

**THE EFFECTS OF PENSION-RELATED
POLICIES ON HOUSEHOLD SPENDING**

2019

Susana Párraga Rodríguez

**Documentos de Trabajo
N.º 1913**

BANCO DE ESPAÑA
Eurosistema



THE EFFECTS OF PENSION-RELATED POLICIES ON HOUSEHOLD SPENDING

THE EFFECTS OF PENSION-RELATED POLICIES ON HOUSEHOLD SPENDING

Susana Párraga Rodríguez (*)

BANCO DE ESPAÑA

(*) Banco de España. Directorate General Economics, Statistics and Research, Alcalá 48, 28014 Madrid, Spain. Email: susana.parraga@bde.es. The author declares that she has no relevant or material financial interests that relate to the research described in this paper. This work was conducted at University College London before the author joined the Bank of Spain. The views expressed in this paper are therefore those of the author and do not necessarily reflect those of the Banco de España. I am very grateful to the Banco de España for their financial assistance, and for comments and advice from, and discussions with, Clodomiro Ferreira, José María Labeaga, Ralph Lueticke, Francisco Martí, Morten Ravn, Raffaele Rossi, João Santos Silva, and Ernesto Villanueva

The Working Paper Series seeks to disseminate original research in economics and finance. All papers have been anonymously refereed. By publishing these papers, the Banco de España aims to contribute to economic analysis and, in particular, to knowledge of the Spanish economy and its international environment.

The opinions and analyses in the Working Paper Series are the responsibility of the authors and, therefore, do not necessarily coincide with those of the Banco de España or the Eurosystem.

The Banco de España disseminates its main reports and most of its publications via the Internet at the following website: <http://www.bde.es>.

Reproduction for educational and non-commercial purposes is permitted provided that the source is acknowledged.

© BANCO DE ESPAÑA, Madrid, 2019

ISSN: 1579-8666 (on line)

Abstract

This paper estimates the impact of pension-related policies on household spending. The identification strategy exploits the deviation in pensioner income and expenditure caused by the introduction of a new pension system during the 1980s and 1990s in Spain and constructs a new narrative series of legislated pension changes. I present a variety of estimates, some of them imply that increases in the average pension have a roughly one-for-one effect on pensioner spending. The strongest effects are on the pensioners with the highest levels of expenditure, income, and wealth. Estimates for different categories of expenditure indicate that benefit increases trigger these pensioners to spend more on durables. At the same time, pension-related policies targeted to pensioners with low income levels seem to affect the spending on non-durables and necessities such as food positively.

Keywords: fiscal policy, narrative public pensions changes, household expenditure.

JEL classification: D12, H31, H55, N14.

Resumen

Este documento estima el impacto de las políticas de pensiones en el gasto de los hogares. La estrategia de identificación utiliza la desviación de los ingresos y del gasto de los pensionistas causada por la introducción de un nuevo sistema de pensiones durante las décadas de los años 1980 y 1990 en España, y construye una nueva serie narrativa de cambios legislados en el sistema de pensiones. Se presentan una variedad de estimaciones, algunas de las cuales implican que los aumentos en la pensión media tienen un efecto de, aproximadamente, uno por uno en el gasto de los pensionistas. Los efectos más significativos afectan a los pensionistas con altos niveles de gasto, de renta y de riqueza. Estimaciones para diferentes categorías de gasto indican que el aumento de las prestaciones induce a estos pensionistas a gastar más en bienes duraderos. Al mismo tiempo, las políticas de pensiones dirigidas a los pensionistas con menor nivel de renta parecen influir positivamente en el gasto en bienes no duraderos y de primera necesidad como alimentación.

Palabras clave: política fiscal, narrativa de cambios en las pensiones públicas, gasto de los hogares.

Códigos JEL: D12, H31, H55, N14.

1 Introduction

Concerns about financial sustainability and the projected population aging have led to a renewed interest in pension systems. As a result, the research agenda has turned attention to the economic impact of social security benefits. Recent papers have made progress in the quantification of the aggregate effects of pension-related policies (see, for example, Romer and Romer 2016, and Parraga-Rodriguez 2016, 2018); however, the question remains what the direct impact of pension-related policies on household spending is. Filling this gap in the literature is essential because estimates of the effects of pension-related policies on the aggregate economy cannot fully explain the distributional impacts of changes in benefits.

This paper presents evidence on the impact of unexpected permanent changes in public pensions on net recipients (pensioners). Consistent with the lifecycle/permanent-income hypothesis of consumption theory, increases in the average pension have a roughly one-for-one effect on pensioner spending. To gain insights into the components of these high responses, I look into the implied impact for different categories of expenditure, as well as across the distribution of pensioners' spending, income and wealth.

My findings have significant implications for the growing macroeconomic literature on the heterogeneous effects of fiscal policy. First, the results seem to be driven by the consumption, income and wealth-rich pensioners. Second, not only do I find different effects across the distribution of household expenditure, but also by categories of expenditure. Pensioners at the bottom of the distribution of expenditure appear to spend increases in benefits mostly on non-durables and necessities such as food, whereas pensioners at the top allocate a substantial fraction of the benefit increases to durables. Third, a simple classification concerning net worth suffices to obtain significant heterogeneous effects out of benefit increases regarding wealth. The latter contrasts with the recent theoretical advances in heterogeneous agent models that fiscal policy is more effective the more significant the proportion of liquidity-constrained households (Kaplan and Violante 2014, Eggertsson and Krugman 2012). The results also contrast with the empirical works that study the heterogeneous effects of temporary tax changes (Cloyne and Surico 2016, Misra and Surico 2014). A comparison with these papers points to the lower outstanding debt

of the elderly, compared to working-age individuals, as the primary explanatory factor for the divergences.

The estimation method exploits the significant departure in the spending path of pensioners caused by the introduction of a new welfare state legislation in Spain during the 1980s and 1990s. In the framework of difference-in-difference (DD) models for multiple policy interventions, I estimate the impact of changes in the average pension, an aggregate fiscal variable, on household-level spending from survey data corresponding to 1977q2-1997q1. This strategy circumvents the lack of data on household income in the survey covering the essential pre-treatment years. The DD framework is an attractive method because it recognizes that in the absence of random assignment, treatment and control groups may differ for many reasons. The eligibility to collect a pension defines the treatment and control groups such that only the treated (pensioners) receive the benefits increase. The control group consists of working-age households net contributors to the pension system. A sufficiently high age-threshold for working-age households guarantees the comparability of both groups. Compared to the standard DD exercise though, all households might be potentially affected by aggregate shocks such as pension-related policies. Even so, I show that if pensioners and workers are similarly affected by increases in aggregate income, time effects will efficiently control for the general equilibrium effects in the regressions.

Like Stephens (2003), I study the consumption behavior of Social Security beneficiaries. Stephens (2003) estimates the consumption response to the regular arrival of Social Security checks exploiting the fact that participation in the Consumer Expenditure Survey is independent of the date the checks arrive. However, whereas he studies how recipients react to known check amounts, this paper estimates the consumption response to surprise changes in pensions. Indeed, he excludes any observations within the window around the arrival of checks with unknown amounts that incorporate cost-of-living adjustments (COLAs). Wilcox (1989) and Romer and Romer (2016) exploit the variation in the COLAs to estimate the aggregate effects of benefit increases. Compared to them, this paper does a better control of the potential estimation bias due to the positive correlation between inflation adjustments, current macroeconomic conditions, and benefit increases.

The identification strategy to deal with the reverse causality in the relationship between pension-related policies and household spending is to use a narrative approach. This paper presents a new narrative series of legislated changes in public pensions adopted in Spain between 1979 and 1997. Marked by the Spanish Constitution of December 1978, the narrative covers the implementation of a new Social Security system in Spain, with significant variation in pension-related policies. The result of the narrative analysis is a record of likely exogenous pension-related policies that will be used as an instrumental variable for aggregate expenditure in public pensions to estimate the effects of changes in the average allowance.

Last but not least, the high estimates for pensioners' spending out of benefit increases point to a significant impact on aggregate consumption and output. However, to correctly quantify the aggregate effects of benefit increases, one needs to account for the general equilibrium effects that could have amplified or diminished the initial impulse to spending. Still, this paper provides sound evidence to support the implicit assumption made in research using aggregate data that recipients of social security benefits have high marginal propensity to consume, especially for durables expenditure.

The next section gives details on the construction of the new narrative series of exogenous pension-related policies and the household expenditure surveys. Section 3 presents the empirical design. Once section 4 establishes a robust and significant impact of unexpected permanent changes in public pensions on pensioners' spending, section 5 reports the heterogeneous effects of pension-related policies on household spending. Section 6 offers concluding remarks.

2 Dataset

2.1 Institutional background

The estimation sample corresponds to the implementation of a new welfare state in Spain. In this new system, public pensions became the most important component of public expenditure. According to OECD, the average expenditure on public pensions in Spain during 1985-1997 was 10.6% of GDP, above the average for OECD countries (8.9%). During this period, public pensions accounted, on average, about 25% of total public spending.¹

¹The Ministry of Finance Macroeconomic Database of the Spanish Economy.

Public pensions in Spain are a pay-as-you-go system of defined benefits. There exist two primary modalities, contributory pensions covering the contingencies of old age, disability and survivors, and non-contributory pensions for old age and disability. Social contributions carry the weight in the financing of contributory pensions. For example, in 1980 contributions for social insurance accounted for 89.4% of total revenues into the Social Security, while in 1990, social contributions accounted for 71.9% of total revenues, a fall mainly explained by the sharp rise in the government transfers to the Social Security to finance the public health care. The benefits amount depends on the number of years a worker contributes to the system and the contribution basis. Benefit amounts below a minimum threshold are topped-up to guarantee a minimum pension. Pensions cannot exceed a maximum benefit amount established by the law either.

Within contingencies, old-age pensions account for more than 60% of total expenditure on public pensions (see Appendix A1). In the sample period, the normal retirement age was 65 years old, although early retirement was possible without penalty in certain professions, and with a penalty for all other employees. The old-age benefit ratio (the ratio between the average old-age pension benefit and the economy-wide average wage) increased substantially during the sample period. In 1980 the benefit ratio was 28%, compared to 32% in 1997.² Moreover, the replacement rate at retirement (the average first pension as a share of the economy-wide average wage at retirement) was above 80% by 1995.³ The little importance of private pension plans in Spain can partly explain these high replacement rates.⁴

On the other hand, those who can prove need but have not made sufficient contributions are eligible for a non-contributory pension. These pensions cover the contingencies of old age and disability. General taxes and government transfers finance non-contributory pensions. Taking into account those who receive a minimum pension supplement, about 30% of beneficiaries receive a non-contributory pension.⁵ Regarding the entitlements, non-contributory pensions pay subsistence benefits, as highlighted by the fact that the minimum old-age pension for those over 65 did not match the legal minimum wage until 1990.

²Data sources: Ministry of Labor and Social Security and Ministry of Economy.

³See Monasterio et al. (1996).

⁴According to the earliest data available from OECD pensions database, and as documented by Luengo-Prado and Sevilla (2013), total assets in private pension funds were about 2% in 2001, compared with 75% in the US.

⁵Data from the National Institute of Social Security.

2.2 Narrative series of pension-related policies

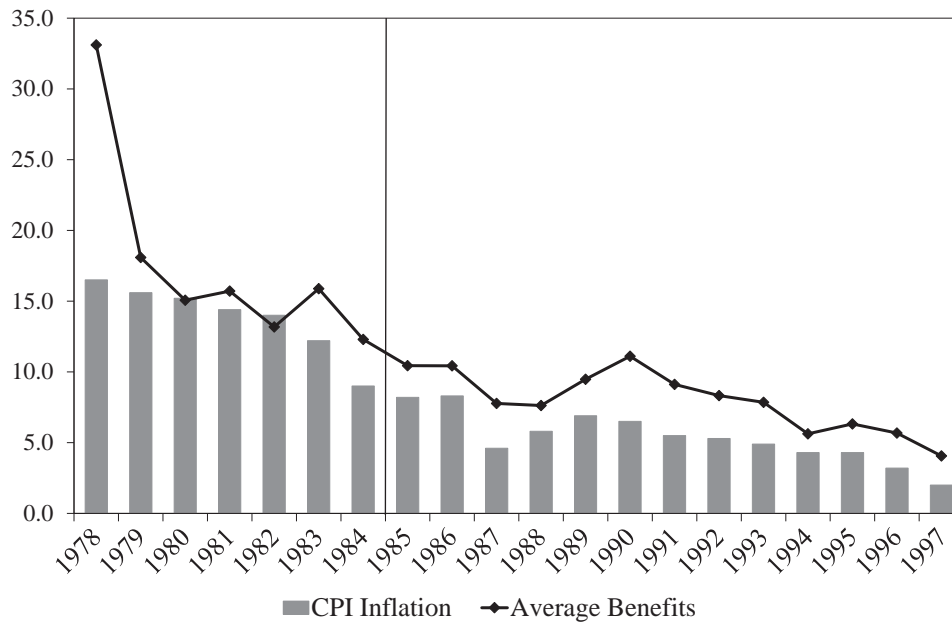
Directly using benefits income to estimate the impact of pension-related policies on household spending would ignore endogeneity problems. In other words, pension-related policies that respond to short-run macroeconomic developments fail to isolate the effect of other shocks affecting household spending, therefore producing biased estimates. In this regard, inflation adjustments are a special feature of Social Security benefits compared to other forms of public spending. To the extent that inflation might reflect the recent economic evolution, it is necessary to purge the benefits series from changes directly attributable to inflation.

Figure 1 plots the inflation rate as measured by the year-to-year change in CPI December together with the evolution in the average benefits per pensioner. The close evolution of inflation and the growth of average benefits suggests a potential endogeneity problem. Put it differently, the high correlation between both series makes a necessity to account for the relationship between inflation and pension adjustments to determine the causal effects of pension-related policies. However, growth in average benefits tends to exceed the inflation rate with a changing gap. Using the year-on-year change in CPI November instead would result in similar gaps.⁶ These frequent and heterogeneous gaps suggest that other factors besides inflation determine increases in average benefits.

Another endogeneity problem relates to how the timing of policies might depend on the available fiscal space, which in turn, partly depends on the economic cycle. However, the evolution of expenditure in public pensions does not seem to be conditioned by the balance of the Social Security budget. During the 1980s and early 1990s, many policies were implemented to improve the generosity and coverage of the Social Security system, but without an equivalent counterpart to the financing of the same. In other words, with the establishment of democracy takes place an accelerated formation of the welfare state in Spain. Carreras and Tafunell (2010) document that while the welfare state represented only 13 percent of GDP in 1970, its weight increased to about 25 percent by 1985. Among expenditure items, public pensions more than doubled their share of GDP, reaching a ratio of 10 percent of GDP in 1985, compared to 4 percent in 1970, and absorbing a quarter of total public expenditure.

⁶Until 1986 pensions indexation effectively based on the year-on-year change in CPI December. After, indexation used the CPI November.

Figure 1: Inflation and Public Pensions, 1978-1997



Notes: The plots show average benefits per recipient and CPI December. The vertical line indicates the waves of the expenditure survey. Authors calculations using data from the Annex of the Economic and Financial Reports to the Social Security Budget, and National Institute of Statistics.

The identification strategy to control for reverse causality in the relationship between pension-related policies and household spending is to use a narrative approach. This paper presents a new narrative series of legislated changes in public pensions adopted in Spain between 1979 and 1997. Because current macroeconomic developments, including inflation or the available fiscal space, may determine pension-related policies, I exclude interventions with short-run stabilization goals as the primary motive. As a norm, I also discount the inflation rate from all benefit increases. The result of the narrative analysis is a record of likely exogenous pension-related policies to be used as an instrument for retirement income. Because pension-related policies affect household spending through changing their disposable income, one could use the exogenous pension-related policies to identify the exogenous variation in household income and estimate the effects on household spending. However, the household survey corresponding to the early period of the estimation sample, the *Encuesta Permanente de Consumo* (EPC), does not report information on household income. Even so, using this survey is essential to guarantee a sufficient number of periods before the treatment begins. Then, since the goal is to estimate the effects of pension-related policies on household expenditure, the narrative series will be used instead to instrument aggregate expenditure in public pensions to estimate the impact of changes in the average allowance.

Multiple sources have been used to identify and analyze the policy changes. The Economic and Financial Reports to the State Budgets (IEF by its abbreviation in Spanish) - a detailed account of the economic context, the government goals and spending policies involved in the Budget Law- served as a starting point for identifying significant policy changes.⁷ These reports have been digitalized and are available online since 2000.⁸ One can find hard copies of reports for earlier fiscal years in the library of Banco de España.⁹ After identifying the laws, I used the Spanish Official State Bulletin (BOE for its acronym in Spanish) to collect the legislative texts of the enacted laws. News articles, mainly from the digital archive of *El País* where occasionally used to fill information gaps.¹⁰ This line of action makes it very likely to identify the majority and most significant policy measures.

The narrative analysis categorizes policies as either exogenous or not exogenous based on their motivation. Examination of the introductory comments of each bill, press releases, media news, and different reports was used to assess the motivation of each measure. I establish three exogenous motivations based on similar classifications by Romer and Romer (2016), Cloyne (2013), and Gil et al. (2017). First, “ideological” changes due to philosophical reasons such as fairness or redistribution. For example, the introduction of new benefits for the social integration of the disabled in 1984, or the introduction in 1985 of a war pension for those who fought for the losing side in the Spanish Civil War. From all types of measures with an ideological motivation, rises of minimum and non-contributory pensions stand out because of their quantity and importance in the budget. Discussions about these measures in the legal texts and reports often involve motivations such as *“to improve the level of social protection, political will to increase minimum pensions above the CPI, equation of the minimum pensions to the legal minimum wage, equation of the minimum survivors [widows] pension with the amount of the minimum individual retirement pension,”*.

⁷The Economic and Financial Reports analyze the main characteristics and figures of the State Budget. The Reports consist of three parts. The first part describes the economic context of the State Budgets. The second part describes the government priorities and main characteristics of the Budgets, as well as an analysis of the spending policies that make them up. The third part analyses the budgets for different agencies integrated into the general government, including the Social Security.

⁸<http://www.sepg.pap.minhafp.gob.es/sitios/sepg/es-ES/Presupuestos/InformeEconomicoFinanciero>

⁹For missing volumes one can use as an alternative the proposal of State Budget.

¹⁰This is done introducing in the archive’s searcher keywords related to a particular policy and in a window around the vicinity of the event.

Second, increases in benefits other than minimum and non-contributory pensions constitute another category of “purchasing power” improvements beyond (or below) the annual change in CPI. Due to concerns about the correlation between inflation and the short-run macroeconomic conditions, the impact of all policies employs as a benchmark the annual increases in pensions equal to the inflation rate. Besides, the calculations also net out the evolution in the number of beneficiaries and the higher value of new pensions compared to existing ones.

As shown in Figure (1), often there is a gap between the inflation rate and the growth rate of average benefits. One explanation could be that policymakers repeatedly miscalculated their inflation forecasts, which, by law, were used to set the annual inflation adjustments. The latter seems unlikely given that the number of continuous and significant gaps extends for more than a decade. Additionally, during another entire decade starting in 1999 indexation of contributory pensions perfectly matched CPI inflation. Therefore, these gaps may also reflect discretionary increases in pensions because of ideological motivations and with the aim of improving the purchasing power of pensioners. The empirical strategy is to use these measures in the base-line estimates while addressing any remaining doubts about their exogeneity in the robustness section. As shown later on, the exclusion of these measures does not significantly influence the estimates obtained.

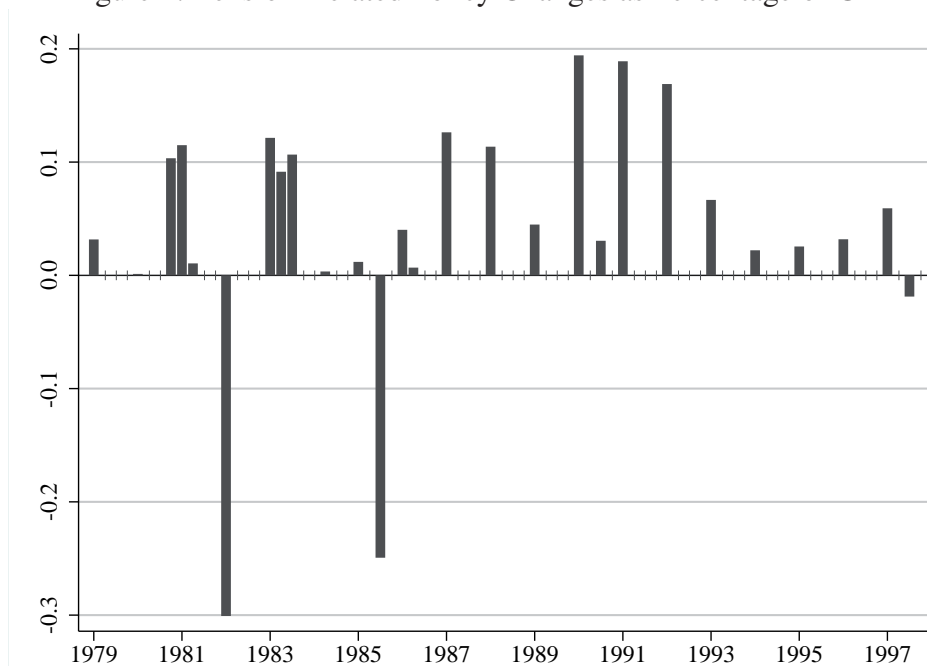
Finally, we find policies caused by a structural reform with long-run objectives such as fiscal consolidations or to address challenges stemming from demographic phenomena. This category also includes reforms and changes in expenditure the result of a court ruling. Unlike other spending items, there tend to be fewer reductions in pensions to improve the budget balance in the short term. For example, in the IEF for 1990 we find “*The content of the State Budget for 1990 has an important social character [e.g., spending in public pensions], although this does not imply neglecting the conservative economic policy applied in recent years*”. In the IEF for 1993 similar remarks were made, adding “*The effort of spending restraint has been concentrated on the other spending items [instead of social spending such as pensions], with reductions or increases that in practice represent a freeze in nominal terms.*” Other reports and for different years include similar remarks.

The previous three categories cover most of the contingencies regarding pension-related policies. For the remaining policies, those in compensation for other fiscal actions, or to boost economic growth in the short run have been classified as endogenous because of their counter-cyclical motivation. Reassignments of some benefits over time, which do not imply a change in coverage, have not been considered as a policy.

In total, 64 exogenous pension-related policies are identified, with 47 policies taking place after 1985. While there are roughly the same amount of actions by type of benefits (contributory, minimum, non-contributory, and other pensions), ideological reasons seem to be the most common motivation. Combining all the changes for the different benefits yields a total of 27 economic shocks, of which only three correspond to net reductions of benefits. The narrative series has been quantified in annualized terms, that is, as the additional expenditure equivalent to one fiscal year as a result of a legislated change in pensions. The final series is in real and per beneficiary terms, dividing by the CPI general index with base 1992 and the total number of pensions (contributory and non-contributory). The cumulative yearly impact of pension changes is on average 4,552 pesetas (about 28 euros) per beneficiary, at 1992 prices. This impact rises to 6,676 pesetas per beneficiary when we only account for net increases. All official sources indicate the date of enactment; I follow Romer and Romer (2016) and consider that a policy is effective when beneficiaries cash in the pensions. Then, policies are assigned to the quarter when they become active. For example, while Congress usually passes the State Budget in the last quarter of the year, we attribute budget policies to January of the following year (1st quarter).

For comparability with the literature, Figure 2 shows the narrative series normalized by GDP. Compared to other fiscal instruments, the budgetary impact of increases in pensions is rather small. For example, Gil et al. (2017) estimate a yearly amount of permanent tax cuts in Spain of about 0.25% of GDP (0.22% for increases) between 1986 and 2015. In contrast, pension increases have an average impact of 0.07% of GDP for a similar sample from 1986 to 2014. The appendix A2 contains further details on all identified policies during the narrative analysis corresponding to 1979-97. Details of the Spanish pension system were provided earlier in the text.

Figure 2: Pension-Related Policy Changes as Percentage of GDP



Notes: The plot shows the combined changes in contributory, minimum, non-contributory, and other pensions as percentage of GDP corresponding to 1979-1997

Sample restrictions - While the complete narrative analysis covers legislated changes in public pensions adopted until 2014, this paper restricts attention to policy actions during 1979-1997. This sample choice responds to various reasons. First, starting in 1998 the new household expenditure surveys are not directly comparable with the earlier waves. For example, the survey corresponding to 1998-2005 uses a different classification for goods and services and scheme of household participation. While households report on all their spending in the surveys until 1997, in the survey corresponding to 1998-2005 households alternate between reporting all their spending (full participation) and their infrequent spending such as durable goods purchased during the three months before their interview (partial participation). The scheme of participation for a household participating eight consecutive quarters would have been G G g g G G g g, where G denotes full reporting and g denotes partial participation. Since 2006 the expenditure survey has changed to annual frequency. Second, the starting date in the first quarter of 1979 guarantees a period of relatively institutional stability. The death of the dictator Francisco Franco in November 1975 marked the beginning of a transition period to a new democratic regime. In this historical context, the Spanish Constitution of December 1978 established the basis for the current system of Social Security. Therefore, the narrative analysis starts within the initial years of a new welfare system and covers a period with substantial variation in spending

Table 1: Predictability Tests

	(1) Full sample	(2) ECPF85
F-test	0.480	0.875
Granger Causality	0.310	0.543
Ordered Probit Enactment Date	0.390	0.235
Ordered Probit Implementation Date	0.388	0.659

Notes: p-value of predictability tests for the amount and timing of exogenous pension-related policies. Full Sample from 1979q1:1997q4; ECPF85 sample in column 2 from 1985q1:1997q4. Macro variables include the log of GDP, CPI inflation, the average implicit personal income tax rate, and the short-term interest rate. All regressions include four lags of the macro variables and the narrative series.

on public pensions. Later, one finds a relatively infrequent policy activity. For example, during an entire decade starting in 1999 indexation of contributory pensions perfectly matched CPI inflation. Finally, data restrictions and the availability of reports from government agencies also determine the starting point.

Predictability tests - Next, I analyze the predictability of the exogenous pension-related policies to past macro developments in output, inflation, other fiscal policies or the monetary policy stance. These are standard tests that the literature on narrative fiscal changes has proposed as a suggestive alternative to the non-testable exogeneity assumption (see, for example, Gil et al. 2017). The results in Table 1 uniformly indicate that macro developments do not help forecast decisions on pension-related policies or their magnitude. First, I fit a linear regression for the narrative series on four lags of the same and the log of GDP, CPI inflation, the average implicit personal income tax rate, and the short-term interest rate to perform an F-test of the joint significance of the macro covariates. Second, using the same covariates and lag length, I perform a VAR Granger causality test. Next, following Mertens and Ravn (2012), I test whether the *decisions* on pension-related policies can be forecasted by past information using an ordered probit approach. These tests require constructing an indicator variable based on the enactment date rather than the implementation date that takes the value 1 (-1) at the announcement of benefit increases (cuts), and 0 otherwise. The predictability of pension-related policy announcements is assessed using a likelihood ratio test on ordered probit regressions with and without the macro covariates. The third and fourth rows of Table 1 show the p-value for these tests of the macro aggregates having no predictive power on the timing of legislated pension-related policies. The

fourth line performs a similar likelihood ratio test but defining the dependent variable at the implementation date instead. Again, the last two tests also include four lags of the covariates and the dependent variable.

2.3 The Spanish household expenditure surveys

The household level data for this paper comes from two quarterly Spanish household expenditure surveys, the *Encuesta Permanente de Consumo* (EPC) and the *Encuesta Continua de Presupuestos Familiares* (ECPF85). The EPC was carried out from the 2nd quarter of 1977 to the 4th quarter of 1983, while the ECPF85 corresponds to 1985-97. Therefore, the sample period spans from the 2nd quarter of 1977 to the 1st quarter of 1997. Based on personal interviews and expenditure diaries, these surveys report detailed information on households expenditure and other characteristics, albeit only the ECPF85 includes data on household income. The earlier survey interviewed about 2,000 families every quarter, while the ECPF85 interviewed about 3,200 families. In either survey part of the sample is renewed each period, which yields an unbalanced panel. While we observe some households for up to 24 quarters in the EPC, participation in the ECPF85 shortens to a maximum of 8 consecutive quarters.

The eligibility to collect a pension defines the treatment and control groups such that only the treated receive the benefit increases. Given that old-age pensions represent the bulk of social security benefits (see section 2.1 or appendix A1) and the need to minimize composition changes, retirement status defines the treatment and control groups. The treated consist of households with a reference person collecting benefits since their first interview in either of the surveys. Moreover, their age is restricted to be at least 58 years old at the time of their first interview. The age threshold is set lower than 65 to cover cases of early retirement. By definition, the treated include households collecting benefits and no longer paying social contributions. In other words, net recipients at the time of a pension-related policy. I will refer to this group as the “pensioners”.

The control group consists of households with a reference person in working-age but not entitled to a pension. Out of lack of a better name, I will refer to the control group as the “workers”. Even if the reference earner might not collect a pension, the household could nonetheless

receive benefits through other earners. Families with another earner older than 58 have been excluded to avoid this circumstance. I have also dropped households whenever the reference person is less than 25 or over 58 years old by their last interview. The lower-bound on age allows taking the education decision as given, while the upper-bound has a twofold purpose. Firstly, setting an upper bound lower than the standard retirement age minimizes composition changes attributable to pension-related policies, which would invalidate the grouping of households according to retirement status. Secondly, unlike the standard difference-in-difference (DD) exercise, the treatment affects both the treated (pensioners) and the non-treated (workers) because all households are affected by aggregate shocks like pension-related policies. Given positive multiplier effects, increases in benefits might lead to higher national income. A high age threshold for the control group makes it more likely that pensioners and workers are affected similarly by general equilibrium effects amid increases in aggregate income.

On the other hand, a pay-as-you-go pension system finances benefit increases with current social insurance contributions, which might induce workers to cut consumption. The control group includes households which are net contributors at the time of a pension-related policy. It includes families paying social security insurance but not entitled to any benefits. Thus, if benefit increases triggered changes in taxation that induced households to cut spending, one might worry that contemporary policy changes affecting workers could positively bias the DD estimates. However, there is little evidence of contemporaneous changes in the taxation of personal income. For example, the general social insurance rate mainly decreased or remained unchanged over the sample period.¹¹ Nevertheless, the robustness checks will include controls for other policy changes related to the taxation of personal income.

Table 2 compares pensioners and workers. The estimation sample has more observations for workers than pensioners. Workers also contribute a larger share into total expenditure, 77.8% compared to 23.4%. Regarding characteristics of the reference person (lines 3-5), pensioners and workers differ in characteristics other than age. Pensioners are on average less educated than workers and over three times more likely to be a woman. Not surprisingly either, workers

¹¹Between 1977 and 1984 the total social insurance rate fell 10 pp, followed by a stable 28.8% rate between 1985 and 1992. After that, although the tax rate temporally rose half percentage point in 1993-94, a new lower 28.3% rate since 1995 counterbalanced any previous hikes. Even so, the evolution of the rates might not entirely reflect the growth of the average contribution for social insurance, which decreased between 1977 and 1985 but showed an upward trend after that.

Table 2: Comparison of Characteristics of Pensioners and Workers

	Pensioners	Workers
Observations	45,115	89,550
Share in aggr. expenditure	23.4%	77.8%
Age	71	43
Spouse's age	66	40
College education	2.9%	9.7%
Female	28.8%	9.0%
Household size	2.4	4.1
Total expenditure	170,386 pts	189,834 pts
Non-durables	143,043 pts	150,933 pts
Durables	18,275 pts	30,238 pts
Food	54,849 pts	52,974 pts
Homeownership	81.2%	74.5%
Other real estate	9.4%	10.4%

Notes: The share in aggregate expenditure refers to the average share over 1977q2-1997q1. Age, education, and sex of the reference person. The household size is measured as the number of family members. Median expenditures per equivalent consumption units and quarter at 1992 prices. The percentage of home ownership does not distinguish whether a household has any outstanding debt.

have larger household sizes.¹² Regarding median levels of expenditure (lines 8-12), the table indicates that pensioners have a lower level of total expenditure. In relative terms, pensioners also spend on average a larger fraction of non-durables, while workers spend more on durables. Finally, lines 13-14 report that pensioners have a higher rate of homeownership. Although the surveys do not contain direct information about households wealth, real state variables indicate that pensioners are wealthier than workers. Moreover, although the surveys do not distinguish between mortgagors and non-mortgagors, the one-time surveys of 1980-81 and 1990-91 reveal that pensioners usually have a much lower outstanding debt than workers.

I define nine categories of expenditure: (1) food and non-alcoholic beverages; (2) shelter, which includes utilities and household services; (3) apparel and footwear; (4) transportation (public and private), vehicles, and communications; (5) leisure, which includes entertainment, meals away from home and hotels; (6) non-durables, which include all non-durable goods and services in (1)-(5), plus expenditure in tobacco and alcohol, education and other personal services (7) homeware, furnishings and fittings, including durables for the personal care; (8) durables, which include vehicles, therapeutic material, leisure durable goods, furnishings and

¹²See Attanasio and Weber 2010 and references therein for other papers documenting this fact.

other personal durables; (9) total expenditure as the sum of non-durables and durables. Table A2 in the appendix provides more details about the classification of expenses.

The adjustment for the reference period of expenditure deserves special mention. The surveys collect expenses with non-recurring purchases as the spending incurred during the last three months before the interview. Consequently, there may be a gap between the quarter of the meeting and the time of the expenditure. Following Pou and Alegre (2002), I reallocate infrequent spending to the previous quarter whenever the week of interview falls within the first three to four weeks of a quarter to correct for this gap.

Another concern relates to the zero expenditure records. The nature of observed zeros depends on the category and, among others, might result from non-participation, infrequency of purchases, or a corner solution. The estimation method assumes there is one primary source of zeros for each category. Moreover, it is presumed a positive expenditure for the consumption of necessities food and housing.¹³ Notice that as a result, total and non-durable expenditures are also always positive. The remaining categories pile up at zero with varying intensity. First, given the broad definition of leisure, zero records might result from a corner solution. In other words, one could assume that if the market value for these kinds of goods and services were to be below a reservation price, households would have spent a definite amount on leisure goods and services. Secondly, the infrequency of purchases refers to those categories with zero records because the survey period is too short compared to the rate of purchases. As long as goods have some durability and there are transactions costs, consumption will occur more frequently than purchases. The categories that might be affected by the infrequency of purchases include durables, apparel, health, transport and furnishings, homeware and fittings. The infrequent purchases could arise together with corner solutions; however, either option implicitly assumes participation. To simplify the analysis, I exclude categories with zero records most likely because of non-participation. Precisely, I do not estimate the effect of pension-related policies on tobacco, alcohol, health and education expenditures.¹⁴

¹³In practice, this implies deleting 5,234 observations (2.8% of the original sample).

¹⁴Another reason to disregard health expenditures is the substantial subsidies toward healthcare in Spain. As a result, this category in the EPC accounts on average only 25.3% of the National Accounts data, and, according to Pou and Alegre (2002), 45% in the ECPF85. These could explain the adverse effects on spending in health found during the initial stages of estimation. Luengo-Prado and Sevilla (2013) and Labeaga and Osuna (2007) reach similar conclusions.

Finally, the data match relatively well the national accounts figures; this is especially so for the ECPF85. On average, total expenditure in the ECPF85 accounts for more than 75% of consumption in the Spanish national accounts (see Pou and Alegre, 2002). The underreporting is stronger in the early survey though, and on average total expenditure in the EPC accounts only for 55% of consumption in the national accounts. By categories, food expenditure is particularly well represented in either survey and accounts on average for 88% of the national accounts figures. Nevertheless, the discrepancies between micro and aggregate data are frequent in some other countries (see, for example, Campos, Reggio, and Gracia-Piriz, 2013), while for the Spanish data the underreporting is not concentrated in any particular year.

3 Empirical design

This section presents a regression difference-in-difference model for multiple policy interventions to estimate the impact of pension-related policies, an aggregate fiscal shock, on household spending. Following Angrist and Pischke (2015), consider variants of the following specification

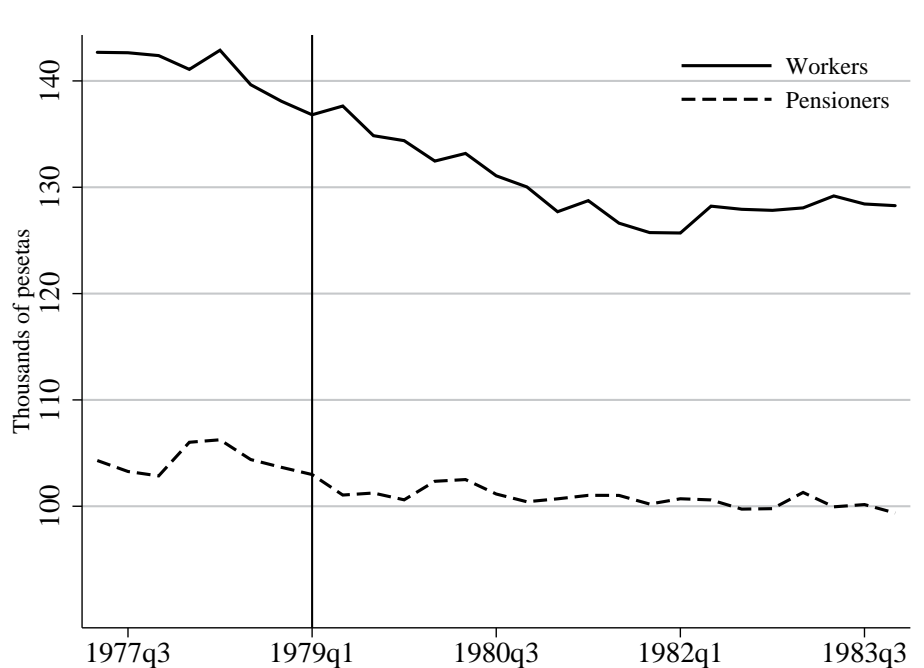
$$c_{it} = b_0 + b_1(P_{it} \times SS_t) + b_2P_{it} + b_3H_{it} + \sum_{j=2}^T \gamma_j yq_{jt} + u_{it} \quad (1)$$

where c_{it} refers to quarterly household expenditure in either of the nine categories described in section 2.3. Importantly, expenditure is transformed into (real) equivalent consumption units to account for the household size. P_{it} is a dummy variable indicating whether a household head is a pensioner, and SS_t represents pension-related policies measured as the (real) average pension. The interaction term $P_{it} \times SS_t$ indicates pensioners' observations at the time of a pension-related policy, while the coefficient b_1 captures the direct effects of changes in average benefits. The time effects γ_j , are the coefficients on the year-quarter dummies, yq_{jt} , indexed with a subscript t for quarter t and the index j to keep track of the period supplying the observations. H_{it} denotes a set of household characteristics which include age, sex and education attainment of the reference person.¹⁵

An essential identifying assumption is that absent the policies, the change in pensioners and workers expenditure would have shown common trends. Figure 3, which plots the median level

¹⁵Base categories are men and no schooling/primary education.

Figure 3: Median Total Expenditure at 1992 Prices for Pensioners and Workers



Notes: 5-quarters centered moving average. The vertical line indicates the Constitution of December 1978.

of expenditure for pensioners and workers, provides graphical evidence in support of this assumption. The data for this period comes from the early survey corresponding to 1977-83, the EPC. A reference line indicates the starting date of the multiple policies which have affected pensioners spending. Before 1979, the evolution of the median total expenditure of pensioners and workers suggests a common trend. Afterward, workers' spending shows a marked downward trend compared to the pensioners' reasonably constant level of consumption. Thus, one could argue that the introduction of a new welfare system and pension-related policies helped to maintain the consumption level of pensioners.¹⁶ Over time, because treatment effects emerge gradually, it is more difficult to distinguish so clearly the impact on spending of the multiple and continuous changes in public pensions.

Another concern is that pension-related policies may not be exogenous if policymakers time policies to economic developments in the short-run. The identification strategy uses the new narrative series as an instrument. The first stage takes the form

$$SS_t = \pi_0 + \pi_2 t + \pi_1 NV_t + v_t \quad (2)$$

¹⁶Several studies on the income and consumption distribution in Spain found that the development of the welfare system contributed significantly to the reduction of inequality during the transition to democracy. See, for example, Labeaga and Osuna (2007), Alcaide (2000), Calonge and Manresa (1997), or Bel (1997).

where the narrative series NV_t of pension-related policies is used as a source of exogenous variation in public pensions (see section 2.2) and a linear time trend is included to control for deterministic time effects. The estimation procedure takes the control function approach of Lee (2007), which adds a flexible real-valued function of \hat{v}_t as an additional explanatory variable to the linear equation (1) to retrieve the causal effect of pension-related policies on expenditure. The first stage can be estimated using least squares regression under the assumption that $E[v|NV] = 0$.

Regarding the estimation method, quantile regression is more robust to extreme values than estimates of the conditional mean, which is particularly relevant given the skewness in the distribution of household expenditure. In this sense, household level data and disaggregated expenditure categories often imply zero expenditure records. The impact of zero records might be attenuated defining broad categories; however, some categories like durables will still pile up at zero. Other estimates usually found in the literature instead estimate the conditional mean response of household expenditure to income changes. Compared to methods based on distributional assumptions to obtain either a likelihood function or an appropriate censored conditional mean, censored quantile regression is not sensitive to misspecification of the error distribution. Nonetheless, censored quantile regression implicitly restricts that the same stochastic process determines consumption and purchases. Relaxing this assumption usually implies to model a purchase probability dependent on household characteristics. It is not straightforward though, what observables one could exclude from the consumption decision and at the same time determine the purchases policy. At the same time, given a dependency of the purchases probabilities on household characteristics, when controlling for individual characteristics in the regressions, we are also partly controlling for the effects of infrequent purchases.

For τ in $(0,1)$, the linear equation (1) implies that the conditional τ -quantile function of household expenditure takes the form $Q_\tau(c_{it}|X_{it}) = X_{it}'\beta_\tau$, with $X_{it} = (P_{it}, SS_t, H_{it}, YQ_t)$, and YQ_t summarizes the year-quarter dummies.¹⁷ When latent expenditure is left-censored at zero,

¹⁷For τ in $(0,1)$, the τ th quantile of any real valued random variable X is that x that splits the data into proportions τ below and $(1 - \tau)$ above. Formally, the τ th quantile of X can be expressed as

$$Q_\tau(X) = \inf\{x : F(x) \geq \tau\}$$

where $F(x) = \text{Prob}(X \leq x)$ defines the cumulative distribution function of X . Like the distribution function, the quantile function provides a complete characterization of the random variable X .

we observe the maximum between zero and the right-hand-side of equation (1). Then, exploiting the equivariance of quantiles with respect to monotonic transformations, the conditional τ -quantile function of household expenditure takes the form $Q_\tau(c_{it}|X_{it}) = \max\{0, X'_{it}\beta_\tau\}$.

To complete the model, assume that $Q_{U|SS,NV}(\tau|ss,nv) = Q_{U|NV,V}(\tau|nv,v) = Q_{U|V}(\tau|v) = \lambda(v)$. The first equality uses the relationship involved in the first stage, while the second hinges on the independence of the error term (u) and the instrument (NV), conditional on the variation in pensions not explained by the instrument that is captured by the residual of the first stage (v). Under this assumption, $b_1(\tau)$ can be estimated fitting linear quantile regressions of expenditure on the covariates and $\lambda(\hat{v})$, which represents a flexible real-valued function of the fitted residuals from the first stage. The procedure can also be implemented with censored data (Chernozhukov, Fernandez-Val and Kowalski, 2015).¹⁸

Finally, the (censored) quantile regression estimator is consistent when the data are dependent, as might be the case with repeated observations of expenditure taken on the same household (see, for example, Chen, Wei and Parzen 2003; Abrevaya and Dahl 2008). However, the standard asymptotic-variance formula or standard bootstrap methods to compute the estimators' standard errors are invalid. Instead, one could estimate clustered standard errors following the formulas of Machado, Santos Silva and Wei (2016). An alternative option would be to use the bootstrap method suggested by Abrevaya and Dahl (2008). Appendix A5 does a simple simulation exercise to compare the efficiency of both methods. The simulation implies similar confidence intervals for either method, but bootstrapped standard errors are unfeasible in practice. The bootstrap method is too time-consuming for the current application, which involves a considerable number of observations and includes numerous covariates in the regressions. Given the inclusion of aggregate variables in the regressions for household level data, one also needs to take into account the correlation among different households subject to the same macroeconomic shock in any given quarter. Petersen (2009), Thompson (2011) or Cameron et al. (2011) suggest a relatively simple procedure to compute two-way clustered standard errors.

¹⁸The user-written CQIV Stata command implements these methods of estimation. It is available at <http://EconPapers.repec.org/RePEc:boc:bocode:s457478>.

4 The direct effects of pension-related policies on household spending

As the starting point, I provide evidence of a significant and robust impact of unexpected changes in public pensions on net recipients' (pensioners) spending. This section focuses on the effects on spending at the median for different categories of expenditure, while section 4.1 presents a battery of robustness checks. Section 5 will shed light over the results looking into the effects at other points in the distribution of spending, as well as classifying pensioners by their income and wealth.

Table 3 reports the first-round effects of pension-related policies on different categories of household expenditure. The estimates represent the pesetas-change in the level of spending caused by a unit increase in the average pension. The standard errors reported in parenthesis are clustered by household and quarter. Column (1) reports the baseline estimates from (censored) median regressions. For brevity, the first stage results are omitted; however, it is crucial to verify that the first stage has sufficient predictive power. For example, the R-squared is 0.80 and the F-statistic for the weak instrument's test on the narrative series 11.52. Using only the early survey corresponding to 1977q2-1983q4, the R-squared reduces to 0.01, but the F-statistic remains slightly above three.

Benefits increases have a positive and statistically significant effect at the median on the majority of expenditures. The first line of the table presents the estimates for total expenditure. Every unit increase in average benefits causes total expenditure to raise about 0.8 pesetas. The estimated impact is close to one within the normal-based 95 percent confidence interval [0.51, 1.03]. Romer and Romer (2016) also find that permanent benefit increases in the US have a roughly one-for-one effect on consumer spending in the month the larger checks arrive.

The effects on total expenditure seem to be dominated by non-durables (second line), with a marginal effect for non-durables of about 0.6 pesetas and corresponding normal-based 95 percent confidence level interval [0.40, 0.78]. This dominance of non-durables though might only reflect that non-durables represent on average 80% of total expenditure. In contrast, the

Table 3: The Effects of Pension-Related Policies on Spending

	(1)	(2)	(3)	(4)
Total	0.770 (0.132)	0.914 (0.174)	0.352 (0.353)	0.338 (0.229)
Non-durables	0.590 (0.097)	0.668 (0.128)	0.184 (0.189)	0.131 (0.197)
Durables	0.095 (0.043)	0.246 (0.067)	0.168 (0.281)	0.339 (0.111)
Food	0.284 (0.042)	0.312 (0.054)	0.067 (0.103)	0.371 (0.139)
Shelter	0.397 (0.033)	0.608 (0.064)	-0.120 (0.130)	0.073 (0.03)
Leisure	-0.198 (0.017)	-0.192 (0.035)	0.147 (0.079)	-0.016 (0.015)
Apparel	0.085 (0.022)	0.100 (0.024)	0.051 (0.114)	0.231 (0.043)
Furnishings	0.014 (0.007)	0.108 (0.024)	0.073 (0.101)	0.051 (0.014)
Transport	-0.148 (0.016)	-0.068 (0.041)	0.113 (0.163)	-0.039 (0.015)
Estimator	LAD-DD	OLS-DD	FD	LAD-DD
Observations	130,623	134,665	108,057	25,106

Notes: Change in the level of spending caused by a unit increase in the average pension. Column (2) reports least squares estimates; column (3) reports results from a fixed effects regression; Columns (1) and (4) use median regression. Estimation sample from 1977q2 to 1997q1; estimates reported in column (4) use the early survey corresponding to 1977q2-1983q4. All regressions include time effects and controls for households characteristics. Robust standard errors clustered by household and time in parenthesis. Minimum number of observations across regressions by type of expenditure.

low level of spending in durables at the median yields a much lower impact between 0.01 and 0.18 pesetas (third line).

Lines 4 and 5 show that spending on the necessities food and shelter also increase significantly, respectively, 0.28 pesetas and 0.40 pesetas. These results are consistent with the findings of Stephens (2003). Stephens (2003) exploits the randomization of households in the Consumer Expenditure Surveys Diary to estimate changes in daily household consumption around the arrival date of social security checks. He finds an increase in the amount and probability of spending on non-durables and food. Notably, he finds that daily non-durable expenditures increase by \$1.40 during the immediate days after receipt of the checks, which is in line with my estimates. However, compared to Stephens (2003), my coefficients are generally lower due to

using quarterly data instead of daily. On the other hand, regarding other categories of durable expenditure, spending on apparel increases roughly by 0.1 pesetas (line 7), while line 8 shows an insignificant effect of benefit increases on furnishings at the median.

The negative coefficient for spending on leisure reported in line 6 requires further explanations. Although the negative impact presents a puzzle, it does not necessarily mean that pensioners cut leisure expenditure. For one, assuming leisure is a normal good, increases in income should translate into higher spending. Second, Stephens (2003) finds an increase in the spending amount of instantaneous forms of leisure around the arrival date of social security checks. Third, estimates for the dependent variable in logs show that benefits increases trigger a positive response of leisure expenditure. Precisely, an increase of 100 pesetas in the average pension causes a rise of 0.01 percent in leisure expenditures.¹⁹ Thus, the negative sign might indicate that while both groups increase their spending on leisure, workers increase their spending more than pensioners. The lower level of pensioners' expenditure compared to workers leads to a positive difference in the corresponding effects in percentage-terms. Similar arguments apply to the puzzling negative coefficients for transport expenditures (line 9).

However, the question remains why workers might increase their spending relatively more. One possibility could be that regressions for spending on leisure or transport do not control well for spillover effects. Estimates in Table 5 point to regional differences in spending on leisure. However, estimates in Table 4 prove to be robust to the inclusion of different controls for regional effects. Another possibility could be spillover effects due to older people giving money to their children (either inter-vivos or as a bequest), albeit this cannot be tested with the current data. Finally, the heavy subsidies for retirees to use public transport in Spain might have played a role in the negative effects on transportation. Nevertheless, the spillover effects and other factors imply a negative bias such that one can consider the coefficients as a lower bound estimate.

¹⁹Estimates for other expenditures in logs imply similar effects in terms of the underlying level of expenditure. A 100 pesetas rise in average benefits increases 0.07 percent total expenditure, which, given a median expenditure of pensioners of 170,386 pesetas, implies a 95 percent normal-based confidence interval between 0.89 to 1.41 pesetas in terms of the underlying level of expenditure. Non-durables rise 0.06 percent, or between 0.61 and 1.01 pesetas; durables would also increase 0.06 percent, or between 0.05 and 0.15 pesetas in terms of the underlying level of expenditure.

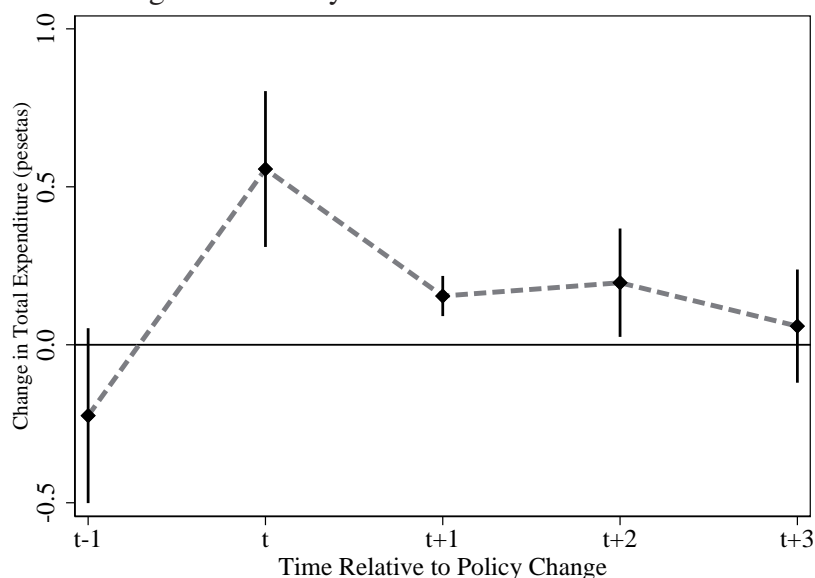
Next, we could compare our baseline estimates with the results from alternative specifications. First, using only the early survey corresponding to 1977q2-1983q4 yields very imprecise estimates (column 4). The large standard errors could be due to a much smaller estimation sample compared to the baseline; idiosyncrasies of the alternative sample could also have played a role. Secondly, least squares estimates allow for comparability with the literature. The least squares estimates (column 2) resemble their median regression counterparts (column 1) especially for the categories mainly made of non-durable expenditures. Overall, least squares estimates tend to be larger than their median regression counterparts. Finally, an alternative could be to estimate the model in first differences to deal with the fixed effects (column 3). First differences estimates are in general very imprecisely estimated. Here, it is worth noticing the impact that a limited variation of the covariates across households (aggregate policies and time dummies) could have for identification in any fixed effects specification.

4.1 Robustness checks

This section presents additional checks for the identifying assumptions discussed in section 3, including further exploration of the potential endogeneity of the narrative series, or the potential bias in the estimates due to other contemporary policies. I also investigate the existence of regional effects and local spillovers, as well as the possibility of cohort effects. The section ends with other standard checks.

Insofar that macroeconomic developments might affect inflation and the adjustments of pensions, there might exist endogeneity concerns about the new narrative series. In response to these concerns, the quantification of all exogenous pension-related policies has been against the benchmark of annual increases in pensions equal to the inflation rate (see section 2.2). The narrative analysis also excludes any pension-related policies with a clear countercyclical motivation. Moreover, the results in Table 1 reject that macro variables including output, inflation, and the short-term interest rate predict the timing or size of exogenous pension-related policies. Despite these actions, including inflation adjustments among the pension-related policies could still introduce a positive bias in the estimates. For example, the estimation sample includes a period of demand-side based economic growth driven by the entry of Spain into the European

Figure 4: The Dynamics of Benefit Increases



Notes: The plot reports the results for a median regression of total expenditure on the contemporaneous value, a lead, and three lags of benefits, household characteristics and time effects. The vertical lines report 95 percent confidence intervals. Sample 1977q2 to 1997q1; observations 40,498.

Community. This model tends to generate higher inflation and implies a positive relationship between inflation, consumption, and benefit increases. With these considerations in mind, column (2) of Table 4 reports the results of regressions that exclude inflation adjustments from the narrative variable. Column (1) reproduces the baseline estimates for convenience again. Comparing (1) and (2), the estimates across categories of expenditure are robust to the exclusion of these policies.²⁰

Given that the sample includes multiple periods, we could modify the specification to test the Granger causality of the policy changes (Autor, 2003). If the policy changes cause spending and not vice versa, introducing dummies for future policy changes in the specification (1) should not matter. At the same time, we can introduce lagged effects to investigate how causal effects evolve. The estimated leads and lags, running from one quarter ahead to three quarters behind, are plotted in Figure 4. The estimates show no significant effects the quarter before pension-related policies take place, a maximum effect upon impact, and gradually decaying effects in the following quarters.²¹ This pattern seems consistent with a causal interpretation of the results.

²⁰For brevity, Table 4 omits estimates excluding other categories of pension-related policies. Nonetheless, the results do not seem driven by any particular type of policy or motivation. Excluding ideological policies yields a coefficient of 0.782 and associated two-way clustered standard error of 0.131. Excluding reforms yields a coefficient of 0.779 with a standard error of 0.132.

²¹Using from one or two lags, instead of three, implied very similar dynamics. All the specifications yielded a statistically insignificant coefficient for the lead.

Time effects capture other sources of variation in household expenditure induced by other economic shocks. Importantly, time effects will capture the general equilibrium forces that determine the ultimate impact on consumer spending and output caused by any initial benefits increase. In a pay-as-you-go system for old-age-pensions, an increase in pensioners' disposable income comes at the expense of working-age individuals. Then, if benefit increases hurt working-age individuals through expected higher taxes, estimates of b_1 could be positively biased. On the other hand, if more generous pensions have a positive effect on working-age individuals through an increase in national income or expected pension wealth, then estimates of b_1 could be seen as a lower bound. The latter raises fewer concerns, while the former demands robustness checks to test for this possibility. In this line of reasoning, the possible existence of regional spillovers could also compromise the ability of the time dummies to control for general equilibrium effects.

Regarding other contemporaneous policies, little evidence indicates that the evolution of public pensions influenced tax policy over the estimation period. To begin with, the narrative exercise of Gil et al. (2017) finds but one simultaneous increase in Social Security benefits and contributions, that is an increase of 0.5% in social contributions the first quarter of 1993. However, the most substantial increase in social benefits attributable to the 1992-93 economic crisis was due to unemployment benefits rather than public pensions. Moreover, Gil et al. (2017) argue that the tax reforms of 1991 and later extensions also responded to European directives and past reforms. Other tax policies in 1992 and 1995 decreased revenues. Lacking a series of exogenous tax changes that go sufficiently back in time, column (3) of Table 4 includes as an additional regressor the previous year (real) average monthly contribution for social insurance.²² Reassuringly, the estimates appear robust to including this additional covariate.

Next, I explore the role of regional spillovers. The estimates could suffer from omitted variables bias if the effects of pension-related policies on consumption depend on the share of pensioners living in each region.²³ One could control for these possible regional spillovers

²²The real average monthly contribution for social insurance refers to total contributions for social insurance divided by the number of insured employees and the CPI base year 1992. The annual average contribution is divided by 14 to obtain a monthly equivalent. Sources: Table I.10 and I.28 from the Annex to the Economic and Financial Report of the Social Security budget of 2016; Table 12.31 from Carreras and Tafunell (2005); Spanish Statistics Office.

²³The one-time surveys of 1980-81 and 1990-91 show an unequal distribution of pensioners over the Spanish territory. For example, in regions such as Madrid, Cádiz or Navarra less than 20% of the population were pensioners. In contrast, pensioners represent more than 35% of the population in the regions of Ourense or Soria.

Table 4: Robustness Checks - Effects of Pension-related Policies on Spending

	(1)	(2)	(3)	(4)
Total	0.770 (0.132)	0.751 (0.133)	0.668 (0.204)	0.729 (0.13)
Non-durables	0.590 (0.097)	0.580 (0.097)	0.552 (0.153)	0.551 (0.097)
Durables	0.095 (0.043)	0.087 (0.045)	0.123 (0.071)	0.089 (0.041)
Food	0.284 (0.042)	0.271 (0.042)	0.314 (0.068)	0.279 (0.044)
Shelter	0.397 (0.033)	0.394 (0.034)	0.381 (0.047)	0.395 (0.034)
Leisure	-0.198 (0.017)	-0.193 (0.018)	-0.251 (0.031)	-0.207 (0.018)
Apparel	0.085 (0.022)	0.079 (0.023)	0.070 (0.037)	0.085 (0.022)
Furnishings	0.014 (0.007)	0.015 (0.007)	-0.001 (0.011)	0.013 (0.007)
Transport	-0.148 (0.016)	-0.145 (0.016)	-0.123 (0.021)	-0.161 (0.015)
Observations	130,623	130,616	130,615	130,622
Controls	Baseline	No Indexation	Tax Policy	Share Pensioners

Notes: Change in the level of spending caused by a unit increase in the average pension. Column (1) reproduces the baseline estimates; Column (2) excludes exogenous pension-related policies due to indexation. Column (3) includes the previous year average social insurance contributions to control for simultaneous tax policies. Column (4) includes the share of pensioners in the population of each region. All regressions control for time effects and household characteristics. Robust standard errors clustered by household and time in parenthesis. The minimum number of observations across regressions.

including an additional covariate that represents the share of pensioners in each region. Although the ECPF85 does not report information about the regions where households live, one could use other information included in the survey to construct such a variable. The procedure assigns the grossing-up factors to the different regions using the information we have of the theoretical number of households by regions and “zones”.²⁴ Column (4) of Table 4 reports estimates including as an additional covariate the share of pensioners in each region to capture

²⁴See “Cuadro 1: ECPF. Distribución espacial del número de secciones y viviendas muestrales” in INE (1988). According to López (1993), every Spanish region includes three “zones” (except Madrid and Catalunya with four zones, and Ceuta-Melilla with one zone) concerning the size of township based on Census information. The factors represent the ratio between population size and sample size for each “zone”. In total, there are 51 different grossing-up factors each quarter.

the potential regional spillover effects. Again, the alternative coefficients are very close to the baseline estimates in column (1).²⁵

An alternative test of the influence of regional spillovers would be to run regressions for semi-aggregated data at the regional level. Consider the following linear relationship of semi-aggregated expenditure on the explanatory variables

$$C_{gt} = b_0 + b_1 \times (\pi_g \times SS_t) + \sum_{s=2}^S \alpha_s G_{sg} + \sum_{j=2}^T \gamma_j y_{jt} + u_{gt} \quad (3)$$

where C_{gt} represents the average (real) expenditure of region g , and π_g represents the annual fraction of pensioners in each region g as a measure of the exposure of each group to the policies. In other words, we adjust the aggregate spending on public pensions by the importance of pensioners in each region. The regressions control for time effects and every region but one gets its own dummy variable, G_{sg} , indexed with a subscript g for region g and an index s to keep track of the group supplying the observations.

If regional spillovers were not an issue, we should find non-significant coefficients. Otherwise, a significant coefficient would indicate that the share of pensioners in the population influences the impact of benefit increases on spending. Except for leisure, the implied effects are insignificant and the standard errors large (Table 5). Regarding leisure expenditure, the estimates suggest that regions with a higher presence of pensioners tend to have higher levels of spending in leisure. However, estimates for the effects on leisure reported in Table 4 are close to the baseline. Overall, the results do not support different effects between regions.

On a related note, it is a possibility that cohort effects might drive the estimates. To explore the role of cohort effects pensioners were subdivided depending on their date of birth. Specifically, historical and social events occurred between the 1980s and 1930s suggest three subgroups: pensioners born before 1920s, born in the 1920s, and born in the 1930s. Importantly, this grouping guarantees an even split of households. Then, we can fit a regression that

²⁵Alternatively, one could introduce dummies for each of the 18 regions that constitute Spain, or an indicator for whether households live in a rural, intermediate, or urban habitat (urban habitats correspond to townships with more than 50,000 inhabitants and the capitals of province; intermediate includes townships with 10,000-50,000 inhabitants; rural habitats include townships with less than 10,000 inhabitants). It is reassuring that the implied effects are not statistically different from the baseline at the standard levels of confidence.

Table 5: Regional Differences in the Effects of Pension-Related Policies on Spending

	Total	Non-durables	Durables
$\pi_{sg} \times \hat{SS}_t$	-0.015 (0.253)	-0.016 (0.143)	-0.024 (0.064)
	Food	Shelter	Leisure
$\pi_{sg} \times \hat{SS}_t$	0.009 (0.066)	0.018 (0.082)	0.047 (0.020)
	Apparel	Furnishings	Transport
$\pi_{sg} \times \hat{SS}_t$	-0.034 (0.031)	0.011 (0.036)	0.011 (0.068)

Notes: Median regression estimates for the change in average regional expenditure caused by a unit increase in the average pension. A shorthand for the dependent variable stated on top. Details about the specification given in the text. Regressions include controls for time effects and regional dummies. Standard errors in parenthesis clustered by region. 1,341 observations; sample 1977q2 to 1997q1.

Table 6: The Effects of Pension-Related Policies on Total Expenditure by Cohort

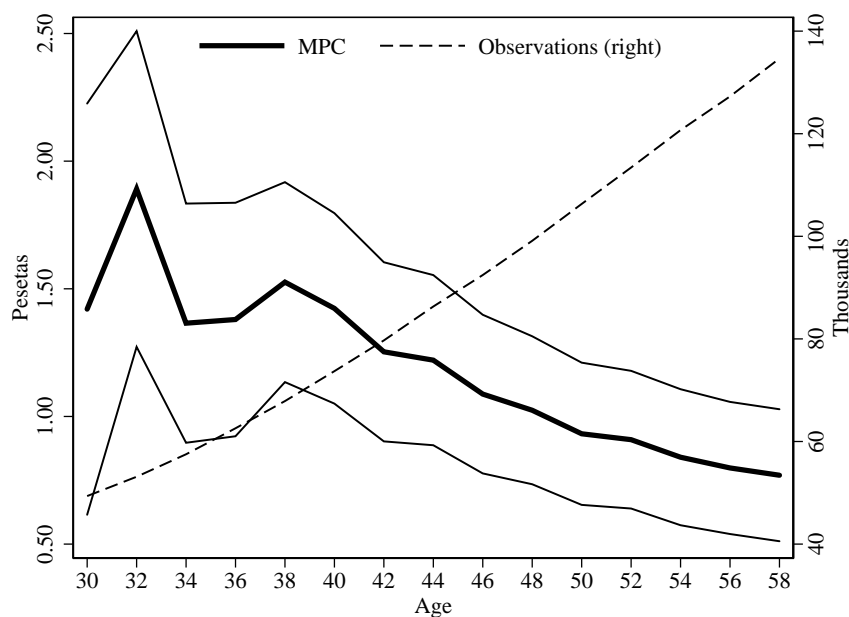
	(1) $\leq 1920s$	(2) 1920-29	(3) 1930-39	(4) $\leq 1930s$
b_s	-0.194 (0.268)	-0.171 (0.302)	0.432 (0.256)	-0.013 (0.418)
Households	3,963	4,089	4,411	12,463

Notes: Median regression estimates for the change in average total expenditure by cohort caused by a unit increase in average benefits. A shorthand for the cohort is stated on top. The regressions control for cohorts and time effects. Standard errors in parenthesis are clustered by cohort. Sample 1977q2 to 1997q1. Number of observations 429.

aggregates observations by cohort and replaces P_{it} with a set of cohort dummies. Notice that the sum of interactions between cohort dummies and aggregate expenditure on public pensions simplifies to $1[\leq 1930s] \times SS_t$, where $1[\leq 1930s]$ is an indicator function that takes the value of one for all cohorts formed by pensioners, and zero otherwise. The results reported in Table 6 disagree with different effects between cohorts. For each cohort, and on average, the coefficients are not statistically significant, with substantial standard errors.

Finally, it is also relevant to check the definition of the control group. To this end, one can fit alternative regressions that set different age thresholds for ‘workers.’ Figure 5 plots the estimated MPC of total expenditure for the alternative control groups, along with the 95 percent confidence level bands. The implied marginal effects out of a unit increase in the average pension decrease the higher the age threshold. Up to 39 years old the implied effect is the highest,

Figure 5: Marginal Effects on Total Spending for Alternative Control Groups



averaging 1.5 pesetas, although the estimation samples also involve a much lower number of observations. Between 40-49 years old the average response is roughly 1.2 pesetas. Including worker 50 and older the average response is roughly 0.8 pesetas.

The choice of a high age threshold for the control group primarily seeks to guarantee a correct control of general equilibrium effects. A condition for time effects to capture general equilibrium forces is that pensioners and non-pensioners are affected similarly by an increase in aggregate income. The lower the age threshold set for the control group, the more unlikely this assumption will be satisfied. Standard consumption theory predicts that ‘young workers’ will respond less to changes in expected retirement income than ‘older workers’ (see, for example, Attanasio and Rohwedder, 2003). Intuitively, an individual that is 30 years old by the time they report to either consumption survey should, on average, discount 35 years any expected change in their future retirement income! At the same time, remember that ‘older workers’ do not collect any money from the pension-related policies. The control group includes households with neither their reference person nor any other family member earning benefits. Hausman (2016), Parker et al. (2013) or Stephens (2003) used similar strategies for constructing their treatment and control groups.

5 The heterogeneous effects of pension-related policies

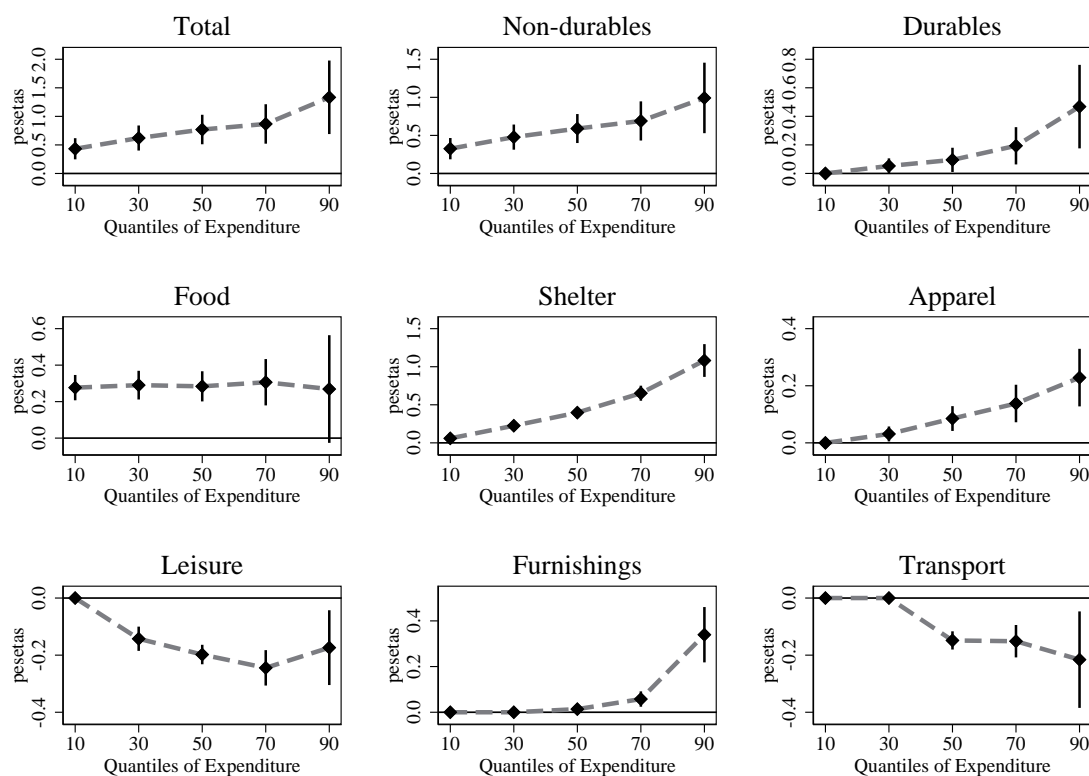
5.1 Other quantiles of expenditure

While estimates of the effects at the median are informative, one could also expect to find different effects for high and low-level spenders. This paper suggests fitting quantile regressions at other points of the conditional distribution of expenditures to investigate the different effects of pension-related policies on spending. Figure 6 shows the 0.1, 0.3, 0.5, 0.7 and 0.9th quantile estimates for total expenditure, and the sub-aggregated categories of non-durables, food and shelter. Censored quantile regression estimates are reported for the remaining categories. The estimates report the pesetas-change in the level of spending caused by a unit increase in the average pension. Notice that the median estimates correspond to the reported coefficients in Table (3). The plots also report normal based 95 percent confidence intervals.²⁶

The top row of Figure (6) shows the estimates for total expenditure and the broad categories of non-durables, and durables. The plots show that benefit increases affect consumption-rich pensioners the most, especially regarding durables. The left panel on the top shows that pensioners at the 0.9th quantile increase their spending on durables almost ten times more than those at the 0.3th quantile, 0.47 pesetas compared to 0.05 pesetas respectively. In a fairer comparison with observations further away from the censoring point, pensioners at the top quantile still increase their spending on durables five times more than those at the median (0.09 pesetas). Subcategories of durables such as apparel (right panel in the middle row) and furnishings, furniture and fittings (middle panel at the bottom) also show a rising effect with the level of expenditure. These patterns are in line with Misra and Surico (2014), who find a positive correlation between the tax rebates of 2001 and 2008 in the US and spending. Although the authors did not estimate the effects of the US tax rebates on a broad category of durables, they find similar upward patterns for spending on apparel and transportation. Regarding this last category, my results are entirely different from those of Misra and Surico (2014). In this regard, the discussion in Section 4 on the adverse effects on transportation extends to all quantiles of transport expenditure with a definite level of spending (right panel at the bottom).

²⁶Two-way clustered standard errors by household and quarter.

Figure 6: Effects of Pension-related Policies by Quantile of Household Expenditure



Notes: The dashed lines with marker report quantile regression estimates. Censored quantile regression estimates for durables, leisure, apparel, furnishings, and transport. Details on the specification can be found in the text. All the regressions control for time effects and household characteristics. Sample 1977q2 to 1997q1. Observations 130,624. The vertical lines reports 95 percent confidence intervals.

At the same time, the left panel on the middle row shows that pension-related policies also have substantial effects on the consumption-poor spending on food. For every unit increase in the average pension, pensioners at the bottom of the distribution of food expenditure increased their spending by almost 0.3 pesetas. Although the effects appear stable across quantiles of expenditure, the insignificant coefficient at the 0.9th quantile points to a decreasing effect of pension-related policies with the level of food expenditure. Regarding shelter, the other category that can be considered a necessity, the increasing effects with the level of expenditure (middle panel in the middle row) contrast with the little evidence of heterogeneity in utilities, household operations and housing found by Misra and Surico (2014).

5.2 The role of wealth

To investigate whether pension-related policies affect similarly households across the distribution of income and wealth one could estimate the different effects by wealth level. However,

Table 7: Characteristics of Pensioners, Grouping According to their Wealth

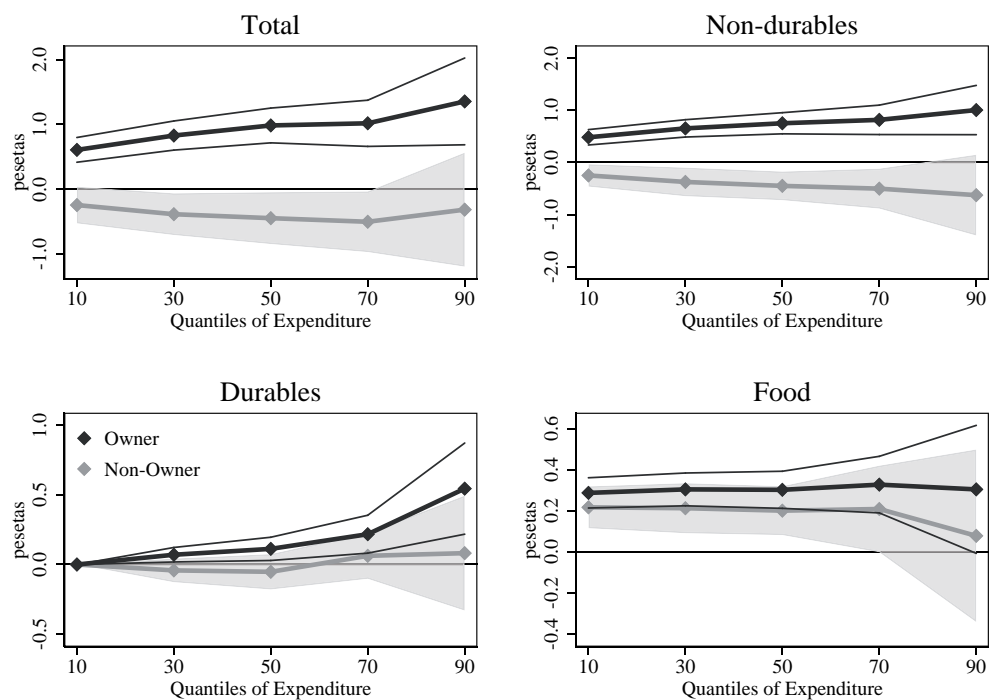
	(1) Owners	(2) Non- Owners	(3) Wealthy	(4) Income- poor	(5) Wealthy Hand-to- Mouth	(6) Hand-to- Mouth
Households	7,186	1,380	4,373	4,193	2,833	1,360
Observations	38,085	7,030	22,419	22,696	15,754	6,942
Home owner	98.05%	0.0%	97.96%	67.76%	97.62%	0.0%
Other real estate	14.86%	0.0%	21.10%	4.09%	5.90%	0.0%
Age	71	72	70	72	71	72
Spouse's age	66	66	65	67	67	66
Woman	26.46%	41.68%	24.72%	32.90%	28.98%	41.80%
No/Primary education	91.39%	89.74%	86.85%	95.37%	97.69%	90.10%
Total expenditure (pts)	176,108	142,323	209,626	137,457	135,736	141,733
Non-durables (pts)	147,625	117,853	176,206	114,947	113,988	117,197
Durables (pts)	18,651	16,572	22,069	15,123	14,608	16,408
Food (pts)	54,706	55,590	56,785	52,536	51,252	55,540

Notes: 'Owner' refers to pensioners that own real estate. 'Wealthy' ('income-poor') refers to pensioners with a level of capital income above (below) the median. 'Wealthy hand-to-mouth' refers to pensioners with a level of capital income below the median but owners of real estate; 'Hand-to-mouth' refers to pensioners with a level of capital income below the median and without real estate. Predicted probabilities have been estimated for observations without information on household income. Age, sex and education attainment of the reference person. Median expenditures per equivalent consumption units and quarter at 1992 prices.

the surveys do not contain direct information on the wealth of households. A second best could be to group pensioners depending on their real estate. Housing constitutes a significant component of households' wealth, particularly relevant in Spain and for older individuals.²⁷ Columns (1) and (2) in Table 7 compare the characteristics of pensioners that own some real estate with those that do not. 'Owners' refers to pensioners that own their primary residence or any other real estate such as a second home, parking garages, or office buildings. Both groups have reference persons with similar average age and educational attainment. Even so, non-owners are more likely to be a woman, report lower levels of expenditure, and spend relatively more on non-durables and food. Here, notice that the unequal distribution of pensioners between the two groups constitutes a caveat of this classification, with more than 80 percent of pensioners owning some housing.

²⁷For evidence using other surveys see, for example, Díaz-Giménez and co-authors (1997), Masier and Villanueva (2011), or Banco de España (2004). Bover and co-authors (2005) offer a good international comparison between the balance sheets of households in Spain, the United States, Italy, and the United Kingdom.

Figure 7: Effects on Spending by Quantile of Household Expenditure, Grouping of Pensioners Based on their Real Estate



Notes: The black lines report the effects on owners of real estate of a unit increase in the average pension. The light gray lines report the effects on pensioners that do not own any real estate. (Censored) Quantile regression estimates for (durables) total expenditure, non-durables, and food. Regressions include controls for household characteristics and time effects. The thin lines and shaded area report normal based 95 confidence level intervals. Estimation sample 1977q2 to 1997q1. Observations 130,625.

Figure 7 shows the different effects that a unit increase in the average pension has on pensioners grouped according to their real estate. For brevity, the figures report estimates for total expenditure, as well as spending on non-durables and durables. The figures also report estimates for food expenditure as a representative of spending in necessities and strictly non-durables. This selection completely summarizes the effects of pension-related policies on spending. The plots show that the effects are the strongest on ‘owners’ (black lines). Regarding durables (bottom-left panel), while both groups of pensioners are similarly affected, the estimates are not statistically significant for ‘non-owners’ (light gray lines). Nevertheless, both groups are similarly affected when it comes to spending on food.

Alternatively, we could use capital income earnings as a proxy for wealth. Everything else equal, the higher the level of capital income, the more likely a household holds high levels of assets and wealth. However, the EPC does not report information on household income either. Even so, we can use the information reported in the ECPF85 about household income

to estimate the probability that households have a level of capital income above the median on individual characteristics common to both surveys. Specifically, one could estimate the probit model

$$Pr[y_i = 1|X] = \Phi(X'_i\beta) \quad (4)$$

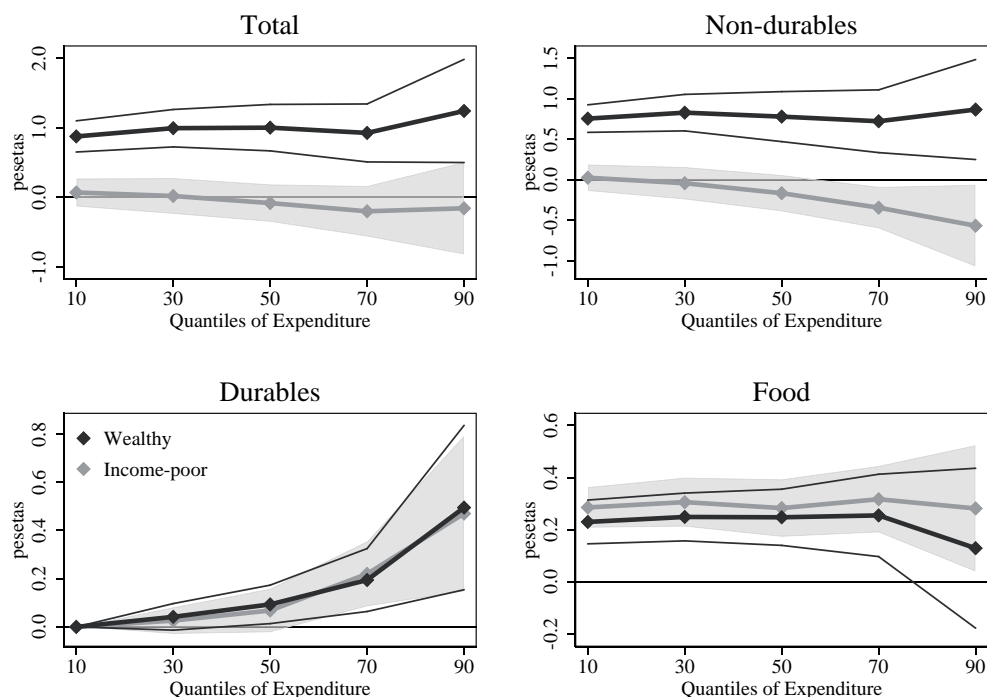
where $\Phi(\cdot)$ is the cumulative distribution function for the standard normal. The choice of a probit model has the obvious advantage of bounding the estimated probabilities between zero and one.²⁸ The dependent variable takes the value of one if a household average capital income is above the median, and zero otherwise. The regressors X include a polynomial of second order for the age of the reference person, a dummy for whether they are a woman or have no/primary education, the household size in equivalent consumption units, dummies for whether the household owns any real estate, and dummies for the region of residence.

The estimation sample includes all households of the ECPF85 whose reference person is a pensioner, which provides 37,886 observations, sufficient to correctly estimate the probability that a household has a level of capital income above of the median as a function of household characteristics. For example, the pseudo- R^2 was 0.23, and the histograms included in Appendix A4 show similar profiles of the empirical distribution of probabilities for either survey. Given the predicted probabilities, pensioners in the EPC were assigned a level of capital income above the median ($y = 1$) if their predicted probabilities were in the upper half of the distribution. This classification implies similar characteristics for pensioners with $y = 1$ in both surveys (Table A3) and suggests that the procedure yields reasonable estimates.

Given the probabilities, we can now make an alternative classification of pensioners based on their capital income as a proxy for their wealth. ‘Wealthy’ pensioners either reported an average level of capital income above the median or their estimated probability is in the upper half of the distribution. On the other hand, ‘income-poor’ pensioners either reported capital income below the median or had too low predicted probabilities. Columns (3) and (4) in Table 7 show an even split of pensioners between both groups. Compared to ‘income-poor’ pensioners, ‘wealthy’ pensioners are on average younger, more educated, less likely to be a woman, own

²⁸There was little difference with the predicted probabilities from a logit model. A simple linear probability model produced some predicted probabilities that were either negative or exceeding one.

Figure 8: Effects on Spending by Quantile of Household Expenditure, Grouping of Pensioners Based on their Capital Income



Notes: The black lines report the response of pensioners with capital income above the median to a unit increase in the average pension. The light gray lines report the response of pensioners with capital income below the median. The thin lines and shaded areas report normal-based 95 confidence level intervals. (Censored) Quantile regression estimates for (durables) total expenditure, non-durables, and food. Regressions include controls for household characteristics and time effects. Estimation sample 1977q2 to 1997q1; Observations 130,621.

more real estate and report higher levels of expenditure. Regarding items of expenditure, while both groups spend about the same share on durables, ‘income-poor’ pensioners spend more on food.

Figure 8 indicates that, under this alternative classification, the effects on the wealthy remain the strongest for total expenditure and non-durables, while both groups show similar responses when it comes to durables and food. Regarding this last category though, highlights the difference at the top quantiles of the distribution: not only do wealthy pensioners spend a lower share of the benefits increases on food, but the effects also become insignificant. The results are consistent with the findings of Giavazzi and McMahon (2013) on the effects of government spending policies on household spending. Misra and Surico (2014) also found that the income-rich had the most significant response out of the 2001 and 2008 tax rebates in the US. On the other hand, the results contrast with the findings of Parker et al. (2013) in their series of studies of the US tax rebates. Their estimates suggest the most significant spending response for the low-income, old age, and borrowing constrained households. Even so, their estimates

also suggest that there are no statistical differences in the spending response between low- and high-income groups.

A classification of pensioners based either on their real estate or their capital income only covers net worth partly. In turn, each type of wealth covers assets with different liquidity. On the one hand, capital income includes liquid returns in the form of interests, dividends, or rents.²⁹ On the other hand, real estate usually constitutes the most important illiquid asset for households. Based on these observations, Kaplan and Violante (2014) proposed a quantitative model that serves as a theoretical basis for the extensive empirical evidence that temporary changes in income can generate high MPCs (for example, Parker et al. 2013, Misra and Surico 2014, Cloyne and Surico 2016, Jappelli and Pistaferri 2014). A vital feature of the model is that besides hand-to-mouth households, it also features what has been called wealthy hand-to-mouth households. These are households that hold sizable amounts of illiquid wealth, yet deviate from the consumption behavior predicted by the permanent income hypothesis. The wealthy hand-to-mouth act as if they are constrained, but they would not appear constrained from the viewpoint of a classification based on net worth.

Ideally, one would like to have four groups of pensioners according to whether they have a high or low level of capital income and own or not any real estate. Because only 20 households had high capital income and no real estate, I assigned pensioners to the group previously called ‘wealthy’ whenever they have an average capital income above the median. Otherwise, I set two groups for those pensioners with no or little capital income. Those that own real estate will be called the ‘wealthy hand-to-mouth,’ while those that do not own any real estate will be called the ‘hand-to-mouth.’ The latter fits well the stereotype of liquidity constrained households in theoretical models (Cloyne and Surico, 2016). Moreover, one might worry that the number of debtors in either group might influence the results. However, pensioners usually have low rates of outstanding debts, as reported by the one-time surveys of 1980/81 and 1990/91.

²⁹Capital income in the ECPF85 defined as interest income from current accounts, savings accounts, and other accounts; Dividends and distribution of profits; Bond yields, bills of exchange and other disposals of equity; Income that companies pay to the members of their boards of directors, provided that they are not salaried employees; Income from temporary or life annuities; Yields of intellectual or industrial property (if the author is not the recipient of the profits, since in this case they will be considered as self-employment income); Rental of homes, premises and land; Participation of the owner or the beneficial owner in the price of subletting or transfer; Leasing, rights, business or mines.

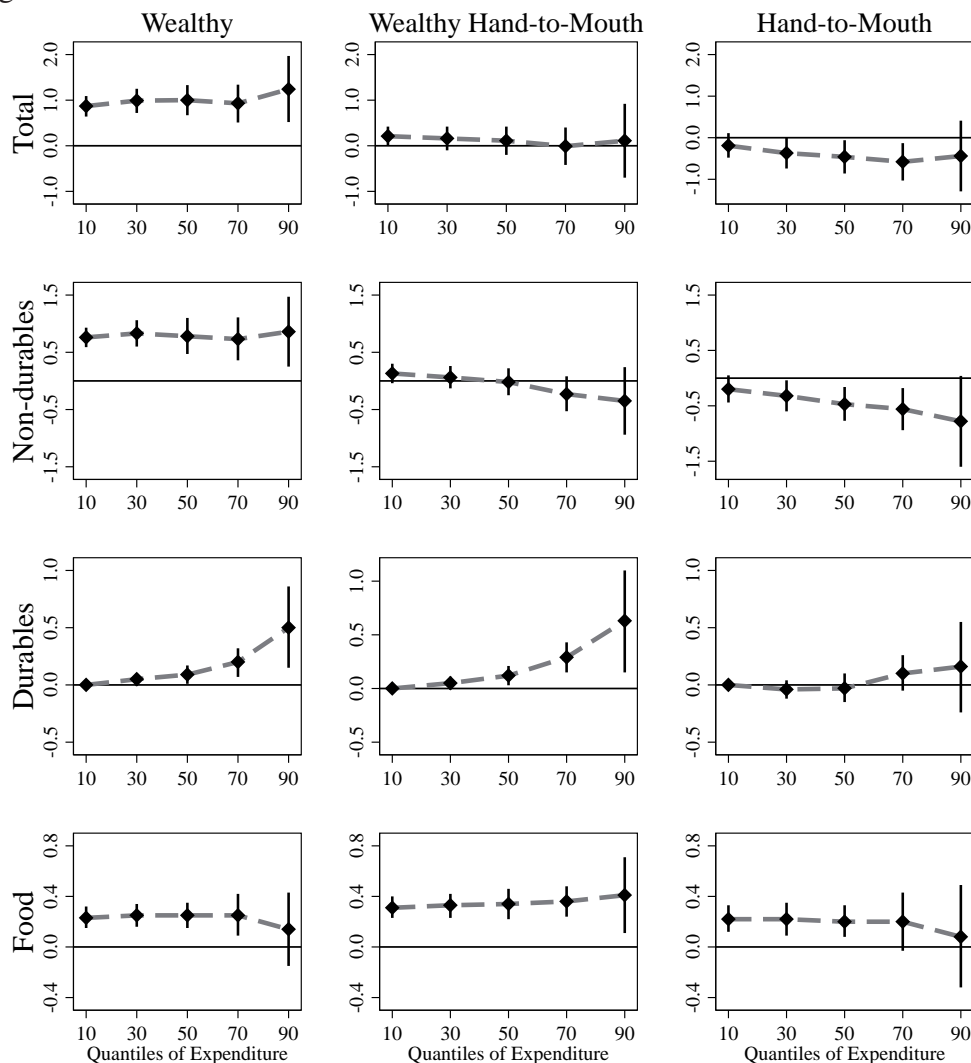
Table 7 offers a comparison between the three groups. Columns (3) and (5) compare ‘wealthy’ and ‘wealthy hand-to-mouth’ pensioners. Both groups show very similar homeownership rates, while ‘wealthy’ pensioners show, on average, higher rates of other types of real estate. ‘Wealthy’ pensioners are also younger, more educated, and report higher levels of expenditure. Regarding items of expenditure, while all groups spend about the same share on durables, ‘wealthy hand-to-mouth’ and ‘hand-to-mouth’ pensioners (column 6) spend a larger share on food. ‘Hand-to-mouth’ pensioners are the least numerous group, older, and more likely to be a woman.

Figure 9 shows the different effects that pension-related policies have on pensioners concerning their wealth. As before, the figure plots (censored) quantile regression estimates together with normal-based 95 percent confidence level intervals.³⁰ Overall, Figure 9 also suggests that the wealthy pensioners are the most affected by increases in benefits. However, the most affected group depends on the expenditure category, which partly agrees with the negative correlation between MPC and cash-on-hand found by Jappelli and Pistaferri (2014). While the ‘wealthy’ seem to be driving the results for total expenditure (top row) and non-durables (second row), the effects on durables (third row) are very similar for the ‘wealthy’ and ‘wealthy hand-to-mouth’ pensioners, and even slightly stronger on the latter. When it comes to food (bottom row), all groups show similar and significant effects, with the most substantial effects again for the ‘wealthy hand-to-mouth.’ In contrast to the other two groups, the effects on food expenditure for the ‘wealthy hand-to-mouth’ do not decline with the level of expenditure. Moreover, to the exception of durables, the effects are more homogeneous across quantiles of expenditure within groups.

Taken all together, the estimates for the three alternative groupings indicate that the response of wealthy pensioners is driving the results. In turn, this implies little support for the existence of strong voluntary bequests motives to save the benefit increases. The introduction of a consumption floor provided by the programs of the Social Security could have also reduced the incentive to save against bad income shocks. This contrasts with previous work on the savings of the elderly. For example, Jappelli and Pistaferri (2014) cite bequests motives, survival risk or large medical expenses as possible explanations for the savings of the elderly. De Nardi, French, and Jones (2010) can replicate the savings of the elderly, especially the richer ones, in

³⁰Standard errors clustered by household and time.

Figure 9: Effects on Spending by Quantile of Household Expenditure, Grouping of Pensioners According to their Wealth



Notes: The dashed lines with markers report the pesetas increase in expenditure to a unit increase in the average pension. The vertical lines report normal based 95 confidence level intervals. Definitions for the groups of pensioners given in the text. (Censored) Quantile regression estimates for (durables) total expenditure, non-durables, and food. Regressions include controls for household characteristics and time effects. Estimation sample 1977q2 to 1997q1; Observations 130,621.

the United States with a model that features these three elements. However, one should factor in the generalized tenure of real estate and the strong response of spending on durable goods before drawing any firm conclusions on the bequests saving behavior of Spanish pensioners.

Regarding the definition of ‘wealthy,’ a simple classification in terms of net worth appears sufficient to obtain significant heterogeneous effects of benefit increases. Thus, I find no compelling evidence to support adding layers of complexity to model pensioners in macro models, as proposed by the recent theoretical advances on heterogeneous agent models to study the effectiveness of fiscal policy (Kaplan and Violante 2014, Eggertsson and Krugman 2012). Moreover, a comparison with empiric applications that study the heterogeneous effects of tax changes

such as Cloyne and Surico (2016) or Misra and Surico (2014) points to the lower outstanding debt of pensioners compared to working-age individuals, as the primary explanatory factor for the divergences.

6 Concluding remarks

This paper estimates the impact of permanent exogenous changes in the average pension, and aggregate fiscal policy, on household level spending. The estimation strategy exploits the deviation in pensioner income and expenditure relative to working-age individuals caused by the introduction of a new welfare system at the onset of the democracy in Spain during the late 1970s. This paper presents a new narrative series of legislated pension changes in Spain corresponding to 1979q1-1997q4 to deal with the endogeneity issues related to benefit increases.

The results imply that pension-related policies have real direct effects on household spending. First, increases in the average pension have a roughly one-for-one impact on pensioner spending. Second, an exploration of the heterogeneous effects of benefit increases reveals the most robust results on the wealthy pensioners, with associated high levels of expenditure, income, and wealth. Moreover, given the low levels of outstanding debt owed by pensioners, the results suggest that using a simple classification concerning net worth suffices to obtain significant heterogeneous effects out of benefit increases. Last but not least, a detailed analysis of the impacts for different categories of expenditure indicates that benefit increases trigger the wealthy pensioners to spend more on durables. At the same time, pension-related policies targeted to the well off pensioners like improvements of the minimum pension, also affect the spending on non-durables and necessities such as food positively.

Finally, the results have significant policy implications. According to the latest OECD report on pension systems (Pensions at a Glance 2017), recent reforms addressing the financial sustainability of pension systems will lower pension benefits in many countries. The results in this paper predict that such policies will result in a substantial drop in pensioners' spending, with an associated fall in their welfare and living standards, while suggesting significant adverse effects on the aggregate economy. However, further advances in the study of the aggregate impact of transfer changes are needed to draw a firmer conclusion on the aggregate effects of pension-related policies.

References

Abrevaya, J. and C. M. Dahl (2008). The effects of birth on birthweight: evidence from quantile estimation on panel data. *Journal of Business & Economic Statistics*, 26:4, 379-397.

Angrist, J., and J. S. Pischke (2015). *Mastering 'metrics: The path from cause to effect*. Princeton University Press.

Alcaide J. (2000). La renta nacional de España y su distribución. Serie años 1898 a 1998. In “1900-2000. historia de un esfuerzo colectivo: cómo España superó el pesimismo y la pobreza”. Ed. by Juan Velarde Fuertes, Vol. 2, 375-450.

Attanasio, O., and S. Rohwedder (2003). Pension wealth and household saving: evidence from pension reforms in the United Kingdom. *American Economic Review*, 93 (5), 1499-1521.

Attanasio, O. and G. Weber (2010). Consumption and Saving: Models of intertemporal allocation and their implications for public policy. NBER Working Paper 15756.

Autor, D. (2003). Outsourcing at will: the contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics*, 21, 1-42.

Banco de España (2004). Encuesta Financiera de la Familias (EFF): descripción, métodos y resultados preliminares. Boletín Económico, Noviembre.

Bel, G. (1997). Desigualdad social, redistribución y Estado del Bienestar. *Sistema: Revista de ciencias sociales*, 137, 81-91.

Bover, O., C. Martínez-Carrascal, and P. Velilla (2005). La situación patrimonial de las familias españolas: Una comparación microeconómica con Estados Unidos, Italia y el Reino Unido. Boletín Económico Banco de España, April.

Calonge S. and A. Manresa (1997). Consecuencias redistributivas del estado del bienestar en España: Un análisis empírico desagregado. *Moneda y Crédito*, 204.

Cameron, C., J. Gelbach, and D. Miller (2011). Robust inference with multiway clustering. *Journal of Business & Economic Statistics*, 29 (2), 238-249.

Campos, R. G., I. Reggion, and D. García-Píriz (2013). Micro versus macro consumption data: the cyclical properties of the consumer expenditure survey, *Applied Economics*, 45 (26), 3778-3785.

Carreras, A. and X. Tafunell (2010). *Historia económica de la España contemporánea (1789-2009)*. Barcelona: Crítica, second edition.

Carreras, A. and X. Tafunell (2005). *Estadísticas históricas de España : siglos XIX-XX*. Second edition. Fundación BBVA.

Chen, L., L.J. Wei, and M. Parzen (2003). Quantile regression for correlated observations. *Proceedings of the Second Seattle Symposium in Biostatistics: Analysis of Correlated Data* (Eds. Danyu Lin and Patrick Heagerty). Lecture Notes in Statistics. Springer, New York.

Chernozhukov, V., I. Fernández-Val, and A. Kowalski (2015). Quantile regression with censoring and endogeneity. *Journal of Econometrics*, 186, 201-221.

Cloyne, J. (2013). Discretionary tax changes and the Macroeconomy: New narrative evidence from the United Kingdom. *American Economic Review*, 103:4, 1507-1528.

Cloyne, J. and P. Surico (2014). Household debt and the dynamic effects of income tax changes. *Review of Economic Studies*, 84 (1), 45-81.

- De Nardi, M., E. French, and J. B. Jones (2010). Why do the elderly save? The role of medical expenses. *Journal of Political Economy*, 118, 39-75.
- Díaz-Giménez, J., V. Quadrini and J. Ríos-Rull (1997). Dimensions of inequality: Facts on the U.S. distributions of earnings, income and wealth. *Federal Reserve Bank of Minneapolis: Quarterly Review*, 21 (2), 3-21.
- Eggertsson, G. and P. Krugman (2012). Debt, deleveraging, and the liquidity trap: A Fisher-Minsky-Koo approach. *Quarterly Journal of Economics*, 127, 1469-513.
- Gil, P., F. Martí, R. Morris, J.J. Pérez, and R. Ramos (2017). The output effects of tax changes: Narrative evidence from Spain. Banco de España Working Paper 1721.
- Giavazzi, F., and M. McMahon (2012). The household effects of government spending. In “Fiscal Policy after the Financial Crisis”, Eds. Alesina, A. and Giavazzi, F., University of Chicago Press.
- Hausman J. K. (2016). Fiscal policy and economic recovery: The case of the 1936 veterans’ bonus. *American Economic Review*, 106 (4), 1100-1143.
- INE (1988) Encuesta Continua de Presupuestos Familiares año 1986. Metodología y resultados.
- Jappelli, T., and L. Pistaferri (2014). Fiscal Policy and MPC heterogeneity. *American Economic Journal: Macroeconomics*, 6 (4), 107-136.
- Kaplan, G. and, G. Violante (2014). A model of the consumption response to fiscal stimulus payments. *Econometrica*, 82:4, 1199-1239.
- Kowalski, A. (2016). Censored quantile instrumental variable estimates of the price elasticity of expenditure on medical care. *Journal of Business & Economic Statistics*, 34:1, 107-117.
- Labeaga, J. M., and R. Osuna (2007). Expenditures at retirement by Spanish households. FEDEA Working Paper 2007-36.
- Lee, S. (2007). Endogeneity in quantile regression models: A control function approach. *Journal of Econometrics*, 141, 1131-1158.
- López, A. (1993) An assessment of the Encuesta Continua de Presupuestos Familiares (1985-89) as a source of information for applied research. Economics Working Paper 53, Universitat Pompeu Fabra.
- Luengo-Prado, M. J., and A. Sevilla (2013). Time to cook: Expenditure at retirement in Spain. *Economic Journal*, Royal Economic Society, 123, 764-789.
- Machado, J., J.M.C. Santos Silva, and K. Wei (2016). Quantiles, corners, and the extensive margin of trade. *European Economic Review*, 89, 73-84.
- Masier, G., and E. Villanueva (2011). Consumption and initial mortgage conditions. Evidence from survey data. ECB Working Paper Series, 1297, February.
- Mertens, K., and M. O. Ravn (2012). Empirical evidence on the aggregate effects of anticipated and unanticipated US tax policy shocks. *American Economic Journal: Economic Policy*, 4 (2), 145-81.
- Misra, K., and P. Surico (2014). Consumption, income changes, and heterogeneity: evidence from two fiscal stimulus programs. *American Economic Journal: Macroeconomics*, 6:4, 84-106.
- Monasterio, C., I. Sánchez, and F. Blanco (1996). Equidad y estabilidad del sistema de pensiones español. Fundación BBV.
- OECD (2017). Pensions at a Glance 2017: OECD and G20 Indicators, OECD Publishing, Paris. http://dx.doi.org/10.1787/pension_glance-2017-en

Parker, J. A., N. S. Souleles, D. S. Johnson, and R. McClelland (2013). Consumer spending and the economic stimulus payments of 2008. *American Economic Review*, 103 (6), 2530-2553.

Parraga-Rodriguez, S. (2016). The aggregate effect of government income transfers shocks - EU evidence, Banco de España working paper 1629.

Parraga-Rodriguez, S. (2018). The dynamic effect of public expenditure shocks in the United States, *Journal of Macroeconomics*, 56, 340-360.

Petersen, M. (2009). Estimating standard errors in finance panel data sets: Comparing approaches. *The Review of Financial Studies*, 22 (1), 435-480.

Pou, Ll. and J. Alegre (2002). La encuesta continua de preseupuestos familiares (1985-1996): descripción, representatividad y propuestas de metodología para la explotación de la información de los ingresos y el gasto. FUNCAS Working Paper 172/2002.

Romer, C. and D. Romer (2016). Transfer Payments and the Macroeconomy: The Effects of Social Security Benefit Increases, 1952-1991. *American Economic Review*, 8:4, 1-42.

Stephens, M. Jr. (2003) 3rd of the month: Do Social Security recipients smooth consumption between checks? *American Economic Review*, 93:1, 406-422.

Thompson, S. (2011). Simple formulas for standard errors that cluster by both firm and time. *Journal of Financial Economics*, 99, 110.

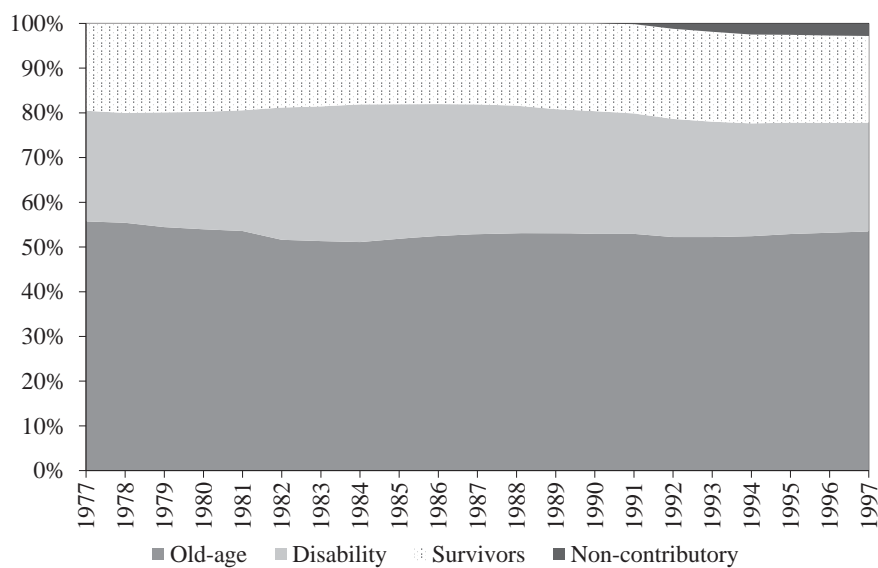
Wilcox D. W. (1989). Social security benefits, consumption expenditure, and the life cycle hypothesis. *Journal of Political Economy*, 97, 2883-04.

Appendix

A1 Types of benefits

Figure A1 shows that old-age benefits accounted for more than half the expenditure on public pensions. Here, notice that the government paid disability benefits after the beneficiary was 65 years old until 1997. Since 1998, recipients of disability benefits have been transferred to old-age pensions when they turn 65. At the time of the change, the weight of old age pensions in the Social Security's budget increased about 10 pp. Survivors benefits include benefits for widows, orphans, and other relatives. Although there were some special subsidies, non-contributory pensions did not exist until 1991. Data from Economic-Financial Reports to the Social Security budget.

Figure A1: Weight in the Budget of the Different Social Security Benefits



A2 Narrative series of pension-related policies in Spain

Table A1 summarizes all exogenous pension-related policies corresponding to 1979q1 to 1997q4. For each policy, the table reports the source, the enactment and implementation date, a short description, the motivation and the estimated annualized impact in millions of pesetas of 1992. While the sources were all in Spanish, this table provides a useful summary in English. A more detailed account of each policy, including quotes and explanations for the motivations, is available upon request. About the date of legislation and implementation, these correspond, respectively, to the day of passing the corresponding piece of legislation and the day when it becomes effective. Finally, the acronyms PP, I, and R represent the motivations, which stand for, respectively, Purchasing Power, Ideology and structural Reform.

Table A1: Exogenous Pension-Related Policies

Source	Enactment	Implementation	Description	Motivation	Mill pts of 1992
The Spanish Constitution	29/12/1978	29/12/1978	Article 41 provides the legal framework to the system of public pensions.
Royal Decree Law 43/1978, December 21 of 1978; Royal Decree Law 35/1978, November 16 of 1978.	16/11/1978	01/01/1979	New war pensions derived from the Spanish Civil War.	I	13,059
Royal Decree 47/1980	11/01/1980	01/02/1980	Adjustment social security pensions below CPI inflation. Discretionary rise of minimum pensions below CPI inflation.	PP I	-45,133 -1,436
Law 5/1979, September 18 of 1979	16/11/1979	01/01/1980	New war pensions derived from the Spanish Civil War.	I	47,087
Law 35/1980, June 26 of 1980	16/10/1980	10/01/1980	New war pensions derived from the Spanish Civil War.	I	47,087
Law 74/1980 General State Budget for 1981	29/12/1980	01/01/1981	Increase in spending on war pensions derived from the Spanish Civil War.	I	34,300
Newspapers. Special government report BOE 18/12/1995, 184, E.	07/05/1981	07/05/1981	Extraordinary expenses due to the break-out of the Toxic Oil Syndrome.	I	4,413
Royal Decree 77/1981	16/01/1981	01/01/1981	Adjustment social security pensions above CPI inflation. Discretionary rise of minimum pensions.	PP I	5,951 6,129
Law 44/1981 General State Budget for 1982	28/01/1981	01/01/1982	Increase in spending on war pensions derived from the Spanish Civil War.	I	6,419
Royal Decree 3218/1981	29/12/1981	01/01/1982	Adjustment social security pensions below CPI inflation.	PP	-98,008

Table A1 – Continued from previous page

Source	Enactment	Implementation	Description	Motivation	Mill pts of 1992
			Discretionary rise of minimum pensions below CPI inflation.	I	-30,816
Royal Decree 93/1983	19/01/1983	23/01/1983	Adjustment social security pensions above CPI inflation. Discretionary rise of minimum pensions.	PP I	10,302 40,258
Newspapers. Constitutional Court Ruling 103/1983	22/11/1983	04/01/1983	Equation of the criteria required to collect survivors [widows] benefits for men and women.	R	39,331
Law 9/1983 General State Budget for 1983	13/07/1983	01/08/1983	Increase in spending on war pensions derived from the Spanish Civil War.	I	47,345
Royal Decree 383/1984	01/02/1984	01/04/1984	New pensions for the social integration of the disabled	I	1,511
Law 37/1984	22/10/1984	01/01/1985	New war pensions derived from the Spanish Civil War.	I	11,988
Law 50/1984 General State Budget for 1985	30/12/1984	01/01/1985	Adjustment social security pensions below CPI inflation. Discretionary rise of minimum pensions. Extension of pensions for the social integration of the disabled.	PP I I	-19,811 12,075 1,040
Law 26/1985	31/07/1985	01/08/1985	Reform of contributory old age pensions. Introduction of automatic indexation beginning in 1986.	R	-117,762
Law 46/1985 General State Budget for 1986	27/12/1985	01/01/1986	Adjustment social security pensions below CPI inflation. Discretionary rise of minimum pensions.	PP I	-3,406 22,187
Suprem Court ruling Ar. 1741	10/04/1986	01/05/1986	Change in the criteria granting pensions for the disabled.	R	3,332
Law 21/1986 General State Budget for 1987	23/12/1986	01/01/1987	Increase in spending on war pensions derived from the Spanish Civil War.	I	13,560

Table A1 – Continued from previous page

Source	Enactment	Implementation	Description	Motivation	Mill pts of 1992
			Adjustment social security pensions above CPI inflation.	PP	11,804
			Discretionary rise of minimum pensions.	I	28,245
			Increase in spending on pensions for the disabled.	I	9,133
Law 33/1987 General State Budget for 1988	23/12/1987	01/01/1988	Increase in spending on war pensions derived from the Spanish Civil War.	I	24,832
			Adjustment social security pensions below CPI inflation.	PP	-21,242
			Discretionary rise of minimum pensions.	I	29,369
			Increase in spending on pensions for the disabled.	I	19,543
			Lower retirement age for FAS pensions.	I	7,488
Law 37/1988 General State Budget for 1989	28/12/1988	01/01/1989	Increase in spending on war pensions derived from the Spanish Civil War.	I	1,732
			Adjustment social security pensions below CPI inflation.	PP	-28,096
			Discretionary rise of minimum pensions.	I	40,411
			Discretionary rise of FAS pensions.	I	5,922
			Increase in spending on pensions for the disabled.	I	4,925
Law 4/1990 General State Budget for 1990	29/06/1990	01/07/1990	Increase in spending on war pensions derived from the Spanish Civil War.	I	14,416
			Increase in spending on pensions for the disabled.	I	4,141
Royal Decree Law 7/1989	29/12/1989	01/01/1990	Adjustment social security pensions above CPI inflation.	PP	31,690
			Discretionary rise of minimum pensions.	I	80,329
Law 31/1990 General State Budget for 1991	27/12/1990	01/01/1991	Increase in spending on war pensions derived from the Spanish Civil War.	I	45,922

Table A1 – Continued from previous page

Source	Enactment	Implementation	Description	Motivation	Mill pts of 1992
			Adjustment social security pensions above CPI inflation.	PP	23,981
			Discretionary rise of minimum pensions.	I	32,796
			Lower retirement age of FAS pensions.	I	1,369
Law 26/1990	20/12/1990	01/01/1991	Introduction of non-contributory pensions in the system of Social Security.	I	9,826
Law 31/1991 General State Budget for 1992	30/12/1991	01/01/1992	Fall in spending on war pensions due to ageing of the beneficiaries.	I	-458
			Adjustment social security pensions above CPI inflation.	PP	15,566
			Discretionary rise of minimum pensions.	I	51,997
			Extension of non-contributory pensions.	I	40,718
Law 39/1992 General State Budget for 1993	30/12/1991	01/01/1993	Adjustment social security pensions above CPI inflation.	PP	11,698
			Discretionary rise of minimum pensions.	I	6,236
			Extension of non-contributory pensions. Significant transfer of beneficiaries from other programs of social assistance. Adjustment of existent non-contributory pensions above CPI inflation.	I	23,195
Royal Decree Law 1/1994	20/06/1994	01/09/1994	Consolidation of the General Law on Social Security		
Law 21/1993 General State Budget for 1994	29/12/1994	01/01/1995	Extension of non-contributory pensions. Significant transfer of beneficiaries from other programs of social assistance. New pensions for the elderly emigrants.	I	13,965
Law 41/1994 General State Budget for 1995	30/12/1994	01/01/1995	Extension of non-contributory pensions. Significant transfer of beneficiaries from other programs of social assistance.	I	10,129
Royal Decree 728/1993	14/05/1993	01/01/1995	New pensions for the elderly emigrants	I	6,538

Table A1 – *Continued from previous page*

Source	Enactment	Implementation	Description	Motivation	Mill pts of 1992
Royal Decree-Law 12/1995	28/12/1995	01/01/1996	Discretionary rise of minimum pensions.	I	4,879
			Adjustment social security pensions above CPI inflation.	I	5,722
			Extension of non-contributory pensions. Significant transfer of beneficiaries from other programs of social assistance. Adjustment of existent non-contributory pensions above CPI inflation.	I	5,722
Law 12/1996 General State Budget for 1997	30/12/1996	01/01/1997	Adjustment social security pensions above CPI inflation.	PP	23,651
			Discretionary rise of minimum pensions.	I	9,703
			Extension of non-contributory pensions. Significant transfer of beneficiaries from other programs of social assistance. Adjustment of existent non-contributory pensions above CPI inflation.	I	8,119
Law 24/1997	15/07/1997	01/08/1997	Reform of old age and survivors [orphans] pensions.	R	-13,520

A3 Items by category of expenditure

Table A2: Items by Category of Expenditure

Expenditure	Items
Food	Rice, flour, bread, cereals, pulse, cakes, pasta, meat, delicatessen, fish, shellfish, milk, yogurt, butter, cheese, eggs, oil, fruit, juice, nuts, vegetables, potatoes, sugar, coffee, tea, cocoa, jelly, honey, chocolate, ice creams, spices, mineral water, other non-alcoholic drinks.
Shelter	Rent (real or imputed); bills including local taxes, trash taxes, water, electricity, telephone, heating, house community expenditures; house repairs; cleaning products and services.
Apparel	Clothes, footwear, and accessories including repairs.
Leisure	Media (phones, TVs, laptops, etc.), sports equipment, books, instruments, other small gadgets for leisure and repairs; shows (cinema, theater, concerts, etc.), museums, pet and garden care expenditures, journal and magazines, stationery, bars and restaurants, hotels, holidays, bet games, and other services for leisure and culture.
Furnishings	Furniture, house textile, large and small appliances (microwaves, fridges, blenders, etc.), tableware, garden tools, electric material, and repairs.
Transport	Vehicles and spare parts, repairs in a garage, fuel and lubricants, car renting, insurances, parking expenditures, tolls, urban transport (subway, bus, etc.), cabs, trains, air transport, sea transport, telegraph, and postal expenditures.
Non-Durables	Food and non-alcoholic beverages, shows, pet and garden care expenditures, press and stationery, bars and restaurants, hotels, holidays, bet games, hairdresser, stylist, beauty products, services fees, donation to other members of the household, donation to other households or institutions, alcoholic beverages, tobacco and cigarettes, medicines, medical services, education.
Durables	apparel, furnishings, vehicles and spare parts, media equipment, sports equipment, books (including textbooks), instruments, orthopedic material, tombstones, jewelry, suitcases, buggies, small personal appliances (e.g., electric razor).

A4 Imputation of capital income to pensioners in the EPC

Figure A3 shows the empirical distribution of the predicted probabilities from the estimation of a probit model for whether households own an average level of capital income above the median on household characteristics common to the EPC and the ECPF85. The covariates include the age, sex and education of the reference person, the household size, a dummy for whether the household owns real estate, and regional dummies. The histograms show a similar profile, albeit more households have intermediate predicted probabilities in the EPC. This is partly compensated by more predicted probabilities close to zero in the ECPF85. All in all, more probabilities below 0.5 were predicted for the EPC.

Pensioners in the EPC were assigned a level of capital income above the median ($y = 1$) if their predicted probabilities were in the upper half of the distribution of probabilities. This classification implies similar characteristics for pensioners with $y = 1$ in both surveys (Table A3).

Alternatively, we could use the one-time survey of 1980-81 to estimate the probabilities that pensioners have a positive level of capital income. However, less than 50% of pensioners reported a definite amount of capital income. As a result, the probit model was modified such that the dependent variable $y = 1$ if the pensioners reported a positive level of capital income, and zero otherwise.

Again, pensioners in the EPC were assigned a positive level of capital income ($y = 1$) if their predicted probabilities were in the upper half of the distribution of probabilities. Table A4 shows that this classification implies crucial differences between pensioners with $y = 1$ in either survey, especially the number of observations and their tenure of real estate, an essential component of wealth. Even so, Figure A4 shows that the estimated effects on spending using these alternative probabilities are halfway the effects grouping pensioners according to their real estate, and using the ECPF85 to determine the likelihood that pensioners have a level of capital income above the median.

Figure A2: Empirical Distribution of Predicted Probabilities by Survey

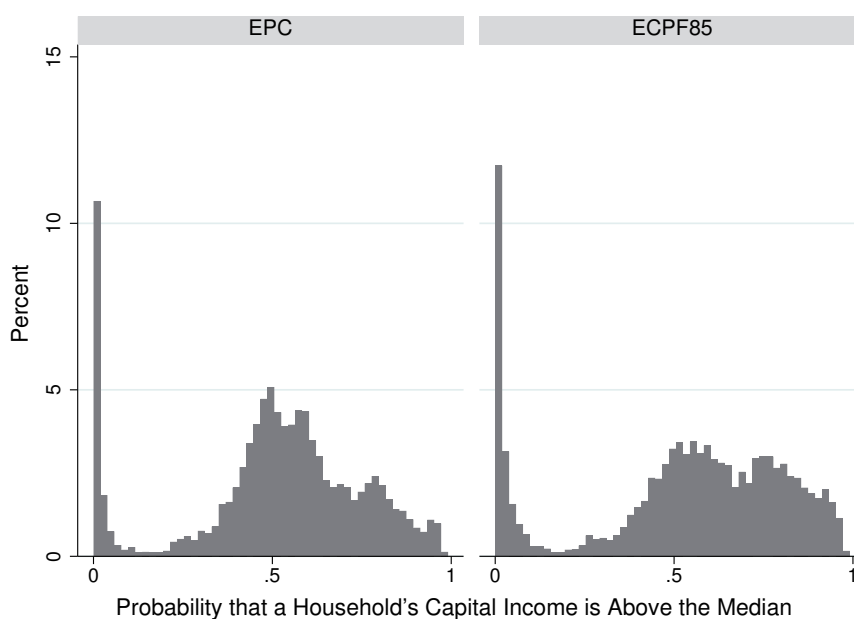


Table A3: Predicted Characteristics of Pensioners with Capital Income Above the Median in the EPC based on the ECPF85

	EPC	ECPF85
$y = 1$	48.0%	50.0%
Age	71	70
Woman	17.9%	25.9%
Primary education	86.8%	86.9%
Home owner	95.1%	98.5%
Other real estate	23.2%	20.7%
Ratio 80/20 total expenditure	2.7	2.3

Table A4: Predicted Characteristics of Pensioners with a Positive Capital Income in the EPC based on the EPF80/81

	EPC	EPF80/81
$y = 1$	50.9%	36.2%
Age	72	71
Woman	18.2%	25.8%
Primary education	86.1%	89.6%
Home owner	87.1%	77.1%
Other real estate	18.4%	4.9%
Ratio 80/20 total expenditure	3.8	3.7

Figure A3: Empirical Distribution of Predicted Probabilities by Survey

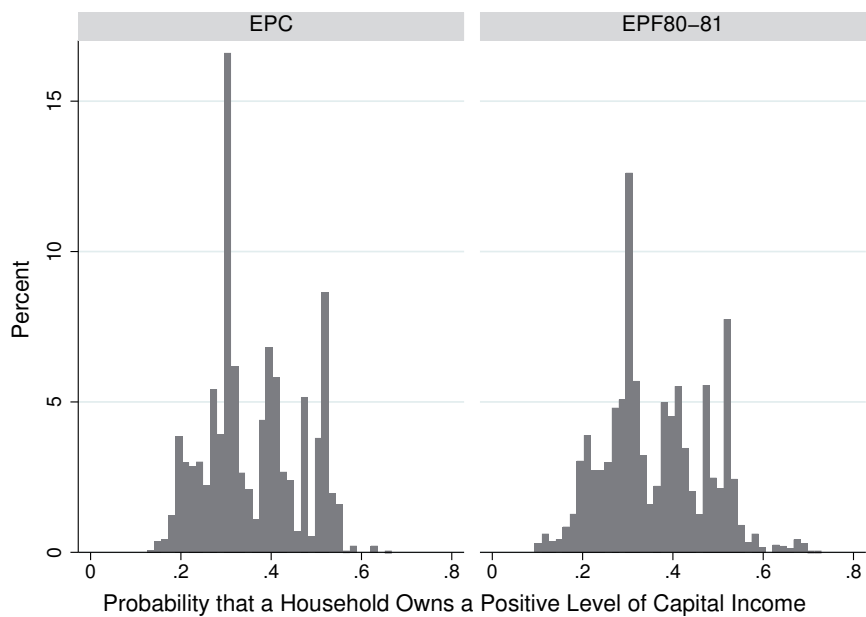
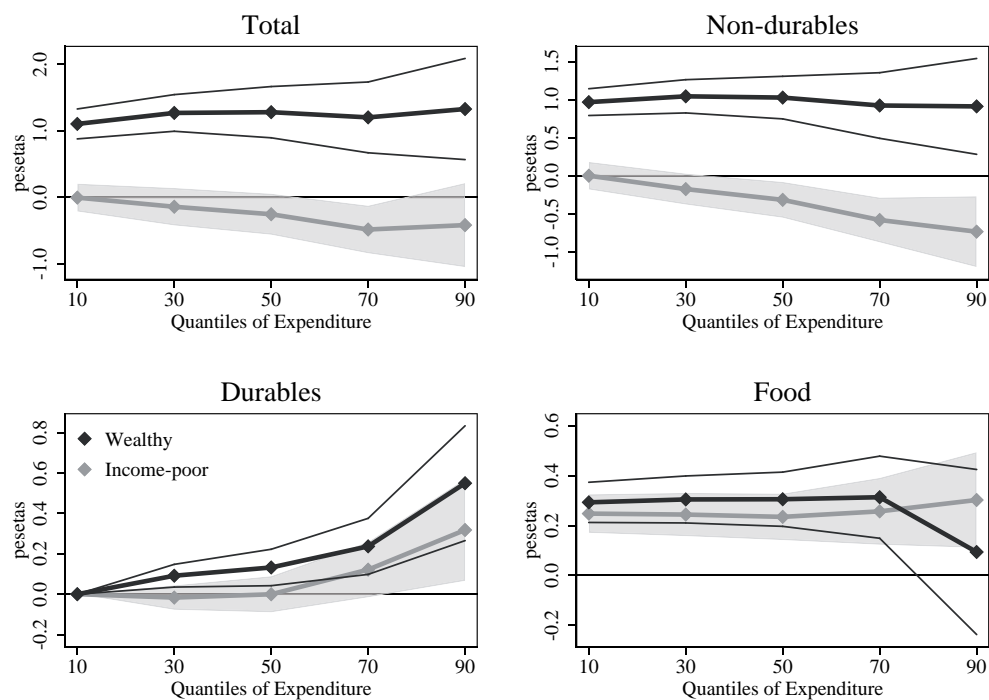


Figure A4: Effects on Spending by Quantile of Household Expenditure, Grouping of Pensioners Based on their Capital Income



Notes: The dark lines with marker report the response of pensioners with a positive level of capital income to an increase of 1,000 pesetas in the average pension. The light lines with marker report the response of pensioners with no capital income. The solid lines and shaded area report 95 confidence level intervals. Predicted probabilities for observations without information on household income using the EPF80/81. (Censored) Median regression estimates for (durables) total expenditure, non-durables, and food. Regressions include controls for household characteristics and time effects. Estimation sample 1977q2 to 1997q1; Observations 130,621.

A5 A simulation exercise for the confidence intervals

This appendix presents a simple simulation exercise to compare the efficiency of alternative methods to compute standard errors for quantile regression with dependent data. In a recent paper, Parente and Santos Silva (2016) developed a standardized routine to compute clustered standard errors for quantile regression estimates. Machado, Santos Silva and Wei (2016) prove the necessary modifications for the method to be applied to quantile regressions for corner solutions data. Alternatively, the influential paper by Abrevaya and Dahl (2008) suggests a bootstrap method to compute standard errors for (censored) quantile regression estimates when the data are dependent. The bootstrap samples are generated by repeatedly drawing (with replacement) a unit from the sample of G groups, and including all observations for such unit. However, it appears that so far does not exist a formal comparison of both methods to guide the empirical researcher. The gap is even more so for censored quantile regression. Thus, this appendix contributes to the discussion with a simulation exercise that compares the efficiency of the clustered and bootstrap-computed confidence intervals for censored quantile regression with dependent data.

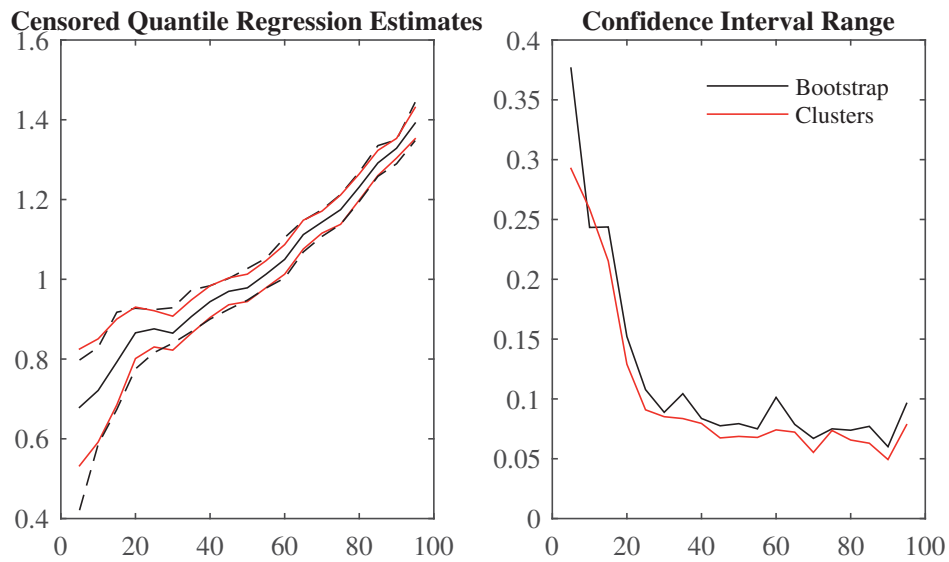
The exercise uses a simple linear model with one covariate such that

$$y = \max\{0, -0.5 + x + (0.25 + 0.25x)e\}$$
$$x, e \sim N(0, 1)$$

The number of observations is set to 10,000 and the number of clusters to 1,250. The average number of observations per cluster is 8, with a minimum of 1 and a maximum of 18 observations per cluster. The relation between the number of observations and the number of clusters has been chosen to resemble the Spanish ECPF85 household expenditure survey. Moreover, to mimic real applications, I draw 200 bootstrap samples (see Kowalski 2016).

The first subplot of Figure A5 shows the censored quantile estimates along with the normal based 95 percent confidence intervals using either method. For the bootstrap method, the confidence intervals are obtained as the 0.025 and 0.975 quantiles of the bootstrap coefficients. In

Figure A5: Simulation Results



a standard desktop computer, the cluster-method (solid and red lines) took 16 seconds, while the bootstrap-method (dashed and black lines) took 38 minutes and 28 seconds. These timings side with the known unfeasibility of bootstrap methods in similar real applications, which may involve a more substantial number of covariates and/or observations. On the other hand, both methods yield similar confidence intervals, albeit clustered standard errors tend to be slightly tighter (second subplot).

BANCO DE ESPAÑA PUBLICATIONS

WORKING PAPERS

- 1810 ROBERTO RAMOS and CARLOS SANZ: Backing the incumbent in difficult times: the electoral impact of wildfires.
- 1811 GABRIEL JIMÉNEZ, ENRIQUE MORAL-BENITO and RAQUEL VEGAS: Bank lending standards over the cycle: the role of firms' productivity and credit risk.
- 1812 JUAN S. MORA-SANGUINETTI and ROK SPRUK: Industry vs services: do enforcement institutions matter for specialization patterns? Disaggregated evidence from Spain.
- 1813 JAMES CLOYNE, CLODOMIRO FERREIRA and PAOLO SURICO: Monetary policy when households have debt: new evidence on the transmission mechanism.
- 1814 DMITRI KIRPICHEV and ENRIQUE MORAL-BENITO: The costs of trade protectionism: evidence from Spanish firms and non-tariff measures.
- 1815 ISABEL ARGIMÓN, CLEMENS BONNER, RICARDO CORREA, PATTY DUIJM, JON FROST, JAKOB DE HAAN, LEO DE HAAN and VIKTORS STEBUNOV: Financial institutions' business models and the global transmission of monetary policy.
- 1816 JOSE ASTURIAS, MANUEL GARCÍA-SANTANA and ROBERTO RAMOS: Competition and the welfare gains from transportation infrastructure: evidence from the Golden Quadrilateral of India.
- 1817 SANDRA GARCÍA-URIBE: Multidimensional media slant: complementarities in news reporting by US newspapers.
- 1818 PILAR CUADRADO, AITOR LACUESTA, MARÍA DE LOS LLANOS MATEA and F. JAVIER PALENCIA-GONZÁLEZ: Price strategies of independent and branded dealers in retail gas market. The case of a contract reform in Spain.
- 1819 ALBERTO FUERTES, RICARDO GIMENO and JOSÉ MANUEL MARQUÉS: Extraction of inflation expectations from financial instruments in Latin America.
- 1820 MARIO ALLOZA, PABLO BURRIEL and JAVIER J. PÉREZ: Fiscal policies in the euro area: revisiting the size of spillovers.
- 1821 MARTA MARTÍNEZ-MATUTE and ALBERTO URTASUN: Uncertainty, firm heterogeneity and labour adjustments. Evidence from European countries.
- 1822 GABRIELE FIORENTINI, ALESSANDRO GALESÌ, GABRIEL PÉREZ-QUIRÓS and ENRIQUE SENTANA: The rise and fall of the natural interest rate.
- 1823 ALBERTO MARTÍN, ENRIQUE MORAL-BENITO and TOM SCHMITZ: The financial transmission of housing bubbles: evidence from Spain.
- 1824 DOMINIK THALER: Sovereign default, domestic banks and exclusion from international capital markets.
- 1825 JORGE E. GALÁN and JAVIER MENCÍA: Empirical assessment of alternative structural methods for identifying cyclical systemic risk in Europe.
- 1826 ROBERTO BLANCO and NOELIA JIMÉNEZ: Credit allocation along the business cycle: evidence from the latest boom bust credit cycle in Spain.
- 1827 ISABEL ARGIMÓN: The relevance of currency-denomination for the cross-border effects of monetary policy.
- 1828 SANDRA GARCÍA-URIBE: The effects of tax changes on economic activity: a narrative approach to frequent anticipations.
- 1829 MATÍAS CABRERA, GERALD P. DWYER and MARÍA J. NIETO: The G-20 regulatory agenda and bank risk.
- 1830 JACOPO TIMINI and MARINA CONESA: Chinese exports and non-tariff measures: testing for heterogeneous effects at the product level.
- 1831 JAVIER ANDRÉS, JOSÉ E. BOSCA, JAVIER FERRI and CRISTINA FUENTES-ALBERO: Households' balance sheets and the effect of fiscal policy.
- 1832 ÓSCAR ARCE, MIGUEL GARCÍA-POSADA, SERGIO MAYORDOMO and STEVEN ONGENA: Adapting lending policies when negative interest rates hit banks' profits.
- 1833 VICENTE SALAS, LUCIO SAN JUAN and JAVIER VALLÉS: Corporate cost and profit shares in the euro area and the US: the same story?
- 1834 MARTÍN GONZÁLEZ-EIRAS and CARLOS SANZ: Women's representation in politics: voter bias, party bias, and electoral systems.
- 1835 MÓNICA CORREA-LÓPEZ and BEATRIZ DE BLAS: Faraway, so close! Technology diffusion and firm heterogeneity in the medium term cycle of advanced economies.
- 1836 JACOPO TIMINI: The margins of trade: market entry and sector spillovers, the case of Italy (1862-1913).
- 1837 HENRIQUE S. BASSO and OMAR RACHEDI: The young, the old, and the government: demographics and fiscal multipliers.
- 1838 PAU ROLDÁN and SONIA GILBUKH: Firm dynamics and pricing under customer capital accumulation.

- 1839 GUILHERME BANDEIRA, JORDI CABALLÉ and EUGENIA VELLA: Should I stay or should I go? Austerity, unemployment and migration.
- 1840 ALESSIO MORO and OMAR RACHEDI: The changing structure of government consumption spending.
- 1841 GERGELY GANICS, ATSUSHI INOUE and BARBARA ROSSI: Confidence intervals for bias and size distortion in IV and local projections – IV models.
- 1842 MARÍA GIL, JAVIER J. PÉREZ, A. JESÚS SÁNCHEZ and ALBERTO URTASUN: Nowcasting private consumption: traditional indicators, uncertainty measures, credit cards and some internet data.
- 1843 MATÍAS LAMAS and JAVIER MENCÍA: What drives sovereign debt portfolios of banks in a crisis context?
- 1844 MIGUEL ALMUNIA, POL ANTRÀS, DAVID LÓPEZ-RODRÍGUEZ and EDUARDO MORALES: Venting out: exports during a domestic slump.
- 1845 LUCA FORNARO and FEDERICA ROMEI: The paradox of global thrift.
- 1846 JUAN S. MORA-SANGUINETTI and MARTA MARTÍNEZ-MATUTE: An economic analysis of court fees: evidence from the Spanish civil jurisdiction.
- 1847 MIKEL BEDAYO, ÁNGEL ESTRADA and JESÚS SAURINA: Bank capital, lending booms, and busts. Evidence from Spain in the last 150 years.
- 1848 DANIEL DEJUÁN and CORINNA GHIRELLI: Policy uncertainty and investment in Spain.
- 1849 CRISTINA BARCELÓ and ERNESTO VILLANUEVA: The risk of job loss, household formation and housing demand: evidence from differences in severance payments.
- 1850 FEDERICO TAGLIATI: Welfare effects of an in-kind transfer program: evidence from Mexico.
- 1851 ÓSCAR ARCE, GALO NUÑO, DOMINIK THALER and CARLOS THOMAS: A large central bank balance sheet? Floor vs corridor systems in a New Keynesian environment.
- 1901 EDUARDO GUTIÉRREZ and ENRIQUE MORAL-BENITO: Trade and credit: revisiting the evidence.
- 1902 LAURENT CAVENAILE and PAU ROLDAN: Advertising, innovation and economic growth.
- 1903 DESISLAVA C. ANDREEVA and MIGUEL GARCÍA-POSADA: The impact of the ECB's targeted long-term refinancing operations on banks' lending policies: the role of competition.
- 1904 ANDREA ALBANESE, CORINNA GHIRELLI and MATTEO PICCHIO: Timed to say goodbye: does unemployment benefit eligibility affect worker layoffs?
- 1905 CORINNA GHIRELLI, MARÍA GIL, JAVIER J. PÉREZ and ALBERTO URTASUN: Measuring economic and economic policy uncertainty, and their macroeconomic effects: the case of Spain.
- 1906 CORINNA GHIRELLI, JAVIER J. PÉREZ and ALBERTO URTASUN: A new economic policy uncertainty index for Spain.
- 1907 CARLOS SANZ, ALBERT SOLÉ-OLLÉ and PILAR SORRIBAS-NAVARRO: Economic hardship, political corruption, and the emergence of challenger parties. Evidence from the Great Recession in Spain.
- 1908 SERGIO MAYORDOMO and OMAR RACHEDI: The China syndrome affects banks: the credit supply channel of foreign import competition.
- 1909 MÓNICA CORREA-LÓPEZ, MATÍAS PACCE and KATHI SCHLEPPER: Exploring trend inflation dynamics in Euro Area countries.
- 1910 JAMES COSTAIN, ANTON NAKOV and BORJA PETIT: Monetary policy implications of state-dependent prices and wages.
- 1911 JAMES CLOYNE, CLODOMIRO FERREIRA, MAREN FROEMEL and PAOLO SURICO: Monetary policy, corporate finance and investment.
- 1912 MARIO ALLOZA and CARLOS SANZ: Jobs multipliers: evidence from a large fiscal stimulus in Spain.
- 1913 SUSANA PÁRRAGA RODRÍGUEZ: The effects of pension-related policies on household spending.