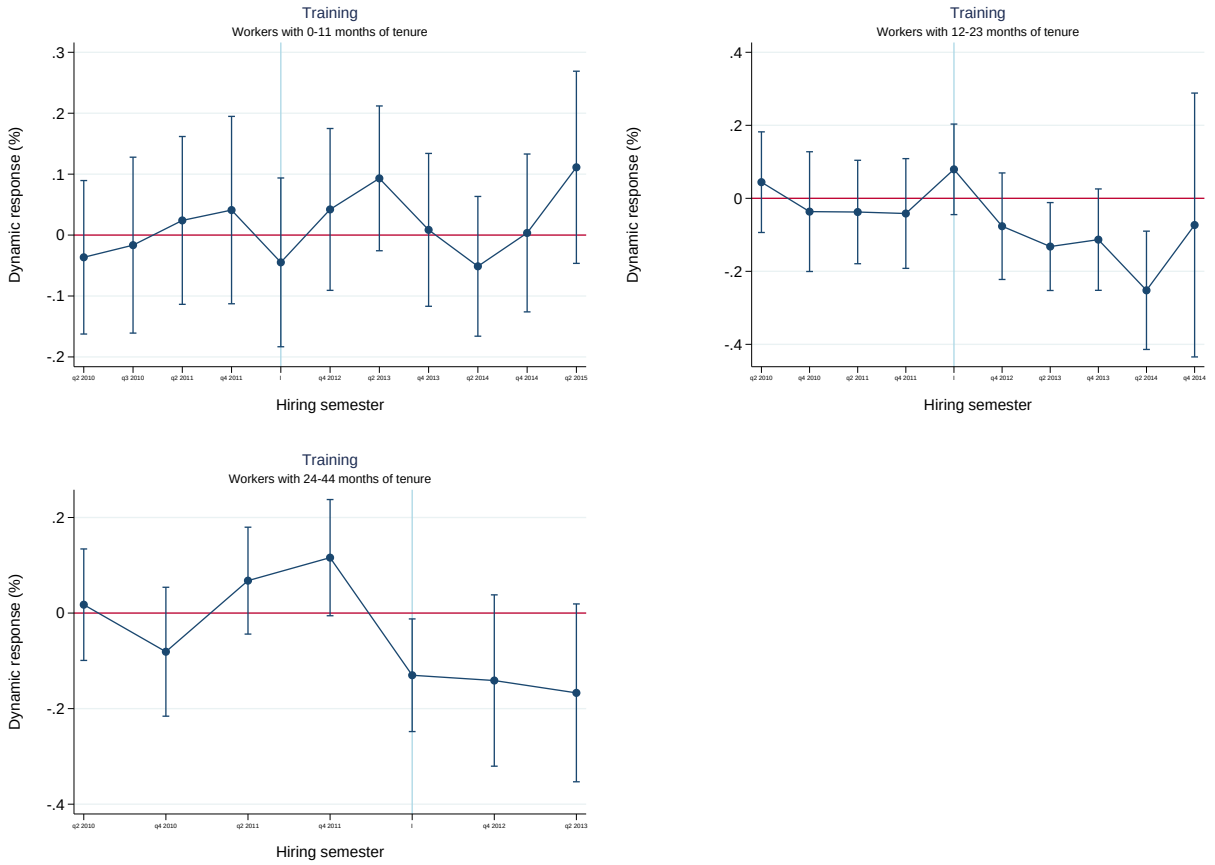
Source: UK Labour Force Survey 2009-2015.
Note: These graphs report the hazards of dismissal and training investment as a function of tenure in the firm. The sample is restricted to individuals between 45 and 96 months of tenure, working more than 16 hours per week, with a permanent contract and known tenure. The dotted lines represent the hazard of individuals interviewed before the 2012 reform, while the solid lines refer to those interviewed after the reform. The sample comprises 56,640 individuals, among which 28,703 interviewed before the reform and 27,937 interviewed after.

FIGURE 3.5 – Conditional trends for the hazard of dismissal, by tenure group



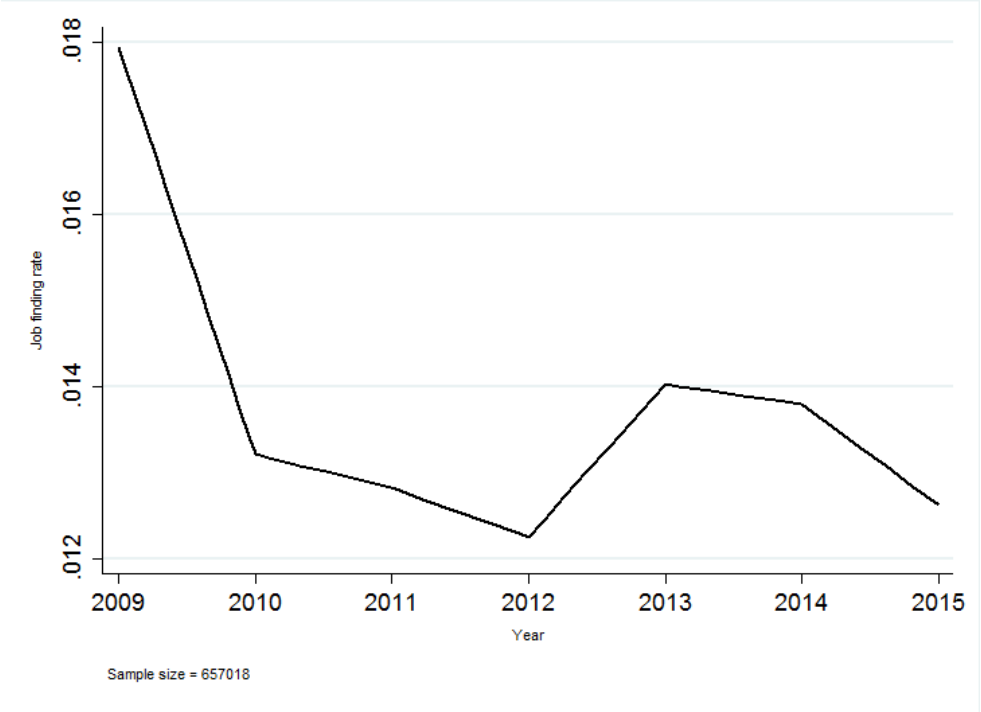
Source: UK Labour Force Survey 2009-2015.
Note: These graphs report the coefficients of the leads and lags of the reform, as estimated using regression 3.2. "I" refers to April 2012, when the reform is introduced. The estimation sample is restricted to individuals working more than 16 hours per week, with a permanent contract and known tenure. Table 3.8 reports the p-values of the test performed on the joint significance of the coefficients on the leads of the reform, run separately for each tenure group.

FIGURE 3.6 – Conditionnal trends for the hazard of training, by tenure group



Source: UK Labour Force Survey 2009-2015.
Note: These graphs report the coefficients of the leads and lags of the reform, as estimated using regression 3.2. "I" refers to April 2012, when the reform is introduced. The estimation sample is restricted to individuals working more than 16 hours per week, with a permanent contract and known tenure. Table 3.8 reports the p-values of the test performed on the joint significance of the coefficients on the leads of the reform, run separately for each tenure group.

FIGURE 3.7 – Evolution of the job finding rate



Source: UK Labour Force Survey 2009-2015.
Note: This graph reports the evolution of the job finding rate, over the period 2009-2015. A hiring takes place either when an unemployed individual finds a job or when an employed worker moves from one job to another.

Appendix 3.B: Tables

TABLE 3.1 – Training purpose by industry

	(1) Induction	(2) Security	(3) Job-specific	(4) Supervisory	(5) Management	(6) New technologies	(7) Personal Development
Agriculture	0.26	0.44	0.51	0.15	0.13	0.27	0.02
Energy	0.52	0.65	0.66	0.30	0.26	0.35	0.03
Manufacturing	0.43	0.58	0.58	0.25	0.23	0.32	0.02
Construction	0.38	0.57	0.53	0.22	0.18	0.25	0.01
Distribution	0.53	0.62	0.64	0.38	0.38	0.33	0.02
Transport	0.39	0.48	0.60	0.21	0.21	0.42	0.03
Banking	0.49	0.57	0.70	0.28	0.31	0.44	0.04
Public adm, edu, health	0.70	0.85	0.85	0.46	0.54	0.49	0.06
Other services	0.51	0.63	0.69	0.31	0.31	0.36	0.03

Source: UK Employer Survey Skills 2011.
Note: The table reports the share of firms declaring to train their workers for the specific purpose indicated on top of each column. Figures are broken down by industry. The sample comprises all firms participating in the UKCES 2011, a total of 86,522 entities.

TABLE 3.2 – Employer-provided training by length of the training spell

	(1) $\leq$ 1 week	(2) Btw 1-2 weeks	(3) Btw 2-3 weeks	(4) $\geq$ 3 weeks
Agriculture	0.46	0.06	0.02	0.02
Energy	0.56	0.08	0.03	0.06
Manufacturing	0.49	0.07	0.03	0.06
Construction	0.48	0.07	0.02	0.06
Distribution	0.48	0.09	0.03	0.08
Transport	0.50	0.08	0.03	0.06
Banking	0.54	0.09	0.04	0.06
Public adm, edu, health	0.65	0.11	0.04	0.06
Other services	0.53	0.08	0.04	0.08

Source: UK Employer Survey Skills 2011.
Note: The table reports the share of firms declaring to train their workers for a specific amount of time, as indicated on top of each column. Figures are broken down by industry. The sample comprises all firms participating in the UKCES 2011, a total of 86,522 entities.

TABLE 3.3 – Employer-provided training by firm size

	(1) Employer-provided training
1-24 employees	0.39
25-49 employees	0.55
50-99 employees	0.58
100-249 employees	0.60
250+ employees	0.58

Source: UK Employer Survey Skills 2011.
Note: The table reports the share of firms declaring providing training, by firm size. Employer-provided training comprises both training done at the employer's premises and outside, provided that it is financed by the employer. The sample comprises all firms participating in the UKCES 2011, a total of 86,522 entities.

TABLE 3.4 – Reason for leaving the last job

	Percentage of workers reporting a specific reason
Dismissal	2
Redundancy	24
Temporary job which came to an end	8
Resigned	18
Gave up work for health reasons	13
Retired	1
Gave up work for personal reasons	2
Education or training	8
Left for some other reason	25
Total	100

Source: UK Labor Force Survey 2009-2015.
Note: The sample comprises workers who have left a paid job in the last three months, and those who have been unemployed for at most three months. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Moreover, it is further restricted to individuals interviewed before April 2012.

TABLE 3.5 – Proportion of workers participating in training by subgroup [1]

	Training
Panel A.Tenure	
Tenure $\leq$ 11 months	0.29
Tenure btw 12-23 months	0.28
Tenure btw 24-36 months	0.27
Tenure $\geq$ 44 months	0.26
Panel B. Educational level	
Higher education	0.34
Upper secondary education	0.26
Lower secondary education	0.22
Panel C. Age	
16-34 years old	0.28
35-70 years old	0.26
Panel D. Gender	
Male	0.24
Female	0.30
Panel E. Working schedule	
Full-time	0.28
Part-time	0.24
Panel F. Sector	
Private sector	0.22
Public sector	0.39

Source: UK Labor Force Survey 2009-2015
Note: The sample comprises all individuals who are employed in the first quarter, work at least 16 hours and whose tenure is known. In this table, the sample is further restricted to individuals interviewed in the pre-reform period. As a result, it comprises 106,115 observations.

TABLE 3.6 – Proportion of workers participating in training by subgroup [2]

	Training
Panel A. Occupation	
Professional occupations	0.40
Personal Service Occupations	0.38
Associate Professional	0.32
Managers and Senior Officials	0.25
Administrative	0.22
Skilled Trades Occupations	0.21
Sales Occupation	0.18
Machine Operatives	0.16
Elementary Occupations	0.14
Panel B. Industry	
Public adm, edu, health	0.40
Banking	0.25
Energy	0.25
Other services	0.24
Construction	0.21
Transports	0.20
Manufacturing	0.17
Distribution	0.16
Agriculture	0.14

Source: UK Labor Force Survey 2009-2015
Note: The sample comprises all individuals who are employed in the first quarter, work at least 16 hours and whose tenure is known. In this table, the sample is further restricted to individuals interviewed in the pre-reform period. As a result, it comprises 106,115 observations.

TABLE 3.7 – Impact of the 2012 reform on the hazard of dismissal and training

	(1) Dismissal	(2) Training
Tenure 0-11*after	-0.189	0.0107
	(0.181)	(0.0361)
Tenure 12-23*after	0.0596	-0.0829**
	(0.213)	(0.0400)
Tenure 24-44*after	0.142	-0.135***
	(0.266)	(0.0495)
Tenure<= 11 months	0.628***	0.190***
	(0.115)	(0.0267)
Tenure btw 12 and 23 months	0.314***	0.159***
	(0.118)	(0.0244)
Tenure btw 24 and 44 months	0.228**	0.0764***
	(0.0991)	(0.0195)
Observations	127,616	127,616

Source: UK Labor Force Survey 2009-2015
Note: All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.8 – Testing the parallel trend assumption

	P-values for the test on the joint significance of the leads of the reform		
	0-11	12-23	24-44
Dismissals	0.687	0.054	0.117
Training	0.916	0.871	0.160

Source: UK Labor Force Survey 2009-2015
Note: The table reports the p-values of tests on the joint significance of the leads of the reform, as estimated using regression 3.2 when the outcome is either the hazard of dismissal or the hazard of training. Each test is performed separately for each tenure group. The sample used to estimate regression 3.2 is restricted to individuals working more than 16 hours per week, with a permanent contract and known tenure.

TABLE 3.9 – Impact of the 2012 reform on the hazard of training - competing risks model

	(1) Training	(2) Separation
Tenure 0-11 * After	-0.002	-0.131*
	(0.024)	(0.069)
Tenure 12-23 * After	-0.080***	-0.079
	(0.025)	(0.082)
Tenure 24-44 * After	-0.133***	0.116
	(0.030)	(0.101)
Tenure $\leq$ 11 months	0.189***	1.148***
	(0.018)	(0.054)
Tenure btw 12 and 23 months	0.156***	0.564***
	(0.015)	(0.055)
Tenure btw 24 and 44 months	0.075***	0.328***
	(0.012)	(0.048)
Observations	127,616	127,616

Source: UK Labor Force Survey 2009-2015
Note: All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.10 – Impact of the 2012 reform on the hazard of dismissal - varying the control group size

	(1) Dismissals	(2) Dismissals	(3) Dismissals
Tenure 0-11*after	-0.189	-0.116	-0.221
	(0.181)	(0.192)	(0.176)
Tenure 12-23*after	0.0596	0.0898	0.0429
	(0.213)	(0.223)	(0.209)
Tenure 24-44*after	0.142	0.160	0.149
	(0.266)	(0.277)	(0.260)
tenure<= 11 months	0.628***	0.566***	0.641***
	(0.115)	(0.124)	(0.111)
tenure btw 12 and 23 months	0.314***	0.269**	0.320***
	(0.118)	(0.127)	(0.114)
tenure btw 24 and 44 months	0.228**	0.184*	0.232**
	(0.0991)	(0.110)	(0.0949)
Observations	127,616	103,713	143,334
Control group	$\leq$ 8 years	$\leq$ 6 years	$\leq$ 10 years

Source: UK Labor Force Survey 2009-2015

Note: Column 1 reports the main results. Column 2 restricts the control group to workers having 45-72 months of tenure. Column 3 extends the control group to workers having 45-120 months of tenure. All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.11 – Impact of the 2012 reform on the hazard of training - varying the control group size

	(1) Training	(2) Training	(3) Training
Tenure 0-11*after	0.0107	0.0203	0.00806
	(0.0361)	(0.0376)	(0.0354)
Tenure 12-23*after	-0.0829**	-0.0690*	-0.0880**
	(0.0400)	(0.0416)	(0.0393)
Tenure 24-44*after	-0.135***	-0.117**	-0.138***
	(0.0495)	(0.0511)	(0.0488)
tenure<= 11 months	0.190***	0.171***	0.196***
	(0.0267)	(0.0283)	(0.0261)
tenure btw 12 and 23 months	0.159***	0.140***	0.164***
	(0.0244)	(0.0261)	(0.0238)
tenure btw 24 and 44 months	0.0764***	0.0600***	0.0808***
	(0.0195)	(0.0212)	(0.0189)
Observations	127,616	103,713	143,334
Control group	$\leq$ 8 years	$\leq$ 6 years	$\leq$ 10 years

Source: UK Labor Force Survey 2009-2015
Note: Column 1 reports the main results. Column 2 restricts the control group to workers having 45-72 months of tenure. Column 3 extends the control group to workers having 45-120 months of tenure. All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.12 – Impact of the 2012 reform on the hazard of dismissal - placebo tests

	(1) Dismissals	(2) Dismissals	(3) Dismissals
Tenure 0-11*after	-0.189		
	(0.181)		
Tenure 12-23*after	0.0596		
	(0.213)		
Tenure 24-44*after	0.142		
	(0.266)		
Tenure 0-11*after_placebo1		0.185	
		(0.319)	
Tenure 12-23*after_placebo1		-0.593	
		(0.524)	
Tenure 24-44*after_placebo1		-0.0574	
		(0.378)	
Tenure 0-11*after_placebo2			-0.0929
			(0.202)
Tenure 12-23*after_placebo2			-0.340
			(0.244)
Tenure 24-44*after_placebo2			-0.198
			(0.305)
Observations	127,616	102,912	84,094

Source: UK Labor Force Survey 2009-2015

Note: This table reports the impact of two placebo reforms on the hazard of dismissal, by pretending that the intervention took place before April 2012. In the first column, we report our main results. Then, in columns 2 and 3 we report the impact of two fake reforms that should have taken place, respectively, in October 2011 and January 2010. In both cases we restrict the estimation sample to workers hired before the introduction of the actual 2012 reform. Yet, we allow both reforms to display their effect during the subsequent two years, that is, respectively, until the second quarter of the year 2015, and until the third quarter of the year 2013. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.13 – Impact of the 2012 reform on the hazard of training - placebo tests

	(1) Training	(2) Training	(3) Training
Tenure 0-11*after	0.0107		
	(0.0361)		
Tenure 12-23*after	-0.0829**		
	(0.0400)		
Tenure 24-44*after	-0.135***		
	(0.0495)		
Tenure 0-11*after_placebo1		0.0588	
		(0.0777)	
Tenure 12-23*after_placebo1		-0.0266	
		(0.0780)	
Tenure 24-44*after_placebo1		0.0983	
		(0.0665)	
Tenure 0-11*after_placebo2			0.00932
			(0.0491)
Tenure 12-23*after_placebo2			0.0000411
			(0.0473)
Tenure 24-44*after_placebo2			0.0344
			(0.0525)
Observations	127,616	102,912	84,094

Source: UK Labor Force Survey 2009-2015
Note: This table reports the impact of two placebo reforms on the hazard of training, by pretending that the intervention took place before April 2012. In the first column, we report our main results. Then, in columns 2 and 3 we report the impact of two fake reforms that should have taken place, respectively, in October 2011 and January 2010. In both cases we restrict the estimation sample to workers hired before the introduction of the actual 2012 reform. Yet, we allow both reforms to display their effect during the subsequent two years, that is, respectively, until the second quarter of the year 2015, and until the third quarter of the year 2013. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.14 – Impact of the 2012 reform on the probability of working more than 16 hours

	(1) Working more than 16h	(2) Dismissal	(3) Training
Tenure 0-11*after	0.00470	-0.187	0.00855
	(0.0269)	(0.181)	(0.0355)
Tenure 12-23*after	-0.0357	0.0545	-0.0877**
	(0.0299)	(0.213)	(0.0392)
Tenure 24-44*after	-0.00143	0.139	-0.145***
	(0.0375)	(0.266)	(0.0491)
tenure<= 11 months	-0.213***	0.629***	0.199***
	(0.0197)	(0.115)	(0.0262)
tenure btw 12 and 23 months	-0.0926***	0.300**	0.161***
	(0.0188)	(0.118)	(0.0239)
tenure btw 24 and 44 months	-0.0448***	0.222**	0.0825***
	(0.0151)	(0.0991)	(0.0191)
Observations	134,510	134,510	134,510

Source: UK Labor Force Survey 2009-2015

Note: This table analyzes whether the reform might have affected the probability of working more than 16 hours. In detail, column one reports the impact of the reform on the probability of working more than 16 hours. Column 2 and 3 report the impact of the reform on the hazard of dismissal and training, including in the estimation sample individuals working less than 16 hours. Otherwise, the sample is still restricted to individuals employed in the first quarter and with a permanent contract. All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.15 – Impact of the 2012 reform on the hazard of dismissal and training - restricting the treatment groups

	(1) Dismissals	(2) Training
Tenure 0-9*after	-0.220	0.0229
	(0.192)	(0.0381)
Tenure 12-21*after	0.00198	-0.0918**
	(0.225)	(0.0418)
Tenure 24-44*after	0.171	-0.135***
	(0.266)	(0.0496)
tenure<= 9 months	0.661***	0.191***
	(0.122)	(0.0284)
tenure btw 12 and 21 months	0.367***	0.169***
	(0.124)	(0.0258)
tenure btw 24 and 44 months	0.226**	0.0765***
	(0.0991)	(0.0195)
Observations	120,746	120,746

Source: UK Labor Force Survey 2009-2015
Note:: All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.16 – Impact of the 2012 reform on finer tenure groups

	(1) Dismissals	(2) Training
Tenure 0-6*after	-0.226	0.0353
	(0.221)	(0.0425)
Tenure 7-11*after	-0.140	-0.0304
	(0.255)	(0.0464)
Tenure 12-17*after	-0.116	-0.0889*
	(0.280)	(0.0467)
Tenure 18-23*after	0.279	-0.0836
	(0.293)	(0.0518)
Tenure 24-44*after	0.146	-0.136***
	(0.266)	(0.0496)
tenure<= 6 months	0.661***	0.199***
	(0.139)	(0.0319)
tenure btw 7 and 11 months	0.585***	0.181***
	(0.152)	(0.0325)
tenure btw 12 and 17 months	0.341**	0.170***
	(0.152)	(0.0297)
tenure btw 18 and 23 months	0.284*	0.148***
	(0.155)	(0.0290)
tenure btw 24 and 44 months	0.228**	0.0767***
	(0.0991)	(0.0195)
Observations	127,616	127,616

Source: UK Labor Force Survey 2009-2015
Note: All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Standard errors are clustered at the individual level. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.17 – Characteristics of individuals hired before and after the 2012 reform

	Highly-educated	Younger 21	Female
Before	0.42	0.05	0.51
After	0.45	0.11	0.51
Total	0.42	0.06	0.51

Source: UK Labor Force Survey 2009-2015
Note: The table reports the share of individuals highly-educated, younger than 21 years old and the share of women hired before and after the 2012 reform.

TABLE 3.18 – Impact of the 2012 reform on the hazard of job mobility

	(1) Job mobility
Tenure 0-11*after	-0.179* (0.096)
Tenure 12-23*after	-0.176 (0.114)
Tenure 24-44*after	0.186 (0.127)
Tenure<= 11 months	1.265*** (0.0811)
Tenure btw 12 and 23 months	0.722*** (0.083)
Tenure btw 24 and 44 months	0.404*** (0.069)
Observations	127,616

Source: UK Labor Force Survey 2009-2015
Note: The regression includes controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, the regression has tenure, quarter and region fixed effects. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.19 – Impact of the 2012 reform on gross, nominal hourly wages

	(1) Wage
Tenure 0-11*after	-0.249 (0.194)
Tenure 12-23*after	-0.216 (0.217)
Tenure 24-44*after	-0.515* (0.308)
tenure<= 11 months	-0.787*** (0.155)
tenure btw 12 and 23 months	-0.698*** (0.144)
tenure btw 24 and 44 months	-0.211 (0.202)
Observations	51,840

Source: UK Labor Force Survey 2009-2015
Note: The table reports the results of the estimation of an OLS model where the outcome is individual's nominal, gross hourly wage. All regressions include controls for sex, age, education, marital status. Controls for job characteristics such as full-time, occupation, industry, sector are also included. Finally, all regressions have tenure, quarter and region fixed effects. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Only two observations per worker are used in the estimation. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

TABLE 3.20 – Impact of the 2012 reform by subgroup

	Dismissals			Training		
	Tenure 0-11*after	Tenure 12-23*after	Tenure 24-44*after	Tenure 0-11*after	Tenure 12-23*after	Tenure 24-44*after
Panel A. Educational level						
No college degree	-0.0937	0.870*	0.0862	-0.0395	-0.112*	-0.224
	(0.276)	(0.481)	(0.421)	(0.052)	(0.06)	(0.079)
College degree	-0.234	0.394	0.212	0.0547	-0.0568	-0.0728
	(0.245)	(0.247)	(0.342)	(0.0502)	(0.0536)	(0.0633)
P-value difference	0.704	0.0193	0.817	0.189	0.491	0.135
N	127,616	127,616	127,616	127,616	127,616	127,616
Panel B. Age						
Young	0.268	-0.0101	0.569	0.0117	-0.00785	-0.232***
	(0.305)	(0.412)	(0.485)	(0.0575)	(0.0644)	(0.0854)
Old	-0.505**	0.114	0.00111	0.0272	-0.122**	-0.0728
	(0.241)	(0.252)	(0.321)	(0.0469)	(0.0511)	(0.0606)
P-value difference	0.0466	0.797	0.328	0.834	0.164	0.127
N	127,616	127,616	127,616	127,616	127,616	127,616
Panel C. Working schedule						
Part-time	0.193	0.402	-1.064	-0.00683	-0.114	-0.0643
	(0.382)	(0.444)	(1.055)	(0.0681)	(0.0777)	(0.0991)
Full-time	-0.315	-0.0489	0.255	0.0184	-0.0723	-0.150***
	(0.208)	(0.244)	(0.277)	(0.0423)	(0.0463)	(0.0560)
P-value difference	0.0466	0.797	0.328	0.752	0.646	0.443
N	127,616	127,616	127,616	127,616	127,616	127,616
Panel D. Gender						
Women	0.0507	-0.0449	-0.126	0.0320	-0.0445	-0.155**
	(0.290)	(0.347)	(0.466)	(0.0481)	(0.0533)	(0.0650)
Men	-0.337	0.127	0.282	-0.0145	-0.129**	-0.102
	(0.232)	(0.272)	(0.324)	(0.0545)	(0.0605)	(0.0759)
P-value difference	0.0466	0.797	0.328	0.752	0.646	0.443
N	127,616	127,616	127,616	127,616	127,616	127,616

Source: : UK Labor Force Survey 2009-2015

Note : The table reports the impact of the reform by subgroup. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroups considered. Otherwise, all regressions include the standard controls. The estimation sample comprises individuals working more than 16 hours per week, with a permanent contract and known tenure. Standard errors are clustered at the individual level and are presented in parentheses. $*$, $**$ and $***$ denote significance at the 10, 5, and 1% level, respectively.

Conclusion

Les travaux présentés dans cette thèse s'intéressent aux rendements individuels de l'éducation et de la formation et visent à identifier les modalités d'intervention publique susceptibles d'accroître ces rendements. Les deux premières analyses s'attachent à identifier de potentiels leviers d'efficacité des systèmes éducatifs et de formation. La troisième étude met en lumière les liens entre l'investissement en capital humain et l'environnement légal ou économique qui l'encadre.

Chapitre 1

Dans le premier chapitre de cette thèse, nous évaluons l'effet de la scolarisation dans une école privée en CP et CE1 sur les résultats scolaires en CE2. L'écart de distance au domicile des parents entre les écoles privée et publique les plus proches est utilisé comme variable instrumentale afin de prendre en compte l'endogénéité du choix de scolarisation dans une école privée. Nous montrons qu'une fois le biais de sélection pris en compte, les élèves scolarisés dans une école primaire privée n'obtiennent pas de meilleurs résultats par rapport aux élèves scolarisés dans une école publique, et n'ont pas une probabilité plus élevée de redoubler. Il faut toutefois souligner la durée restreinte de scolarisation que nous considérons : en général l'impact de la scolarisation dans le privé est mesuré à la fin d'un cycle entier, dans le premier degré (Reardon et al. 2005 ; Elder et Jepsen 2014) ou au lycée (Joseph G. Altonji et al. 2005 ; Cohen-Zada et Elder 2009). Vandenberghe et S. Robin (2004) adoptent une approche différente en comparant les performances scolaires des élèves de 15 ans scolarisés dans le privé à ceux scolarisés dans le public. Si une fenêtre plus restreinte de deux ans risque de ne pas être suffisante pour mesurer un effet significatif de la scolarisation dans le privé, cette démarche conserve néanmoins l'intérêt de considérer un échantillon relativement homogène.

Cette étude répond à deux questions distinctes : d'une part nous souhaitons évaluer dans quelle mesure le choix de la scolarisation dans le privé durant les deux premières années du primaire est pertinent pour des parents qui souhaiteraient inscrire leur enfant dans un établissement différent de l'école publique de secteur. L'absence d'effet significatif de la scolarisation dans le privé sur les résultats scolaires nous conduit à affirmer qu'un tel choix ne semble pas fondé s'il est uniquement motivé par le souhait de mieux réussir dans une école privée. En revanche le choix du privé peut répondre à des aspirations autres qu'académiques : l'environnement proposé par le privé est par exemple susceptible d'influencer les compétences non-cognitives des élèves, telles que la concentration, la motivation ou les compétences sociales. Cette hypothèse est en partie étayée par le biais de sélection que nous observons : à l'entrée en CP, les élèves ayant de bons résultats scolaires mais présentant des difficultés à suivre de nouveaux apprentissages sont plus souvent inscrits dans une école privée [28]. Il est possible que certains parents inscrivent dans le privé un enfant dont

28. Une régression naïve conduirait à évaluer un effet négatif du privé en CE2 pour les élèves ayant les

la marge de progression risque d'être réduite du fait de problèmes de comportement ou de concentration, en comptant sur un suivi plus individualisé des élèves dans une école privée. Evaluer l'impact de la scolarisation dans le privé vise d'autre part à évaluer dans quelle mesure la combinaison des caractéristiques institutionnelles et organisationnelles des écoles privées permet d'améliorer les performances scolaires des élèves. L'absence d'effet significatif de la scolarisation dans le privé sur les résultats scolaires suggère que l'organisation des écoles privées ne diffère pas radicalement de celle des écoles publiques, ou à défaut la différence est trop marginale pour avoir un effet significatif. Les établissements privés sous contrat sont en effet asujettis à des obligations de volumes horaires, programmes scolaires ou de qualifications requises des professeurs proches de celles du secteur public. A l'inverse, les écoles privées hors contrat axées sur des méthodes pédagogiques spécifiques se démarquent *a priori* plus nettement des méthodes utilisées dans les établissements publics. A l'instar des *charter schools* aux Etats-Unis, concentrer les ressources des établissements sur certains principes permettrait d'élaborer un modèle homogène se distinguant du système éducatif public.

Chapitre 2

Le deuxième travail de cette thèse s'attache à mesurer l'efficacité des formations destinées aux demandeurs d'emploi sur leur retour à l'emploi, en particulier les programmes de formation préparant à l'obtention d'une certification. Nous estimons les taux de hasard des trois risques concurrents auxquels un demandeur d'emploi fait face, i.e trouver un emploi, entrer en formation certifiante ou non-certifiante. La méthode du *timing-of-events* nous permet de prendre en compte le caractère endogène de la participation à ces différents types de formation. Nous montrons que les formations préparant à un diplôme délivré par un Ministère réduisent de façon significative la durée de chômage une fois la formation terminée. Ce résultat fait écho à l'effet plus connu du diplôme lorsqu'il est acquis en formation initiale, dont on sait qu'il protège du chômage [29]. En revanche, les programmes préparant aux autres certifications ne sont pas plus efficaces que les formations non-certifiantes. Des données distinguant les certifications inscrites au Répertoire National de Certifications Professionnelles (RNCP) nous permettraient de tirer des conclusions plus précises concernant de potentiels effets de signaux. D'autre part nous n'étudions pas les effets d'équilibre général de ces programmes sur le marché du travail. L'impact positif des formations considérées est mesuré en faisant l'hypothèse que la participation de certains individus à ces

meilleurs résultats en CP initialement, tandis que la prise en compte de caractéristiques non-observées conduit à mesurer un effet nul.

29. Le Rhun et Pollet (2011) montrent qu'en 2010, le taux de chômage des jeunes actifs sortis du système éducatif depuis moins de 5 ans était de 44 % parmi les non-diplômés, ou diplômés du brevet des collège. L'effet du diplôme perdure dans le temps : après 10 ans de vie professionnelle, les peu diplômés observent un taux de chômage de 10 % contre 6 % pour les individus diplômés.

programmes n'influence pas la durée de chômage des autres demandeurs d'emploi [30], or plusieurs travaux montrent l'existence d'effets de déplacement des politiques actives de retour en emploi (Cahuc et al. 2008 ; Albrecht et al. 2009 ; Crépon, Duflo et al. 2013 ; Ferracci et al. 2014). Etendre les formations les plus efficaces à l'ensemble des demandeurs d'emploi ne serait probablement pas aussi efficace, cependant des mécanismes d'efficacité peuvent être identifiés afin d'améliorer les programmes de formation existants.

Les possibles mécanismes d'efficacité des formations certifiantes ont des implications différentes en termes de politiques publiques. Il est probable que l'efficacité de ces programmes soit en partie due au signal associé aux diplômes délivrées par des Ministères. Dans ce cas les certifications sont attachées à des organisations reconnues dans la formation initiale, ce qui renvoie *a priori* un signal fort aux employeurs. Si l'effet de signal explique en grande partie la relation causale entre la participation à ces formations et le retour en emploi, accroître la lisibilité des certifications préparées et acquises par le biais de la formation continue peut permettre aux employeurs d'identifier plus facilement les individus disposant des compétences nécessaires. En identifiant les organisations délivrant des diplômes "de qualité", la Commission Nationale de la Certification Professionnelle (CNCP [31]) peut également contribuer à renforcer la confiance des employeurs envers le système de certification.

Il est cependant probable qu'un tel impact positif s'explique également par le contenu des formations elles-mêmes. Si tel est le cas, appliquer les mécanismes d'efficacité de ces programmes à d'autres formations, certifiantes ou non, permettrait de rationnaliser l'efficacité du système de formation continue des demandeurs d'emploi. Tout d'abord, obtenir un diplôme du Ministère de l'Education Nationale, qui délivre la majorité des diplômes délivrés par un Ministère, nécessite de valider des compétences spécifiques propres au domaine professionnel, mais également des savoirs de base tels que la maîtrise de l'écrit [32]. Les titres du Ministère du Travail portent quant à eux sur des connaissances techniques et des savoir-faire professionnels, dont la connaissance est validée par des épreuves de mise en situation de travail. D'autre part les formations préparant à un diplôme délivré par un Ministère demeurent attachées à un domaine spécifique ; on peut penser qu'à l'inverse certaines certifications plus générales ne sont pas suffisamment liées à un secteur identifié pour retrouver rapidement un emploi. L'ancrage de ces certifications dans un domaine spécifique

30. De façon plus formelle, la "Stable Unit Treatment Value Assumption" (SUTVA) induit que la réponse d'un individu dépend uniquement du traitement qui lui est assigné, et non du traitement assigné aux individus dans son environnement.

31. La CNCP instruit les demandes d'enregistrement des certifications professionnelles. Ce sont les compétences et connaissances délivrées qui sont examinées, la demande de travail ainsi que la fiabilité du processus mis en œuvre pour cette qualification.

32. Rose (2007b) souligne ainsi que les diplômes de l'Education nationale sont élaborés dans la perspective de continuer à progresser dans la maîtrise de savoirs permettant le développement personnel et l'insertion sociale des individus.

ouvre d'ailleurs plus facilement la voie à de la formation en entreprise, ce qui peut non seulement compléter la formation délivrée en classe mais également engendrer des effets de réseau non-négligeables. Enfin, les moyens déployés dans les organismes de formation préparant à ces diplômes expliquent probablement en partie la qualité des programmes de formation. En effet, sur le marché de la formation le niveau requis des formateurs ou les moyens pédagogiques mis en œuvre varient d'un prestataire à l'autre sans qu'un référentiel de standards qualité soit uniformément appliqué [33]. En revanche les organismes de formation préparant à certains diplômes affichent des exigences plus marquées en termes de moyens mis en œuvre. Le Ministère de l'Education Nationale a par exemple développé le label "GretaPlus", qui assure que la formation mise en œuvre dans les Groupements d'établissement (GRETA) labellisés est "sur mesure [34]". La récente création d'une liste de labels reconnus va dans le sens d'une rationalisation de l'offre de formation [35].

Identifier les formations de qualité constitue non seulement un enjeu pour les politiques publiques visant les demandeurs d'emploi mais plus globalement pour la sécurisation des parcours professionnels. Le Compte Personnel de Formation (CPF) récemment crée, qui vise à renforcer l'accès à la formation continue des individus indépendemment du statut occupé, n'est mobilisable que pour certains types de formations, notamment celles permettant l'accès à la qualification des personnes en recherche d'emploi. On peut penser que les critères d'éligibilité retenus pour figurer sur la liste de formations proposées devraient distinguer les différents types de certifications et leur implications en termes de signal sur le marché du travail. Les moyens mis en œuvre dans les programmes de formation devraient également être considérés pour sélectionner les formations financées par le CPF. D'autre part, dans la mesure où les formations certifiantes ministérielles sont plus longues qu'en moyenne, une réflexion concernant l'abondement d'un tel dispositif doit être engagée afin qu'il permette aux individus un temps de formation suffisamment long, et d'une qualité reconnue.

33. Le décret du 30 juin 2015, qui fait suite à la loi du 5 mars 2014, s'inscrit ainsi cependant dans la volonté d'améliorer la qualité de l'offre de formation disponible. Il confie notamment aux organismes financeurs de formation une mission de contrôle de "la capacité du prestataire de formation (...) à dispenser une formation de qualité ", en s'appuyant sur sept critères préalablement définis, portant par exemple sur les moyens pédagogiques ou la qualification des professionnels.

34. Pour obtenir le label, le GRETA dépose sa candidature auprès des services de l'Education Nationale, avant d'être audité sur site. Un comité national de labellisation décide de l'attribution du label. Cette démarche est ouverte aux partenaires des GRETA.

35. Suite à la loi de mars 2014, le Conseil National de l'Emploi, de la Formation et de l'Orientation Professionnelle (CNEFOP) a identifié les labels qui permettaient d'identifier les prestataires de formation dispensant des formations de qualité.

Chapitre 3

La troisième étude montre le lien existant entre la flexibilisation du marché du travail et l'accès à la formation des travailleurs. Nous étudions l'effet de la réforme anglaise de 2012 qui a allongé de un à deux ans la période d'essai, et a de fait diminué les coûts de licenciement des individus dont l'ancienneté dans l'entreprise est supérieure à un an. La méthode des différences de différences nous conduit à comparer les fonctions de hasard de licenciement et de participation à une formation des individus embauchés avant et après la réforme. Dans un second temps, nous estimons un modèle bivarié à risques concurrents pour prendre en compte le fait que les individus participant à une formation sont un échantillon sélectionné, dans la mesure où ils n'ont pas quitté l'entreprise. Nous soulignons ainsi qu'accroître la durée de période d'essai d'un contrat n'est pas neutre pour l'accès à la formation des salariés d'une entreprise. En effet, la réforme accroît le taux de licenciement des travailleurs dont l'ancienneté dans l'entreprise est supérieure à un an, tandis que le taux de création d'emplois semble augmenter. La hausse induite de la mobilité diminue la durée moyenne travaillée par un employé dans une entreprise, ce qui influence à la baisse le taux d'accès à la formation des travailleurs. Il faut cependant souligner que nos données nous permettent d'observer un effet durant un peu moins de 4 ans. Un recul temporel plus important nous permettrait d'évaluer l'impact de la réforme sur une plus longue période.

La réforme que nous étudions s'inscrit dans un ensemble de mesures visant à augmenter la flexibilité du marché du travail, et *in fine* à diminuer le taux de chômage. Notre étude ne nous permet pas de mesurer l'efficacité de la réforme sur ce plan [36], mais permet de mettre en lumière son effet potentiel sur l'accès à la formation des salariés. Les conclusions que nous tirons concernent donc les mécanismes à mettre en œuvre pour limiter l'effet négatif d'une telle réforme sur l'accès à la formation ; elles ne concernent pas le bien-fondé d'une politique de flexibilisation du marché du travail concernant le taux de chômage ou les flux de travailleurs.

Pour compenser la différence d'investissement en formation par rapport à un marché du travail moins flexible mais favorisant l'accès à la formation, les pouvoirs publics ont la possibilité de subventionner la formation dans les entreprises ou les secteurs où les flux d'emplois sont les plus élevés. Une autre solution est de rendre moins dépendant l'accès à la formation des individus de leur statut d'employé. Si les individus ont l'initiative d'investir dans leur capital humain, ils seront moins influencés par une modification de leur environnement de travail. Par exemple, la création en France d'un Compte Personnel de Formation (CPF) vise à procurer un droit individuel à la formation continue, quel que soit

36. De nombreux exemples montrent par ailleurs que mesurer l'effet d'une politique flexibilisant le marché du travail n'est pas trivial. La plupart des études empiriques montre un effet négatif d'une politique du travail peu flexible sur les flux d'emplois et de travailleurs (Boeri et Jimeno 2005 ; A. Kugler et Pica 2008 ; Bassanini et Garnero 2013), mais le consensus est moins clair concernant son impact sur le niveau de chômage (Lazear 1990 ; Autor, Donohue III et al. 2006 ; Behaghel et al. 2008).

le statut occupé. En revanche, un tel mécanisme revêt de nouveaux enjeux : l'accompagnement des individus dans cette démarche et l'accès à la information doivent permettre aux individus de réaliser ces choix d'investissements quel que soit leur environnement social.

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Résumé. Trois essais empiriques en économie de l'éducation et de la formation. Les travaux présentés dans cette thèse s'intéressent aux rendements individuels de l'éducation et de la formation, et cherchent à mettre en évidence des modalités d'intervention publique plus efficaces pour accroître ces rendements. Les deux premiers chapitres de ce travail étudient la manière dont les rendements des investissements individuels en éducation peuvent être optimisés en améliorant l'efficacité des systèmes éducatifs et de formation professionnelle. La dernière étude porte sur les liens entre l'investissement en capital humain et l'environnement légal ou économique qui l'encadre. Le premier article de cette thèse s'intéresse en particulier à l'effet de la scolarisation dans une école privée française en CP et CE1 sur les résultats scolaires en CE2. Le second chapitre vise à mesurer l'efficacité des formations certifiantes destinées aux demandeurs d'emploi français sur leur retour en emploi. La dernière étude montre le lien existant entre la flexibilisation du marché du travail anglais et l'accès à la formation des salariés. Les analyses réalisées reposent sur des méthodes microéconométriques qui visent à identifier l'effet causal des politiques publiques étudiées. Nous utilisons ainsi la méthode des variables instrumentales et celle des différences de différences. Cette thèse repose également sur l'estimation de modèles de durée, en utilisant la méthode du *timing-of-events* ou en estimant un modèle à risques concurrents bivarié.
Descripteurs : Rendements de l'éducation, Evaluation des politiques publiques, Formation professionnelle, Politique du marché du travail, Modèle de durée, Législation de protection de l'emploi.

Abstract. Three empirical essays on the economics of education and training. This thesis work focuses on individual returns to education and training, and aims at highlighting more efficient public interventions in order to increase these returns. The first two chapters of this study consider how returns to individual investments can be optimized by improving public policies efficiency in the field of education and vocational training. The last study analyzes the link between investment in human capital and the legal and economic environment in which it is realized. In details, the first article of this thesis focuses on the effect of private schooling during the first and second grade in France on test scores achievement in third grade. The second chapter aims at measuring the efficiency of certifying training programs followed by unemployed individuals on their probability to find a job. The last study shows the link between the English labor market flexibility and workers' access to training. The analyses conducted rely on microeconometric methodologies which aim at identifying the causal effect of considered public policies. We thus use the instrumental variable methodology as well as the difference-in-difference one. This thesis also relies on the estimation of duration models, using the "timing-of-events" methodology, and estimating a bivariate competing risks model.
Keywords: Returns to education, Evaluation of public policy, Vocational training, Labor market policy, Duration model, Legislation on employment protection.