Essays on Labor Economics and Public Finance

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Declaration

I certify that this thesis is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

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Antoine Goujard.

Abstract

Public policies are an important determinant of the welfare of individuals and the society at large. Careful evaluation of the impact of public policies on welfare is therefore imperative for our understanding of the positive and normative implications for these institutions. The three chapters of this thesis examine the welfare consequences of specific economic and political institutions.

Chapters 1 and 2 study two distinct channels through which social housing, a common feature of developed countries, may impact the neighborhoods in which they are built and the labor market outcomes of their low income tenants. Chapter 1 is concerned with the effect of the provision of social housing on neighboring private flats. It assesses the spillovers of low-income tenants and the change in the composition of the housing stock that are to be expected from the provision of new social housing units. In particular, it uses the direct conversion of private rental flats into social units without any accompanying rehabilitation to identify the impact of the inflow into the neighborhood of low income tenants, separately from the effects of social housing on the quality of the existing housing stock.

Chapter 2 shows that social housing influences the location of low income tenants, and that the neighborhood of social housing units may improve the labor market outcomes of the poorest tenants. I observe the relocation of welfare recipients through the selection process of social housing applicants in the city of Paris from 2001 to 2007. The institutional process acts as a conditional randomization device across residential areas in Paris. The empirical estimates outline that neighborhoods have weak short- and medium-run effects on the economic self-suffiency of poor households.

Chapter 3, by contrast, focuses on how regional migrations of unemployed workers may affect their job search prospect in Europe. Using a longitudinal sample of French unemployment spells, the empirical estimates outline positive migration effects on transitions from unemployment to employment that depends on the previous duration of the unemployment spells.

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Preface

Economic and political institutions are of fundamental importance for the formation of public policies. Careful evaluation of the impact of public policies on welfare is therefore imperative for our understanding of the positive and normative implications for these institutions. Each of the three chapters of this thesis examines the welfare consequences of specific economic and political institutions.

In a broad sense, my research contributes to the related fields of Public Finance (Chapter 1 and 2) and Labor Economics (Chapter 3). Chapters 1 and 2 study two distinct channels through which social housing, a common feature of developed countries, may impact the neighborhoods in which they are built and the labor market outcomes of their low income tenants. Chapter 1 is concerned with the effect of the provision of social housing on neighboring private flats. It assesses the spillovers of low-income tenants and the change in the composition of the housing stock that are to be expected from the provision of new social housing units. While Chapter 1 argues that social housing units induce spillovers on neighboring private flats by shifting the social composition of neighborhoods, Chapter 2 shows that social housing influences the location of low income tenants and that the neighborhood of social housing units may improve the labor market outcomes of the poorest tenants. Chapter 3, by contrast, focuses on how regional migrations of unemployed workers may affect their job search prospect.

Chapter 1 investigates the impact of social housing on the sales price of neighboring flats in Paris. I construct a unique dataset including flat sales and social housing projects at the building level. To account for endogenous placement of social housing projects, I use a difference-in-differences strategy that includes fine geographical controls and trending unobservables. In my preferred specifications which control for building fixed effects, a particular spatial pattern emerges: a 10 percentage points increase in the social housing share implies a 1.2% increase in housing value within a radius of 50 meters. However, private properties located farther away from the social projects within a 350 to 500 meter belt experience price decrease by 5.5%. The positive effects appear more important for small dwellings and for properties located in poor neighborhoods while negative impacts dominate in high income neighborhoods and for family dwellings. Further estimates exploit the unexpected win of a left-wing mayor in Paris, which was followed by a sharp increase in social housing units driven by the direct conversion of private rental flats into social units without any accompanying rehabilitation. This natural experiment allows to identify the impact of the inflow into the neighborhood of low income tenants, separately from the effects of social housing on the quality of the existing housing stock. I do not find evidence of a positive impact of the conversion projects on housing prices.

Chapter 2 investigates the effects of neighborhood on the labor market outcomes of poor households. I construct a longitudinal data set from the administrative records of welfare recipients in the city of Paris from 2001 to 2007. I observe the relocation of welfare recipients through the selection process of social housing applicants. The institutional process acts as a conditional randomization device across residential areas in Paris. I measure the impact of location characteristics on future labor market outcomes. I find that -(i) successful applicants tend to relocate in the vicinity of their initial neighborhoods; -(ii) the quality of neighborhood matters for the job finding rate of poor households; -(iii) such effect is stronger for households with children and single women; -(iv) most of the positive effect is driven by unstable jobs that do not allow the individuals to exit the welfare program. These estimates outline that neighborhoods have weak short- and medium-run effects on the economic self-sufficiency of poor households.

Chapter 3 investigates the effect of residential mobility on unemployment using a longitudinal sample of French unemployment spells. I evaluate the impact of migration on unemployment duration at the individual level. Using matching estimators and repeated unemployment spells, the empirical results suggest the existence of significant positive effects on transitions from unemployment to employment, which can be predicted by job search theory. These effects depend on both previously elapsed unemployment duration and the time spent in the new region of residence.

Chapter 1

The externalities from social housing Evidence from housing prices

1 Introduction

Neighborhood effects and externalities are key issues in the social sciences and in the design of social policy. A large existing literature investigates the causes and impacts of neighborhood and peer effects in a range of scenarios such as education, labor markets, health and crime[1]. Social housing is an important and growing component of social policy. Various countries have seen an increasing Government involvement in this area at least partly motivated by the intention to create or maintain mixed neighborhoods (see Currie, 2006, for the USA, Cheshire et al., 2008, for the UK and Laferrère and le Blanc, 2006, for France). However, there is little evidence on the impacts of low-income housing developments on the neighborhoods in which they are built.

While economists' knowledge of the effects of social housing in local neighborhoods is still relatively thin (recent exceptions include Baum-Snow and Marion, 2009, and Schwartz et al., 2006), assessing such effects is crucial to compare the benefits of social housing for low-income tenants to the costs (if any) of creating and maintaining mixed neighborhoods. The overall effect of social housing on nearby private housing is potentially ambiguous. On the one hand, by bringing in an inflow of relatively low-income residents, social housing affects the socio-economic mix of a neighborhood and may lower the value of the neighborhood to existing residents. On the other hand, project-based assistance that complements social housing projects may provide an offset to the above effects, and more generally to urban decay. Rosen (1985) argues that social housing units may be justified to replace distressed properties in low-income neighborhoods where social units may be better maintained than private rental units. Thus the effect of social housing concentration on local housing prices is ultimately an empirical question.

This chapter estimates the impact of social housing on the private housing market, using information on new housing developments and property sales at the building level for the city

[1]See among recent examples: Oreopoulos, 2003, Kling et al., 2007, Currie et al., 2010, Linden and Rockoff, 2008, the review of Oreopoulos, 2008, and references therein.

of Paris between 1995 and 2005. I ask how proximity to social housing units affect the housing prices of nearby private flats and what are the underlying mechanisms. Paris provides a compelling setting to study the externalities of social housing for three main reasons. First, recent social housing policies in 2001 lead to a rapid expansion of the social dwelling stock with 18 thousands social units, provided between 2000 and 2005. Social units accounted for 23.8% of the occupied rental housing stock at the end 1995 and nearly 27.3% at the end of 2005. Second, Paris is by far the most densely populated city in Europe, and as a result new social housing units potentially affect a large number of private sales. I will be able to exploit the underlying variation using information on private sales at the building level. Finally, by comparing sales affected by new constructions, rehabilitation of existing housing developments, or conversion of private housing, I can obtain a precise picture of the mechanisms driving the externalities stemming from social housing developments.

To analyze the effects of new social housing projects on neighboring private flats, I exploit two complementary research designs. The first identification strategy builds on a difference-in-differences specification. An important contribution here is the introduction of a rich set of local controls. Both developers and housing authorities have some control on the location of new social units and it is therefore important to control for unobserved determinants of project location. In my difference-in-differences estimates, I can control for local unobservables down to the building level. Using the share of social housing within different neighborhoods as an explanatory variable, I examine whether private flats located near social housing projects experience different price changes once the social housing projects are created.

My difference-in-differences estimation strategy delivers two main results. First, without fine local controls the estimated impacts of social housing on housing prices is mainly negative. This mostly stems from the endogenous location of social housing in declining or deprived parts of small neighborhoods. When building fixed effects and local linear trends are included, the private housing stock located within 50 meters of the social housing projects experience positive price growth. Specifically, a new social housing project of typical size (35 units) raises local housing prices by around 2.6% and a 10 percentage points increase of the social housing share raises housing prices by about 1.2%. The timing of these effects is consistent with a causal impact and the estimates are robust to the inclusion of local linear trends. This result challenges the belief that the potential inflows of low-income tenants could offset the benefits of the rehabilitations and new constructions associated with social housing projects. Second, the impacts of social housing projects appear either close to zero or negative for private flats located farther away from the projects. For neighborhoods located 350 to 500 meters away from the social projects, a 10 percentage points increase in the social housing share (corresponding to about one standard-deviation change) would imply a 5.5% decrease in housing values. These average effects are the result of important heterogeneity with respect to neighborhood's characteristics and dwelling size. The positive impacts measured within 50 meter of the projects are driven by small flats in low-income neighborhoods, while the negative externalities measured within the outer belt from 350 to 500 meters are mainly driven by family dwellings and high income neighborhoods.

To investigate the mechanisms driving these externalities, I exploit the election of the current mayor, Bertrand Delanoë, in March 2001. The Delanoë administration marked a sharp increase in the number of social units and a change in their usual channel of provision from new constructions and rehabilitations of distressed private properties towards the conversion of private rental properties into social units. As these direct conversions (*acquisition sans travaux*) do not involve new buildings or rehabilitations, they allow me to identify the effect of the inflow into the neighborhood of low income tenants, separately from the effect of social housing on the quality of the existing housing stock. This policy experiment points towards zero effects of low-income tenants inflows.

This chapter builds on the research assessing externalities of housing policies in the private housing market. A first stream of this literature is based on difference-in-differences estimation strategies controlling for census tract or block unobservables. Schwartz et al. (2006) investigate the effects of subsidized housing projects in New York between 1987 and 2000. Using a difference-in-differences hedonic regression at the census tract level, they define the houses located within 600 meters of a project as treated. They find that both rental and owner occupied subsidized housing projects tend to have large positive externalities, mainly due to the construction of new buildings and the removals of disamenities in distressed neighborhoods. Santiago et al. (2001) find similar results for the dispersed housing subsidy program in Denver which led to an increase in small scale rental projects over the period 1987 to 1997. Autor et al. (2009) analyze the effects of the elimination of rent control in Cambridge (USA) during 1995-1997 and document negative externalities of rent controlled properties on neighboring houses, having controlled for detailed property characteristics. Hartley (2008) finds that the timing of closures and demolitions of high rise public housing buildings in Chicago is associated with an increase in housing prices in the vicinity of the past projects, consistent with the removal of disamenities.

Baum-Snow and Marion (2009) tackle more directly the issue of the endogenous location of the new social housing projects. They exploit a discontinuity in the formula for the eligibility for Low Income Housing Tax Credit (LIHTC) subsidies, which creates quasi random variations in the number of new buildings between census tracts. Their regression discontinuity design shows that additional new projects and LIHTC tenants stimulate home-ownership turn-over, housing prices in declining areas and lower median income in poor gentrifying areas.

My identification strategy differs from both the usual difference-in-differences strategies and Baum-Snow and Marion (2009) in three important dimensions that are likely to explain the difference in my findings. First, most papers have used aggregate census data at the tract level[2], while my data gives me the exact location of each sale and each new social housing unit. This spatial richness allows me to get a more detailed picture of spatial spillovers and to control for building unobservables. This is important when the effects considered are extremely localized

[2] For example, Baum-Snow and Marion (2009) use US census data at the tract level and define neighborhood as a 1-km circle around the census tract's center. Chay and Greenstone (2005) and Greenstone and Gallagher (2008) use comparable data.

and if the location of new projects is endogenous within census tracts. Second, the regression discontinuity design adopted by Baum-Snow and Marion (2009) focuses their analysis on the impacts of social housing in poor neighborhoods, while Paris is one the wealthiest city in Europe, the median pre-tax household income ranging from $13,985$ euros in the poorest census tract to $61,783$ euros in the highest in 2001. This allows me to uncover heterogeneous effects of social housing on housing prices. Third, most of the point estimates provided by the existing literature reflect the combined impact of the revitalization effects of new housing projects and the inflows of low-income tenants into a neighborhood. The Parisian set-up allows me to distinguish between the impact of new social housing created via new constructions and rehabilitations of existing dwellings and that of straight conversions of private rental units into social housing and therefore more closely isolate the impact of additional poor households on neighborhoods.

The chapter is organized as follows. The next section discusses the features of Parisian social housing that are relevant for my analysis, describes data construction and some summary statistics. Section 3 describes my identification strategies. Section 4 describes my main empirical findings on the externalities of social housing on private housing prices. Section 5 investigates further the mechanisms driving these externalities. Section 6 concludes.

2 Institutional background and summary statistics

2.1 Institutional background

The Parisian social housing system is based on rental units subsidized by low interests loans and tax deductions. Housing units are owned by private local companies, *HLM*[3]. Despite their private status, these companies are closely monitored by the central government and the municipality, that sometimes contribute to rehabilitation, maintenance or demolition of buildings. Moreover, in Paris, the municipality is the main joint owner of the largest *HLM* companies.

Project-based assistance is used by *HLM* companies to create new social units either through subsidized construction, rehabilitation or conversion of private buildings[4]. Once a social housing unit is created, it remains in the social sector forever[5]. Figure 1.1 breaks down the number of new units created in Paris from 1970 by year of completion and type of creation. The timing and types of the new units match closely the city mayoral elections that took place every six years from 1977 to 2001 and in 2008. The overall production of social dwellings is lower after the change of mayor in 1995 and increases significantly after the first election of a left-wing mayor in 2001. Until 2001, the main method to create new social housing units was new buildings. During the 1980s the rehabilitations of existing distressed properties increased significantly. At the same time, figure 1.1 reveals a sharp decline in the number of social units created through

[3] *Habitations à loyer modéré.*

[4] *HLM* companies were allowed to buy a minority of dwellings of private new projects before their completions (*VEFA*) by the *décret* 2001-104 (08/02/2000).

[5] The French government created incentives for *HLM* companies to sell social units to their low-income tenants. The main *HLM* companies in Paris do not apply this policy.

new buildings during the 1990s, from an average of approximately 2,700 annual new dwellings at the end of the 1980s, to an average of 900 new dwellings between 2000 and 2010. The purchase of 20 year old buildings without any rehabilitation was only authorized by a change in law in 2001[6]. From 2001, rehabilitation of existing properties and conversions of private rental flats were the main methods used to increase the stock of social dwellings.

The French Government has designed several incentives for each municipality to develop a comparable stock of social dwellings. A law adopted in 2000 imposes a minimum share of 20% of social housing units among the occupied housing stock in each municipality and therefore Paris, with a social housing share of 13.1% in 2001, is directly concerned[7]. However, the spatial distribution of social housing inside Paris is a joint decision of the *HLM* developers and the municipality. The municipality intervenes through the selling of public land and buildings to developers, the authorization of new buildings and the design of the subsidies that add non-trivial monetary and non-monetary incentives to the location of new social units. The main objective since 2001 has been to reach a better spatial distribution of the social housing stock. The municipality decided to apply the 20% limit to all the *arrondissements* in Paris and created an inclusionary zoning which stipulates that any large private project located in central Paris should incorporate at least 25% of social dwellings. Figure 1.2 plots the location of the new units over time. Small dots represent social housing projects created before 2001 and larger dots the projects created after 2001. The conversion projects created after 2001 are represented by large squares. The underlying map presents the median housing price (per square meter) in 1995. Overall a negative correlation appears between the number of projects and housing prices. Interestingly, recent projects are spread throughout the city while older social housing units are located in fewer neighborhoods. The unequal distribution of the variation in the social housing share across the city would pose problems in the presence of localized shocks (e.g. renewal programs, industrial clean-ups etc.). The widespread distribution of the new social housing units mitigate the influence of these local shocks.

The expected impacts of social units on surrounding properties depend crucially on the characteristics of the social dwellings. Each dwelling is subject to some level of rent control according to the subsidies used to finance the project. *HLM* companies have a restricted choice over the eligible tenants who are determined mainly through income, number of children and previous housing (Laferrère and le Blanc, 2006). As a priority is given to households in financial difficulties, the income of the successful applicants appears far below the maximum income levels. Allocation of the two main types of social housing published by the municipality in 2005 shows that the income of the new tenants was below 60% of the usual income threshold in 90% of the cases (APUR, 2006). Hence, new social tenants are typically below the 20th percentile of income by consumption unit[8].

Table 1.1 summarizes the characteristics of the *HLM* dwellings and tenants with respect

[6] *Décret* 2001-336 (18/04/2001) for the financing of *PLUS* dwellings and Préfecture de Paris (2004). The purchase of existing buildings for *PLA-I* was authorized earlier by the *Décret* 1990-151 (16/02/1990).

[7] Recently, Glaeser and Gottlieb (2008) advocate the use of subsidized housing in the USA to increase the supply of affordable housing in highly productive areas.

[8] Eurostat consumption unit. There are large variation of eligibility level by household size.

to the private accommodation sector according to the French Housing Survey in 2002. The first column shows the characteristics of the stock of social housing dwellings, the second shows the characteristics of the social dwellings with new tenants[9] while the last two columns show the characteristics of private rental dwellings and owner occupied dwellings. Panel A provides information about the structural characteristics of the units. Social dwellings are located in larger and more recent buildings: 19% were built after 1982 against 7% in the private rental sector or 4% for owner occupied units. They are also larger than private rental units by around 25% or 0.6 rooms and located in larger buildings. The average rent by square meters in the social sector is less than half the rent in the private sector. As a result of this rent difference and the scarcity of the social offer, the duration of tenancy in the social sector is greater than in the private sector by 5 years.

Panel B of table 1.1 displays the main characteristics of the social tenants. The income by consumption unit of the social tenants is one third, approximately one standard-deviation, below the corresponding average in the private rental sector. This lower level of income is related to larger shares of non qualified, unemployed and inactive individuals. Social tenants are also older and less likely to be born in France than households in private accommodation. The shares of families and single parents are also significantly higher.

Finally, panel C of table 1.1 reports the opinion of the households on the neighborhood and maintenance of social dwellings. Flooding appears less of a concern in the social sector as the buildings are more recent. However, 38% social tenants report that the building has been degraded last year while this number is only of 18% in the private sector. The number of households that declares being victims of robberies (or attempts) is also substantially higher than in the private sector. While the average social tenant thinks that his neighborhood is less safe than the average private tenant, new social housing tenants have a more positive view of the neighborhoods of their social units.

Due to the difference in households' income and the characteristics of the social buildings, investments in low-income housing could have different externalities according to both construction types, level of income of the tenants and initial neighborhoods. Depending on the projects, the main spillover effects could be through the low-income tenants living in social housing, the upgrade of existing buildings or through complementary investments. For example, social housing units are often created through urban renewal operations and associated with new public facilities such as new roads, additional playgrounds or schools' investments.

2.2 Data and summary statistics

The definition of social housing adopted in this article is restrictive, it closely follows the French law of 2000 (*SRU*). Social units belong to an *HLM* landlord and receive an agreement from the state which give rights to construction and rent subsidies in exchange for some level

[9] I define as new tenants the households who moved in during the last four years.

of control on the rents and the choices of the tenants[10]. The only exception are the dwellings which belong to the *HLM* companies since 1977 or before. As formal rental agreement (*conventionement*) did not exist before 1977, all these *HLM* rental dwellings are considered as social housing. Furthermore I restrict the sample of projects to the family dwellings excluding the few students' residences, collective accommodation for the elderly and temporary accommodation for the homeless[11]. These restrictions are motivated by the fact that these social housing units represent a very small fraction of the inflows and are not considered as social housing in the available surveys or in the existing literature.

The public housing stock and its evolution is constructed from seven yearly exhaustive surveys completed by the regional planning agency (*DREIF*). These surveys are mandatory and were carried out in 1998, 2002, 2003, 2004, 2005, 2006 and 2007. Each year the planning agency asks the *HLM* landlords to update a description of their social housing dwellings. The results are used to compute tax transfers to municipalities and as planning instruments for the public housing policy at national and local levels. I have complemented these surveys by administrative records from the City of Paris which contain the same information on a more recent period. This dataset tracks the new and planed social housing units from 2001 to 2012 as of June 2007. In the two data-sets, projects are defined by an address, a subsidy type and a year of completion[12]. Information on each project include: the completion year[13], the year of agreement, the address and the number of dwellings by level of subsidy. The completion and agreement of the projects are only known up to the year level. The completion year corresponds to the year of the first occupancy of the building by a social tenant. The agreement year is the signing year of the formal subsidy agreement between the State and the *HLM* company (*conventionnement*). The amount of time between these two dates depends crucially on the mode of provision of social housing units, from less than a year for the conversion of existing private rental properties into social units up to an average of three years for new buildings or rehabilitations. The created dataset is then matched to the geographical location using the addresses of the buildings to leave me with an address-year panel of the social housing stock.

Data on property sales are from the Commission of Parisian Notaries, BIEN dataset[14]. The data has been used to produce official statistics, evaluate the impact of school quality (Fack and Grenet, 2010) and the efficiency of urban renewal projects (Barthélémy et al., 2007). In France, each property sale has to be registered by a Notary who is in charge of setting up the contract and collecting taxes for the State. The sample is restricted to arm's-length sales of Parisian

[10]There is no unique definition of the social housing stock. The French census, housing surveys and administrative records use different definitions (see data appendix, CNIS, 2001 and Briant et al., 2010).

[11]In the French 2000 law, these types of housing are considered as social housing. Each bed or room has a weight that is a fraction of a family dwelling.

[12]The same address or building may contain units financed by different subsidies. This represents several projects in my data.

[13]I corrected two obvious mistakes. First, there were two main mergers between *HLM* companies and some of the buildings were recorded at the merger year in the following surveys. Second, early *HLM*, *HBM* buildings, were described as completed at the time of a public renovation. I recoded them at the time of construction. Some of the projects started in 2007 were not completed. I used the estimated completion year provided by the City of Paris in 2007.

[14]*Base d'Informations Economiques Notariales.*

flats without occupant owner. The transactions data set is almost comprehensive from 1995 to 2005 and contains 333, 590 flats transactions inside Paris. The INSEE evaluated the coverage rate of all housing transactions in Paris at 90% in 2004 (Gouriéroux and Laferrère, 2006)[15]. As my outcome variable is the log price, the quality of the information on prices is a main issue. The reported prices may be biased by tax evasion and money laundering. The French National Assembly notes that the permissive regulation of French property-owning companies is the main source of fraudulent transactions in the real estate market (Assemblée Nationale, 2002). This issue is less tangible for the sales between private households. In 95% of these sales, a false price record would require collusion between four parties: the buyer, the seller, the real estate agent and the notary (OECD, 2008). As a result, I restrict my sample to the sales between private households. The sales between private households represent 231, 803 transactions (69.5% of the initial sample). This restricted sample avoids the problem of sales to and from *HLM* companies and other administrative bodies. However these restrictions discard the sales from developers to private households occurring in new buildings that may be located close to social housing projects in urban renewal programs. In the empirical section, I present evidence that these restrictions do not imply sample selection issues. The sales located close to social housing projects are not more likely to have private buyers before or after the projects' completion. Furthermore, the number of sales at the building level does not depend on the local evolution of the social housing share.

The control variables include the characteristics of the flats and the sales, namely: size, number of bedrooms and bathrooms, date of construction of the building, day of the sale and the address. Each address was located in Lambert grid coordinates (Lambert 1 North) by matching on its exact name[16]. Table 1.2 provides broad descriptive features of the flats sold in Paris in 1995 and 2005: for the whole sample, for the flats sold between private households and repeated sales within the same building. Panel A shows the characteristics of the flats. There was first no independent check on the accuracy of the dwellings attributes[17]. This is particularly striking for the dwellings' size, as nearly half the information was missing in 1995. As data quality control increases, there was less than 10% missing values for the same attribute in 2005. During the sample period the average price per square meter in 2005 euros increases by 100% between 1995 and 2005, while the number of sales also increases twofold from 1995 to 2000 and remains stable afterwards. The main characteristics of the sales remain homogeneous over the sample period. The average flat is around 51 square meters, 60% of the sold properties have one or two rooms and 90% of them were built before 1992. Interestingly, 90.1% of the

[15]This number is for the whole universe of housing transactions and does not distinguish private households from firms or public bodies.

[16]Incorrect spellings were manually corrected. The main remaining mistakes were corrected using local tax lots (*parcelles cadastrales*) and additional location information (*compléments d'adresses*). The spatial location has a precision of the order of five meters. The addresses were matched to the census blocks (*Ilots*) and tracts (*IRIS*) that are clusters of blocks using the French statistical office coding file. In Paris, census tracts represent small areas of around 2, 500 inhabitants and census blocks have an average of less than 500 inhabitants.

[17]The French statistical office now produces quarterly housing prices using these data.

sales between private households ($208,918$) occur within buildings[18] having at least two sales (between private households). Consequently, it is reassuring that my results based on controls at the building level will not be driven by a small subsample.

Panel B of table 1.2 presents the main explanatory variable of my analysis. It was constructed by combining the precise geographic coordinates of sales and the mapping of new social housing projects. To describe the relative intensity of social housing in the vicinity of a sale i at time t, I define different neighborhoods by distance d. $S_{it}(d)$ represents the share of social housing in the neighborhood of sale i with respect to the number of flats in the same circle according to the last comprehensive census in 1999:

$$S_{it}(d) = \frac{H_{it}(d)}{N_i(d)} , \tag{1.1}$$

where $H_{it}(d)$ is the number of social housing units completed at or before time t within a circle of radius d around the flat and $N_i(d)$ is the estimated number of occupied flats in the circle of radius d according to the census in 1999. The break-down of the number of flats at the tract level is the smallest publicly available data from the 1999 census. Thus it is not possible to get a direct estimate of $N_i(d)$. Figure 1.3 illustrates the process used to compute the social housing share. It shows a map of the 13$^{\text{th}}$ *arrondissement* in Paris. Plain lines represent census blocks and dots the social housing buildings in 2010. Three circles of 50, 250 and 500 meter radii are centered around a particular sale. For each circle, $N_i(d)$ is the sum of the occupied dwellings over all intersected census tracts, each tract being weighted by the fraction of its area located within the circle[19].

In Panel B of table 1.2, the average transacted flat in 1995 has 10% of social housing units within 500 meters. This number decreases slightly once smaller circles of 350, 250, 150 and 50 meter radii are considered. Within the smallest geography of 50 meters, the social housing share in 1995 is 7%. This pattern is very similar in the cross-sections in 1995 and 2005. It corresponds to the spatial bunching of social housing units in a few neighborhoods observed in figure 1.2. The circles are centered around private properties and the smallest radius of 50 meters takes only into account immediate neighbors which are less likely to be social housing units. Furthermore, the standard-deviations of the radial measures of the social housing share are increasing when I consider smaller radii. In 1995, the standard-deviation of the 500 meter measure (0.10) is nearly five times lower than the standard-deviation for the 50 meter measure (0.46). However, all the radial measures display a similar evolution from 1995 to 2005. Over the sample period 1995 to 2005, the share of social housing in the housing stock increases by $27,773$ units or 2.5% of the occupied housing stock in 1999.

[18]I define a building as the intersection of an address and a period of construction. According to this definition, 69.6% of the addresses have a unique building (89.5% for the repeated sales sub-sample). Using building rather than address as the unit of analysis has the advantage of not considering demolition and new construction on the same address as an upgrade of an existing entity. In practice, the results are not sensitive to using building or address fixed effects once I control for the period of construction of the buildings.

[19]The implicit assumption that the density is constant within census tract is likely to approximately hold in Paris. The regulation of building height, *épannelage*, is strictly applied.

The last row of table 1.2 gives the evolution of an alternative measure of the social housing share. This measure uses a parametric definition of neighborhood: the census tract of the 1999 census. I consider the total number of social units located in each tract. The denominator of the census tract measure, N_i, is known without uncertainty. The descriptive statistics for this measure are close to those obtained for a circle of radius 150 meters. The median size of a census tract is indeed equivalent to a circle of radius 146 meters. However, from figure 1.3, the radial measures of the social housing share have two main advantages. First, they can be computed at different geographical levels. Second, the census tract boundaries follow the middle of the streets. Thus the crossing of a street implies a partly artificial discontinuity in the measured social housing share.

3 Empirical strategy

3.1 Main specifications

Exposure to social housing varies across time and location. This paper seeks to identify a traditional hedonic equation where the log-price of a flat sale is related to the flat's various characteristics:

$$ln(p_{ibgt}) = x_{ibgt}\beta + \gamma S_{bt}(d) + \alpha_{gt} + \varepsilon_{ibgt} \ , \tag{1.2}$$

where i is an index for flats, b for buildings, g for various geography levels and t for time. x_{ibgt} is a row vector of observable dwelling characteristics that may affect housing prices. Specifically, x_{ibgt} includes number of rooms; size in square meters; floor; age of the building; and dummy variables if the flat has a bathroom, a parking lot, a cellar or a lift. $S_{bt}(d)$ is the share of social housing dwellings in the neighborhood of the building within a given radius d and α_{gt} represents geographical unobservable characteristics. My main specifications correspond to a difference-in-differences set-up where $\alpha_{gt} = \delta_g + \alpha_t$.

OLS estimates of the impact of public housing on housing prices are unlikely to identify γ, the parameter of interest, because $S_{bt}(d)$ may be correlated to unobserved neighborhood characteristics or unobserved characteristics of the dwelling through α_{gt} or ε_{ibgt}. This identification problem is difficult to circumvent for three main reasons.

First, the location of social housing projects is a joint decision between the *HLM* developers and the municipality. As the rent of social units is fixed at the city level, landlords have incentives to target distressed properties and neighborhoods with low or declining housing values[20]. Similarly, the municipality may value the removal of slums and their replacement by public housing. Thus, the specific unobservables of the private properties surrounding social housing projects may differ from the characteristics of properties not affected by the projects.

Second, the timing of the effects of new social housing dwellings is ambiguous. Changes in

[20] Anecdotal evidence suggests that most *HLM* companies do not take into account the potential residual market value of social properties when they compute the expected returns of social housing projects (Inspection Générale des Finances and Conseil Général des Ponts et Chaussées, 2002).

neighborhood composition could be anticipated by buyers and sellers. Social housing buildings take an average of three to four years to be completed after the initial agreement and, in the case of new buildings, public hearings are mandatory. Furthermore, there is a time lag between the flat buying decisions and the recorded time of the sales.

Third, the local public housing stock may evolve jointly with other factors having direct impacts on dwellings' values. For example, new public housing projects may be accompanied by better transportation links, infrastructure investments and new commercial or public services. These complementary investments could be planed by the municipality or the result of a political process. Anecdotal evidence suggests that the affected populations may organize themselves to lobby local governments and *HLM* companies in order to obtain various forms of compensation or amendments to the initial projects (Paris, 2006). Developers may also target new social buildings according to adverse neighborhood shocks such as fire or lack of maintenance of nearby buildings. Mean reversion could also bias upwards the measure of the impacts of social housing on nearby private properties. Even in the same census tract, the characteristics of the sales before and after the creation of social housing units may differ in a systematic manner which would bias difference-in-differences estimates. Finally, the observed changes in price may be driven by changes in the own characteristics of the dwellings such as buildings' upgrades, or by changes in the valuation of observable dwelling's characteristics.

To circumvent the endogeneity of location problem, I take advantage of the high population density in Paris to control for local unobservables. Most previous papers have considered the geographical unit of interest g as a census aggregate (tracts, blocks or counties)[21]. I extend these geographical controls by defining my smallest geographical unit at the building level. Precisely, I define a building as the interaction between an address and a period of construction. This allows me to control for numerous time invariant characteristics of the dwellings. For example, Parisian school catchment boundaries do not follow census tract definition (Fack and Grenet, 2010) and most of the major investments that could impact sales prices take place at the building level (e.g. water provision, sanitation, lift maintenance etc.). Moreover, building fixed effects mitigate a main source of time varying unobservables that may be correlated with the social housing share. The replacement of distressed private buildings by new private buildings is not confounded as a neighborhood upgrade.

A first test of the causality of the estimates is to generalize regression (2) by allowing the externalities of social housing to decay with the distance to the projects. In this case, the effects of the social housing projects are measured by a vector (γ_r) corresponding to the impact of the social housing share in different rings (r) around a sale:

$$ln(p_{ibgt}) = x_{ibgt}\beta + \sum_r \gamma_r S_{bt}(r) + \alpha_{gt} + \varepsilon_{ibgt} \tag{1.3}$$

where the ring variables $S_{bt}(r)$ are mutually exclusive and define concentric belts with different treatment intensities. I would expect to see larger effects for private properties located closer to

[21] In most set-ups, repeated sale specifications imply some issues of sample selection.

the social projects because they have a more direct exposure to the potential buildings' upgrades and inflows of low income tenants.

I address the problem of the timing of the impacts by allowing the effects of interest to depend on the completion date of the projects. As the same transaction can be affected by several housing projects occurring at different points in time, I need to consider the inflows of social housing units over time and not pre and post treatment dummy variables. Specifically, I introduce lead and lag flows of social housing divided by the number of flats in the neighborhood in 1999. $F_{b,t+2c}(d)$ represents the additional share of social housing due to projects completed between $2(c-1)$ and $2c$ after the time of the sale, t, within a circle of radius d. I use two year changes to ensure sufficient variation even within small neighborhoods. These new variables can be expressed in terms of the share of social housing within a circle of d meter radius at time t, $S_{b,t}(d)$:

$$F_{b,t+2c}(d) = S_{b,t+2c}(d) - S_{b,t+2(c-1)}(d) \ . \tag{1.4}$$

For example, $F_{b,t-2}(d)$ takes into account all projects completed two and three years prior to the sale at time t and $F_{b,t}(d)$ measures the inflows of social housing units during years t and $t-1$. The final regression corresponds to:

$$ln(p_{ibgt}) = x_{ibgt}\beta + \gamma_i S_{b,t-14}(d) + \sum_{c=-6}^{3} \gamma_c F_{b,t+2c}(d) + \alpha_{gt} + \varepsilon_{ibgt} \ . \tag{1.5}$$

This specification assumes that projects built more than 14 years before the time of the sales have a constant impact on housing prices (γ_i) and that projects that will be built more than 6 years after the sale can not be anticipated by the housing market. Under the assumption that flats and neighborhoods unobservable characteristics do not evolve systematically with social housing inflows, the γ_c's measure the differential impact of the closeness to social housing dwellings with respect to the year of completion of the projects. Specification (5) can be extended as specification (3) to incorporate heterogenous impacts on housing values by distance belts.

I test the robustness to potential time varying unobservables correlated with the social housing share by including local linear trends at different geographical levels. In my most flexible specification, this heterogenous growth model includes controls for building unobservables and census tract linear trends.

To get an idea of the precision of my local controls, it is useful to compare the geography of Paris to the one used by Schwartz et al. (2006) to evaluate the externalities of subsidized housing in New-York. The smallest level of the French census is the block for which no public data are available. French census tracts are small clusters of blocks that are designed for the release of statistical information. The French census tracts match the main political units. Each of the twenty *arrondissements* of Paris are divided into four administrative *quartiers* which are subdivided into census tracts. A direct comparison of the 2000 US census and the 1999 French census show that the typical Parisian tract is much smaller than the average New-York tract: the population is on average one third below and the area five times smaller. In terms of area, the average Parisian census tract is also between the Chicago census block groups and census blocks considered by Autor et al. (2009).

3.2 Isolating the effects of low-income tenants

The previous specifications have two remaining shortcomings. First, they estimate an aggregate impact: the creation of new social units through rehabilitation and new buildings and the inflow of low income tenants. Second, even after controlling for local trends, disentangling social housing effects from local complementary investments is not straightforward. In order to obtain a more precise idea of the effects of low income tenants on housing prices, I exploit variation in the stock of social housing units following the election of the current mayor in 2001. The current mayor of Paris, Bertrand Delanoë, was virtually unknown before his electoral win in 2001. This electoral poll was close and uncertain: at the second round of the election, the left-wing alliance received 49.6% of the votes against 50.4% for the divided right wing.

Following this electoral win, a sharp increase in the provision of social housing units was achieved through the conversion of existing buildings into social housing units (Figures 1.1 and 1.2) or *acquisition sans travaux*[22]. There were no conversion projects before 2001. These projects were not accompanied by new construction or rehabilitation and thus one can infer that their effects on housing prices were limited to the inflow of low income tenants into the neighborhoods and the consequent changes in their socio-economic compositions. Bacquet et al. (2010) describe the new process for two projects in Paris based on interviews with the tenants. The *HLM* company or the municipality buys an existing rental building from private landlords using social housing subsidies. The vacant flats are allocated to *HLM* applicants and the remaining private tenants are slowly replaced by *HLM* households when they leave the building or their tenancy expires. This process was particularly controversial as it was judged costly in respect to the other ways to provide social housing. Moreover, it was mainly used in wealthy neighborhoods to create dwellings for very low income households. The APUR (2010)[23] provides descriptive statistics from a survey of the *HLM* landlords of converted buildings in April 2009. During the first two years after the mayoral election, $3,933$ social dwellings, more than 60% of the total number of agreed dwellings, were created using this financing scheme. At the time of the survey, 80% of these dwellings were occupied by social tenants. From 2001 to 2005, $6,913$ private dwellings were converted into social housing units.

I use this policy shock to isolate the impact of the share of social tenants in the neighborhood of the sales. This policy has two main advantages. It was arguably unpredictable by homebuyers of nearby sales and it is not systematically associated with other public investments in the neighborhood of the sales. From the data provided by the City of Paris, I construct the evolution of the share of the converted social housing in the occupied housing stock in 1999 from 2001 to 2005 as in equation (1).

[22] This process is also known as *acquisition conventionnement*.
[23] *Atelier Parisien d'URbanisme*.

4 Empirical results

4.1 Cross-sectional estimates and parametric neighborhood definition

Table 1.3 shows how the log price of sales (in 2005 euros) changes with existing and future social housing projects from 1995 to 2005. The sample is restricted to the sales occurring within building with repeated sales to ease the comparison with the estimates controlling for building unobservables. I use my two alternative measures of the social housing shares: by radii from 500 meters to 50 meters in columns (1) to (4) and within census tract in column (5)[24].

The regressions in panel A control only for the time of the sales. These cross-sectional estimates reveal that housing values are negatively correlated with the share of social housing in the vicinity of the sales. This conclusion is robust to the neighborhoods I consider. The magnitude of the cross-sectional estimate at 500 meters indicates that an increase in the share of social housing by 10 percentage points (approximately one standard-deviation) is correlated with a decrease of 14% in housing prices. The negative impact of social housing on housing prices is decreasing with the closeness to the sales even if the standard-errors remain low. When the social housing share is measured only within 50 meters to the sales, the cross-sectional point estimate is divided by 21. However a one standard-deviation increase of the share of social housing within 50 meters would still imply a significant decrease in housing price by 2.6%. A simple computation can help to get a better sense of the size of the measured effect. As the average property has 161 surrounding flats within 50 meters, an average project of 35 flats would decrease the property value by 1.4%. The census tract measure of the social housing share does not provide a different picture from the radial measures. As expected from the descriptive statistics, the point estimates and standard-errors match closely the results obtained for the 150 meter radius.

The second and third rows of panel A investigate further the causality of these point estimates. In row 2, the negative point estimates are stronger when the social housing includes only the projects created within the past 10 years. The point estimate for the 500 meter radius is multiplied by 7 and the one for the 50 meter radius by nearly 2. New social housing projects appear to have more negative externalities than existing low income housing. This could be consistent with more negative externalities. New social tenants are poorer than established tenants and new social housing dwellings have more stringent income eligibility requirements than *HLM* created before 1977 (table 1.1). However, no causal interpretation can be given to this phenomenon. New social housing projects may also be located close to private housing having worse observable and unobservable characteristics than older projects. In row 3, housing prices are also correlated with future social housing units which will be built in the next five years. Interestingly, the magnitude of the point estimates in columns (2) and (3) are close. Within

[24] As the pattern of the point estimates is smooth over radii, table 3 does not report the estimates for the 350 meter measure to save some space. The appendix Table 1.D presents descriptive statistics for the social housing share measures by circles and belts around the sales.

the 50 meter radius, the effect of the future social units is more than twice as high as that of the current units. Flats located in neighborhoods where the share of social units will increase by 10 percentage points in the next five years have 2.6% lower values. On the one hand, the time pattern of the point estimates could be consistent with the fact that social dwellings are located in large deprived neighborhoods and tend to replace distressed properties at the local level. On the other hand, the same pattern could also be consistent with rational expectations of the home buyers if they are able to predict future social housing developments.

In panel B, I introduce an extensive set of controls for flats characteristics[25]. The estimated coefficients decrease slightly in absolute value but are also more precisely estimated. The smallest estimate at 50 meters still implies that a new social housing project would decrease housing values by 1.1% and it remains significant at the 1% significance level. In summary, the linear covariate adjustment leads to similar results as the specification without these controls. Although the set of controls is large, it may not be adequate to solve the endogeneity of the new projects' location. To isolate the causal impact of social housing on housing prices more precise local controls may be needed.

4.2 Geography fixed effects

Table 1.4 presents the results of the difference-in-differences specifications (2) to (4) at various geographical levels: 80 *quartiers*, 902 census tracts and 36,274 buildings[26]. The idea is to control for the particular local characteristics around social housing projects. All regressions include an extensive set of controls for the flat characteristics and the time of the sales. I use my main measure of the social housing share: by radii from 500 meters to 50 meters. Columns (1) to (3) introduce the share of social housing within 500 meters of the sales, columns (4) to (6) within 250 meters, columns (7) to (9) within 150 meters and columns (10) to (12) within 50 meters.

Panel A of table 1.4 does not control for different house price trends around the social housing projects. While using *quartier* or census tract controls, the estimates appear consistently negative, their sign changes once the fixed unobserved characteristics of the buildings are controlled for. Column (1), the point estimate using *quartier* fixed effects indicates that an increase of 10 percentage points of the share of social housing within 500 meters would decrease housing value by 6.0%. This estimate is divided by two, a 2.8% decrease, when I control for census tract fixed effects in column (2). However, once I control for building unobservables, column (3), I observe a different story in Paris. The same change would imply a 3% increase in housing value. The price increase estimate is statistically significant at the 10% level. At the same time, the R-squared rise from 0.871 to 0.911 when building rather than census tract controls are included. This means that building and precise location characteristics play a key role to determine both housing prices and social projects' location. The change in the values

[25] Appendix Table 1.E presents the specification and the summary statistics for all the control variables included.

[26] For all specifications, the sample is restricted to the sales between private households occurring within buildings with repeated sales. Controlling for building fixed effects or address fixed effects does not affect significantly the point estimates.

of the point estimates and R-squared across fixed effects from *quartier* to building is consistent over the different radii.

Focusing on the specification controlling for building unobservables and variation within the 50 meter circle, column (12), the positive impact of the share of social housing within 50 meters of the sale is statistically significant at the 1% level. A new social project of 35 flats would imply an increase in housing value by 1.4%. As projects are associated with new buildings and rehabilitations, positive estimates could correspond to disamenity removals and buildings' upgrades at a small spatial scale. Based on census tract controls, the estimates for the impact of the share of social housing on housing price seem to be biased by omitted variables and have a negative sign. The social housing share is proxying for buildings having worse unobservable characteristics. However, the positive estimates are consistent with another story related to time varying unobservables. The creation of social housing units could be associated with complementary investments in small neighborhoods, such that additional playgrounds or new public services. Even controlling for building fixed effects, the estimates of the impact of the social housing share could be confounded by mean reversion and the selection of locations with particular underlying price trends.

Panel B of table 1.4 presents the results of the same specifications as panel A but including 80 *quartier* linear trends[27]. In all the fixed effect specifications the overall impact of social housing appears similar to the estimates reported in panel A. At the same time, the R-squared for all the regressions are not affected by the inclusion of these trends. The *quartier* trends explain neither the location of social housing nor the evolution of the log housing price.

Table 1.5 presents the results of the difference-in-differences specification (3) that investigates further the causality of the relationships of table 1.4 by introducing the share of social housing within different belts around the flats. As the share of social housing in the different belts are mutually exclusive, each coefficient represents the effect of the social housing share in a given belt. Estimates in columns (1) to (3) condition on flat controls, time of the sales and geographic fixed-effects, while the specifications in columns (4) to (6) also include 80 *quartier* linear trends. In columns (1) and (2), with geographical controls at the *quartier* or tract levels, the spatial pattern of the point estimates is not consistent with a negative externality centered around the projects. The estimate for the 350 to 500 meter social housing share in column (1) is nearly 20 times higher than the point estimate for the 50 meter circle. The pattern of the standard-errors is also informative. Given that the 350 to 500 meter ring is much larger than the 50 meter circle, one possible concern is that the observed spatial difference in point estimates may be driven by measurement error. However, the near zero point estimate for the share of social housing within 50 meters in column (1) is very precisely estimated and still significant at the 1% level. Thus it is unlikely that the results are generated by some kind of attenuation bias. Once building fixed effects are included in columns (3) and (6) the estimates are consistent with positive externalities decreasing with distance from social projects. In my preferred specification including both building fixed effects and linear trends by *quartiers* in column (6),

[27]The linear trends are measured as the number of days between the sale and the 31$^{\text{st}}$ December 1994.

the point estimate for the 50 meter circle remains similar to the one obtained in table 1.4 panel B specification (12). The estimates for the impact of the social housing share within the 50 to 150 meters, 150 to 250 meters and 250 to 350 meter belts appear consistent with some positive externalities and decline with distance. In this specification, properties located within 50 meters of a new social housing project experience a 1.2% increase in housing prices once the project is completed.

Finally, figure 1.4 plots the difference-in-differences estimates of the social housing projects impacts over time as in specification (5) for the circles from 500 meters (panel a) to 50 meters (panel d). These specifications introduce leads and lags flows of social housing and control for building fixed effects and linear trends by *quartiers*. On the solid lines, each point corresponds to the estimate of γ_c, the time-varying impact of the social housing share on the log of housing prices[28]. The last point, 15 years after the projects completion, is the estimate for γ_i, the long-run impact of social housing on the log of housing prices. The vertical bars represent the 95 confidence interval and the dashed lines represent the 90% confidence interval.

In figure 1.4 panel a, the long run estimates of the effects of the share of social housing within 500 meters on housing prices appear negative. The timing of the impacts matches closely the completion of the social housing buildings. Estimates are slightly increasing over time before the projects completion but insignificant and close to zero three years and one year before the project completion. They become slightly positive just after the completion of the projects and start to decline five years later. They display constant magnitude after nine years. Based on these estimates, an increase of 10 percentage point of the social housing share would imply on the long-run a 6.2% decrease of private property values located in the vicinity of the projects.

In figure 1.4, panels b to d replicate the estimates of panel a using circles of 250 meters, 150 meters and 50 meters around the private properties. No clear time pattern emerge from these figures. Panel b, the estimates using the 250 meter share of social housing decrease after the completion of the projects as in figure 4 panel a but they are insignificant at the 10% level. Figure 1.4 panel c reports the estimates for the impact of social housing within 150 meters. Housing values appear to rise slightly after the completion of the projects. However, the estimates can not be statistically distinguished from zero at the 10% significance level. Finally, figure 1.4 panel d plots the estimates for the impact of the share of social housing on housing values within 50 meters. The estimates have a clear time pattern. They can not be statistically distinguished from zero before the completion of the social projects and start rising just after. They remain positive and stable three year after the projects' completion. Private properties located within 50 meters of a new social project of 35 units experience in the long run a 2.6% price increase.

Figure 1.5 shows the results of the extension of specification (5) that allows the impact of social housing to vary with both time and distance for the outer belt from 350 to 500 meters, panel a, and the circle of 50 meters, panel b. The specification includes sales' controls, building fixed effects and linear *quartier* trends. Panel a display only the point estimates over time for

[28]The $\widehat{\gamma_c}$s are displayed at the middle of the two year intervals $(-2c+1)$.

the house price impacts of the social housing share within the 350 to 500 meter belt. In the outer belt, housing prices decrease after the completion of the social projects. The estimated impacts become significant at the 5% level seven years after the projects' completion and remain stable afterwards. A 10 percentage points higher social housing intensity leads to a 5.5% decrease housing prices 15 years after the projects' completion.

On the contrary, panel b, in the 50 meter circles around the projects, if the social housing share increases by 10 percentage points, housing prices would increase by 1.2%. This last point estimate is very close to the one obtained in figure 1.4 panel d where I only introduced the social housing share within 50 meters. The estimates for the other distance belts have more mixed patterns insignificant at the 10% significance level.

4.3 Sample selection issues

As previously mentioned, a possible concern for measuring the externalities of social housing on housing values is that I restricted my sample to the sales between private households and that my sample is restricted to the properties that transact. If the flats that transact after or before the projects' completion become harder or easier to sell to private buyers or if they have different unobservable characteristics, this would likely bias my point estimates. I estimate a linear probability model where my dependent variable is a dummy variable if the flat is sold to a private buyer as in specifications (2) and (5). In this specification, my sample includes the whole universe of transactions from private sellers, administrative bodies and firms[29].

I also investigate if there is any relationship between the number of sales and the timing of the social housing projects at the building level. To do this, I modify my specification to capture the fact that the sales of flats within a building are irregular events but that the number of sales each year is a continuously updated outcome. I construct a panel of building-year observations. I treat a building constructed before 1995 as if it contributed for 11 building-year observations[30]. The new dependent variable is coded as the total number of sales if there are some observed sales in the current year and 0 in all other periods. My specification includes building fixed effects, dummy variables by years and linear trends for the 80 *quartiers*. I then estimate a linear count data model similar to specifications (2) and (5) for the whole sample of buildings and for the balanced panel of buildings constructed before 1992.

Table 1.6 panel A reports the marginal effects of the social housing share at 500 and 50 meters on the probability to sell a property to a private buyer for the whole universe of sales. The estimated marginal effects are small both for the whole sample, columns (1) and (2), and the sales of private properties within buildings constructed before 1992, columns (3) and (4). In columns (1) and (3), a 10 percentage points increase in the social housing share within

[29] A limitation of this analysis is that I only observe the realized sales. All my estimates are conditional on the properties being sold.

[30] As the observation of the year of construction is censored by intervals, I consider that the buildings constructed before 2000 contribute to the sample after 2001 for 5 years and discard the buildings constructed after 2001. I do not observe buildings leaving the sample because they are closed or demolished. My dependent variable is coded as 0 in these cases.

500 meters would decrease the probability that a flat is sold to a private buyer by 0.7 to 1 percentage point[31]. These estimates are not statistically significant at the 10% significance level. In columns (2) and (4) the marginal effect of a 10 percentage points increase of the share of social housing within 50 meters on the likelihood to sell to a private buyer is between 0.06 and 0.02%. The standard-errors are precise but the point estimates remain not statistically significant at the 10% level. The pattern of the point estimates of specification (5) over time do not reveal any irregularities with respect to the timing of the projects (not reported).

Panel B of table 1.6 shows the estimates of the linear count data model for the yearly number of sales at the building level. In columns (2) and (4), the point estimates for the impact of the social housing share within 500 meters are imprecisely estimated but small. A 10 percentage points increase of the social housing share within 500 meters would imply a decrease of almost 0.03 sales by year[32]. This figure is consistent with a weak association between social housing projects and urban renewal programs. However, this relationship does not hold for the share of social housing within 50 meters. A 10 percentage points increase of the social housing share would have no distinguishable effects on the number of transactions at the building level.

Overall the estimates in table 1.6 suggest that my main estimates are unlikely to be biased by the selection of the flats that are transacted and sold to private households. A 10 percentage points increase in the social housing share at 50 meters was generating an increase of 1.2% on housing prices. For the average sale in my sample, this represents $2,125$ euros. The lower bound of the 95% confidence interval in Panel B column (4) implies that an increase of 10 percentage points of the social housing share could reduce the number of transactions by $0.01 \times (0.015 + 1.96 \times 0.019) = 0.005$ sales. The prices of the non-transacted flats after the projects completion would have to be as low as 1.3% of the average price of the transacted flats in order to generate the observed positive effects on housing prices.

4.4 Discussion

Compared to the existing literature, the estimate for the outer belt from 350 meters to 500 meters have of the same sign and magnitude as the estimates of Autor et al. (2009) for rent control housing, where a one standard-deviation increase in rent control intensity implies a 3% to 7% decrease in non-controlled property values within 0.25 miles (400 meters). They interpret their point estimates as the result of investment complementarities in the housing market. Rent controlled properties are less well maintained than non-controlled properties and imply lower level of housing investments in their vicinity. This story does not fit well the Parisian context where most of the new social projects are associated with rehabilitations and new buildings. Other mechanisms include inflows of low-income private tenants, local increase in crime rates and deterioration of public and private schools quality within the school zones of the projects. These mechanisms can not be tested directly due to the lack of available data for Paris. Baum-

[31] The mean of the dependent variable is 0.855 in columns (1) and (2) and 0.861 in columns (3) and (4).

[32] For the whole sample of buildings, the average number of sales by year is 0.435 with standard-deviation 1.083. For the buildings created before 1992, the mean and standard-deviation of the yearly sales are both slightly higher: 0.473 and 1.129.

Snow and Marion (2009) find that LIHTC programs in Chicago were associated with inflows of low income tenants in the private housing market. Hartley (2008) reports that the demolition of high rise social housing buildings is associated with a decrease in crime rate but that small projects do not have significant impacts on local crimes.

Another stream of the literature has found positive impacts of social housing developments on housing values in line with the estimate of the impact of the evolution of the social housing share within the 50 meter circle. Baum-Snow and Marion (2009) estimate positive impacts of new LITHC developments on housing values. However, their estimates are difficult to compare with the ones obtained here as the geographies of Paris and the US metropolitan areas are quite different. They use neighborhoods of one kilometer radius and their explanatory variable is the total number of projects, not the share of social housing units in the occupied housing stock. In New-York city, Schwartz et al. (2006) find a positive impact of subsidized housing on surrounding properties values. They define 150 meter neighborhoods and, in the case of fully rental multifamily projects, a new project leads to an average increase in housing prices by 3.5%, while in the Parisian case within 50 meters of a new project I observe a 2.6% increase in housing value. But their average project is much larger, 250 units, than the typical Parisian development of 35 units.

The overall pattern of the point estimates is difficult to reconcile with a theory based on complementary investments. This would need a public infrastructure making better off the close neighbors and worse off the private owners located farther away from the social housing projects. A first explanation is that if new social projects replace distressed properties the benefits may be extremely localized while other negative externalities (e.g. crime, school performance, etc.) may operate at larger spatial scales. Another story consistent with this evidence would be based on initial taste sorting within small neighborhoods. As social housing projects are located in the distressed parts of neighborhoods, the close neighbors may have lower aversion against low-income tenants than neighbors located farther away in initially better located properties.

Compared to the other determinants of housing prices, the magnitude of my estimates is sizeable and plausible. Fack and Grenet (2010) found that a one standard-deviation increase in middle school quality tends to increase property value by 1.4% to 2.4% in Paris. This estimate is slightly smaller than the first estimate of Black (1999) and in the middle range of the empirical literature on housing prices and school quality reviewed by Gibbons and Machin (2008). The literature on the impact of local crime on property values displays estimates of similar magnitude. Linden and Rockoff (2008) estimate that the average price of a home declines by around 4% once a sex-offender arrives in a neighborhood. Gibbons (2004) reports that a one standard-deviation decrease in the local density of domestic property crime adds 10% to the price of an average London property. Concerning the clean-up of hazardous waste sites, Greenstone and Gallagher (2008) report a maximum positive impact on housing prices of 2.3% once the clean-up is completed through the US Superfund program. Finally, Chay and Greenstone (2005) and Bajari et al. (2010) use quasi-experimental and structural estimation methods and find that a 10% increase in air quality tends to increase property values by 2% to

8%[33].

5 Disentangling different mechanisms

5.1 Heterogeneity by neighborhoods and sales' observables

In the absence of available data to directly test the mechanisms leading to positive social housing externalities in small neighborhoods and negative externalities further away from the projects[34], I investigate the heterogeneity of the treatment effects. So far the results use the full sample of sales in Paris between private households, but the heterogeneity of the effects by neighborhoods and sales' characteristics is potentially important.

Table 1.7 reports the estimates by neighborhood characteristics. I focus on the impact of the social housing share within 50 meters on housing prices for my preferred specification with building fixed effects and *quartier* linear trends. Panel A shows the estimates of four sub-samples by quartile of housing price in 1995. The quartiles correspond to the median housing price per square meter computed from the 1995 sample of sales with information on flats size. The median prices are computed for each of the 80 *quartiers* of Paris[35]. A clear pattern emerges by neighborhoods' initial housing prices. Most of the positive impact of social housing is driven by neighborhoods with low housing prices (lowest quartile) while the second and third quartile of initial housing prices display smaller point estimates. Interestingly, the estimates are virtually identical if I estimate a constrained specification where the quartiles of housing prices are only interacted with the social housing share and for the sake of brevity I do not report them[36]. Thus my estimation is robust to the implicit assumption that the return to private flats characteristics are homogeneous over space. Overall, the positive estimates decreasing with neighborhood initial wealth are consistent with the view that the renewal effects and the improvement of the quality of the housing stock should dominate any externalities of low-income tenants when the income differential between the current neighborhood population and the social tenants is small.

Panel B of table 1.7 shows the estimates of an identical specification but using the quartiles of the social housing shares in 1995 by *quartiers*[37]. For comparison with panel A, the quartiles are displayed in reverse order. The externalities of new social housing appears clearly positive in neighborhoods with high initial social housing shares, while they are close to zero otherwise.

Finally, figure 1.8, panels a and b plot the impact of the 50 meter social housing share on housing prices over time for the lowest and highest quartiles of housing price in 1995. The

[33] These estimates are long-run effects. Currie and Walker (2009) find no immediate effects of the sharp reduction in emissions from motor vehicles induced by electronic toll collection technology on housing prices

[34] French police forces record crime at a geographically localized level. However, it is not possible to obtain this data at the present time for research purposes. Fougère et al. (2009) use the most geographically detailed French data. Paris is one of their data points.

[35] At this level, the spatial distribution of prices is stable over time. Figure A1 plots the quartile of housing prices in 1995.

[36] Estimates using this alternative specification are available upon request.

[37] Appendix Figure 1.B plots the corresponding quartiles. They are almost perfectly negatively correlated with the quartiles of figure 1.C.

estimates correspond to specification (5). Panel a, before the completion of the projects, the estimates can not be distinguished from zero at the 10% significance level and raise after the completion of the projects to become stable five years later. The long-run point estimate is higher than for the average Parisian flat: 0.179 against 0.120 log points. An addition of 35 social units would imply an increase of private housing prices by 3.9%. On the contrary, in high income neighborhoods, the social housing share has no statistically significant impact and the point estimates are close to zero or negative (-0.065 log points) in the long-run[38].

Table 1.8 and figure 1.7 replicate the results of table 7 and figure 6 using the 500 meter measure of the social housing share. Most of the estimates are not significant at the 10% significance level. In panel A of table 8 and figure 7, the basic finding that any positive impact of social housing decreases with the level of initial housing price holds true. The negative estimates for the effects of the social housing share within 500 meters are driven by high income neighborhoods. The estimates of table 8 panel B, which divides the sample by social housing share in 1995, are less clear-cut.

I now study the heterogeneity of the effects with respect to flat size. Table 1.9 presents the estimates for the effects of the social housing share within 500 and 50 meters by different number of rooms. As my preferred specification includes building fixed effects, in columns (1) and (3), I introduce the heterogeneity with respect to flat size by interacting the share of social housing with dummy variables for flats of one or two rooms, three or four rooms and more than four rooms. Columns (2) and (4) report the estimates of a more parsimonious specification where the local share of social housing is linearly interacted with the number of rooms of the private flats. In both specifications, all the positive impact of the social housing share on housing prices are measured for small flats of one or two rooms which are mainly made up of single households and couples without children. On the contrary, estimates for the effects of the 500 meter share of social housing becomes negative for flats of more than four rooms and estimates for the effects of the 50 meter share of social housing can not be distinguished from zero for family dwellings. Figures 1.8 and 1.9 plot the point estimates over time for the flats of less than two rooms and more than four rooms for the 50 and 500 meter measures of the social housing share. The time pattern of the point estimates is consistent with a causal effect on housing prices for one or two room flats and the 50 meter share and for family dwellings and the 500 meter share of social housing.

5.2 Conversion projects after 2001

In this subsection, I report the estimates based on *acquisition sans travaux* projects (conversion projects). Table 1.10 presents the estimates of the effects of the share of social housing units created by conversion of existing private buildings between 2001 and 2005 on housing prices within neighborhoods of 500 to 50 meters around the sales. I restrict my sample to the

[38]The pattern observed for the 2nd and 3rd quartiles of housing prices in 1995 is the same. The time pattern obtained when pooling the 2nd to 4th quartiles of housing prices is the same but more precisely estimated.

flats transacted after 2001. All the specifications include building fixed effects and *quartier* linear trends.

Panel A reports the estimates using the full sample of flats sold after 2001. The point estimates for the share of social housing units created by conversion of private buildings within 500 meters is negative. In column (1), a 10% increase in share of the flats rented to social tenants would imply a housing price decrease by 3.1%. However, this estimate is not significant at the 10% significance level. The concentration of social tenants within smaller circles of 250 meters, column (2), to 50 meters, column (4) are also imprecise. They become economically close to zero. The last two point estimates for the share of social housing within 150 and 50 meters are positive but more than twice below the corresponding point estimates reported for the share of all social housing units and the same specification in table 1.4 panel B columns (11) and (12).

These positive point estimates raise concerns that conversion projects may be associated with larger social housing developments and proxy for rehabilitations of distressed buildings and new constructions. This will be the case if *HLM* developers buy buildings located close to each other and decide to convert part them into social housing or to rehabilitate them according to the occupation and maintenance status of the properties. Panel B examines this assumption by controlling for the evolution of the share of other social housing projects within the same neighborhoods. The estimate in column (4) for the share of the conversion projects within 50 meters is divided by two and remains insignificant at the 10% significance level. For wider radii, the estimates for the impact of the share of converted private properties on housing prices become more negative than the corresponding estimates in panel A but they are all insignificant at the 10% level. Overall the conversion projects provide evidence that social housing not associated with new buildings and other public investments does not have a positive impact on private properties located in the vicinity of the new social housing tenants. The estimates for the effects of the share of new social tenants within 500 meters on housing prices are sizeable and negative, but there is not enough variation to provide a definite answer.

6 Conclusion

This chapter investigates the indirect effects of social housing on private property values in Paris. I find that social housing projects tend to have a positive average impact on housing prices in small neighborhoods of 50 meters around the social projects while the estimated impact become negative farther away from the projects.

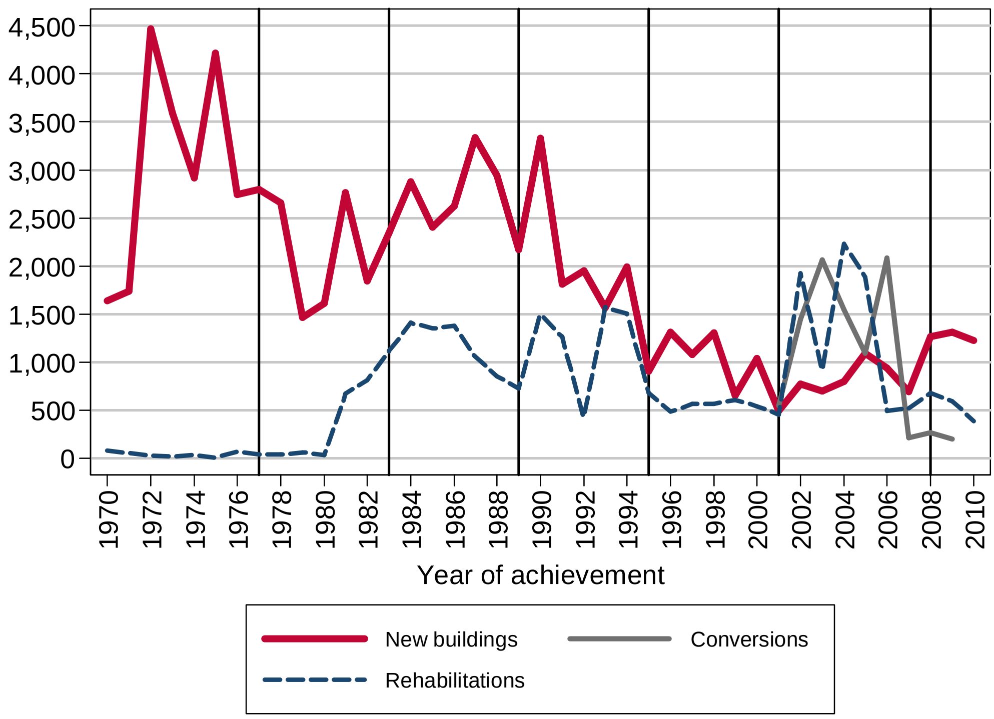
The analysis is based on a unique dataset which combines the whole universe of social housing projects and flat transactions during eleven years at the building level. I exploit the high population density of Paris to identify the impacts of social projects on housing values. I rely on a difference-in-differences identification strategy within small neighborhoods controlling for building unobservables and local linear trends. The timing of the effects provides additional support for a causal interpretation of my results.

I show that a sharp increase of the social housing stock of 10 percentage points, as planned by the French 2000 law, would account for an average increase of around 1.2% of neighboring houses' prices within 50 meters of the projects. However, the measured impacts on housing prices become negative if the share of social housing units is measured at wider radii where private properties are less exposed to the renewal effects of the projects. Within an outer belt from 350 to 500 meters around the projects, the average housing value would decrease by 5.5%.

The empirical results are consistent with the idea that social housing projects associated with new buildings or rehabilitations of distressed properties have two distinct impacts. They improve the quality of the existing housing stock but they lead to an inflow of social tenants into the neighborhood. First, the positive effects of new social housing units are entirely concentrated in small neighborhoods around the projects. Private properties located between 350 and 500 meters experience price decrease. Second, the increase in property value is concentrated in low-income neighborhoods, while high income neighborhoods would not experience housing price increase. The price increase is also entirely driven by small flats of one or two rooms while family dwellings of more than four rooms would not benefit from social housing developments. Third, when I isolate the inflows of low income tenants using the direct conversion of private rental units into social housing without any rehabilitation, the point estimates show that social housing projects that are not associated with an improvement of the quality of the existing housing stock do not have positive effects.

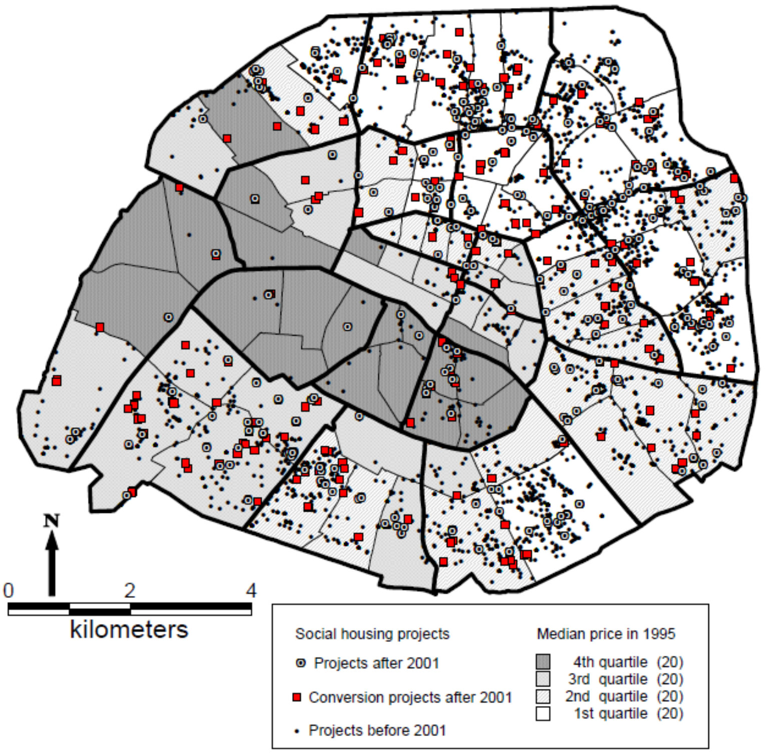
My results suggest that policies intended to create or maintain mixed communities through social housing have significant impacts on the neighborhoods in which they are located and that these externalities depend on neighborhoods and flats' characteristics. The goal of future work would be to evaluate the whole welfare effect of social housing policies. This raises several challenges. First, the subsidized supply of housing is costly and the potential long-run benefits for the social tenants are unclear. Second, the misallocation of the rent controlled dwellings due to the allocation through a queuing mechanism rather than to the households who value them the most is an important concern (Glaeser and Luttmer, 2003).

Figure 1.1: Yearly social housing inflows by provision methods in Paris 1970-2010



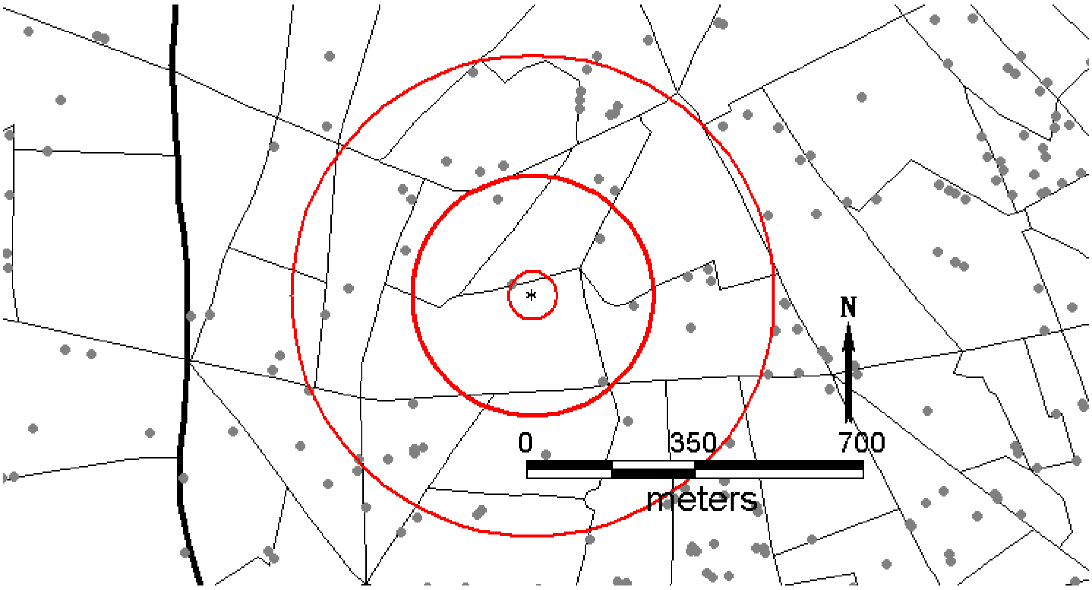
Note: Family dwellings subject to rent regulation: PLA-I, PLUS and PLS. Projects completed or to be completed before 2010.
Source: EPLS surveys 1998 to 2007 and City of Paris/APUR 2007.

Figure 1.2. Location of the social housing inflows in Paris up to 2010



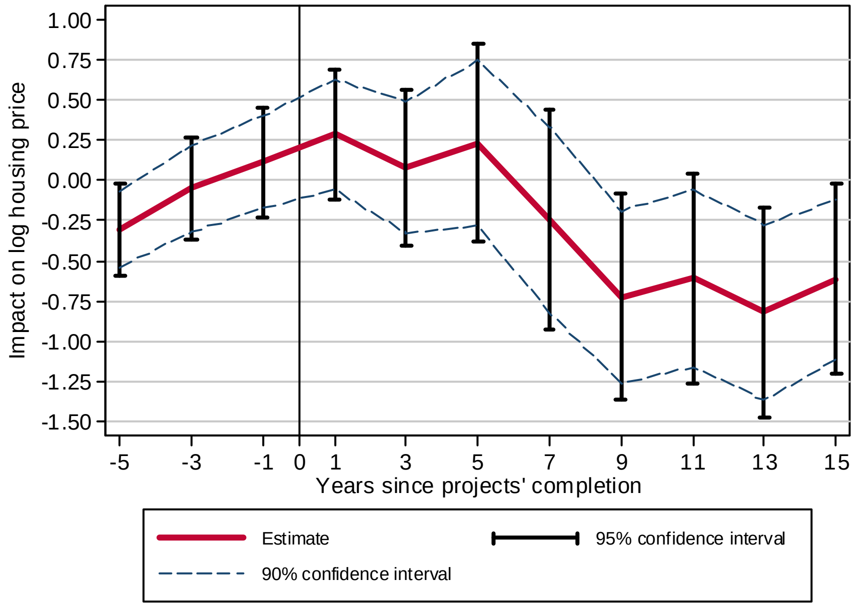
Note: Family dwellings subject to rent regulation: PLA-I, PLUS and PLS. Thick lines are boundaries between *arrondissements*, small lines are boundaries between *quartiers*. Projects completed or to be completed before 2010.
Source: EPLS surveys 1998 to 2007 and City of Paris/APUR 2007

Figure 1.3. Estimation of the share of social housing in the neighbourhood of a sale

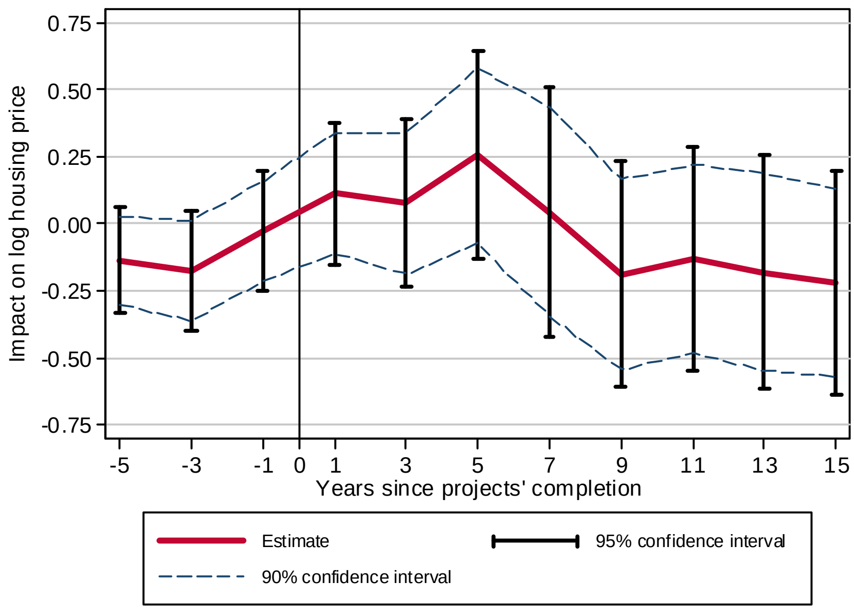


Note: 3 circles (50, 250 and 500 meters), grey dots represent social housing buildings created before 2010. Small lines are IRIS boundaries and the thick line is the boundary between the 13th and 14th *arrondissements*.
Source: EPLS surveys 1998 to 2007 and City of Paris/APUR 2007. Family dwellings subject to rent regulation: PLA-I, PLUS and PLS.

Figure 1.4. Impact of the social housing share on housing prices over time controlling for building unobservables
4.a. Social housing share at 500 meters

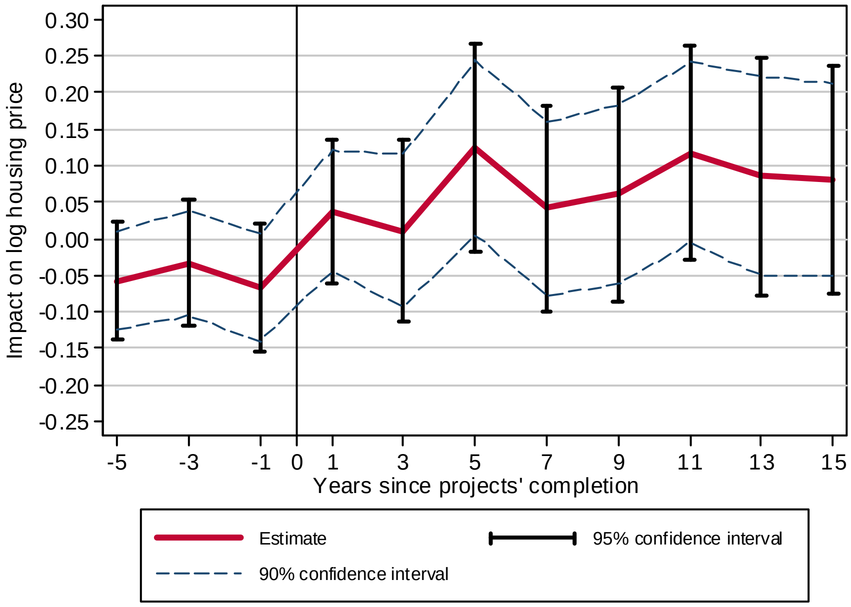


4.b. Social housing share at 250 meters

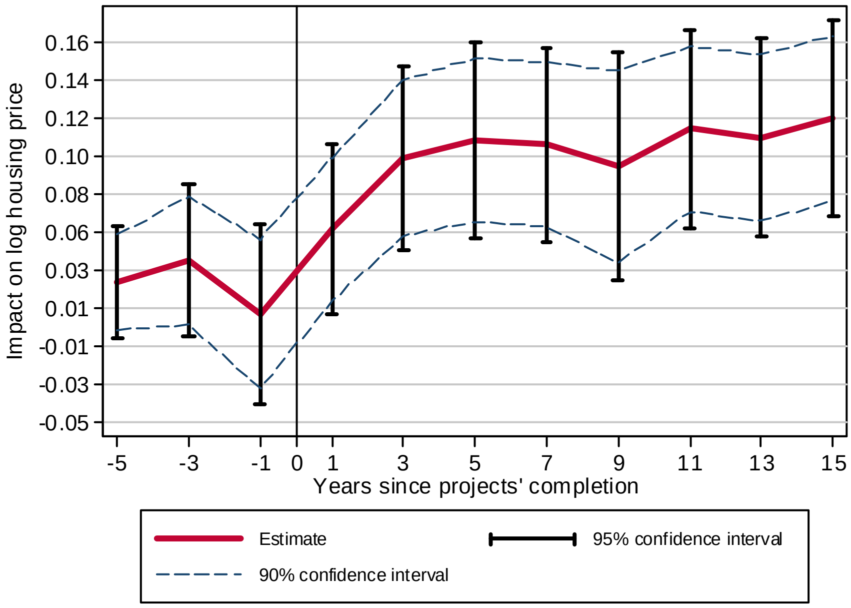


Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Figure 1.4. Impact of the social housing share on housing prices over time controlling for building unobservables
4.c. Social housing share at 150 meters



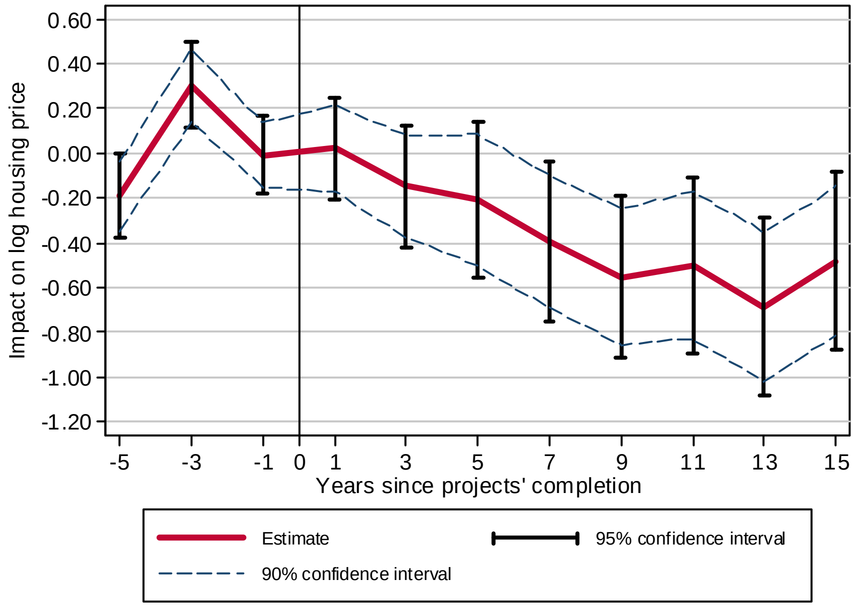
4.d. Social housing share at 50 meters



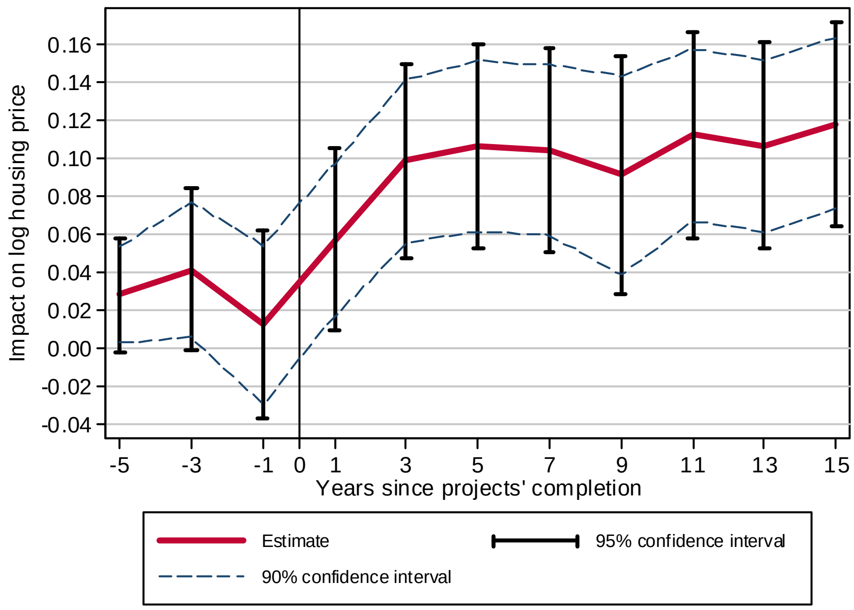
Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Figure 1.5. Impact of the social housing share on housing prices over time and by distance belts

5.a. Controlling for building unobservables (350-500 meter belt)



5.b. Controlling for building unobservables (50 meter circle)

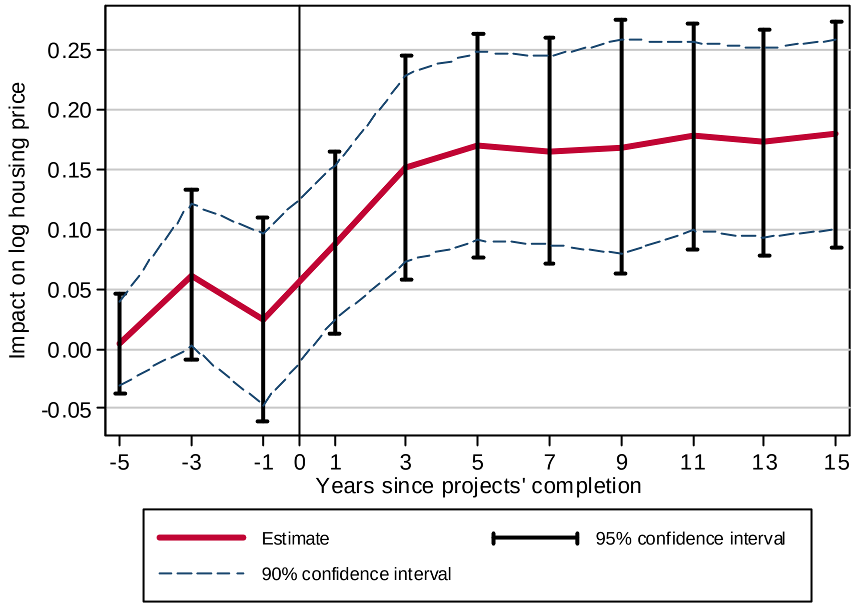


Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.

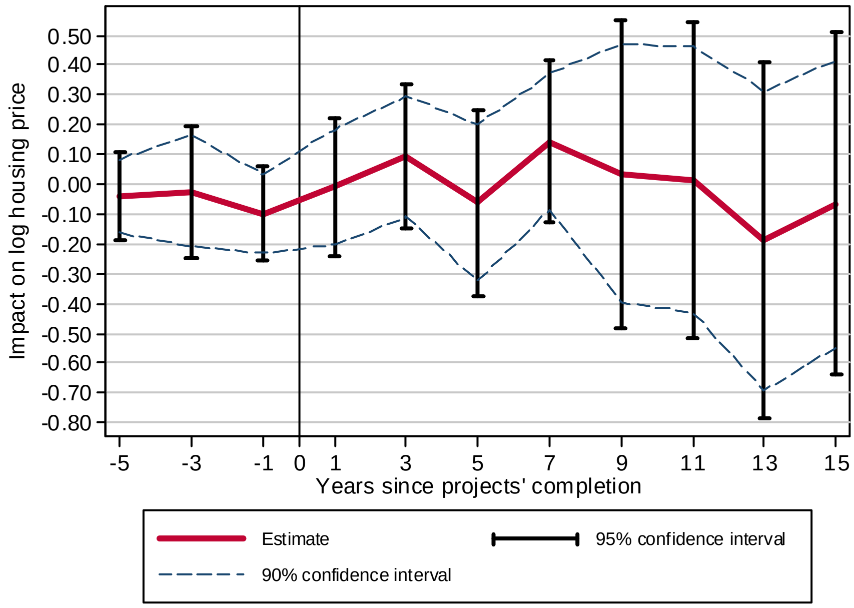
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).

Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Figure 1.6. Impact of the social housing share on housing prices over time by neighborhood initial housing prices (50 meters)
6.a. In low-income neighborhoods (1st quartile of housing price in 1995)



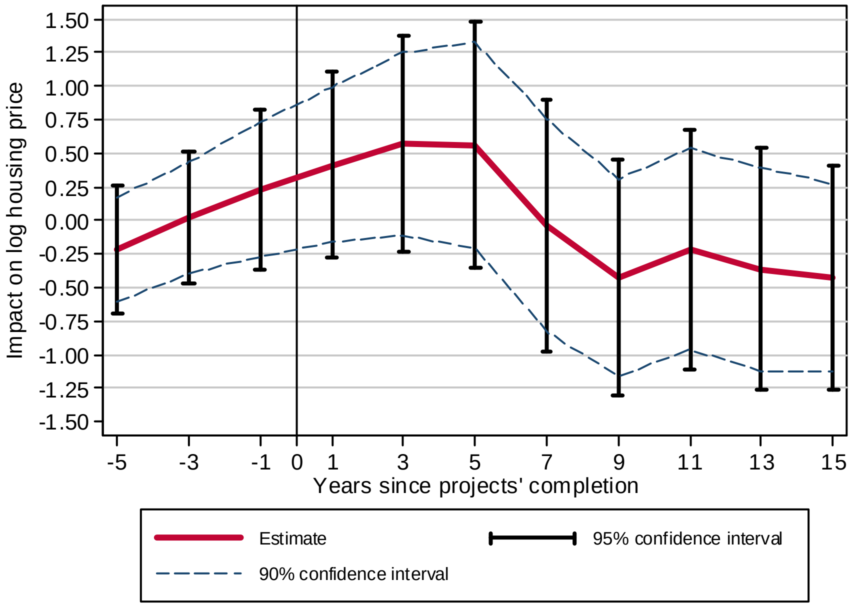
6.b. In high-income neighborhoods (4th quartile of housing price in 1995)



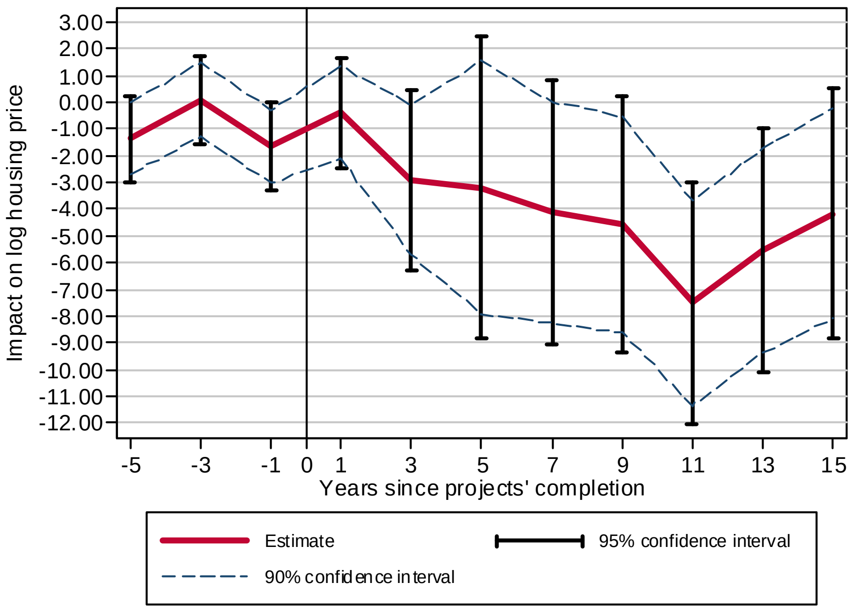
Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Figure 1.7. Impact of the social housing share on housing prices over time by neighborhood initial housing prices (500 meters)

7.a. In low-income neighborhoods (1st quartile of housing price in 1995)



7.b. In high-income neighborhoods (4th quartile of housing price in 1995)

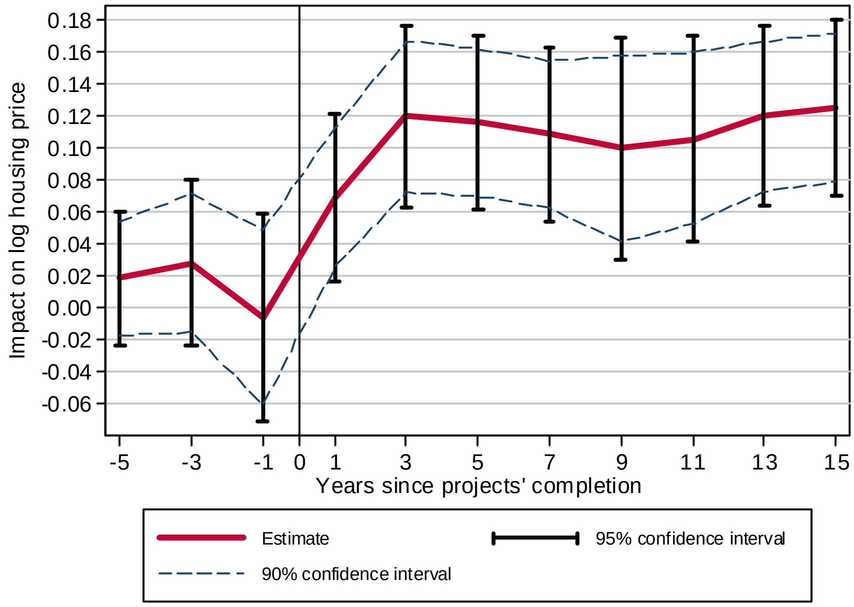


Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.

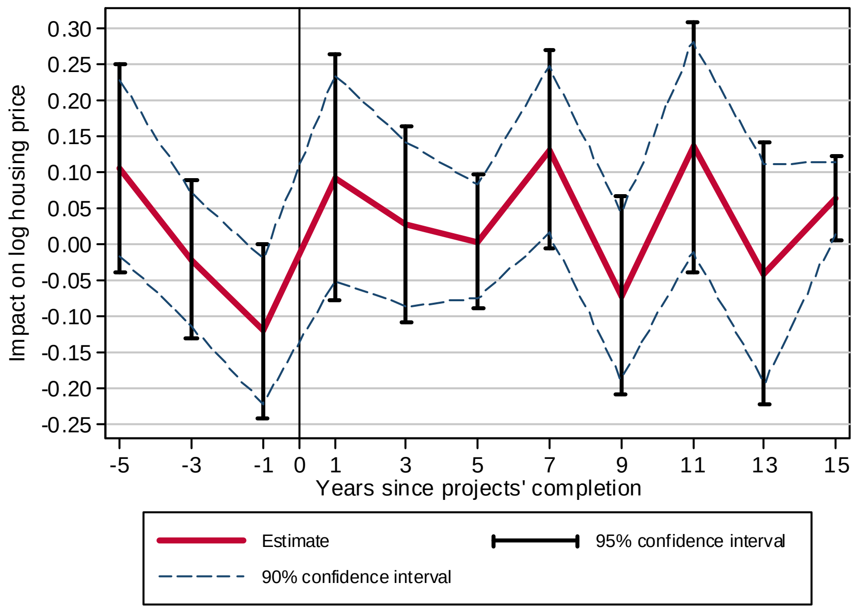
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).

Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Figure 1.8. Impact of the social housing share on housing prices over time by neighborhood initial housing prices (50 meters)
8.a. For small flats (one or two rooms)

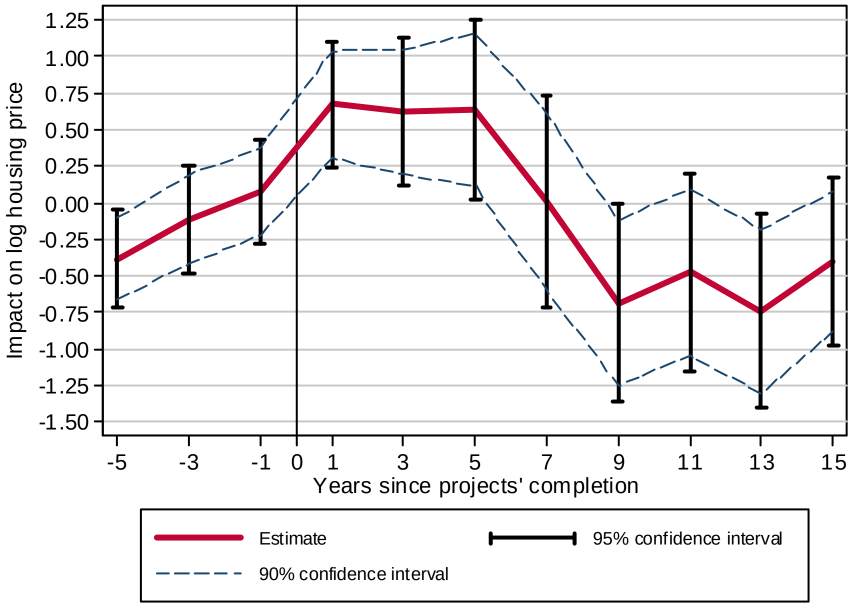


8.b. For large flats (five rooms or more)

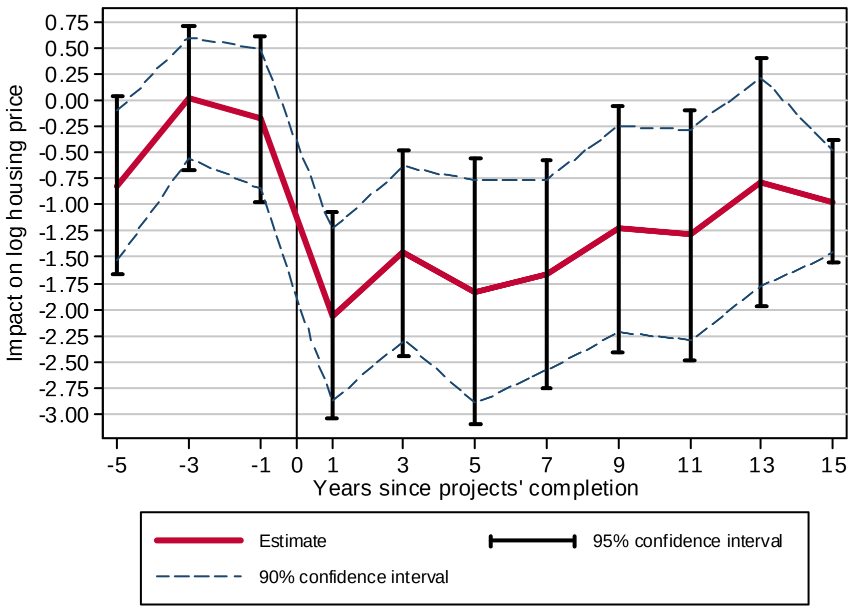


Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Figure 1.9. Impact of the social housing share on housing prices over time by neighborhood initial housing prices (500 meters)
9.a. For small flats (one or two rooms)



9.b. For large flats (five rooms or more)



Note: The figure represents the impact of the social housing share on housing prices over time including confidence intervals at the 95% level. The zero value on the horizontal axis defines the year in which the first social tenants moved in.
The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 and *quartier* linear trends (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.1. Public and Private dwellings and tenants in Paris in 2002

	Social tenants		New Social tenants (1)		Private tenants		Home owners	
Panel A. Dwelling's characteristics	**Mean**	**S.d.**	**Mean**	**S.d.**	**Mean**	**S.d.**	**Mean**	**S.d.**
Flat size (in m2)	61.80	23.79	58.48	23.41	49.04	32.35	71.19	37.86
Number of rooms	2.90	1.22	2.73	1.20	2.27	1.30	3.22	1.60
Number of dwellings in the building	66.51	81.73	52.25	50.04	32.05	61.35	53.99	113.79
Built before 1914	0.02	0.13	0.02	0.13	0.56	0.50	0.53	0.50
Between 1914 and 49	0.46	0.50	0.51	0.50	0.23	0.42	0.19	0.40
Between 1949 and 81	0.33	0.47	0.15	0.36	0.14	0.35	0.23	0.42
Between 1982 and 90	0.10	0.30	0.12	0.32	0.02	0.15	0.01	0.12
After 1991	0.09	0.29	0.20	0.40	0.05	0.21	0.03	0.18
Monthly rent (euros per m2)	6.17	2.37	6.84	2.48	13.88	6.26		
Years of tenancy/ownership	14.28	12.18	2.24	1.13	9.00	12.65	16.82	15.21
Panel B. Household's characteristics (2)								
Age	51.81	17.28	39.59	12.52	40.94	16.40	57.05	16.75
Foreign born	0.28	0.45	0.36	0.48	0.27	0.45	0.18	0.38
Couple without children	0.18	0.38	0.12	0.33	0.18	0.38	0.28	0.45
Couple with children	0.21	0.41	0.35	0.48	0.14	0.35	0.16	0.37
Single parents	0.15	0.35	0.19	0.39	0.06	0.23	0.03	0.18
Number of children	0.71	1.21	0.91	1.18	0.35	0.81	0.34	0.79
Without High School dipl.	0.53	0.50	0.48	0.50	0.27	0.44	0.25	0.43
Unemployed (if 18/55 yo)	0.04	0.19	0.07	0.25	0.02	0.15	0.02	0.14
Inactive (if 18/55 yo)	0.09	0.28	0.10	0.30	0.07	0.25	0.08	0.27
Income (euros by Eurostat UC)	16731	8768	15767	8591	24240	20173	32666	23161
Panel C. Building's maintenance and safety								
Flood damage last year (flat)	0.19	0.40	0.15	0.35	0.24	0.43	0.22	0.42
Degradation of common space (building)	0.38	0.49	0.26	0.44	0.18	0.38	0.21	0.41
Flat's robbery (or attempt of)	0.22	0.41	0.09	0.28	0.12	0.32	0.13	0.34
Think that neighborhood is not safe	0.23	0.42	0.14	0.34	0.12	0.33	0.15	0.36

Note: (1) New tenants moved in during the last 4 years. (2) For the head of the household. All statistics are weighted using the households' survey weights.
Source: French Housing Survey (ENL) in 2002, Paris.

Table 1.2. Summary statistics. Features of sales and social housing shares in 1995 and 2005 by selected samples

Sample:	All buyers and sellers		Buyers and sellers are private households		Buyers and sellers are private households	
	All sales		All sales		Repeated sales Within building	
	(1)		(2)		(3)	
Sold in year:	**1995**	**2005**	**1995**	**2005**	**1995**	**2005**
	Mean	**Mean**	**Mean**	**Mean**	**Mean**	**Mean**
	(s.d.)	**(s.d.)**	**(s.d.)**	**(s.d.)**	**(s.d.)**	**(s.d.)**
Panel A. Individual Flat characteristics						
Price (euros 2005)	153702	303099	142917	279375	140389	270321
	(168283)	(301588)	(142331)	(241548)	(133931)	(228837)
Log price	11.6	12.33	11.56	12.29	11.56	12.27
	(0.8)	(0.74)	(0.75)	(0.69)	(0.74)	(0.68)
Price per m2 (euros 2005)	2573.9	5241.3	2520.2	5207.5	2516.4	5165.0
	(1043)	(1504.5)	(859.4)	(1340.9)	(844)	(1308.8)
Flat size (in m2)	52.92	54.18	50.95	51.34	50.67	50.22
	(34.52)	(39.09)	(32.06)	(34.27)	(31.20)	(32.77)
Missing size	0.48	0.09	0.45	0.08	0.46	0.09
Previous ownership (years)	9.62	9.74	10.61	10.43	10.62	10.44
	(9.10)	(9.70)	(8.80)	(9.45)	(8.79)	(9.45)
Number of rooms						
One	0.23	0.24	0.24	0.24	0.24	0.24
Two	0.35	0.34	0.36	0.36	0.37	0.36
Three	0.21	0.21	0.21	0.21	0.21	0.22
Four or more	0.17	0.19	0.16	0.18	0.16	0.17
Unknown	0.03	0.03	0.02	0.02	0.02	0.02
Building age						
Before 1914	0.50	0.42	0.53	0.43	0.54	0.46
Between 1914 and 92	0.40	0.46	0.42	0.48	0.42	0.47
After 1992	0.04	0.04	0.00	0.02	0.00	0.01
Unknown	0.05	0.09	0.05	0.07	0.04	0.05
Having at least 1 bathroom	0.77	0.88	0.77	0.88	0.77	0.88
# bathrooms unknown	0.01	0.10	0.01	0.09	0.01	0.09
Having at least 1 parking lot	0.14	0.15	0.11	0.13	0.11	0.14
# parking lots unknown	0.10	0.01	0.10	0.01	0.10	0.01
Panel B. Share of social housing at the time of the sale (by vicinity, circles)						
Within 500 meters	0.10	0.12	0.10	0.13	0.10	0.13
	(0.10)	(0.11)	(0.10)	(0.11)	(0.10)	(0.12)
Within 350 meters	0.09	0.12	0.09	0.12	0.09	0.12
	(0.11)	(0.13)	(0.11)	(0.13)	(0.11)	(0.13)
Within 250 meters	0.09	0.11	0.09	0.12	0.09	0.12
	(0.13)	(0.14)	(0.13)	(0.14)	(0.13)	(0.14)
Within 150 meters	0.08	0.10	0.08	0.11	0.08	0.11
	(0.18)	(0.17)	(0.19)	(0.17)	(0.19)	(0.18)
Within 50 meters	0.07	0.09	0.07	0.09	0.07	0.09
	(0.46)	(0.37)	(0.51)	(0.38)	(0.53)	(0.35)
Share of social housing	0.08	0.10	0.08	0.10	0.08	0.11
within the same census tract	(0.13)	(0.16)	(0.13)	(0.16)	(0.13)	(0.16)
# observations	18,437	33,546	12,435	23,686	11,408	20,426

Note: The sample is restricted to the sales between private households for the sets of columns (1) and (2). Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.3. Cross sectional estimates of the effects of social housing on housing prices

	Dependent variable: ln(price in 2005 euros)				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Vicinity of the sales, within:	**500 meters**	**250 meters**	**150 meters**	**50 meters**	**Census tract**
Panel A. Years 1995 to 2005 without flat's controls					
Share of social housing	-1.404***	-0.883***	-0.474***	-0.066***	-0.667***
	(0.101)	(0.081)	(0.078)	(0.024)	(0.069)
Variation in the last 10 years	-7.440***	-2.883***	-1.075***	-0.113**	-1.574***
	(0.505)	(0.359)	(0.297)	(0.045)	(0.240)
Variation in the next 5 years	-7.092***	-2.282***	-1.105***	-0.263***	-0.922**
	(0.742)	(0.435)	(0.309)	(0.070)	(0.375)
Year times quarter dummies	Yes	Yes	Yes	Yes	Yes
Flat's controls	No	No	No	No	No
Panel B. Years 1995 to 2005 with flat's controls					
Share of social housing	-1.152***	-0.735***	-0.376***	-0.051***	-0.557***
	(0.058)	(0.047)	(0.053)	(0.019)	(0.038)
Variation in the last 10 years	-5.576***	-2.190***	-0.806***	-0.076**	-1.165***
	(0.276)	(0.224)	(0.198)	(0.031)	(0.129)
Variation in the next 5 years	-5.168***	-1.959***	-0.894***	-0.143***	-0.887***
	(0.431)	(0.255)	(0.166)	(0.034)	(0.213)
Year times quarter dummies	Yes	Yes	Yes	Yes	Yes
Flat's controls	Yes	Yes	Yes	Yes	Yes
# observations	208,918	208,918	208,918	208,918	208,918
# clusters	902	902	902	902	902

Note: Each cell is from a different OLS regression. * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered by census tract (IRIS). In panel B, a basic set of flat's and sales' controls is included in all the regressions. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction (see appendix table A2). The variation in the last ten years of this measure corresponds to the change between 1985 and 1995 for year 1995. The variation in the next five years of this measure corresponds to the change between 1995 and 2000 for year 1995.
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level. The sample includes only the sales between private households occurring within building with repeated sales.

Table 1.4. Estimates of the effects of social housing on housing prices with alternative geographical controls

	Dependent variable: ln (price in euros 2005)											
Vicinity of the sales, within:	**500 meters**			**250 meters**			**150 meters**			**50 meters**		
Panel A. Estimates without control for different price trends around the sales												
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Share of social housing	-0.603***	-0.276***	0.301*	-0.307***	-0.113***	0.107	-0.132***	-0.046***	0.088	-0.016***	-0.003	0.064***
	(0.044)	(0.046)	(0.177)	(0.027)	(0.018)	(0.099)	(0.019)	(0.010)	(0.057)	(0.005)	(0.002)	(0.018)
R-squared	0.863	0.871	0.911	0.862	0.871	0.911	0.861	0.871	0.911	0.860	0.871	0.911
Fixed effects	Quartier	Tract	Building	Quartier	Tract	Building	Quartier	Tract	Building	Quartier	Tract	Building
Quartiers trends	No	No	No	No	No	No	No	No	No	No	No	No
Sales controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Estimates controlling for different price trends around the sales												
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Share of social housing	-0.608***	-0.304***	0.157	-0.308***	-0.116***	0.097	-0.132***	-0.047***	0.081**	-0.016***	-0.003	0.058***
	(0.044)	(0.045)	(0.139)	(0.027)	(0.018)	(0.071)	(0.019)	(0.010)	(0.041)	(0.005)	(0.002)	(0.016)
R-squared	0.864	0.872	0.912	0.863	0.872	0.912	0.862	0.872	0.912	0.862	0.872	0.912
Fixed effects	Quartier	Tract	Building	Quartier	Tract	Building	Quartier	Tract	Building	Quartier	Tract	Building
Quartiers trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# observations	208,918	208,918	208,918	208,918	208,918	208,918	208,918	208,918	208,918	208,918	208,918	208,918
# clusters	902	902	902	902	902	902	902	902	902	902	902	902

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered at the census tract level. The sample includes only the sales between private households within buildings with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.5. Estimates of the effects of social housing on housing prices by distance to the sales

	Dependent variable: ln (price in euros 2005)					
	Without control for different price trends around the sales			**Controlling for different price trends around the sales**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Share of social housing within:						
Ring 350 to 500m	-0.199***	-0.081***	0.144	-0.202***	-0.094***	0.005
	(0.024)	(0.024)	(0.122)	(0.024)	(0.023)	(0.100)
Ring 250 to 350m	-0.137***	-0.080***	0.098	-0.138***	-0.087***	0.069
	(0.017)	(0.016)	(0.064)	(0.017)	(0.016)	(0.055)
Ring 150 to 250m	-0.156***	-0.086***	0.015	-0.157***	-0.091***	0.025
	(0.017)	(0.013)	(0.061)	(0.017)	(0.013)	(0.049)
Ring 50 to 150m	-0.080***	-0.055***	0.037	-0.080***	-0.057***	0.040
	(0.012)	(0.009)	(0.045)	(0.012)	(0.009)	(0.034)
Circle of 50m	-0.010***	-0.008***	0.062***	-0.010***	-0.008***	0.056***
	(0.003)	(0.002)	(0.017)	(0.003)	(0.002)	(0.016)
R-squared	0.863	0.871	0.911	0.864	0.872	0.912
Fixed effects	Quartier	Tract	Building	Quartier	Tract	Building
Quartiers trends	No	No	No	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes	Yes	Yes
# observations	208,918	208,918	208,918	208,918	208,918	208,918
# clusters	902	902	902	902	902	902

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered at the census tract level. The sample includes only the sales between private households within buildings with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.6. Estimate of the effects of social housing on the probability to sell a flat to a private buyer and the number of transactions

Panel A. Estimation	**Linear Probability Model**			
Dependent variable	**1 if sold to a private household / 0 otherwise**			
Sample	**All buildings**		**Buildings built before 1992**	
	(1)	(2)	(3)	(4)
Vicinity	500m	50m	500m	50m
Share of social housing	-0.066	0.006	-0.104	0.002
	(0.110)	(0.008)	(0.114)	(0.012)
R-squared	0.272	0.272	0.253	0.253
# observations	310,184	310,182	273,757	273,757
# Buildings	66,023	66,023	54,949	54,949
# clusters	924	924	903	903
Fixed effects	building	building	building	building
Quartier trends	Yes	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes
Panel B. Estimation	**Linear Count data Model**			
Dependent variable	**Number of sales in the building / 0 if no sale**			
Sample	**All buildings**		**Buildings built before 1992**	
	(1)	(2)	(3)	(4)
Vicinity	500m	50m	500m	50m
Share of social housing	-0.290	-0.016	-0.258	-0.015
	(0.259)	(0.017)	(0.280)	(0.019)
R-squared	0.396	0.396	0.405	0.405
# observations	732,499	732,499	618,541	618,541
# Buildings	67,325	67,325	56,231	56,231
# clusters	926	926	904	904
Fixed effects	building	building	building	building
Quartier trends	Yes	Yes	Yes	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered by census tract (IRIS). The sample includes all the sales in Paris. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction (see appendix table A2). Columns (3) and (4) exclude observation from sales occurring in buildings built after 1992 (1,757) and in buildings of unknown age (9,748).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.7. Heterogeneity of the effects of social housing on housing prices by neighborhood characteristics (circle of 50 meters)

	Dependent variable: ln (price in euros 2005)			
Panel A. Initial price level in the neighborhood (quartier) in 1995				
	Lowest quartile	**2nd quartile**	**3rd quartile**	**Highest quartile**
Share of social housing	0.091***	0.037	0.034	0.041
within 50 meters	(0.027)	(0.031)	(0.022)	(0.096)
R-squared	0.889	0.898	0.909	0.916
# Observations	77,201	56,714	46,850	27,038
# Clusters	297	239	211	152
Panel B. Initial social housing share in the neighborhood (quartier) in 1995				
	Highest quartile	**3rd quartile**	**2nd quartile**	**Lowest quartile**
Share of social housing	0.073***	0.047**	0.052	0.035
within 50 meters	(0.026)	(0.022)	(0.041)	(0.075)
R-squared	0.900	0.911	0.903	0.916
# Observations	72,781	78,181	32,931	23,910
# Clusters	326	290	135	148
Fixed effects	Building	Building	Building	Building
Quartiers trends	Yes	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered at the census tract level. The sample includes only the sales between private households within buildings with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.8. Heterogeneity of the effects of social housing on housing prices by neighborhood characteristics (circle of 500 meters)

	Dependent variable: ln (price in euros 2005)			
Panel A. Initial price level in the neighborhood (quartier) in 1995				
	Lowest quartile	**2nd quartile**	**3rd quartile**	**Highest quartile**
Share of social housing	0.202	0.089	0.000	-0.120
within 500 meters	(0.263)	(0.233)	(0.194)	(0.871)
R-squared	0.889	0.898	0.909	0.916
# Observations	77,201	56,714	46,850	27,038
# Clusters	297	239	211	152
Panel B. Initial social housing share in the neighborhood (quartier) in 1995				
	Highest quartile	**3rd quartile**	**2nd quartile**	**Lowest quartile**
Share of social housing	0.026	-0.030	1.417***	-1.331
within 500 meters	(0.192)	(0.193)	(0.535)	(0.993)
R-squared	0.900	0.911	0.903	0.916
# Observations	72,781	78,181	32,931	23,910
# Clusters	326	290	135	148
Fixed effects	Building	Building	Building	Building
Quartiers trends	Yes	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered at the census tract level. The sample includes only the sales between private households within buildings with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.9. Heterogeneity of the effects of social housing on housing prices by flats' characteristics (circles of 500 and 50 meters)

	Dependent variable: ln (price in euros 2005)			
Share of social housing	**within 500 meters**		**within 50 meters**	
Interacted with:	**(1)**	**(2)**	**(3)**	**(4)**
No variable		0.600***		0.082***
		(0.136)		(0.019)
Number of rooms is 1 or 2	0.299**		0.065***	
	(0.136)		(0.016)	
Number of rooms is 3 or 4	-0.022		0.051***	
	(0.140)		(0.016)	
Number of rooms is greater than 4	-0.417***		0.008	
	(0.142)		(0.018)	
Number of rooms		-0.190***		-0.010**
		(0.012)		(0.005)
Number of rooms is unknown	0.362*	-0.245**	0.060	-0.022
	(0.190)	(0.123)	(0.053)	(0.053)
R-squared	0.912	0.912	0.912	0.912
Fixed effects	Building	Building	Building	Building
Quartiers trends	Yes	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes
# Observations	208,918	208,918	208,918	208,918
# Clusters	902	902	902	902

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered at the census tract level. The sample includes only the sales between private households within buildings with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction and a series of quarterly dummies for each quarter from 1995q1 to 2005q4 (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.10. Estimates of the effects of the conversion projects on housing prices

	Dependent variable: ln(price in 2005 euros)			
	(1)	**(2)**	**(3)**	**(4)**
Vicinity of the sales, within:	**500 meters**	**250 meters**	**150 meters**	**50 meters**
Panel A. Effect of the conversion projects				
Share of conversion projects	-0.314	-0.055	0.023	0.032
	(0.340)	(0.135)	(0.066)	(0.033)
R-squared	0.924	0.924	0.924	0.924
# observations	116,105	116,105	116,105	116,105
# clusters	892	892	892	892
Panel B. Controlling for the share of other social housing projects				
Share of conversion projects	-0.510	-0.122	-0.062	0.017
	(0.354)	(0.142)	(0.074)	(0.034)
Share of other projects	0.500**	0.186	0.201**	0.055*
	(0.232)	(0.115)	(0.080)	(0.030)
R-squared	0.924	0.924	0.924	0.924
# observations	116,105	116,105	116,105	116,105
# clusters	892	892	892	892
Fixed effects	Building	Building	Building	Building
Quartiers trends	Yes	Yes	Yes	Yes
Sales controls	Yes	Yes	Yes	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard errors are clustered by census tract (IRIS). The sample includes only the sales between private households occurring within building with repeated sales. Sales' controls include: a cubic in size, dummy for unknown size, dummies by numbers of rooms interacted with unknown size, dummies for having a bathroom, a parking, a cellar, a lift interacted with the floor of the flat, dummies by periods of construction (see appendix table A2).
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Appendix.1.A. Data appendix

1.A.1. Social housing stock from EPLS surveys

The EPLS data-set distinguishes between several types of social housing subsidies which were available during different time periods. Differences over time are due to two main reforms in 1977 and in 1997. From 1977, new social housing projects are subject to a formal agreement between the State and the *HLM* companies called *conventionnement*. This agreement is a condition to the subsidies. The existing stock created before 1977 has been subject to various agreements in 1979, 1985 and 1995. In exchange for these subsidies, the *HLM* companies agree to have regulated rents and a limited choice of tenants. The agreement holds in most cases over the whole period of the subsidized loans and is tacitly re-approved. The agreement of the dwellings is the main condition for future tenants to be eligible to means tested benefits, *APL*. I have regrouped this different categories into four main groups according to their level of rents:

- Very low income tenants: *PLA-I* and *PLA d'intégration* (10), *PLA social* (12).
- Low income tenants: *PLUS* (13) created in October 1999 to replace the *PLA-LM/PLA-TS/PLAI* (11).
- Middle/low income tenants: *PLS* and *PLS/PPLS/PLA-CFF* (14), *ILM* (53), *ILN* (54).
- Stock before 1977: Other financing sources before 1977 (99), *HBM* (50), "Ordinary" *HLM* or *HLM-O* (52).
- *ANAH* subsidies (18).

The *EPLS* surveys take into account various form of subsidies to middle income tenants that are not considered as social housing by the 2000 law. I discard all the projects financed through a *PLI* (16), *PAP-locatif* (15), *PCL* (17) or other financing sources after 1976 (49). None of these subsidies is subject to a *conventionnement*.

In Paris, *HBM* buildings have been renovated from 1984. As substantial improvements were done to the buildings new agreements between the State and the *HLM* companies took place. In this paper, all the *HBM* units are considered entering in the social housing stock when they are built.

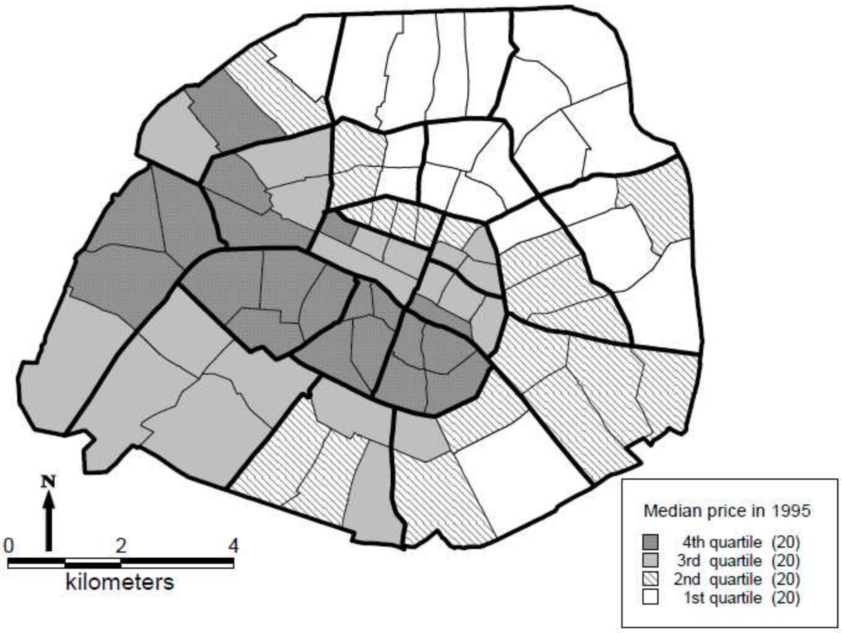
1.A.2. Estimation of the share of social housing at the local level

To estimate the number of dwellings in a circle of radius d around sale i, N_d, I use the French 1999 census. For each census tract, I know the number N_j of dwellings. Denoting the census tract polygons by (T_j) and the circle around the sale by C_d, I use the area operator, $a()$, to define:

$$N_d = \sum_j \frac{a(T_j \bigcap C_d)}{a(T_j)} \times N_j.$$

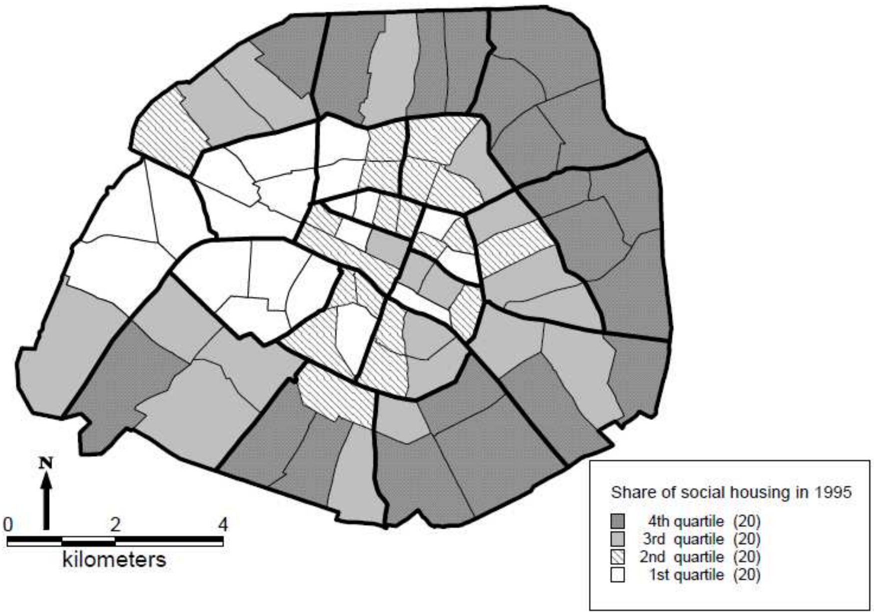
Appendices

Figure 1.B. Housing prices per square meters in Paris in 1995 by *quartiers*



Note: The sample includes only the sales between private households in 1995 with information about price and surface. Thick lines represent the boundaries of the 20 *arrondissements*.
Source: BIEN dataset.

Figure 1.C. Social housing share in Paris in 1995 by *quartiers*



Note: Thick lines represent the boundaries of the 20 *arrondissements*.
Source: DREIF EPLS surveys 1998-2007 and 1999 census.

Table 1.D. Summary statistics. Social housing share for the sample of sales 1995-2005 by circles and belts

Year	1995	2005	1995-2005		1995	2005	1995-2005
	Mean (s.d.)	**Mean (s.d.)**	**Mean (s.d.)**		**Mean (s.d.)**	**Mean (s.d.)**	**Mean (s.d.)**
Vicinity	**Within circles**			**Vicinity**	**Within belts**		
500 m	0.10	0.13	0.11	350-500 m	0.10	0.13	0.12
	(0.10)	(0.12)	(0.11)		(0.11)	(0.13)	(0.12)
350 m	0.09	0.12	0.11	250-350 m	0.10	0.13	0.12
	(0.11)	(0.13)	(0.12)		(0.13)	(0.15)	(0.14)
250 m	0.09	0.12	0.10	150-250 m	0.09	0.12	0.11
	(0.13)	(0.14)	(0.13)		(0.14)	(0.16)	(0.15)
150 m	0.08	0.11	0.09	50-150 m	0.08	0.11	0.10
	(0.19)	(0.18)	(0.18)		(0.20)	(0.19)	(0.18)
50 m	0.07	0.09	0.08	50 m	0.07	0.09	0.08
	(0.53)	(0.35)	(0.49)		(0.53)	(0.35)	(0.49)
Within census tract	0.08	0.11	0.09	Within census tract	0.08	0.11	0.09
	(0.13)	(0.16)	(0.14)		(0.13)	(0.16)	(0.14)
# observations	11,408	20,426	208,918		11,408	20,426	208,918

Note: The sample is restricted to the sales between private households within buildings with repeated sales.
Source: BIEN dataset, DREIF EPLS surveys 1998-2007, City of Paris records. 1999 census at the IRIS (tract) level.

Table 1.E. Summary statistics. Characteristics of the repeated sales within buildings

Variable	Mean (Std. Dev.)
Price (euros 2005)	173,650.9
	(173,133.1)
Log (price)	11.743
	(0.784)
1 room	0.236
2 rooms	0.367
3 rooms	0.219
4 rooms	0.101
5 rooms or more	0.060
Number of rooms unknown	0.017
1 room and unknown flat size	0.054
2 rooms and unknown flat size	0.084
3 rooms and unknown flat size	0.050
4 rooms and unknown flat size	0.024
5 rooms or more and unknown flat size	0.016
Rooms and flat size unknown	0.007
Flat size (0 if unknown)	38.274
	(35.445)
Flat size squared/100	27.212
	(58.153)
Flat size cubed/10000	27.941
	(147.307)
Flat size unknown	0.235
At least one bathroom	0.825
Number of bathrooms unknown	0.032
At least one parking space	0.126
Number of parking spaces unknown	0.140
Having a lift	0.458
Having a cellar	0.715
Ground floor	0.079
1st floor	0.152
2nd floor	0.161
3rd floor	0.157
4th floor or higher	0.435
Floor unknown	0.016
1st floor and lift	0.059
2nd floor and lift	0.063
3rd floor and lift	0.063
4th floor or higher and lift	0.060
Floor unknown and lift	0.007
Period of construction	
1850 or before	0.053
1850 / 1913	0.454
1914 / 1947	0.157
1948 / 1969	0.136
1970 / 1980	0.123
1981 / 1991	0.018
1992 / 2000	0.008
After 2001	0.001
Unknown	0.050
# observations	208,918

Note: The sample is restricted to the sales between private households within buildings with repeated sales. Source: BIEN dataset.

Chapter 2

Social housing location, and labor market outcomes

1 Introduction

Economists and policymakers have shown increasing interest in the importance of neighborhood effects in a variety of contexts, including schooling, labor market outcomes and crime[1]. There is now a large and expanding literature investigating the impact of the characteristics of one's neighborhood on individual outcomes. A pervasive problem in the literature on neighborhood and peer effects is identification. Households may choose their location according to partly unobservable characteristics related to educational and labor market outcomes. Thus the measured effects are likely to be biased (Oreopoulos, 2008).

This chapter examines the impact of neighborhood quality on welfare recipients' labor market outcomes in Paris between 2001 and 2007. I take advantage of the quasi-random assignment of households to social housing units through a known administrative process. In Paris, social housing applicants are allocated on the basis of their preferences among twenty large areas, the *arrondissements*, together with several observable variables. I present evidence that conditional on these observable characteristics and the preferred *arrondissements*, new social housing tenants are randomly assigned to neighborhoods. I use the variation in neighborhood quality obtained through this natural experiment to compare the medium-run labor market outcomes of tenants allocated to different social housing units.

The effect of social housing location on the labor market outcomes of its tenants is of interest for several reasons. From a policy perspective, considerable effort has been made to improve the location of the public housing units (see Currie, 2006, for the USA, Cheshire et al., 2008, for the UK and Laferrère and le Blanc, 2006, for France). These mixed communities' policies are based on the underlying belief that peer effects or the proximity to jobs could influence the labor market outcomes of social housing tenants. The relocation of welfare recipients to social housing units may allow households to move to lower-poverty areas. This could impact their

[1]See among recent examples: Oreopoulos, 2003, Kling et al., 2007, Currie et al., 2010, Kling et al., 2005, the review of Oreopoulos, 2008, and references therein.

labor market outcomes through four main mechanisms. First, peers and social networks may influence the individual outcomes on the labor market through contagion effects (Akerlof and Kranton, 2000, Crane, 1991) or informational effects (Granovetter, 2005, Montgomery, 1991). Second, the new location may avoid the discrimination against the initial neighborhood of residence (Kain, 1968, Zenou, 2002). Third, some local public goods such as local unemployment agencies or greater access to social services (e.g. pre-school childcare) and transport networks may have positive impacts on welfare recipients' labor market outcomes. Fourth, for new social housing tenants, the move itself may have an adverse impact due to the mobility costs or if the new households have characteristics that are not adapted to their new neighborhoods or suffer from higher level of discrimination.

In order to investigate the extent of these neighborhood effects, I combine information about welfare recipients' residential location and employment with information on neighborhood characteristics. As I do not observe the households' applications to social housing, I identify the timing of their moves from private accommodation to the social sector. To the extent that the allocation to a particular social housing unit is unrelated to unobservable household characteristics, one can use the natural experiment created by the application process to measure the impact of neighborhood on the labor market outcomes of welfare recipients. I find that welfare recipients who obtain a social housing unit are located close to their previous neighborhoods. Welfare recipients that get allocated to better neighborhoods experience slightly higher job finding rates. My estimates indicate that a decrease of one standard-deviation of the allocated neighborhoods' unemployment rate (5 percentage points) increases the exit from welfare by around 1.5 percentage points after 18 months and that this increase in welfare exits is associated with an increase in the job finding rate by around 3 percentage points. These figures represent substantial effects for welfare recipients, whose baseline transition rates are fairly low. In particular, they represent an increase of 8% and 17% in their average exit rate from welfare and in their job finding rate, respectively. However, there is no significant improvement in wages or in the long-term exits from the welfare program. The main neighborhood effects seem to be driven by an increase in the turn-over between the welfare program and temporary part-time contracts. Moreover, the impact of neighborhoods appears highly heterogeneous among individuals. Women experience significant positive impacts on their job finding rates. The welfare exits of women increases by around 3 percentage points after 18 months if the unemployment rate of their allocated neighborhoods decrease by one-standard-deviation. This effect is twice as large as the average effect in the population and I do not find any significant impacts of neighborhood characteristics on men labor market outcomes.

This chapter builds on the existing studies of the impact of neighborhood on labor market outcomes. The main empirical evidence is based on the Moving To Opportunity program (MTO). The MTO program was authorized by the US congress in 1992 and took place between 1994 and 1998. The program randomly allocates housing vouchers to around 4,600 volunteer poor households living in public housing projects. Households were divided into three groups. The experimental group was given vouchers only for relocation in census tracts with fewer than

10% of households below the poverty line, while the control group was not offered any voucher and a third group was offered vouchers without any constraints. Kling et al. (2007), Katz and Kling (2004) find no significant neighborhood effects of the MTO program on economic self-sufficiency or physical health but significant benefits on mental health. They also point out that female youths are more affected than men by neighborhood quality. Kling et al. (2005) investigate the impact of neighborhood on criminal behaviors and find also that female youth relocated to better neighborhoods tend to commit less violent crime and property crime while for males the reduction of crimes concerns only violent crimes in the short-run.

A second stream of the empirical literature uses social housing as a source of quasi-experimental variation in neighborhood quality. Jacob (2004) observes the consequences of the closure of high-rise public housing units in Chicago's low-income neighborhoods on children's outcomes. Households living in public housing projects set for demolition were offered housing vouchers to move. Children affected by the demolitions did not better than their peers on a wide variety of achievement measures. In France, Goux and Maurin (2007) use the distribution of the dates of birth in the group of the individual's young close neighbors to predict the average rate of early school success of the neighbors of the individual, they find some evidence of strong positive peer effects. Their estimates suggest that a one standard deviation increase in the proportion of young neighbors being held back at the age of 15 would raise the probability of being held back by 11% at the age of 16. Goux and Maurin (2007) also use the stock of social housing tenants in order to identify aggregated neighborhood and peer effects. The local share of high-school dropouts and the proportion of unemployed households appear strongly related to the probability of being held back a grade at the age of 16. On the contrary, Oreopoulos (2003) finds no relationship between childhood neighborhood quality and future earnings, unemployment likelihood or welfare participation in Toronto. He uses the allocation to public housing projects as a source of quasi experimental variation in childhood neighborhoods. He tracks children assigned to different neighborhoods, but he does not find any significant impact on long-run labor market outcomes for various metrics of neighborhood quality: local levels of parental education, share of single parents and welfare recipients.

My approach differs from that of these papers along two main dimensions. First, the results of my quasi-experimental identification strategy are complementary to the Gautreaux and Moving To Opportunity findings (Kling et al. 2007, Rosenbaum 1995). I study the impact of social housing location as opposed to the effect of housing vouchers. The social housing policy is likely to generate more important variation in neighborhood quality than housing vouchers. The location choices of the households have no impact on their budget constraints as the rent of social housing flats is regulated for the whole municipality. Moreover, households' location are not restrained by any possible discrimination in the private housing market. The natural experiment takes also place in a different part of the economic cycle as the MTO experiment. Second, as Oreopoulos (2003), I focus on neighborhood variation created by the location of social housing units and this strategy identifies the effect of social housing policies that aim at creating or maintaining mixed communities. However I do not focus on the childhood neighborhood, I investigate the neighborhood impacts on medium run labor market outcomes up to two years after the households moved in social housing.

The remainder of this chapter is organized as follows. The next section describes the selection process of social housing applicants and the data. Section 3 discusses the empirical framework and econometric concerns. Section 4 describes the main results and discusses the heterogeneity of neighborhood effects across welfare recipients. Section 5 concludes.

2 Institutional background and summary statistics

2.1 Allocation to social housing units

The Parisian social housing system is based on rental units subsidized by low interests loans and tax deductions. The social housing stock represents 15.4% of the occupied housing stock in Paris (APUR, 2008). Housing units are owned by private local companies, *HLM*[2]. Despite their private status, these companies are closely monitored by the central government and the municipality, that sometimes contributes to rehabilitation, maintenance or demolition of buildings. Moreover, in Paris, the municipality is the main joint owner of the largest *HLM* companies.

Project-based assistance is used by *HLM* companies to create new social units either through subsidized construction, rehabilitation or conversion of private buildings. The allocation process of the dwellings and social housing (voluntary) applicants is complex. As a results of their financing part in the different social projects, each financing institution gets some rights on the social housing stock (Laferrère and le Blanc, 2006). About the half of the Parisian social rented housing stock is allocated to working people by their firms through the private workers' housing scheme (*1% logement*). Concerning the other half of the social housing stock, the reservation rights are shared by two main administrative entities: the municipality and the *Préfecture* of Paris. The *Préfecture* of Paris proposes around 18% of the candidates and the municipality around 32% of the candidates. The two administrative entities partly use their nomination rights to provide affordable accommodations to civil servants[3].

The eligibility of the households to social housing in Paris is a function of the family structure and the total household income during the penultimate year. All the applicants for social housing have to submit a unique application form. This application form can be downloaded on municipality website and it is also available in each of the twenty *arrondissement* city halls. The applicants have to give information on: income, handicap, the healthfulness and crowding of the actual accommodation, age (a priority is given to young household) and actual rent. Households can rank their preferred locations at the *arrondissement* level[4]. Once the form is

[2] *Habitations à loyer modéré.* Several French administrative bodies use slightly different definitions of the social housing stock (Préfecture de Paris, 2007, CNIS, 2001 and Briant et al., 2010)

[3] The local administrative process of allocation is determined by law since 1986. The goal of the allocation process and the composition of the allocation commissions at the municipality level changed marginally in 1996 and 2005. Furthermore, for the municipality of Paris, one half of the available units are allocated by the central authority, while the other half is allocated through *arrondissement* authorities.

[4] The application form is reproduced in appendix, figure 2.A1. The application form was changed in 2008. The default answer is now being indifferent between all the *arrondissements*.

completed the household is registered on the waiting list. In 2005, there were around $100,000$ applicants and $4,000$ social dwellings were allocated.

As soon as a social unit becomes available, local commissions determine the nominated applicants. Commissions use only information from the application form. For each available dwelling, the commissions rank three eligible households and submit this list to the social landlord in charge of the dwelling. The first successful applicant is contacted by the social landlord who checks the eligibility of the applicant. If the social landlord agrees with the commission, the household can move in or refuse the dwelling to re-enter the waiting list[5]. In case of refusal by the first applicant, the second applicant is offered the dwelling and the first applicant goes down in the priority list[6].

The final neighborhood allocation is driven by the first two steps of this administrative process. Due to the scarcity of available lettings, the commissions have little control on the final location of the households. Moreover, welfare recipients meet always the income conditions, so that eligibility and allocation are determined by the family structure and the interplay of households' location choices and available dwellings. Most of the choices of the households correspond to their current locations or central *arrondissements*. Due to the length of the waiting list and the relative low rent of the social units[7], the compliance to the assigned social housing units is near perfect for low income households. Table 2.1 presents the rate of refusal of social housing units according to the French Housing Surveys in 1996 and 2002. In average, the rate of refusal of welfare recipients is 0% and around 2.6% for low-income households. Unfortunately, the sample sizes are small but this provides suggestive evidence that households' non compliance to the assigned social units is unlikely to be a severe source of bias.

The administrative process and the high demand of social dwellings help identify the impact of location on labor market outcomes. Under perfect compliance to the administrative allocation process, the location choice of the households is fully determined by the characteristics in the application form. As the social housing stock is spatially unbalanced, this process generates large variation in the allocated neighborhoods. Most of the high-rise and medium-rise social housing developments conducted in the sixties and in the seventies were built in the periphery of the municipality to take advantage of affordable land prices and new public equipments. However, after the election of a new mayor, Bertrand Delanoë, in March 2001, the municipality accepted numerous financial efforts in order to create mixed-income neighborhoods and increase the social rented stock. The goal is to reach the 20% of public housing in every of the twenty *arrondissements* of Paris before 2020. The challenge is important as social housing units represented only 13.4% of the primary residences at the beginning of 2001. The municipality took

[5] At this stage, a main reason for the landlords' refusals are potentially too high incomes for a particular type of public housing, but this does not apply to welfare recipients.

[6] Information leaflets associated to the application form explain that a non-motivated refusal would either downgrade the priority of the application or place the household in the last position of the waiting list.

[7] The rent in the private sector is twice as high as in the public sector. In Paris, there are three rent levels according to the financing system of the housing unit. In 2006, they are 5.25 euros/m^2, 5.90 euros/m^2 and 8.85 euros/m^2 while the rent on the private sector is on average 20.70 euros/m^2 (commission of Parisian notaries). As a result, the supply of social dwellings is dried by a very low turnover rate (5% in Paris in 2006 against 10% at the national level, and 18% in the private sector).

two main actions. First, the agencies in charge of social housing in Paris have been mobilized to produce more new accommodations, using state or city properties. Second, an inclusionary zoning was partially implemented since 2001 and voted in 2006. Every new large housing project should present a level of 25% social housing units[8]. As a result of these policies, the number of social housing residences has increased by $13,079$ units between January 2001 and 2005 while the stock of public housing represented $167,393$ primary residences the 1^{st} January 2005 or 15.4% of all the dwellings (APUR 2008). Thus the supply of available social dwellings is driven by both significant inflows of new projects and the existing stock. This guaranties large variation in neighborhoods.

2.2 Data and summary statistics

To measure the impact of public housing location on the labor market outcomes of the poorest households in Paris, I combine a unique exhaustive French administrative data set which contains the variables used in the administrative process of social housing allocation and neighborhood level information.

The empirical analysis is based on *the welfare recipients of the French minimum income*, the *RMI*. The *RMI* was the French guaranteed minimum income until 2009 (Bourguignon, 2009). This welfare program is accessible to any individual aged 25 and over, provided that the sum of all resources available to his or her household is below a threshold that depends on family composition. The administrative data is collected by the French Institute in charge of payment of welfare, family and housing benefits and its local agency (the *CNAF* and the *CAF* of Paris). The sample covers all the *RMI* recipients from June 2001 to December 2005 in the municipality of Paris. The employment history of this sample of households is observed until June 2007. The longitudinal data set is constructed using the cross-sectional administrative information on employment and welfare status collected every 6 months from June 2001 to December 2004 and every 3 months from December 2004 to June 2007.

Each observation corresponds to a household's situation either during a welfare spell or during the subsidies period when one member of the household finds a job, or is working in a subsidized job[9]. I observe the moves from private accommodation to social housing from cross-section to cross-section. As social housing benefits are paid directly to the social landlords, I define a move to the social housing sector as a change from a private sector dwelling to a dwelling rented by a social landlord. I identify social housing as the rental social units which give right to housing subsidies for the tenants and belong to or are administrated by social landlords. This definition does not include student halls, temporary accommodations for young and poor workers or elderly halls which have different allocation rules.

My explanatory variable of interest is the quality of neighborhoods. Each year the data are localized at the census tract level. I use two external measures of neighborhood quality:

[8]New housing projects including social housing can have higher density than private developements. Three historical areas are excluded of this new zoning regulation: the Sénat, Panthéon and Marais.

[9]The subsidy period is known as *Intéressement*. Subsidized contracts include the part-time CI-RMA in the private sector and the CAV in the non-profit sector.

the unemployment rate from the 1999 census and the median income that are available at the census tract level from 2001 to 2005[10]. These measures of neighborhood quality includes all the households of the neighborhood. I decide first to focus on the unemployment rate in the census 1999. The quality of this indicator depends on the persistence of spatial inequalities over time in particular for the last cohorts of observations in 2004 and 2005.

Table 2.2 investigates the linear and rank correlations between the unemployment rate, the long-term unemployment in 1999 and the median taxable incomes in 2001 and 2005. The unemployment rate in 1999 is strongly linearly correlated with all these measures, the lowest linear correlations being of order 0.8. Comparing the linear correlations to the rank correlations (in backets), there is no evidence of non-linear relationships between the different measures of neighborhood quality[11]. There is also no significant changes in the correlations when I compare the income measures in 2001 and 2005. The correlation between the neighborhood unemployment rate in 1999 and the median income in 2001 is 0.77 and 0.76 when the median income is measured in 2005. These correlations suggest that the unemployment rate is a stable scalar index of neighborhood quality which is linearly related to the overall neighborhood quality. A similar method is used by Kling et al. (2007). They summarize the quality of neighborhoods using the poverty rate. Under a unique metric of neighborhood quality, a change in the value of this metric, the neighborhood unemployment rate, may imply a change of other non-orthogonal neighborhood characteristics that may be important for the job search process of the individual such as job accessibility or the education of close neighbors. Thus the results can not be interpreted as the effect of the unemployment rate on labor market outcomes holding every other neighborhood characteristics constant. An alternative technique would be to create a weighted index of neighborhood quality but these metrics have two disadvantages. First, they have no clear scale. Second, they are not comparable over different studies. I prefer to introduce only the neighborhood unemployment rate as it is a directly interpretable indicator. However, this may cause a loss of power to discriminate "good" and "bad" neighborhoods if the underlying assumption of correlation of this indicator with an underlying linear neighborhood quality index is violated.

My potential control variables include virtually all the characteristics asked in the application form: age, number of children, marital status, past income level, some past labor market outcomes and the current housing rent[12]. However I do not observe the location preferences of the households and I do not know if the final allocation to a given *arrondissement* corresponds to the choice expressed in the application form. Alternatively I choose to control for the *arrondissement* of the social housing unit and the past location of the household.

Table 2.3 provides broad descriptive features of the data on welfare recipients. The first col-

[10]Income data at the census tract level are not available before 2001. Income data in year n is coded at the census tract of residence on the 1[st] January of year $n+1$.

[11]Other non-reported correlations with the neighborhood social housing share, the education of the neighbors or the quality of the housing stock present similar patterns.

[12]The quality of the current accommodation of the households is defined by several proxies rent and location - while the public housing application form contains more precise information on the current housing conditions of the applicants.

umn presents the descriptive statistics for the whole sample while columns (2) and (3) focus on single women and single men. I observe 2,178 adult individuals in 1,686 households relocated to social housing units in 407 different census tracts over the period 2001 to 2005. Panel A presents the descriptive characteristics of these individuals. Single women are overrepresented among the individuals allocated to social housing units and represents nearly 37% of the sample. This is explained by the priority given to single family households in the social housing application process. 63% of the single women live with children and 21% of them have children of less than six years old. Couples with children represent also a large share of the sample. Overall 56% of the individuals live with children when they move to a social housing units. A large fraction of individuals are from Non-European countries (41%) while the fraction of European individuals is marginal (2%). New social tenants are in average 44.5 years old, single men being older than the other successful applicants by nearly two years. The past unemployment history of these households is important. The average duration of a welfare spell is 4.5 years at the time of entry into social housing. The inflows of social housing tenants are stable over the period 2001 to 2005, each cohort represents nearly 10% of my final sample.

Panel B of table 2.3 describes the observed past labor market outcomes before the allocation to a social housing unit. 10% of the new public tenants had some form of employment six months before their allocation to a social unit. A large part of them, 53%, did not have any taxable income in the penultimate year before the social housing allocation. Their average yearly income is low below 3,000 euros. This matches the long duration of the welfare spells. Panel C presents the past housing conditions of the new social housing tenants. Only a small fraction of them paid some form of rent. The observed rents are low in average 370 euros per month and slightly higher for couple and single women than for single men.

Finally, Panel D of table 2.3 presents the variation in the explanatory variable of interest, the unemployment rate of the census tract in 1999. In average, welfare recipients are allocated to neighborhoods where the unemployment rate was 16% in 1999. This is substantially larger than the unemployment rate of the average Parisian census tract (12%). The standard deviation of the neighborhood metric is 0.05. The distribution of the unemployment rate within the allocated neighborhoods is nearly symmetric. The bottom 10th percentile of the allocations corresponds to an unemployment rate of 9.5% while the 90th percentile represents an unemployment rate of 23.4%. There is no noticeable difference between the neighborhoods allocated to single women, men and couples. The last two rows compare the variation within the *arrondissements* of allocation and within the households coming from the same and moving to the same *arrondissements*. Nearly 60% of the unemployment rate variation occurs within the *arrondissements* of allocation. Even controlling for both past and current *arrondissements*, the variation in the neighborhood quality is still substantial and represents more than 50% of the overall variation in local unemployment rates. This is partly due to the low mobility of the households between *arrondissements*. In my sample, 59% of the individuals get allocated a social unit in their current *arrondissement*. Interestingly, this proportion is smaller (32%) in the most deprived *arrondissements*, the 18, 19 and 20, which have the largest numbers of social housing units. This suggests that households' preferences play indeed a substantial role in the final allocation. As my metric of neighborhood quality is likely to be subject to some kind of measurement error, it is reassuring that my results will not be driven by small changes in neighborhood characteristics and attenuation bias when I control for the *arrondissements* of allocation.

I focus on three main labor market outcomes of the successful social housing applicants after their relocation to social housing units: the exit of welfare, the job finding rate and the wage of the individuals who find a job. The transitions from the minimum income program, *RMI*, to the program designed for new single parent of children of less than three years old (API), are aggregated into the same welfare spell. As the households receive some extra transfer when they find a job, I identify both their welfare exits and the timing of their job findings in their administrative records. Unfortunately, some individuals may quit the welfare programs for reasons not related to employment: change of household structure, geographic mobility or because they do not fill the required quarterly forms. I construct three main dependent variables: dummy variables for not receiving welfare benefits after n months, dummy variables for having found a job before n months and an earnings variable. The exit of welfare and the employment variables are defined 6, 12, 18 and 24 months after the relocation. The employment measure aggregates all the possible employment spells of the individuals[13]. I consider that this measure is subject to attrition when the individuals are not in any welfare program after n months but they did not benefit from any period of cumulative wage and welfare benefits. Finally, there is no direct measure of the wage in the dataset but it contains a variable for the monthly earnings. For the employed people, this is very close to their wages. When the individual are in couple, I know the identity of the individual working and I impute him the full amount of earnings. If the two adults of the same household are working at the same time, I impute half of the earnings to each of them (this occurs only for 3 households in my sample). I assume that the job finding hazard rate is constant and I convert this variable into an expected monthly wage when the individuals find a job by multiplying it by 2/3.

Table 2.4 presents the welfare exits and employment transitions after the allocation to a social housing unit for the different categories of welfare recipients. Panel A, after 24 months, 32% of the individuals did quit the welfare programs. This aggregates exits due to employment, geographic mobility, change of family structure and non reporting of their quarterly income to their local agencies. In panel B, the share of welfare recipients who took a job raises quickly after the allocation to social housing, 9% after 6 months, and doubles one year later to reach 18% after 18 months. Panel C displays the attrition rate for this measure. There is a substantial fraction of individuals who quits the welfare program without employment reasons. This attrition rate is probably overstated as a large fraction of these individuals receive again the minimum income program a few months later. This suggests that this number are mainly due to administrative reasons and the suspension of the eligibility to the benefits when the households do not fit their quarterly income forms.

[13]They are identified through different form of earnings top-ups and directly subsidised contracts (*Intéressement*, temporary job bonuses, RMA and CAV).

Panel D of table 2.4 shows the wages of the individuals during their first employment spells at the time they enter the minimum income top-up program. The average earnings appear below the full-time minimum wage. This is consistent with part-time employment contracts concentrated around the minimum wage and corresponds to the large fraction of subsidized part-time contracts of the welfare recipients (Gurgand and Margolis, 2008).

3 Empirical strategy

3.1 Main specification

In order to identify the causal relationship between the quality of a neighborhood and job search outcomes of welfare recipients, I exploit the quasi-experimental variation created by the social housing allocation process. Once I control for the information in the public housing application forms, the allocation to a particular public housing unit is arguably exogenous with respect to the future labor market outcomes of the welfare recipients. This empirical strategy can be seen as a reduced form equation from the *linear-in-means* model of Manski (1993) under some additional assumptions.

My main specification relates the labor market outcomes Y_{ijt} of household i allocated to a social dwelling in neighborhood j at time t to the unemployment rate of this neighborhood, U_j, measured in 1999:

$$Y_{ijt} = \beta_1 + \gamma U_j + X_{ijt}\beta_2 + L_{ijt}\beta_3 + E_{ijt}\beta_4 + \varepsilon_{ijt} \ , \tag{2.1}$$

where γ is the parameter of interest. γ summarizes the reduced form effect of neighborhood characteristics on the labor market outcomes. It allows to test if labor market outcomes of new social housing tenants are influenced by the location of public housing units. X_{ijt}, L_{ijt} and E_{ijt} denote three different categories of control variables that are needed to focus on quasi-experimental variation of U_j and consistently estimate γ. Specifically, X_{ijt} contains individuals characteristics, age at the time of entry into social housing, nationality of the head of the household (French, European, other and unknown nationalities), gender[14], marital status (in couple or not), number of children, the fact to have young children, a cubic in the monthly duration of the welfare spell interacted with the year and semester of entry into social housing. These cohort controls are important as welfare recipients' employment is highly dependent on publicly subsidized jobs (Rioux, 2001, Gurgand and Margolis, 2008) and the supply of subsidized jobs is correlated with the national elections occurring in 2002. I include the interaction with the duration of the welfare spells as the eligibility to some subsidized contracts is conditional on the welfare duration. For example, a new subsidized private job program, the RMA, created in December 2003 was only available to welfare recipients who spent more than 24 months with the RMI. L_{ijt} contains information about the past and new *arrondissements* of residence of house-

[14] The gender of the spouse is unknown and coded as spouse of a male or spouse of a female. The rules to choose the household head are discussed in Jacquot (2001). In my sample, the women is the head of the household for nearly half of the couples.

hold i. In my most constrained specification, L_{ijt} is a set of 182 interacted dummy variables for the past and current *arrondissements* of location. Given these controls, the causal effect of neighborhood is identified by variation in neighborhood allocation between households from the same *arrondissement* moving to the same *arrondissement*. E_{ijt} is a vector of past employment, housing and income characteristics that may be used as additional controls. When the full set of controls is included all the information of the application form is taken into account.

Under my identification strategy, the unobserved factors affecting the labor market outcomes, ε_{ijt}, have to be unrelated to the allocated neighborhood characteristics conditional on my control variables:

$$E[\varepsilon_{ijt}|U_j, X_{ijt}, L_{ijt}, E_{ijt}] = E[\varepsilon_{ijt}|X_{ijt}, L_{ijt}, E_{ijt}] \ . \tag{2.2}$$

The variation of the local unemployment rate is as good as randomly assigned once I control for X_{ijt}, L_{ijt}, and E_{ijt}. This assumption is valid if I am able to control for all the characteristics that may influence the allocation of a public housing applicant to a particular neighborhood and there is perfect compliance to the administrative process. This type of assumption and reduced form strategy have been used in a wide variety of contexts to estimate the effect of peers or neighborhood: student achievement with respect to their college roommates (Sacerdote, 2001), immigrants' outcomes and first location (Edin et al., 2003) and the consequences of living in a poor neighborhood (Oreopoulos, 2003). To evaluate the robustness of my findings, I introduce sequentially these controls in my regressions. My baseline specification controls only for basic household characteristics, X_{ijt}. Then I introduce the *arrondissement* variables that may affect the selection process of social housing applicants, L_{ijt}. Finally, I also control for the observable past labor market outcomes and housing conditions, E_{ijt}. This last specification is close to a lag-dependent variable model.

3.2 Falsification exercise and robustness checks

My identifying assumption (2.2) could be violated in two main cases. First, the social housing commissions could allocate dwellings and households in the waiting list according to characteristics that are not presented in the application form and these characteristics may be correlated to unobserved determinants of labor market outcomes. Second, welfare recipients could manipulate the allocation process through strategic non compliance. If assumption (2.2) is not met, the coefficient γ in specification (2.1) does not consistently estimate the causal impact of social housing location on the labor market outcomes of welfare recipients. If (2.2) holds, I should not observe any specific relationship between past outcomes of the welfare recipients and the current quality of their allocated neighborhoods. Thus, I assess the relevance of this assumption by comparing some past outcomes of the new tenants to the quality of their allocated neighborhoods. Specifically, I regress previous labor market outcome such as past employment and earnings on all the right-hand side variables of equation (1). These variables are present in the application form. However, the fact that, conditional on households' characteristics, they are not correlated with the current local unemployment rate suggests that other unobservables driving labor market outcomes would also be uncorrelated with the allocated neighborhoods.

An additional concern arises from the fact that some labor market outcomes are not observed for the whole sample due to sample attrition. I only observe welfare recipients in Paris and I do not know the whole labor market history of each household during their employment or unemployment spells. Thus, welfare recipients moving to other cities disappear from the data set. This geographic mobility is unlikely to bias the main results because once a household has moved in a social housing dwelling, there is a very low turn-over of public accommodations in Paris (5% in 2006). However, changes in family structure and non response to the quarterly income inquiries also impact the eligibility to the minimum income program. If this sample attrition is correlated with unobservable determinants of labor market outcomes and the local unemployment rate, U_j, it may bias the estimates of the impact of neighborhood quality on labor market outcomes. The following assumption is required to rule out differential attrition rates by neighborhood quality:

$$E[\varepsilon_{ijt}.M_{ijt}(Y_{ijt})|U_j, X_{ijt}, L_{ijt}, E_{ijt}] = E[\varepsilon_{ijt}.M_{ijt}(Y_{ijt})|X_{ijt}, L_{ijt}, E_{ijt}] \ , \tag{2.3}$$

where $M_{ijt}(Y_{ijt})$ denotes a dummy variable taking value 1 if the information on Y_{ijt}, the labor market outcome is missing. I test the missing at random assumption (2.3) by estimating regression (1) with a new dependent variable $M_{ijt}(Y_{ijt})$. If, conditional on $X_{ijt}, L_{ijt}, E_{ijt}$, the characteristics of the neighborhood have no significant effect on the transitions out of the sample, the labor market outcomes estimates are not biased by selective sample attrition[15].

4 Empirical results

4.1 Falsification exercise and quasi-random allocation to social housing

First, I directly assess the relevance of the identifying assumption (2). Table 2.5 presents the results of falsification tests for the new social housing tenants. Panel A displays the results of a linear probability model of the probability of having a positive previous taxable income in the penultimate year prior to the social housing allocation[16]. In column (1), I control only for observable households characteristics. The estimate indicates that a 5 percentage points increase in the unemployment rate of the allocated neighborhood (approximately one standard-deviation) is correlated with a decrease of the probability of having a positive income by 1.4 percentage points. However this point estimate is not significant at the 10% significance level. The negative association between the unemployment rate of the allocated neighborhoods and past income disappears once I control more precisely for the *arrondissements* of allocation in columns (2) and (3). In column (2), I control only for the *arrondissement* of the allocated social unit. The absolute value of the point estimate is divided by six while the standard-error increases by only one third. A 5 percentage points increase in the unemployment rate of the allocated neighborhood would is now associated with a 0.2 percentage points increase in the

[15]DiNardo et al. (2006) detail the same issue for the MTO experiment.

[16]This corresponds to the last available yearly income tax form.

probability of having a positive income. When I introduce further control for both the past and current *arrondissements* of residence in column (3), the point estimate changes sign but remains of same magnitude in absolute value.

Panel B displays the results of the same falsification tests for the previous taxable income in the penultimate year prior to the social housing allocation. In all the specifications, the coefficient of the allocated unemployment rate indicates a weak relationship between this variable and the allocated unemployment rate. A 5 percentage points increase in the allocated unemployment rate is associated with an increase in previous income by 115 to 170 euros, less than 3% of the standard-deviation of this measure. Moreover these estimates are all insignificant at the 10%, and would indicate that households with higher income get allocated to neighborhoods with higher unemployment rate.

Finally, panel C presents the association between previous employment measured as the fact to receive some wage deduction six months before the allocation to social housing and the unemployment rate of the allocated neighborhoods. In column (1) when only the individual the individual controls are included, the estimate indicates that a one-standard deviation decrease in the local unemployment rate would imply an increase of 0.7 percentage points of the probability to have some form of employment. However, this positive correlation between the past employment status and the quality of the allocated neighborhood disappears once I control for the *arrondissements* of allocation in columns (2) and (3). The point estimates have the same magnitude in absolute value, but change of sign and remains non-significant at the 10% significance level. In summary, the impacts of the allocated unemployment rate on the past labor market outcomes are never economically or statistically significant. This is not the case when I estimate the same specifications for the new private tenants (estimates not reported). In the private rental sector, households tend to present past labor market outcomes that are directly correlated with the current quality of their neighborhoods.

4.2 Neighborhood effects on labor market outcomes

Table 2.6 shows the effects of public housing location described by the local unemployment rate at the census tract level on the welfare exits and employment 12 months, 18 months and 24 months after the initial relocation of the new social tenants[17].

Table 2.6 panel A presents the estimates of the impact of the local unemployment rate on the welfare exits. In column (1), a decrease of one standard deviation of the local unemployment rate (5%) increase the likelihood to find a job within 12 months by 0.3 percentage points. The included control have little impact on this point estimates. In all the specifications, the point estimates remain of same sign and magnitude. In column (4), when fixed effects for each pair of *arrondissements* are included, the estimated effect drops to 0.2%. All these impacts are non-significant at the 10% level. The point estimates at 18 months are much larger. In columns (6) to (8), they indicate that an increase of one standard-deviation of the neighborhood quality

[17] The estimates after 6 are similar to the estimates after 12 months and not reported.

would increase the probability to exit the welfare programs by 1.6 percentage points. However, these estimates remain insignificant at the 10% level and the point estimates drops in magnitude when I consider the exits of welfare at 24 months in columns (9) to (12). This may correspond to the high turn-over of the welfare recipients between short-term part-time contracts and welfare spells or to short periods of administrative non-eligibility.

Panel B of table 2.6 investigates further the reasons of this time pattern. The dependent variable is now a dummy variable for being observed during an employment spell before leaving the welfare program. In this second panel, all the point estimates have the expected negative sign that indicates that lower local unemployment rates increase the likelihood of finding a job. The point estimates are all between 0.2 and 0.6 which indicate that a one standard deviation decrease in the local unemployment rate would increase the likelihood of finding a job by 1 to 3 percentage points. All the point estimates after 18 months are significant at the 5% level which would indicate that the observed increase in the welfare exits was indeed due to employment spells. Panel C examines the attrition of this measure. The dependent variable is a dummy variable taking value one when the individual left the welfare program without starting an employment spell beforehand. None of these estimates is significant at the 10% significance level.

Finally, Table 2.7 shows the estimated impact of neighborhood unemployment of the wages of the welfare recipients when they find a job. Much of the variation in the wage measure is driven by the difference between part-time and full-time jobs and my data do not allow distinguishing the effect of the working hours and the hourly wage. Panel A. displays the results from the regressions of the unemployment rate on the wage of the individuals (in level) for the individuals starting an employment spell. The estimates are small and insignificant at the 10% level. The largest point estimate in column (3) indicates that a one standard deviation decrease in the neighborhood unemployment rate would imply a 50 euros increase (1/10 standard deviation of the earnings' measure). The panel B of table 6 controls for the self-selection of the new employees using a two-step selection model. I use the children variables as exclusion restriction in the selection equation. The estimates for the wage equation are even smaller while the standard-errors significantly increase. In the selection equation, as in table 6, a positive correlation appears between the probability to take a job and neighborhood quality. However these point estimates are insignificant at the 10% level when I control for the *arrondissements* of allocation.

These results are consistent with the MTO literature (Kling et al., 2007). However, the absence of clear effects for the whole sample could be due to two main reasons. First, if the neighborhood effects are heterogeneous across welfare recipients, it is possible to observe stronger results for some subgroups. I examine this possibility in the next sub-section. Second, as there is no clear difference in job accessibility inside Paris, this does not rule out the possibility that at another spatial scale the redevelopment of the public housing system could have a positive effect on the search outcomes of the welfare recipients. This could be the case at the scale of the Paris region if the spatial mismatch hypothesis of Kain (1968) holds. However, this partly rules out the assumption that relocation to better neighborhoods can improve the job related social networks of the welfare recipients (at least on the short and medium run) and the assumption that the residence in areas exposed to crime or in low-skilled minorities' neighborhoods inhibit the job access of welfare recipients.

4.3 Heterogeneous neighborhood effects by individual characteristics

While the quality of public housing location appears to have small positive effects on the job finding rate and welfare exits of welfare recipients, these low average impacts could be the result of heterogeneous treatment effects. Table 2.8 and Table 2.9 reproduce the results of Table 2.6 on the subsamples of women and single women.

For the whole sample of women in table 2.8, the impacts of the neighborhood unemployment rate on the exit of welfare is always negative and as in the whole population the effects become larger at 18 months and decrease in absolute value at 24 months (panel A). The point estimates at 12 and 18 months imply that a decrease of one standard-deviation if the allocated unemployment rate would raise the exits from welfare by 1.5 at 12 months to 3 percentage points after 18 months. Panel B of table 8 indicates that this increase in the exits from the welfare program is mostly due to employment spells which increase in the same proportion at 12 and 18 months. However, the estimates of panel B should be interpreted carefully. A large fraction of women is observed leaving the welfare programs without starting an employment spell and this proportion of women appear related to the quality of neighborhoods in panel C.

In table 2.9, the estimates for the subsample of single women have the same pattern. The sample size drops from $1,257$ to 815 observations and the standard-errors become more imprecise. The impact of neighborhood of welfare exits appear stronger at 12 months and decrease at 18 and 24 months. On the contrary, the estimate for the job finding rate of the welfare recipients are constant over time and indicate that a decrease of one standard-deviation of the unemployment rate increase the overall transition to work by 0.5 percentage points in column (11) to 3.8 percentage points in column (4). This again indicates that the quality of neighborhood has a positive impact on the job finding rate of single women but that these effects are not strong enough to allow durable exits from the welfare programs.

Table 2.10 displays the estimates of neighborhood effect on the welfare exits and employment of men. The estimates have no well-defined sign as in the previous tables 8 and 9. Their absolute values is in average nearly twice below the corresponding estimates for women and all the point estimates are insignificant at the 10% level. Despite the large standard-errors, this provides suggestive evidence that neighborhood effects are very weak for men. The non-reported estimates for the smaller subsample of the 479 single men point out towards an even stronger negative answer. The estimate for the impact of the unemployment rate on welfare exits is positive and around 0.4 in all the specifications. Similarly, nearly all the point estimates for the effects of neighborhood on the probability to find a job have the opposite sign as those on the subsample of women and are positive.

Finally, Table 2.11 reports the estimates of the neighborhood effects on the welfare exits and employment of individuals below 40 years old[18].Younger welfare recipients may be more employable and concentrate most of the employment spells, thus the neighborhood effects should be easier to detect for this sub-population. In panel A, the estimates for the impact of the welfare exits are all negative as expected and larger than the corresponding estimates for the whole population reported in table 2.6 but they remain insignificant at the 10% significance level. In panel B, the estimated impact on the job finding rates are even larger than for the subsample of single women in table 9. They suggest that a one standard-deviation decrease in the allocated unemployment rate would increase the transitions to work by 4.4 percentage points 18 or 24 months after the allocation to social housing. As the average rate of transition to work is 24

4.4 Discussion

The allocated local unemployment rate impacts welfare recipients' employment and their temporary exits from the welfare program. These estimates are mainly driven by women and individuals below 40 years old. On the contrary, neighborhood quality has no positive impact on the employment probability of men and more particularly single men. The different estimates by gender are consistent with the MTO findings on crime and mental health Kling et al., 2005, 2007. Kling et al. develop three alternative explanations to the observed gender differences: peer sorting, if new male and female tenants tend to resort or not into the same type of peer groups in their new and old neighborhoods (Jencks and Mayer, 1990), search strategies, if females tend to rely more on interpersonal relationships for their job search than men, and comparative advantages, if females have human capital that made them more able to exploit the job opportunities in good neighborhoods. In absence of other external evidence, it is difficult to distinguish the role of these three factors in my results. The results for the welfare recipients below 40 year old suggest that the most employable individuals may be able to take advantage of better location. Moreover, the time pattern of the estimates could also be consistent with this explanation. Once the individuals with adapted human capital have been able to find a job, the effect of better location on the job finding rate does not increase over time after one year spent in social housing.

5 Conclusion

This chapter examines the effect of the location of public housing on the labor market outcomes of welfare recipients in Paris. Using the social housing allocation process as a quasi-natural experiment, I take into account the endogeneity of the location process and study an important policy: the location of public housing units. I find that welfare recipients allocated to better neighborhoods have slightly higher job finding rates, but I do not find any evidence

[18]The results are not sensitive to this age cut-off. Additional results for different sub-sambles, individuals with and without children, long term and short-term welfare recipients or by age group, do not have a clear pattern.

that neighborhoods have any impact on the wages or strong effects on the long-term exits from the welfare program. The higher job finding rate is mostly driven by women and seems related to low paid and temporary employment contracts. These estimates shed some light on the impact of the redevelopment of the French social housing system taking place in wealthy neighborhoods due to a recent change in the French legislation. The results confirm the weak effect of location on the economic self-sufficiency of poor households obtained in the USA with the MTO experiment. They suggest that creating new social housing units to relocate welfare recipients into better neighborhoods will not improve their economic self-sufficiency[19].

However it is possible that the redevelopment of social housing units across cities may have an impact on the economic self-sufficiency of the welfare recipients if they are located in deprived cities where the job offers are scarce (Olof et al., 2010). The development of social housing units in better neighborhoods within Paris could be desirable for other reasons than economic self-sufficiency. Location could have higher impacts on children educational and long-term outcomes. For example, Goux and Maurin (2007) obtained complementary evidence that children educational outcomes are influenced by the outcomes of their close neighbors in France. New social housing buildings could also improve the quality and healthfulness of the lettings of the poor households or influence the criminal activity and exposure to crime. Finally, the effects of social housing developments on construction, overall neighborhood composition, housing prices and crime in a given neighborhood have received little attention. The potential detrimental or positive effects of public housing redevelopment on the existing neighborhood are mostly unknown. Recent US evidence (Baum-Snow and Marion, 2009) suggest that the impact in good neighborhoods is in general weak. Further research is needed to determine the overall efficiency of social housing policies.

[19]Welfare recipients represent only a small share of the social housing tenants.

Table 2.1. Compliance to the social housing allocation among low-income applicants

	Welfare recipients (1)		Low income Households (2)	
	Non weighted	**Weighted**	**Non weighted**	**Weighted**
Refused a dwelling in:				
1996	0	0	0	0
2002	0	0	1	1,128
Applicants in:				
1996	3	2,665	29	27,136
2002	10	10,139	18	16,986
Refusal rate	0.00%	0.00%	2.10%	2.60%

Note: (1) Welfare recipients are households for whom at least one individual received the RMI during the last year. (2) Households whose income by OECD consumption unit is below the second national decile. Source: French Housing Surveys 1996 and 2002. Households who live in Paris and declare having applied to social housing.

Table 2.2. Linear and non-linear correlations between different metrics of neighborhood quality

	Unemploy-ment rate (1999) (1)	Long term Unemployment rate (1999) (2)	Median Income by UC in 2001 (3)	Median Income by UC in 2005 (4)
	Correlation (Rank correlation)	**Correlation (Rank correlation)**	**Correlation (Rank correlation)**	**Correlation (Rank correlation)**
Unemployment rate in 1999	1.00 (1.00)			
Long term unemployment rate in 1999 (1)	0.83 (0.91)	1.00 (1.00)		
Median Income by UC in 2001 (2)	-0.77 (-0.81)	-0.76 (-0.79)	1.00 (1.00)	
Median Income by UC in 2005	-0.76 (-0.79)	-0.74 (-0.77)	0.99 (0.99)	1.00 (1.00)

Note: The computations are based on 968 census tracts for the 1999 census data and 915 census tract for the other indicators. (1) Long term unemployed workers have been unemployed for more than one year. (2) UC are Eurostat consumption units: the first adult (individual of more than 14 y.o.) has weight 1, other adults have weight 0.5 and children have weight 0.3. Source: French census in 1999, Taxable income at the census tract level in 2001 and 2005 (INSEE, DGI).

Table 2.3. Characteristics of the welfare recipients allocated to social housing

	Whole sample (1)	Single women (2)	Single men (3)
Variable	**Mean (s.d.)**	**Mean (s.d.)**	**Mean (s.d.)**
A. Individual characteristics			
Single female	0.37		1.00
Single male	0.22	1.00	
Head female	0.11		
Head Male	0.10		
French (1)	0.47	0.48	0.55
European	0.02	0.02	0.02
Non-European	0.41	0.39	0.29
Unknown nationality	0.11	0.10	0.14
Age	43.44	43.62	45.49
	(10.20)	(9.88)	(9.63)
RMI Months at the entry	54.56	54.06	56.59
into social housing	(45.25)	(46.32)	(46.84)
Children of less than 3 y.o.	0.13	0.02	0.00
Children of 3 to 6 y.o.	0.21	0.19	0.01
Children	0.56	0.63	0.05
Number of children	1.19	1.05	0.08
	(1.48)	(1.14)	(0.39)
Cohort 06/2001	0.10	0.10	0.10
Cohort 12/2001	0.09	0.09	0.09
Cohort 06/2002	0.10	0.11	0.10
Cohort 12/2002	0.12	0.12	0.10
Cohort 06/2003	0.11	0.11	0.11
Cohort 12/2003	0.10	0.11	0.09
Cohort 06/2004	0.13	0.12	0.15
Cohort 12/2004	0.11	0.10	0.14
Cohort 06/2005	0.15	0.13	0.14
Cohort 12/2005	0.10	0.10	0.10
B. Past labor market outcomes			
Employment at t-6 months	0.10	0.13	0.09
Income>0 in year t-2	0.47	0.43	0.43
Income of year t-2	2,943.49	2,137.17	2,494.91
(2005 euros)	(5,355.63)	(3,934.09)	(4,275.51)
C. Past housing conditions			
Monthly rent at t-6 months	371.07	357.17	260.09
if known and >0 (2005 euros)	(261.87)	(209.17)	(183.63)
Rent is 0 at t-6 months if known	0.55	0.53	0.63
Unknown rent at t-6 months	0.15	0.13	0.14
D. Neighborhood allocation			
Unemployment rate in 1999	0.16	0.16	0.16
	(0.05)	(0.05)	(0.06)
Variance intra arrondissement (2)	3.44	1.21	0.89
[share]	[0.58]	[0.59]	[0.62]
Variance intra pair of	3.16	1.04	0.81
arrondissements (3) [share]	[0.53]	[0.51]	[0.56]
# Observations	2,178	815	479
# Allocated census tracts	407	312	236

Note: (1) Nationality of the head of the household. (2) Arrondissement of the social unit. (3) Interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Table 2.4. Labor market outcomes of the welfare recipients: descriptive statistics

	Whole sample (1)	**Single women (2)**	**Single men (3)**
Variable	**Mean (s.d.)**	**Mean (s.d.)**	**Mean (s.d.)**
A. Exit of welfare (1) at:			
6 months	0.06	0.05	0.06
12 months	0.14	0.12	0.12
18 months	0.18	0.15	0.18
24 months	0.32	0.28	0.32
B. Employment (2) at:			
6 months	0.09	0.11	0.11
12 months	0.14	0.15	0.16
18 months	0.18	0.20	0.20
24 months	0.20	0.22	0.23
C. Attrition of the employment measure (3) at:			
6 months	0.06	0.05	0.06
12 months	0.11	0.09	0.10
18 months	0.14	0.11	0.14
24 months	0.24	0.20	0.23
# Observations	2,178	815	479
# Allocated census tracts	407	312	236
D. monthly wage during the first employment spell (4):			
Wage (2005 euros)	901.83 (584.49)	821.52 (534.17)	804.67 (576.66)
# Observations	240	106	53
# Allocated census tracts	149	83	47

Note: (1) This dummy variable takes value 1 if the individuals is no longer at RMI or API or in a related subsidized job. (2) The employment measure is cumulative and take into account any employment spell observed after the allocation to social housing. It is measured for the subsample (3) The attrition for the employment measure corresponds to the number of individuals who are not in welfare at n months but did not take a job before the end of their welfare spell. (4) The sample contains all the individuals who find employment after their allocations to social housing. The wage is computed from the quarterly income forms when the individual enters the top-up program associated with the guaranteed minimum income. All wages are converted in 2005 euros using the INSEE purchasing power time series. This average amount is close to the (net employee) full-time minimum wage (933 euros in 2005). Source: CNAF and CAF welfare recipients' registry 2001-2007.

Table 2.5. Falsification tests. Impact of the allocated neighborhoods on past labor market outcomes

Specification	**(1)**	**(2)**	**(3)**
A. Dependent variable: Having a positive income in year t-2			
Unemployment rate	-0.282	0.044	-0.058
	(0.212)	(0.309)	(0.312)
B. Dependent variable: Income in year t-2			
Unemployment rate	2,599.338	3,078.780	3,520.347
	(2,366.716)	(3,619.749)	(3,898.799)
C. Dependent variable: Employment 6 months before allocation			
Unemployment rate	-0.142	0.120	0.165
	(0.133)	(0.163)	(0.176)
Individual controls (1)	Yes	Yes	Yes
Arrondissements fixed effects (2)	No	Yes	Yes
Pair of arrondissements fixed effects (3)	No	No	Yes
# Observations	2,178	2,178	2,178
# Clusters	407	407	407

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old, having children between 3 and 6 years old, number of children and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Table 2.6. Impact of the allocated neighborhoods on welfare exits and employment, whole population

A.	**Welfare exit at 12 months**				**Welfare exit at 18 months**				**Welfare exit at 24 months**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Unemployment rate	-0.062	-0.098	-0.013	-0.044	-0.090	-0.336	-0.280	-0.325	0.031	-0.091	0.036	-0.014
	(0.167)	(0.203)	(0.210)	(0.199)	(0.205)	(0.238)	(0.251)	(0.244)	(0.230)	(0.290)	(0.292)	(0.292)
# Observations	2,178	2,178	2,178	2,178	2,178	2,178	2,178	2,178	1,851	1,851	1,851	1,851
# Clusters	407	407	407	407	407	407	407	407	381	381	381	381
B.	**Employment at 12 months**				**Employment at 18 months**				**Employment at 24 months**			
Unemployment rate	-0.233	-0.271	-0.304	-0.343	-0.379**	-0.486**	-0.511**	-0.595**	-0.246	-0.238	-0.166	-0.267
	(0.168)	(0.206)	(0.215)	(0.211)	(0.187)	(0.229)	(0.252)	(0.246)	(0.230)	(0.300)	(0.320)	(0.335)
# Observations	1,934	1,934	1,934	1,934	1,865	1,865	1,865	1,865	1,577	1,577	1,577	1,577
# Clusters	395	395	395	395	400	400	400	400	367	367	367	367
C.	**Attrition at 12 months**				**Attrition at 18 months**				**Attrition at 24 months**			
Unemployment rate	-0.034	-0.120	-0.049	-0.075	-0.021	-0.312	-0.288	-0.320	0.073	-0.147	-0.101	-0.131
	(0.156)	(0.181)	(0.189)	(0.182)	(0.175)	(0.216)	(0.230)	(0.226)	(0.180)	(0.237)	(0.232)	(0.232)
# Observations	2,178	2,178	2,178	2,178	2,178	2,178	2,178	2,178	1,851	1,851	1,851	1,851
# Clusters	407	407	407	407	407	407	407	407	381	381	381	381
Controls												
Individual controls (1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Arrondts FE (2)	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pair of arrondts FE (3)	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Additional controls (4)	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old, having children between 3 and 6 years old, number of children and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. (4) Additional controls include: having a job six month before the allocation to social housing, income during the penultimate year, dummy for no income, dummy for unknown income, the rent six month before the allocation and dummies for not paying any rent and unknown rent six month before the allocation.
Source: CNAF and CAF welfare recipients’ registry 2001-2007 and census in 1999.

Table 2.7. Impact of the allocated neighborhoods on wages, whole population

A.	OLS: wage of the 1st employment spell			
	(1)	**(2)**	**(3)**	**(4)**
Unemployment rate	246.882	874.332	1,045.491	849.819
	(701.029)	(1,120.438)	(1,051.495)	(1,023.947)
# Observations	240	240	240	240
# Clusters	149	149	149	149
Controls				
Individual controls (1)	Yes	Yes	Yes	Yes
Arrondts FE (2)	No	Yes	Yes	Yes
Pair of arrondts FE (3)	No	No	Yes	Yes
Additional controls (4)	No	No	No	Yes
B.	**Heckman: wage of the 1st employment spell**			
Wage equation				
Unemployment rate	-96.232	306.913	-482.009	
	(1,372.755)	(1,511.059)	(2,580.927)	
Selection equation (5)				
Unemployment rate	-1.296*	-0.775	-0.936	
	(0.749)	(0.975)	(0.997)	
# Observations	2,178	2,178	2,178	
# Clusters	407	407	407	
Controls				
Individual controls (1)	Yes	Yes	Yes	
Arrondts FE (2)	No	Yes	Yes	
Additional controls (4)	No	No	Yes	

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. (4) Additional controls include: having a job six month before the allocation to social housing, income during the penultimate year, dummy for no income, dummy for unknown income, the rent six month before the allocation and dummies for not paying any rent and unknown rent six month before the allocation. (5) I use the children variables as exclusion restriction: having children between 3 and 6 years old, number of children. The model is fitted by the two-step procedure. Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Table 2.8. Impact of the allocated neighborhoods on welfare exits and employment, women

A.	**Welfare exit at 12 months**				**Welfare exit at 18 months**				**Welfare exit at 24 months**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Unemployment rate	-0.215	-0.351	-0.317	-0.344	-0.255	-0.542*	-0.527*	-0.591**	-0.000	-0.230	-0.106	-0.157
	(0.179)	(0.245)	(0.277)	(0.259)	(0.215)	(0.281)	(0.308)	(0.284)	(0.217)	(0.278)	(0.312)	(0.307)
# Observations	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,151	1,151	1,151	1,151
# Clusters	363	363	363	363	363	363	363	363	349	349	349	349
B.	**Employment at 12 months**				**Employment at 18 months**				**Employment at 24 months**			
Unemployment rate	-0.477**	-0.497*	-0.426	-0.449*	-0.520**	-0.587*	-0.487	-0.508	-0.428	-0.363	-0.141	-0.165
	(0.210)	(0.270)	(0.281)	(0.265)	(0.233)	(0.299)	(0.332)	(0.322)	(0.262)	(0.336)	(0.363)	(0.362)
# Observations	1,227	1,227	1,227	1,227	1,217	1,217	1,217	1,217	1,079	1,079	1,079	1,079
# Clusters	362	362	362	362	361	361	361	361	338	338	338	338
C.	**Attrition at 12 months**				**Attrition at 18 months**				**Attrition at 24 months**			
Unemployment rate	-0.128	-0.309	-0.281	-0.287	-0.061	-0.419*	-0.458*	-0.493*	0.143	-0.166	-0.120	-0.148
	(0.175)	(0.223)	(0.251)	(0.243)	(0.178)	(0.246)	(0.277)	(0.265)	(0.182)	(0.264)	(0.285)	(0.284)
# Observations	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,257	1,151	1,151	1,151	1,151
# Clusters	363	363	363	363	363	363	363	363	349	349	349	349
Controls												
Individual controls (1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Arrondts FE (2)	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pair of arrondts FE (3)	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Additional controls (4)	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old, having children between 3 and 6 years old, number of children and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. (4) Additional controls include: having a job six month before the allocation to social housing, income during the penultimate year, dummy for no income, dummy for unknown income, the rent six month before the allocation and dummies for not paying any rent and unknown rent six month before the allocation.
Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Table 2.9. Impact of the allocated neighborhoods on welfare exits and employment, single women

A.	Welfare exit at 12 months				Welfare exit at 18 months				Welfare exit at 24 months			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Unemployment rate	-0.245	-0.477*	-0.484	-0.556*	-0.185	-0.394	-0.382	-0.500	0.288	-0.005	0.089	0.009
	(0.216)	(0.284)	(0.345)	(0.334)	(0.258)	(0.327)	(0.373)	(0.362)	(0.263)	(0.316)	(0.397)	(0.403)
# Observations	815	815	815	815	815	815	815	815	709	709	709	709
# Clusters	312	312	312	312	312	312	312	312	289	289	289	289
B.	**Employment at 12 months**				**Employment at 18 months**				**Employment at 24 months**			
Unemployment rate	-0.611**	-0.668**	-0.615*	-0.769**	-0.543*	-0.566	-0.386	-0.509	-0.408	-0.482	-0.112	-0.214
	(0.276)	(0.339)	(0.348)	(0.334)	(0.299)	(0.381)	(0.422)	(0.424)	(0.337)	(0.418)	(0.445)	(0.438)
# Observations	739	739	739	739	726	726	726	726	627	627	627	627
# Clusters	300	300	300	300	297	297	297	297	276	276	276	276
C.	**Attrition at 12 months**				**Attrition at 18 months**				**Attrition at 24 months**			
Unemployment rate	-0.204	-0.444*	-0.421	-0.456	-0.034	-0.297	-0.298	-0.395	0.142	-0.210	-0.092	-0.157
	(0.195)	(0.255)	(0.304)	(0.300)	(0.198)	(0.283)	(0.326)	(0.319)	(0.222)	(0.307)	(0.364)	(0.364)
# Observations	815	815	815	815	815	815	815	815	709	709	709	709
# Clusters	312	312	312	312	312	312	312	312	289	289	289	289
Controls												
Individual controls (1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Arrondts FE (2)	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pair of arrondts FE (3)	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Additional controls (4)	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old, having children between 3 and 6 years old, number of children and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. (4) Additional controls include: having a job six month before the allocation to social housing, income during the penultimate year, dummy for no income, dummy for unknown income, the rent six month before the allocation and dummies for not paying any rent and unknown rent six month before the allocation.
Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Table 2.10. Impact of the allocated neighborhoods on welfare exits and employment, men

A.	Welfare exit at 12 months				Welfare exit at 18 months				Welfare exit at 24 months			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Unemployment rate	0.072	0.153	0.230	0.227	0.084	-0.153	-0.107	-0.136	0.038	0.050	0.070	0.054
	(0.225)	(0.253)	(0.261)	(0.258)	(0.265)	(0.302)	(0.335)	(0.337)	(0.271)	(0.342)	(0.359)	(0.362)
# Observations	921	921	921	921	921	921	921	921	856	856	856	856
# Clusters	326	326	326	326	326	326	326	326	316	316	316	316
B.	**Employment at 12 months**				**Employment at 18 months**				**Employment at 24 months**			
Unemployment rate	0.053	-0.003	-0.090	-0.103	-0.157	-0.231	-0.313	-0.336	-0.133	-0.095	-0.030	-0.063
	(0.226)	(0.288)	(0.321)	(0.324)	(0.246)	(0.300)	(0.343)	(0.353)	(0.326)	(0.443)	(0.488)	(0.514)
# Observations	887	887	887	887	878	878	878	878	797	797	797	797
# Clusters	320	320	320	320	322	322	322	322	306	306	306	306
C.	**Attrition at 12 months**				**Attrition at 18 months**				**Attrition at 24 months**			
Unemployment rate	0.055	0.086	0.161	0.165	0.023	-0.225	-0.192	-0.216	0.111	-0.062	-0.092	-0.100
	(0.208)	(0.225)	(0.237)	(0.239)	(0.253)	(0.294)	(0.326)	(0.328)	(0.242)	(0.302)	(0.321)	(0.324)
# Observations	921	921	921	921	921	921	921	921	856	856	856	856
# Clusters	326	326	326	326	326	326	326	326	316	316	316	316
Controls												
Individual controls (1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Arrondts FE (2)	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pair of arrondts FE (3)	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Additional controls (4)	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old, having children between 3 and 6 years old, number of children and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. (4) Additional controls include: having a job six month before the allocation to social housing, income during the penultimate year, dummy for no income, dummy for unknown income, the rent six month before the allocation and dummies for not paying any rent and unknown rent six month before the allocation.
Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Table 2.11. Impact of the allocated neighborhoods on welfare exits and employment, below 40 years old

A.	**Welfare exit at 12 months**				**Welfare exit at 18 months**				**Welfare exit at 24 months**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Unemployment rate	-0.402	-0.264	-0.124	-0.267	-0.283	-0.224	0.019	-0.086	-0.175	-0.305	-0.283	-0.372
	(0.310)	(0.350)	(0.390)	(0.375)	(0.331)	(0.353)	(0.401)	(0.401)	(0.344)	(0.406)	(0.481)	(0.483)
# Observations	834	834	834	834	834	834	834	834	706	706	706	706
# Clusters	294	294	294	294	294	294	294	294	267	267	267	267
B.	**Employment at 12 months**				**Employment at 18 months**				**Employment at 24 months**			
Unemployment rate	-0.334	-0.601	-0.613	-0.757*	-0.529	-0.826*	-0.770	-0.937*	-0.310	-0.713	-0.722	-0.881
	(0.312)	(0.382)	(0.447)	(0.416)	(0.359)	(0.448)	(0.560)	(0.531)	(0.404)	(0.532)	(0.653)	(0.622)
# Observations	728	728	728	728	711	711	711	711	599	599	599	599
# Clusters	279	279	279	279	278	278	278	278	247	247	247	247
C.	**Attrition at 12 months**				**Attrition at 18 months**				**Attrition at 24 months**			
Unemployment rate	-0.478*	-0.473*	-0.369	-0.460	-0.309	-0.344	-0.162	-0.218	-0.331	-0.514	-0.484	-0.523
	(0.271)	(0.281)	(0.321)	(0.317)	(0.282)	(0.276)	(0.346)	(0.350)	(0.262)	(0.325)	(0.394)	(0.400)
# Observations	834	834	834	834	834	834	834	834	706	706	706	706
# Clusters	294	294	294	294	294	294	294	294	267	267	267	267
Controls												
Individual controls (1)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Arrondts FE (2)	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Pair of arrondts FE (3)	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Additional controls (4)	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Note: * significant at the 10% level, ** at 5%, *** at 1%. Standard-errors are clustered at the census tract level. (1) Individual controls include: age, age squarred, dummy variables for single women, single women with children, single men, male head of couple, female head of a couple, spouse of a male, spouse of a female, nationality of the household head (French, European, Non European), having children of less than 3 years old, having children between 3 and 6 years old, number of children and a full set of cohort (year interacted with semester) dummies interacted with a cubic in the previous duration of the welfare spell. (2) The arrondissements fixed effects are defined at the location of the allocated social dwelling. (3) A pair of arrondissement is defined as the interaction between the past arrondissement of residence and the arrondissement where the social housing unit is located. (4) Additional controls include: having a job six month before the allocation to social housing, income during the penultimate year, dummy for no income, dummy for unknown income, the rent six month before the allocation and dummies for not paying any rent and unknown rent six month before the allocation.
Source: CNAF and CAF welfare recipients' registry 2001-2007 and census in 1999.

Appendix figure 2.A. Application form

Paris :	❑ Tout Paris	
	❑ Arrondissements par ordre de préférences :	
Banlieue :		
La Ville de Paris ou l'OPAC de Paris disposent de logements dans d'autres communes d'Ile-de-France, souhaiteriez-vous avoir un logement dans une commune ci-contre :	❑ La Celle-St-Cloud (78170) ❑ Plaisir (78370) ❑ Trappes (78190) ❑ Athis-Mons (91200) ❑ Vigneux-sur-Seine (91270) ❑ Asnières-sur-Seine (92600) ❑ Boulogne Billancourt (92100) ❑ Malakoff (92240) ❑ Bagneux (92220) ❑ Bagnolet (93170) ❑ Saint-Denis (93200)	❑ Aubervilliers (93300) ❑ Boissy Saint Léger (94470) ❑ Champigny-sur-Marne(94500) ❑ Fontenay-sous-Bois (94120) ❑ Ivry-sur-Seine (94200) ❑ Thiais (94320) ❑ Villiers-sur-Marne (94350) ❑ Vitry-sur-Seine (94400) ❑ Gentilly (94250) ❑ Limeil-Brévannes (94450)

Motif de votre demande de logement : ....

....

....

....

....

Source: Municipality of Paris.

Chapter 3

Migration and unemployment duration

1 Introduction

High unemployment and wage differentials between European regions and urban areas emphasize the role of mobility as a potential adjustment mechanism. Blanchard and Katz (1992) and Oswald (1996, 1997) argue that the low labor migration rates in Europe can partly explain its high unemployment rate compared to the United States. Therefore, low migration rates are a growing policy concern in Europe (Decressin and Fatas, 1995, OECD, 2005, Gáková and Dijkstra, 2008), and investigating the internal migrations of unemployed workers is a natural way of understanding the dynamics of European labor markets, and the persistence of regional unemployment imbalances.

This chapter examines the impact of migration on the labor market outcomes of French unemployed workers over the 1995-2004 period. I take advantage of a unique longitudinal administrative dataset recording each month the place of residence of unemployed workers. I compare the job search prospects of migrant and non-migrant unemployed workers using the changes in place of residence over the same unemployment spell. I ask how migrations and job search are related. Migrations appear associated with higher job finding rates, and long-term unemployed workers appear to have higher returns to migration. Furthermore, the migration effects do not appear systematically correlated with individual unobserved heterogeneity, and regional unemployment differentials do not explain the higher job finding rates of the migrants.

Migration is as an important human capital investment decision (Sjaastad, 1962). Spatial mismatches between job offers and unemployed workers are a possible cause of unemployment and poverty (Kain, 1968). Negative peer effects may also lead to poor labor market outcomes in residential areas with high long-term unemployment rates (Cutler and Glaeser, 1997 and Zenou, 2009). Therefore, geographic inequalities appear not only as a result of individual labor market outcomes but also as one of their determinants. Unemployed workers migrate if they can obtain higher wages, more job offers, or if there are sufficient local amenities to compensate lower job market prospects in high unemployment regions (Moretti, 2011). However, unemployed workers' migrations may also be due to market imperfections. Gregg et al. (2004) provide evidence that liquidity constraints may force constrained workers to leave high living cost regions, rather than take advantage of higher job search opportunities in the United Kingdom. Therefore, the effect of migration on unemployment duration is ultimately an empirical question.

This chapter builds on two main strands of literature. First, macroeconomic studies have focused on regional mobility as an adjustment mechanism that may reduce wage differentials (Barro and Sala-i-Martin, 1992), unemployment disparities and mismatches between employers and workers (Blanchard and Katz, 1992). These studies use aggregate migration rates and differences in unemployment across geographic areas to identify the role of migrations on regional labor markets. Pissarides and McMaster (1990) find that regional migrants in the United Kingdom respond to regional unemployment differentials and wages differentials, but that the difference between the private benefits and the social benefits of migration lead to an inefficient adjustment process. This partly explains the persistence of regional unemployment disparities. In continental Europe, Crozet (2004) also finds that mobility costs were too high to significantly reduce wage differentials over the $1980-90$ period. Second, microeconomic evidence has focused on the drivers of residential mobility, such as the employment status and homeownership (Garner et al., 2001, Gobillon, 2001), and the potential wage gains of migration (Borjas et al., 1992, Ham et al., 2004). However, while this strand of literature underlines that unemployed workers are more mobile than employed workers, it mostly ignores the importance of migration for the job search process of unemployed workers[1].

This chapter investigates the effect of migration decisions on the job search process of unemployed workers in continental Europe. This question has two aspects. First, do migrations affect the probability of getting a job after different unemployment durations? Second, are the measured effects of migration due to the migration itself or to the difference in local labor market conditions? I measure the effect of regional migration on unemployment duration using a sample of French unemployed workers. France has high regional unemployment disparities and relatively high gross and net regional migration rates in Europe (OECD, 2005). Furthermore, many French policies are dedicated to reduce regional unemployment differentials and provide subsidies aimed at reducing the cost of geographical mobility of labor[2]. I first suggest a simple job-search model with migrations that allows discussing the potential effects of migration on unemployment duration. I start the empirical analysis by presenting several estimates of the effect of unemployment assuming that the decision to migrate and future labor market outcomes are independent conditional on large set of observable characteristics. I document that unemployed workers' migrations are associated with large increase in their job finding rates. 18 months after a migration, job finding rates appear 10 percentage points higher than those of non-movers. These estimates are large, especially for long-term unemployed workers whose baseline job finding rates are fairly low. Consistent with job search theory, increases in

[1] Gobillon and Le Blanc (2003) survey the recent microeconomic evidence and provide additional evidence of the wage gains associated to regional migrations in Europe.

[2] The laws of July 1st, 2001 and of January 1st, 2002 define new subsidies for unemployed workers who find a job outside their residential region. The law of July 1st, 2005 introduces a negative income tax for all unemployed worker who migrates to accept a job offer located more than 200 kilometers away from her home.

job finding rates appear also larger for long-distance migrations between French *régions* than short-distance migrations between French *départements*. Accounting for unobserved individual heterogeneity using repeated unemployment spells does not change the main point estimates, though the results become less precise. Finally, the estimated effects of migration should be compared with the average job finding rate of the migrants if the two regions had similar labor market conditions. I use semi-parametric duration models to adjust for the change in local labor market conditions and isolate the effect of mobility on unemployment duration from the effect of regional unemployment differentials. The unemployment rates in the initial region and in the region of destination have similar impact on unemployment duration. Migrants do not appear to have higher job finding rates because they move towards low unemployment rate regions. Regional mobility itself seems to explain the effect of migration on their job finding rates.

The remainder of this chapter is organized as follows. Section 2 describes a stylized model of job search and migrations, and discusses the economic parameters of interest. Section 3 discusses the sample of French unemployed workers, and provides some descriptive evidence on the regional migration patterns of unemployed workers. Section 4 describes the empirical strategies. Section 5 discusses the empirical results. Section 6 concludes.

2 A stylized model of regional migration and job search

Since Sjaastad (1962), migrations are considered as a self selection process related to human capital investment[3]. In this human capital framework, migrations are analyzed in relation to net utility change. Migrations are justified if local labor markets are heterogeneous and provide job seekers with different employment opportunities, if workers have skills that are valuated differently between geographic areas, or if, for example, the housing market or the living costs are more attractive in another region (Pissarides and Wadsworth, 1989, Gobillon, 2001).

I incorporate the migration decision in a stylized partial-equilibrium job search model. Unemployed workers can engage in two methods of job search: stay and search or migrate and search[4]. This assumes that local labor markets are segmented and that job seekers apply predominantly to local job offers. This is consistent with the empirical part that uses migrations between French *départements* and *régions* that are in average distant of around 43 and 89 kilometers, respectively. Manning and Petrongolo (2011) find that most unemployed workers tend to limit their job search to local vacancies and that the attractiveness of a job falls by about 4.5 times if one pulls the job 5 km further away. The workers know perfectly the transition probabilities between the different states and their environment is overall stationary. U_0 and U_1 are the stationary values of unemployment for a non migrant and a migrant, $W(w)$ is the payoff of employment at wage w, and r is the constant discount factor. In a discrete time setting, the

[3]Todaro (1969), Harris and Todaro (1970) and Mincer (1974) also provide early economic models of the migration decisions.

[4]This set-up is not specific to migration. Pissarides (1979) uses the same discrete time setting model with a fixed wage to examine the relative efficiency of several job search methods.

payoffs of the different states of the labor market are:

$$
\begin{aligned}
U_0 &= b + \frac{p_0}{1+r}\int (U_0 + max(W(w) - U_0, 0))dF_0(w) + \frac{(1-p_0)}{1+r}U_0 \ , \\
U_1 &= b - c + \frac{p_1}{1+r}\int (U_1 + max(W(w) - U_1, 0))dF_1(w) + \frac{(1-p_1)}{1+r}U_1 \ , \\
W(w) &= w + \frac{W(w)}{1+r} \ ,
\end{aligned}
\tag{3.1}
$$

where p_0 and p_1 represent the probabilities to receive a job offer and b the unemployment benefits. F_0 and F_1 are the cumulative density functions of the wage distributions in the two regions. I assume that migrations have a constant cost, c, which summarizes all the non labor-market costs and benefits due to the change of geographic area and may vary with distance and other factors. c takes not only into account accommodation and borrowing costs, but local amenities and local unemployment benefits that varies across municipalities in France (L'Horty et al., 2002). Hence, c may be negative if low living costs in one region compensate moving costs. In particular, short distance migrations may be less related to employment reasons (Gobillon and Le Blanc, 2003). For instance, the prospect of saving money by living with one's parents might drive some short-distance migrations of young unemployed workers. By making the time interval tend to zero, I obtain the following three Bellman equations:

$$
\begin{aligned}
r.U_0 &= b + \lambda_0.\int max(W(w) - U_0, 0)dF_0(w) \ , \\
r.U_1 &= b - c + \lambda_1.\int max(W(w) - U_1, 0)dF_1(w) \ , \\
r.W(w) &= w \ .
\end{aligned}
\tag{3.2}
$$

Non-migrants accept a job offer if $W(w) \geq U_0$ and migrants accept a job offer if $W(w) \geq U_1$. Assuming that the expected value of the wage under F_0 and F_1 are finite, the reservation wages are:

$$
\begin{aligned}
w_{R0} &= b + \frac{\lambda_0}{r}\int_{w \geq w_{R0}} (1 - F_0(w))dw = b + \frac{\lambda_0}{r}\int_{w \geq w_{R0}} (w - w_{R0})dF_0(w) \ , \\
w_{R1} &= b - c + \frac{\lambda_1}{r}\int_{w \geq w_{R1}} (1 - F_1(w))dw = b - c + \frac{\lambda_1}{r}\int_{w \geq w_{R1}} (w - w_{R1})dF_1(w) \ .
\end{aligned}
\tag{3.3}
$$

Migrants and non migrants coexist if $U_0 = U_1$. Therefore, the reservation wages in the two regions are equal to w_R, and migration costs have to satisfy the condition:

$$
\lambda_1(1 - F_1(w_R))E_{F_1}(w - w_R | w \geq w_R) - \lambda_0(1 - F_0(w_R))E_{F_0}(w - w_R | w \geq w_R) = r.c \tag{3.4}
$$

The right hand side of equation 3.4 is the difference in the expected labor market earnings of migrants and non-migrants. This difference depends both on the job arrival rates and the wage offers distributions in the two regions. By contrast, the left hand side of equation 3.4 represents the discounted moving costs. This expression simplifies when the two regions have the same wage distribution, F:

$$(\lambda_1 - \lambda_0)(1 - F(w_R))E_F(w - w_R|w \geq w_R) = r.c \ . \tag{3.5}$$

There are two parts in the expected gains of migrations. First, the difference between the hazard rate from unemployment to employment of the migrants and the one of the non migrants is $(\lambda_1 - \lambda_0)(1 - F(w_R))$. Second, the expected wage when the workers accept a job offer is $E_F(w - w_R|w \geq w_R)$. Hence, high moving costs, c, may justify the persistence of large regional differences in employment prospects, $\lambda_1 - \lambda_0 \neq 0$. As the gains of migrations depend on the expected wage, the model also explains that low skilled workers may be less mobile. By contrast, migrations may be motivated by other reasons than labor market outcomes if there are sufficient compensating differentials ($c \leq 0$) to justify lower labor market outcomes, $(\lambda_1 - \lambda_0) < 0$ in equation 3.5.

I estimate the first part of the returns to migration, the difference in unemployment duration of migrants and non-migrants. More precisely, if an unemployed worker decides to move at a given elapsed unemployment duration, t, I compare her probability to exit unemployment at time $t + n$ with the probability to exit unemployment she would have had by staying in her previous region of residence. In the model described above, the average effect of the decision to migrate at time t on the likelihood to find a job of the migrants is:

$$\mathrm{ATT}_{t,n} = \exp(\int_t^{t+n} \{-\lambda_0(1 - F_0(w_R))(s)\}ds) - \exp(\int_t^{t+n} \{-\lambda_1(1 - F_1(w_R))(s)\}ds) \ . \tag{3.6}$$

The Average Treatment effect on the Treated, ATT, is the difference between the survival function that migrants would have had if they did not have moved and their actual survival function. I assume that the average effect of migration on job search outcomes may be duration dependent, and I introduce the unemployment duration at the time of migration, t, in the empirical specifications. Unemployment benefits, human capital and the rate of arrival of job offers may change over the duration of an unemployment spell. Moreover, recent empirical evidence suggests that the effects of local labor markets on unemployment exits are duration dependent (Gobillon et al., 2011).

3 Data and descriptive statistics

3.1 Unemployed workers and migrations

The empirical analysis is based on a unique $1/12^{\text{th}}$ nationally representative sample of French unemployed workers registered to the French employment agency, the *ANPE*[5]. The registration to the *ANPE* is mandatory to receive unemployment benefits. Therefore, the data cover nearly all unemployed workers between January 1995 and December 2004. Chardon and Goux

[5]This longitudinal dataset, known as *Super Fichier Historique Statistique*, is maintained by three French institutions: the *ANPE*, the *DARES* and the *INSEE*.

(2003) estimate that 90% of the job-seekers are registered with the *ANPE*[6]. Other versions of this dataset have been used to study the efficieny of Active Labor Market Policies (Crépon et al., 2007) or the extent of regional disparities in unemployment duration (Duguet et al., 2009, Gobillon et al., 2011). Each unemployment spell is identified by the day of registration to the French employment agency and the day of the end of the unemployment spell. The data allow controlling for important characteristics of the unemployed workers such as gender, age, qualification, education, nationality, marital status, children, the reasons of the unemployment spell (dismissal, labor market entry, etc.), and the types of unemployment or welfare benefits. The two main variables of interest are the migrations between French regions and the exits of unemployment[7].

Regional migrations are observed through the places of residence that are recorded each month by the local unemployment agencies. Places of residence are *département*. This administrative zoning divides the French metropolitan territory into 95 areas. It is a natural zoning as most of local employment policies are designed along these administrative boundaries (Boissard, 2008). Over the 1995-2004 period, I observe $9,206$ migrations that affect 2.4% of the unemployment spells. Given the small number of migrants, I consider migration as a binary variable taking value 1 when workers change region, 0 otherwise. This definition of the migration of unemployed workers is subject to measurement error. Some migrations may be misreported and considered as ends of unemployment spells for unknown reason. This may occur when a job seeker does not address her monthly form to the local employment agency. This issue seems to have weak consequences: the average number of migrations per unemployed workers is consistent with previous empirical works (Appendix 3.A and Gobillon, 2001, Baccaïni, 2005). Furthermore, the administrative areas defined as *département* are not homogenous. For example, the Paris region is divided into small geographic areas and represents around 17% of the migrations. Therefore, I use an alternative definition of migrations, between larger French regions, known as *régions*, to check the robustness of my results. Each of the 22 French *régions* corresponds to a group of *départements*[8]. With this more restrictive definition of migration, I observe only $6,644$ residential moves that affect 1.7% of the unemployment spells. Second, some residential moves during and unemployment spells may be due to the acceptance of a job offer in another region. This phenomenon seems residual in the data and I drop every unemployment spells ending less than 15 days after a migration to limit its possible extent.

The outcome variable of interest is the *exit out of unemployment*. If the exits of the unemployment registry are perfectly observed, the reasons of the ends of the unemployment spells are only partially known. The end of an unemployment spell can occur for other reasons than employment, such as starting a training program, leaving the labor force, suffering from long time illness, or unknown reasons. When the exits of unemployment do not correspond to an

[6]This estimate is based on the International Labour Organization (ILO) definition.

[7]The 2005 dataset is the only *ANPE* dataset which records the places of residence of unemployed workers over time. This sample was drawn to assess the sensitivity of public unemployment statistics to regional migrations. Earlier and later datasets record only the place where the unemployment spell starts or ends.

[8]The dataset does not distinguish the two *départements* of Corsica. Thus, in the empirical analysis, Corsica is both a *département* and a *région*.

employment spell, I dropped the corresponding unemployment spells from the sample. However, the exits motivated by unknown reasons represents nearly one third the sample[9]). Most of the exits for unknown reasons correspond to unemployed workers who do not send their monthly form to the *ANPE* so that the reason of the end of their unemployment spells is unobserved. Using additional survey data, Chazal et al. (2003) estimate that about half of these exits are towards employment. Although there are no significant differences in the composition of the outflows between migrants and non-migrants, I use two alternative definitions of the exit of unemployment to confirm the robustness of my empirical results. First, I adopt a strategy comparable to right censoring to keep in the sample a large number of migrations. I treat unknown exits as postponed outflows. I take them into account not as instantaneous outflows but as outflows two months later (Duguet et al., 2009). Second, I consider the unknown exits as missing as random and I drop all the uncertain observations (Crépon et al., 2007 and Appendix 3.B for details). The results in the main text correspond to the first definition and the results relative to the second definition are reported in Appendix 3.D.

3.2 Descriptive analysis

Figure 3.1 displays the average migration rates of unemployed workers between *départements*, *régions*, and the share of the migrations between *régions*. It shows a clear increase in regional migrations. The yearly migration rate is about 2.8% in 1995 and is about 5% in 2004. Moreover, the share of long-distance migrations between *régions* is steadily increasing from 73.5% of the migrations between *départements* in 1995 to 77.0% in 2004. These results are very similar to the results obtained for the residential mobility of the whole population (Baccaïni, 2005). They confirm a general increase in residential mobility in France. The destinations of the migrations of the unemployed workers tend also to be comparable to the destinations of the whole population. Hence, *départements* from the South East of France or the Atlantic coast have the largest positive unemployed migration rates (Appendix 3.A). Furthermore, contrary to the pattern of migrations in the United Kingdom observed by Gregg et al. (2004), there is no clear association between variation in the French mobility rates and the aggregate business cycle over the 1995-2004 period.

Table 3.1 reports some simple correlations between regional migration rates, regional costs of living and labor market conditions, which were predicted to affect the migrations and job finding rates of the unemployed workers in the stylized model of Section 2. Migration outflows and inflows appear strongly positively correlated (0.73). Areas where there are high outflow rates to other *départements* tend also to have high inflow rates of unemployed workers, and net migration rates are small[10]. Second, Column 3, migration rates appear negatively correlated with local rent levels in public housing and local costs of living (-0.28). This correlation is mainly driven by outflows of unemployed workers out of regions with high rent levels (0.52),

[9] The decomposition of the outflows out of unemployment is given in Appendix 3.B.

[10] Appendix 3.A reports additional descriptive statistics by *département*.

while inflows are relatively smaller in regions with high renting costs (0.31). Finally, the correlation between the unemployment rate and the migration rate in 2002 is positive and stands at 0.16. Unemployed workers do not appear to move particularly to low unemployment areas. Nevertheless, the correlation between the migration rate and the increase of the unemployment rate which is also quite low has the expected negative sign (-0.09). The inflows of migrants are high in regions where the unemployment rates are decreasing (-0.36). Hence, at the aggregate level, there is no clear evidence that migrations have a positive impact on unemployment duration, or that unemployed workers in depressed regions are motivated to move to look for work in other regions where the employment possibilities are better.

I now investigate the migration at the individual level. In order to provide a descriptive analysis of the timing of the migration and the unemployment exits, I consider a particular cohort of unemployed workers who registered to the unemployment agency between January 2001 and June 2002. I restrict the sample to individuals between 18 and 55 years old who are immediately available to work[11]. I follow this cohort of $377,317$ individuals from the moment they register to the *ANPE* until the end of December 2004. As Crépon et al. (2007), if the workers have repeated unemployment spells, I retain only the first spell to avoid correlation of unobservable variables over consecutive spells. Figure 3.2 reports the monthly empirical hazard rates of transition into a new place of residence from the start of an unemployment spell up to 18 month unemployment. These transition rates are between 0.2 and 0.4% per month and pick between the 2^{nd} and 10^{th} months of unemployment. By contrast, 2.4% of the unemployment spells are associated with a migration, and these migrations are realized at specific durations of unemployment. Unemployed workers appear unlikely to migrate during the first two months of an unemployment spell and after one year unemployment. Figure 3.3 displays the quarterly empirical hazard rates from unemployment to employment for migrants and non migrants and the hazard rates from unemployment to employment of the migrants after their migration decision. The Kaplan-Meier estimates illustrate the importance to take the timing of the migration into account. The migrants present the lowest rates of outflow out of unemployment, but after the change of region, the level of their hazard rates is similar to those of non migrants while they stayed longer in unemployment.

Table 3.2 reports descriptive statistics of the characteristics of migrants and non-migrants over the duration of an unemployment spell. I focus on three previously elapsed unemployment durations: 2 months, 6 months and 10 months. I obtain three sub-samples of $258,179$ unemployed workers after 2 months of unemployment, $214,577$ after 6 months, and $151,118$ after 10 months. After each of the three unemployment periods, I consider two groups: the unemployed workers who migrated during the previous two months and the unemployed who did not. The migrants always represent less than 1% of the unemployed workers. Moreover, there are significant differences in the observable characteristics of the migrants and non-migrants. Migrants appear in average younger than non-migrants. Unemployed workers without children or in single households are more likely to move than those living in couple or having children.

[11]This is coherent with the ILO definition of unemployment. Furthermore, I exclude the unemployed moving to and living in the French overseas territories (DOM-TOM) from the sample.

The nationality of the unemployed workers seems also to matter. While non French unemployed appear more likely to migrate at the beginning of an unemployment spell, this is not the case once the duration of unemployment is greater than 6 months. Expected labor market outcomes are also tightly related to the migration decision. More qualified unemployed workers, looking for full time permanent contracts, and without recurrent unemployment history appear the most likely to migrate. Therefore, the identification of the effect of migration on unemployment duration faces two issues. First, migrants are not a random sample of the unemployed workers. Second, the timing of the migration matters. Unemployed workers take the decision to migrate at any point in time during their unemployment spell and the previously elapsed unemployment duration is likely to affect both their outcomes on the labor market and their decision to migrate.

4 Empirical strategies

It is impossible to observe the outflow rates of the same individual under two mutually exclusive states and to estimate equation 3.6 from the sample of unemployed workers without additional assumptions. Previous studies about the wage gains of migration have used three main alternative methods: instrumental variables (Raphael and Riker, 1999), matching estimators (Ham et al., 2004) and panel data (Gobillon and Le Blanc, 2003). Private costs, such as the number of children, have been used as instruments for the migration of employed workers. The number of children is arguably correlated with the likelihood to migrate and uncorrelated with future wages. However, this is not the case for unemployed workers whose private costs both determine the propensity to move and the incentives to work. For example, in France, the number of children was a key determinant of the replacement rate of the past minimum program (*RMI*). The presence of children also influences women labor market participation and their likelihood to accept a job offer (Hyslop, 1999). Moreover, the low proportion of migrants is likely to lead to weak instruments' problems in instrumental variables' models. In the absence of a feasible instrumental variables strategy, comparing unemployed who are very similar and likely to migrate for similar reasons, should approximate the estimates of the effect of migration on unemployment duration. My first estimator relies on a conditional on observables independence assumption. Under this assumption, the observable characteristics of the non migrants can be used to recover a valid counterfactual outcome for the migrants. Second, I use repeated unemployment spells and individual fixed effects to control for time-invariant unobserved heterogeneity that may be correlated with the migration decision. Finally, I extend the conditional on unobservable assumption and I control for the change in labor market conditions associated to migration process in duration models to isolate the migration effect from the change in labor market conditions.

4.1 Conditional on observable assumption and matching estimators

I focus on the same sample of unemployment spells described in Section 3.2. I estimate the difference of outflow out of unemployment due to the decision to migrate after 2, 6 and 10 month unemployment. The research design is closely related to the studies of Sianesi (2004) or Fredriksson and Johansson (2003). After t month unemployment, I compare the labor market outcomes of migrants and non-migrants who are still unemployed. I define a migration variable, D_t, if the unemployed worker migrated between month $t-2$ and t. I then relate this migration variable to later unemployment exits from $t+3$ months, until $t+18$ months. Y_{t+n} denotes this series of dummy variables, n being equal to 3, 6, 9, 12, 15, or 18 months. Y_{t+n} takes value one if the unemployed left unemployment before time $t+n$, 0 otherwise. In potential outcome notations, Y^1_{t+n} and Y^0_{t+n} are the employment status of the same invidual if he migrated between $t-2$ and t ($D_t = 1$) or if he stayed in the same region ($D_t = 0$). Then the average treatment effect of the migration is given by:

$$\text{ATT}_{t,n} = E(Y^1_{t+n} - Y^0_{t+n}|D_t = 1) \; . \tag{3.7}$$

I focus on the migration effect on the migrants rather than on the average treatment effect (ATE) because I am primarily interested in the returns of migrations for the migrants. Moreover, computing the ATE would require to make inference about the unconditional effect of the migrations and this raises two main issues. First, many unemployed workers may already be in a location with high employment opportunities, so the average effect of the migrations may be null or negative. Second, the gains immobile workers would have experienced if they had moved. This requires finding a valid counterfactual for a large number of non migrants in a small sample of migrants (Ham et al., 2004). Therefore, the computation appeared uninformative when I used the matching estimators. Matching estimators are often implemented to assess policy evaluation issues (Blundell and Costa Dias, 2009, Smith and Todd, 2005). The identification of the treatment effect is based on the conditional independence assumption (CIA):

$$E(Y^0_{t+n}|X_t, D_t = 1) = E(Y^0_{t+n}|X_t, D_t = 0) \; , \tag{3.8}$$

where X_t is a vector of observable characteristics of the unemployed worker at time t. This assumption leads to a semi parametric estimator which involves pairing migrants and non migrants who are similar in terms of observed characteristics that could influence both unemployment duration and the selection process. This leads to a computable formula for the ATT:

$$\text{ATT}_{t,n} = E(Y^1_{t+n}|D_t = 1) - E\{E(Y^0_{t+n}|X_t, D_t = 0)\} \; . \tag{3.9}$$

An important practical issue is the dimension of the vector of the control variables, X_t. Rosenbaum and Rubin (1983) showed that the CIA with respect to observables could be rephrased as a conditional independence assumption with respect to the propensity score. The propensity score is the probability to migrate conditional on a vector of relevant observable variables, $s(X_t) = P(D_t = 1|X_t)$. This reduces the dimensionality of the matching problem:

$$\text{ATT}_{t,n} = E(Y^1_{t+n}|D_t = 1) - E\{E(Y^0_{t+n}|s(X_t))\} \ . \tag{3.10}$$

To estimate the average treatment effect on the treated, I construct an empirical analogue of equation 3.10. Rosenbaum and Rubin (1983) proposed Inverse Propensity score Weighted (IPW) estimators, while Heckman et al. (1997, 1998) suggested Kernel estimators. The IPW estimator is consistent and asymptotic normality if the propensity score is estimated non parametrically (Hirano et al., 2003). The IPW estimator of equation 3.10 is:

$$\widehat{\text{ATT}}_{t,n} = \frac{1}{\sum_k D_{kt}} \sum_i [Y_{i,t+n}.\{D_{it} - (1 - D_{it}).\frac{\hat{s}(X_{it})}{1 - \hat{s}(X_{it})}\}] \ , \tag{3.11}$$

where i and k are indices for unemployed workers, t for unemployment duration at the time of migration, and n for the time spent in the new region of residence. $\hat{s}(X_{it})$ is a consistent estimator of the propensity score of migration (Wooldridge, 2010). This estimator associates to each non-migrant unemployed worker a weight determined to correct the selection bias. Non-migrants with low propensity to migrate have less weight than the other non-migrants in the counterfactual group.

Kernel estimators also intend to construct a counterfactual to deal with the selection problem but these estimators are based on individual counterfactuals. For each moving unemployed worker, i, the estimator attributes particular weights, w_{ij}, to the non-migrants, j. The weights are function of the difference between the propensity score of the migrant unemployed worker and the propensity score of the non-migrants:

$$\begin{aligned}
&\widehat{\text{ATT}}_{t,n} = \frac{1}{\sum_k D_{kt}} \sum_i [Y_{i,t+n}.D_{it} - \sum_j \{Y_{j,t+n} w_{ij} (1 - D_{jt})\}] \ , \\
&\text{with } w_{ij} = \frac{K([\hat{s}(X_{it}) - \hat{s}(X_{jt})]/h_j)}{\sum_l (1 - D_{lt}).K([\hat{s}(X_{it}) - \hat{s}(X_{lt})]/h_j)} \ , \\
&\text{and } K(u) = (1 - u^2).1_{|u|\leq 1} \ .
\end{aligned} \tag{3.12}$$

I use an Epanechnikov Kernel, K, with a constant Silverman window, h_j, to determine the weight of each non-migrants[12]. Kernel estimators are asymptotically normal and consistent (Heckman et al., 1998). IPW and Kernel estimators differs as the former defines a counterfactual at the group level, while the latter defines a counterfactual for each migrant. Moreover, since the propensity score is estimated, there is no closed form for the Kernel estimators' variance, while an asymptotic approximations exist for IPW estimators (Crépon and Iung, 1999, Hirano et al., 2003)[13]. Eichler and Lechner (2002) and Caliendo and Kopeinig (2008) suggest that,

[12]The bandwidth is fixed according to Silverman rule-of-thumb (Silverman, 1986). There is a trade-off between the precision of the estimation which depends positively on the number of individuals included in the control group and the bias. The bandwidth is computed using: $h_j = 1.364.15^{1/5}.\hat{\sigma}[\hat{s}(X_{jt})].N_{0t}^{-1/5}$. N_{0t} is the number of non migrants at duration t.

[13]Appendix 3.C describes the asymptotic approximation used in the empirical section.

in large samples, the uncertainty in the estimated propensity scores is negligible and that the variance of the Kernel estimator can be computed considering the weights of the counterfactual as fixed. I use this method to compute the standard-errors of the Kernel estimates[14].

I also consider Linear Probability Models (LPM), and logit and probit models. In these specifications, I include migration as a dummy variable, D_{it}, and the same individual control variables, X_{it}, as in the previous IPW and Kernel estimators. For example, the probability to exit unemployment before time $t+n$ is approximated by a Linear Probability Model:

$$Y_{it+n} = \gamma D_{it} + X_{it}\beta + \varepsilon_{it} \ , \tag{3.13}$$

where D_{it} takes value 1 if the unemployed changed region between $t-2$ and t. γ requires stronger identifying assumptions than the matching estimator to estimate consistently the ATT of migration. In particular, the LPM would lead to inconsistent estimators in the presence of heterogeneous returns to migrations. As shown by Blundell et al. (2005), matching estimators would be consistent if there is observable or unobservable heterogeneity in migration returns. For example, migration returns may depend not only on observable educational attainment, but also, on unobservable variables, such as family ties or unobserved ability. Matching estimators would be consistent in both cases, while the LPM would fail to estimate consistently the ATT even if idiosyncratic migration gains are uncorrelated with the unobservable variables that determine the non-migrants' outcome. However, if unobservable variables determine the non-migrants' outcomes, both LPM and matching estimators would be inconsistent. In order to assess the magnitude of the selection process of the migrants with respect to observable variables, I also compute the mean difference in the labor market outcomes of the migrant and non-migrant workers. This estimator would be justified if the migrations were randomly assigned to unemployed workers, and give the order of magnitude of the selection of the migrants conditional on the observables.

4.2 Individual unobserved characteristics and regional unemployment differentials

There are two main issues with respect to the estimates based on the conditional independence assumption 3.8. First, the migration decision may be correlated to unobservable characteristics of the unemployed workers. Second, the estimates based on equations 3.11 and 3.12 do not distinguish the returns to migration from the change in labor market conditions. For example, workers' mobility decisions may be strongly affected by local unemployment and wages. In this case, unemployed workers' mobility decisions may be a relative indicator of local labor market conditions.

In order to assess the plausibility of the assumption 3.8, I construct a different sample of unemployment spells. This sample contains only repeated unemployment spells and allow comparing the labor market outcomes of the same unemployed worker for an unemployment spell

[14] A simple comparison of the asymptotic variance of the IPW estimator and the one of the same estimator considering the propensity score as fixed suggests that this approximation is valid given the large sample size.

with a migration, and an unemployment spell that was spent in a unique region, *département* or *région*. More precisely, the assumption underlying this approach is that the decision to move depends only on observable variables and an individual fixed effect. For migration at t months of unemployment, the specification compares an unemployment spell with a migration to another unemployment spell without migration[15]:

$$Y_{ist+n} = \gamma D_{ist} + X_{is}\beta + \alpha_i + \delta_s + \varepsilon_{ist} \ . \tag{3.14}$$

where s is an index for the unemployment spells of the unemployed worker i. α_i is an unobservable individual fixed effect, and δ_s a common shock to all unemployed workers with s repeated spells. As before, D_{ist} is a dummy variable taking value 1 if the unemployed changed region between month t and $t-2$ of unemployment spell s. The key point is that the individual unobserved transitory shocks, ε_{ist}, are mean independent of the mobility decision, conditional on the observable variables and the individual fixed effect. Thus, the included time varying observable variables, X_{is}, are important to control for observable shocks[16]. While gender or diploma are absorbed through the fixed effects, α_i, I control for changes of age, marital status, reasons of entry into unemployment, number of children, the desired type of contract, and regional dummies for each *égion* that may differ over unemployment spells. These observable variables proxy for many individual transitory shocks that may affect the mobility decision (Garner et al., 2001, Dixon, 2003). I estimate specification 3.14 using a fixed effect linear probability model.

Furthermore, the estimates of the effect of migration in specification 3.11 to 3.14 may be due to the change in labor market conditions rather than the migration itself. Specification 3.14 controls for the spell-invariant characteristics of the unemployed workers but not for the differential of market conditions that occurs with the migration. I estimate semi-parametric Cox duration models. I account for the unemployment rate of the region of origin, u_{io}, and the time-varying difference in unemployment rate between the region of origin and the region of destination, Δu_{it}. Δu_{it} is set to 0 for the non-migrants. The hazard rate of transition of unemployment to employment, h, is given by:

$$h(t, X_{id}) = h_0(t).\exp(\gamma D_{it} + X_{it}\beta + u_{io}\theta_1 + \Delta u_{it}\theta_2) \ , \tag{3.15}$$

$$h(t, X_{it}) = h_0(td).\exp(\gamma D_{id} + \delta TM_{it} + X_{it}\beta + u_{io}\theta_1 + \Delta u_{it}\theta_2) \ , \tag{3.16}$$

$$h(t, X_{it}) = h_0(t).\exp(\gamma D_{it} + \delta TM_{it} + \phi TSM_{it} + X_{it}\beta + u_{io}\theta_1 + \Delta u_{it}\theta_2) \ , \tag{3.17}$$

where $h_0(t)$ is a fully flexible baseline hazard rate across the duration of unemployment t. X_{it} is a vector of observable characteristics of worker i. D_{it} is a dummy variable taking value 1 if the unemployed worker changed region before duration t, and 0 otherwise. The main additional parameter of specifications 3.15 to 3.17 is θ_2 that measures if the local unemployment rate has a different effect on the migrants after their regional move. The baseline specification 3.15 as-

[15] The migration variable is defined for 2, 6, and 10 month unemployment. In each case, the sample is restricted to unemployed spells lasting more than 2, 6, and 10 months, respectively.

[16] I measure X_{is} at the beginning of each unemployment spell.

sumes that the returns to migration are not duration dependent. By contrast, specification 3.16 assumes that long-term unemployed workers may have different gains of migration from new unemployed workers. TM_{id} is the duration of the unemployment spell prior to the migration in log months. TM_{id} is set to 0 for non-migrants. Finally, specification 3.17 allow the return to migration to differ with the time spent in the new region of residence, TSM. TSM is measured in log months and set to 0 for non migrants.

5 Empirical results

5.1 Likelihood of migration

The precedent section emphasizes the central role of the propensity score. The identifying assumption 3.10 states that the future employment trajectories are independent to migrations conditional on this score. The control variables that are included in the selection equations play a key role. I first control for the previously elapsed unemployment duration. On the one hand, it could lead people to move less if they lose gradually employment likelihood (human capital depreciation, motivation, stigma effects). On the other hand, it can also be viewed as an incentive to move if an individual lose gradually his employability in a local labor market. Moreover the design of the French unemployment benefits implies growing incentives to find a job. The choice of the other control variables is based on the empirical literature on the individual determinants of migrations (Gobillon, 2001, Boheim and Taylor, 1999, Kan, 1999). I introduce four categories of variables that may influence both moving decision and future employment status: demographic characteristics, education and qualification, labor market history, and cohort effects.

Table 3.3 reports the estimates of logistic model of migration (Wooldridge, 2010). The three estimated models predicts well the migrations. In each case, the percentage of concordant prediction is around 59%[17]. The estimates for the effect of demographic characteristics on the likelihood of migration have the expected signs. The likelihood of regional migrations decreases with age and the number of children. This is consistent with the life-cycle and human capital theories, because older job seekers have less time to benefit from their investment in migration. The effect of having a foreign citizenship is also positive but seems to decrease with the elapsed unemployment duartion. Men in couple appear more likely to move than single men and women for an elapsed duration of 2 months. After 6 month unemployment, single men appear the less mobile individuals, while for an elapsed duration of 10 months estimates are not significant at the 10% level. Education and qualification also play a role. Education and occupation represent prior investments that influence both the likelihood of moving and the economic returns from mobility. Furthermore, the quality and the quantity of the information as well as the potential returns to migration are increased with higher education and qualification (Greenwood, 1975).

[17] This is obtained by using the usual threshold of 0.5. This may underestimate the predictive power of the model, as the number of migrants is much lower than the number of non migrants (Cramer, 1999).

Indeed, educated and qualified job seekers appear significantly more mobile.

Finally, the individual labor market history since 1995 is a powerful predictor of migration. After 2 and 6 months unemployment, fired people and new labor market entrants are more mobile than people who are unemployed because they ended a temporary contract. Moreover, the unemployed workers, who have known a lot of unemployment spells or long-term unemployment since 1995, are also more mobile. Intensive job seekers who are looking for permanent and full time employment are also more mobile than the unemployed workers who register at the *ANPE* to look for a part time or/and a non permanent contract. Similarly, job seekers already in part time jobs are less likely to migrate than those who are not. This is consistent with the fact that they have fewer incentives to find a job and that their job search is likely to be less intensive. Finally, welfare recipients and handicapped workers are less mobile, perhaps because they are less likely to find a "good" job or more constrained in their housing choices.

5.2 Migration effect and matching estimators

I now turn to the estimate of the effect of migration on unemployment duration using the propensity scores estimated in Section 5.1. The distributions overlap well and their common support is large given the small number of migrants[18]. I limit the comparison of migrants and non-migrants to individuals having similar propensity to migrate. I trim the 1% highest and lowest values of the propensity score.

Table 3.4 reports the estimates of the effect of migration on the job finding rate of the migrants. Column 1 displays the difference in means, while Columns 2 to 6 report the estimates of the IPW and Kernel estimators, the LPM, and the average marginal effects of the logit and probit models. Columns 2 to 6, the control variables are those used in Table 3.3. The estimates can be interpreted as the component inherent to migration in the probability of leaving unemployment at different time horizons until 18 months beyond the decision to migrate. For instance, the IPW estimate for the decision to migrate after 2 months unemployment and 18 months in the new place of residence is 7.8 percentage points (Column 2). This means that, in average (over the different types of a migration and individual returns), the unemployed who decided to migrate during the first two months of their unemployment spell had 7.8 percentage points higher likelihood to find a job 18 months later, than if they had stayed in their initial region of residence. Overall the results of the Kernel and IPW and traditional estimators are very similar. In particular, the LPM estimates give us a good approximation of the ATT obtained with the logit and probit models which are themselves comparable to the matching estimates.

For the five estimators, the estimates show a positive effect of moving for all previously elapsed unemployment durations[19]. The estimates are large and in general significant at the 1% level, except for the decision to migrate at 6 months. Table 3.4 also displays some hetero-

[18] The distributions of the estimated propensity scores are reported in Appendix 3.D.

[19] The results of the alternative definition of the unemployment exits are even higher and reported in Appendix 3.E. This suggests that the estimates of the mobility-effects provided in Table 3.4 may be a lower bound of the migration-effects if the conditional independence assumption is satisfied.

geneous effects with respect to the timing of the migrations. Unemployed workers who move during the first two months of their unemployment spell, have lower returns to migration than those moving during their 5$^{\text{th}}$ and 6$^{\text{th}}$ months or 9$^{\text{th}}$ and 10$^{\text{th}}$ months of unemployment. Furthermore, the effects of the decision to migrate after the three previously elapsed unemployment durations evolve differently with the time spent in the new region of residence. After 2 month unemployment, the decision to move yields immediate and constant over time returns, while the returns to migration after 6 and 10 months unemployment are not immediately significant but now present a growing concave shape with the time spent in the new place of residence and the highest rates of returns after 18 months. One interpretation could be that local opportunities become scarcer for people who are staying without finding a job, arguably because of a mismatch between their skills and the local labor demand while movers not only extend their search horizons but also seem to be able to find more favorable labor markets. The time pattern of the effect is also difficult to reconcile with unemployed finding a job before moving and suggests that the observed effects of migration capture the effect of geographic mobility on the job search process of the unemployed.

These general results remain valid for the long-distance migrations between French *régions*. Table 3.5 reports the estimated returns of migrations on the job finfing rates of unemployed workers. The average distance of migration increases by a factor 2 between the two definitions of migrations[20]. The estimated effects are systematically higher than the estimates for the migrations between *départments* reported in Table 3.4. This is consistent with the stylized model of Section 2. For example, migrations after 10 month unemployment between *régions* are found to increase the job finding rate of unemployed workers by 12.9 percentage points after 18 months spent in the new region of residence, Column 2. By contrast, a similar migration between *départements* would increase the likelihood of finding a job by 11.0 percentage points.

Finally, Tables 3.4 and Tables 3.5 reports magnitude of the estimated effects using Kernel or IPW estimators that are significantly lower than the magnitude of the difference between the average outcomes of the migrants and non-migrants, Column 1. This suggests that migration and labor market outcomes are partly driven by those observable variables. Moreover, the similarity between matching and the usual estimators indicates that both observable and unobservable comparative advantages are negligible for the selection process. In other words, migrants do not seem to be selected with respect to unobservable relative gains to migration, though unobservable relative outcomes in the original regions of residence may explain the migrations. In the potential outcome notation of Section 3, the potential outcome in the original region, Y^0_{t+n}, may drive the decision of migration. In this case, linear models provide an accurate approximation of the true effect of migration on the job finding rate of the migrants.

[20]The average distances are 43 and 89 kilometers, respectively.

5.3 Repeated unemployment spells

Up to this point, I measured the effect of mobility using a single unemployment spell for each individual and considering both the time elapsed since the entry into unemployment and the time elapsed since the decision to migrate. The evidence indicates a clear positive effect of mobility on unemployment duration. I now use specification 3.14 and repeated unemployment spells to control for possible biases caused by worker specific unobserved heterogeneity. I select all the unemployment spells starting from January 2001 to August 2002. I observe $570,800$ unemployment spells. There is an important selection of unemployed workers when I use specification 3.14. Around 22% of those observations are repeated unemployment spells. Among the repeated spells, I observe $4,208$ migrations (3.4%). This unbalanced panel dataset oversamples repeated short spells. Hence, a concern is that time varying individual unobservable shocks that characterize the likelihood to exit unemployment and determine the number of repeated spells may be correlated with the decision to change region. Nevertheless, it seems feasible that these time varying individual unobservable shocks are uncorrelated with the decision to migrate conditional on observable variables. In this case, fixed effects allow estimating consistently the impact of the migration decision. In addition to individual fixed effects, I control for spell fixed effects and the individual characteristics as they appear at the beginning of the unemployment spell. These variables are: age, marital status, number of children, type of desired contract, reason of entry into unemployment, the type of unemployment benefits and regional dummies[21]. These variables controls for many transitory shocks that may affect the job search behavior of the unemployed workers.

Table 3.6 reports the estimated effects of migration according to specification 3.14. In addition to the estimates of the fixed-effect specifications in Columns 1 and 3, Table 3.6 displays the results of pooled OLS in Columns 2 and 4. The exits out of unemployment and the explanatory variable of interest (the decision to migrate) are constructed as in Section 5.3. The coefficients in columns 1 and 3 suggest a positive impact of migration on the probability to exit unemployment. Each estimate is either positive and significant or insignificant. The estimates in convention 1 appear this time sensibly higher than in convention 2, for which given the low number of migrations the estimates are more erratic. Nevertheless the main features of the estimates seem to confirm the previous results. There are substantial mobility effects particularly for the unemployed who decide to migrate after a long period of unemployment.

5.4 Controlling for local labor market characteristics

While the previous results provide some evidence of a positive mobility effect on unemployment duration, they do distinguish the mobility effect and the effect of the new labor market. Mobile workers are likely to leave depressed local labor market areas to find better search prospects in a new labor market. In this case, the positive effects found above may mainly reflect unemployment and wage differentials rather than the returns to migration.

[21] Appendix 3.F provides descriptive statistics for the different samples of repeated unemployment spells.

To control for the change of local labor market conditions, I use the specifications 3.15 to 3.17 controlling for the time varying *département* unemployment rate[22]. Table 3.7 reports the estimates of the Cox duration models. In these specifications, the exponential of the estimate of the migration effect can be interpreted as the multiplicative effect of the decision to migrate on the rate of exit out of unemployment. Panel A Column 1, I only include the dummy variable migration. The estimated migration effect on the hazard rate out of unemployment is positive but appears not statistically significant at the 10% level. Column 2, the introduction of the local unemployment rates in specification 3.15 does not change significantly this result. As expected, the specifications indicate the important role of unemployment differentials for the job search process. The initial local unemployment rate appears with the expected negative sign and significant at the 1% level, while the effect of the new local unemployment rates after the migration has a similar sign and magnitude (-0.030 and -0.028). If the unemployed worker moves to a region where the unemployment rate is higher (respectively lower) than in her initial region, her probability to leave unemployment decreases (respectively increases). Columns 3 and 4 of panel A estimate specification 3.16 controlling for the timing of migration, TM_{id}. As in Section 5.2, the estimated effects of migration on unemployment duration appear to depend on the timing of the migrations. The estimates for the dummy variable migration, that indicates the effect of a migration at the beginning of an unemployment spell, are now clearly positive. By contrast, the positive effect of migration appears decreasing with the duration of unemployment. The impact of the migration on the hazard rate is positive if the migration takes place before 6 month unemployment.

In all the specifications of Table 3.5, Panel B, when included, the local unemployment rates are again highly significant, but have small negative effects on the hazard rate. The inclusion of local unemployment rates does not affect the estimates obtained for the migration effects. The estimated effects of workers'mobility appear primarily driven by the migrations rather than the changes in regional labor market conditions. Columns 1 and 2 of panel B estimate specification 3.17 controlling for the timing of migration, TM_{id}, and the time spent in the new region of residence, TSM_{id}. Finally, Columns 3 and 4 allow for interactive effects between the timing of migration and the time spent in the new region of residence. This last specification confirms the concave shape of the effect of migration on the probability to exit unemployment with respect to the time in the new region of residence observed in section 5.2. The migration effect on the hazard rate remains significant until 10 months after the migration. This is a rather intuitive result if the unemployed move to search for work in a new location. Though the residential moves may have disruptive effects, their positive effects on unemployment exits may only last a few months. The estimates suggest that, if after a few months in the new region of residence - here around 10 months -, the unemployed do not find a job, they have exhausted most of the new local opportunities. Then, the residential moves have been unsuccessful and do not have any more impact on the hazard rates out of unemployment.

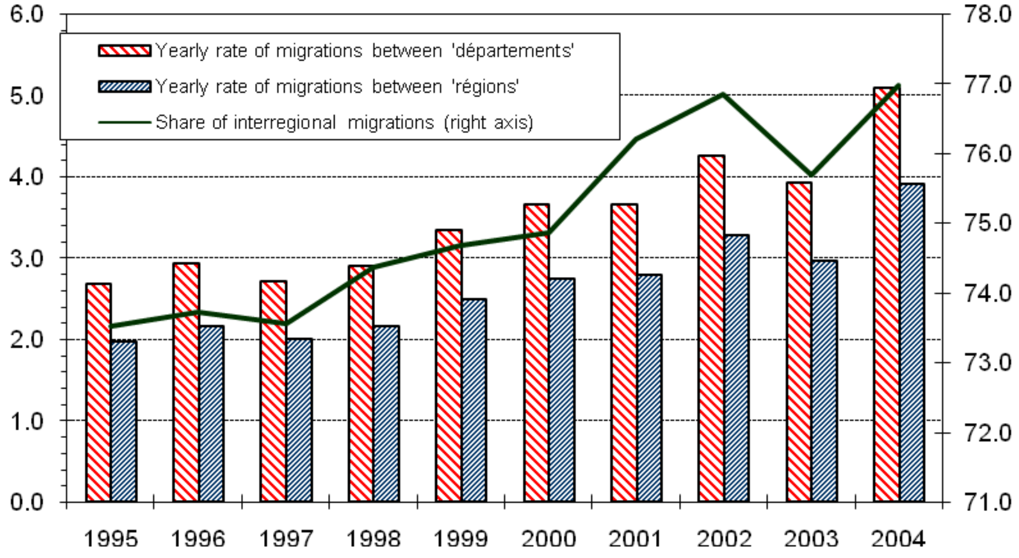
[22] Hachid and Vallon (2006) describe this dataset.

6 Conclusion

I have documented the effect of regional migrations on unemployment duration. Migrations play an important and increasing role in the search process among unemployed workers. The gross migration rates of French unemployed workers increased from 2.9 percents to 5.1 percents over the 1995-2004 period. Job search theory predicts that migrations may provide high returns for unemployed in terms of exit out of unemployment. I rely on several identifying assumptions to approximate the effect of migration on future labor market outcomes. The empirical evidence suggests a significant positive effect of migration on the exits out of unemployment.

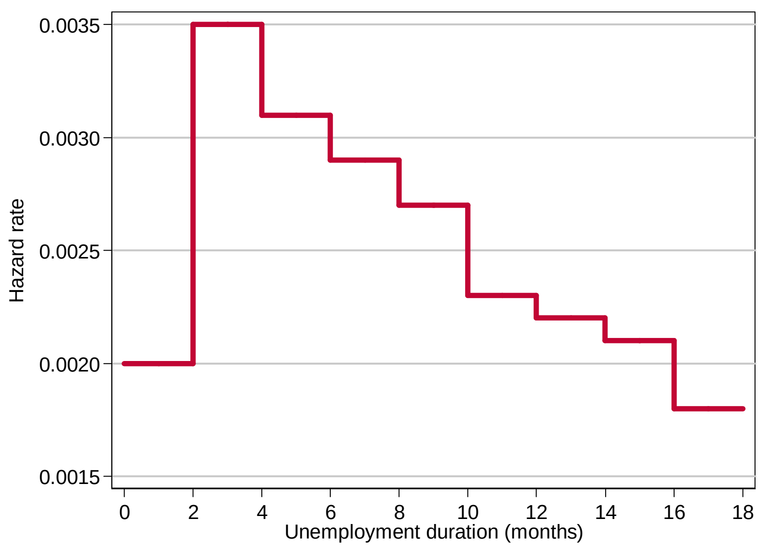
Controlling for numerous observable characteristics using matching estimators reduces the estimated returns to migration, but the estimated returns remain high. I do not find significant differences between the matching estimates and the LPM estimates. Therefore, the main selection process of migrant unemployed workers may be with respect to their potential labor market outcomes in their region of residence rather than on the expected gains of migration to another region. Accounting for unemployed unobservable heterogeneity using repeated unemployment spells and individual fixed effects, my estimates are more erratic but still positive. Finally, I use duration models to disentangle the change in the local labor market conditions from the migration process. The inclusion of time varying local unemployment rates does not appear to reduce the estimated returns to migration.

Figure 3.1. Residential mobility of Unemployed Workers, 1995-2004



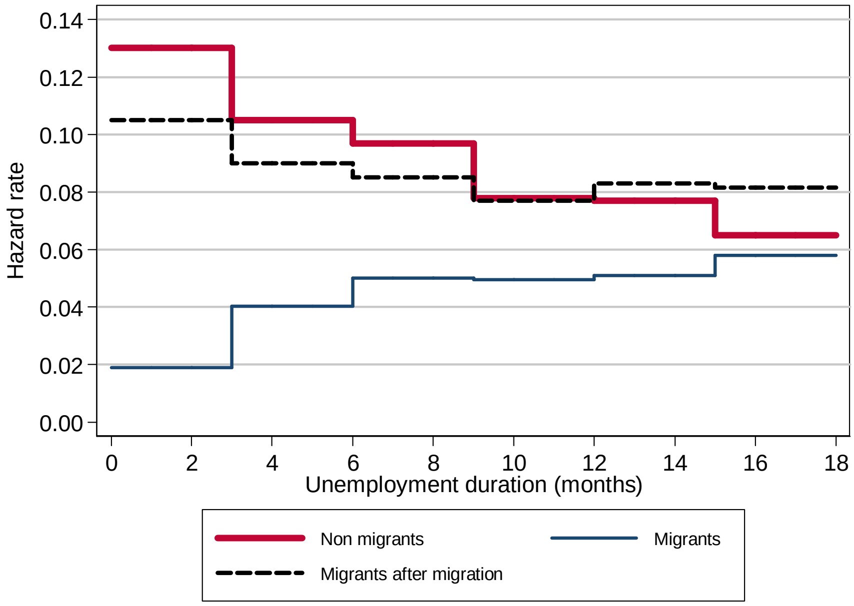
Note: The right and left axes are in percentage. The yearly migration rates are the total of migrations during the year t divided by the number of unemployed workers registered at the beginning of the year t. The sample is restricted to unemployed workers who are immediately available to work, between 18 and 55 years.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Figure 3.2. Monthly empirical hazard rate of migration by months of unemployment



Note: First entering spells per individual between January 2001 and June 2002. Unemployment spells without migration are considered as right censored.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Figure 3.3. Monthly empirical hazard rates out of unemployment by months of unemployment



Note: First entering spells per individual between January 2001 and June 2002. Unemployment spells without migration decision are considered as right censored.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Table 3.1. Correlations between migration rates and regional variables in 2002

	Entry rate (a)	**Exit** rate (b)	**Net migration** rate (c)	**Public rent** level (d)	**Unemployment** rate (e)
Entry rate	1				
Exit rate	0.73	1			
Net migration rate	0.40	-0.33	1		
Public rent level	0.31	0.52	-0.28	1	
Unemployment rate 2002	0.04	-0.07	0.16	0.23	1
Δ unemployment rate (2002-2001)	-0.36	-0.30	-0.09	-0.26	0.02

Note: (a) All the local variables are at the “département” level. The entry rates are the inflows in “département” i during the year 2002 divided by the number of unemployed workers registered at the beginning of the year. (b) The exit rates are the outflows out of “département” i during the year 2002 divided by the number of unemployed workers registered at the beginning of the year. (c) The net migration rates are the net migrations in “département” i during the year 2002 divided by the number of unemployed workers registered at the beginning of the year. (d) The public rent level corresponds to the local average rent per square meter in 2005 (EPLS survey, French Ministry of Housing). (e) INSEE statistics in 2002, see Hachid and Vallon (2006).
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE. French Census 1999, INSEE.

Table 3.2. Unemployment duration and migration

Unemployment duration	2 months		6 months		10 months	
Migration :	Non movers	Movers	Non movers	Movers	Non movers	Movers
Houaehold characteristics (a)						
Male and single	29.66	33.06	27.97	28.40	26.57	29.77
Female and single	27.64	31.55	26.77	31.21	26.20	30.65
Male and in couple	16.68	12.02	17.44	14.25	18.16	15.06
Female and in couple	26.02	23.37	27.82	26.14	29.07	24.52
Number of Children						
No Child	59.38	69.48	56.59	66.35	54.74	62.88
One Child	16.59	14.12	17.69	18.44	18.34	19.96
Two children or more	24.03	16.40	25.72	15.21	26.92	17.16
Age						
Less than 25 years old	25.22	32.03	21.27	28.06	18.37	24.87
25 to 35 years old	34.45	40.84	34.28	41.96	33.62	42.73
35 to 45 years old	24.04	17.52	25.72	19.14	27.00	21.02
45 to 55 years old	16.29	9.61	18.73	10.84	21.01	11.38
Nationality						
French	91.04	83.47	90.91	91.08	90.92	93.35
Not French	8.96	16.53	9.09	8.92	9.08	6.65
Education						
Left school before high school	35.60	28.55	37.18	25.09	37.93	21.19
End of high School diploma	16.17	17.74	15.60	17.57	15.26	19.79
Secondary education	18.93	24.58	18.04	27.10	17.68	24.69
Others	29.30	29.13	29.18	30.24	29.13	34.33
Qualification						
Unskilled workers	19.53	20.28	20.02	16.96	10.68	13.83
Skilled workers	19.87	13.81	20.19	11.36	20.32	11.56
Employees	43.35	41.87	44.27	47.38	54.80	51.84
Executives and managers	10.44	11.53	11.00	16.00	11.65	14.36
Unknown	6.81	12.51	4.52	8.30	2.55	8.41
Desired Contract (duration and type)						
Full time and permanent	77.90	83.96	75.72	81.56	73.49	81.44
Others	22.10	16.04	24.28	18.44	26.51	18.56
Labor market characteristics						
No part time job	47.00	48.44	41.25	45.98	38.31	39.40
Part time job	53.00	51.56	58.75	54.02	61.69	60.60
Minimum Income Program	10.68	8.53	11.11	8.22	11.24	7.53
Disabled	6.28	2.86	7.09	3.23	7.71	3.15
Reason of entry into unemployment						
End of contract	7.61	5.05	8.72	7.26	9.66	9.63
Dismissal	22.39	24.04	24.15	26.57	25.66	30.30
Demission	37.65	34.05	36.17	36.10	34.54	30.65
First entry on the labour market	5.60	10.50	4.32	6.82	3.58	4.38
Others	26.75	26.36	26.64	23.25	26.56	25.04
Unemployment history since 1995						
Less than 2 unemployment spells	38.12	31.10	38.08	35.31	38.48	32.57
More than 2 unemployment spells	61.88	68.90	61.92	64.69	61.52	67.43
No previous unemployment duration	36.28	30.56	35.12	32.00	35.09	31.87
1 to 6 months unemployment	18.03	20.51	18.04	19.14	18.20	23.12
More than 6 months	45.69	48.93	46.84	48.86	46.71	45.01
Total	256 381	1 798	213 443	1 144	150 147	571
Percentage	99.30	0.70	99.47	0.53	99.62	0.38

Note: (a) The numbers refer to the column percentages. The sample includes the first unemployment spell of each worker between January 2001 and June 2002. For each unemployment duration, t, the movers are the unemployed workers who changed region during month t-2 and t.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Table 3.3. Logistic estimation of the propensity score by previously elapsed duration

Previously elapsed unemployment duration	**2 months** (a)	**6 months**	**10 months**
	Estimate	**Estimate**	**Estimate**
Intercept	-5.394***	-5.351***	-5.409***
	(0.138)	(0.183)	(0.248)
Personal characteristics (male and single)(b)			
Female and single	0.010	0.125	-0.005
	(0.056)	(0.080)	(0.113)
Male and in couple	-0.192***	0.167	-0.016
	(0.080)	(0.108)	(0.151)
Female and in couple	0.002	0.235**	-0.105
	(0.069)	(0.096)	(0.138)
Number of Children (No Child)			
One Child	-0.219***	-0.118	0.062
	(0.069)	(0.088)	(0.123)
Two children or more	-0.343***	-0.562***	-0.307**
	(0.072)	(0.101)	(0.140)
Age (Less than 25 years old)			
25 to 35 years old	-0.094*	-0.134*	-0.101
	(0.057)	(0.081)	(0.116)
35 to 45 years old	-0.411***	-0.434***	-0.397***
	(0.074)	(0.101)	(0.142)
45 to 55 years old	-0.544***	-0.725***	-0.744***
	(0.087)	(0.117)	(0.164)
Nationality (French)			
Not French	0.897***	0.190*	-0.098
	(0.061)	(0.107)	(0.172)
Education (Left school before high school)			
End of high School diploma	0.227***	0.229**	0.576***
	(0.069)	(0.098)	(0.139)
Secondary education	0.384***	0.442***	0.666***
	(0.072)	(0.100)	(0.146)
Others	0.229***	0.303***	0.589***
	(0.058)	(0.082)	(0.119)
Qualification (Unskilled workers)			
Skilled workers	-0.317***	-0.325***	-0.397**
	(0.077)	(0.117)	(0.164)
Employees	-0.088	0.145*	0.012
	(0.061)	(0.088)	(0.122)
Executives and managers	0.033	0.385***	0.014
	(0.093)	(0.124)	(0.176)
Unknown	0.482***	0.606***	1.126***
	(0.080)	(0.131)	(0.185)
Desired Contract (Permanent and full time)			
Others	-0.237***	-0.202**	-0.326***
	(0.060)	(0.079)	(0.111)

Note: Standard-errors are in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels.
(a) For each date, we model the probability to move during the first two months, the 5th and 6th months and the 9th and 10th months conditionally to being still unemployed at the end of the 2nd, 6th and 10th months. (b) Reference category in parenthesis.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Table 3.3. Logistic estimation of the propensity score by previously elapsed duration (continued)

Previously elapsed unemployment duration	**2 months** (a)	**6 months**	**10 months**
	Estimate	**Estimate**	**Estimate**
Labor market characteristics			
Part time job	-0.073*	-0.283***	-0.141
	(0.043)	(0.062)	(0.089)
Minimum Income Program	-0.333***	-0.326***	-0.442***
	(0.079)	(0.113)	(0.165)
Disabled	-0.653***	-0.588***	-0.708***
	(0.129)	(0.171)	(0.244)
Reason of entry into unemployment (End of contract)(b)			
Dismissal	0.397***	0.221***	0.085***
	(0.104)	(0.125)	(0.156)
Demission	0.096***	0.129***	-0.260***
	(0.103)	(0.125)	(0.161)
Entrants to the labor force	0.762***	0.495***	-0.073***
	(0.122)	(0.169)	(0.257)
Others	0.264***	0.063***	-0.058***
	(0.105)	(0.130)	(0.164)
Unemployment history since 1995			
More than 2 unemp. Spells	0.199***	0.010*	0.094
	(0.074)	(0.100)	(0.143)
1 to 6 months unemployment	0.330***	0.204*	0.258*
	(0.080)	(0.112)	(0.154)
More than 6 months	0.383**	0.326***	0.106
	(0.073)	(0.100)	(0.141)
Cohort Effect (First term 2001)			
Second term 2001	0.014	0.323***	-0.038
	(0.077)	(0.102)	(0.150)
Third term 2001	0.153**	-0.295***	-0.330**
	(0.071)	(0.113)	(0.157)
Fourth term 2001	-0.224***	-0.214**	0.340***
	(0.081)	(0.115)	(0.141)
First term 2002	-0.192**	0.065	-0.157
	(0.092)	(0.118)	(0.173)
Second term 2002	0.031	0.160	-0.091
	(0.093)	(0.122)	(0.177)
Model statistics			
Percentage Concordant	58.9	58.9	59.1
Percentage Discordant	25.2	27.7	21.2
Percentage Tied	15.9	13.4	19.6
-2 Log L	25943.2	13856.8	7250.0
Number of observations	258 179	214 577	151 118

Note: Standard-errors are in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels.
(a) For each date, we model the probability to move during the first two months, the 5th and 6th months and the 9th and 10th months conditionally to being still unemployed at the end of the 2nd, 6th and 10th months.
(b) Reference category in parenthesis.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Table 3.4. Migration and exit from unemployment after 2, 6 and 10 months

	Naive	**Weighting** (b)	**Kernel** (c)	**LPM** (d)	**Logit** (e)	**Probit** (e)
	Average Treatment effect on the Treated after 2 months					
After 4 months (a)	0.079***	0.052***	0.053***	0.051***	0.050***	0.049***
	(0.010)	(0.006)	(0.012)	(0.009)	(0.012)	(0.012)
After 7 months	0.104***	0.070***	0.071***	0.068***	0.068***	0.068***
	(0.011)	(0.009)	(0.015)	(0.010)	(0.012)	(0.012)
After 10 months	0.109***	0.071***	0.072***	0.069***	0.069***	0.069***
	(0.011)	(0.011)	(0.016)	(0.010)	(0.012)	(0.012)
After 13 months	0.102***	0.064***	0.065***	0.062***	0.062***	0.063***
	(0.010)	(0.012)	(0.017)	(0.010)	(0.012)	(0.012)
After 16 months	0.112***	0.076***	0.078***	0.074***	0.074***	0.074***
	(0.010)	(0.013)	(0.018)	(0.010)	(0.012)	(0.012)
After 19 months	0.110***	0.078***	0.080***	0.075***	0.076***	0.075***
	(0.010)	(0.014)	(0.018)	(0.009)	(0.011)	(0.011)
Observations	258 179	258 179	258 179	258 179	258 179	258 179
	Average Treatment effect on the Treated after 6 months					
After 8 months	0.035***	0.009	0.010	0.010	0.010	0.010
	(0.012)	(0.007)	(0.014)	(0.012)	(0.015)	(0.014)
After 11 months	0.060***	0.023**	0.023	0.024	0.023	0.024
	(0.014)	(0.011)	(0.018)	(0.014)	(0.016)	(0.016)
After 14 months	0.092***	0.052***	0.052**	0.052***	0.052***	0.052***
	(0.015)	(0.013)	(0.021)	(0.014)	(0.017)	(0.017)
After 17 months	0.098***	0.057***	0.057***	0.057***	0.057***	0.057***
	(0.015)	(0.015)	(0.022)	(0.014)	(0.017)	(0.017)
After 20 months	0.104***	0.065***	0.065***	0.065***	0.065***	0.065***
	(0.014)	(0.017)	(0.023)	(0.014)	(0.016)	(0.016)
After 23 months	0.107***	0.072***	0.071***	0.071***	0.072***	0.071***
	(0.014)	(0.018)	(0.024)	(0.014)	(0.016)	(0.016)
Observations	214 577	214 577	214 577	214 577	214 577	214 577
	Average Treatment effect on the Treated after 10 months					
After 12 months	0.063***	0.030***	0.034*	0.031*	0.030	0.030
	(0.017)	(0.009)	(0.019)	(0.016)	(0.019)	(0.019)
After 15 months	0.129***	0.084***	0.087***	0.083***	0.083***	0.084***
	(0.020)	(0.014)	(0.026)	(0.020)	(0.022)	(0.022)
After 18 months	0.163***	0.115***	0.119***	0.115***	0.115***	0.115***
	(0.021)	(0.018)	(0.030)	(0.021)	(0.023)	(0.023)
After 21 months	0.151***	0.105***	0.108***	0.104***	0.104***	0.104***
	(0.021)	(0.021)	(0.032)	(0.021)	(0.023)	(0.023)
After 24 months	0.138***	0.094***	0.095***	0.093***	0.093***	0.092***
	(0.020)	(0.023)	(0.033)	(0.020)	(0.022)	(0.022)
After 27 months	0.143***	0.110***	0.112***	0.110***	0.111***	0.109***
	(0.019)	(0.025)	(0.035)	(0.019)	(0.021)	(0.021)
Observations	151 118	151 118	151 118	151 118	151 118	151 118

Note: Standard-errors in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels. (a) Duration of the unemployment spell in months. (b) Standard-errors are computed by delta-method. (d) Standard-errors are computed assuming fixed weights. (e) LPM denotes Linear Probability Model. Huber-White standard-errors. (f) Average marginal effect of migration on the treated. Standard-errors are computed by delta-method. The sample includes the first unemployment spell of each worker between January 2001 and June 2002.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Table 3.5. Long-distance migration and exit from unemployment after 2, 6 and 10 months

	Naive	**Weighting** (c)	**Kernel** (d)	**LPM** (e)	**Logit** (f)	**Probit** (f)
	Average Treatment effect on the Treated after 2 months (a)					
After 4 months (b)	0.093***	0.065***	0.065***	0.064***	0.063***	0.062***
	(0.012)	(0.007)	(0.014)	(0.011)	(0.013)	(0.013)
After 7 months	0.128***	0.092***	0.092***	0.090***	0.089***	0.090***
	(0.012)	(0.011)	(0.018)	(0.012)	(0.014)	(0.014)
After 10 months	0.126***	0.087***	0.088***	0.085***	0.085***	0.085***
	(0.012)	(0.013)	(0.019)	(0.012)	(0.014)	(0.014)
After 13 months	0.123***	0.083***	0.084***	0.081***	0.081***	0.081***
	(0.012)	(0.014)	(0.020)	(0.012)	(0.013)	(0.013)
After 16 months	0.127***	0.089***	0.091***	0.087***	0.088***	0.087***
	(0.011)	(0.015)	(0.021)	(0.011)	(0.013)	(0.013)
After 19 months	0.121***	0.087***	0.089***	0.084***	0.086***	0.085***
	(0.011)	(0.016)	(0.021)	(0.011)	(0.013)	(0.013)
Observations	258 179	258 179	258 179	258 179	258 179	258 179
	Average Treatment effect on the Treated after 6 months					
After 8 months	0.036***	0.010	0.010	0.011	0.011	0.010
	(0.015)	(0.008)	(0.017)	(0.014)	(0.017)	(0.016)
After 11 months	0.065***	0.030**	0.030	0.031*	0.030	0.031*
	(0.017)	(0.012)	(0.021)	(0.016)	(0.018)	(0.018)
After 14 months	0.096***	0.058***	0.058***	0.058***	0.058***	0.058***
	(0.017)	(0.016)	(0.024)	(0.017)	(0.019)	(0.019)
After 17 months	0.106***	0.068***	0.068***	0.068***	0.068***	0.068***
	(0.017)	(0.018)	(0.026)	(0.017)	(0.019)	(0.019)
After 20 months	0.114***	0.077***	0.077***	0.077***	0.077***	0.077***
	(0.017)	(0.020)	(0.028)	(0.017)	(0.018)	(0.018)
After 23 months	0.114***	0.080***	0.079***	0.080***	0.080***	0.079***
	(0.016)	(0.021)	(0.029)	(0.016)	(0.018)	(0.018)
Observations	214 577	214 577	214 577	214 577	214 577	214 577
	Average Treatment effect on the Treated after 10 months					
After 12 months	0.085***	0.052***	0.055**	0.052***	0.051**	0.051**
	(0.020)	(0.010)	(0.023)	(0.019)	(0.022)	(0.022)
After 15 months	0.141***	0.094***	0.097***	0.094***	0.093***	0.094***
	(0.024)	(0.016)	(0.031)	(0.022)	(0.025)	(0.025)
After 18 months	0.180***	0.131***	0.134***	0.130***	0.130***	0.130***
	(0.024)	(0.021)	(0.035)	(0.023)	(0.025)	(0.025)
After 21 months	0.166***	0.118***	0.120***	0.117***	0.117***	0.118***
	(0.024)	(0.024)	(0.037)	(0.023)	(0.025)	(0.025)
After 24 months	0.154***	0.109***	0.111***	0.108***	0.108***	0.108***
	(0.023)	(0.026)	(0.038)	(0.023)	(0.025)	(0.025)
After 27 months	0.162***	0.129***	0.131***	0.129***	0.130***	0.129***
	(0.022)	(0.029)	(0.041)	(0.022)	(0.024)	(0.023)
Observations	151 118	151 118	151 118	151 118	151 118	151 118

Note: Standard-errors in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels. (a) Duration of the unemployment spell in months. (b) Standard-errors are computed by delta-method. (d) Standard-errors are computed assuming fixed weights. (e) LPM denotes Linear Probability Model. Huber-White standard-errors. (f) Average marginal effect of migration on the treated. Standard-errors are computed by delta-method. The sample includes the first unemployment spell of each worker between January 2001 and June 2002.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Table 3.6. Migration and exit from unemployment after 2, 6 and 10 months, repeated unemployment spells

	Convention 1		Convention 2	
	Within	**Pooled OLS** (b)	**Within**	**Pooled OLS** (b)
Average Treatment effect on the Treated after 2 months				
After 4 months (a)	0.058 ***	0.068 ***	0.002	0.024
	(0.022)	(0.010)	(0.055)	(0.018)
After 7 months	0.028	0.062 ***	0.020	0.019
	(0.017)	(0.009)	(0.033)	(0.013)
After 10 months	0.028 **	0.053 ***	0.004	0.014
	(0.015)	(0.008)	(0.028)	(0.012)
After 13 months	0.009	0.044 ***	-0.003	0.015
	(0.012)	(0.007)	(0.024)	(0.011)
After 16 months	0.011	0.034 ***	-0.009	0.012
	(0.010)	(0.007)	(0.020)	(0.009)
After 19 months	0.004	0.026 ***	0.007	0.008
	(0.008)	(0.006)	(0.016)	(0.008)
Observations (c)	238 626	537 506	80 611	195 616
Average Treatment effect on the Treated after 6 months				
After 8 months	0.077	0.010	-0.009	0.006
	(0.050)	(0.011)	(0.086)	(0.017)
After 11 months	0.012	0.016	0.047	0.002
	(0.048)	(0.013)	(0.081)	(0.018)
After 14 months	-0.005	0.024	0.057	0.017
	(0.041)	(0.013)	(0.071)	(0.017)
After 17 months	-0.022	0.015	-0.059	0.025
	(0.034)	(0.012)	(0.061)	(0.016)
After 20 months	0.009	0.027 **	-0.006	0.036
	(0.028)	(0.011)	(0.050)	(0.013)
After 23 months	-0.012	0.024 **	0.006	0.034
	(0.023)	(0.009)	(0.039)	(0.011)
Observations	66 843	308 130	20 753	106 757
Average Treatment effect on the Treated after 10 months				
After 12 months	0.309	0.056 ***	0.028	0.040***
	(0.210)	(0.015)	(0.165)	(0.025)
After 15 months	0.171	0.050 ***	0.058	0.082***
	(0.193)	(0.018)	(0.165)	(0.027)
After 18 months	0.065	0.058 ***	0.162	0.075***
	(0.164)	(0.019)	(0.144)	(0.025)
After 21 months	0.162	0.031 *	0.224**	0.058***
	(0.134)	(0.017)	(0.119)	(0.023)
After 24 months	0.065	0.013	0.126	0.024
	(0.107)	(0.015)	(0.095)	(0.021)
After 27 months	0.029	0.016	-0.028	0.023
	(0.072)	(0.013)	(0.073)	(0.018)
Observations	17 854	192 541	5 332	65 285

Note: Standard-errors in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels. (a) Duration of the unemployment spell in months. (b) Huber-White standard errors in parenthesis. (c) For the within estimator, observations are repeated unemployment spells. For the pooled OLS, observations are all unemployment spells.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

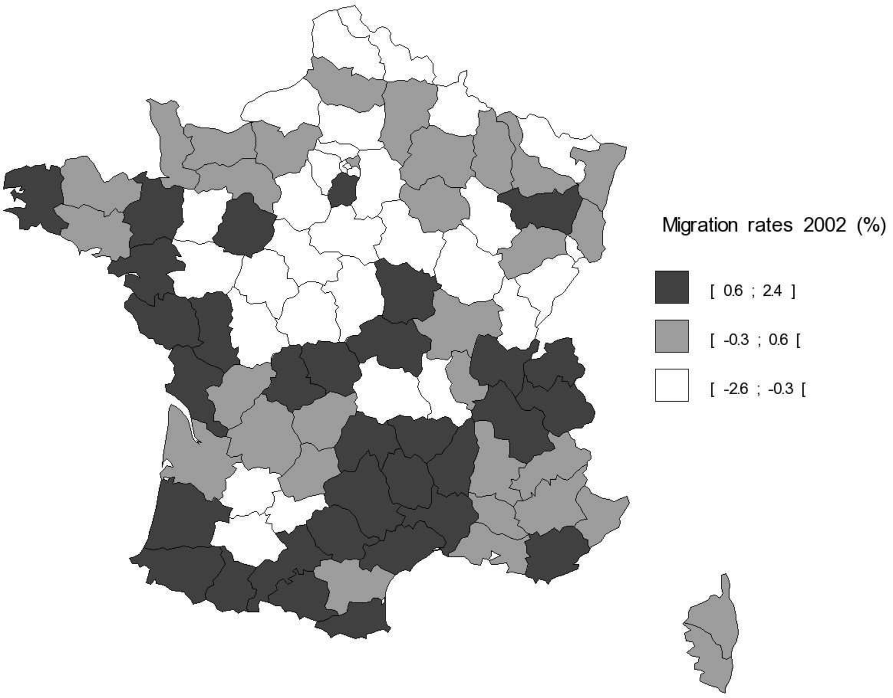
Table 3.7. Migration and unemployment duration controlling for regional unemployment differentials

	Cox semiparametric duration model			
Panel A. Controlling for the timing of migration				
	(1)	**(2)**	**(3)**	**(4)**
Migration (b)	0.036	0.038	0.372***	0.372***
	(0.037)	(0.037)	(0.063)	(0.063)
Migration*ln(number of months before the migration, T)			-0.212***	-0.211***
			(0.034)	(0.034)
Unemployment rate at the beginning of the unemployment spell		-0.030***		-0.030***
		(0.002)		(0.002)
Migration*unemployment differential (c)		-0,028**		-0.025**
		(0.011)		(0.011)
Number of observations	377 317	377 317	377 317	377 317
Panel B. Controlling for the timing of migration and the time spent in the new region of residence				
	(1)	**(2)**	**(3)**	**(4)**
Migration (b)	0.481***	0.481***	0.906***	0.908***
	(0.099)	(0.099)	(0.123)	(0.129)
Migration* ln(number of months before the migration, T)	-0.207***	-0.206***	-0.494***	-0.495***
	(0.034)	(0.034)	(0.068)	(0.068)
Migration*ln(number of months after the migration, TA)	-0.061	-0.061	-0.366***	-0.368***
	(0.043)	(0.043)	(0.074)	(0.074)
Migration*ln(T)*ln(TA)			0.193***	0.192***
			(0.037)	(0.037)
Unemployment rate at the beginning of the unemployment spell		-0.030***		-0.030***
		(0.002)		(0.002)
Migration*unemployment differential (c)		-0,025**		-0,027**
		(0,011)		(0,011)
Number of observations	377 317	377 317	377 317	377 317

Note: (a) Standard-errors in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels. Each model incorporates the non time varying covariates described in table 2. (b) The migration variable is a time varying dummy variable taking value one after the migration. (c) The unemployment differential is the difference between the local unemployment rates before and after the migration.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

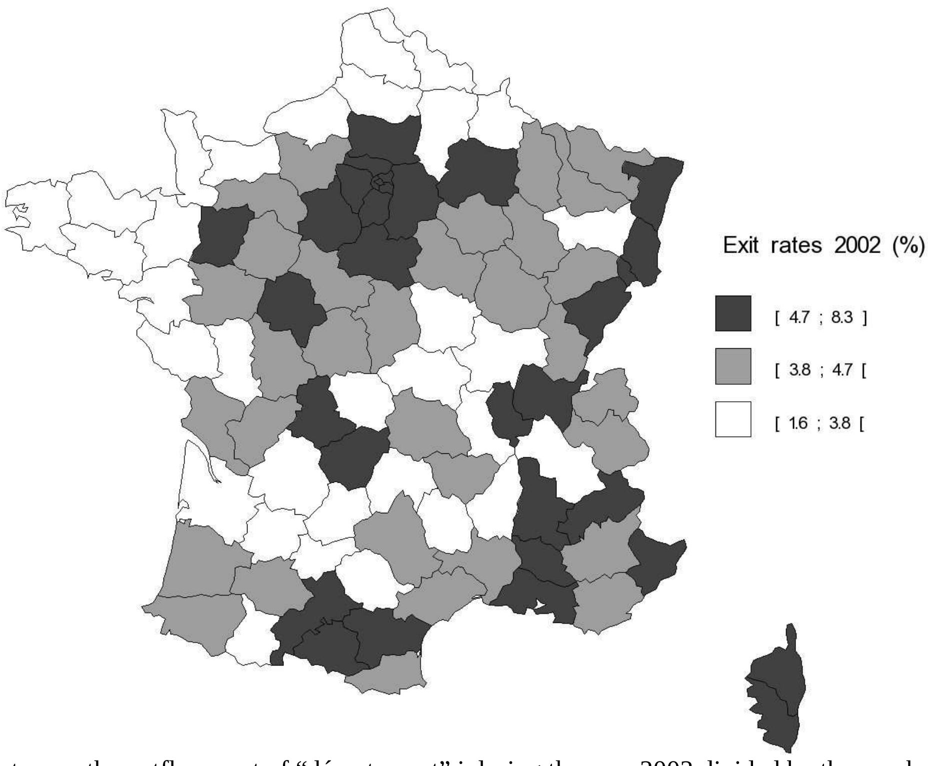
Appendix 3.A. Descriptive statistics

Figure 3.A.1 Migrations rates in 2002



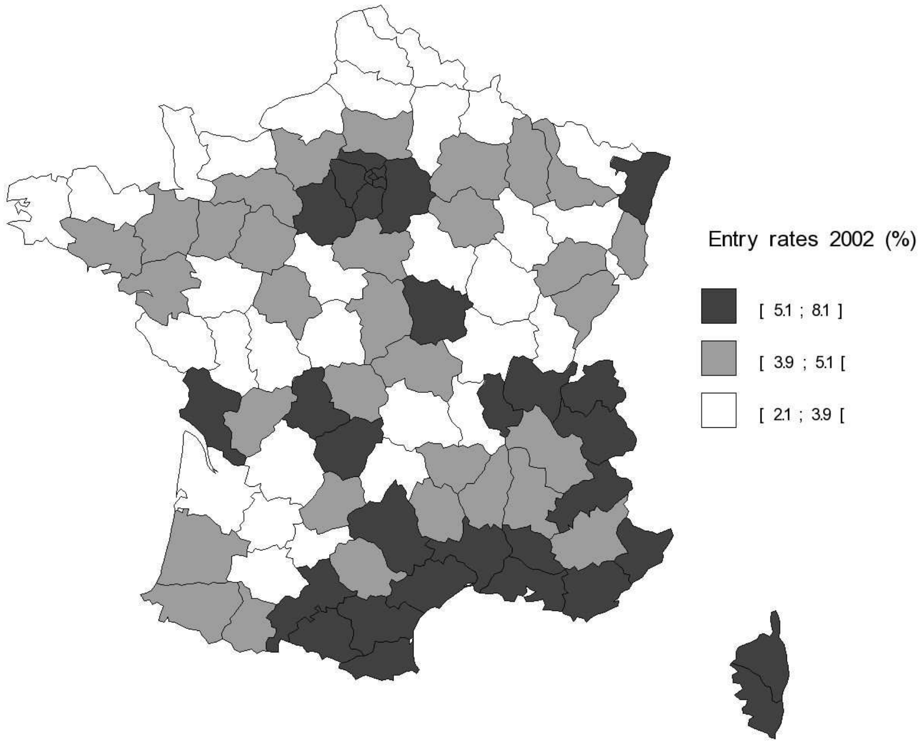
Note: the migration rates are the net migrations in "département" i during the year 2002 divided by the number of unemployed workers registered at the beginning of the year.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Figure 3.A.2 Exit rates in 2002



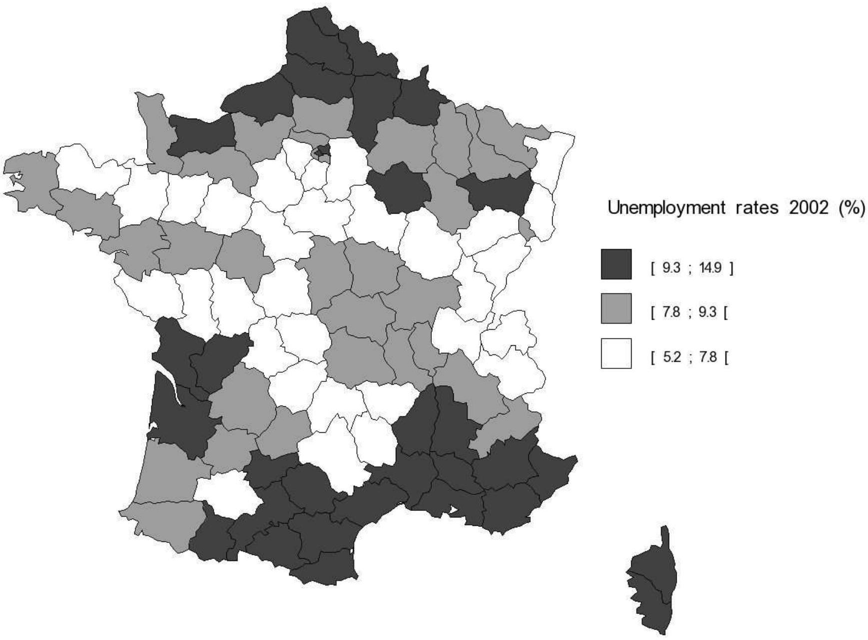
Note: the exit rates are the outflows out of "département" i during the year 2002 divided by the number of unemployed workers registered at the beginning of the year.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Figure 3.A.3 Entry rates in 2002



Note: the entry rates are the inflows in “department” i during the year 2002 divided by the number of unemployed workers registered at the beginning of the year.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Figure 3.A.4 Unemployment rates in 2002



Source: INSEE unemployment rates (Hachid and Vallon, 2006).

Appendix 3.B. Outflows out of unemployment and status of observations

Destinations of outflow out of unemployment	Non movers		Movers		Definition	
	Number	Percentage	Number	Percentage	Definition 1	Definition 2
Employment	104565	28.41	2336	25.37	None	None
Training	33153	9.01	995	10.81	Censored (b)	Dropped
Health problems	19210	5.22	639	6.94	Dropped	Dropped
Exemption of job search	5275	1.43	179	1.94	Dropped	Dropped
Pension	91	0.02	0	0.00	Dropped	Dropped
Change of ASSEDIC (a)	3933	1.07	129	1.40	Censored	Censored
Military service	110	0.03	1	0.01	Dropped	Dropped
Other ends of job search	10821	2.94	219	2.38	Censored	Dropped
Unknown	124525	33.83	2478	26.92	Censored	Dropped
Administrative removals for missed appointment	9961	2.71	200	2.17	Censored	Dropped
Other administrative removals	18430	5.01	399	4.33	Censored	Dropped
Other motivations	18441	5.01	410	4.45	Censored	Dropped
Missed follow-up	969	0.26	22	0.24	Censored	Dropped
Right censored	18627	5.06	1199	13.02	None	None
Total	368111		9206		331986	111954
Percentage	97.56		2.44		87.99	29.67
Retained immobile workers					324 798	109467
Retained mobile workers					7188	2487

Note: First spell entering into unemployment between January 2001 and June 2002.
(a) A change of ASSEDIC corresponds to a migration but in this case the unemployed workers is considered has a new unemployed worker in his new place of residence.
(b) I treat these observations as postponed outflows. I adopt a right censoring strategy and I take these observations into account not as instantaneous outflows out of unemployment but as outflows 2 months later.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Appendix.3.C. Variance of the Inverse Propensity score Weighted Estimator

The Inverse Propensity score Weighted (IPW) estimator of equation 3.10 is:

$$\widehat{\mathrm{ATT}}_{t,n} = \frac{1}{\sum_k D_{kt}} \sum_i [Y_{i,t+n}.\{D_{it} - (1 - D_{it}).\frac{\hat{s}(X_{it})}{1 - \hat{s}(X_{it})}\}] \ ,$$

where i and k are indices for unemployed workers, t for unemployment duration at the time of migration, and n for the time spent in the new region of residence, and $\hat{s}(X_{it})$ is a consistent estimator of the propensity score of migration (Wooldridge, 2010). I omit the t and n indices:

$$\widehat{\mathrm{ATT}} = \frac{N}{\sum_k D_k} \frac{1}{N} \sum_i [Y_i.D_i] - \underbrace{\frac{N}{\sum_k D_k} \frac{1}{N} \sum_i [Y_i.(1 - D_i).\frac{\hat{s}(X_i)}{1 - \hat{s}(X_i)}]}_{=Z} \ ,$$

where N is the number of unemployed workers.
By a law of large numbers and Slutsky's theorem, under standard regularity conditions (e.g. Van Der Vaart, 1998):

$$\frac{N}{\sum_k D_k} \frac{1}{N} \sum_i [Y_i.D_i] = E(D_i)^{-1} E(D_i.Y_i) + \mathrm{o}_p(1) = E(Y_i|D_i = 1) + \mathrm{o}_p(1)$$

Similarly,

$$Z = (E(D_i) + \mathrm{o}_p(1))^{-1}.\frac{1}{N} \sum_i [Y_i.(1 - D_i).\frac{\hat{s}(X_i)}{1 - \hat{s}(X_i)}] \ .$$

I estimate $\hat{s}(X_{it})$ with a flexible logit model so that:

$$\begin{aligned}
Z &= (E(D_i) + \mathrm{o}_p(1))^{-1}.\frac{1}{N} \sum_i [Y_i.(1 - D_i).\exp(X_i\hat{\beta})] \\
Z &= (E(D_i) + \mathrm{o}_p(1))^{-1}.\frac{1}{N} \sum_i [Y_i.(1 - D_i).\{\exp(X_i\beta) + \exp(X_i\beta)X_i(\hat{\beta} - \beta) + \mathrm{o}_p(\hat{\beta} - \beta)\}] \\
Z &= (E(D_i) + \mathrm{o}_p(1))^{-1}.\{E[Y_i.(1 - D_i).\exp(X_i\beta)] + E[Y_i.(1 - D_i)\exp(X_i\beta)X_i](\hat{\beta} - \beta) + \mathrm{o}_p(1)\}
\end{aligned}$$

Hence, as $\hat{\beta} - \beta = \mathrm{O}_p(N^{-1/2})$, Z is asymptotically equivalent to:

$$Z = E(D_i)^{-1} E[Y_i.(1 - D_i).\exp(X_i\beta)] + \mathrm{o}_p(1) \ .$$

If the propensity score is well specified, equation 3.8 implies:

$$\begin{aligned}
Z &= E(D_i)^{-1} E[E(Y_i^0.(1 - D_i)|X_i).\exp(X_i\beta)] + \mathrm{o}_p(1) \\
Z &= E(D_i)^{-1}.E[E(Y_i^0|X_i, D_i = 0).E(D_i|X_i)] + \mathrm{o}_p(1) = E(Y_i^0|D_i = 1) + \mathrm{o}_p(1)
\end{aligned}$$

Then, $\widehat{ATT} = E(Y_i^1 - Y_i^0|D_i = 1) + \mathrm{o}_p(1) = ATT + \mathrm{o}_p(1)$.

I now derive the asymptotic distribution of the estimator:

$$\begin{aligned} N^{1/2}.(\widehat{ATT} - ATT) &= N^{1/2}\{E(D_i)^{-1}.\frac{1}{N}\sum_i [Y_i D_i - \exp(X_i\beta)Y_i(1-D_i)] \\ &\quad - E(D_i)^{-1}E(Y_i(1-D_i)\exp(X_i\beta)X_i)(\hat{\beta}-\beta) - ATT\} + \mathrm{o}_p(1) \end{aligned}$$

using $\frac{\sum_k D_k}{N} = E(D_i) + \mathrm{O}_p(N^{-1/2})$.
The first order condition of the estimation of the propensity score is:

$$\frac{1}{N}.\sum_i [(D_i - \hat{s}(X_i))X_i^t] = 0 \ .$$

Expending around the true value β:

$$\begin{aligned} \frac{1}{N}.\sum_i [(D_i - \hat{s}(X_i))X_i^t] &= \frac{1}{N}.\sum_i [(D_i - s(X_i))X_i^t] + \\ &\quad \frac{1}{N}.\frac{\partial \sum_i [(D_i - s(X_i))X_i^t]}{\partial \beta^t})(\hat{\beta}-\beta) + \mathrm{o}_p(N^{-1/2}) = 0 \\ &= \frac{1}{N}.\sum_i [(D_i - s(X_i))X_i^t] - \\ &\quad \frac{1}{N}.\sum_i [(1 - s(X_i))s(X_i)X_i^t X_i](\hat{\beta}-\beta) + \mathrm{o}_p(N^{-1/2}) = 0 \ . \end{aligned}$$

This leads to the approximation of $\hat{\beta} - \beta$:

$$\begin{aligned} N^{1/2}(\hat{\beta}-\beta) &= \{\frac{1}{N}.\sum_i [(1 - s(X_i))s(X_i)X_i^t X_i]\}^{-1}\frac{1}{N}.\sum_i [(D_i - s(X_i))X_i^t] + \mathrm{o}_p(1) \\ &= \{E[(1 - s(X_i))s(X_i)X_i^t X_i]\}^{-1}\frac{1}{N}.\sum_i [(D_i - s(X_i))X_i^t] + \mathrm{o}_p(1) \ . \end{aligned}$$

Therefore, $N^{1/2}.(\widehat{ATT} - ATT)$ is also asymptotically equivalent to:

$$\begin{aligned} N^{1/2}.(\widehat{ATT} - ATT) &= N^{1/2}\{E(D_i)^{-1}.\sum_i [Y_i D_i - \exp(X_i\beta)Y_i(1-D_i)] - \\ &\quad E(D_i)^{-1}E(Y_i(1-D_i)\exp(X_i\beta)X_i)\{E[(1 - s(X_i))s(X_i)X_i^t X_i]\}^{-1} \\ &\quad \times\frac{1}{N}\sum_i [(D_i - s(X_i))X_i^t] - ATT\} + \mathrm{o}_p(1) \end{aligned}$$

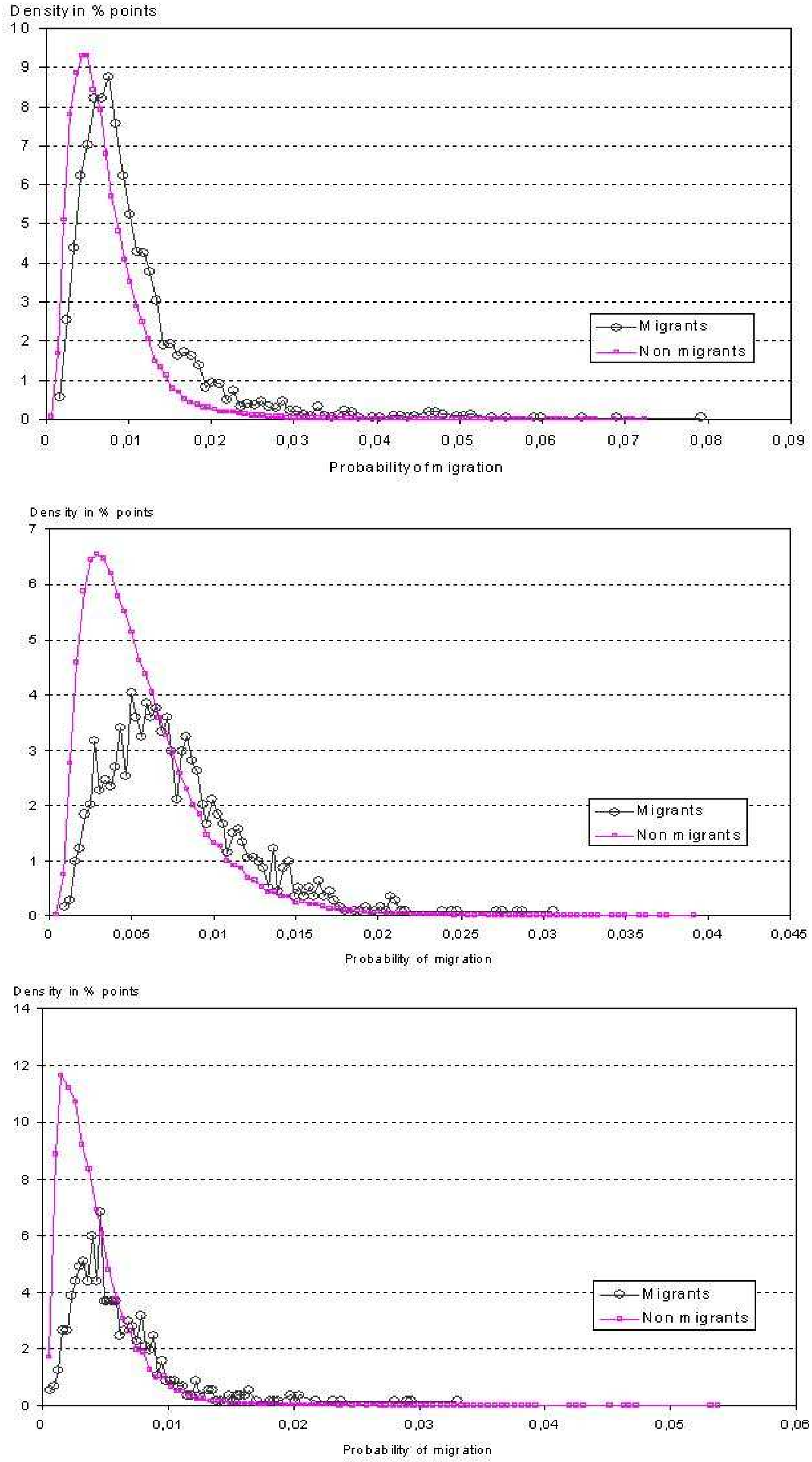
That is:

$$N^{1/2}.(\widehat{ATT} - ATT) = N^{1/2}[\frac{1}{N}.\sum_i \Omega_i] + \mathrm{o}_p(1)$$

And, by a central limit theorem, $N^{1/2}.(\widehat{ATT} - ATT) \rightsquigarrow N(0, V_{as}(\widehat{ATT}))$.
Thus, I estimate:

$$\hat{V}_{as}(\widehat{ATT}) = \frac{1}{N(N-1)}\sum_i (\hat{\Omega}_i - \bar{\hat{\Omega}}_i)^2 \ .$$

Appendix 3.D. Histograms of estimated probability of migration after 2, 6 and 10 month unemployment (convention 1)



<u>Source:</u> Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Appendix Table 3.E. Migration and exit from unemployment after 2, 6 and 10 months, second definition of unemployment outflows

	Naive	**Weighting** (c)	**Kernel** (d)	**LPM** (e)	**Logit** (f)	**Probit** (f)
			Average Treatment effect on the Treated after 2 months (a)			
After 4 months (b)	0.098***	0.039***	0.040***	0.044**	0.042**	0.043**
	(0.018)	(0.013)	(0.022)	(0.017)	(0.021)	(0.021)
After 7 months	0.171***	0.091***	0.092***	0.097***	0.095***	0.097***
	(0.019)	(0.020)	(0.029)	(0.018)	(0.021)	(0.021)
After 10 months	0.186***	0.099***	0.100***	0.105***	0.104***	0.105***
	(0.018)	(0.023)	(0.031)	(0.017)	(0.020)	(0.020)
After 13 months	0.197***	0.108***	0.109***	0.113***	0.113***	0.111***
	(0.016)	(0.025)	(0.033)	(0.016)	(0.019)	(0.019)
After 16 months	0.230***	0.142***	0.142***	0.146***	0.147***	0.142***
	(0.014)	(0.027)	(0.035)	(0.014)	(0.017)	(0.017)
After 19 months	0.239***	0.155***	0.156***	0.158***	0.160***	0.152***
	(0.012)	(0.028)	(0.036)	(0.013)	(0.016)	(0.016)
Observations	126 953	126 953	126 953	126 953	126 953	126 953
			Average Treatment effect on the Treated after 6 months			
After 8 months	0.103***	0.057***	0.060***	0.062***	0.060**	0.063**
	(0.024)	(0.013)	(0.028)	(0.024)	(0.027)	(0.027)
After 11 months	0.163***	0.093***	0.097***	0.099***	0.097***	0.098***
	(0.026)	(0.021)	(0.036)	(0.026)	(0.029)	(0.029)
After 14 months	0.245***	0.162***	0.167***	0.168***	0.167***	0.166***
	(0.026)	(0.026)	(0.042)	(0.025)	(0.028)	(0.028)
After 17 months	0.288***	0.198***	0.202***	0.204***	0.204***	0.203***
	(0.023)	(0.030)	(0.045)	(0.023)	(0.026)	(0.026)
After 20 months	0.325***	0.233***	0.237***	0.239***	0.239***	0.238***
	(0.020)	(0.032)	(0.048)	(0.021)	(0.024)	(0.024)
After 23 months	0.336***	0.244***	0.248***	0.250***	0.250***	0.247***
	(0.018)	(0.034)	(0.050)	(0.019)	(0.022)	(0.022)
Observations	87 658	87 658	87 658	87 658	87 658	87 658
			Average Treatment effect on the Treated after 10 months			
After 12 months	0.127***	0.080***	0.082***	0.085***	0.081***	0.083**
	(0.033)	(0.016)	(0.039)	(0.031)	(0.036)	(0.035)
After 15 months	0.287***	0.213***	0.216***	0.219***	0.217***	0.219***
	(0.038)	(0.026)	(0.055)	(0.037)	(0.040)	(0.040)
After 18 months	0.381***	0.294***	0.297***	0.302***	0.301***	0.301***
	(0.035)	(0.033)	(0.063)	(0.035)	(0.038)	(0.037)
After 21 months	0.402***	0.308***	0.310***	0.317***	0.317***	0.314***
	(0.032)	(0.038)	(0.066)	(0.033)	(0.035)	(0.035)
After 24 months	0.402***	0.304***	0.306***	0.314***	0.314***	0.308***
	(0.029)	(0.041)	(0.069)	(0.031)	(0.033)	(0.033)
After 27 months	0.392***	0.324***	0.326***	0.336***	0.336***	0.333***
	(0.022)	(0.046)	(0.072)	(0.025)	(0.026)	(0.027)
Observations	67 880	67 880	67 880	67 880	67 880	67 880

Note: Standard-errors in parenthesis. ***, **, * denote estimates significant at the 1, 5 and 10% levels. (a) Duration of the unemployment spell in months. (b) Standard-errors are computed by delta-method. (d) Standard-errors are computed assuming fixed weights. (e) LPM denotes Linear Probability Model. Huber-White standard-errors. (f) Average marginal effect of migration on the treated. Standard-errors are computed by delta-method. The sample includes the first unemployment spell of each worker between January 2001 and June 2002.
Source: Super Fichier Historique Statistique, 1995-2004, ANPE-DARES-INSEE.

Appendix 3.F. Descriptive statistics, panel data samples

Table 3.F.1. Descriptive statistics, convention 1, all unemployment spells

Previously elapsed duration	2 months	6 months	10 months
Personal characteristics			
Age	31.69	32.67	33.47
Couple	0.38	0.41	0.44
Number of children	0.73	0.80	0.86
Desired contract			
Unspecified term (CDI) and full-time	0.82	0.80	0.78
CDI and part-time	0.10	0.12	0.14
Fixed term (CDD and other contracts)	0.08	0.08	0.08
Reason of entry into Unemployment			
End of contract	0.41	0.40	0.38
Firing	0.17	0.19	0.21
Demission	0.06	0.06	0.06
First entry on the labour market	0.07	0.05	0.05
Others	0.29	0.30	0.30
Unemployment benefits			
Minimum income RMI	0.08	0.09	0.09
Minimum income ASS	0.02	0.02	0.03
Insurance (AUD)	0.64	0.71	0.75
Others	0.26	0.18	0.13
Order of the unemployment spell	1.30	1.11	1.05
Migration	0.01	0.01	0.01
Observations	537 506	308 130	192 541

Table 3.F.2. Descriptive statistics, convention 1, repeated unemployment spells

Previously elapsed duration	2 months	6 months	10 months
Personal characteristics			
Age	31.24	32.76	34.16
Couple	0.37	0.40	0.43
Number of children	0.70	0.81	0.90
Desired contract			
Unspecified term (CDI) and full-time	0.83	0.81	0.79
CDI and part-time	0.09	0.11	0.13
Fixed term (CDD and other contracts)	0.08	0.08	0.08
Reason of entry into Unemployment			
End of contract	0.45	0.43	0.39
Firing	0.35	0.16	0.18
Demission	0.05	0.05	0.05
First entry on the labour market	0.05	0.04	0.03
Others	0.10	0.32	0.35
Unemployment benefits			
Minimum income RMI	0.10	0.10	0.12
Minimum income ASS	0.02	0.02	0.03
Insurance (AUD)	0.65	0.72	0.74
Others	0.23	0.16	0.11
Order of the unemployment spell	1.67	1.53	1.50
Migration	0.01	0.01	0.01
Observations	238 626	66 843	17 856

Table 3.F.3. Descriptive statistics, convention 2, all unemployment spells

Previously elapsed duration	2 months	6 months	10 months
Personal characteristics			
Age	32.41	33.48	34.25
Couple	0.41	0.44	0.46
Number of children	0.70	0.78	0.85
Desired contract			
Unspecified term (CDI) and full-time	0.83	0.81	0.79
CDI and part-time	0.09	0.11	0.13
Fixed term (CDD and other contracts)	0.08	0.08	0.08
Reason of entry into Unemployment			
End of contract	0.46	0.42	0.38
Firing	0.19	0.22	0.25
Demission	0.06	0.06	0.06
First entry on the labour market	0.05	0.05	0.04
Others	0.24	0.25	0.27
Unemployment benefits			
Minimum income RMI	0.06	0.06	0.07
Minimum income ASS	0.02	0.02	0.03
Insurance (AUD)	0.72	0.77	0.79
Others	0.20	0.15	0.11
Order of the unemployment spell	1.28	1.10	1.04
Migration	0.01	0.01	0.01
Observations	195 616	106 757	65 285

Table 3.F.4. Descriptive statistics, convention 2, repeated unemployment spells

Previously elapsed duration	2 months	6 months	10 months
Personal characteristics			
Age	32.30	33.99	35.04
Couple	0.40	0.44	0.46
Number of children	0.69	0.80	0.86
Desired contract			
Unspecified term (CDI) and full-time	0.83	0.81	0.80
CDI and part-time	0.08	0.10	0.12
Fixed term (CDD and other contracts)	0.09	0.09	0.08
Reason of entry into Unemployment			
End of contract	0.51	0.47	0.41
Firing	0.14	0.17	0.21
Demission	0.06	0.05	0.05
First entry on the labour market	0.04	0.03	0.02
Others	0.25	0.28	0.31
Unemployment benefits			
Minimum income RMI	0.08	0.08	0.09
Minimum income ASS	0.02	0.03	0.04
Insurance (AUD)	0.75	0.78	0.78
Others	0.15	0.11	0.09
Order of the unemployment spell	1.67	1.54	1.51
Migration	0.01	0.01	0.01
Observations	80 611	20 753	5 332

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