Three Essays in Applied Economics

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Abstract

This thesis is based on works in econometrics and the use of original data. It focuses on three themes: the economics of natural disasters, the relationship between the price of crude oil and the price of gasoline, and the effects of internships on employability, wages and satisfaction at work.

Keywords: Applied Economics, New Sources of Data, Econometric Modeling.

Résumé

Cette thèse s'appuie sur des travaux économétriques et l'emploi de données originales. Elle se concentre autour de trois thématiques: l'économie des catastrophes naturelles, les relations entre les prix du pétrole brut et des carburants, et l'effet des stages sur l'employabilité, les salaires et la satisfaction au travail.

Mots Clés: Économie Appliquée, Nouvelles Sources de Données, Modélisation Économétrique.

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Introduction

This thesis consists of 3 articles in applied economics. It sheds new light on practical issues, with a reflection on the data and econometric methods.

The first article, co-authored with Eric Strobl, studies the determinants of damage associated with natural disasters. With climate change awareness, a new applied literature has emerged measuring the effects of risk and development on the observed increase of damage. However, there is a lack of data concerning losses (monetary and human) associated with natural disasters. This article explains the low robustness of the results of this recent literature, analyzing the accuracy of the data as well as the econometric methods used. Monetary losses are indeed misinformed, which can be explained by a lack of accounting standards measuring this damage, and by the threshold effects for inclusion in databases as well as the difficulty to obtain reliable statistics in developing countries. Estimates for the number of casualties are more accurate but the results from conventional econometric models focus on a medium effect. There are some extreme natural events with several thousand dead people. The use of quantile regression highlights that the determinants of casualties are not the same for disasters that are rare and of great intensity, and those of low intensity but more frequent.

The second article, co-authored with Erwan Gautier, is a study of the fuel market in France, and focuses on price rigidity and asymmetry in price transmission. The data, taken from a website are original and had never been exploited. They correspond to the daily price observation which has taken place for more than 2 years in 10.000 fuel gas stations. This new data allow developing a new methodology. A model of price rigidity linked to economic theory is estimated for each fuel gas station. Individual price paths are then aggregated and simulated to study the adjustment periods and the effect of an increase or a decrease in the price. Prices appear rigid and consistent with new models of price stickiness with variable menu cost and information costs. We do not detect asymmetry in price transmission. Aggregation paths give close results to those obtained with the macroeconometrics models using aggregated data.

The third article, co-authored with Elise Coudin, examines the role of internships on academic choices, employability, and job satisfaction of young graduates in France. While compensation for internships has been the subject of recent legislation, there are no studies measuring their economic impacts. Each academic background includes a number of mandatory internships. It is therefore not possible to observe a counterfactual. In the French engineering schools, students can make the choice to undertake an optional full year of internship between the two years of the Master diploma. This selection is used to measure the impact of internships. A sequential schooling decisions framework highlights several potential effects of internships. Our data come from an original survey about the labor market integration of graduates matched with the students' attainments during their schooling. This enables us to account for student's abilities. They correspond with the observation of 6 classes in a French engineering school. Performing a full year of internship is less valued by employers than a real year of professional experience. This year of internship improves the ability to find a job faster. It is possible to refine the major choices when several internships are made in different areas. Public policies implemented, which ban unpaid internships or more than six months, appear consistent with our results.

Published work and Coauthoring

The first chapter is co-authored with Eric Strobl. The second chapter is co-authered with Erwan Gautier and has been accepted for publication at Journal of Money, Credit and Banking. The third chapter is co-authered with Elise Coudin.

This thesis has been written with the Lyx layout created by Matthieu Perreira Da Silva.

The death toll and the damage of natural disasters : beyond the averages

Chapter 1

The death toll and the damage of natural disasters: beyond the averages

This chapter discusses the accuracy of data and the econometric tools used to link natural disasters mitigation with economic development or risk exposure. It is argued that these methods do not take into account measurement errors and falsely do not distinguish the large events with thousands of death and billions of damage from other relatively minor ones. Using a database widely used in the literature, we show that damage are less informative for developing countries and thus are less robust in predicting or explain losses for these ones. Using censored quantile regressions for the number of death due to natural disasters, we find that results are mainly valid for extreme quantiles, thus suggesting that different behaviors for preventing exposure may exist towards large and rare disasters, and small but more frequent ones.

1.1 Introduction

Climate change has been argued to potentially lead to an increase in natural disasters. This view has in part been reinforced by some recent dramatic events with that resulted in thousands of deaths and billions of dollars in damage, such as the Europe heat wave in 2003, the Hurricane Katrina in 2005 or the United-Kingdom floods and storms in 2014. But the nature is not underlying factor. A similar tropical cyclone in United States, in Haiti or in Bangladesh is not likely to have the same impacts. As a matter of fact a significant part of the observed damage due to natural disasters that have been witnessed since the 1980's can be attributed to the growth in vulnerability due to socio-economic factors. That is, more assets and people in exposed areas mean higher damage too. The economic analysis of losses (human and monetary ones) induced by these events appears to be essential to effectively guide public policy, including adaptation to climate change. Understanding the role of development and risk can improve the evaluation of the impacts of climate change and the costs associated to it. In this regard it appears necessary to identify the countries that will be the most affected by climate change.

This paper investigates so the link between damage from natural disasters and economic development and risk exposure. The existing literature seems to suggest a negative correlation between the number of death and development, but the findings are not consistent for damage. Our aim here is so to reconcile and challenge these different results by analyzing the accuracy of data and the econometric tools used to analyze them. More specifically, natural disasters appear to be an unusual economic aspect that is characterized by a lack of standards to measure impacts and extreme events. Here we explain why actual and observed data for natural disasters differ and implications of this for the econometric analysis. We also examine the role of largest and most extreme events on results. We conclude by analyzing the implications for the on-going literature and future research.

Beyond questions of econometrics methods, we show that the accuracy of data on damage is dependent on countries' level of development. This causes a selection bias and a lack of predictability and robustness of econometric results. More specifically, one can falsely identify a positive link between development and damage. Concerning the econometric method, we use censored quantile regression (Chernozhukov and Hong 2002) to explicitly discern the role of extremest of events. We find that results are mainly valid for these extreme quantiles. Thus there may be different behaviors for preventing exposure towards large and rare disasters, and small but more frequent disasters. Indeed, while a global database on natural disasters maybe provide a good overview on losses of natural disasters, researchers still need to practice caution in applying econometric tools to it. In this regard, we find that data on deaths are better than those on damage for which an accounting framework should be defined.

Data on natural disasters are not widely available. A global database is crucial to have some figures and to compare countries facing natural disasters. However the few available global databases have been largely used to explain determinants of losses on natural disasters, without detailing the accuracy of data under the econometrics methods used (Cavallo *et al.* 2010; Kahn 2005; Kellenberg and Mobarak 2008; Neumayer *et al.* 2013; Raschky 2008; Schumacher and Strobl 2011; Strömberg 2007; Toya and Skidmore 2007). Furthermore, these data are used too to measure the impact of shocks on growth (Noy 2009; Loayza *et al.* 2012; Cavallo *et al.* 2013; Ahlerup 2013) or as an exogenous instrument on several macroeconomic indicators (Felbermayr and Gröschl 2013 for international trade). This article contributes to this literature also, as it gives reasons why losses are not exogenous and measured with errors (this has been partly mentioned in Loayza *et al.* 2012 and Cavallo *et al.* 2013), which could alter previous results of macroeconomic studies.

Section 2 describes the basic concepts (classification and measurement methods) and provides an account of a simple outline of the economics of natural disasters. Section 3 describes the data and its limits. Section 4 details the distinction between actual and observed data and the potential implications for econometrics. Section 5 analyses the determinants of damage and number of death from natural disasters. Section 6 concludes.

1.2 What are natural disasters?

CRED (Center for Research on the Epidemiology of Disasters) defines a disaster as "a situation or event, which overwhelms local capacity, necessitating a request to national or international level for external assistance; An unforeseen and often sudden event that causes great damage, destruction and human suffering." This definition is not restricted to natural events, but it underlines the fact that a disaster only exists when there is a significant impact on human activity. That is why database on disasters (Tschoegl 2006, Beckman 2009) use thresholds on mortality or damage (or declaration of a state of emergency) to define a natural event as a natural disaster or not. This approach can thus be described as socio-economic rather than physical, where the definition would be solely based on the physical definition of events (maximum wind speed, Richter scale...). Thresholds may however vary between databases considered. There is indeed a lack of international standards to define and classify disasters (Wirtz *et al.* 2009, Gall *et al.* 2009). Moreover, the duration and location of an event are sometimes also used to characterize disaster, where there are distinctions between extensive events (repeated, localized and mean intensity) and intensive events (large intensity and areas affected). Differences in predictability can be denoted. For example, while an earthquake seems unpredictable with current knowledge, tracks of hurricanes can be observed directly and have improved recently in short-term predictability. Furthermore, some of events are extreme events (thousands of deaths, billion of damage), for which no probability of occurrence can be computed. Our following analyses are based on the CRED classification, distinguishing climatological events (drought, extreme temperature, wildfire), geophysical (earthquake, volcano), hydrological (flood) and meteorogical (storm).

There is currently no widely accepted system for compiling a comprehensive database containing the losses of natural disasters. Total damage is sometimes deduced from insured losses, and thus neglects uninsured losses[1]. Thus, CRED uses several sources to compile information on monetary and human losses. These data do not separate the types of loss (property by sectors, agricultural products, cleanup and response costs, and adjustment cost such as temporary living aids) and do not identify who bears the loss. This issue is not new but remains unresolved[2]. Although this of course does not mean that these data cannot be used for analysis, it is arguably still necessary to know what types of costs are considered, what is included or not, and what metrics can be used to assess damage and impacts of natural events. What is really measured when estimated damage of a disaster are published is not clear (Rose 2004, Hallegatte and Przyluski 2011). Actually, each study seems to adopt its own definition of cost, but generally distinguish or at least acknowledge the direct and indirect effects. Direct effects refer to consequences of physical destruction, including both property damage and business interruption, whereas indirect effects are due to the chain reaction associated with business interruption (lack of inputs, changes in demand and supply) and can be seen as a "multiplier" of direct damage.

Impacts of a natural disaster come from several factors (cf. Figure 1.1, Cavallo and Noy (2010) and Kousky (2013) for complete surveys on economics of natural disasters). In this regard two main concepts must be taken into account. One is the hazard associated with a natural event, as for example the maximum wind speed at landfall for hurricanes. The second is the vulnerability of the area where the natural event takes place. This vulnerability depends on physical exposure (high ground, distance to the sea...), socioeconomic factors (population and assets in exposed areas for example) and measures of risk reduction. Benson and Clay (2004) note that vulnerability is a dynamic process, which depends on the type of natural hazard, the overall structure of an economy (including natural resource endowments), the geographic size of a country, the country's income level and stage of development, environmental change (destruction of ecosystems which modify ecosystem services), disaster management, and resiliency. National policies to reduce or to prevent damage from natural disasters can hence explain large discrepancies between countries. At the international level, a comprehensive initiative to reduce disaster risks is led by the United Nations with the Hyogo Framework for Action, adopted in 2005, which intended "the substantial reduction of disaster losses, in lives and the social, economic and environmental assets of communi-

[1]Hallegatte *et al.* (2011) assume for industrialized countries that "uninsurable losses represent about 40% of insurable losses".

[2]In 1999, National Research Council (USA) proposed a framework for loss estimation. In 2003, the United Nations Economic Commission for Latin America and the Caribbean (ECLAC) has proposed a standard methodology to assess damage and human losses from natural disasters.

ties and countries." Some international reports (World Bank 2005, UNISDR 2009) have thus identified factors of vulnerability and built indices of risk associated to natural disasters for nearly every country in the world.

1.3 Data

1.3.1 The EM-DAT global database on natural disasters

Three main global databases on natural disasters exist. Two of them depend on reinsurance companies and are private, Nathan (from Munich Re) and Sigma (from Swiss Re). The other one is public, EM-DAT: The OFDA/CRED International Disaster Database – www.emdat.be - Université Catholique de Louvain - Brussels - Belgium. The latter has been maintained by the Centre for Research on the Epidemiology of Disasters (CRED) at the School of Public Health of the Université catholique de Louvain located in Brussels, Belgium, since 1988. EM-DAT includes all disasters from 1900 until present at a country level, which fit at least one of the following criteria: 10 or more people killed, 100 or more people affected, declaration of a state of emergency, call for international assistance. If a disaster occurred in several countries, the disaster event will result in several country-level disasters data points. The database is compiled from various sources, including UN agencies, non-governmental organizations, insurance companies, research institutes and press agencies. Priority is given to data from UN agencies, governments and the International Federation of Red Cross and Red Crescent Societies.

We have accessed the EM-DAT raw data on all natural disasters worldwide from 1900 to 2007. The data includes the following information: disaster type, disaster sub-type, event name, localization, start and end date, people killed, estimated damage, disaster magnitude scale. The type of the event is geophysical (earthquake, volcano, mass movement dry), hydrological (flood, mass movement wet), meteorological (storm) or climatological (extreme temperature, drought, wildfire). Disaster magnitude scales are Richter scale for an earthquake, maximum wind speed (kph) for a storm, and km2 affected for a flood. The estimated damage is entered in US $ (in thousands) in the value of the year of occurrence. We assume here that the indirect losses are small compared to the direct damage in these estimated damage or at least that they are strongly correlated. All damage figures are deflated with the Implicit Price Deflator of USA real chained GDP for the base year 2005 (from US Bureau of Economic Analysis).

One should note that the data are by construction not homogenous. There is no single threshold on damage for the inclusion in the database. The four criteria are indeed "10 or more people killed", "100 or more people affected", "a declaration of a state of emergency" or "a call for international assistance". One event with high damage but with only few killed and no needs for international assistance will not be include. Gall *et al.* (2009) call this a threshold bias that exists in other databases on natural disasters. For example, if we compare data on tropical cyclone on EM-DAT for the USA and data from the NOAA (National Oceanic and Atmospheric Administration) formatted by Pielke *et al.* (2008) and Nordhaus (2006), it can be easily seen that tropical storms and small hurricanes (category 1 on Saffir-Simpson scale) are generally excluded from EM-DAT. For example, before 1980, there are 133 observations from NOAA and only 37 from EM-DAT. Quality in the EM-DAT is also probably questionable prior to the creation of the database (1988). More precisely, CRED has compared the three main global databases, EM-DAT, Nathan and Sigma (Guha-Sapir and Below 2004) and found significant discrepancies between them. The report underlines that observations before the 1980's have greater discrepancies than those from the 1990's. Most of the existing econometric analyses are indeed based on EM-DAT begin at 1980. We have similarly chosen to use the period 1980-2007 for our main econometric works, after checking that the determinants of non-response weren't different for the periods 1980-2007 and 1988-2007. Climatological events (extreme temperature, drought, wildfire) are excluded as their duration can be longer (several months) than other disasters. Furthermore, the Global Assessment Report: Risk and Poverty in a Changing Climate (UNISDR 2009) has underlined that a drought is a much more complex type of hazard often linked with the local political situation. We finally restricted our analysis to earthquakes, floods and tropical cyclones, as we can observe a measure of disaster magnitude for those events and it is consistent with our risk exposure index defined at country level. Some countries have been excluded since there are no economic data easily available for islands or territories that are not independent (Guadeloupe, Reunion, New Caledonia...), former communist countries (before 1990), small pacific islands (Kiribati, Nauru...), or unstable counties (North Korea, Myanmar, Somalia...). We also did not include small countries with no events (Malta...) and those for which our risk exposure index is not available. The final sample used for our econometric analysis contains 3898 natural events for 121 countries.

1.3.2 Risk exposure, socio-economic and geographical data

In order to characterize the level of development of a country, several indicators have been collected, such as real chained GDP from Penn World Table and Human development indicator (HDI) from the United Nations Development Program (UNDP). For vulnerability to natural disasters, we use the index computed by Schumacher and Strobl (2011), which measures the expected hazard of a natural disaster from the natural disaster global hot-spots data (World Bank 2005). As robustness checks we also used an index computed by the UN, i.e., the Mortality Risk Index (UNISDR, 2009), which ranges from 1 (negligible) to 10 (extreme). It should be noted that since it is based on modeling hazards in both frequency and intensity, human exposure and identification of their vulnerability, it is determined endogenously. The linear correlation between the two indices is low, being around 0.21. Moreover, the global hot-spots index is more correlated with mean deaths and damage (0.24 and 0.31) than the Mortality Risk Index (0.21 and -0.05).

1.3.3 An unequal distribution of risk

One important fact is that only few disasters cause most of the damage and killed. Two main indicators enable us to check this. The first one is the Gini index, where the closer this index is to one the more the distribution is concentrated. For data we find that damage is less concentrated (0.890 for positive values) than human impacts (0.922 for positive values). In all case, the Gini index is notably very high. The other severity indicator is the categorical variable from Munich Re, where extreme natural catastrophes are those with more than 2000 killed or 2 billion real $ of damage. These represent only 3.4% of disasters but 78% of damage and 73% of killed. Compared to real chained GDP, only 8.5% of disasters (with known damage) have damage greater than 0.5 % of GDP. Human impacts mainly appeared in developing countries. In developed regions, damage can be costly (11% in Europe for 9% of natural disasters) but human impacts are lower (1% in Europe). Moreover, 8 of the 10 more expensive disasters took place in developed countries (United States, Japan. . .) whereas the more deadly ones are all in developing countries. This unequal distribution is confirmed with the distribution of Risk Index. Southern Asia and some countries of Southern America appear to be the riskier countries in the world. Northern America and some countries of Sub-Sahara Africa appear a little less risky. Europe has a relatively low risk exposure.

1.4 Distinguishing actual and observed data on natural disasters

1.4.1 What does a zero value mean?

EM-DAT Frequently Asked Questions mention “Empty fields are usually the way missing values or non reported information are entered into EM-DAT. A “0” in EM-DAT does not represent a value and can mean that no information is available.”. It is strangely never discussed in the articles using this database (or other ones on natural disasters). However, we find that non-response (or real zeros) is large in our sample, accounting for 57% for damage and 26% for the number of deaths.

In terms of the disaster magnitude scale, non-response appears to depend on the type of the event, where it is low for earthquakes (around 6%) but higher (around 60%) for floods and cyclones.

We discuss here what is included and observed in a natural disaster database and its impacts for econometrics works. Two endogenous thresholds (i.e. linked with the level of development or the index of risk of the country) can explain that damage from a natural disaster remain unknown. The first one is linked with the threshold used for the inclusion in the database, there is no threshold on damage with EM-DAT. The second one is directly linked with the level of development as statistics have often less accuracy in developing countries.

Figure 1.2 illustrates this phenomenon, detailing 4 cases (high/low risk and developing/developed countries). In this example, 100 events occurred in countries with a high risk of disasters and 50 in those with a low risk, which can be controlled in econometrics works using an index of risk.

However, a potential natural disaster can occur without being included in the database (black blocks in figure 1.2). Several explanations are possible. One is that an event never translated into a natural disaster, as for instance when it takes place in an unpopulated area. The other is that a natural disaster occurred but did not meet the cut-off criteria to be classified as one. For example, in developed countries some disasters with high damage but no deaths can be unobserved. A declaration of a state of emergency or a call for international assistance is more likely in developing countries than in developed ones. If death are lower in developed countries, events won't be included that would be observed in developing countries. For a country with a low risk of natural disaster, preventive measures may be less strong and explain more deaths for a similar event. This rate for non-inclusion would be lower in developing countries and for countries with a low risk of natural disasters. For deaths, we only unobserve small events, which is not a major issue. For damage, we potentially unobserve events with high damage, which is related endogenously with economic development and risk exposure. Another possibility is too that a natural disaster occurred but was not recorded because of measurement error (potentially systematic).

But even if the event is included, damage could be unknown (white blocks in figure 1.2), if there is a lack of statistical institutes for instance. In our database, this non-response rate is linked with the Human Development Indicator rank (cf. figure 1.3). A higher rank of 10 places increases non-response rate of damage nearly 2.4% and reduces the zero rate of people killed by 1%. Nevertheless, an event with no deaths cannot be considered as an error whereas a natural disaster with no damage is impossible. If there is "10 or more people killed", "100 or more people affected", "a declaration of a state of emergency" or "a call for international assistance", it is difficult to argue that there are no damage but it is possible there are a lot of people affected but no death in particular in developed countries. In figure 1.2, actual damage in developing countries is higher than in developed ones but observed damage is lower. Thus one could falsely conclude a positive relationship between development and damage and there would be an upward bias in the case that unknown damage is of comparable size.

For known damage, there are major events, which may be assumed to be observed for all countries. In this case, if damage is over-dispersed, potential relationships between development and damage may result from these events only (and the two previous selection bias would be low). Also political limitations can affect figures, especially for developing countries. For example, in order to increase humanitarian aid, political authorities could overestimate damage when they are declared (Stromberg 2007). It can engender a downward bias.

Zero values can come from several phenomena: censorship (events which never translated into a disaster due to prevention), truncation (no events with less than 10 people killed), selection and measurement errors (non-response and accuracy of data linked with development). All in all, there may be significant biases, mainly for damage. It is difficult to assess the direction of this bias, as high damage can be unobserved due to a threshold inclusion bias in high-developed countries but other damage can be over-estimated in developing countries.

1.4.2 A characterization of a selection bias

We now examine the possibility of a selection bias using a probit model of non-response to estimated damage and people killed. We model the probability to observe estimated damage (or people killed) higher than 0 with a Probit model as $\mathbb{P}\left(\mathrm{Damage}_{\mathrm{it}} > 0/\mathrm{X}\right) = F\left(\alpha + \beta \cdot X_{it}\right)$ estimated with maximum likelihood, F the c.d.f. of the standard normal distribution and X_{it} including the ranking compared to the Human Development Index (HDI, to account for the level of development), log of people killed (for the size of the events), log of population (for the size of the country), a trend and the type of events. For damage, we model the probability for each type of disaster including the disaster magnitude (Richter scale for earthquake, km2 area impacted for flood, maximum wind speed for cyclones) in predictors. Results (probit average marginal effects) are presented in table 1.1. The four main conclusions are 1) the probability of non-response is reduced in very high developed countries (compared to low developed countries), 2) the probability of non-response is reduced for large scale disasters, coefficients are negative and significant for magnitude disaster and level of killed (or damage), 3) the effect of years is ambiguous and small, 4) data quality for people killed seems to be higher than for damage. As a conclusion, a selection bias will exist if we do not take into account non-response for econometric analyses on damage. Nevertheless, it will be probably small because non-response is more important for small-scale disasters. Furthermore, the maps (figure 1.4) show the unequal distribution of non-response for damage with higher rate in Africa and South-America. For the number of deaths, the geographical distribution appears more random. In view of this we will test different samples, one for all countries, the other one for countries with a non-response rate less than 60 %. Observations will be also considered as censored.

1.4.3 A low predictive power for damage

We have also tried to correct non-response by linking damage and people killed as in Cavallo *et al.* (2010). They make an assessment of the damage caused by the 2010 earthquake in Haiti with the information available at the date of the earthquake (nowcasting). They model the damage as $\text{Log of Damage}_{it} = \alpha + \beta \cdot X_{it} + \varepsilon_{it}$ estimated with OLS and X_{it} including the log of people killed (information is more readily available), real GDP per capita, population, a trend and the number of previous events. Results are presented in table 1.2. The model for damage and main events is similar to Cavallo *et al.*. There is a positive effect of number of death, development and population and a negative one for past events. As the number of deaths and damage are simultaneous events, the number of death is endogenous. If we apply their methodology to predict damage of the 2010 Haiti Earthquake, we find a higher estimation of 12-14 2005 billions dollars. Nevertheless, one should note a mistake in Cavallo *et al.* (2010) as the prediction is done with $e^{\text{Ln}(\widehat{Damages})}$, instead of the log-model corrected prediction $e^{\text{Ln}(\widehat{Damages})} \cdot e^{\widehat{\sigma^2}/2}$ (as $\mathbb{E}\left(\text{Log}\left(y\right)/x\right) = \text{Log}\left(y\right)$ but $\mathbb{E}\left(y/x\right) = y \cdot e^{\sigma^2/2}$). The prediction in Cavallo *et al.* (2010) is so underestimated. We find an estimation of 35-39 2005 billions dollars with the corrected prediction. The adjusted R^2 is similar with the model for all events, and even better for the ratio damage/population. The trend becomes significant in these models. We can note that there are some leverage points (i.e., outlier observations for predictors) in those models which mainly correspond to small islands and some bad individual predictions (figure 1.5). The damage is often overestimated (96% of cases) but major events are often underestimated. The use of log transformation avoids having non-robust model (results with a median or a weighted regression are similar) but does not provide a good quality of prediction out-of-sample for extreme events. We apply the model for all events at the observations with no known damage and find again that those unknown damage is probably smaller than the known damage. Lastly we estimate the model with a two-step Heckman methodology to control for selection bias (table 1.2, column 5), where it is found that the estimator of the GDP/Population is 32% lower. Thus it seems difficult to obtain a precise estimate for the link between damage and development.

1.5 Take into account the role of major events

1.5.1 Linking damage with development and risk exposure

Concerning the role of development[3], Kahn (2005) and Toya and Skidmore (2007) conclude that direct losses of natural disasters are larger for emerging countries than for industrialized ones, which can be explained by the increasing demand for safety with higher incomes, the increasing resources spent on prevention efforts and disasters reduction plans, and better institutions. Other determinants identified are factors such as economic inequalities, degree of openness, or the average level of education. More precisely, Schumacher and Strobl (2011) construct a theoretical risk model to explain this fact and to study the links between risk, development, and direct costs of natural disasters. The adaptation expenditure to prevent damage of natural disasters are thus explained by development, cost of adaptation, probability, and expected intensity of a disaster (this probability is assumed to be known). Their econometric analysis shows a non-linear relationship between damage and wealth, which depends on the risk of natural disasters faced by countries. An inverse u-shaped link between direct damage and GDP is found for low and medium risky countries whereas there is a u-shaped link for high risky countries, which is consistent with their theoretical model. Using quantile regression, Neumayer *et al.* (2013) find a positive link between development and damage but no clear relationship for small disasters. They thus consider small damage as random and unpreventable. In order to predict damage for the 2010 Haiti earthquake, Cavallo *et al.* (2010) use OLS linking damage, killed people and development. They find a positive link between development and damage. Taken together, these articles conclude that there is a negative correlation between number of deaths and development, but the findings are not similar for damage[4].

[3] A lot of descriptive approaches have been published from the late 1990s, which aimed at normalizing damage with different variables (general price index, population, wealth, housing...). Their conclusions differ as the presence or absence of an increasing trend after normalization of data over the last 30 years, which may be associated with climate change (Bouwer 2011, Neumayer and Barthel 2011).

[4] Raschky (2008) analyses too the role of institutions, better institutions implying less damage. Stromberg (2007) explores the determinants of aids. Ambarci *et al.* (2005) found negative correlation between development and people killed for major earthquakes and controlling by exogenous measure of disaster magnitude.

1.5.2 Censored quantile regressions

The aim of this part is to extend the econometric analysis of Kahn (2005), Toya and Skidmore (2007) and Schumacher and Strobl (2011) linking risk exposure, development, and losses by using the censored quantile regression methodology as adapted from Chernozhukov and Hong (2002). More specifically, the use of the observed events (raw data) does not take into account the hazard exposure to natural disasters faced by countries, even when controlling for the hazard associated with a natural event (e.g., for example the maximum wind speed at landfall for hurricane). If, for instance, one compares a rich country with a high risk of natural disasters to a poor country with a low risk of natural disasters then one could falsely conclude there being a positive link between development and damage. Exclude the years with no death or damage observed would thus create a classical selection bias. The use of panel data with zero values for years without disasters can correct this bias and can explain some divergent results.

We estimated the panel model proposed by Schumacher and Strobl (2011) from their theoretical risk model with our new data:

$$y^*_{it} = \log\left(1 + \frac{Damage_{i,t}}{Pop_{i,t-1}}\right) = \alpha + \beta_1 \log\left(\frac{GDPi,t-1}{Pop_{i,t-1}}\right) + \beta_2 \log^2\left(\frac{GDPi,t-1}{Pop_{i,t-1}}\right) + \sum_j \delta_j X^j_{i,t-1} + \varepsilon_{i,t}$$

Where i is a country and t a year (from 1980 to 2007)[5], damage is the sum of estimated damage from EM-DAT (deflated with index price deflator from BEA) for year t and type of disasters considered, log value of GDP per capita come from chained accounts of the World Penn Tables, per capita variables are considered to take into account growth of vulnerability. For variables of population and GDP, the previous year is considered to avoid potential problems of endogeneity, X are a set of control variable, which include log value of population, a trend, a measure of risk. In order to be consistent with our index of risk, only earthquakes, floods and tropical cyclones are taken into account.

In the theoretical model of Shumacher and Strobl (2011), observed damage (the costs of natural disasters) can be seen as the solution of a maximization problem depending on economic development and prevention expenditure. The censoring point is partly a corner solution (but can be seen too as a selection process due to the hazard exposure). No damage is observed due to economic development and prevention expenditure. To deal with censorship of the data (years with no disasters or no damage associated to a disaster), Schumacher and Strobl (2011)

[5]We have adopted three measures for the year of occurrence of a disaster, such as 1) the start year, 2) the end year, 3) the end year if the disaster ends before October, else the end year +1. It doesn't change the results.

use a Tobit I model $y_{it} = \max(0, y^*_{it})$. Kahn (2005) uses a zero inflated negative binomial model (for deaths), which assumes that the process contains two parts, one that explains if a natural disaster occurred with a logit model and the other to explain the deaths with a negative binomial model. The ZINB model is more appropriate when data are over-dispersed and there is an excess of real zero. In this latter model, we do not observe deaths either because no natural disaster has occurred or because there were no deaths associated with a disaster. Furthermore, robust standard errors are implemented by within country clustering of the error term and a random effect assumption[6].

The selection bias as shown in the description of data is not taken into account with these approaches. It is difficult to solve selection bias with a high rate of non-response (more than 60%) and no valid instrumental variables with a generalized Tobit. We so use predictions as defined in the previous section, to check if results change on a large scale.

Furthermore, Tobit and ZINB models measure a mean effect whereas the effects may be heterogeneous between small and large disasters even without outliers. They are based on a parametric choice. If the errors are not correctly specified (for damage and selection equation), estimators are biased and not consistent.

We estimate censored quantile regressions in order to study heterogeneous effects with a methodology robust to a misspecification of the error term. Instead of defining the conditional mean of the dependant variable $\mathbb{E}(Y/X) = X'\beta$, quantile linear regressions (QR) define the conditional quantiles such as

$$Q_{Y/X}(\tau) = inf_{v \epsilon \mathbb{R}} \left\{ v : F_{Y/X}(v) > \tau \right\} = X'\beta(\tau)$$

where $Q_{Y/X}(\tau)$ is the τ conditional quantile of Y and the conditional quantile of the error term is 0. With n observations (Y_i, X_i), the QR estimator of is found by solving

$$\min \sum_i \rho_\tau \left(Y_i - X_i'\beta(\tau) \right)$$

with $\rho_\tau = \tau - \mathbf{1}\left\{ Y_i < X_i'\beta(\tau) \right\}$.

Using uncensored quantile regression with endogenous censored data gives biased estimators (appendix for an illustration with simulations). This bias exists even for high quantiles, if for some value of the predictor the dependent variable is almost always censored. The bias will be low if the rate of censoring is overall low.

[6] Our hazard exposure index is not time-varying. We so do not used a fixed effect model. The number of events affecting multiple countries is few. Spatial dependencies between countries are so assumed to be weak and are not taken into account.

Powell (1986) has defined a censured quantile regression (CQR) model for censoring points known C_i as

$$Q_{Y/X,C_i}(\tau) = \max\left(C_i, X'\beta(\tau)\right)$$

whose consistent and asymptotically normal estimator is found by solving

$$\min \sum_i \rho_\tau^{'}\left(Y_i - \max\left(C_i, X'\beta(\tau)\right)\right)$$

with $\rho_\tau^{'} = \tau - \mathbf{1}\left\{Y_i < \max\left(C_i, X'\beta(\tau)\right)\right\}$.

Unfortunately, only a few applications exist due to computational difficulty. Some computational algorithms have been designed such as Fitzenberger (1997, 2007), but these are efficient only for a low degree of censorship. This is not the case of our EM-DAT, which has a very high degree of censorship of around 73%. We thus used the three-step censored quantile regression developed by Chernozhukov and Hong (2002) with a separation restriction on the censoring probability. This estimator is easy to compute. Furthermore, it is asymptotically as efficient as Powell's estimator and has good finite-sample properties. The three steps are the followings with a censoring point $C_i = 0$:

Step 1: Estimate a parametric classification model $\mathbb{P}\left(Y > 0/\dot{X}\right)$, of being not censoring with logit, probit, or extreme value model, and desired transforms of explanatory variables $\dot{X}$ (polynomial, interactions…). A sub-sample $J_0 = \left\{i : \mathbb{P}\left(Y_i > 0/\dot{X}_i\right) > 1 - \tau + c\right\}$ is thus selected with c between 0 and τ.

Step 2: Obtain the initial (inefficient) estimator $\hat{\beta}_0(\tau)$ with standard QR on sample J_0 and select a new sample $J_1 = \left\{i : X_i'\hat{\beta}_0(\tau) > \delta_n\right\}$ where δ_n is a small positive number as $\delta_n \searrow 0$ and $\sqrt{n}\delta_n \to \infty$.

Step 3: Obtain the CQR estimator $\hat{\beta}_1(\tau)$ with standard QR on sample J_1, after checking that $J_0 \subset J_1$.

We have computed it for quantiles 0.75 to 0.99 with the Stata Package cqiv (Chernozhukov *et al.* 2012)[7]. We do not compute lower quantiles due to the eventual degeneracy of the selected design matrix. Uncensored quantile regressions have been estimated. Standard errors are obtained with block bootstrap (Bilias *et al.* 2000) at the country level.

Censored quantile regression can be related to a Tobit model. More precisely, censored quantile estimators converge to Tobit estimators of the latent dependent variable y^* if the "real" model is a Tobit model. Comparing average marginal

[7]Gustavsen *et al.* (2008), Fack et Landais (2009) for examples of application.

effects is difficult as these ones depend on parametric choices for Tobit (cf. appendix). There is however no direct link with the negative binomial estimators of the ZINB model.

1.5.3 Results and robustness to measurement errors

Using classical (non censored) and three step censored quantile estimators, we found interesting and consistent results. In figure 1.6, the continuous black line represents the three-step estimates of $\hat{\beta}_1(\tau)$, i.e., the coefficients of the QR for the (conditional) quantile of the direct damage, and the grey one the non censored QR estimate. Each QR explains this conditional quantile by log value of GDP per capita (and its square), the index of risk exposure, log value of population, and year dummies.

For the number of deaths, the estimated parameters of GDP per capita are positive and its squares are negative, which is consistent with previous study of Schumacher and Strobl (2011). The number of deaths rises with GDP for countries with GDP lower than a threshold and decrease after this threshold. This result is only significant for higher quantiles, and rises with quantiles. Thus, the relationship found with Tobit or ZINB model is mainly valid for higher quantiles, i.e., for larger deaths or major events. For lower quantiles (<0.95) the relationship between development, risk exposure and deaths does not appear so clear. Risk exposure has a higher effect for higher deaths too. For damage, we find inconsistent results, which confirm the lack of robustness of the analyses conducted using damage. We have not included the analysis on imputed data, as the imputation does not reproduce the distribution of the observed damage but its mean.

Results for uncensored and censored quantile estimators are close, which is difficult to explain. It would be the case if the censorship threshold was weakly endogenous or if the rate of censorship was low (cf. appendix for more details). But that is not the case here, as the rate of censorship is high and linked with economic development. One may argue that predictors have low power to explain censorship and that specification of the model may be improved. However, we note that the parameters of GDP per capita (and its square) are underestimated for the number of deaths with the uncensored quantile estimators.

Tables 1.3 and 1.4 give the average marginal effects of the Tobit and the ZINB models for losses (monetary and human ones) due to earthquakes, floods and tropical cyclones[8]. Using a Tobit model, we find consistent results with previous

[8]In the theoretical model of Shumacher and Strobl (2011), observed damage (the costs of natural disasters) can be seen as the solution of a maximization problem depending on economic development and prevention expenditure. The censoring point is partly a corner solution

study of Schumacher and Strobl (2011). At a global scale, GDP per capita has a positive effect while its squared value has a negative effect. The exact coefficients suggest that damage rises with GDP for countries with GDP lower than 17,400 2005 dollars per inhabitants and decrease after this threshold. The amount is smaller for the number of deaths, i.e., around 4,000 2005 dollars. Population and the risk index have a positive impact. If we use imputed damage (table 1.3, columns 4 and 5), the threshold drops to around 9,000 2005 dollars per inhabitants. The coefficients for uncorrected damage thus would result in an upward bias. If we restrict the sample to countries with a rate of non-response lower than 60% (tables 1.3 and 1.4, column 2), the coefficient on GDP per capita become negative but insignificant and its squared value positive but insignificant. The exact coefficients would imply a null or negative correlation between losses and development. Using the ZINB model (tables 1.3 and 1.4, column 3), one discovers inconsistent results for damage with a negative coefficient for population and a positive (but not significant) correlation with development. For deaths, results remain consistent but coefficients for GDP per capita are 13 times higher. This may be due to the fact that Tobit model is not suitable to model over-dispersed variables. Nevertheless, the implied elasticities would be unrealistic. All in all, results for damage thus seem to be more non robust. Heterogeneous effects are highlighted for deaths.

For further robustness we experimented with including interactions between the measure of risk and GDP. Additionally we used a time trend rather than year dummies, as well as our alternative measure of risk, and different samples. We found that results to be robust for the case for deaths but not for damage, which is consistent with the main finding that results for damage are not robust.

1.5.4 Implications of the results

We find a non-linear relationship between disaster mortality, development and risk exposure only for higher conditional losses. It may suggest that different behav-

(but can be seen too as a selection process due to the hazard exposure). No damage is observed due to economic development and prevention expenditure. We are so interested in the marginal effects on the expected value for y (censored and uncensored), which is for the Tobit model $\frac{\partial \mathbb{E}(y_i)}{\partial x_k} = \phi\left(\frac{X_i\beta}{\sigma}\right)\beta_k$ where ϕ is the standard normal cdf. The marginal effects are not constant and depend of the predictors, we so use average marginal effects. Average marginal effects for Tobit models correspond to a scale factor of the estimators. There is not such a scale factor for ZINB models, which assume that the process contains two parts, one that explains if a natural disaster occurred with a logit model and the other to explain the deaths with a negative binomial model. The signs of the average marginal effects can be different of the estimators for the count process (that is the case for damage). Finally, the dependent variable is not the same in both models, $\log(Y)$ with the Tobit model, Y with the ZINB model. In order to have homogenous quantities, we compute $\partial\mathrm{log}\left(\mathrm{y}\right)/\partial\mathrm{x}$ for ZINB models (instead of the usual marginal effect $\partial y/\partial x$).

iors may exist between large and rare disasters and small but more frequent ones. Neumayer et al. (2013) find a close result for damage using quantile regression too. Due to market failures caused by collective action, information asymmetry and myopic behavior, countries with a higher risk exposure will experience lower damage for a same hazard magnitude but for large natural disasters only. They argue that "small-scale damage is often unavoidable and essentially random. Where disaster preparedness should have its strongest effect is in the mitigation and prevention of large-scale damage.". They use MunichRe database for which non-response is low, but still have a lack of measurements standards. Their results entail two main drawbacks. They exclude all country-years with zero damage observed, which may only be valid to study extreme events and not the all distribution. In the theoretical model of Shumacher and Strobl (2011), observed damage (the costs of natural disasters) can be seen as the solution of a maximization problem depending on economic development and prevention expenditure. Using this framework, excluding zero values engenders a classical selection bias. Furthermore, Neumayer et al. (2013) argue that for high quantiles, all conditional damage is positive which may remove this selection bias. Appendix 1 gives details why it is not the case. Using censored quantile regression is so an improvement, which takes into account partly this selection bias.

We differ too concerning our interpretation of higher effects for higher conditional quantiles. One may argue that this result comes from a higher variability of higher losses. But the finding of strong impacts (of development and risk exposure) at high losses indicates the presence of a threshold, with three possible explanations corresponding to other assumptions that the model of Schumacher and Strobl (2011). Different behaviors may indeed exist between large and rare disasters and small but more frequent ones. All countries could prevent the small-scale events but not the larger ones. First, there is the irreversible nature of some damage. Damage will not be a linear function of the natural hazard. For instance, building standards allow that damage associated to an earthquake is null up to a threshold. If the building collapses, damage will be very high. This threshold, which can be seen as the maximum damage prevented, would be endogenously determined with characteristics of countries. Secondly, large and rare disasters can be seen as an emerging risk whose probability is not really known. There would be two types of disasters, those for which the probability of occurrence is known (as in the model of Schumacher and Strobl (2011)) and the unpredictable events. Uncertainty to predict future climate and trends in natural disasters can cause an under-investment to adapt and mitigate their potential effects (Hallegatte 2009). The long-term prospective could be better in riskier or developed countries. Furthermore, due to the absence of short-term political benefits, prevention measures are not adapted to the worst case scenario (Neumayer et al. 2013). Thirdly, risk perception can be different towards rare and more frequent disasters. The perceived probability can be different from the objective probability. The "memory of risk" is not good for large disasters. People can simply forget the actual risk. It appears so that large-scale damage is much more random than small-scale disasters. Occurrence of small-scale disasters could be easier to predict and so to prevent and mitigate. They may be less related to economic development.

Natural disasters are used to measure the impact of shocks on growth (Noy 2009; Loayza et al. 2012; Cavallo et al. 2013; Ahlerup 2013) or as an exogenous instrument on several macroeconomic indicators (Felbermayr and Gröschl 2013 for international trade). Our article shows these analyses have two main limits. First, losses are measured with errors and there is a selection bias associated to the inclusion in the database. Loayza et al. (2012) and Cavallo et al. (2013) use the people affected or killed to take into account the low accuracy of damage. However, losses (human and monetary ones) are not exogenous and are linked with development and risk exposure. Defining major events using observed losses does not allow inferring causal effects. Ahlerup (2013) uses the number of events registered as a measure of shock, noting that there is less endogeneity then. Figure 1.2 shows that is not obviously the case. The use of measures independent of losses (geophysical or meteorological data) appears to be necessary to infer causal effects of natural disasters on growth and may explain the divergent results of this literature (Cavallo et al. 2013). Felbermayr and Gröschl (2014) create such a database (named GeoMet) containing measures of the physical strength for each disaster worldwide. They find too a selection bias associated to the inclusion of observed events in a database such as EM-DAT. Strobl (2012) defines a meteorological measure for the hurricanes in the United States. Two meta-analyses on the link between natural disasters and growth (Lazzaroni and van Bergeijk (2014) and Klomp and Valckx (2014)) have shown that the divergent results come especially from the estimation technique, the use of panel or not, the consideration of outliers, the period, or the sample of countries. However, the problem of correcting for endogeneity with geophysical or meteorological data is not taken into account as Klomp and Valckx (2014) note, “there are too few observations to obtain reliable estimation results examining these measures separately”. Further studies using such measures independent from losses can solve the disagreement between these studies.

1.6 Conclusion

Many existing empirical studies of the economics of natural disasters suffer from two main drawbacks. One is the accuracy of data. We show that the rate of non-response for damage is very high and correlated with the level of development. This fact is rarely discussed. It causes a selection bias that is difficult to correct, even if its non-response seems more prevalent for small events. It also seems difficult to find robust econometric results for damage in the absence of an accounting framework to measure them. The use of this damage as predictors in macroeconomic studies appears questionable too because of measurement errors. The use of measures independent of losses (Strobl 2012, Felbermayr and Gröschl 2014) could be more adapted to infer causal effects of natural disasters on growth and other macroeconomic indicators.

The other weakness in many existing studies is that commonly used econometric models do not seem to be adapted for studying determinants of human losses of natural disasters. Rather their results apply mainly for extreme quantiles. Thus quantile regressions seem to better fit such data on natural disasters. At last, if the theoretical model is not completely consistent with our results, the finding of strong impacts (of development and risk exposure) at high losses indicates the presence of a threshold. This threshold can be explained by the irreversible nature of some damage (infrastructures...), or could be endogenously determined with characteristics of countries. Moreover, different behaviors can exist in terms of large and rare disasters and, small but more frequent disasters. Thus theoretical models need to be developed to take account of this.

1.7 Tables and graphics

	(1) Zero Damages	(2) Zero Damages	(3) Zero Damages	(4) Zero Damages	(5) Zero Killed
HDI rank	0.003***	0.003***	0.003***	0.003***	-0.002***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Trend	-0.004*	-0.006*	0.011***	-0.004	0.000
	(0.002)	(0.003)	(0.003)	(0.004)	(0.001)
Log(Population)	0.006	-0.024**	0.003	0.004	-0.040***
	(0.011)	(0.009)	(0.019)	(0.021)	(0.007)
Log(1+Killed)	-0.090***	-0.094***	-0.084***	-0.098***	
	(0.004)	(0.007)	(0.013)	(0.024)	
Log(1+Damages)					-0.021***
					(0.001)
Flood	Ref				Ref
Earthquake	-0.021				0.079**
	(0.034)				(0.036)
Cyclone	-0.202***				-0.075***
	(0.038)				(0.027)
Disaster magnitude		-0.069***	-0.337**	-0.099***	
		(0.024)	(0.139)	(0.033)	
Observations	3898	574	935	373	3898
Pseudo R2	0.151	0.255	0.126	0.205	0.132
Ref. probability	0.570	0.633	0.495	0.325	0.255

Note: Column 1 is the average marginal effects of the probit model for zero damage, for all countries and for earthquakes, floods and cyclones from 1980 to 2007. Column 5 is for zero killed. Columns 2, 3 and 4 are the average marginal effects of the probit model for zero damage taking into account disaster magnitude (Richter scale for earthquake, km2 area impacted for floods (x1000000), maximum wind speed for cyclones (x100)). Standard errors (clustered by country levels) are in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

Table 1.1 : Zero damage and zero killed determinants - probit average marginal effects

	(1)	(2)	(3)	(4)	(5)
	Log(Damages)	Log(Damages)	Log(Damages /Population)	Log(Damages /Population)	Log(Damages /Population)
	Main Events	All Events	Main Events	All Events	All Events
Log(Killed)	0.581***	0.727***			
	(0.078)	(0.042)			
Log(Killed/Population)			0.928***	1.288***	0.967***
			(0.088)	(0.078)	(0.137)
Log(Gdp/Population)	0.924***	1.138***	0.996***	0.973***	0.665***
	(0.127)	(0.154)	(0.103)	(0.161)	(0.207)
Log(Population)	0.347***	0.340***			
	(0.050)	(0.070)			
Log(Population/Area)			0.248*	0.204	0.185
			(0.131)	(0.136)	(0.131)
Trend	-0.017	-0.027***	-0.034**	-0.037***	-0.038***
	(0.010)	(0.008)	(0.015)	(0.011)	(0.012)
Flood	Ref	Ref	Ref	Ref	Ref
Earthquake	0.127	0.097	0.059	-0.407*	-0.309
	(0.250)	(0.233)	(0.241)	(0.243)	(0.240)
Cyclone	0.394**	0.575**	0.276*	0.476*	-0.030
	(0.155)	(0.229)	(0.154)	(0.266)	(0.248)
Past Event 1960-1979	-0.011***	-0.022***	-0.010***	-0.022***	-0.023***
	(0.004)	(0.007)	(0.003)	(0.005)	(0.005)
Constant	-1.446	-4.710***	-1.151	-2.648*	2.185
	(1.367)	(1.243)	(1.051)	(1.484)	(1.904)
Observations	761	1675	761	1675	3898
Adjusted R^2	0.391	0.397	0.588	0.430	.

Note: Columns 1, 2, 3 and 4 are estimated with OLS, clustered by countries. Earthquakes, floods and cyclones with positive damage from 1980 to 2007 for all countries are taken into account (columns 2 and 4). Main events are natural disasters with damage higher than 10 millions dollars and 10 people killed (columns 1 and 3). Column 5 is Heckman two-step estimators to control for non-response. Exclusion variables are those uses in table 1.1. Standard errors (clustered by country levels) are in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

Table 1.2 : Main determinants of damage as in Cavallo *et al.* (2010)

	(1) Log(Damages /Population) All Countries Tobit	(2) Log(Damages /Population) NR<60% Tobit	(3) Log(Damages /Population) All Countries ZINB	(4) Log(Damages /Population) All Countries Tobit	(5) Log(Damages /Population) NR<60% Tobit
Log(Gdp/Population)	4.178***	-0.227	1.532	4.139***	-0.991
	(1.592)	(2.827)	(1.918)	(1.574)	(3.644)
Log^2(Gdp/Population)	-0.214**	0.014	-0.028	-0.227**	0.051
	(0.093)	(0.160)	(0.111)	(0.094)	(0.205)
Log(Population)	0.648***	0.666***	-0.234***	0.810***	0.759***
	(0.050)	(0.066)	(0.081)	(0.065)	(0.097)
Risk Index	0.016***	0.023***	0.014***	0.018***	0.022***
	(0.005)	(0.006)	(0.005)	(0.004)	(0.008)
Trend	0.029***	0.051***	-0.022*	0.055***	0.043***
	(0.008)	(0.014)	(0.013)	(0.010)	(0.015)
Observations	3161	1389	3161	3161	1389
Censored	2310	896	2310	1618	691
Pseudo R^2	0.061	0.047	.	0.047	0.040

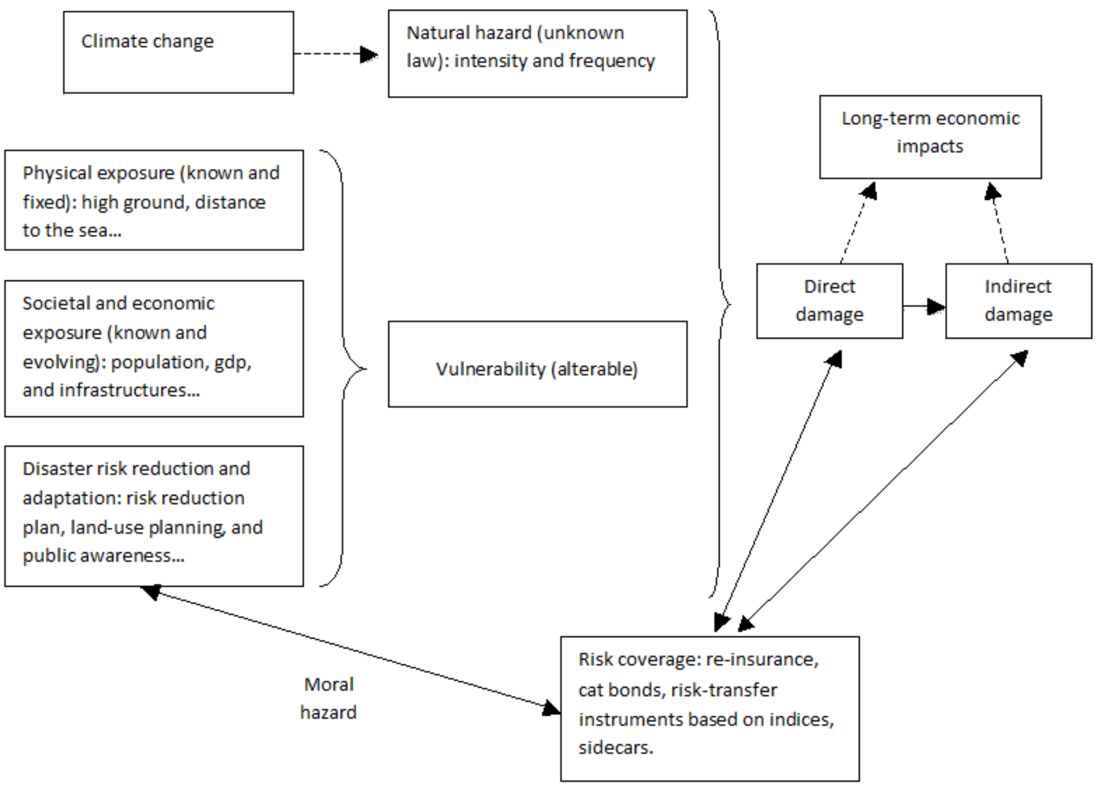
Note: Earthquakes, floods and cyclones from 1980 to 2007 for all countries are taken into account in columns 1, 3 and 4. In column 2 and 5, only countries with a non-response rate lower than 60% are taken into account. Columns 4 and 5 use predictions for non-response for damage. Standard errors (clustered by country levels) are in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

Table 1.3 : Determinants of damage - Tobit and ZINB average marginal effects

	(1) Log(Killed /Population) All Countries Tobit	(2) Log(Killed /Population) NR<60% Tobit	(3) Log(Killed /Population) All Countries ZINB
Log(Gdp/Population)	0.948***	-0.335	12.423***
	(0.260)	(0.540)	(2.572)
Log^2(Gdp/Population)	-0.058***	0.012	-0.755***
	(0.015)	(0.030)	(0.152)
Log(Population)	0.100***	0.075***	0.114
	(0.012)	(0.015)	(0.097)
Risk Index	0.003***	0.003***	0.013**
	(0.001)	(0.001)	(0.006)
Trend	0.005***	0.005**	0.011
	(0.001)	(0.002)	(0.018)
Observations	3161	1389	3161
Censored	1932	832	1932
Pseudo R^2	0.060	0.056	.

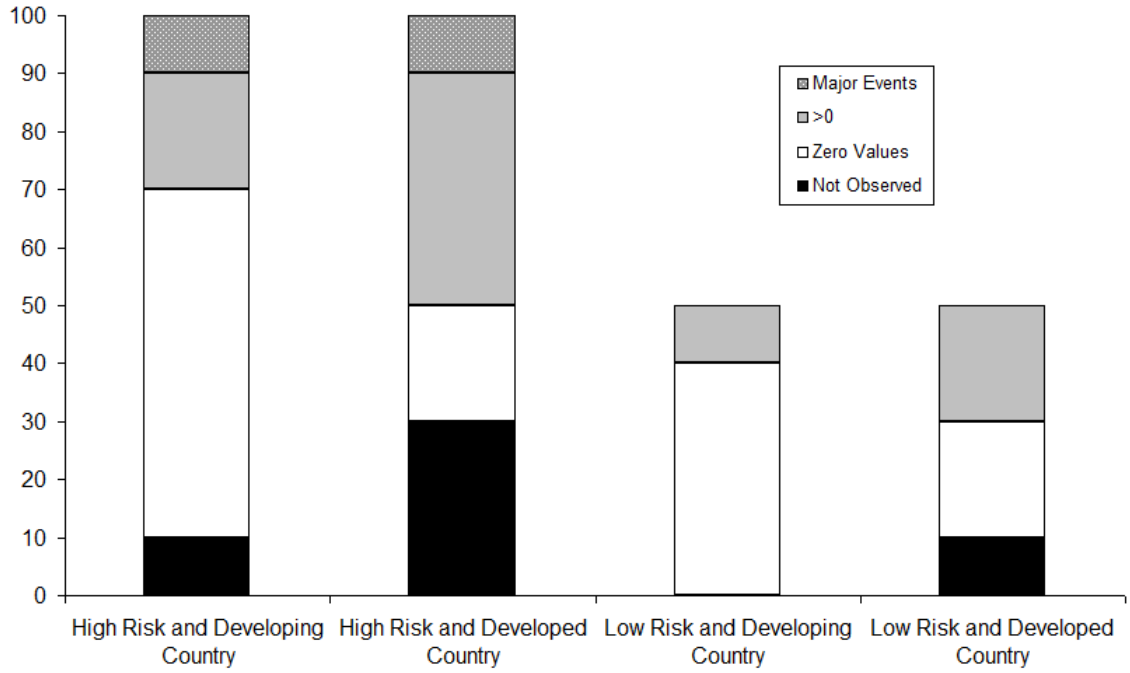
Note: Earthquakes, floods and cyclones from 1980 to 2007 for all countries are taken into account in columns 1 and 3. In column 2, only countries with a non-response rate lower than 60% are taken into account. Standard errors (clustered by country levels) are in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

Table 1.4 : Determinants of killed people - Tobit and ZINB average marginal effects



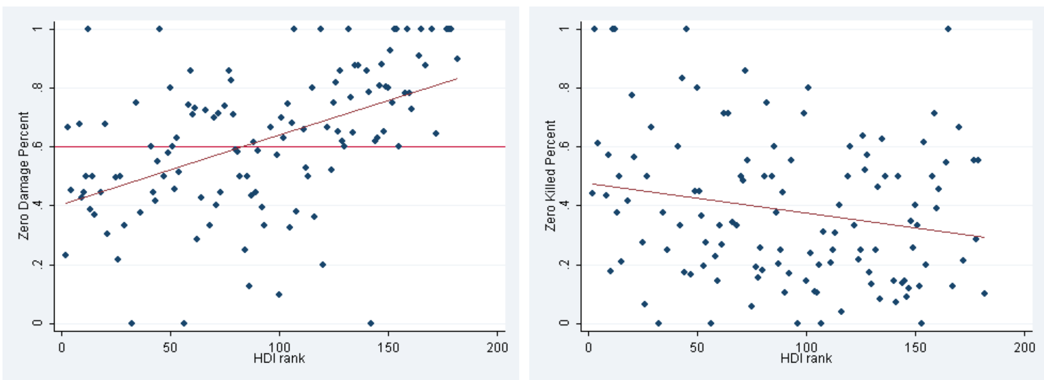
Source: Author's own representation

Figure 1.1 : A simple representation of economics of natural disasters



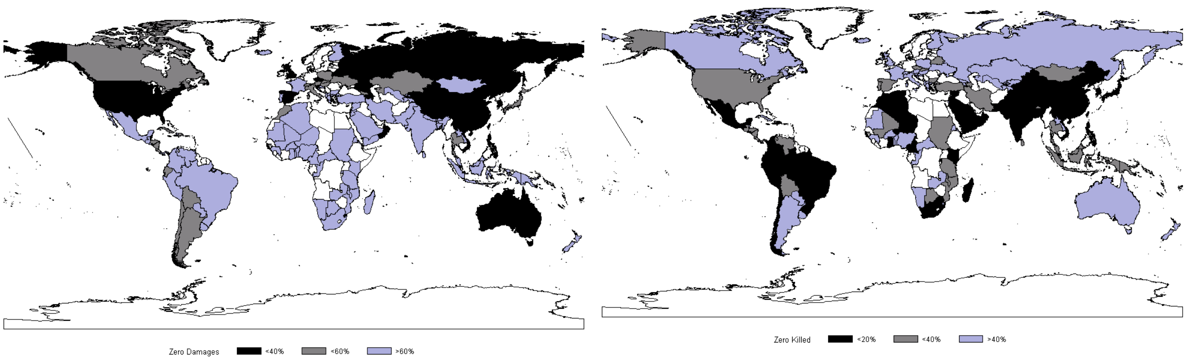
Note: In the high risk and developing country, 100 "potential" natural disasters occurred. 10 are not observed in the database, 60 are observed but damage is not known ("zero values"), 30 are observed with known damage. 10 are major events.

Figure 1.2 : Observed and actual data for damage



Note: Earthquakes, floods and cyclones from 1980 to 2007 are taking into account. The rank of HDI (Human Development Index) is for the year 2005. We compute frequency of events without damage (or killed) for each country. Fitted line is the linear trend between variables. The horizontal line represents the 60% exclusion rate.

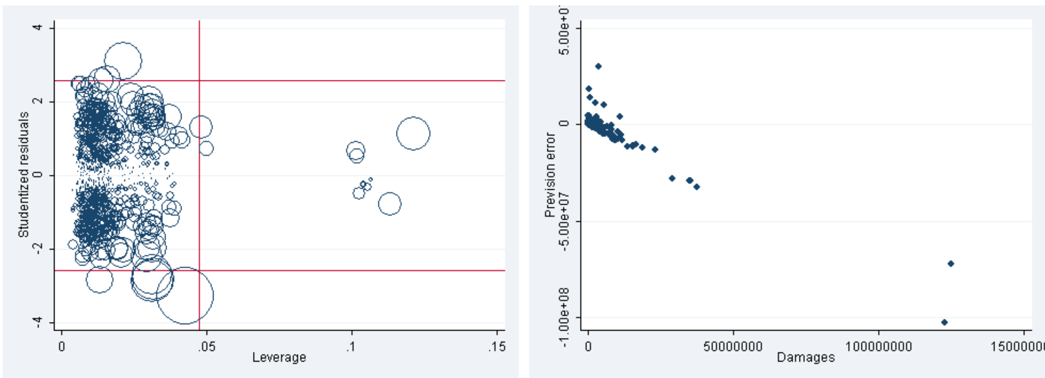
Figure 1.3 : HDI (Human development Index) rank and percent of events without damage or killed



Zero Damage Rate   Zero Killed Rate

Note: Map is done with SAS.

Figure 1.4 : Map of the word for zero damage and zero killed rates

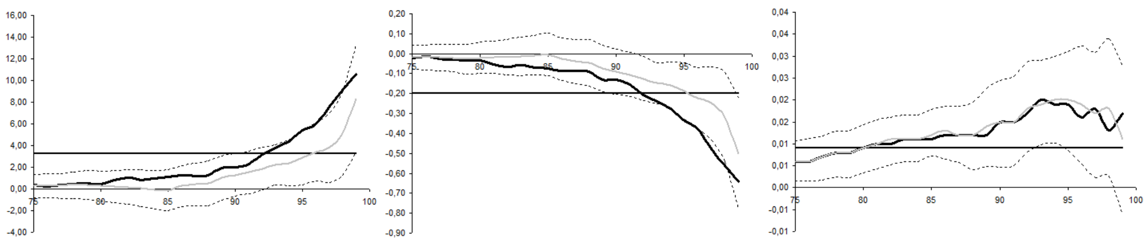


Leverage, studentized residuals and cook's distance   Absolute residuals

Note: Earthquakes, floods and cyclones from 1980 to 2007 are taking into account. In the left figure, the bubbles are weighted with the cook's distance. The horizontal lines represent the 0.5% quantile of a standard normal distribution. The vertical line is 3p/n with p the number of predictors and n the number of observations. In the right figure, absolute residuals are $\widehat{Damage} - Damage$ with $\widehat{Damage} = \text{Exp}\left(\text{Log}\left(\widehat{Damage}\right)\right) \cdot \text{Exp}\left(\widehat{\sigma^2}\right)$.

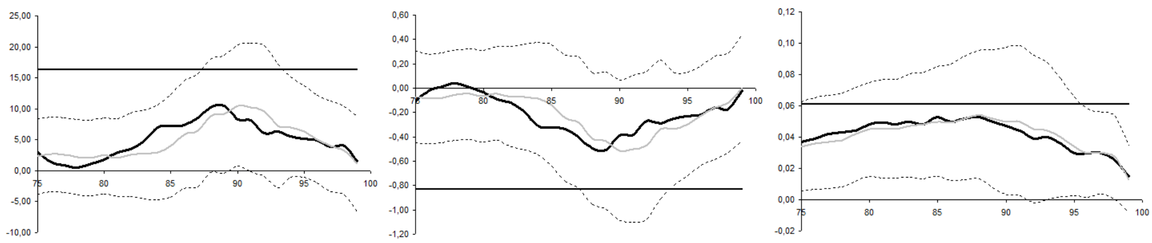
Figure 1.5 : Diagnostics tools for determinants of damage with more than 10 millions $ of damage and more than 10 people killed

Killed / Population



Log(GDP/Population) Log^2(GDP/Population) Risk Index

Damage / Population



Log(GDP/Population) Log^2(GDP/Population) Risk Index

Note: The X-axis is (conditional) quantiles of direct losses. The Y-axis is coefficients of the quantile regression (QR) for the (conditional) quantile. The continuous lines represent the three-step estimates of censored QR (black) and non-censored QR (grey). The dashed lines represent the confidence interval of non-censored QR estimators, obtained with block-bootstrap at country level. Tobit (black horizontal line) estimators are included to comparison.

Figure 1.6 : Three step censored quantile regression estimators

The Dynamics of Gasoline Prices: Evidence from Daily French Micro Data

Chapter 2

The Dynamics of Gasoline Prices: Evidence from Daily French Micro Data

Using millions of individual gasoline prices collected at a daily frequency, we examine the speed at which market prices of refined oil are transmitted to retail gasoline prices in France. For that, we estimate a reduced form model of state-dependent pricing where thresholds triggering price changes are allowed to vary over time and depend on the duration since the last price change. We find that the degree of pass through of wholesale prices to retail gasoline prices is on average 0.77 for diesel and 0.67 for petrol and depend on local market characteristics. The duration for a shock to be fully transmitted into prices is about 10 days. There is no significant asymmetry in the transmission of wholesale price to retail prices. Finally, the duration since the last price change has a significant effect on thresholds triggering price changes but a large variance of idiosyncratic shocks on thresholds is also crucial to replicate the size-distribution of price changes.

2.1 Introduction

At which speed agents incorporate specific or common shocks into their prices is a crucial issue in macroeconomic models. Why are prices sticky and fail to adjust immediately to their fundamentals? Using more than 5 million of individual gasoline prices collected at a daily frequency in thousands of gas stations in France, we examine in this paper to which extent retail gasoline prices are rigid and how long it takes for a gas station to incorporate an oil price shock.

Our paper is a contribution to the empirical literature on price rigidity models. We first document that gasoline prices adjust on average once a week whereas the price of wholesale gasoline fluctuates every day. Different theoretical models are used in macroeconomics to rationalize that retail prices respond sluggishly to shocks. The choice of a particular model of price rigidity has often large consequences on the macroeconomic dynamics (see Nakamura and Steinsson (2013)) and several papers have recently examined patterns of micro prices to assess the relevance of the different models.[1] However, at the micro level, one difficulty is to observe firm-level determinants of pricing behaviour: do prices adjust infrequently because shocks are rare or because adjustment costs are large? The market of gasoline is an unusual clean case to test the relevance of price rigidity models since a precise and economic meaningful proxy for marginal costs is available at a daily frequency. Our aim is here to investigate in details microfoundations of price rigidity and provide more evidence on the sources of this price rigidity.[2] While in many recent empirical papers, sectoral inflation is often used to identify aggregate shocks on marginal costs (see for example Fougere *et al.* (2007)), we here use daily prices of diesel and unleaded petrol set at the Rotterdam market to approximate the marginal cost of retail gasoline firms. We then relate prices to costs by estimating a reduced form of a menu cost model.[3] In this type of model, the probability of price changes depends on the gap between the nominal price and the price that would be observed without frictions. If this gap exceeds a certain threshold, prices are adjusted; the threshold triggering price changes is assumed to depend positively of the adjustment cost. In gas stations, the physical cost of changing prices may be rather small. Moreover, a standard menu cost model (e.g. Golosov and Lucas 2007) would not predict the M-shape distribution of price changes we observe in our sample but rather a bimodal distribution of price changes. Two types of theoretical models that both incorporate imperfect information in price rigidity models help to reproduce more flexible price change distributions. First, Woodford (2009) provides some microfoundations for random menu cost model: firms have fuzzy information about current economic conditions and they might change their prices not exactly when the loss in profits exceeds the menu cost since they only receive a noisy signal on the current price gap. Contrary to Woodford (2009) where the observation and menu costs are lumped together, some recent models (e.g. Alvarez et al. (2011) or Bonomo et al. (2011)) consider separately a price adjustment cost and an observation cost. In those models, price

[1] See Nakamura and Steinsson (2008) for the United States or Dhyne *et al.* (2006) and Vermeulen *et al.* (2012) for the euro area.

[2] One caveat is that our results obtained using daily price data might have relatively small consequences for the dynamics of aggregate business cycles. However, Abe and Tonogi (2010) show that using daily price data instead of monthly data can have large implications for our assessment of the aggregate frequency of price changes and for CPI inflation.

[3] We here extend the model used by Davis and Hamilton (2004) on gasoline prices by examining both the extensive and the intensive margins of price adjustments.

change decisions depend on the price gap but also on the elapsed duration since the last price adjustment. This prediction would be quite consistent with periodic spikes in the hazard function as observed in our data at half a week, one week and two weeks. In our microeconometric model, we allow thresholds triggering price changes to vary over time (for a given gas station): they depend on the elapsed duration since the last price change (as predicted by Alvarez et al. (2011)) and on a random idiosyncratic shock (as in Woodford (2009) model). This empirical model is rather flexible since it encompasses a simple Calvo model when the threshold varies a lot but also a standard menu cost model when the threshold does not vary at all (Gautier and Le Bihan, 2011). We estimate our model for about $8,500$ French gas stations and we provide a distribution of parameters. First, we find that the degree of long-term pass-through of wholesale prices to retail prices is on average 0.77 for diesel and 0.67 for unleaded petrol and depend on local market's characteristics. Moreover, we find that adjustment thresholds are quite large but also vary a lot over time. The elapsed duration since the last price adjustment explains a significant part of this variability which is consistent with predictions of Alvarez et al. (2011) model but the large variance of the idiosyncratic shock on thresholds is also a key ingredient to reproduce the distribution of price changes as predicted by Woodford (2009).

The recent increase in the volatility of raw material prices raises some issues on its transmission to inflation (see for instance ECB (2010) for a discussion of some policy implications). Our second contribution is to examine how long it takes for gas stations to incorporate a shock on wholesale prices to their retail gasoline prices. For that, we use simulated trajectories of our microeconometric model and study the aggregation of non-linear pricing strategies followed by heterogenous firms. We find that, on average, it takes about 10 days for a shock to be fully transmitted into retail gasoline prices. We also compare the aggregate responses of prices obtained when we assume different models of price rigidity. Using aggregate data and time-series analysis, several papers examine delays and possible asymmetry in transmission of a shock to retail gasoline prices. Results are quite contrasted depending on the methodology used (Frey and Manera, 2007). Geweke (2004) also points out two possible aggregation issues in this type of literature. The first one is related to aggregation of observations over time and arises when the frequency of observations is lower than the frequency of price adjustments. The second issue is related to aggregation across firms: most studies use average national prices and do not investigate the heterogeneity of gasoline price dynamics. Using daily individual data, we are here able to take into account non-linearities in the price-setting behaviour and replicate the infrequency of price changes. We then examine the heterogeneity across firms in price responses to shocks. We compare the aggregate response of prices obtained using simulated individual trajectories and the response obtained using a linear error correction model on aggregate data and find rather small differences. Moreover, we investigate the role of adjustment costs as possible source of asymmetric transmission of cost shocks to retail prices. Using parameter estimates of our microeconometric model, for a given cost shock, we test whether the threshold triggering price increases is lower than the threshold triggering price decreases. We find no significant asymmetry.

The layout of the paper is as follows. In Section 2, we describe our micro data set and provide the main stylized facts on gasoline price rigidity in France. Section 3 presents our estimated empirical model of price rigidity. Our main results are presented in Section 4. In section 5, we estimate the delay for retail prices to respond to aggregate shocks for different price rigidity models and investigate possible asymmetric response of retail prices to cost shocks. Section 6 concludes.

2.2 Daily Micro Data On Gasoline Prices

2.2.1 Data

Our data set consists of individual prices reported by all gas stations selling more than 500 m^3 of gasoline per year in France. Since January 2007, gas stations have to report all their price changes for unleaded petrol and diesel to the Ministry of Economy. The data collected are then made available on a governmental web site (www.prix-carburants.gouv.fr). This web site is intended to foster competition by providing a public and free information on prices. Some other private web sites offer similar services but the updating of prices is only voluntary whereas in our case, the public administration may force retailers to report their price changes.[4] We use historical data extracted every day at 23:59 from this governmental web site for the period from January 1st 2007 to May 31th 2009. The main variables available in our data set are the following. First, the price of a liter of diesel and unleaded petrol; this price includes all taxes, expressed in euros with three decimals. Another variable is the date of the report expressed in DD/MM/YY, which enables us to follow the same price in a given retailer. An identification number is associated to each retailer. We have some information on the retailer: brand name, location and services. Our data set is not exhaustive in terms of gas stations because there is a threshold requirement for participation; it still covers a

[4] The French competition authority is in charge of the website and the accuracy of information. For instance, between January 2011 and June 2011 about 6,000 gas stations were controlled and 72 notices of violation were issued by inspectors. Moreover, 600,000 consumers visit regularly the website and can declare (via mail and phone) if the price is different on the website.

large majority of gas stations operating in France.[5] The French market of gasoline consists of four types of retailers: (i) gas stations belonging to supermarkets; (ii) gas stations belonging to major oil companies like Total, Elf, Shell...; (iii) small independent retailers which do not depend from major oil companies; (iv) gas stations located on motorways which often belong to oil companies. One important feature is that the level of prices is on average lower in supermarkets than in other gas stations. We have no information on the demand addressed to each individual gas station.

The retail gasoline price can be decomposed in three main elements: (i) the wholesale price of fuel (after refining) which represents about 75% to 85% of total operating expenses of the gas stations (according sectoral national accounts published by Insee). We approximate this cost by using the price of refined fuels quoted in Rotterdam; (ii) distribution costs include costs of operating the outlet (rent, wages...) and transportation costs. They are not observed *per se* in our study, those costs might not change at a daily frequency and depend on each gas station, they will be considered as idiosyncratic; (iii) taxes. The two main taxes (VAT and TIPP) represent about 65% of the unleaded petrol prices and 60% of diesel prices. TIPP (domestic tax on petroleum products) is an excise tax of 42.84 cents per liter of diesel and 60.69 cents per liter of unleaded petrol whereas VAT is a proportion of prices including TIPP (19.6%). Both tax rates were not modified during our sample period. We use prices excluding all taxes according the following formula: $p_{excl\_taxes} = \frac{p_{inclu\_taxes}}{1+VAT} - TIPP$ where p_{excl_taxes} is the price excluding taxes, p_{inclu_taxes} the price including all taxes, VAT the VAT rate and $TIPP$ the TIPP excise. Gas stations usually list their prices with three decimal places.

Data are corrected for some measurement errors.[6] We also exclude from our sample prices observed on Sundays. Many gas stations are opened on Sunday but very few price changes are observed on Sundays since employees are not present to decide price changes. Moreover, for identification reasons, we also restrict our sample to gas stations that are observed at least 300 days (excluding Sundays) which represent more than 85% of gas stations of our initial data set. All in all, our sample consists of $8,802$ gas stations for diesel and $8,565$ for unleaded petrol, on average each gas station is observed during 615 days for diesel and 619 days for

[5] We test the consistency of our micro data set by comparing the average price computed on our data to the weekly aggregate price of unleaded petrol and diesel published by the Ministry of Economics (Figure B1 in Appendix B). The two series are highly correlated. The online appendix is available upon request or at the following address: https://sites.google.com/site/erwangautiereconomics

[6] We drop gas stations where price durations are longer than one month (a little less than 10% of gas stations). Occurrences of large price increases (larger than 8%) followed by large price decreases the next day have been corrected. These adjustments concern less than 0.2% of all price adjustments.

unleaded petrol. Our data set contains more than 5.4 millions of individual price quotes for diesel and about 5.3 millions for unleaded petrol.

2.2.2 Stylized Facts on Gasoline Price Rigidity

Table 2.1 summarizes basic findings on frequency and size of price changes and on price durations. The average price duration is about one week (5.6 days for diesel and 5.9 days for unleaded petrol).[7] There is some heterogeneity across firms: the average price duration is about 5 days in supermarkets versus 6 days in other gas stations. Overall, retail gasoline prices are rigid: they remain unchanged during several days whereas wholesale prices are modified every day.

Figure 2.1 plots the hazard functions of price adjustment for diesel and unleaded petrol (i.e. the instantaneous conditional probability of a price change given that a price was not modified since a given duration). The hazard rates are not monotone and show regular peaks at durations equal to 3, 6, 12 and 18 days (which corresponds to half a week, one week, two weeks and three weeks since Sundays are excluded). This suggests that gas stations change their price following a regular schedule: collecting and processing information might be costly[8] and gas stations review their price only infrequently. This would be consistent with predictions of observation cost models (e.g. Alvarez et al. 2011) where the hazard function of price changes depends on the elapsed duration since the last price change. The timing of price adjustment would be not only driven by changes in wholesale Rotterdam prices but also by the duration since the last price adjustment.

On the size of price changes, price increases are almost as frequent as price decreases for diesel whereas for unleaded petrol, the frequency of price decreases is smaller than the frequency of price increases (9.5% versus 10.6%). The average price increase (3.4% for diesel and 4.6% for unleaded petrol) is close to the average price decrease in absolute values (resp. 3.5% and 4.9%).[9] Price changes are larger for supermarkets than for other gas stations. Figure 2.2 plots the distributions of price changes different from 0. One original pattern of those distributions is their $M-$shape, where small price changes (in percentage) are rare.[10] This pattern of

[7] Using US wholesale price data, Davis and Hamilton (2004) and Douglas and Herrera (2010) find price durations shorter than one week.

[8] Zbaracki *et al.* (2004) and Levy *et al.* (2010) both mention that adjusting prices involve a rather long process because managers have to collect information on costs or on prices of competitors.

[9] We here provide figures controlling for the share of the wholesale price in total cost (i.e. we divide price change by the estimated firm-specific share of wholesale price (see below)). Without controlling, the average price increase (resp. decrease) is 2.6% (resp. −2.7%) for diesel and 3.1% (resp. −3.3%) for unleaded petrol.

[10] Asplund *et al.* (2000) using Swedish daily gasoline prices find a similar distribution for price changes.

the data is not consistent with a standard menu cost model (e.g. Golosov and Lucas, 2007) which predicts a strong bimodality of the price change distribution.[11] On the contrary, a Calvo model predicts a size-distribution of price changes close to the distribution of cost changes. In this latter model, the scarcity of small price changes would be due to frequent large changes in production costs. However, the distribution of wholesale price changes is close to a normal distribution with zero mean (Figure 2.2). Finally, models assuming imperfect information (e.g. Alvarez et al. 2011 or Woodford 2009) have more flexible predictions and are able to reproduce the $M-$shape distribution of price changes. In those models, the shape of price change distribution depends on the degree of imperfect information. In particular, in Alvarez et al. (2011) where the menu cost and information cost are separated, the shape of the distribution depends on the ratio of the two costs: if the menu cost is much larger than the information cost, the size-distribution of price changes is bimodal whereas if the menu cost is very small compared to the information cost, the size-distribution is close to a normal distribution.[12]

2.3 Gasoline Price Rigidity: An Empirical Model

Gasoline prices appear as a textbook example to illustrate price stickiness models: price changes in gas stations are infrequent whereas marginal costs are modified every day, price changes are on average rather large and small price changes are rare. In those models, firms set their prices in a monopolistic competition framework. The price level that would be observed in the absence of rigidity (called frictionless price) is given by a mark-up over marginal costs. In most empirical papers dealing with price rigidity, one difficulty is to measure at the firm-level the marginal costs. Some empirical studies relate prices to aggregate or sectoral inflation (Cecchetti (1986) or Fougere *et al.* (2007) for example) whereas other papers use more precise approximations of costs like aggregate wage costs or wholesale prices (e.g. Fougere *et al.* (2010), Ratfai (2006) or Dutta *et al.* (2002)). Using a statistical decomposition, Dhyne *et al.* (2011) identify the marginal cost to a product-specific unobserved factor. In our case, the marginal cost of gasoline prices is approximated by the wholesale prices of gasoline (diesel and unleaded petrol) quoted at the Rotterdam market. An alternative index of marginal cost would have been crude oil prices (e.g. Brent) but this index excludes refining costs and the marginal cost measure would be the same for unleaded petrol and diesel, which

[11] Cavallo and Rigobon (2011) provide some results on bimodality of supermarket price change distributions.

[12] Another possible explanation of this M-shape distribution of price changes might be related to the number of digits used to display prices and pricing points (see Table D1 in Appendix D for details).

would provide less economically meaningful results on the pass-through. Moreover, in France, a large share of diesel consumption is imported and Rotterdam is the major trading area for refined products in Europe.[13] Thus, some firms buy their gasoline at the Rotterdam spot price and for those outlets, the marginal cost is quite precisely measured. For unleaded petrol and diesel produced in France, the wholesale price may vary across stations in the same area since it is determined by the different refineries which supply gasoline retail outlets. Differences in the wholesale price across refineries are unobserved and depend on specific refining costs. However, according to Asplund et al. (2000), when stations are owned and operated by a refiner, Rotterdam prices are often used as transfer price between refineries and outlets.

We estimate a rather flexible form of state-dependent model. This model aims at reproducing the main characteristics of gasoline price adjustments: (i) price changes are infrequent; (ii) the timing of price changes depends on the evolution of wholesale prices but also on the elapsed duration since the last price change; (iii) small price changes are quite rare but price changes vary a lot over time for a given firm. A standard adjustment cost model (Golosov and Lucas, 2007) would predict that price adjustments are discrete, quite large and depend only on price fundamentals. In that model, there is a cost to adjust prices and firms trade off between the opportunity cost of deviating from the optimal price (i.e. the foregone profit) and the adjustment cost. Firms tolerate that their actual posted price deviates from the frictionless price (i.e. the price that would have been observed without adjustment cost) as long as this deviation is not too large.[14] The optimal frictionless price would be defined as:

$$p^*_{it} = \alpha_i + \beta_i p^o_t + \varepsilon_{1,it} \tag{2.3.1}$$

where p^*_{it} is the logarithm of the optimal price in the gas station i at date t, α_i is a gas-station specific effect correlated to the mark-up[15], and p^o_t the logarithm of the price of refined oil product sold in Rotterdam at the spot market at day t. $\varepsilon_{1,it}$ is a firm- and time-specific shock ($\varepsilon_{1,it}$ are supposed normally distributed

[13] As noted by Meyler (2009), 168 million tonnes of refined product were traded in Rotterdam in 2009. However, some Mediterranean ports handle also smaller amounts of liquid petroleum products (Marseilles (66 million tons) and Trieste (36 million tons)).

[14] Another possible margin of adjustment are inventories. However, according to the French competition authority inventory capacities are small at the retail level and firms most often use a replacement value to set their prices (Hosken *et al.* 2008). Our frictionless price can be considered as a replacement cost.

[15] If we suppose a Cobb Douglas cost function, the log-marginal cost of retailing one liter of gasoline would be equal to: $mc = \beta p^o + (1-\beta)w$ where β is the share of wholesale gasoline in the cost functon, p^o the log-price of a liter of wholesale price and w is the log-price of other operating costs. The markup is then equal to: $p - mc = p - \beta p^o - (1-\beta)w = \alpha$. Since we do not observe w, we estimate $\widetilde{\alpha} = p - \beta p^o$ and it implies that $\widetilde{\alpha} < \alpha$ (if $\ln(W) < 0$).

with mean 0 and variance σ_{1i}^2). We could interpret p_t^o as a common shock to the frictionless price and $\varepsilon_{1,it}$ as an idiosyncratic shock to the frictionless price.[16] Under some conditions shown to be of the (S,s) type (see Sheshinski and Weiss (1977) or Hansen (1999)), the optimal adjustment rule is then to adjust the price only if the difference between the optimal price p_{it}^* and the price $p_{it-\tau}$ modified at period $t-\tau$ (where τ is the duration since the last price change), exceeds some threshold S_i (for price decreases) or s_i (for price increases). When prices are reset, new prices are set at the optimal frictionless price. The firm's pricing decision depends on the distance covered by p_{it}^* between dates $t-\tau$ and t.

However, the (S,s) model puts strong restrictions on the patterns of price adjustments. First, the size of the price change is the same for all price decreases (S_i) and all price increases ($-s_i$). This prediction is not consistent with the observed variance of price changes over time (for a given firm). Second, in a standard (S,s) model, the timing of the price adjustment only depends on the frictionless price, which is not fully consistent with our observations. Thus, we rely here on a time-varying thresholds model to reproduce the timing and the size-distribution of price changes. In particular, we assume that the thresholds depend on a random idiosyncratic shock (as predicted by Woodford (2009) random menu cost model[17]) but also on the elapsed duration since the last price change (as predicted by Alvarez et al. (2011) model). The price decision rule can be summarized as follows:

$$\begin{array}{rl} \text{if } p_{i,t-\tau} - p_{i,t}^* \geq S_{it} & p_{it} = p_{i,t}^* \\ \text{if } p_{i,t-\tau} - p_{i,t}^* \leq s_{it} & p_{it} = p_{i,t}^* \\ \text{if } S_{it} > p_{i,t-\tau} - p_{i,t}^* > s_{it} & p_{it} = p_{it-\tau} \end{array} \tag{2.3.2}$$

where upper and lower bands are defined as:

$$S_{it} = \gamma_{iS} X_{it} - \varepsilon_{2,it} \tag{2.3.3}$$

$$s_{it} = \gamma_{is} X_{it} + \varepsilon_{2,it} \tag{2.3.4}$$

where S_{it} and s_{it} are the upper and the lower stochastic bands, X_{it} are exogenous variables that modify the timing of price adjustments (here the elapsed duration

[16] Theoretical models of price rigidity used in macro models often assume that idiosyncratic shocks are persistent. Some recent models also assume that idiosyncratic shocks have fat-tailed distributions in order to replicate the variance of price changes (Gertler and Leahy, 2008 or Midrigan, 2011). However, such assumptions would make the estimation of our empirical model less tractable. Moreover, the log-difference of p_t^0 shows also some persistence and p_t^0 might capture (at least partly) the persistence in shocks often assumed in macro models. We here follow the empirical literature estimating reduced (S,s) models on prices and assume the idiosyncratic shock as normal and transitory.

[17] As shown by Caballero and Engel (1999), thresholds that fluctuate over time can be obtained under a random menu-cost assumption. In particular, they define an adjustment hazard function which relates the probability of a price change to the gap between the current price and a frictionless optimal price (see Figure C1 in Appendix C).

since the last price change[18]) and $\varepsilon_{2,it}$ is firm- and time-specific shock to the timing of price adjustment ($\varepsilon_{2,it}$ are supposed normally distributed with mean 0 and variance σ^2_{2i}).[19] The exogenous shock on the timing of adjustment is assumed to be independent from the shock $\varepsilon_{1,it}$ on the optimal price. Our model is rather flexible. For instance, it encompasses the Calvo model: when the threshold varies a lot (the variance of $\varepsilon_{2,it}$ is very large), the model predicts a constant probability for a price change and generates small price changes. If the threshold does not vary at all, the empirical model would reproduce the predictions of a standard fixed adjustment cost model (see also Dhyne *et al.* (2011), Fougere *et al.* (2010), Gautier and Le Bihan (2011) and Honore *et al.* (2012) for similar empirical models).

This specification is close to the model considered in Davis and Hamilton (2004) or Douglas and Herrera (2010). However, we depart from those studies in one dimension. We here use the size of price changes to identify first the idiosyncratic shock on the frictionless price and second to disentangle the volatility of the frictionless price and the volatility of the adjustment cost over time. We here also extend Dhyne *et al.* (2011) by allowing S and s to be not necessarily equal in absolute values: a lower absolute value of s compared to S would imply a quicker adjustment to wholesale price increases than to wholesale price decreases. This potential asymmetry in the thresholds triggering price changes is introduced to mimic possible asymmetries in adjustment costs. However, in the long term, all firms will incorporate the wholesale price variations in their prices and the degree of long-term pass-through is equal to β.

We estimate a pricing decision rule for every single gas station (separately for diesel and unleaded petrol) since the time dimension of our sample is large (prices are observed during more than 600 days on average for a given gas station). We are then able to estimate the distributions across gas stations of α, β, σ_1, γ_s, γ_S and σ_2 and to examine the heterogeneity in the pass-through of oil prices to retail prices and in adjustment costs. There are two main stochastic processes in our econometric specification and two groups of parameters to estimate: the first process is associated to the frictionless price and we estimate for each gas station i, α, β and σ_1. The second group of parameters is associated to the time-varying adjustment thresholds where we estimate γ_S, γ_s and σ_2. Formally, our model is a bivariate sample selection model where the first equation gives the price change

[18] We here consider 8 dummy variables corresponding to durations equal to 1 day, 2, 3, 4, 5, 6, 7 days and more than 7 days in order to reproduce the shape of the hazard function (Figure 2.1).

[19] Some recent theoretical papers suggest different assumptions on the distribution of menu costs to replicate the variance of the size of price changes (e.g. Midrigan (2011)). However, the mapping between these alternative distributions of menu costs and the form of threshold processes in our reduced-form model is not obvious. We assume normal transitory shocks to the thresholds because we also want to keep the estimation of our model tractable.

decision and the other one the size of the price changes.[20] If the same regressors appear in both equations, the identification comes from the functional form. We here use an exclusion restriction and we assume that the adjustment thresholds vary with the duration since the last price adjustment. Following Alvarez et al. (2011) or Bonomo et al. (2011), in presence of observation costs, firms decide the date of the next price review and the hazard of price adjustment depends on the duration since the last price change. Observation costs prevent firms from monitoring economic conditions at a daily frequency. In our case, we find some periodic spikes in the hazard of price durations at half a week, one week and two weeks that cannot be easily related to changes in costs. These delays between price changes may correspond for oil companies to the duration for monitoring and processing information and deciding a new price; Levy *et al.* (2010), Zbaracki *et al.* (2004) or Asplund *et al.* (2000) provide direct evidence on information costs paid by firms when they change their prices. These regularities in the price revision process would affect the timing of price adjustment but not the frictionless price which is mainly governed by wholesale prices. We estimate this model using standard maximum likelihood function procedures. Details on the likelihood function can be found in Appendix A.

2.4 Results

Table 2.2 reports statistics on the distribution of parameters estimated firm by firm using the time-varying (S, s) models separately for diesel and unleaded petrol prices.

2.4.1 Frictionless Price

The first three lines of Table 2.2 report results associated with the frictionless price p^*. The long-term pass-through of the wholesale market price (Rotterdam prices) to retail prices is positive and significant in all firms. For diesel, the median pass-through is 0.79 whereas for unleaded petrol it is lower (0.68). This parameter captures in our model the weight of wholesale gasoline in marginal costs. Using national accounts in the retail gasoline sector, the share of wholesale gasoline cost in total costs is about 75 to 85%, which is quite consistent with our results.

Figure 2.3 displays the whole distribution of β parameters estimated using gas station data. For both diesel and unleaded petrol, the distribution displays two

[20] We assume that α, β, and σ_1 parameters associated to the frictionless price are the same in both equations (probability and size of price changes), which gives us additional degrees of freedom for the estimation and allows us to identify threshold parameters γ associated to all duration dummy variables and also σ_2.

modes. This heterogeneity corresponds to differences in the degree of pass-through between supermarkets and other gas stations: the pass-through is a little higher in supermarkets than in other gas stations (e.g. for diesel, 0.82 in supermarkets versus 0.71 in other gas stations, see Table 2.3). The heterogeneity of β_i can also be related to local competition, demand characteristics and services provided in gas stations. Tables E1 and E2 in Appendix E present results of OLS regressions relating β_i to those variables for diesel and unleaded petrol. We find that local competition affects significantly and positively β_i.[21]

The parameter α_i captures the average firm's mark-up. We find rather small values for this parameter: about 2% on average for diesel and around -2% for unleaded petrol. Since we do not observe all costs at the gas station-level, the parameter α will also capture the effect of other costs on the frictionless price, which reduces the estimated mark-ups.[22] However, we assume that other costs such as labour costs are not modified at a daily frequency and did not move significantly during our sample period. So, the parameter α_i should be correlated with mark-ups and we interpret heterogeneity in α_i among gas stations as differences in mark-ups. For instance, we find that mark-ups are lower in supermarkets than in other gas stations for both diesel and unleaded petrol (Table 2.3). Tables E1 and E2 in Appendix show further results on the heterogeneity of mark-ups among gas stations. In particular, we find a negative correlation between the degree of competition and α_i and the presence of a store, car services or high quality gasoline in the station has a positive effect on markup because those services might lead to a more pronounced product differentiation.

Finally, the estimated values of parameters σ_1 (Table 2.2) are on average 1.58% for diesel prices and 1.92% for unleaded petrol prices. We can interpret the impact of the market Rotterdam price as a common sectoral shock and ε_1 captures all firm- and time-specific shocks. During our sample period, the standard deviation of Rotterdam price changes over a week is about 5 to 6%. So, idiosyncratic shocks appear to play a less important role in triggering price changes compared to recent results obtained using macro models (e.g. Golosov and Lucas (2007) or Midrigan (2011)).

[21] Detailed comments are provided in Appendix E.

[22] The estimated markup could even become negative for unleaded petrol. Suppose markups are given by $p - mc = p - \beta p^o - (1-\beta)w$ where p is the log retail price, β is the share of wholesale gasoline in the cost functon, p^o the log price of a liter of wholesale price and w the log price of other costs. For unleaded petrol, we find $\beta = 0.67$ on average, if $p = \ln(0.47)$, we observe on Figure A that $p^o = \ln(0.34)$. We obtain a markup equal to $-0.03 - 0.33w$. If w is unobserved, we estimate a negative markup close to -3% whereas if w is negative, we can obtain a positive markup, for instance if $w = \ln(0.6)$, markup is closer to 10%.

2.4.2 Thresholds

The second part of Tables 2.2 and 2.3 reports the parameters associated with the adjustment thresholds. In a standard constant adjustment threshold model, large values of the bands reflect large costs of price changes and all price changes are equal to the size of the inaction band. In our framework, adjustment thresholds are time-varying, allowing for variability in the size of price changes over time for a given gas station. However, in that case, the mapping between those parameters and the adjustment cost is not trivial because price adjust more frequently when thresholds are small than when they are large. Thus, a large variance of thresholds may imply frequent price adjustments even if the average threshold is large. For instance, when both the mean and the variance of adjustment thresholds are large, the price setting behaviour is very close to a Calvo price setting model (Gautier and Le Bihan 2011). We compare our results obtained with a time-varying (S, s) model with results obtained from a standard (S, s) model, a Calvo model and a time-varying (S, s) model without idiosyncratic shock on the thresholds (Tables F1, F2 and F3 in Appendix). Our main results are the following.

First, the average estimated adjustment thresholds in time-varying (S, s) models are much larger than those obtained for a fixed menu cost model. If adjustment costs are supposed random, firms are more likely to change their prices when the cost is small. So, adjustment thresholds are on average smaller when prices actually adjust than when prices are not modified. Thresholds in a time-varying (S, s) model are thus larger on average than in a standard fixed (S, s) model (Gautier and Le Bihan, 2011).

Second, the thresholds vary with the duration since the last price change: they are lower in absolute values for durations of 3 and 6 days but much larger for durations of 1 day, 2, 5 and 7 days. For diesel prices, the median of the threshold triggering price increases (γ_s) is a little larger than -4.3 and -4.4 for durations of 3 and 6 days whereas it is close to -5 for other durations. Results are very similar for price decreases and when considering supermarkets or other gas stations separately (Table 2.3). The parameters associated to thresholds reproduce the patterns of the hazard function (Figure 2.1). Variations in thresholds over the duration since the last price adjustment are consistent with predictions of price rigidity models assuming both a menu cost and an observation cost.

To examine in more details the heterogeneity of thresholds across gas stations, we present in Tables E1 and E2 in Appendix OLS regressions of estimated band width (average of S and s) on competition, firms' characteristics and local variables. In particular, we find that the degree of competition has a significant negative impact on the thresholds: competition may provide more incentives for gas stations to quickly review their prices and pay more frequently the information cost. Moreover, firms that use only attractive prices (ending in 0 or 9) are likely to have larger adjustment cost and show larger inaction bands (see Knotek (2010) for similar evidence).

Finally, the median variance of the shock ε_2 on thresholds is quite large about 3.2% for diesel prices and 3.9% for unleaded petrol prices (Table 2.2). Though we here restrict our analysis to one homogenous product and we take into account quite precisely the fluctuations of marginal cost, price changes vary a lot over time for a given firm.[23] A high value of σ_2 helps to replicate the large variability in the size of price changes over time (for a given station). This result would be consistent with predictions of Woodford (2009) where firms do not necessarily adjust their prices at the exact date when the loss in profits exceeds the menu cost. In this model, price changes are not only driven by variations in marginal costs but also by idiosyncratic shocks on information about the price gap. As a consequence, the size of price changes can vary a lot over time for a given firm. In our case, variations in thresholds associated to the elapsed duration since the last price adjustment help to reproduce the variability of price changes but are not sufficient.

2.4.3 Fit of the Model

In this subsection, we assess the goodness of fit of our model by testing its ability to replicate some moments of the data. We run Monte Carlo simulations on the basis of our parameter estimates and explanatory variables are taken at their sample values.[24] Then, we compute aggregate statistics like the frequency of price changes and several statistics on the distribution of price changes. Moreover, we compare the simulated statistics obtained with our time-varying (S, s) model with aggregate statistics obtained using estimates from three other models (Calvo[25], fixed (S, s) model[26] and elapsed duration fixed (S, s) model[27]).

Results are summarized in Table 2.4. First, the Calvo model does better in reproducing the average frequency of price changes whereas (S, s) models overestimate the frequency of price changes. This overestimation is however quite

[23] We decompose the variance of price changes (conditional on the day of the week) and find that on average, about 80% of the total variance of price changes comes from the variance of price changes within firm (over the sample period).

[24] We simulate each price trajectory 150 times.

[25] Prices are modified only if the outcome of an idiosyncratic process is above an estimated constant threshold which determines the exogenous probability of price change. This model is also an extreme case of the Woodford (2009) model where information imperfections are very large.

[26] The thresholds trigerring price changes are assumed not to vary over time and σ_2 is set to zero.

[27] The thresholds trigerring price changes are assumed to vary over time only because of the duration since the last price change and σ_2 is set to zero.

limited for the time-varying (S, s) model. On the average size of price changes, the time-varying (S, s) model and the Calvo model are very close to the average sample sizes. The Calvo and the time-varying (S, s) models differ in replicating the distribution of price changes. The Calvo model is unable to replicate the small fraction of small price changes whereas in simple menu cost models, most price changes are quite large. The time-varying (S, s) model is better able to reproduce the sample distribution of price changes, in particular its bimodality and its large variance. Moreover, this model can replicate regular peaks in the hazard function of price changes.

2.5 Aggregate Dynamic Response Of Gasoline Prices

How gasoline prices respond to a cost shock? We here simulate price trajectories using our microeconometric model and examine the aggregate response of prices to different shocks to the Rotterdam wholesale prices.

2.5.1 Simulation Exercise

Our simulation exercise consists of two steps: first we simulate individual price trajectories using our parameter estimates then aggregate those trajectories to calculate a simulated inflation rate. First, for all gas stations present in our sample, we simulate individual trajectories of prices. For that, we use parameter estimates obtained for every gas stations and run Monte Carlo simulations. Shocks $\varepsilon_{1i,t}$ and $\varepsilon_{2i,t}$ are drawn from two independent i.i.d. normal distributions with mean 0 and variances $\widehat{\sigma_{1i}}$ and $\widehat{\sigma_{2i}}$. We also assume Rotterdam wholesale prices to be constant, price decisions are only driven by the two idiosyncratic shocks. Each individual price trajectory is simulated for 55 days and the first 15 days are dropped to eliminate some possible initial conditions issues. We simulate each trajectory 500 times. We then compute the log difference of simulated prices for all trajectories and calculate the average of those differences to obtain an aggregate inflation rate. Then, we run again the same exercise but we introduce a permanent shock on the wholesale gas price (i.e. compared to the previous exercise, Rotterdam prices increase by a given percentage at date 16). We consider different shocks on the wholesale Rotterdam prices: -1%, $+1\%$, $+2\%$ and $+5\%$. For each experiment, we calculate the average aggregate inflation rate. Finally, we compute the difference between the average inflation rates obtained including exogenous shocks with the aggregate inflation rate obtained without any shock. This difference will capture the aggregate response of inflation to different shocks. We also compare the aggregate response of prices in our time-varying threshold model with aggregate responses obtained using two limiting cases of our flexible threshold model, the Calvo model and a fixed (S, s) model.[28] Finally, we compare our results with standard impulse response functions obtained from the estimation of a dynamic model on aggregate data. For that, we first compute the average price across firms on our sample period. Then, we relate the average price of diesel or unleaded petrol to Rotterdam prices by estimating a standard linear error correction model (ECM): we first estimate a long-term relation between the average retail prices and Rotterdam prices (in logarithms), then we regress the first difference of the average retail price on its lagged values, on the first difference of Rotterdam prices and its lagged values and on the stationary residuals from the first regression which are interpreted as deviations from the equilibrium level (Engle and Granger, 1987).[29]

2.5.2 Speed of Adjustment

Figure 2.4 displays the inflation response of diesel and unleaded petrol prices to a 1%-shock on Rotterdam prices for our three types of models. In all three models the long-term impact of the shock is equal to the average value of β (i.e. 0.77 for diesel and 0.67 for unleaded petrol). Using an ECM on the average gasoline price, we find a similar long-term pass-through of wholesale price shock to retail prices for both diesel and unleaded petrol.

Table 2.5 provides details on the delay of adjustment of retail prices, it gives the proportion of the shock absorbed in inflation after a certain duration. Using the time-varying (S, s) model as a DGP for the simulations and a $1\%-$shock, about 85% of the total response of retail gasoline prices is observed after 5 days and 95% of the total response is incorporated into prices after 10 days. There are some differences in the transmission delay of a shock between the different models used as DGP for the simulation. The transmission is shorter in the case of the fixed (S, s) model and longer in the Calvo model: 95% of the total response is obtained after 3 days in a fixed (S, s) model versus 15 days in the Calvo model (Figure 2.4). The time-varying threshold model appears as an intermediate case. Those differences in delays to incorporate shocks are consistent with the macro literature

[28] In the Calvo model, we assume that the decision of price change does not depend on the frictionless price but is only driven by an estimated constant parameter. In the constant (S, s) model, we estimate a constrained model where the variance of the shock on thresholds is zero. We assume that the thresholds are either constant for all durations or constant for the different values of durations but simulation results are quite close in both cases. Estimation results are provided in Tables D, E and F in Appendix.

[29] Using standard selection criteria, we select seven lags for Rotterdam prices and for the endogenous variables. The long-term equation relates the average price of diesel or unleaded petrol at date t to Rotterdam prices at date $t-6$ to take into account the infrequency of price changes.

on state- versus time-dependent models. In the Calvo model, the probability of price change is constant over time and exogenous, so it is not modified after a shock. Consequently, firms change their prices gradually and the inflation adjustment comes from the size of price changes when prices are modified. In a standard menu cost, the reaction is quicker because of a selection effect: firms adjusting their prices are those the closest to the threshold implying an increase in the frequency of price changes proportional to the shock. Thus, for (S, s) models, larger shocks are more quickly incorporated into prices since when the shock is large, the proportion of firms adjusting their prices is higher (Table 2.5). The random (S, s) model reduces the selection effect and makes the adjustment delay longer. If σ_2 is extremely high, price changes are not driven any more by changes in fundamentals but only by idiosyncratic shocks on thresholds and in that case, the model is close to a Calvo model.

We also investigate the heterogeneity in the transmission delays across gas stations. Figure 2.5 displays the cross-sectional distribution of durations before a full price adjustment to a 1% shock to Rotterdam prices assuming Calvo, constant (S, s) or time-varying (S, s) models. In all cases, the variance of the distribution is quite large. In the time-varying (S, s) model, the first quartile of the distribution is 5 days and the third quartile 11 days whereas the average delay is about 8.5 days. In supermarkets, the average delay before a full adjustment is 7.2 days for diesel (resp. 7.8 days for unleaded petrol) versus 9.2 days in other gas stations (resp. 9.4 days).

Finally, we compare our inflation responses obtained by aggregating individual responses to the impulse response function obtained using our error-correction model on the average gasoline prices. Overall, the dynamic response obtained with aggregate data shows a rather quick adjustment: after 10 days, 90% of the shock is transmitted, which is quite consistent with our results using a time-varying (S, s) model or Calvo model. However, some differences appear: two days after the shock, the response obtained using aggregate data is quite slower than responses obtained by aggregating individual responses. This slow initial reaction may be explained by stronger reductions of margins the first two days. Then, 3 days after the shock, the shape of the response is quite similar to the average of individual responses in Calvo or time-varying (S, s) models and the response is even a little quicker than the response obtained using a Calvo model.

2.5.3 Asymmetry of Adjustment

Using aggregate data, several empirical papers assess the asymmetry in the response of gasoline prices to oil shocks. Since the seminal paper of Borenstein et al. (1997), results are quite contrasted since they often depend on the frequency of data used (see Geweke (2004) or Bachmeier and Griffin (2003) for a discussion) and on the estimation methods (Frey and Manera (2007) for a survey). In most studies, the sources of asymmetry are not clearly identified. We here investigate asymmetry in adjustment costs as a possible cause for asymmetric response of prices to shocks. For that, we use the estimates obtained for the adjustment thresholds. If there is some downward price rigidity, it would imply that on average, for a given absolute value of the price gap $|p_{i,t-\tau} - p^*_{i,t}|$, a firm would be more likely to hit the threshold triggering price increases than the threshold triggering price decreases. In other words, it would imply that in absolute values, the threshold associated to price increases is smaller than the threshold associated to price decreases. To assess empirically the degree of asymmetry in price changes, we test that the difference between thresholds triggering price increases and decreases is significant at each duration (for every gas station) using a simple Wald test ($\gamma_s + \gamma_S = 0$ versus $\gamma_s + \gamma_S > 0$). Table 2.6 presents the proportion of firms for which the asymmetry is significant at a 1%-level. Only between 5% and 10% of gas stations selling diesel have asymmetric thresholds for durations between 3 and 7 days. This proportion is a little higher for unleaded petrol (between 8% and 12%). We also find that asymmetric behaviour is less frequent in supermarkets than in other gas stations. To compare those results, we run the same test on the standard fixed menu cost model and we find that only a very small proportion of gas stations have an asymmetric behaviour.

To test consequences of this small degree of asymmetry in price thresholds on the aggregate response of prices to a shock, we run simulations with a positive and a negative 1%-shock on Rotterdam prices for diesel and unleaded petrol. As expected, we obtain no difference in the speed of reaction of retail prices to a positive or a negative market price shock (Table 2.5). In all cases and for all models considered, after a positive or a negative shock, we find very similar average delays in the transmission of shocks.[30]

2.6 Conclusion

In this paper, we examine the degree of price rigidity in French gas stations using a data set of millions of price quotes collected at a daily frequency, for more than 8,500 gas stations on the period between January 2007 and June 2009.

For every gas station, we estimate a time-varying (S, s) model which allows us to replicate the infrequency of price changes and the large variance in the distribution of price changes. We find that the degree of pass-through of wholesale market

[30] As robustness exercise, we also run linear error correction models allowing for asymmetries in the short run dynamics, we find small differences in the response of average price of diesel or unleaded petrol to positive or negative wholesale shock but no specific asymmetry in the long run.

prices to retail gasoline prices is consistent with the share of wholesale gasoline cost in total costs: 0.77 on average for diesel prices and 0.67 for unleaded petrol prices. This pass-through is somewhat larger in supermarkets than in other gas stations. Moreover, local competition variables play a significant role to explain differences in pricing strategies across firms. Lastly, thresholds triggering price changes are rather large on average but vary substantially over time.

Finally, we simulate the aggregate response to shocks of gasoline retail prices. The adjustment of gasoline prices to wholesale price shocks is rather quick: it takes about 10 days for a shock to be transmitted into prices. We also compare responses obtained with alternative models of price rigidity and the impulse response function from a linear macro model. We also test for the asymmetry in the response of retail gasoline prices to market wholesale price shock. We assess whether thresholds triggering price increases and decreases are different. A larger threshold for price decrease implies that firms react more slowly to a decrease in costs. A small proportion of gas stations show some asymmetry in their price-setting behaviour and when it is the case, the asymmetry is small. Overall, there is no significant difference in the speed of retail price reaction to a positive or a negative wholesale price shock.

From a macroeconomic perspective two types of conclusions can be drawn. First, gasoline prices, often considered as highly flexible, are somewhat rigid. This rigidity might have small quantitative effects on business cycle since the delay of price response to a shock is rather short. However, contrary to other empirical studies, we are here able to investigate the sources of this price rigidity: menu costs and information imperfections play a significant role to account for the specific distribution of price changes. In particular, to reproduce the size-distribution of price changes, we need some variations over time in thresholds triggering price changes. We find that the duration elapsed since the last price adjustment has a significant impact on price adjustment thresholds as predicted by Alvarez et al. (2011). However, the large variance of idiosyncratic shocks on the thresholds triggering price changes is also crucial to reproduce the variability of price changes over time (for a given firm). This latter result is more consistent with models assuming random menu costs (e.g. Woodford (2009)). Second, gasoline prices have often a large contribution to the volatility of CPI inflation and Geweke (2004) underlines the need to better understand the implications of firm heterogeneity on aggregate dynamics of gasoline prices. We here find some significant heterogeneity across firms but it seems to have only a limited impact on the aggregate response of prices to a shock.

2.7 Tables and Figures

		Frequency			Implied	Size	
	Nb obs.	All	Increases	Decreases	duration	Increases	Decreases
- Diesel							
All	8,802	21.50	10.61	10.88	5.55	3.35	-3.54
Supermarkets	4,269	23.81	11.75	12.06	5.14	3.40	-3.58
Other stations	4,533	19.32	9.54	9.78	5.93	3.30	-3.51
- Unleaded petrol							
All	8,565	20.10	10.57	9.53	5.93	4.61	-4.94
Supermarkets	4,195	22.09	11.54	10.55	5.55	4.82	-5.11
Other stations	4,370	18.20	9.65	8.55	6.30	4.41	-4.77

Note: We compute firstly frequency and size of price changes for each retailer. Then we compute the average of sizes and frequencies. For the durations, we first compute the inverse of individual frequencies and then compute the average of durations. Size of price changes are calculated using prices excluding taxes. Size of price changes are divided by the estimated firm-specific share of wholesale prices into retail prices (β) (see Table 2.2) to control for differences in the cost structure of the different gas stations.

Table 2.1 : Price durations (in days), frequency and size of price changes (in %)

		Diesel				Unleaded petrol			
		Mean	Q25	Q50	Q75	Mean	Q25	Q50	Q75
α		2.15	1.15	1.92	3.12	-2.17	-3.29	-2.28	-1.18
β		0.77	0.71	0.79	0.83	0.67	0.62	0.68	0.72
σ_1		1.58	1.41	1.55	1.72	1.92	1.67	1.87	2.10
	1 day	-8.02	-7.94	-5.29	-3.73	-10.61	-9.96	-6.55	-4.59
	2 days	-6.45	-6.86	-4.65	-3.33	-8.74	-8.71	-5.91	-4.32
γ_s	3 days	-6.20	-6.29	-4.31	-3.12	-8.58	-8.07	-5.63	-4.10
	4 days	-6.40	-6.31	-4.50	-3.28	-8.37	-8.03	-5.76	-4.27
	5 days	-6.24	-6.47	-4.80	-3.59	-7.67	-8.00	-5.99	-4.50
	6 days	-5.63	-5.95	-4.41	-3.20	-6.88	-7.35	-5.46	-4.01
	7 days	-7.66	-7.30	-5.27	-3.89	-9.59	-8.55	-6.22	-4.67
	$>$ 7 days	-5.14	-6.29	-4.89	-3.83	-6.32	-7.61	-5.96	-4.71
	1 day	8.42	3.81	5.37	8.30	10.89	4.65	6.68	10.53
	2 days	7.00	3.37	4.71	7.05	9.17	4.35	6.05	9.05
γ_S	3 days	6.20	3.17	4.38	6.50	8.12	4.23	5.84	8.44
	4 days	5.97	3.24	4.49	6.36	8.39	4.36	5.99	8.36
	5 days	6.66	3.52	4.71	6.40	8.93	4.56	6.24	8.45
	6 days	5.62	3.08	4.28	5.81	7.84	4.07	5.63	7.67
	7 days	7.54	3.71	5.07	6.99	9.52	4.72	6.44	8.92
	$>$ 7 days	5.04	3.74	4.82	6.18	6.43	4.73	6.03	7.76
σ_2		3.33	2.64	3.22	3.96	4.05	3.14	3.89	4.82

Note: We estimate for each individual gas station a time-varying (S,s) model and then compute statistics on the parameter estimates we obtained. We consider all gas stations with more than 300 individual observations of prices (excluding Sundays). For diesel prices, $8,802$ values of parameters estimates are available and $8,565$ for unleaded petrol prices.

Table 2.2 : Estimation results - Time-varying $(\mathbf{S}, \mathbf{s})$ model

		Diesel prices		**Unleaded petrol prices**	
		Supermarkets	Others	Supermarkets	Others
α		1.32	2.97	-2.75	-1.73
β		0.82	0.71	0.71	0.63
σ_1		1.60	1.49	1.93	1.80
	1 day	-5.80	-4.87	-7.46	5.88
	2 days	-4.66	-4.64	-6.16	-5.75
γ_s	3 days	-4.11	-4.42	-5.48	-5.76
	4 days	-4.16	-4.75	-5.41	-6.03
	5 days	-4.40	-5.09	-5.67	-6.23
	6 days	-4.03	-4.69	-5.10	-5.73
	7 days	-4.98	-5.51	-6.01	-6.39
	$>$ 7 days	-4.75	-4.99	-5.85	-6.01
	1 day	5.89	4.98	7.70	5.93
	2 days	4.74	4.66	6.38	5.85
γ_S	3 days	4.17	4.52	5.63	5.96
	4 days	4.17	4.72	5.59	6.24
	5 days	4.32	4.99	5.74	6.55
	6 days	3.97	4.52	5.17	5.98
	7 days	4.65	5.37	6.12	6.71
	$>$ 7 days	4.62	4.95	5.92	6.11
σ_2		3.22	3.23	4.01	3.92

Note: We estimate for each gas station a time-varying (S, s) model and then compute statistics on the parameter estimates. We consider all gas stations with more than 300 individual observations of prices (excluding Sundays). For diesel prices, $4,269$ and $4,533$ parameters estimates are available for supermarkets and other stations respectively and $4,195$ and $4,370$ for unleaded petrol prices. We report in this table the median of the distribution of parameters.

Table 2.3 : Estimation results - Time-varying $(\mathbf{S}, \mathbf{s})$ model - Supermarkets versus other gas stations

	$F+$	$F-$	$dp+$	$dp-$	Prop. of $\|dp\|$ (in %)			
					$]0;1]$	$]1;2]$	$]2;5[$	$[5;+\infty[$
Diesel								
Sample	11.17	11.37	2.95	-3.04	13.31	28.62	43.04	15.04
Variable menu cost	12.80	12.68	3.53	-3.38	12.84	15.71	43.98	27.47
Duration menu cost	14.72	14.94	7.40	-6.79	0.11	0.92	22.24	76.73
Fixed Menu Cost	15.01	15.31	7.73	-7.04	0.03	0.55	19.30	80.12
Calvo	11.52	11.33	2.84	-2.93	24.43	19.62	36.43	19.52
Unleaded Petrol								
Sample	11.08	10.00	4.13	-4.24	8.28	15.90	49.28	26.54
Variable menu cost	11.96	11.51	4.91	-4.68	8.94	10.44	34.12	46.50
Duration menu cost	14.26	14.01	10.59	-9.80	0.05	0.21	6.68	93.06
Fixed Menu Cost	14.61	14.37	11.00	-10.16	0.02	0.11	4.95	94.92
Calvo	10.82	10.49	4.11	-3.89	18.01	15.13	33.25	33.61

Note: We compute simulated price trajectories using our parameter estimates and taking exogenous variables at their sample values. We then compute the average frequency of price increases and decreases ($F+$ and $F-$), the average size of price changes ($dp-$ and $dp+$) and the proportion (Prop. of $|dp|$) of small and large price changes. Size of price changes are divided by the estimated firm-specific share of wholesale prices into retail prices (β) (see Table 2.2) to control for differences in the cost structure of the different gas stations. We use simulations from the time-varying threshold model ("duration menu cost" refers to the model with thresholds depending on past duration whereas in "Variable menu cost" model, we add the idiosyncratic shock ε_2 to the model), the fixed (S,s) model and the "Calvo" model.

Table 2.4 : Simulated aggregated statistics

		Fixed (S,s)				Time-varying (S,s)				Calvo	
	nb days	-1%	1%	2%	5%	-1%	1%	2%	5%	1%	-1%
Diesel											
	1	0.74	0.74	0.75	0.82	0.39	0.39	0.41	0.53	0.22	0.22
	2	0.90	0.90	0.91	0.95	0.60	0.60	0.62	0.74	0.38	0.38
	3	0.95	0.95	0.96	0.98	0.73	0.73	0.75	0.84	0.50	0.50
	5	0.98	0.98	0.98	1.00	0.85	0.85	0.87	0.93	0.67	0.67
	10	0.99	0.99	0.99	1.00	0.95	0.95	0.96	0.98	0.86	0.86
	15	0.99	0.99	0.99	1.00	0.98	0.98	0.98	0.98	0.93	0.93
Petrol											
	1	0.73	0.73	0.73	0.77	0.36	0.36	0.37	0.44	0.20	0.20
	2	0.89	0.89	0.89	0.92	0.57	0.57	0.58	0.66	0.36	0.36
	3	0.95	0.94	0.95	0.96	0.69	0.69	0.71	0.77	0.47	0.47
	5	0.98	0.97	0.97	0.99	0.83	0.83	0.84	0.88	0.64	0.64
	10	0.99	0.99	0.99	0.99	0.94	0.94	0.94	0.96	0.84	0.84
	15	0.99	0.99	0.99	1.00	0.97	0.97	0.97	0.97	0.91	0.91

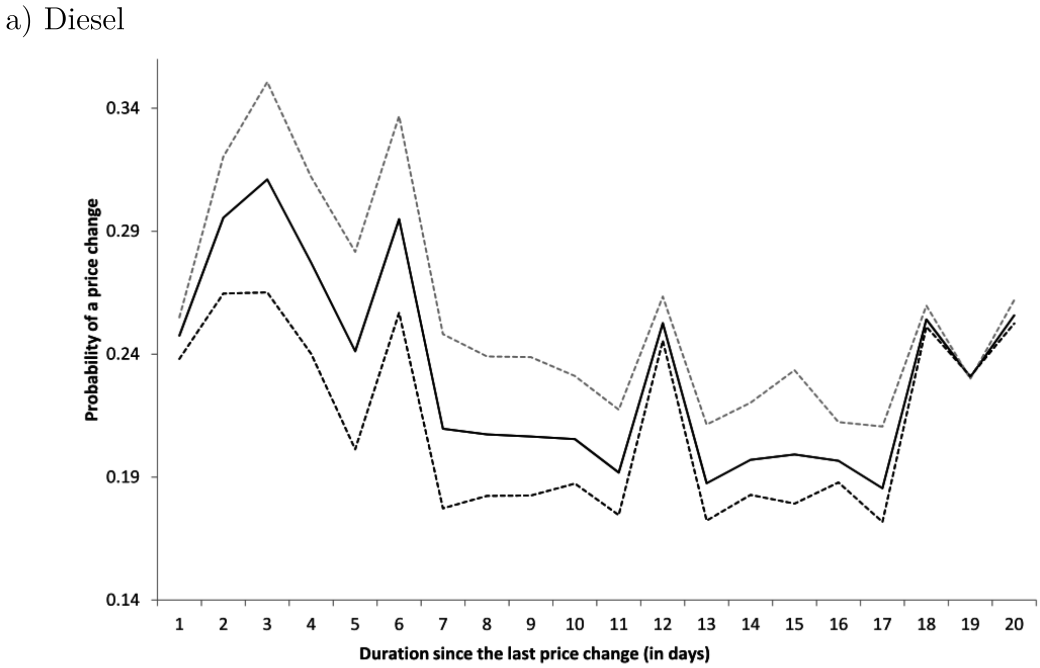
Note: We compute simulated price trajectories using our parameter estimates and aggregate those price trajectories. Then we run the same exercise but adding a permanent shock on market prices. We compute the difference between the two aggregate price indices obtained. We then calculate the cumulated response of retail prices to a shock as the cumulated difference. Finally, we compute the ratio as the cumulated response after a certain duration from the date of the shock on the total response measured as the cumulated response after 45 days. We use simulations from the time-varying threshold model, the fixed (S, s) model and the "Calvo" model.

Table 2.5 : Dynamic response of gasoline prices to shocks on wholesale market prices

	Diesel			Unleaded petrol		
	All	Supermarkets	Other stations	All	Supermarkets	Other stations
1 day	19.10	14.58	22.65	18.53	17.90	19.00
2 days	14.75	9.83	18.61	15.75	15.18	16.18
3 days	9.20	6.12	11.63	11.69	10.78	12.37
4 days	7.46	4.93	9.46	10.32	9.97	10.59
5 days	5.59	4.08	6.78	7.51	6.95	7.93
6 days	5.32	3.41	6.82	7.71	7.20	8.09
7 days	5.35	4.13	6.31	7.51	7.50	7.52
$>$ 7 days	7.21	4.54	9.32	10.37	10.10	10.57
Fixed Cost	1.84	0.82	2.64	2.39	2.15	2.57

Note: We compute the proportion of gas stations for which the hypothesis $\gamma_{s_i}+\gamma_{S_i} > 0$ is not rejected at a 1%-level. For the time-varying threshold models, we compute this test duration by duration and for the fixed (S, s) model we test the hypothesis $\gamma_s+\gamma_S > 0$.

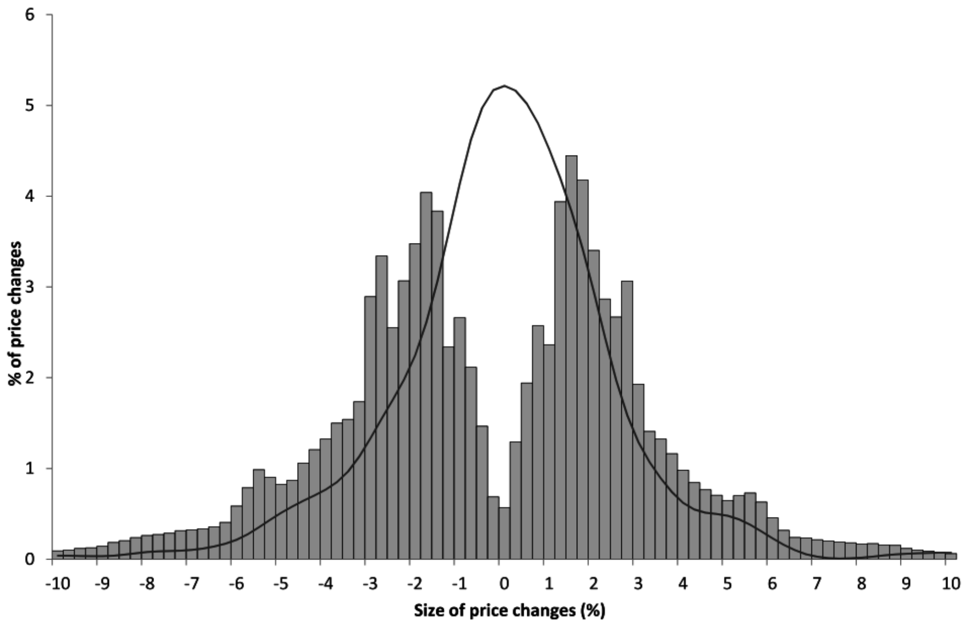
Table 2.6 : Proportion (in %) of gas stations with significant downward asymmetric reaction (Wald test $\gamma_s+\gamma_S > 0$)



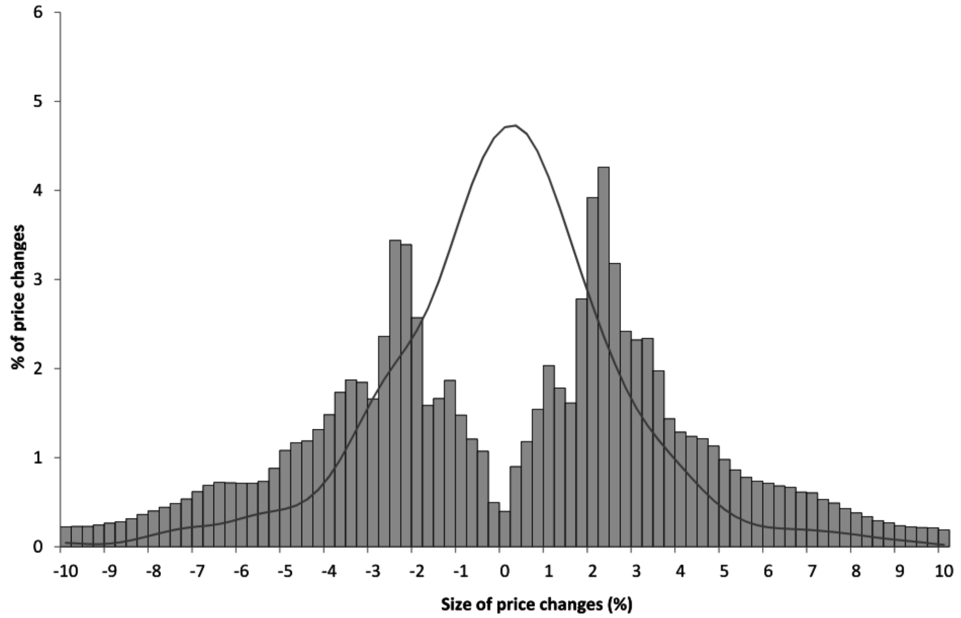
Note. Solid black line for all gas stations, dashed grey line for supermarkets, dashed black line for other gas stations. Censored price paths are excluded.

Figure 2.1 : Hazard functions for gasoline price changes

a) Diesel

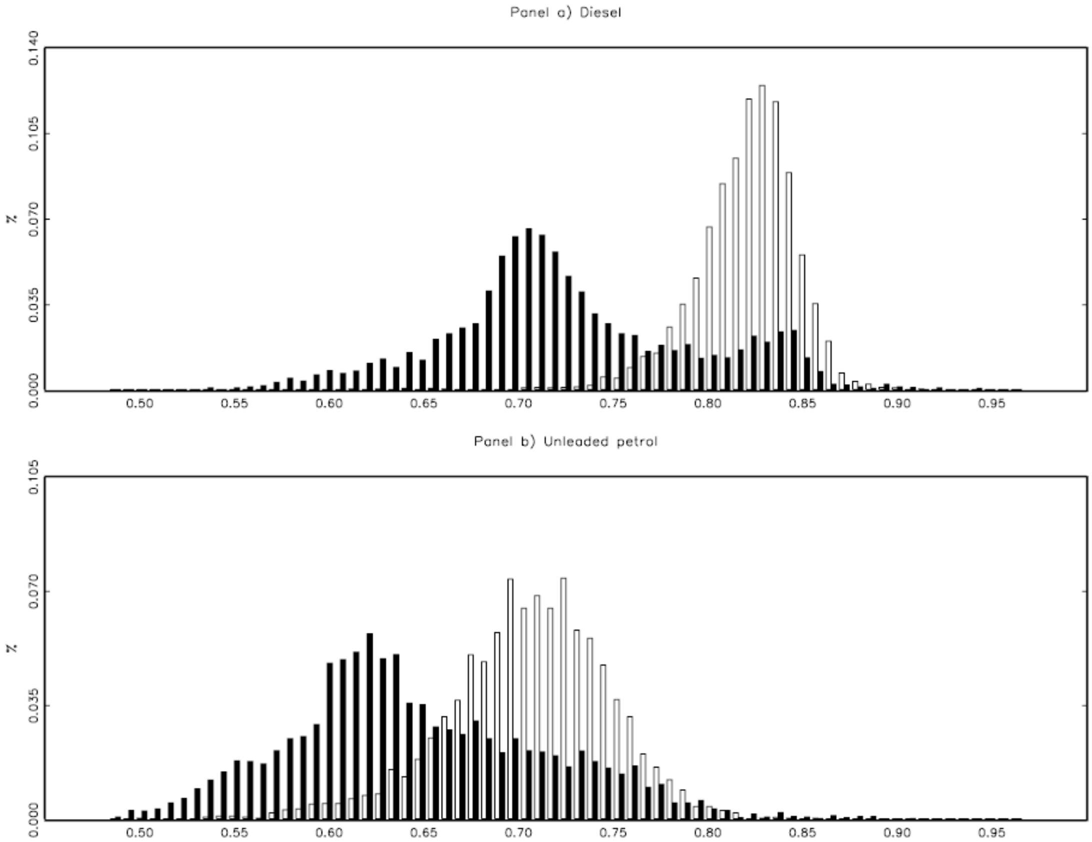


b) Unleaded petrol



Note. Observations are individual non-zero price changes. Grey bars represent the distribution of retail price changes (in %) and grey lines are the kernel density estimators for distributions of Rotterdam price changes (in %). Retail price changes are calculated using prices excluding taxes. Size of price changes are divided by the estimated firm-specific share of wholesale prices into retail prices (β) (see Table 2.2) to control for differences in the cost structure of the different gas stations.

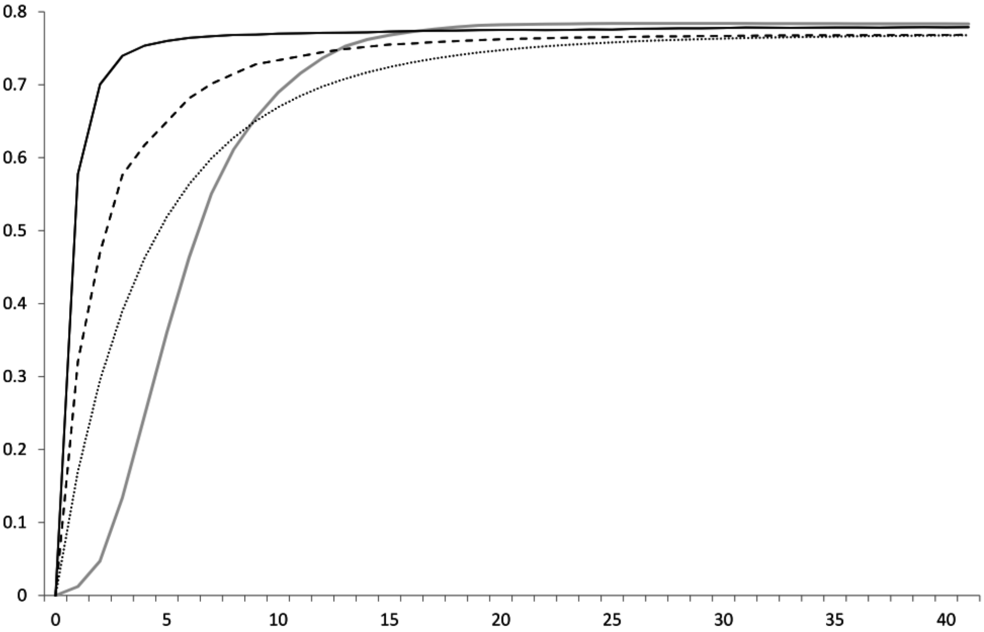
Figure 2.2 : Distributions of individual retail price changes (in %)



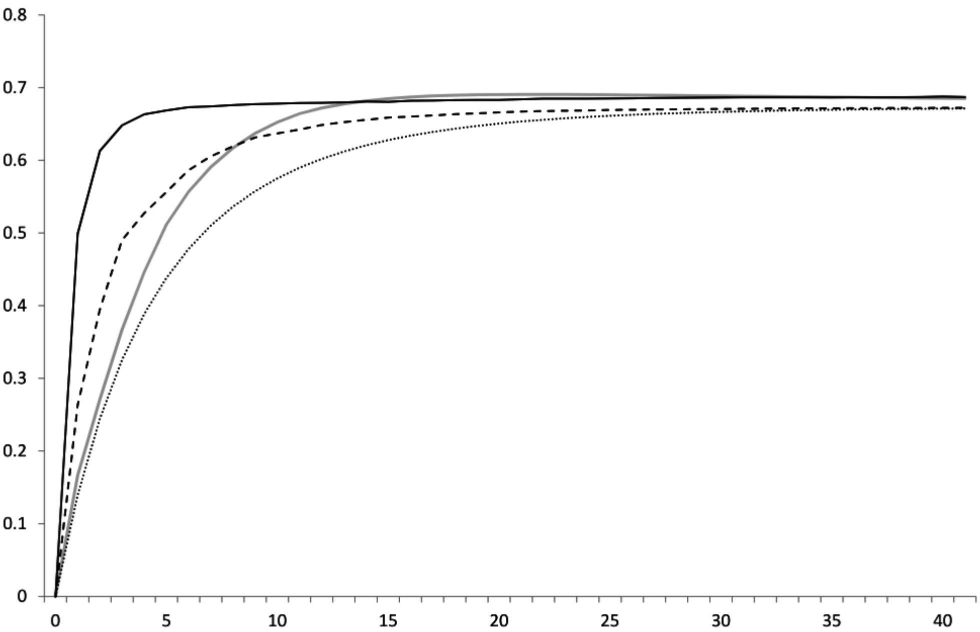
Note: Each observation is a value of β estimated for an individual gas station. White bars are for prices in supermarkets and black bars for prices in other gas stations.

Figure 2.3 : Distribution of β_i (degree of pass-through) using the time-varying threshold model

a) Diesel

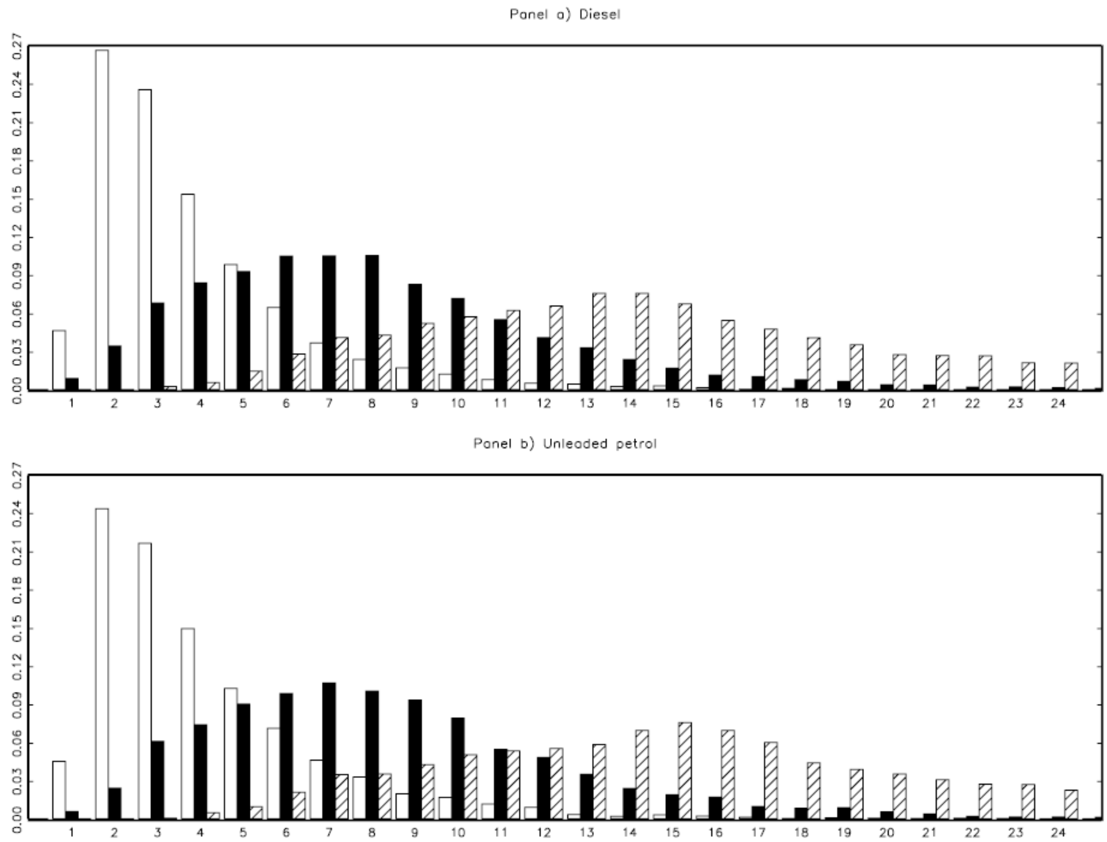


b) Unleaded petrol



Note: dashed line for the time-varying (S, s) model, short dashed line for the Calvo model, solid dark line for the fixed adjustment cost model and grey line for the IRF obtained using an error correction model.

Figure 2.4 : Aggregate responses of gasoline inflation to a 1%-shock on Rotterdam price for the fixed $(\mathbf{S}, \mathbf{s})$ model, Calvo model and time-varying $(\mathbf{S}, \mathbf{s})$ model



Note: Each observation is a value of the delay to achieve 95% of the full adjustment estimated for every individual gas station. White bars are for durations obtained in the fixed menu cost model, black bars for the time-varying (S, s) model and dashed bars for the Calvo model.

Figure 2.5 : Distributions of delays (in days) before a full price adjustment to a 1%-shock on Rotterdam price assuming a fixed (S,s), Calvo or time-varying (S,s) models

Internships, major choices and labor market outcomes of French Grandes Ecoles graduates

Chapter 3

Internships, major choices and labor market outcomes of French "Grandes Ecoles" graduates

We study the role of internships in students' orientation and career beginning. On-the-job experiences accumulated during schooling and before specialization, via internships, give to students some information they may exploit to choose their majors, and to employers some insights on the skills of potential future co-workers. In recent years, more and more French "Grandes Ecoles" students made the choice to undertake an optional full year of internships between the two years of their Master degree. We use this selection process to test potential effects of internships on subsequent major choice and labor market outcomes, with an original survey about the integration of graduates from a French "Grande Ecole" matched with students' attainments obtained during the schooling program. A full year of internships is less valued by employers than a real year of professional experience. Internships are perceived as a signal of ability by employers more than a return to experience and a gain in human capital. A year of internships improves the ability to find a job faster. It is possible to refine the major choices when several internships in different areas are made. Public policies implemented, which ban unpaid internships or more than six months, appear consistent with our results.

3.1 Introduction

Whereas internships have always represented one of the main foundations of vocational and professional formations, e.g. apprenticeship, their occurrence has recently increased in general higher education in France, far above what is required to graduate. In particular, choosing to undertake a full year of internships between the two years of a Master degree, which is not a requirement of the Master program, has greatly expanded in the recent years in some French "Grandes Ecoles". Beyond formal education, on-the-job experiences accumulated during schooling, via internships, increase the student operational and practical skills in a professional setting.

These are also privileged periods during which students gather information about the characteristics of a given job, firm, industry, and their preferences for them. In parallel, employers gather information about a potential future co-worker that they may employ at the end of his/her studies. So, internships are likely to first influence students' own decisions about their future field of specialization and future career, and second, their wages and job conditions.

However, when comparing labor market outcomes for students who undertake or not a full year of internships, the effects appear much more mixed. In forty five engineering schools[1] for which this year of internship is common but not compulsory, we found no effect on satisfaction at work and only a small increase of wage with a 3% premium. There are 2% more likely to find a job before graduating (50% versus 48%). These low benefits are unlikely to justify entering the labor market one year later and the associated financial loss. It is not possible to understand the choice of taking-up a one year internship with these aggregated comparisons. Using detailed data on labor market outcomes matched with students' attainments and characteristics for one "Grande Ecole", we study the effect of internships undertaken during general higher education studies, on student choices about their specialization, and on their employability and wages after graduation. This analysis is important for two main reasons. While many political debates were held on internships in France, little is known about their potential effects. In France, each academic background includes a number of mandatory internships but it is therefore not possible to observe a counterfactual. In the French engineering schools, students can make the choice to undertake an optional full year of internship between the two years of the Master. This selection is used to measure the impact of internships. The second reason is that the special case of a full year internship helps to understand how are perceived the first work experience and internships by firms, whether they are seen as a signal or as a gain of human capital, which is a sensitive issue in economics.

Internships were largely studied in sciences of education and psychology. Taylor (1988) underlines that internships improve employment opportunities, and in a more partial way, knowledge of interest and work values. In management studies, Gerken et al. (2010), Narayanan et al. (2010) and Gault et al. (2000) focused on internships in business schools. To design effectiveness of internships, three actors are involved: firms, universities and students. For firms, internships may be help for special projects and a signal for future employees. For universities, it engenders a stronger connection with the corporate world and a better reputation. For students, this allows less time to get first job, a gain in wage, knowledge of interest and work values. Those results are mainly based on qualitative and

[1] These figures come from surveys "integration of graduates" described below.

descriptive analysis with few data. The effects on wages and employability are so not clear. Gault et al. (2000) surveyed 223 students graduating during 1994-1996 and found positive effects on wages (9% higher), job search duration (less of more than 2 months) and satisfaction at work. Sandvig (2005) found a 10% returns of internships during boom period but 27% during poor period from 126 graduates during the years 1997-2003. But other studies (as Fang and Lee, 2005) found no significant effects. These parsimonious and inconsistent results can come from a lack of theoretical background of these studies. In economics, Saniter and Sandler (2014) find a positive effect on wages in the german labor market, using IV analysis with the introduction and abolishment of mandatory internships. We are not aware of other papers studying workplace training periods outcomes for students in higher education/ university programs.[2] Most of the literature on youth training in the workplace focuses indeed on apprenticeship, that is, fully designed programs alternating workplace based training and formal education periods[3], see Wolter-Ryan (2011) for a complete recent review and the seminal work of Becker (1962)[4]. The main concern of these studies is to account for the endogenous selection of individuals between apprenticeship and full-time formal vocational education. Our approach is close, as we consider the optional full year as an endogenous selection process too.

A closely related literature concerns the determinants of college majors and associated returns (see Altonji et al. (2012) for a complete review and a general but mainly theoretical approach of sequential-decision schooling models). Edu-

[2] Stevens (1994) considers apprenticeship in the British engineering industry but from the firm point of view.

[3] Wolter-Ryan (2011) define "apprenticeship as programs that comprise both work-based training and formal education, in most countries at upper-secondary level, and lead to a qualification in an intermediate skill, not just to semiskilled labor."

[4] The questions addressed include where to develop specific-occupation skills in the workplace or at school, the firm behavior in entering and financing an apprenticeship system, the outcomes for apprentices. Answers greatly depend on the country-level institutional context. Some other papers compare secondary education apprenticeship to full-time formal vocational high school in terms of labor market outcomes, especially risk of unemployment. In France, Bonnal el al. (2003) show a positive impact of apprenticeship versus full-formal vocational high school on getting rapidly a first job, and Alet and Bonnal (2011), a positive impact on subsequent schooling outcomes. Studies are more numerous in German speaking countries, in which the system of apprenticeship is widespread. Developing a full structural approach, Adda et al. (2010) find an increase in wages and an accelerated experience gain profile in Germany. Exploiting apprentice vacancies, Parey (2009) finds lower risk of unemployment for apprentices fading out with time. Exploiting small firms closures and consequent exogenous variations in apprenticeship durations, Fersterer el al. (2008) find a 5% pay increase of apprenticeship. A parallel literature focuses on the advantages of developing some occupation-specific skills, through some vocational courses, in general high school, to increase students' cognition and motivation, see, e.g., Altonji (1995), Mane (1999) and Bishop and Mane (2004).

cation decisions are made sequentially and outcomes are uncertain, see Altonji (1993), Heckman et al. (2003). Preferences, beliefs, abilities, expected earnings and non-pecuniary employment conditions revealed along the schooling process, are expected to affect students' choices at the next step of the schooling process, and especially when they choose their specialization majors. Arcidiacono (2004) analyzes the role of ability in major choices, distinguishing verbal and maths abilities. Stinebrickner (2003) shows that students change majors after observing their grade performance in the first years. For France, Beffy et al. (2012) find very low, though significant, elasticity of college major choice to expected earnings and conclude that non-pecuniary factors are the key determinants. For the U.S., Arcidiacono et al. (2012) show that uncertainty about wage expectations generate inefficient decisions. Optional internships may provide the occasion for students to revise their beliefs about their preferences, their occupation-specific abilities, their expected earnings and some non-pecuniary employment conditions. They consequently help to reduce the uncertainty of related decisions.

We analyze the effect of a full year of internships on student's subsequent choice of specialization, and labor market outcomes after graduation, *i.e.* unemployment risk, wages, satisfaction at work in a French Grande Ecole. Our data come from an original survey about the labor market integration of graduates matched with the students' attainments during their schooling. This enables us to account for student's abilities. We consider that schooling decisions are sequential. The decision of undertaking an optional full year of internships is indeed done before the major choice one. During their first years of schooling (or other experience), students receive information about their abilities and preferences. At the end of the first year of the Master degree, they decide whether or not they would do an optional year of internships between the two years of their Master degree. At the beginning of the second year, they choose a major after having or not done a year of internships the year before. Graduates finally enter the labor market or decide to do a PhD (or other studies).

The sequential schooling decisions framework highlights several potential effects of internships, that we test using reduced-forms models. Students who undertook a full year of internships between the 2nd and the 3rd years of their program may have higher wages or reduce their job search duration at their labor market entry for several reasons. 1) Students may have delayed their entry into the labor market in times of economic slowdowns. This effect is nevertheless expected to be weak (Gaini et al., 2012). 2) It may be a signal to employers. With similar characteristics and in the absence of other available information, firms are likely to choose graduates who completed internships, as the individual ability is not commonly observed by the employer. 3) The experience accumulated as an intern may be valued by employers, which can be compared to the return to experience.

4) Doing a full year of internships may improve opportunity of careers (a first job effect) and the returns to experience. Hence, we can compare the return to experience with and without doing a full year of internship. 5) Students may have improved their orientation, and choose their major specialization in a much precise accordance with their preferences and abilities. There will be a better matching between graduates and their job. Students who did a full year of internships are likely to be more satisfied of their job.

Empirical results indicate that the gross wage return of a full year of internships is higher than 60 % of the return of one year of work experience but is lower than a real year of professional experience. The effect is mainly determined by the revelation of abilities. Internships are perceived as a signal of ability by employers more than a return to experience and a gain in human capital. The econometric analysis takes into account the ability bias and the endogeneity of the choice of making a full year of internship. With IV analysis, the wage return of a full year of internships appears higher than a real year of professional experience, but this result has only a local interpretation. This year of internship improves the ability to find a job faster. It has a small positive effect on job satisfaction (but only significant with IV analysis and monetary component). It is possible to refine the major choices when several internships in different areas are made. Public policies implemented, which ban unpaid internships or more than six months, appear consistent with our results. Compensation for internships of one third of potential wage output seems necessary to financially equalize the loss. Perform a full year internship should help to refine these major choices. It therefore seems logical to ban internships more than 6 months, which can be considered as a real job and not as an internship.

The layout of the paper is as follows. Section 2 contains contextual elements. It describes the French higher education system with Grandes Ecoles and universities and the place of internships. It also describes the schooling program of the Grande Ecole studied. Section 3 presents the data used. Descriptive statistics are reported in section 4. Section 5 presents the reduced-form estimation strategy and results. Section 6 concludes.

3.2 Institutional context

3.2.1 Overview of French higher education system

Universities and Grandes Ecoles. (cf. figure 3.1) Two types of French higher education institutions can deliver graduate degrees: the universities, which are large state-financed structures, and the "Grandes Ecoles", which are smaller either private or state-financed structures. The "Grandes Ecoles" contain "engineering schools" or "business schools". Their particularity is that they are selective. To enter one of them, students prepare for a contest entry during 2 years. This competitive entry and the ranks of admitted students contribute a lot to the reputation of the Grande Ecole and to the delivered degrees. French Grandes Ecoles students represent about 15% of students in higher education in 2010 (about 8% in 1980). Some of the "Grandes Ecoles" are quite generalists, others are more specialized. Their schooling programs usually last 3 years, leading to a master degree or the grade of Engineer, which is in France equivalent to a master degree. Event though there is a lot of variability making comparison difficult, labor market outcomes of Grandes Ecoles students are in average better than those of students from university. In 2010, the employment rate and the average wages six months after graduation for students from "Grandes Ecoles" are 84% and 35.800 euros (bonus included). The selection at the entrance is one argument in favor of these better labor market outcomes; the fact that Grandes Ecoles programs contain on-the-job training periods is another. A Grande Ecole curriculum is usually composed of periods of formal education and compulsory periods of internships. The load of each, and the sequence in which they intervene depend on the particular program. Even though curriculum contains some work-place experience periods, Grandes Ecole schooling programs do not correspond to some vocational training, neither to apprenticeship, which corresponds most often to certifications below the bachelor's degree.[5]

In some Grandes Ecoles and especially in the one we consider here, a student may choose to undertake internships for a longer duration than the one compulsory to graduate. Our aim is to study the schooling and labor market outcomes of such a choice.

Internships in the French higher education system. Internships are compulsory part of many training programs, but their duration and content really depend on the program, and the type of program. Internships are widespread in Grandes Ecoles programs as some of them are constitutive of the programs. Programs in these schools are labeled by external commissions, which check and insure that the program fits different requirements including a certain number of weeks of internships. Those labels are very important for schools as they entail the real recognition of a certain level of education and equivalences. So all schools do their best for their programs to satisfy those requirements. Most Grandes Ecoles contain so an "internships office", some administrative staff who receive, find, check and

[5]The apprenticeship system has been greatly developed in France in recent years, although it remains low in the higher education: apprentices accounted for 0.9% of students in higher education in 1995, 2.4% in 2000 and 4.8% in 2010. The majority of these apprentices are below the bachelor's degree (90% in 1995, 75% in 2010).

screen internship offers sent by firms and transmit them to the students. In 2010, while almost all students from "Grandes Ecoles" undertook at least an internship (there is usually a compulsory 6-months internship at the end of the last year of the program), they were only 61% to undertake an internship in the last year of the Master in a university. 97% of students in "Grandes Ecoles" were paid for their last year internships versus 76% for university students of equivalent education level. 57% earned more than 600 euros a month versus 29% for university ones.

In the recent years, a policy debate in France has accompanied a change in legislation about internships minimum remuneration and their duration. It has discussed the drawbacks of unpaid internships. First, as stated by Richmond (2010) for the UK, students with lower financial resources would have less opportunities to undertake unpaid internships than their counterparts, which may in turn deteriorate their entry conditions on the labor market. Second, unpaid internships may also be viewed as unfair competition with older workers for whom minimum wage regulation applies. The Law for equality of opportunity of March 2006, implemented in February 2008, forbids unpaid internships longer than 3 months. It has been extended in November 2009 to forbid unpaid internships lasting longer than 2 months. Internships have to be paid at least 400 euros per month, which represents a third of the minimum wage. The Cherpion Law (2011) and the Fioraso Law (2014) prohibits (paid) internships that last more than 6 months and strengthens the rights for interns but the laws are not yet implemented. In contrast, in the U.K. or in the U.S., there is no legal obligation for firms to pay an internship that lasts less than 1 year if it is required as part of students studies. However, if interns satisfy the legal definition of 'workers' in the United Kingdom (i.e. they do work for which others employees are paid) or some equivalent requirements defined from the Fair Labor Standards Act in the U.S., they should normally be paid at least the minimum wage.

3.2.2 French Grande Ecole schooling program

We now describe more precisely the Grande Ecole (ENSAE-ParisTech, Paris Graduate School on Economics, Statistics and Finance), on which rely the data, and its schooling program. The school is a relatively small Grande Ecole. Around 150 students graduate each year. Students who succeed the examination entrance, enter the first year of the curriculum. This first year differs slightly between students upon their previous studies. Those who studied economics before, are taught more maths and those who studied maths more economics. Some other students, selected on records, directly enter the second year of the program. The second year of the program is composed of common and compulsory general courses in Economics, Statistics, and Humanities and students choose a minor in Economics or Applied Mathematics. The minor choice is made by the student upon his/her preferences and expected abilities. There are no quota in registration in a minor, neither restrictions based upon previous specialization. At the end of the 2nd year, students choose either to undertake a compulsory 3-month internship and to enter the 3rd year of the program, or to undertake internships for a complete year. If so, they come back one year later in the 3rd year of the program, and will graduate one year later. There are seven majors in 3rd year, which, due to the small number of students per major, we will gather in two main fields, Finance/Actuarial Sciences and Economics/Statistics/Social sciences. Those are equitably distributed with 54% of students in the Finance oriented majors and 46% in the other ones. Students choose the 3rd-year major at the beginning of the 3rd year. Again, there is no quota, whatever her previous curriculum is, a student enters the major he chooses.

3.3 Data

We use 5 surveys "integration of graduates" of the school (2009 to 2013), which we match with the administrative data of student schooling attainment.[6] The survey " integration of graduates" is organized every year in all French "Grandes Ecoles", which represent approximately 200 schools (including both "engineering schools" and "business schools") and 40,000 students. The survey focuses on the labor market participation and job conditions at the time of the survey, wage and job satisfaction. It questions exhaustively the two (for surveys 2009 and 2010) or three (for surveys 2011, 2012 and 2013) promotions who graduated during the year, the year before or two year before. A student is then interviewed 6 months, 18 months, and 30 months after graduation. Each year the questions are identical using the same definitions.

We only use data on students who join the program in the first or the second year and then have the possibility to choose to undertake optional internships. So we exclude students who joined the program directly in the third year (around 23% of a graduating promotion). We also exclude some students who were registered in double degree programs through conventions with other institutions and consequently replace the 3rd year of the program by a year of education of an equivalent level in another institution (around 12%). We also exclude the very few students who repeated a grade (less than 4%). Finally, we exclude the 14% of civil servants trained by this school. In France, civil servants are selected before

[6] The survey has been declared to the French "Commission nationale de l'informatique et des libertés" (declaration number 1604776) with the precision that the survey data can be matched with data coming from the administrative education software ("pamplemousse"). All data are anonymous.

their entry in the program and are chosen by a public contest. They are paid during their studies. They were excluded as they cannot do an optional full year of internship and have already a job with a non-negotiated fixed salary after the program.

The sample contains both students admitted in the first year or in the second year of the program. Students enter the first year of the curriculum after passing a competitive examination. Students enter the second year with a selection process based on records and past studies. These students are older of one year on average. All students have a high background in economics or mathematics before the admission. At their entrance in the school, they take advanced courses in economics and mathematics, both theoretical and applied, in order to homogenize their skills and knowledge. This part of the program insures that students are homogeneous even if they arrive with different background. In France, each "Grande Ecole" is similar to a brand valuated by firms, for which a shadow "ranking" is obtained according to the difficulties to be admitted.

Schooling attainments during curriculum come from the administrative data (the education management software used), and are matched with the survey data. These data also contain student's choices of 3rd year majors, 2nd year minors; and some basic information on previous studies and on social-demographic characteristics: parental background and location, social scholarships, age, gender, etc... Students do not take the same courses during the 3rd year of the program, their scores are not comparable. So we rely on their achievements during the 2nd year of the program. For each student, we have also computed an inverse ranking based only on the schooling attainments upon common courses of the 2nd year of the program. The best student is ranked 100 and the last one 0. These rankings can be interpreted as quantiles. It makes them comparable to GPA and between years. We have defined a global ranking based on the average score during the year (all courses including group projects, and after the catch-up examinations), and specific rankings based on the written individual evaluation in mathematics (statistics and time series), and economics (micro- and macro-economics). Courses included in the specific rankings have been selected using a principal component analysis.

Finally, our dataset is composed of 452 students who graduated from 2007 to 2012. 39% are women. 65% were admitted in the first year, 35% in the second year (i.e. the first year of a Master degree). The overall response rate is around 71%, lowering with the time after graduation: 82% for the more recent graduating promotion, versus 65% for the others. This overall response rate is between 74% and 78% for surveys 2009 to 2012, but only 57% in 2013. The comparison between the survey response rate and the actual numbers of students per 3rd year major confirms that the sample of respondents is representative. Student who majored in Economics/Statistics/Social Sciences respond more frequently than those who majored in Finance (81% versus 64%). An explanatory model of non-response (logistic model) confirms these observations (cf. appendix). With the exception of the third year major and the number of years after graduation, the other observable characteristics (gender, nationality, age, year of admission, schooling attainment, full year of internships...) do not explain non-response. Hence, we have created weights based on this model as what is usually done in sampling theory. We computed two weights to measure the probability that the observation is included in the sample. Unequal weights are directly based on the probability of the logistic model. Stratified weights are equal weights in the groups defined by the year of graduation, the major and the number of years after graduation. This is a post-stratification weighting scheme based on homogeneity groups of responses. We estimated weighted regressions of the wages equation and compared weighted and unweighted regressions (cf. appendix). Results are close. We conclude that the use of weights is not required, which is consistent with the fact that explanatory variables of non-response are included in our models. In this case, the selection process due to attrition is already taken into account (Solon, Haider and Wooldridge 2013). Partial non-response is very low,[7] and responses to different survey quite consistent.[8] Job satisfaction was not surveyed in 2009.

Variables of interest. The full year of internships and the 3rd year major variables come from the administrative schooling management software data, and labor market outcome variables from the survey data. The wage variable corresponds to the total annual real wages including bonuses. In average 62% of people report bonuses, for approximately 12% of their annual total remuneration. The level of bonuses increases with experience. Job satisfaction is measured by several indexes ranging from 1 (worst) to 5 (best): satisfaction of the relations with colleagues, of the location of the firm, of the remuneration, of the level of autonomy, and of working conditions in general.

We have included job characteristics in the analysis, such as working abroad (without distinguishing between different countries), working in the public sector

[7] equal for example to 6% for duration of the first job search and to 10% for wages.

[8] Students are surveyed about their current job. However, there are additional questions for those for whom this is not the first job. So, depending on the fact that he/she changes or not of job, we either have repetitive information on the same job or information up to 4 different jobs he/she held. 78 students (17%) did not give any information, 157 (35%) one response, 83 (18%) two responses, 99 (22%) three responses and 35 (8%) four responses. So some of them answered several times at the same questions. We analyzed the consistency of responses and memory bias. Concerning the first job search duration, answers between surveys are consistent. They differ in less than 10% of the cases and in such cases for minimal differences: answering "between 2 and 4 months" instead of "between 4 and 6 months", for example.

or whether the job is a fixed term contract. Two main characteristics observed in the survey have not been included, the firm size and the business sector because responses were not exploitable. The firm size is misinformed for multinational companies with a lot of institutions (as banks or insurances) where the majority of graduates work (respondents did not know if the question was related to the size of the firm or the subsidiary entity). One might think that this omitted variable has little impact, because the number of graduates working in small innovative firms is low. For the business sector, 18% of students studying in a major work finally in an other employment sector (a student majoring in economics who works in the banking sector for example). As this figure is low and the distinction between close professional sectors difficult, we assume the major choice is associated to a choice of employment sector. Some other features are unobserved. We do not observe how employers define their wage proposal. The bargaining power of graduates can be reduced by pay scales based on university or "Grande Ecole" followed, which may explain a lower effect of internship. We do not know if the graduate works in the firm where he did an internship because the name of the firm is misinformed. We cannot therefore exclude the hypothesis that the effect of internship is primarily obtained for these students and is different for graduates working in a company different from the one in which they did an internship.

3.4 Descriptive statistics

Figure 3.2 reports the increasing share of students choosing to undertake an optional full-year of internships between the 2nd and the 3rd year of the curriculum. 23% of those who graduated in 2007 used this option and 62% of those who graduate in 2011. 52% of students admitted in the first year completed a full year of internships (32% in 2007 and 76% in 2011) and only 17% of those admitted in the second year did so (almost none before 2010 and 38% in 2011).

3.4.1 Major choice

Tables 3.1 and 3.2 compare student's abilities, given by rankings, and major choices depending on the fact that students did or not a full year of internships. As noted before, 54% of the students major in Finance and 46% in Economics/Statistics/Social Sciences. More students who did a full year of internships chose to major in Finance during the 3rd year of the program (61% of cases vs. 49%) whereas there are no apparent relation between 2nd-year minors and the choice of undertaking or not a full year of internships.

Students who chose to undertake a full year of internships seem to have lower

2nd year schooling attainments in average than others. Their average inverse ranking is 46.1 versus 53.0 for those who did not. Note that we report an inverse ranking to make rankings comparable to GPA. So the best student is ranked 100 and the last one 1. The average ranking is lower for all 2nd-year minors (5.7 ranks for mathematics; 9.6 ranks for economics) and compulsory courses. Students choose their specialization in line with their skills. Those who choose a minor in economics have better ranking in economics courses than in mathematics courses (54.4 versus 41.5), and vice versa for those with a minor in mathematics (56.9 average ranking for mathematics courses versus 51.9 for economic courses). Students do not take the same courses during the 3rd year of the program, their scores are not comparable.

3.4.2 Full year internship

Table 3.3 describes the features of internships done between the two years of the Master. 56% of students doing a full year internship makes two internships, compared to 37% one and 8% three. Doing an internship in Finance is more frequent for students making a full year internship (64% vs. 43%). 14% of students make internships in Finance and in other field. The topics of the internships are linked with minor and major choices.

3.4.3 Labor market outcomes

Table 3.4 reports the average labor market outcomes of graduates whether or not they undertook a full-year of internships. Six months after graduating, 4% of graduates who did a full year of internships were looking for a job compared to 10% of those who did not. Student who chose to complete an optional year of internships work also more often, 73% including volunteering versus 55%. Students who did not continued more often their studies (19% of cases vs. 11%). In average over the 3 promotions interviewed in each survey, students who did a full year of internships earn 13% more than those who did not (53.800 euros bonuses included versus 47.600 euros). Wages rose sharply in the early years of the career, 13% increase between the first and second year, and 7% between the second and third year. The evolution is nevertheless not the same whether or not graduates undertook a full-year of internships. The increases are 28% (between the first and second year) and null (between the second year and the third year) for graduates who did a full year of internships while they are 7% and 13% for others. This may be due to a later entry into professional life. Students majoring in Finance earn in average 12% more. There is no effect (or only a small one) of a full year of internships on the wage differential between specialization fields six months after graduation. These figures suggest that the on-the-job experience acquired during the year of internships is valuated by the employers. Concerning satisfaction at work, the results are less obvious. Internships seem to be associated with a better satisfaction (for all components) but the difference is small, of the order of 0.1 on a scale of 5 (for example with 100 students, 10 choose a score of 5 instead of a score of 4). There is more satisfaction for non-monetary components (4.2 on average for autonomy, working conditions and environment) than for wages (3.8 on average). If we compare averages between responses of students with and without a full year internship, we found a significant effect (at 10%) only for autonomy at work, with a difference of 0.19 point (i.e. for 100 workers, 19 would answer 5 instead of 4 for example). There is a non-significant positive difference for wages and the working environment. There is no difference for the working conditions. Table 3.5 reports the distribution of the job satisfaction categories for the 4 components, wages, autonomy and responsibility at work, the working conditions and the working environment (relations with colleagues). 5 response items are possible, from 1 (the worst one) to 5 (the best one). It is an ordered scale. However, responses 1 or 2 are rare. There are 3% of responses 1 for wages, and 1% for the non-monetary variables. There are 9% of responses 2 for wages, 6% for autonomy and the working conditions, and 2% for the working environment. The response item 4 seems the more stable, with 36% of response for wage, autonomy and the working conditions, and 31% for the working environment. The highest satisfaction (5) is obtained for the working environment (57%), then the working conditions (44%), autonomy (38%) and finally wages (28%). We note low differences between distributions whether students take or not a full year internship, with a significant difference only for the response item 2 of autonomy.

Table 3.6 reports the distribution of the first job search duration. 63% of the graduates who did a full year of internships found a job before graduating compared to 53% of those who did not. They are also less likely to have a search period lasting more than 6 months, 4% versus 10%. 28% of the students declare that internships - including full-year, part-time and other compulsory ones - helped them to find a job. Full-year of internships are also associated to longer job periods after graduation. The first job - when ended - was also in average longer - of 3 months- for the graduates who did a full year of internships. These figures suggest a better matching between the graduates who chose to undertake a full year of internships and the employers occurred. 18% of students studying in a major work finally in an other employment sector (a student majoring in economics who works in the banking sector for example). As this figure is low and the distinction between close professional sectors difficult, we assume the major choice is associated to a choice of employment sector.

3.5 Internships, major choice, and labor market outcomes: reduced-form results

3.5.1 Estimation strategy

Our goal is to test the potential effects of internships. It is not to understand the complete learning process of preferences associated with doing a full year of internship. As the labor market can be considered as an absorbing state (you cannot return to school after entering in the labor market), the effects of undertaking an optional complete year of internships on labor market outcomes after graduation can be tested using a Mincer type model for wages (and close models for duration of job search and satisfaction at work) and controlling for the endogenous selection. For the effects on subsequent schooling decisions (major and full year internship), Rust (1987) assumptions are the conditional independence (the unobserved factors at t have no effects one period later if you control by the decision variable at $t+1$) and that unobserved heterogeneity is a random effect. It means that there is no omitted variable and no serial correlation of unobserved preferences. The subsequent schooling decisions (major and full year internship) can then be then estimated with a classical probit model. These assumptions are strong. One may use an assumption of a type-specific unobserved heterogeneity to estimate a full structural model (Arcidiacono and Ellickson, 2010, Beffy et al. 2012), but it will be necessary to add some assumptions to define the learning process and to have more data. We so limit our analysis for the subsequent schooling decisions (major and full year internship) to this reduced-form approach.

3.5.2 Internships wages outcomes

The choice of the number of years of schooling has already been done by the student at the entry of the "Grande Ecole" (more than 5 years depending on whether the student goes on with a PhD or not). The failure rate is very low in French "Grandes Ecoles" and the degree is obtained 5 years after high school graduation. The wage of a graduate on the labor market is defined with a Mincer type analysis[9]

[9] A huge literature has focused on the returns to education, with estimates based on the traditional Mincer equation, which explains wages by years of schooling, experience and other control factors. Due to the potential endogeneity of the schooling decision (as the student ability is not observed), IV estimators have been proposed with instruments mainly based on compulsory schooling laws. This approach has been debated (see Björklund and Kjellström (2002), Dickson and Harmon (2011) for a brief review). Heckman et al. (2003) analyze the assumptions of the classical Mincer model and conclude that derived returns to schooling estimates are not valid. Indeed, this static model neglects the major determinants of schooling decision such as the uncertainty about the future wages, the cost of schooling, or the varia-

$$\log\left(W_{mt}\right) = \alpha_{0mt} + \alpha_{1mt} \cdot d + \alpha_{2m} A^m + \alpha_{3m} \cdot d \cdot A^m + \alpha_{4m} \mathrm{exp_t} + \alpha_{5m} \cdot d \cdot exp_t + X_t \beta + \varepsilon_{mt}$$

with t, the year, m, the third year major, $\log\left(W_{mt}\right)$ the log of total wages - bonuses included, d, a dummy equal to one if the student did a full year of internships, A, the student ability, which is major specific (a student may choose a major where his/her ability is higher) and can be split into a general and a specific ability, exp the experience in the labor market (a squared term is not necessary for the first years), and X a vector of other characteristics (job characteristics (working abroad, second job since graduation, whether it is in the public sector, whether the job contract is fixed-term or long-term), and student characteristics (gender, nationality, year of graduation). Experience (in years 0, 1 or 2) accounts for the sum of the duration of each job reported in the survey. The year of graduation accounts for effects of economic conditions. Student's ability is measured by their ranking in the second year (the courses are partly common unlike the third year), or their ranking on specific courses (economics and mathematics), which has been defined using a principal component analysis. As robustness check, we also used a potential experience variable.[10] X is assumed to have the same returns for each major and whether or not the student has done a full year of internships. The other variables are major- and internship- specific. The constant term measures the structural differences of wage returns between majors and the gain associated with a full year of internships (to be compared to an additional year of experience).

This reduced-form model entails some predictions that can be tested.

- Waiting for good economic conditions (for duration of job search too): students could delay their entry into the labor market if they think the economy is not favorable. The year of graduation will so have an effect on wages, with $W_{t+1} > W_t$ due to t;
- Signaling to employers (for duration of job search too): with similar characteristics and in the absence of other available information, firms are likely

tion of these returns over the life cycle. Furthermore, the use of IV estimators to address the problem of the omitted ability bias is not powerful, and quite different results occur depending on which instruments are used and which assumptions are made (Heckman and Urzua, 2010). Blundell et al. (2005) compare several methods linked with the evaluation literature: matching, IV, and control function approach. They emphasize the importance of the detailed test score and the family background differences to explain (heterogeneous) schooling returns. Non-monetary benefits of education are generally not taken into account, despite the fact that health and well-being can be improved with a higher education (Heveman and Wolfe, 1984).

[10] The Pearson correlation between the two experience variables is very high, 90%.

to choose graduates who completed internships. The ability of the student A is largely unknown by the firm. Doing an internship so reveals a part of the student ability to the employer, with $\alpha_{2m} = 0$ and $\alpha_{3m} > 0$ in an extreme case where the ability is completely unknown without internship and revealed with internship;

- A cost-benefit analysis between the opportunity cost of entering the labor market one year later, and the wage gain associated with the experience accumulated as an intern: there would be a wage return to a full year of internships. We can so compare the return to experience α_1 and the return to a full year of internship α_4.

- A better return of experience: doing a full year of internships may improve opportunity of careers (a first job effect) and the returns to experience. The returns to experience also depend on first job opportunities, which may differ depending on internships done and majors chosen. Hence, we can compare the return to experience α_5 with and without doing a full year of internship.

Results are reported in table 3.7. Note that this analysis enables one to interpret only correlations. No causal conclusions should be entailed from it. In column 2, we add education characteristics (3rd year major chosen, inverse ranking). In such analyzes, there may be an omitted ability bias. Students have different skills, often unobserved. We add student achievements in the program. We use the inverse ranking of student in 2nd year i.e. 100 for the best student, 0 for the worst, as proxy for ability. In column 3, we separate the overall ability in two specific skills, mathematics and economics. In column 4, we cross the full year internship dummy with each year of professional experience in order to check if the effect could disappear with experience. In column 5, we use a LAD estimator (median regression) to take into account extreme wages. A full year of internships has a positive and significant effect of 6.1-8.6% on wages, which is reduced when we control for extreme wages. A rank increase of general ability by one decile corresponds to a pay increase of 0.8% more, due to the economics skills (as the mathematics skills have an unexpected negative value) with a 12.3% premia. Experience has a positive and significant effect of 10.1% on wages, reduced when we control for extreme wages. Consequently, with a static vision, one may infer that the wage cost-benefit trade-off of undertaking a full year of internships is negative; it would be rewarded less than the year of labor market experience it replaces, around 60%. The students' enthusiasm for it would remain a puzzle. The effect of a full year internship does not disappear with experience. Other job characteristics have expected effects. Fixed-term contracts and working in the public sector entail smaller wages whereas working abroad a 40% premia. Choosing to major in Finance is associated to 8.8-9.5% premia. Finally, over 2007-2012, the year of graduation has no effect.

The effect of a full year of internship or to major in Finance could differ following ability of students. Furthermore, returns to experience could not be the same. We test those assumptions by including interaction effects in our analysis. Results are reported in table 3.8. Columns 1, 2 and 3 report the OLS estimates. Column 1 adds interaction effects between education characteristics (full year of internship), experience and a third year major in Finance. In column 1, the effect of a full year internship would be the same for students majoring in Finance and in other major. Choosing to major in Finance is associated to a non-significant premia. Concerning returns to experience, we note that returns are higher for students majoring in Finance (around 5.4%), and small and non significant for a full year of internship. Concerning returns to ability, a full year of internship would have a positive effect to reveal abilities. In column 2, the effect of general ability is null for students who have not made a full year internship but highly positive and significant for others. In column 3, concerning specific abilities, we note that it reveals mainly the mathematics abilities (there could be an adverse selection effect for students who have not made a full year internship).

In our sequential decision framework, the internship take-up depends on future expectations on wages and non-monetary benefits. Those gains can be explained by several factors, such as heterogeneous preferences, beliefs, risk aversion, or individual abilities. We control our econometrics analysis with schooling attainments, which can be interpreted as a measure of individual ability. However, there may be an omitted part of individual ability, and there may also be some error measurements. Ability for theoretical courses does not say anything about communication skills for example. If the omitted ability is positively correlated with the schooling attainments (i.e. students with better schooling attainments have also a better ability to communicate) and as students who take-up a full year internship have lower schooling results, there would be a downward bias. However as the sign of this correlation is unknown, the direction of the bias is uncertain. Furthermore, if students who did a full year internship were those who have preferences for professional settings and lower preferences for non-monetary benefits, it would interact with monetary and non-monetary benefits. Those students may engage in jobs with better wages and lower working conditions, which may create an upward bias in the wage equation. All in all, the bias may exist but the direction of the bias is largely uncertain.

There are two endogenous variables in the analysis, undertaking or not a full year of internships and majoring in Finance. They depend both on future expectations on wages and non monetary benefits, which can be explained by several factors, such as heterogeneous preferences, beliefs, risk aversion, or individual abilities. In an attempt to control endogeneity for the full year of internships and a major in finance, we conduct an IV analysis. There are two main identifying assumptions behind the IV equations.

The first one is that the instruments do not have to be correlated with the dependent variable (i.e. the exogeneity condition), such as wage, the probability to find a job before graduating or satisfaction at work. Our set of instruments is the student age at the end of the 2nd year of the program, the year of admission, a year of graduation higher than 2010 and the minor choice in 2nd year. Our assumption is these variables are not correlated with wage, the probability to find a job before graduating (except for the year of graduation) or satisfaction at work. In France, each "Grande Ecole" is similar to a brand valuated by firms, for which a shadow "ranking" is obtained according to the difficulties to be admitted. We may assume that, as this school is highly selective, that employability and wages do not depend of past experience such as being graduate from an other university for example. The student age and the year of admission would be thus valid instruments for undertaking a full year of internship. Similarly one may think that firms consider mainly major choices and the "Grande Ecole" standing to fix wages or to hire an employee. The minor choice would be thus uncorrelated with wages and be a valid instrument to explain the major choice. Finally we may think that the economic situation primarily affects employability but not wages which are considered rigid even in a recession. A year of graduation higher than 2010 would be then a valid instrument for wages but not for the probability to find a job before graduating. These assumptions are questionable. If violated, there will be no endogeneity correction with an IV analysis. However, if there are as many instruments as endogenous variables, this exogeneity condition cannot be tested. If there are more instruments than endogenous variables, a Sargan-Hansen test of overidentifying restrictions can be performed, whose the joint null hypothesis is that the instruments are consistent instruments (i.e. the choice of instruments is not invalidated).

The second condition is that the instruments have to explain the endogenous variables (i.e. the rank condition), which is tested in the first stages regressions of 2SLS. The student age at the end of the 2nd year of the program or the year of admission is likely to be negatively correlated with the full year of internships. It will be more expensive for older students to postpone their entry into the labor market. Due to the implementation of laws banning unpaid internships starting in 2008, a year of graduation higher than 2010 (i.e. two years after the law) can be a possible instrument too. The minor choice in 2nd year is a natural instrument for the choice of major. All F-stat are higher than 10. As there are two endogenous variables, a global F-stat is computed who gives the same conclusions. However, we exclude potential instruments because they were found to be weak instruments, such as having a social scholarship or the value index for French banking and insurance equities. With weak instruments, results can be indeed misleading. Due do financial constraints and the fact that undertaking a full year of internships implies postponing labor market entry of one year, having a social scholarship could have been negatively correlated with the full year of internships dummy but not correlated with wages. Furthermore, the choice of undertaking internships during a full year or majoring in finance may depend on the conjuncture in the banking and insurance sector. The value index (base 1 in December 2011) in June for French banking and insurance equities (AXA, Societe Generale, Credit Agricole SA, BNP Paribas) could have been used as alternative instruments.

For IV analysis on wages, we perform an iterative choice of instruments. We begin with the student age and the minor choice, then we add the year of admission, and at last a year of graduation higher than 2010. All tests conclude to the consistency of instruments.

Results are reported in table 3.9 with different instruments, using 2SLS (GMM estimators give consistent results). Columns 3, 4 and 5 are for the model with a dummy for the full year internship and IV analysis. Columns 1 and 2 report the OLS estimations with and without the instruments for comparison. The full year of internships IV estimates are higher than the OLS ones, around 13.0-16.1% but standard errors are often much higher too. Without taking into account the year of admission (columns 3), the effect is not significant. The column (4) with all instruments except year of graduation seems to be the best estimate (no over identification, higher KP F-stat for weak identification). With IV analysis, the wage return of a full year of internships appears higher than a real year of professional experience, but this result has only a local interpretation. All instruments (except for the student age) and endogenous variables are indeed dummies variables. The interpretation will be similar to a Local Average Treatment Effect (LATE), i.e. restricted to a subpopulation. All first stages and the corresponding F-tests are reported in table 3.10. Results are consistent. To be older of one year is associated with a decrease of taking-up a full year internship around 6-7%. Minoring in mathematics is associated with an increase of majoring in finance between 45% and 57%. To be admitted in 2nd year decreases the probability of a full year internship around 30-40% and the probability of majoring in finance around 14-17%. A year of graduation higher than 2010 increases the take-up of internship of 31-36%.

Concerning duration of job search, table 3.11 shows that a full year internship reduces this duration but not by revealing the ability of students (interaction effects are not included but are close to 0 and non significant). With an interval regression, this reduction of job search is estimated less than 2 months. IV analysis with a linear probability model shows that the effect may be underestimated. The year of graduation has no effect on wages but the economic conditions have effects on duration of job search.

3.5.3 Internships satisfaction at work outcomes

If internships enable students to learn about their preferences for a job, an industry, and to adapt in consequence their orientation, one would expect that optional internships would be associated with higher satisfaction once at work. The surveys we use contain several questions on the degree of general satisfaction at work, and satisfaction degrees regarding relations with remuneration, colleague, autonomy in work, and work conditions. We construct an index of non-pecuniary satisfaction by summing the 3 satisfaction variables regarding relations with colleague, autonomy in work, and work conditions. Each satisfaction variable ranges from 1 (worst) to 5 (best). Tables 3.12 and 3.13 report OLS results of wages and non-pecuniary satisfaction on similar characteristics as previously. Experience has a negative effect on satisfaction, which shows that expectations are not fulfilled. Wages are highly correlated with wage satisfaction but not with non-pecuniary satisfaction. Having undertaken a full year of internships is positively related to non-pecuniary satisfaction and negatively to wages satisfaction, but the effects are not significant. An interesting point is that the non-pecuniary satisfaction is very difficult to explain. Unobserved factors play an important role.

An absence of a link between internship and satisfaction may be possible if increasing the satisfaction at work is not a selection criterion for doing an internship as the potential gains in non-monetary utility may have other sources (prestige, altruism or social utility of a job). We may argue that satisfaction at work may be badly anticipated by workers, which is consistent with the negative correlation between experience and satisfaction. However, three critics arise regarding these results. First, tests lack of power. The non-significance may also be due to the sample size. Second, questions are measured with a Likert scale, i.e. an ordered opinion on a subject, which may create a measurement issue. Aggregating the three components of non-monetary satisfaction is valid only if these three components are highly correlated, which is only partly the case with a correlation between 0.34-0.40. Furthermore, the point attributed is a normative choice. We note that the response item 4 corresponds to a median response. What is important is that workers are more satisfied than this median. As the response items 1 or 2, we may think too that they are more penalized situations, and so attribute them lower points. In order to check these assumptions, we performed a disaggregated analysis by estimating a model for each of the three components (autonomy, working conditions and environment). We recoded the variable with lower points for rare responses (-5 for 1, -3 for 2, -1 for 3, 0 for 4, and +1 for 5). We finally estimated probit models of a response item of 5 versus lower than 4. In all cases, the effect of a full year of internship remains non-significant and low. The third critic is that, as for wages, heterogeneous preferences can explain satisfaction at work, but also the choice of doing a full year internship or majoring in finance. If students who did a full year internship or choose a major in finance are those who have preferences for professional settings and lower preferences for non monetary benefits, those students could engage in jobs with better wage and lower working conditions, which may create an downward in the satisfaction equations. Hence, we performed an IV analysis similar to the one conducted for wages (first stage regressions are close to the ones for wages). With this analysis, we find indeed higher effects, with a significant 0.5 points increase of the full year internship for the wage satisfaction and an insignificant 0.3 point increase for the non-monetary satisfaction. Choosing a major in Finance is negatively correlated with satisfaction in these IV analyses.

3.5.4 Internships and major choice

We examine the determinants of the 3rd-year major choice and the decision to do a full year of internship. We assume that individuals choose their majors to maximize the value function V_m, which is the sum of the non-monetary benefits ν_{0m} and the present value of expected earnings ν_{1m}:

$$m^\star = \underset{m}{\operatorname{argmax}} V_m$$

The non-monetary benefits are assumed to be a linear function of observable individual covariates (such as ability for a special topic, gender...). The year of graduation and so on economic conditions do not impact this term.

$$\nu_{0m} = \mathbb{E}_{3^{rd}year} \left(\alpha_m + X\beta_m\right)$$

The present value of expected earnings ν_{1m} can be modeled by

$$\sum_{t=g}^{T+g} \theta^{t-g} \mathbb{E}_{3^{rd}year} \log\left(W_{m(t-g)}\right)$$

with θ the discount factor, g the year of graduation, T the length of active life, W_{mt} the wages in year t given major m. Note that this is dependent of the year of graduation and so, the economic conditions. Furthermore, graduates from Grandes Ecoles usually perform very well in integrating quickly in the labor market, so we neglect the risk of unemployment and focus on wages. One important aspect is that ν_{0m} and ν_{1m} are unknown by the student and approximated when learning of his own ability and preferences.

There are 7 different majors in 3rd year of the program but due to the small number of students per major, we group them in those related to Finance/Acturial Sciences and those related to Economics/Social Sciences/Statistics. We explain the 3rd year major and the decision to do a full year of internship by students social characteristics - sex, place of birth, parents occupations - students admission type, schooling attainments in 2nd year - inverse ranking i.e. 100 for the best student, 0 for the worst, minor choices in 2nd year, and a dummy identifying those who chose to undertake the optional full year of internships (for the major choice only). The constant of the probit model for majoring in Finance can be interpreted as a wage differential.

Table 3.14 reports the results of average marginal effects of a probit model estimation for a major choice in Finance. As there are only two major choices, the constant captures the structural differences between the two majors such as wage differences. However, these differences in wages may change depending on the economic situation and may explain the choices of students. To help them to make these decisions, the administrative staff informs them on the labor market outcomes of former graduates by communicating about the results of the last Integration of Graduates survey. But results are published two years after graduation and are not known over a long period (in particular before 2007). One other solution is to add the year of graduation, which can measure the conjuncture.

Second year minor choice is naturally correlated with the 3rd year major one. Students who chose a minor in applied maths are more likely to major in Finance in 3rd year with an average marginal effect of 26.0-44.5%. The minor choice is a first expression of student's preferences and expected abilities. As seen previously in the descriptive statistics, students who undertook a full year of internships are more likely to major in Finance. However, it is possible to refine the major choices when several internships in different areas are made. Students whose at least one parent is professional or work in the business sector are more likely to major in Finance with an average marginal effect of 12%. 2nd year general ranking is positively correlated with majoring in Finance, but the results are not significantly different from 0. In contrast, the mathematics ability is highly correlated with majoring in Finance.

A student chooses to do an internship if

$$\mathbb{E}_{2^{nd}year}\left[p_1 V_{m_1}(d=1) + (1-p_1) V_{m_2}(d=1)\right] + \lambda > \mathbb{E}_{2^{nd}year}\left[p_0 V_{m_1}(d=0) + (1-p_0) V_{m_2}(d=0)\right]$$

with λ the net value (wages minus cost of living) of doing one year of internships. p_1 (resp. $1-p_1$) is the probability of choosing the major m_1 (resp. m_2) after doing a full year of internships. p_0 (resp. $1-p_0$) is the probability of choosing the major m_1 (resp. m_2) without doing a full year of internship. We can neglect here the cost of the studies, which is more than moderate and rather constant over time (around 500 euros per academic year).

If a student did not complete a full year of internship, he will do a three months internship between the two years of the master's degree. Even without this full year, the expected value function at the end of the second year (the first year of the master's degree) and at the beginning of the third year (the second year of the master's degree) could be different. Nevertheless, we may consider that they are equal, $\mathbb{E}_{2^{nd}year}\left[\ldots/d=0\right]=\mathbb{E}_{3^{rd}year}\left[\ldots/d=0\right]$.

Table 3.15 reports the results for the decision to do a full year of internship (columns 3 and 4). We compute the predicted

$$\mathbb{E}_{2^{nd}year}\left[p_1V_{m_1}\left(d=1\right)+\left(1-p_1\right)V_{m_2}\left(d=1\right)\right]-\mathbb{E}_{2^{nd}year}\left[p_0V_{m_1}\left(d=0\right)+\left(1-p_0\right)V_{m_2}\left(d=0\right)\right]$$

using estimators of previous models, which are highly correlated with general ability. For each student, the probability of majoring in finance is computed with a probit model, with (p_1) or without (p_0) doing a full year internship. The value function V_m is assumed equal to the log of wage the first year after graduation and is computed with information available at the first year of the master's degree with a major in finance (V_{m_1}) or not (V_{m_2}), and with ($d=1$) or without ($d=0$) doing a full year internship. With this analysis, we want to know if the decision of taking-up a full year internship can be rationalized in a cost-benefit framework. There is no correlation between a full year internship and the predicted wage differentials, without controlling with specific abilities. However, when controlling, the predicted wage differentials have a positive effect compensated by the effect of specific abilities. It would indicate that the predicted wage differentials could have an heterogeneous effect, higher for low abilities students for whom the expected wages are lower. Other results are consistent with the first stages of the IV analysis. In contrast the decision to do a full year of internship is not correlated with the second year minor choice, the gender or the nationality. The admission in 2nd year, age in 2nd year, to have a social scholarship and the rank have a significant and negative effect. Increase its ranking of one decile decreases the probability of 2%.

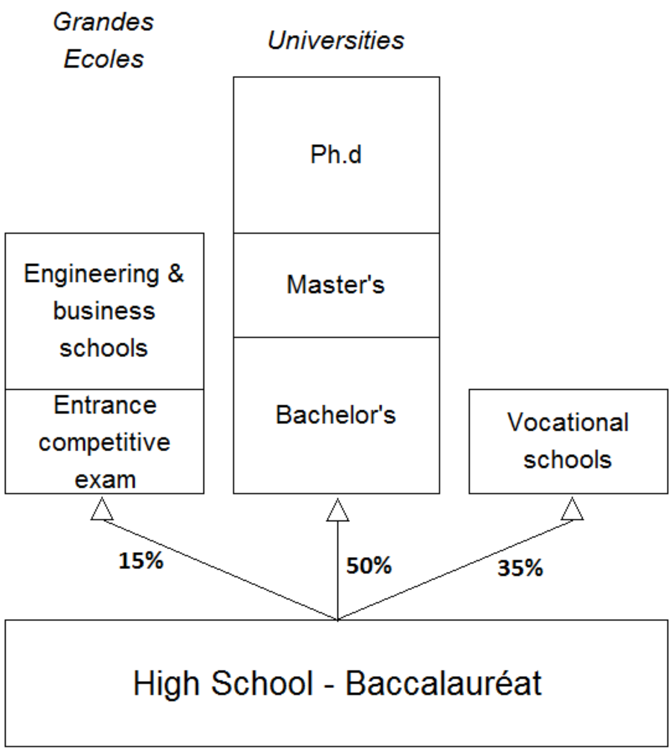
3.6 Conclusion

This article tests different predictions of internships effects using the endogenous selection process of an optional one year internship. Perform a full year of internship is less valued by employers as a real year of professional experience. Internships are perceived as a signal of ability by employers more than a return to experience and a gain in human capital. This year of internship improves the ability to find a job faster. It has a small positive effect on job satisfaction (but only significant with IV analysis and monetary component). It is possible to refine the major choices when several internships in different areas are made. Those questions are to be related to public policies. It opens the debate on compensation of internships, analyzing returns to education for early career professionals. It also provides evidence on the time students need to make effective major choices, and improve the social gain associated. Public policies implemented, which ban unpaid internships or more than six months, appear consistent with our results.

Internships may reduce the uncertainty about the wage expectations for a given major and may reveal the student preferences. But understanding the complete learning process of preferences associated with doing a full year of internship is a tricky issue. Furthermore, the role of non-market benefits such as working and learning in a high level academic or professional environment or with international experts is difficult to assess quantitatively. At last we do not model complete careers even if internships may generate better matching on the labor market and better long-term opportunities. These reduced-form results do not fully account for the dynamic process of doing a full year internship, a major choice and entering in the labor market. A structural model of schooling decision (Arcidiacono, 2004) could be estimated to take into the learning process of preferences associated with doing a full year internship.

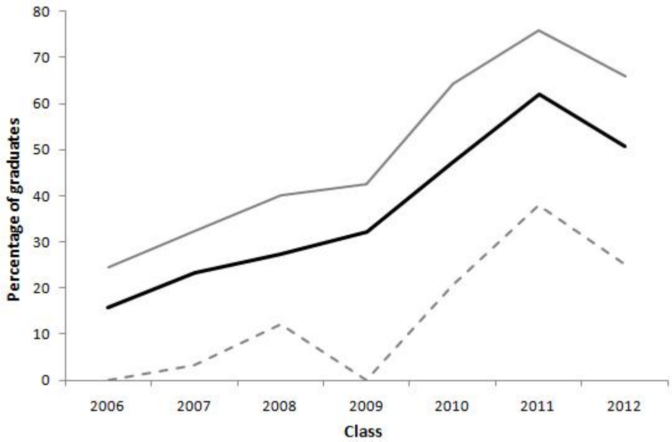
To design effectiveness of internships, three actors are involved: firms, universities and students. Their expectations regarding internships have no reason to be the same. Our approach focuses on the students size and partly on the firm point of view. We leave for future research the understanding of the links between these 3 actors.

3.7 Tables and graphics



Note: figures from Analyse de l'orientation et des poursuites d'études des lycéens à partir de la procédure admission post-bac, Rapport n° 2012-123, Inspection générale de l'éducation nationale, p.53

Figure 3.1 : French higher education system



Note: full grey line for students admitted in first year, dash grey line for students admitted in second year, full black line for all students.

Figure 3.2 : Share of students undertaking a full year of internships.

		Total	Full year internship	No full year internship	Mean Diff. p-value
First year of master					
(year 2)	Mathematics	69.0	68.3	69.5	0.796
	Economics	31.0	31.7	30.5	
Second year of master					
(year 3)	Finance	53.8	61.1	48.9	0.011
	Other	46.2	38.9	51.1	
# Obs.		452	180	272	

Note: data from the administrative education software.

Table 3.1 : Minor and major choices (%) with and without a full year of internships

		Total	Full year internship	No full year internship	Mean Diff. p-value
First year of master		50.3	46.1	53.0	0.012
(year 2)	*Minor*				
All Courses	Mathematics	50.2	46.8	52.5	0.088
	Economics	50.3	44.6	54.2	0.055
Main Courses	*Topics*				
	Mathematics	52.2	49.9	53.7	0.111
	Economics	52.7	50.8	53.9	0.168
2nd-year minor field	*Topics*				
Mathematics	Mathematics	56.9	54.3	58.7	0.099
Main Courses	Economics	51.9	52.1	51.7	0.889
2nd-year minor field	*Topics*				
Economics	Mathematics	41.5	40.5	42.2	0.680
Main Courses	Economics	54.4	47.9	58.9	0.007
# Obs.		452	180	272	

Note: data from the administrative education software. For each 2nd year of the program, the best student obtained the rank of 100, the worst gets 0. The global ranking is based on the average score during the year. Specific rankings are based on the written individual evaluation. Courses included for the specific rankings have been defined using a principal component analysis.

Table 3.2 : Ability of students (average ranking) with and without a full year of internships

		Total	Full year internship	No full year internship
	Number of internships (%)			
	1	74.8	36.7	100
	2	22.1	55.6	
	3	3.1	7.8	
	Internships topics (%)			
Total				
	Finance	45.6	50.0	42.7
	Other	48.7	35.6	57.3
	Various	5.7	14.4	
2nd-year minor field				
Mathematics	Finance	58.0	61.0	56.1
	Other	36.2	24.4	43.9
	Various	5.8	14.6	
Economics	Finance	17.9	26.3	12.1
	Other	76.4	59.7	87.9
	Various	5.7	14.0	
3rd-year major field				
Finance	Finance	72.4	73.6	71.4
	Other	21.0	11.8	28.6
	Various	6.6	14.6	
Other	Finance	14.3	12.9	15.1
	Other	80.9	72.9	84.3
	Various	4.8	14.3	
	# Obs.	452	180	272

Note: data from the administrative education software.

Table 3.3 : Features of internships

		Total	Full year internship	No full year internship	Mean Diff. p-value
Labor market status 6 months after graduation (%)					
	Employment	63.2	73.4	55.3	0.047
	Job search	6.0	2.4	8.7	0.202
	Further studies	15.8	11.3	19.3	0.171
	Ph.d	14.0	12.1	15.5	0.436
	No activity	1.0	0.8	1.2	0.913
	# Obs.	285	124	161	
Average annual wages bonus included(in euros)					
Total		50.000	53.800	47.600	0.002
Months after graduation	6 months	45.400	46.800	44.100	0.264
	18 months	51.500	60.000	47.000	0.000
	30 months	55.200	60.200	53.100	0.180
3rd-year major field 6 months after graduation	Finance	47.300	48.700	45.900	0.088
	Other	42.200	43.100	41.500	0.006
	# Obs.	406	157	249	
Satisfaction at work - 1 (worst) to 5 (best)					
Total		4.10	4.16	4.06	0.152
	Autonomy	4.03	4.14	3.95	0.061
	Working conditions	4.17	4.16	4.17	0.969
	Working environment	4.44	4.51	4.38	0.141
	Wages	3.77	3.82	3.73	0.428
	# Obs.	354	146	208	

Note: data from the survey "integration of graduates" from 2007 to 2012 (2007 excluded for satisfaction variables). The satisfaction variable is the average of satisfaction of working conditions, working environment, autonomy in work, and satisfaction of the wages level. For each them, the scale ranges from 1 (worst) to 5 (best).

Table 3.4 : Labor market outcomes after graduation

	Total	Full year internship	No full year internship	Mean Diff. p-value	Total	Full year internship	No full year internship	Mean Diff. p-value
	Wages				Autonomy			
1 (worst)	3.4	2.0	4.3	0.246	0.8	0.7	1.0	0.781
2	9.0	8.2	9.6	0.653	6.5	3.4	8.6	0.050
3	23.2	23.3	23.1	0.963	18.9	16.4	20.7	0.318
4	36.2	38.4	34.6	0.472	36.2	39.7	33.6	0.243
5 (best)	28.2	28.1	28.4	0.954	37.6	39.7	36.1	0.484
# Obs.	354	146	208		354	146	208	
	Working conditions				Working environment			
1 (worst)	1.1	1.4	1.0	0.721	0.3	0.0	0.5	0.403
2	5.6	3.4	7.2	0.129	2.3	1.4	2.9	0.347
3	13.0	14.4	12.0	0.516	8.8	8.2	9.1	0.765
4	35.9	39.0	33.6	0.299	31.1	28.8	32.7	0.434
5 (best)	44.3	41.8	46.1	0.416	57.6	61.6	54.8	0.201
# Obs.	354	146	208		354	146	208	

Note: data from the survey "integration of graduates" from 2008 to 2012. For each satisfaction variable, the scale ranges from 1 (worst) to 5 (best).

Table 3.5 : Satisfaction at work - Detailed distribution (%)

	Total	Full year internship	No full year internship	Mean Diff. p-value
Before graduating	56.8	62.9	52.7	0.086
Less than 2 months	24.2	20.7	26.6	0.252
Between 2 and 4 months	6.3	6.9	5.9	0.740
Between 4 and 6 months	4.9	5.2	4.7	0.867
More than 6 months	7.7	4.3	10.1	0.075
# Obs.	285	116	169	

Note: data from the survey "integration of graduates" from 2007 to 2012.

Table 3.6 : Duration of first job search (%)

	(1) Log Wage OLS	(2) Log Wage OLS	(3) Log Wage OLS	(4) Log Wage OLS	(5) Log Wage LAD
Full year intern.	0.086***	0.063*	0.061*		
	(0.032)	(0.033)	(0.033)		
Experience	0.101***	0.102***	0.101***		
	(0.015)	(0.014)	(0.014)		
3rd year Major: Finance		0.088***	0.095***	0.089***	0.062***
		(0.030)	(0.031)	(0.030)	(0.020)
2nd year inverse rank		0.081		0.083	0.062**
		(0.053)		(0.053)	(0.030)
Inverse rank - economics			0.123**		
			(0.061)		
Inverse rank - mathematics			-0.024		
			(0.060)		
Full year intern. - No experience				0.037	0.011
				(0.037)	(0.027)
No full year intern. - 1 year experience				0.076***	0.052**
				(0.027)	(0.024)
Full year intern. - 1 year experience				0.168***	0.096***
				(0.044)	(0.030)
No full year intern. - 2 years experience				0.196***	0.148***
				(0.034)	(0.030)
Full year intern. - 2 years experience				0.265***	0.231***
				(0.058)	(0.042)
Women	-0.030	-0.015	-0.013	-0.015	-0.001
	(0.028)	(0.028)	(0.027)	(0.028)	(0.018)
Foreign citizenship	-0.087**	-0.111***	-0.113***	-0.111***	-0.076***
	(0.035)	(0.034)	(0.035)	(0.034)	(0.023)
Working abroad	0.402***	0.383***	0.378***	0.381***	0.334***
	(0.090)	(0.087)	(0.087)	(0.087)	(0.029)
Second job	0.038	0.035	0.045	0.035	0.015
	(0.042)	(0.042)	(0.043)	(0.041)	(0.025)
Public sector	-0.122***	-0.092**	-0.098**	-0.089**	-0.056**
	(0.037)	(0.039)	(0.039)	(0.039)	(0.025)
Fixed term contract	-0.091**	-0.083*	-0.077*	-0.083*	-0.046**
	(0.045)	(0.045)	(0.046)	(0.045)	(0.023)
Constant	10.654***	10.566***	10.552***	10.581***	10.611***
	(0.049)	(0.064)	(0.065)	(0.064)	(0.038)
Observations	444	444	444	444	444
Adjusted R^2	0.342	0.364	0.365	0.362	
Full year intern./Experience	85%	61%	60%		

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2, 3 and 4 report the OLS estimates, Column 5 the LAD estimates. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier (except for the LAD estimator). Year of graduation used as control variables.

Table 3.7 : Wages determinants - OLS and LAD

	(1) Log Wage OLS	(2) Log Wage OLS	(3) Log Wage OLS
Full year intern.	0.061	-0.021	-0.086
	(0.050)	(0.056)	(0.080)
Experience	0.065***	0.103***	0.102***
	(0.019)	(0.014)	(0.015)
3rd year Major: Finance	0.058	0.093***	0.104***
	(0.039)	(0.031)	(0.032)
Full year intern. x Finance	-0.017		
	(0.065)		
Finance x Experience	0.054*		
	(0.028)		
Full year intern. x Experience	0.015		
	(0.032)		
2nd year inverse rank	0.076	0.002	
	(0.054)	(0.070)	
2nd year inverse rank x Full year intern.		0.185*	
		(0.109)	
Inverse rank - economics			0.067
			(0.069)
Inverse rank - economics x Full year intern.			0.061
			(0.141)
Inverse rank - mathematics			-0.124
			(0.083)
Inverse rank - mathematics x Full year intern.			0.233*
			(0.132)
Constant	10.590***	10.606***	10.639***
	(0.060)	(0.066)	(0.074)
Observations	444	444	444
Adjusted R^2	0.365	0.370	0.372

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2 and 3 report the OLS estimates. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier (except for the LAD estimator). Year of graduation and job characteristics used as control variables.

Table 3.8 : Wages interaction effects - OLS

	(1)	(2)	(3)	(4)	(5)
	Log wage	Log wage	Log wage	Log wage	Log wage
	OLS	OLS	IV 2SLS	IV 2SLS	IV 2LS
Full year intern.	0.056*	0.077***	0.151	0.160**	0.130**
	(0.034)	(0.030)	(0.108)	(0.076)	(0.053)
Experience	0.105***	0.106***	0.109***	0.110***	0.108***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
3rd year Major: Finance	0.093***	0.089***	0.080	0.085	0.085
	(0.032)	(0.030)	(0.082)	(0.072)	(0.071)
2nd year inverse rank	0.074	0.078	0.092	0.093	0.088
	(0.050)	(0.052)	(0.061)	(0.059)	(0.056)
Age in 2nd year	-0.006				
	(0.015)				
2nd year minor: applied maths	-0.009				
	(0.040)				
Admitted in 2nd year	-0.034				
	(0.031)				
Year of graduation > 2010	0.005				
	(0.032)				
Constant	10.714***	10.554***	10.515***	10.507***	10.525***
	(0.347)	(0.042)	(0.099)	(0.063)	(0.057)
Observations	444	444	444	444	444
Adjusted R^2	0.366	0.368	0.354	0.350	0.361
KP stat. for rank condition			20.642	36.673	27.443
pvalue			0.000	0.000	0.000
Sargan-Hansen stat for overid.			.	0.016	0.487
pvalue			.	0.900	0.784
KP F for weak ident.			10.283	16.951	15.018
Instruments					
Age in 2nd year			X	X	X
2nd year minor: applied maths			X	X	X
Admitted in 2nd year				X	X
Year of graduation > 2010					X

Note: Standard errors in parentheses data, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns report the 2SLS estimates with full year of internship and 3rd year Major in Finance as endogenous variables. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier. Job characteristics used as control variables.

Table 3.9 : Wage determinants - IV 2SLS

	(1) Full year intern. OLS	(2) Full year intern. OLS	(3) Full year intern. OLS	(4) 3rd year Major Finance OLS	(5) 3rd year Major Finance OLS	(6) 3rd year Major Finance OLS
Age in 2nd year	-0.119***	-0.069***	-0.053**	0.014	0.037*	0.037*
	(0.023)	(0.024)	(0.023)	(0.018)	(0.020)	(0.020)
2nd year minor: applied maths	-0.032	-0.035	-0.039	0.451***	0.449***	0.449***
	(0.078)	(0.069)	(0.065)	(0.069)	(0.067)	(0.067)
Admitted in 2nd year		-0.308***	-0.363***		-0.142**	-0.142**
		(0.063)	(0.057)		(0.059)	(0.059)
Year of graduation > 2010			0.314***			0.001
			(0.055)			(0.052)
Constant	3.392***	2.344***	1.834***	-0.033	-0.518	-0.519
	(0.536)	(0.545)	(0.552)	(0.419)	(0.451)	(0.462)
Observations	444	444	444	444	444	444
Adjusted R^2	0.147	0.222	0.313	0.430	0.444	0.443
F-Stat	13.98	19.63	33.78	22.63	19.57	14.95

Note: Standard errors in parentheses data, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns report the 2SLS first-stages estimates for full year of internship and 3rd year Major in Finance. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier.

Table 3.10 : Wage determinants - IV 2SLS first-stages

	(1) Before graduation Probit	(2) Before graduation IV 2SLS	(3) Full year intern. OLS	(4) 3rd year Major: Finance OLS	(5) Duration Int. Reg.
Full year intern.	0.133**	0.202			-1.334*
	(0.059)	(0.142)			(0.684)
3rd year Major: Finance	0.103*	-0.177			-0.510
	(0.061)	(0.117)			(0.603)
2nd year inverse rank	0.230**	0.265**	-0.184*	0.075	-1.887*
	(0.097)	(0.105)	(0.098)	(0.093)	(1.041)
Graduating in 2008	-0.211**	-0.214**	0.089	-0.099	2.527***
(Ref. Grad. in 2007)	(0.094)	(0.099)	(0.088)	(0.094)	(0.950)
Graduating in 2009	-0.077	-0.053	0.043	-0.030	1.498
	(0.090)	(0.089)	(0.078)	(0.079)	(0.967)
Graduating in 2010	-0.130	-0.158	0.217**	-0.118	1.422
	(0.091)	(0.096)	(0.085)	(0.085)	(0.900)
Graduating in 2011	-0.151	-0.139	0.399***	0.021	1.813*
	(0.094)	(0.103)	(0.083)	(0.085)	(0.944)
Graduating in 2012	-0.275**	-0.322**	0.420***	-0.056	2.861***
	(0.112)	(0.129)	(0.089)	(0.106)	(1.096)
Women	0.042	-0.020	-0.056	-0.137***	-0.599
	(0.060)	(0.068)	(0.053)	(0.052)	(0.597)
Foreign citizenship	-0.159**	-0.090	-0.057	0.146**	2.376***
	(0.075)	(0.086)	(0.074)	(0.061)	(0.863)
Age in 2nd year			-0.069***	0.013	
			(0.023)	(0.020)	
2nd year minor: applied maths			0.016	0.569***	
			(0.062)	(0.055)	
Admitted in 2nd year			-0.335***	-0.063	0.338
			(0.053)	(0.054)	(0.636)
Constant		0.614***	2.051***	-0.054	-1.012
		(0.127)	(0.536)	(0.460)	(0.950)
Observations	285	285	285	285	285
Pseudo R^2	0.067				
Baseline probability	0.563	0.563			
Adjusted R^2		.	0.216	0.321	
KP stat. for rank condition		54.898	27.08	36.42	
pvalue		0.000	0.000	0.000	
Sargan-Hansen stat for overid.		0.119			
pvalue		0.730			
KP F for weak ident.		26.644			

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column 1 is the average marginal effects of a probit model for finding a job before graduation. Column 2 reports the 2SLS estimates with full year of internship and 3rd year Major in Finance as endogenous variables and age in 2nd year, 2nd year minor-applied maths and admitted in 2nd year as instruments. Columns 3 and 4 report the 2SLS first-stages estimates for full year of internship and 3rd year Major in Finance. Column 5 is the estimates of an interval regression of the duration of job search.

Table 3.11 : Duration of job search

	Satisfaction Wage	Satisfaction Wage	Satisfaction Wage	Satisfaction Wage	Full year intern.	3rd year Major Finance
	OLS	OLS	IV 2SLS	IV 2SLS	OLS	OLS
Full year intern.	0.109	-0.027	0.599**	0.400		
	(0.141)	(0.136)	(0.252)	(0.266)		
3rd year Major: Finance	-0.230	-0.296*	-0.802***	-0.846***		
	(0.174)	(0.161)	(0.300)	(0.271)		
Experience	-0.141*	-0.297***	-0.119	-0.270***	0.029	-0.019
	(0.076)	(0.078)	(0.080)	(0.086)	(0.026)	(0.022)
Working abroad	0.465*	-0.198	0.652***	0.019	0.005	0.231***
	(0.237)	(0.248)	(0.245)	(0.261)	(0.107)	(0.068)
Second job	-0.038	-0.043	0.000	-0.012	-0.134**	-0.102
	(0.161)	(0.151)	(0.173)	(0.163)	(0.066)	(0.069)
Public sector	0.317	0.438**	0.090	0.208	-0.117	-0.278***
	(0.213)	(0.204)	(0.257)	(0.242)	(0.098)	(0.083)
Fixed term contract	0.097	0.118	0.030	0.048	-0.053	-0.098
	(0.201)	(0.187)	(0.205)	(0.188)	(0.090)	(0.070)
Log wage		1.533***		1.451***		
		(0.271)		(0.321)		
Age in 2nd year					-0.065***	0.029
					(0.022)	(0.021)
2nd year minor: applied maths					0.028	0.539***
					(0.071)	(0.070)
Admitted in 2nd year					-0.412***	-0.171***
					(0.061)	(0.063)
Year of graduation > 2010					0.358***	0.005
					(0.061)	(0.058)
Constant	3.826***	-12.407***	3.994***	-11.352***	1.893***	-0.320
	(0.167)	(2.890)	(0.249)	(3.354)	(0.492)	(0.489)
Observations	327	327	327	327	327	327
Adjusted R^2	0.047	0.139	.	0.066	0.342	0.479
KP stat. for rank condition			31.840	33.055	41.39	22.33
pvalue			0.000	0.000	0.000	0.000
Sargan-Hansen stat for overid.			0.732	0.704		
pvalue			0.693	0.703		
KP F for weak ident.			17.611	17.789		

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 2 report the OLS estimates. Columns 3 and 4 report the 2SLS estimates with full year of internship and 3rd year Major in Finance as endogenous variables and age in 2nd year, 2nd year minor-applied maths, admitted in 2nd year, and year of graduation as instruments. Columns 5 and 6 report the 2SLS first-stages estimates (without wage) for full year of internship and 3rd year Major in Finance. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier.

Table 3.12 : Job satisfaction - Wage

	Satisfaction Non mon. OLS	Satisfaction Non mon. OLS	Satisfaction Non mon. IV 2SLS	Satisfaction Non mon. IV 2SLS	Full year intern. OLS	3rd year Major Finance OLS
Full year intern.	0.106	0.089	0.176	0.150		
	(0.092)	(0.095)	(0.152)	(0.161)		
3rd year Major: Finance	-0.160	-0.169	-0.284	-0.290		
	(0.106)	(0.108)	(0.211)	(0.213)		
Experience	-0.098**	-0.118**	-0.095**	-0.115**	0.005	-0.034
	(0.048)	(0.049)	(0.047)	(0.049)	(0.028)	(0.024)
Working abroad	0.174	0.091	0.214	0.132	-0.097	0.170**
	(0.149)	(0.163)	(0.159)	(0.172)	(0.112)	(0.082)
Second job	0.004	0.003	0.008	0.006	-0.132**	-0.101
	(0.113)	(0.112)	(0.110)	(0.110)	(0.065)	(0.068)
Public sector	0.210*	0.225*	0.159	0.173	-0.093	-0.264***
	(0.115)	(0.117)	(0.147)	(0.146)	(0.099)	(0.086)
Fixed term contract	-0.113	-0.111	-0.130	-0.128	-0.048	-0.095
	(0.126)	(0.125)	(0.126)	(0.124)	(0.090)	(0.072)
Log wage		0.191		0.188	0.229**	0.136
		(0.196)		(0.212)	(0.110)	(0.099)
Age in 2nd year					-0.060***	0.032
					(0.021)	(0.022)
2nd year minor: applied maths					0.021	0.535***
					(0.070)	(0.072)
Admitted in 2nd year					-0.401***	-0.164***
					(0.060)	(0.063)
Year of graduation > 2010					0.351***	0.001
					(0.061)	(0.057)
Constant	4.288***	2.266	4.341***	2.351	-0.654	-1.831
	(0.101)	(2.079)	(0.158)	(2.209)	(1.355)	(1.342)
Observations	327	327	327	327	327	327
Adjusted R^2	0.026	0.027	0.019	0.020	0.350	0.481
KP stat. for rank condition			31.840	33.055	36.26	20.77
pvalue			0.000	0.000	0.000	0.000
Sargan-Hansen stat for overid.			1.847	1.830		
pvalue			0.397	0.401		
KP F for weak ident.			17.611	17.789		

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 2 report the OLS estimates. Columns 3 and 4 report the 2SLS estimates with full year of internship and 3rd year Major in Finance as endogenous variables and age in 2nd year, 2nd year minor-applied maths, admitted in 2nd year, and year of graduation as instruments. Columns 5 and 6 report the 2SLS first-stages estimates (with wage) for full year of internship and 3rd year Major in Finance. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier.

Table 3.13 : Job satisfaction - Non monetary components

	(1) Major: Finance	(2) Major: Finance	(3) Major: Finance	(4) Major: Finance
Full year intern.		0.164***	0.096	0.090
		(0.042)	(0.071)	(0.071)
Full year intern. Economics			-0.340***	-0.326***
			(0.026)	(0.026)
Full year intern. Economics and Finance			-0.149**	-0.139**
			(0.067)	(0.066)
2rd year minor: applied maths	0.445***	0.442***	0.299***	0.260***
	(0.031)	(0.031)	(0.050)	(0.052)
Full year intern. x applied maths			-0.058	-0.048
			(0.081)	(0.080)
2nd year inverse rank	0.019	0.045	0.131**	
	(0.067)	(0.066)	(0.058)	
Inverse rank - economics				-0.047
				(0.080)
Inverse rank - mathematics				0.287***
				(0.076)
Women	-0.101**	-0.098**	-0.075**	-0.073**
	(0.039)	(0.039)	(0.034)	(0.033)
Foreign citizenship	0.230***	0.237***	0.147***	0.121**
	(0.054)	(0.053)	(0.046)	(0.046)
One parent professional or retail trader	0.122***	0.122***	0.121***	0.122***
	(0.045)	(0.045)	(0.038)	(0.038)
Observations	452	452	452	452
Pseudo R^2	0.280	0.303	0.454	0.470
Baseline probability	0.536	0.536	0.537	0.537

Note: Standard errors in parentheses data, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2, 3 and 4 are the average marginal effects of the probit model for a third year major in Finance. Year of graduation used as control variables.

Table 3.14 : Major determinants - probit average marginal effects

	(1) Full year intern.	(2) Full year intern.	(3) Full year intern.	(4) Full year intern.
2nd year inverse rank	-0.200***			
	(0.074)			
Inverse rank - economics		-0.142		-0.293**
		(0.098)		(0.113)
Inverse rank - mathematics		-0.071		-0.245**
		(0.099)		(0.118)
Predicted Wage Differentials			0.026	1.114***
			(0.269)	(0.414)
2rd year minor: applied maths	0.039	0.064	0.055	0.075
	(0.047)	(0.049)	(0.046)	(0.049)
Women	-0.038	-0.045	-0.048	-0.053
	(0.044)	(0.042)	(0.043)	(0.042)
Foreign citizenship	-0.005	0.041	0.049	0.068
	(0.068)	(0.065)	(0.065)	(0.066)
One parent professional or retail trader	-0.023	0.001	0.015	0.004
	(0.051)	(0.049)	(0.049)	(0.048)
Admitted in 2nd year	-0.235***	-0.294***	-0.305***	-0.299***
	(0.049)	(0.047)	(0.046)	(0.046)
Age in 2nd year	-0.093***	-0.070***	-0.064***	-0.065***
	(0.021)	(0.021)	(0.021)	(0.021)
Social scholarship	0.027	-0.048	-0.051	-0.045
	(0.056)	(0.055)	(0.055)	(0.054)
Observations	452	452	452	452
Pseudo R^2	0.133	0.204	0.196	0.215
Baseline probability	0.396	0.397	0.397	0.399

Note: Standard errors in parentheses data, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1, 2, 3 and 4 are the average marginal effects of the probit model for doing a full year of internship. Year of graduation used as control variables.

Table 3.15 : Full year of internship determinants - probit average marginal effects

Conclusion

This thesis provides ideas on data and methods used in applied economics. The first article points out that without good data accuracy, it is not possible to obtain robust results. It also shows that the method must be adapted to the nature of the data. This first chapter sets the stage for theoretical advances on the economics of natural disasters. The two other articles use original data, from the Internet or administrative data sets. These new data provide answers to new questions and allow the comparison of (different) econometric methods. The growing availability of such data will allow for questioning the aggregation phenomena by comparing the results of micro and macroeconometrics models, as discussed previously with the example of fuel prices. Indicators, such as a measure of individual skill become available which allows for better understanding of the impacts of a phenomenon such as internships.

Appendix: The death toll and the damage of natural disasters

Appendix. Interpretation of Censored Quantile Regression

Censored quantile regressions are models that explain conditional quantiles when the dependant variable is censored. As for uncensored quantile regression, there is no easy way to compute estimators for such models and only few empirical applications exist (Gustavsen et al. (2008), Fack et Landais (2009) for examples of application). Some computational algorithms have been designed such as Fitzenberger (2007), but which are efficient only for low degree of censorship. For higher rate of censoring, recent papers propose simple algorithms with asymptotic convergence in the exogenous (Chernozhukov and Hong 2002) or the endogenous case (Chernozhukov *et al.* 2011). Beyond computational difficulties, interpretation of censored quantile regression remains unclear. Uncensored quantile regression often appears as a solution to censorship when the point of censorship is exogenous. For higher quantiles, observations are indeed all uncensored. This is obviously not the case when censorship is endogenous.

To illustrate these difficulties, we simulate a Tobit I model $y_i = \max(0, y_i^*)$ with $y_i^* = \sum\limits_{i\in\{1;2;3\}} \alpha_i \cdot \mathbf{1}\{Pop_i\} + x_i + \varepsilon_i$. y represents for example the consumption of a durable good and x the income. We assume all observations are i.i.d., $x \sim \mathcal{N}(0,1)$ and $\varepsilon \sim \mathcal{N}(0, 0.333)$. There are 3 subpopulations. $Pop1$ has a high degree of censoring (around 97 % with $\alpha_1 = -2$), $Pop2$ a medium degree (around 50 % with $\alpha_2 = 0$) and $Pop3$ a low degree (around 3 % with $\alpha_3 = 2$). The overall censoring rate is around 50 %. $y*$ is a translation model. Quantile regression won't be useful in this particular case. However, the simple model is a good way to illustrate properties of censored quantile model. To avoid convergence problems, we use a random sample of 150.000 observations.

Quantile regresssion is robust to misspecification of the error term, some types of heteroscedasticity and outliers. It also allows different effects depending on the quantile of the conditional distribution (which it is not the case in our example). We implement uncensored and censored quantile regressions. Figure A1 illustrates the results for quantile regressions in three cases. The solid black line represents the uncensored model for all observations, the dashed black line the uncensored model for positive observations and the solid grey line the censored model. Uncensored estimators are biased, which is not the case for censored ones (estimated with the tree step estimators of Chernozhukov and Hong 2002 and the Stata package cqiv). They are equal to the parameter associated with y^*. To check the size of the bias in terms of rate of censoring, we estimate with an uncensored quantile regression the 90 th quantile estimator in the model $y_i = \max(0, y_i^*)$ with $y_i^* = \alpha_{variable} + x_i + \varepsilon_i$ and a variable constant. The bias appears to be linear with the rate of censoring in figure A2.

Interpretations of Tobit estimators depend on the economic questions (Wooldridge 2010). If it is a "true" issue of censoring as for example the observation of a variable only below a threshold, you can compute marginal effects on the latent dependent variable y^*, $\frac{\partial \mathbb{E}(y^*)}{\partial x_k} = \beta_k$. If the model is the solution of a maximization problem and the censoring point a corner solution (as in our example), you will be interested in the marginal effects on the expected value for y (censored and uncensored) $\frac{\partial \mathbb{E}(y_i)}{\partial x_k} = \phi\left(\frac{x_i\beta}{\sigma}\right)\beta_k$ where ϕ is the standard normal cdf and in the marginal effects on the expected value for y for uncensored observations, $\frac{\partial \mathbb{E}(y_i/y_i>0)}{\partial x_k} = \beta_k\left(1 - \lambda(\alpha)\left[\left(\frac{x_i\beta}{\sigma}\right) + \lambda(\alpha)\right]\right)$ where $\lambda(\alpha)$ is the inverse Mills ratio. The marginal effects are not constant and depend of the predictors, average marginal effects are so often used. Average marginal effects for y (censored and uncensored) correspond to a scale factor of the estimators.

Concerning censored quantile regression, Chernozhukov *et al.* (2011) note that $\frac{\partial Q_{y_i/X_i,c_i}(\tau)}{\partial x_k} = \mathbf{1}\{x_i\beta(\tau) + \varepsilon(\tau) > c_i\} \cdot \beta_k(\tau)$. The marginal effect corresponds to a marginal change on the observed response y for the non-censored individuals. Average marginal effects for y (censored and uncensored) correspond so to a scale factor of the estimators. The scale factor will be higher for the higher quantiles because for higher quantiles, the rate of censorship is lower. The use of average marginal effects will so increase the difference of estimates between extreme quantiles.

Three main conclusions can be done:

- Using uncensored quantile regression with endogenous censored data gives biased estimators. This bias exists even for high quantiles, if for some value of the predictor the dependent variable is almost always censored.
- The bias will be low if the rate of censoring is overall low.
- The censored quantile estimators are defined for the latent variable y^*. Comparing average marginal effects is difficult as these ones depend on parametric choices for Tobit.

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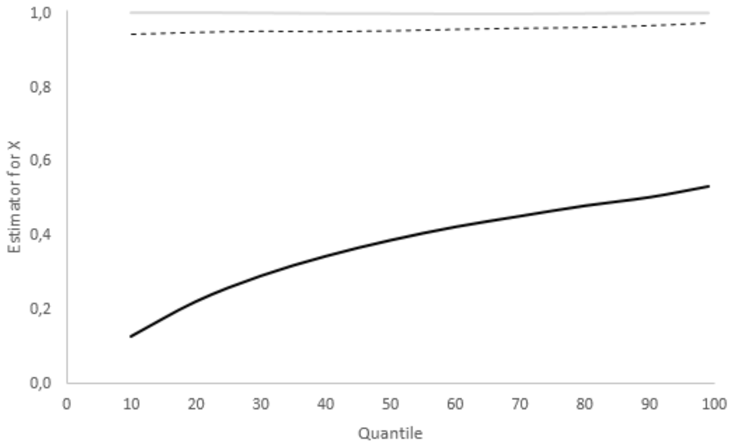
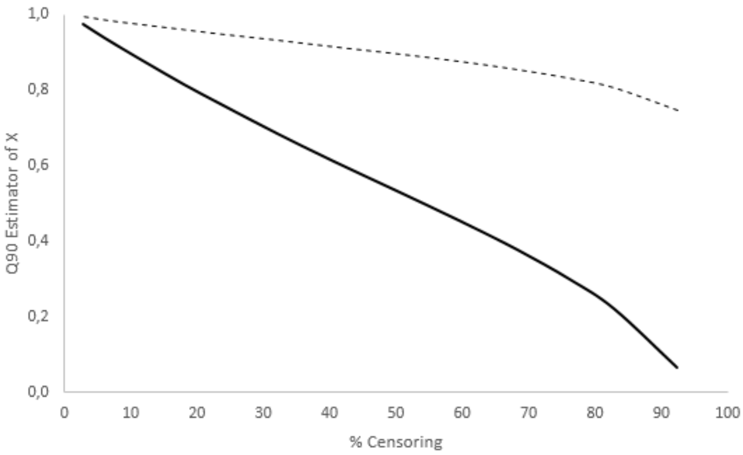
Figure A1: OLS and Tobit results



Note: Abscissa are for the quantiles, ordinates for the estimators. The solid black line represents the uncensored model for all observations, the dashed black line the uncensored model for positive observations and the solid grey line the censored model.

Figure A2: 90th quantile estimator in terms of rate of censoring



Note: Abscissa are for the rate of censoring in the model $y_i = \max(0, y_i^*)$ with $y_i^* = \alpha_{variable} + x_i + \varepsilon_i$ and a variable constant, ordinates for the 90th quantile estimators of X (which is equal to 1 in theory) . The solid black line represents the uncensored model for all observations, the dashed black line the uncensored model for positive observations.

Appendix: The Dynamics of Gasoline Prices

Appendix A. Likelihood function

The contribution to the likelihood function of price constancy in firm i at date t is :

$$\begin{aligned}
l_{1i,t} &= \Pr(dp_{i,t,\tau}|dp_{i,t,\tau}=0, p_{i,t-\tau}, X_{it}, p_t^o)\\
&= \Pr(s_{it} < p_{i,t-\tau} - p_{i,t}^* < S_{it})\\
&= \Pr(\gamma_{is}X_{it} + \varepsilon_{2,it} < p_{i,t-\tau} - \alpha_i - \beta_i p_t^o - \varepsilon_{1,it} < \gamma_{iS}X_{it} - \varepsilon_{2,it})\\
&= \Pr(\varepsilon_{1,it} + \varepsilon_{2,it} < p_{i,t-\tau} - \alpha_i - \beta_i p_t^o - \gamma_{is}X_{it}; p_{i,t-\tau} - \alpha_i - \beta_i p_t^o - \gamma_{iS}X_{it} < \varepsilon_{1,it} - \varepsilon_{2,it})\\
&= \Phi\left[\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o - \gamma_{is}X_{it}}{\sqrt{\sigma_{1i}^2 + \sigma_{2i}^2}}\right]\\
&\qquad -\Phi_2\left[\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o - \gamma_{is}X_{it}}{\sqrt{\sigma_{1i}^2 + \sigma_{2i}^2}}; \frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o - \gamma_{iS}X_{it}}{\sqrt{\sigma_{1i}^2 + \sigma_{2i}^2}}; \frac{\sigma_{1i}^2 - \sigma_{2i}^2}{\sigma_{1i}^2 + \sigma_{2i}^2}\right]
\end{aligned} \tag{3.7.1}$$

where Φ is the c.d.f of the Gaussian distribution and $\Phi_2$is the bivariate c.d.f of the Gaussian distribution.

The contribution to the likelihood function of a price increase in firm i at date t is:

$$\begin{aligned}
l_{2i,t} &= \Pr(dp_{i,t,\tau}|dp_{i,t,\tau}>0, p_{i,t-\tau}, X_{it}, p_t^o)\\
&= \Pr\left(\varepsilon_{1,it} = p_{i,t} - \alpha_i - \beta_i p_t^0\right) \times \Pr\left[p_{i,t-\tau} - p_{i,t}^* \le s_{it},\quad p_{i,t-\tau} - p_{i,t}^* < S_{it} \mid \varepsilon_{1,it} = p_{i,t} - \alpha_i - \beta_i p_t^0\right]\\
&= \frac{1}{\sigma_{1i}}\phi\left(\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o}{\sigma_{1i}}\right)\\
&\times \Pr\left[\begin{array}{c} p_{i,t-\tau} - \alpha_i - \beta_i p_t^0 - \varepsilon_{1,it} \le \gamma_{is}X_{it} + \varepsilon_{2,it},\quad p_{i,t-\tau} - \alpha_i - \beta_i p_t^0 - \varepsilon_{1,it} < \gamma_{iS}X_{it} - \varepsilon_{2,it}\\ \mid \varepsilon_{1,it} = p_{i,t} - \alpha_i - \beta_i p_t^0 \end{array}\right]\\
&= \frac{1}{\sigma_{1i}}\phi\left(\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o}{\sigma_{1i}}\right) \times \Pr\left[-dp_{i,t,\tau} - \gamma_{is}X_{it} \le \varepsilon_{2,it},\quad dp_{i,t,\tau} + \gamma_{iS}X_{it} > \varepsilon_{2,it}\right]\\
&= \frac{1}{\sigma_{1i}}\phi\left(\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o}{\sigma_{1i}}\right) \times \left[\Phi\left(\frac{dp_{i,t,\tau} + \gamma_{iS}X_{it}}{\sigma_{2i}}\right) - \Phi\left(\frac{-dp_{i,t,\tau} - \gamma_{is}X_{it}}{\sigma_{2i}}\right)\right]
\end{aligned} \tag{3.7.2}$$

where ϕ is the p.d.f of the Gaussian distribution and $dp_{i,t,\tau} = p_{it} - p_{it-\tau}$.

The contribution to the likelihood function of a price decrease in firm i at date

t is:

$$
\begin{aligned}
l_{3i,t} &= \Pr(dp_{i,t,\tau} | dp_{i,t,\tau} < 0, p_{i,t-\tau}, X_{it}, p_t^o) \\
&= \Pr\left(\varepsilon_{1,it} = p_{i,t} - \alpha_i - \beta_i p_t^0\right) \times \Pr\left[p_{i,t-\tau} - p_{i,t}^* > s_{it}, \quad p_{i,t-\tau} - p_{i,t}^* \geq S_{it} \mid \varepsilon_{1,it} = p_{i,t} - \alpha_i - \beta_i p_t^0\right] \\
&= \frac{1}{\sigma_{1i}} \phi\left(\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o}{\sigma_{1i}}\right) \\
&\times \Pr\left[\begin{array}{c} p_{i,t-\tau} - \alpha_i - \beta_i p_t^0 - \varepsilon_{1,it} > \gamma_{is} X_{it} + \varepsilon_{2,it}, \quad p_{i,t-\tau} - \alpha_i - \beta_i p_t^0 - \varepsilon_{1,it} \geq \gamma_{iS} X_{it} - \varepsilon_{2,it} \\ \mid \varepsilon_{1,it} = p_{i,t} - \alpha_i - \beta_i p_t^0 \end{array}\right] \\
&= \frac{1}{\sigma_{1i}} \phi\left(\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o}{\sigma_{1i}}\right) \times \Pr\left[-dp_{i,t,\tau} - \gamma_{is} X_{it} > \varepsilon_{2,it}, \quad dp_{i,t,\tau} + \gamma_{iS} X_{it} < \varepsilon_{2,it}\right] \\
&= \frac{1}{\sigma_{1i}} \phi\left(\frac{p_{i,t-\tau} - \alpha_i - \beta_i p_t^o}{\sigma_{1i}}\right) \times \left[\Phi\left(\frac{-dp_{i,t,\tau} - \gamma_{is} X_{it}}{\sigma_{2i}}\right) - \Phi\left(\frac{dp_{i,t,\tau} + \gamma_{iS} X_{it}}{\sigma_{2i}}\right)\right]
\end{aligned}
\tag{3.7.3}
$$

where ϕ is the p.d.f of the Gaussian distribution and $dp_{i,t,\tau} = p_{it} - p_{it-\tau}$.

The likelihood function for an i.i.d. sample of a given firm i is thus:

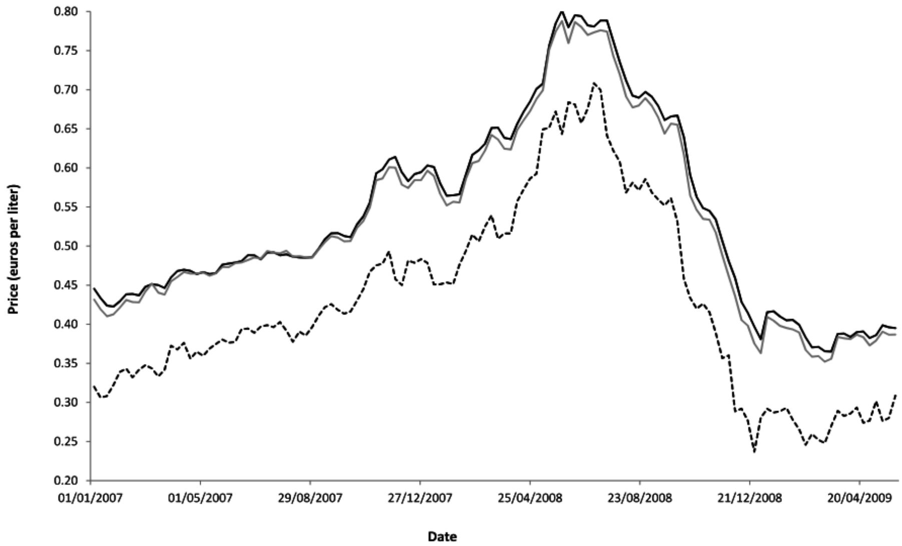
$$\ln L_i(\theta) = \sum_{t=1}^{T_i} \left(l_{1i,t} \times y_{1it} + l_{2i,t} \times y_{2it} + l_{3i,t} \times y_{3it}\right)$$

where $y_{1it} = 1$ if $dp_{i,t,\tau} = 0$ and 0 otherwise, $y_{2it} = 1$ if $dp_{i,t,\tau} < 0$ and 0 otherwise and $y_{3it} = 1$ if $dp_{i,t,\tau} > 0$ and 0 otherwise.

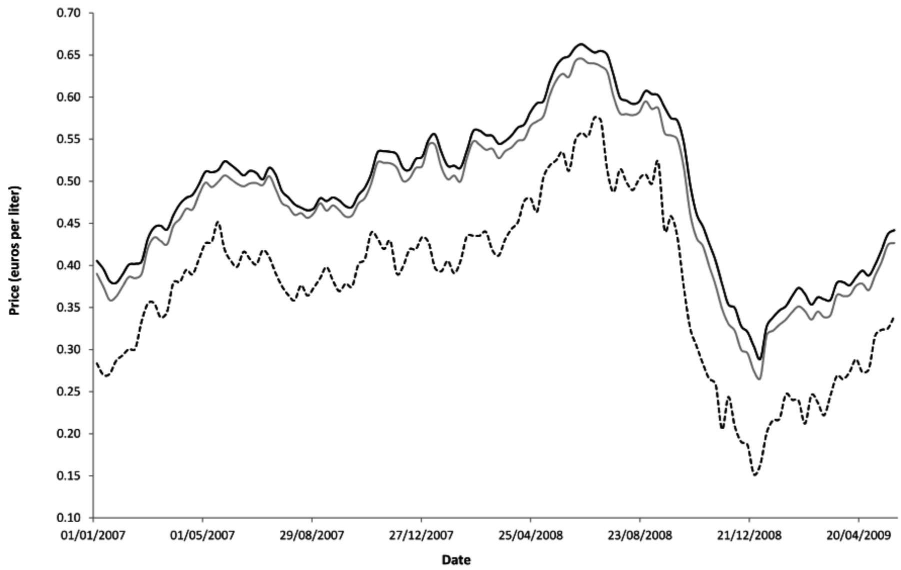
Appendix B

Figure B1: Average retail prices (individual data set), weekly retail prices published by the Ministry of Economy and wholesale market prices (Rotterdam)

a) Diesel



a) Unleaded petrol

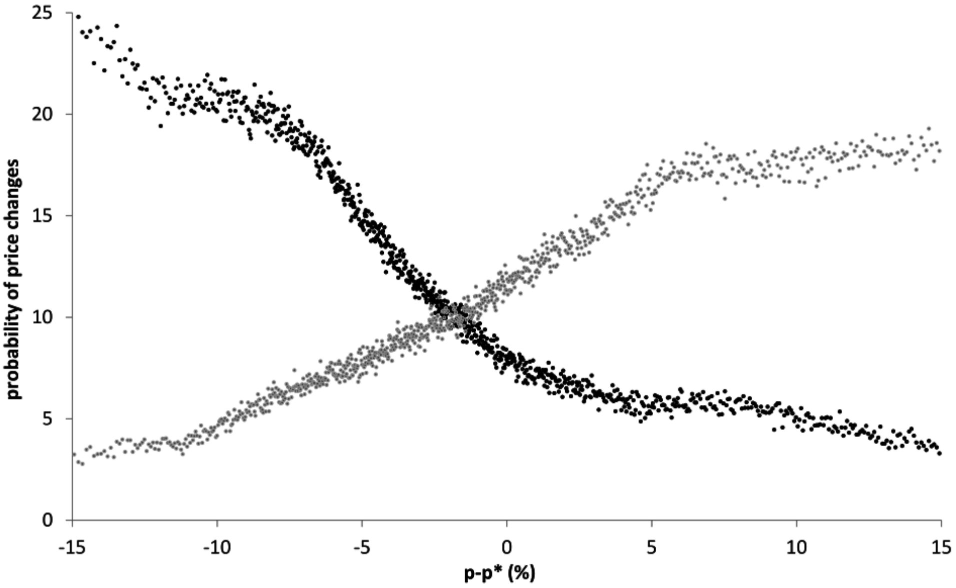


Note: dashed line is for Rotterdam prices, black line is for the average of individual prices collected in our data set and grey line is for the aggregate retail price series published by the Ministry of Economy each Friday.

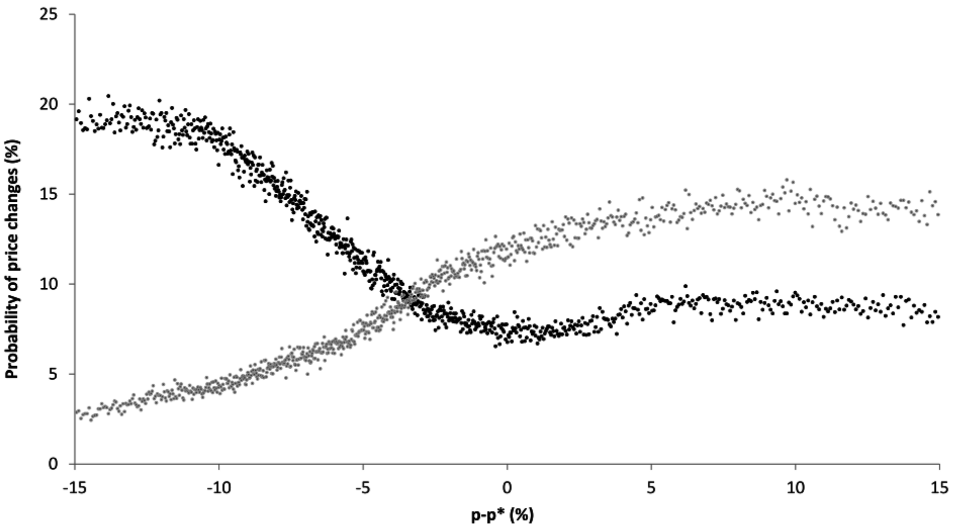
Appendix C

Figure C1: Adjustment hazard functions

a) Diesel



b) Unleaded petrol



Note: Adjustment hazard functions are computed as the probability of price increases or decreases as a function of the difference between log price of gasoline (p) and the log frictionless price ($p*$) (ie Rotterdam market price). For each retailer, the difference $p - p*$ is centered. Black points are for probability of price increases. Grey points are for probability of price decreases.

Appendix D: Pricing points

A possible explanation of the M-shape distribution of price changes might be related to the number of digits used to display prices. A large majority of outlets list their prices with three decimal places. So, in principle price changes could be smaller than 1%. For example, if the diesel price (excluding taxes) is 0.45 euros (i.e. close to the minimum price over our sample period), the retailer can decide to increase its price by 0.001 euros and the price increase in percentage would be 0.22%. However, the distribution of the last digit in prices (including all taxes) is not uniform: 31% of gasoline prices end with "9", 29% with "0", 9% with "5", 7% with "4" whereas for other last digit figures this proportion is smaller than 5% (see Table A) and the distribution of the penultimate digit appears uniform. Knotek (2010) and Levy *et al.* (2011) provide similar evidence on different US products. The timing of price changes might be modified by the presence of pricing points: firms wait for large movements of wholesale prices before changing their prices because they want to increase or decrease their prices by 0.10 or 0.05 euros. Price durations for price points are longer (Table A) and small price changes are less frequent.However, even if we restrict our sample to prices not ending in "9", "0", or "5", we still find that the distribution of price changes exhibit an $M-$shape.

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Table D1: Distribution of Price Changes and Average Duration by the Last Digit of Price

	Diesel		Unleaded Petrol	
	% of price trajectories	Average price duration (in days)	% of price trajectories	Average price duration (in days)
0	29.0	6.7	29.5	7.0
1	2.9	4.2	3.0	4.6
2	4.2	4.1	4.2	4.4
3	3.6	4.3	3.6	4.7
4	7.2	4.2	7.3	4.4
5	8.8	4.5	8.8	4.9
6	3.7	4.4	3.8	4.7
7	4.1	4.4	3.9	4.7
8	4.7	4.7	4.6	5.0
9	31.8	4.9	31.4	5.3

Note: We consider prices including all taxes, the proportion of price trajectories is computed as the ratio of number of price trajectories ending with one figure on all price trajectories and we compute the simple average duration. The last digit is the third one.

Appendix E: Relating local competition, demand and gas stations characteristics to estimated parameters.

From a theoretical point of view, local competition, in particular the number of supermarkets, should have a negative effect on prices (Zimmerman 2012). Due to competitive pressures, gas stations may reduce the share of other operating costs, which has a positive impact on β the share of wholesale product in marginal costs. According to Vita (2000), demand variables like the population density or car density have an ambiguous impact on prices: a high density should decrease the gasoline demand since alternative transportations are more developed and transportation costs from wholesale rack to retailers are reduced. This would imply that the share of wholesale gasoline in marginal costs is higher since the share of other costs including transportation or labor costs is lower. However, in high population density areas, land rental is also more costly, which should have a negative impact on β_i. Finally, services in gas stations like stores or restaurants may imply to hire more employees, which may decrease the share of wholesale gasoline in marginal costs.

Tables B and C present results of OLS regressions relating β_i to those variables. First, as expected, we find that supermarket density has a positive significant impact on β_i for both diesel and unleaded petrol. The degree of pass through of gasoline stations on motorways is also lower on average since the degree of competition on motorways is lower and the share of operating costs may be higher (in particular rents). Gas stations using pricing points strategies tend to have significant lower β_i, competitive pressures may be lower for those stations since they are able to set attractive prices. Local demand characteristics have a significant impact on β. Gas stations in urban areas show larger pass-through coefficients than in rural areas, which is consistent with lower transportation costs or effect of lower demand. In Paris, we find lower values of β_i since the share of operating costs (e.g. rents) might be much higher. Another indicator of density is the share of households owning at least one car, this variable has a positive effect on β_i, which is also quite consistent with theoretical predictions on the effect of higher demand. Unexpectedly, the unemployment rate tends to slightly increase β_i. Finally, services offered in gas stations have a negative impact on the share of wholesale gasoline in costs. The presence of a store or car services may increase the share of labor cost which has a negative impact on β_i. However, for unleaded petrol, the presence of a restaurant in the station has an unexpected positive effect.

Tables B and C also show further results on the determinants of heterogeneity of α_i among gas stations.[11] We find that the degree of competition has a negative significant effect on α_i; Zimmerman (2012) obtains negative effect of the density of

[11] To control for possible differences among gas stations in "other costs" which reduce the estimated mark-ups, we add controls for brand characteristics and regional dummies. We also

supermakets on retail gasoline prices whereas Hosken *et al.* (2008) do not find that competition indicators have a significant effect on stations' margins. Population or car density has a small effect: for diesel, markups are larger in Paris (where gas stations are rare and the share of operating costs may be large due to rents) whereas in big cities, for unleaded petrol, markups are lower since gas stations density is higher. Car density has a negative effect on margins for diesel since a high car density may also be related to a high density of gas stations. Finally, the presence of a store, car services or high quality gasoline in the station tend to have a positive effect on markup because those services might lead to a more pronounced product differentiation.

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run alternative specification including β as exogenous variables to correct for possible cross section differences in cost structure and results remain similar.

Table E1: Determinants of random (S,s) model parameters - Diesel

	α_i	β_i	$\frac{\lvert s_i\rvert+\lvert S_i\rvert}{2}$
Distance to closest supermarket gas station (km)	0.010^{***} (0.003)	-0.001^{***} (0.000)	0.059^{***} (0.011)
Number of supermarket gas stations within 10 kms	-0.008^{***} (0.002)	0.001^{***} (0.000)	-0.030^{***} (0.007)
Motorway	0.064 (0.059)	-0.105^{***} (0.002)	0.369^{*} (0.206)
Pricing points	-0.025 (0.027)	-0.010^{***} (0.001)	1.324^{***} (0.096)
Households owning a car (%)	-0.003^{**} (0.001)	0.000^{***} (0.000)	0.009^{*} (0.005)
Urban area with 20,000 to 100,000 inhabitants	-0.032 (0.035)	0.004^{***} (0.001)	-0.339^{***} (0.123)
Urban area with more than 100,000 inhabitants	-0.078 (0.056)	0.004^{***} (0.002)	-0.205 (0.196)
Paris and its region	0.294^{***} (0.077)	-0.019^{***} (0.003)	-0.232 (0.271)
Unemployment rate (%)	-0.003 (0.004)	0.001^{***} (0.000)	0.041^{***} (0.015)
Pump working with credit/debit cards	-0.062^{**} (0.026)	0.002^{***} (0.001)	-0.098 (0.090)
Store	0.161^{***} (0.036)	-0.006^{***} (0.001)	0.325^{***} (0.125)
Restaurant	0.017 (0.034)	0.002^{*} (0.001)	-0.455^{***} (0.118)
Car services	0.051^{*} (0.028)	-0.003^{***} (0.001)	-0.264^{***} (0.099)
Premium gasoline	0.092^{**} (0.038)	-0.002 (0.001)	-0.097 (0.132)
Adjusted R^2	0.496	0.796	0.322
Number of observations	7,456	7,456	7,456

Note: Columns report the OLS estimates for the time-varying (S,s) model parameters. "Urban area with less than 20,000 inhab." is used as reference. "Motorway" is a dummy variable equal to one if the gas station is on a motorway. "Pricing points" is a dummy variable equal to one if more than 95% of prices end by 0 or 9. Local unemployment and share of households owning at least one car come from the census 2008. "Pump working with credit/debit cards", "Store", "Restaurant", "Car services" and "Premium gasoline" are dummy variables equal to one if the service is provided in the gas station. Dummy variables for 28 different brands and 22 regions are included. Significance level : *** 1%, ** 5%, * 10%.

Table E2: Determinants of random (S,s) model parameters - Unleaded petrol

	α_i	β_i	$\frac{\lvert s_i\rvert+\lvert S_i\rvert}{2}$
Distance to closest supermarket gas station (km)	0.009* (0.005)	−0.000*** (0.000)	0.047*** (0.013)
Number of supermarket gas stations within 10 kms	−0.004 (0.003)	0.001*** (0.000)	−0.038*** (0.010)
Motorway	0.471*** (0.094)	−0.066*** (0.003)	1.621*** (0.269)
Pricing points	0.141*** (0.044)	−0.008*** (0.001)	1.685*** (0.125)
Households owning a car (%)	−0.002 (0.002)	0.000* (0.000)	−0.003 (0.007)
Urban area with 20,000 to 100,000 inhabitants	−0.048 (0.056)	0.005*** (0.002)	−0.817*** (0.160)
Urban area with more than 100,000 inhabitants	−0.146 (0.089)	0.008*** (0.002)	−0.779*** (0.255)
Paris and its region	0.211* (0.124)	−0.011*** (0.003)	−0.468 (0.353)
Unemployment rate (%)	0.001 (0.007)	0.001*** (0.000)	0.047** (0.020)
Pump working with credit/debit cards	−0.056 (0.041)	0.001 (0.001)	−0.055 (0.118)
Store	0.089 (0.058)	−0.008*** (0.002)	0.444*** (0.166)
Restaurant	−0.003 (0.055)	0.004*** (0.001)	−0.731*** (0.156)
Car services	0.020 (0.045)	−0.003** (0.001)	−0.219* (0.130)
Premium gasoline	0.135** (0.061)	−0.002 (0.002)	0.022 (0.174)
Adjusted R^2	0.277	0.659	0.339
Number of observations	7,262	7,262	7,262

Note: Columns report the OLS estimates for the time-varying (S,s) model parameters. "Urban area with less than 20,000 inhab." is used as reference. "Motorway" is a dummy variable equal to one if the gas station is on a motorway. "Pricing points" is a dummy variable equal to one if more than 95% of prices end by 0 or 9. Local unemployment and share of households owning at least one car come from the census 2008. "Pump working with credit/debit cards", "Store", "Restaurant", "Car services" and "Premium gasoline" are dummy variables equal to one if the service is provided in the gas station. Dummy variables for 28 different brands and 22 regions are included. Significance level : *** 1%, ** 5%, * 10%.

Appendix F: Estimation results for alternative price rigidity models

Table F1: Estimation results - Fixed adjustment cost model

	Diesel				Unleaded petrol			
	Mean	Q25	Q50	Q75	Mean	Q25	Q50	Q75
α	2.63	1.56	2.39	3.68	-1.34	-2.58	-1.50	-0.27
β	0.78	0.72	0.80	0.84	0.69	0.64	0.69	0.73
σ_1	2.72	2.31	2.63	3.04	3.38	2.81	3.25	3.80
γ_S	4.29	3.22	4.02	5.16	5.54	4.12	5.20	6.67
γ_s	-4.36	-5.19	-4.15	-3.39	-5.37	-6.38	-5.07	-4.11

Note: We estimate for each individual gas station a fixed (S,s) model and then compute statistics on the parameter estimates we obtained. We consider all gas stations with more than 300 individual observations of prices (excluding Sundays).

Table F2: Estimation results - Calvo model

	Diesel				Unleaded petrol			
	Mean	Q25	Q50	Q75	Mean	Q25	Q50	Q75
α	2.23	1.22	2.00	3.20	-2.03	-3.18	-2.14	-1.02
β	0.77	0.71	0.79	0.83	0.67	0.62	0.68	0.72
σ_1	1.82	1.62	1.78	1.98	2.22	1.92	2.16	2.43
λ_1	-1.27	-1.43	-1.29	-1.12	-1.35	-1.51	-1.36	-1.20
λ_2	1.28	1.13	1.29	1.44	1.28	1.14	1.29	1.43

Note: We estimate for each individual gas station a "Calvo" model and then compute statistics on the parameter estimates we obtained. λ_1 (resp. λ_2) is the intercept triggerring randomly price increases (resp. decreases). We consider all gas stations with more than 300 individual observations of prices (excluding Sundays).

Table F3: Estimation results - Fixed by duration (S,s) model

		Diesel				Unleaded petrol			
		Mean	Q25	Q50	Q75	Mean	Q25	Q50	Q75
α		2.58	1.52	2.37	3.64	-1.37	-2.61	-1.52	-0.30
β		0.78	0.72	0.80	0.83	0.69	0.64	0.69	0.73
σ_1		2.64	2.25	2.56	2.94	3.30	2.75	3.17	3.69
γ_s	1 day	12.10	3.49	4.73	6.67	8.38	4.24	5.94	8.59
	2 days	6.77	3.13	4.11	5.79	7.27	3.91	5.28	7.46
	3 days	5.36	2.94	3.82	5.33	6.68	3.85	5.04	6.93
	4 days	4.96	2.90	3.81	5.18	6.64	3.89	5.09	6.82
	5 days	4.97	3.02	3.96	5.26	6.81	3.99	5.24	7.02
	6 days	4.31	2.61	3.55	4.77	5.99	3.64	4.82	6.40
	7 days	5.60	3.08	4.20	5.76	7.24	4.06	5.40	7.39
	$>$ 7 days	3.83	2.86	3.66	4.75	5.17	3.93	4.93	6.27
γ_S	1 day	-9.72	-6.16	-4.40	-3.29	-7.59	-7.86	-5.54	-4.12
	2 days	-6.50	-5.37	-3.94	-3.04	-6.67	-6.88	-4.99	-3.87
	3 days	-5.12	-4.99	-3.73	-2.92	-6.07	-6.40	-4.69	-3.63
	4 days	-4.86	-5.08	-3.95	-3.13	-6.11	-6.42	-4.91	-3.85
	5 days	-5.19	-5.33	-4.24	-3.42	-6.30	-6.55	-5.11	-4.08
	6 days	-4.81	-5.15	-4.10	-3.24	-5.75	-6.20	-4.75	-3.69
	7 days	-6.14	-6.01	-4.69	-3.73	-7.19	-7.16	-5.43	-4.25
	$>$ 7 days	-4.57	-5.32	-4.48	-3.78	-5.33	-6.34	-5.24	-4.29

Note: We estimate for each individual gas station a time-varying (S,s) model without idiosyncratic shock ε_2 and then compute statistics on the parameter estimates we obtained. We consider all gas stations with more than 300 individual observations of prices (excluding Sundays).

Appendix: Internships, major choices and labor market outcomes of French Grandes Ecoles graduates

Table A1: Major determinants - probit average marginal effects

	Response Probit	Response Probit
Survey 2010	0.012	0.010
(Ref. Survey 2009)	(0.049)	(0.049)
Survey 2011	0.035	0.034
	(0.045)	(0.044)
Survey 2012	0.014	0.012
	(0.048)	(0.048)
Survey 2013	-0.145***	-0.151***
	(0.048)	(0.046)
18 months after graduation	-0.147***	-0.145***
(Ref. 6 months after graduation)	(0.028)	(0.028)
30 months after graduation	-0.165***	-0.160***
	(0.039)	(0.039)
3rd year Major: Finance	-0.178***	-0.173***
	(0.042)	(0.032)
2nd year inverse rank	0.001**	0.001**
	(0.001)	(0.001)
Woman	0.021	
	(0.033)	
Foreign citizenship	0.016	
	(0.045)	
Age in 2nd year	0.020	
	(0.016)	
Admitted in 2nd year	-0.035	
	(0.039)	
One parent professional	0.016	
or retail trader	(0.045)	
Full year intern.	0.003	
	(0.037)	
2nd year minor: applied maths	0.009	
	(0.045)	
Observations	945	945
Pseudo R^2	0.090	0.086
Baseline probability	0.713	0.713

Note: Standard errors in parentheses data, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 2 are the average marginal effects of the probit model for the response to the survey.

Table A2: Wage determinants - Unweighted and weighted regressions

	(1)	(2)	(3)
	Log wage	Log wage	Log wage
	OLS	OLS	OLS
	Unweighted	Weighted	Weighted
Full year intern.	0.063*	0.056	0.060*
	(0.033)	(0.034)	(0.034)
Experience	0.102***	0.104***	0.103***
	(0.014)	(0.015)	(0.015)
3rd year Major: Finance	0.088***	0.083***	0.086***
	(0.030)	(0.031)	(0.031)
2nd year inverse rank	0.081	0.083	0.087
	(0.053)	(0.053)	(0.053)
Admitted in 2nd year	-0.039	-0.048	-0.051*
	(0.029)	(0.031)	(0.030)
Women	-0.015	-0.021	-0.017
	(0.028)	(0.028)	(0.028)
Foreign citizenship	-0.111***	-0.109***	-0.107***
	(0.034)	(0.033)	(0.033)
Working abroad	0.383***	0.388***	0.395***
	(0.087)	(0.085)	(0.084)
Second job	0.035	0.032	0.033
	(0.042)	(0.041)	(0.040)
Public sector	-0.092**	-0.089**	-0.088**
	(0.039)	(0.041)	(0.040)
Fixed term contract	-0.083*	-0.089*	-0.092*
	(0.045)	(0.049)	(0.049)
Admitted in 2nd year	-0.039	-0.048	-0.051*
	(0.029)	(0.031)	(0.030)
Constant	10.566***	10.591***	10.587***
	(0.064)	(0.065)	(0.064)
Observations	444	444	444
Adjusted R^2	0.364	0.377	0.385

Note: Standard errors in parentheses data, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column1 reports the unweighted OLS estimates. Column 2 reports the weighted OLS estimates with an unequal weighting scheme. Column 3 reports the weighted OLS estimates with a stratified weighting scheme. Wages are deflated using price consumption index. The variance-covariance matrix is clustered with the student identifier. Year of graduation used as control variables.

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